meeting summary

Report of the First Prospectus Development Team of the U.S. Weather Research Program to NOAA and the NSF

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Abstract

NOAA and the NSF have jointly commissioned science planning activities under the interagency U.S. Weather Research Program Office at Silver Spring, Maryland, and the Office of the Lead Scientist at NCAR. The lead scientist charged the authors of this report to take a first step in program definition by recommending scientific directions mainly from a fundamental and theoretical perspective. Spirited discussions ranging from advanced concepts of predictability to practical problems in operational forecasting led to the publication of this report. This is the first in a series of reports to the community that, in aggregate, will serve to shape the program.

Concerns are expressed about knowledge pertaining to the economic value of weather information and the costs and benefits associated with potential improvements in observing systems and forecasting techniques. Ten recommendations are made concerning various data infrastructure issues. These address a determination of an optimal mix of observing systems, use of programmable observing systems, land surface properties and processes, improved water vapor measurements, improved measurements in clouds, aircraft measurements in aid of hurricane forecasting, improved measurements of the upper ocean, global rawinsonde coverage, specialized research observing systems, optimal use of existing and emerging operational data sources, open operational data, and improved data access.

Seventeen emerging basic research opportunities are identified. These include fundamental physics of land–air interaction, adaptive observing strategies, dynamical influences of cloud microphysical processes, seasonal and longer timescale variations, the fundamental role of the tropopause in extratropical dynamics, tropical cyclone genesis and intensity change, dynamics of landfallen tropical cyclones, mesoscale convective system dynamics and physics, coupling of atmospheric boundary layers with deep convection, convective ensemble dynamics, orographic and other influences on sources of potential vorticity, orography influences on airflow and precipitation, interaction of balanced and unbalanced circulation systems, mesoscale frontal cyclones, application of models to forecasting fire weather, ensemble forecasting and data assimilation techniques, and advanced model output statistical techniques.

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1. Program context and statement of purpose

Prospectus Development Teams (PDTs) are small groups of scientists and/or technologists that are convened by the United States Weather Research Program (USWRP) Lead Scientist on a one-time basis to discuss critical issues and to provide advice related to future directions of the program. PDTs are a principal source of information for the Science Advisory Committee (SAC). The SAC is a standing committee charged with the duty of making recommendations to the program office based upon overall program objectives. Considerations of the SAC include fundamental science opportunities, societal impacts, technological feasibility, agency needs, and end-user requirements.

The First Prospectus Development Team was constituted in early 1994 to assist the National Oceanic and Atmospheric Administration (NOAA) and the National Science Foundation (NSF) with long-range planning in research in atmospheric dynamics with an emphasis on examining the research objectives of the USWRP. The entire team assembled in Dedham, Massachusetts, in October 1994, and over a 3-day period deliberated the issues contained in this report. Our charge was threefold:

1) To identify and delineate critical issues related to weather and forecasting with emphasis on circulations at the mesoscale and short-term forecasting.
2) To draft an overarching dynamical research prospectus that can be subjected to community scrutiny, published, and disseminated to appropriate fora such as the Science Advisory Committee of the USWRP and the National Academy of Science.
3) To identify the need for any future PDTs and their foci.

We interpreted our mission more broadly as a call to provide a broad overview of opportunities to advance the state of atmospheric science, emphasizing those opportunities that may bear on saving lives and property and on improving the U.S. economy.

We viewed our primary purpose as identifying that subset of the great intellectual challenges of the day that may lead to improvements in weather observations, forecasting, and warning and would thus be of great benefit to society. The members of the first Prospectus Development Team are listed as authors of this meeting summary. Richard Carbone (NCAR), William Hooke (NOAA), and Stephan Nelson (NSF) were observers; Steven Rhodes (NCAR) was an invited speaker.

2. Introduction

Progress in weather forecasting and warning has been one of the great success stories of the postwar era. Advances in basic understanding of weather, the establishment of a global observing system, and the advent of numerical weather prediction put weather forecasting on a solid scientific foundation, while the establishment of national centers dedicated to severe weather prediction, the deployment of weather radar and satellites, and the institution of emergency preparedness programs led to a dramatic decline in deaths from severe weather phenomena such as hurricanes and tornadoes.

Basic research in atmospheric science has been one of the most cost-effective investments that society has made in science. Progress in the basic understanding of phenomena such as severe thunderstorms has led directly to improved warnings and reduction of loss of life, while technical advances in areas such as numerical weather prediction and application of statistics to model output have contributed to much improved forecasts at every level.

At the same time, some weather-related problems faced by society have become more pressing. As we become increasingly dependent on transportation at every level, we are becoming more vulnerable to economic loss caused by the weather. Certain demographic trends have also increased our vulnerability. For example, Hurricane Andrew in 1992 was the costliest natural disaster in U.S. history, owing in large measure to the great increase in population in the coastal plain. Moreover, society faces the possibility of potentially devastating climate change, and the understanding and modeling of climate and climate change depends critically on accurate representation of key weather phenomena.

Society chooses to invest in basic research not just because of perceived tangible benefits but because it is fulfilled by efforts to push back the frontiers of knowledge. Few would deny the largely intangible but very real value of intellectual achievements such as the formulation of quantum mechanics, the discovery of DNA, or the characterization of the physics of deterministic but nonperiodic systems. In our own field, advances in basic understanding often seem insignificant in comparison to progress in such fields as cosmology or molecular biology, but experience shows that the public continues to be fascinated by the weather and very interested in the underlying science. While this report will be concerned more with the potential for advances in atmospheric science that will have immediate application to the benefit of society, the *intrinsic* value of basic research should not be forgotten.

Atmospheric science is poised to make another series of major advances leading to improved weather prediction and warnings. The deployment of a new generation of meteorological satellites and ground-based wind profilers, the recent modernization of the National Weather Service, including the deployment of a network of advanced Doppler radar [NEXRAD (Next Generation Weather Radar)] and the establishment of new means of communicating weather information, combine with great strides in basic understanding of weather systems and new techniques such as ensemble forecasting to offer the promise of much improved forecasts and warnings. But to realize these potential improvements, new means of measuring the atmosphere, ocean, and land surface will have to be developed and implemented, and existing measurement systems such as rawinsondes, mobile radars, and research aircraft will have to be maintained and upgraded. The panel cannot stress enough the continued need for in situ and ground-based remote sensing capabilities and is alarmed at the deterioration of fundamental observing systems such as the global rawinsonde network. In surveying the state of basic research in weather dynamics, time after time the panel came to the conclusion that further progress was limited mostly by lack of appropriate measurement capabilities. For this reason, many of our recommendations focus on the need for better measurement systems. But it must also be recognized that we have the ability to predict just how improvements in observing systems or techniques might actually improve forecasts. This capability is largely unexploited. One of the most important conclusions of our team is that we must do far more to exploit known techniques, such as observing system simulation experiments, to make a priori estimates of optimal combinations of observing systems and forecasting techniques for application to specific forecasting problems and that we must work much more closely with other disciplines, particularly economics, to project the potential costs and benefits of new observing systems and forecasting methods.

At present, we have very little understanding of the costs and benefits of weather data and forecasting information. For example, the National Weather Service is concerned with the $20 million cost of maintaining the North American rawinsonde network, while it costs the U.S. Air Force on the order of $35 million to perform aircraft reconnaissance of Atlantic hurricanes. A single polar-orbiting satellite can cost the National Aeronautics and Space Administration $100 million. An assessment of the relative value of these systems is greatly impeded by a lack of understanding of the underlying economics and the fact that the costs are incurred by a spectrum of different federal agencies with little incentive or ability to compare costs.

We have a limited understanding of the value of weather information to the end user, except in extreme situations. For instance, a missed 24-hour forecast of a major snowstorm in the Northeast is known to incur large costs in unnecessary preparation on the one hand or lack of preparation on the other, and it is estimated that a 1-day greater lead time in the forecast of Hurricane Andrew's landfall in south Florida might have saved the nation $1 billion. The value of weather information in less dramatic cases has not been adequately addressed but is still thought to be significant. Businesses in the state of Oklahoma have realized significant annual savings through better short-term predictions made possible by an advanced surface mesonet in that state.

There may be much to be gained by commissioning a comprehensive report on the economic value of weather information, focusing on the costs and benefits of potential improvements in observing systems and forecasting techniques. If properly executed by a group composed of meteorologists, economists, and policy experts, such a report should prove invaluable to federal agencies charged with funding new observing systems, technique development, and basic research. This belief led to one of the recommendations listed in section 3.

We begin by summarizing the principal recommendations of our team and then in the body of the report discuss specific issues in basic research and operational weather forecasting, drawing on the expertise of individual panel members and, in some instances, scientists from outside the panel, as appropriate. Each issue is addressed with the goal of answering three basic questions:

1) What basic research progress seems possible over the next 15 years, emphasizing progress that leads to improvements in forecasts and warnings?
2) What technical and scientific advances might allow us to achieve this progress?
3) What organizational structures (e.g., government agencies, coordinated field programs) are best suited to facilitate progress?

The specific recommendations that follow emerge from consideration of the issues addressed by the panel and reviewed in section 4.

3. Summary of recommendations

As noted before, we reached the conclusion that progress on a large number of basic and applied research issues is being limited by lack of appropriate measurements. We thus begin by listing 10 principal recommendations pertaining to observations of the atmosphere, ocean, and land surface.

1) Fundamental improvements in forecasting at the 2–7-day range could have enormous potential economic benefits but will require greatly improved data over the oceans and other data-sparse areas. We strongly encourage the support of research seeking to determine optimal combinations of satellite and ground-based remote sensing, aircraft, balloon, and surface observations as well as the support of key technological develop-

ments such as satellite-borne active sensing techniques, near-field remote sensing of atmospheric water vapor, and observations from commercial and, perhaps, pilotless aircraft. Such research should include comprehensive, well-posed observing system simulation experiments (OSSEs). Cost–benefit analysis should play a key role in the definition of "optimal" as it is used above, and *the cost to the nation as a whole, rather than the cost to individual agencies, should be considered.*

2) Recent research strongly suggests that adjoint techniques or breeding methods can be used to target specific regions of the atmosphere for observational scrutiny during the subsequent data assimilation cycle, resulting in greatly reduced forecast error. We advocate enhanced research on programmable observations and their potential for substantial reduction in forecast error.
3) Much improved measurements of land surface properties, especially soil moisture, and better understanding of land–atmosphere interaction may hold the key to dramatic improvements in a number of forecasting problems, including the location and timing of the onset of deep convection over land, quantitative precipitation forecasting in general, and seasonal climate prediction. We see a major opportunity that may be exploited by encouraging interactions between hydrologists and atmospheric scientists coupled with the development of new means of routine and comprehensive measurement of soil properties.
4) Improvement in numerical forecasting, and especially quantitative precipitation forecasting, is severely impeded by poorly resolved and inaccurate measurements of atmospheric water vapor. High priority must be given to new water vapor measurement systems and to research that seeks to delineate the water vapor observations necessary to address specific forecast problems. The panel believes that adequate observation of atmospheric water vapor will rely heavily on in situ measurements.
5) The solution to a variety of outstanding problems ranging from storm dynamics to climate will depend on much better measurements of cloud physical processes involving ice at high altitudes. We must make substantial improvements in our ability to make in situ measurements in clouds in the upper troposphere.
6) The worst natural catastrophes in U.S. history were caused by tropical cyclones. Hurricane Andrew, in 1992, was the costliest natural disaster in U.S. history, causing more than $25 billion in damages. Detection of hurricanes has been greatly facilitated by satellite-based observations, but quantitative prediction of storm motion and of structure and intensity change is known to profit greatly from in situ measurements. Such measurements are particularly lacking at high altitude, where many important interactions with the environment occur. We strongly recommend the support of research seeking to delineate optimal combinations of measurement systems in aid of hurricane forecasting and advocate greatly increased aircraft measurements of hurricanes and their synoptic-scale environment at high altitudes.
7) Tropical cyclones and some classes of extratropical marine cyclones are sensitive to local sea surface temperature and are known to influence ocean temperature through wind-induced stirring and upwelling. Modeling studies show that this feedback has an important effect on hurricane intensity, but observations of this interaction are lacking. The panel strongly encourages enhanced observations of the upper ocean during the passage of tropical and some extratropical cyclones.
8) Global rawinsonde coverage must be maintained and enhanced or a better substitute developed than is now available if progress is to be made on a variety of operational and basic research problems. The reduction of in situ measurements in general, in favor of remote sensing measurements, is in many cases premature.
9) The resources to maintain a balanced national basic research observing infrastructure must be restored, enhanced, and maintained. Satellites do not provide the spatial resolution or three-dimensional coverage required to diagnose many basic physical processes, such as those involving clouds and precipitation. NEXRAD radars have operational constraints that compromise their use in basic research, even if combined with other technologies. Mobile radars, research aircraft, and surface observations used to make high-precision, high-resolution observations with sufficient time continuity are required to determine how many atmospheric processes evolve.
10) Scientific research that attempts to make optimal use of existing and emerging data sources (e.g., satellite and NEXRAD data) could be greatly accelerated by facilitating access to real-time and archived data by researchers inside and outside NOAA. Improved means of forecasting and of communicating weather information would also benefit greatly from increased access. New means of data distribution, especially by user interrogation (as opposed to global distribution) should be explored. We assert that free and open access to meteorological data has contributed greatly to national and global welfare, and we strongly op-

pose the efforts of some nations to restrict access to data.

The following lettered points highlight what we regard as the highest priority emerging basic research opportunities pertinent to the issue of weather forecasting and warning.

(a) *The fundamental physics of land–air interaction.* As mentioned in recommendation 3, better understanding and measurement of land–air interactions could lead to substantial improvements in forecasting of convection, boundary layer cloud cover, and regional climate anomalies.
(b) *Adaptive observation strategies.* Basic research on the use of adjoint and ensemble techniques to target specific regions of the atmosphere for observation by programmable platforms may lead to large gains in the skill of numerical weather forecasts for a relatively small investment in additional observations.
(c) *The influence of cloud microphysical processes on atmospheric dynamics and water vapor distributions.* These processes can be important on both short and long timescales. Improved observations of ice microphysical processes will be necessary for progress on this important research issue.
(d) *Seasonal climate variations and their dependence upon stochastic, internal variability of the atmosphere as well as variations linked to longer timescale phenomena in the oceans, atmosphere, and land surface.* Research on blocking and on land–atmosphere interactions has the potential to lead to significant improvements in seasonal forecasts. The seasonal prediction problem depends sensitively on proper representation of sources and sinks of potential vorticity, whereas short-range prediction depends more on potential vorticity advection.
(e) *The fundamental role of the tropopause in atmospheric dynamics and the possible benefits of better observations at and near the tropopause.* It has become clear that the tropopause is the seat of many important dynamical processes and exchanges of chemical species, yet comparatively little is understood about the possible benefits of enhanced observations and greater model resolution near the tropopause.
(f) *Tropical cyclone genesis and intensity change, including the role of the upper-ocean response and the dynamics of interactions with potential vorticity anomalies in the upper troposphere and lower stratosphere.* The physics of tropical cyclogenesis remains enigmatic, and intensity change remains a major forecast challenge.
(g) *The dynamics of landfallen tropical cyclones, particularly as it relates to flash floods.* Some of the worst disasters in U.S. history were caused by tropical cyclone–associated flooding.
(h) *The dynamics and cloud physics of mesoscale convective systems and their possible relationship to synoptic- and subsynoptic-scale potential vorticity anomalies.* Such systems produce a large fraction of summertime precipitation in parts of the United States.
(i) *The dynamics of deep convective downdrafts.* These play a major role in the dynamics of at least some mesoscale convective systems and in the overall heat balance of the tropical boundary layer but have received comparatively little attention in formulating representations of cumulus convection.
(j) *The coupling of the atmospheric boundary layer with deep convection and the merging of the understanding of cloud-scale dynamics and prediction with the understanding of convective ensemble dynamics.* For example, Tropical Oceans Global Atmosphere Coupled Ocean–Atmosphere Response Experiment data shows that convective downdrafts are important contributors to surface heat fluxes in the western Pacific region, but this is not accounted for in most models.
(k) *Orographic and other influences on sources and sinks of atmospheric potential vorticity.* This includes the fundamental unsolved problem of excitation of internal waves by orography and the interaction of such waves with larger-scale flows. Forecasts beyond a few days rely heavily on an accurate account of such processes.
(l) *The influence of orography upon airflow and precipitation.* Forecasting of heavy precipitation and strong winds in mountainous terrain and in some coastal regions will depend on progress in understanding the interaction of large-scale flows with mesoscale coastal and inland topography.
(m) *The interaction of quasi-balanced and unbalanced circulation systems.* This includes the generation of internal waves in synoptic-scale cyclones and the creation of unbalanced flows such as gap winds or Kelvin waves by orography.
(n) *The development and evolution of mesoscale frontal cyclones.* These are often missed by models, and their dynamics are not well understood.
(o) *The application of mesoscale models to forecasting "fire weather" conditions and the development of interactive models for prediction of actual fire development and movement.* This holds the potential for major improvements in fire prediction and control strategies.

(p) *The continued development of ensemble forecasting and data assimilation techniques.* This includes the use of adjoint methods and breeding techniques for quantitative assessment of the impact of proposed observing systems, including programmable observation platforms.

(q) *Research on advanced statistical techniques applied to model output and on optimal blends of numerical and statistical approaches.* The production of higher-order moments of model output statistics should aid probability forecasts, and statistics applied to areally integrated quantities such as basin-integrated precipitation are examples of potential advances.

In addition to the specific recommendations listed above, we wish to make the following general points.

- As forecasting science and technology enter the twenty-first century, it is possible and, in our view, desirable to target specific forecast needs for improvement, requiring much stronger links between atmospheric research priority setting and economic analysis. Our group strongly recommends the commission of a collateral economic analysis of the costs and benefits of weather forecasting improvements at the margin.
- Improved cooperation between NOAA, other agency and university researchers, and the private sector would serve to accelerate the pace of research leading to improved forecasts and warnings. We encourage more focused workshops aimed at bringing together members of the scientific and operational communities, and we recommend the establishment of an independent scientific committee for providing guidance to the National Weather Service in its efforts to improve models, observations, and data communications.

4. Applied and basic research issues and opportunities

In this section we provide an account of progress and opportunities on a number of research problems related to weather dynamics and forecasting. The recommendations and identifications of high-priority research opportunities listed in section 3 emerge from the more detailed descriptions given here.

a. Convective storms and associated phenomena

NEXRAD offers the unprecedented ability to detect and nowcast hazardous small-scale weather phenomena, such as microbursts and tornadic mesocyclones, almost anywhere in the United States. An important next step is to predict such phenomena at short range, but this will require advances in basic understanding, numerical techniques, data assimilation methods, and, very importantly, improved means of communicating large volumes of data to researchers.

There are several ways in which predictability of convective storms may conceivably be improved. First, at the level of the individual convective cell, there is the hope that some of the techniques used in large-scale numerical weather prediction may be applied to storm-scale prediction, particularly of high-intensity storms such as supercells. This will require the development of specialized data assimilation techniques to make full use of NEXRAD data as well as data from satellites, surface mesonets, etc. Increasing the lead time in severe weather warnings by just 15 minutes would be of great benefit.

We determined three major scientific issues and one operational problem that are limiting progress in understanding and prediction of convection. The first scientific issue is that of cloud microphysical processes, about which we make the following points.

- The dynamics of convective storms are strongly influenced by downdrafts, which are driven by condensate loading, evaporation, and melting, which in turn depend crucially on microphysics. Accurate formulations of convective downdrafts are also very important in simulating the interaction of convective ensembles with the larger-scale flow. Much better measurements of downdraft properties and microphysical processes are needed both for basic research and for validation of parameterizations.
- Sublimation, and advection, melting, and evaporation of cloud ice may play critical roles in the dynamics of mesoscale convective complexes and squall lines, as well as in individual convective cells.
- Hail formation is sensitive to cloud dynamics, but existing measurement techniques and instrumentation are probably inadequate. Much better measurements are again necessary.
- Convective storms may be triggered from benign clouds by cloud microphysical processes.
- Formation of ice may result in total transformation of the dynamics and structure of convective systems.
- The mixing of environmental air into updrafts strongly influences microphysical processes. Hence, entrainment within convective clouds must be better observed and understood.

Better understanding of cloud microphysical processes offers a real opportunity for improved short-term prediction. In all cases this will require better in situ measurements of cloud microphysical properties, particularly in ice clouds, and the application of remote sensing techniques such as polarimetric radar. Cloud microphysical processes cannot be fully understood without data from high-resolution multiple-Doppler radar with adequate time continuity. Only by mapping the three components of air velocity over the full extent of the storm (including the weakest echo regions and clear air) can the microphysical processes be placed in dynamical context, can entrainment processes be better documented, and can the interactions between downdrafts and microphysics be determined.

The second scientific issue, which arises in many contexts besides this one is the dependence of storm initiation and evolution on distributions of atmospheric water vapor. We stress that water vapor, unlike temperature, pressure, and wind, is not constrained by dynamics to vary slowly on the scale of the deformation radius; what evidence exists from aircraft and satellite observations shows profound small-scale structure. Many areas of meteorology will benefit from improved strategies for measuring atmospheric water vapor.

In the present context, the initiation and evolution of convection depends strongly on distributions of water vapor in the subcloud layer, and the dynamics of convective storms is sensitive to evaporation associated with turbulent entrainment and evaporation of falling precipitation, both of which are sensitive to environmental humidity. Storms often undergo strong transition when they experience changing environmental moisture. Yet existing means of characterizing the distribution of atmospheric water vapor are greatly inadequate. *First-order improvements in both the quality and quantity of atmospheric water vapor measurements will be necessary.* Some water vapor information can be obtained from satellite, but integral measures, such as precipitable water, are of limited utility.

The third area of opportunity is in boundary layer and land surface properties, particularly soil moisture. There is increasing evidence that the evolution of the planetary boundary layer over land is strongly influenced by the distribution of soil moisture and, to a lesser extent, temperature, but routine measurements of soil properties are seriously inadequate. *The panel believes that an enhanced research effort on land–atmosphere interaction involving increased collaboration between atmospheric scientists and hydrologists, together with first-order improvements in our ability to routinely characterize soil properties may lead to dramatic improvements in prediction of convective storm initiation.*

Numerical models of convective storms are now considered good enough to be used in observing system simulation experiments to explore the sensitivity of predictions to environmental conditions and to delineate observational requirements necessary to achieve specific predictions. We strongly encourage such experiments.

There is strong concern about the observational infrastructure for basic research on convective storms and about the exploitation of existing observational capabilities pertinent to convective storm research and prediction. We are particularly concerned about the decline of facilities to carry out high quality surface-based multiple-Doppler radar measurements and the capability of fielding portable automated surface observation systems for focused process studies. We also note that prediction of convective conditions and storm movement still depend strongly on rawinsonde observations, and for this and a large variety of other reasons, we support maintenance and enhancement of the rawinsonde network. We also point out that basic research on convective storms would profit from the availability of archived NEXRAD level 2 data.

b. Quantitative precipitation and flash flood forecasting

Although there have been marked improvements in recent years, quantitative precipitation forecasting (QPF) skill has not advanced as rapidly as prediction of other quantities such as temperature. Yet precipitation is often the most important element in the forecast, affecting aviation, agriculture, recreation, etc., and leads, in its most extreme form, to significant loss of life. The fundamental underlying reason for this lack of progress is that precipitation is often concentrated in convective cells or mesoscale bands or clusters, with small space and time scales.

Added to the problem of forecasting the spatial and temporal distributions of precipitation is the problem of forecasting the type of precipitation. Large sectors of the economy are affected by such phenomena as snow, freezing rain, hail, and aircraft icing, and the problem of forecasting the correct mix of precipitation types involves a correct accounting of combined dynamical and cloud physical processes.

An important subset of the QPF problem is flash flood forecasting. This not only involves high accuracy precipitation forecasts but also demands good hydrological models and data.

More careful thought needs to be directed toward suitable definitions of predictability. Elementary considerations of nonlinear dynamics suggests that the detailed evolution of individual convective cells may not be possible beyond a few convective overturning times—on the order of a few hours—except perhaps in special cases such as quasi-steady supercell convection. On the other hand, there is a great deal of evidence that ensembles of convective cells are in statistical equilibrium with mesoscale or synoptic-scale circulations, offering the hope that ensemble-average precipitation might have much longer predictability timescales. We believe that *basin-integrated precipitation forecasting would be of great value and might show much better skill* and that *higher-order statistical moments of precipitation distributions in space and time should be included in model output statistics (MOS)*.

The dynamics of mesoscale convective systems continues to be a major focus of research. A proper accounting of mesoscale convective complexes (MCCs) will involve improved understanding of processes on many scales, ranging from cloud microphysics, which influences anvil formation and downdraft dynamics, among other things, to potential vorticity dynamics involving short waves. The strong tendency for MCCs to form downstream of major mountain ranges suggests that the interaction of convection with potential vorticity anomalies, which may be created by flow over mountains, may be the key dynamical process in some cases of MCC formation.

The physics of lightning continues to be enigmatic. Basic research will lead to better and more complete use of lightning data as an indicator of storm dynamics and better forecasts of electrical activity.

Many of the same issues discussed in section 4a arose in connection with quantitative precipitation forecasting. It was stressed again that significant improvements in QPF might result from improved understanding and measurement of land–surface–atmosphere interaction and from greatly improved measurements of atmospheric water vapor. Radiation at the surface is also important in setting surface heat and water fluxes.

Much more can be done with NEXRAD data to extract information pertaining to precipitation types and storm structure. We therefore once again emphasize the desirability of facilitating access to data by researchers across the country. This access will allow researchers to determine statistics of radar echo structure and to relate them to various weather and climate regimes. This will be vital to assessing flash flood potential and to precipitation forecasting in general.

Improvements in QPF will also depend on much better parameterizations of cloud turbulent and microphysical processes, but these cannot be validated without much improved measurements within and just outside of clouds. Doppler and polarimetric radar measurements are crucial for relating these processes to the storm dynamics.

The need for well-designed and focused process studies remains acute. The increased use of mesoscale models demands better precipitation measurements to calibrate the models. The operational observing network–even after the NWS modernization–is not designed for providing ground truth for detailed microphysical, convective, and boundary layer processes contributing to precipitation. High-resolution multiple-Doppler radars, polarimetric radars, aircraft platforms, and other special instrumentation are needed to calibrate and validate model representations of precipitation processes, winds, and thermodynamics.

c. Atmospheric electricity

Lightning is still the single greatest weather-related cause of fatality in the United States and is also a major cause of fires and damage to electrical power distribution networks. But research in atmospheric electricity is only now starting to move into the mainstream of atmospheric research. Recent advances include the deployment of ground stations that detect cloud-to-ground discharges, the discovery of discharges extending from thunderstorms well up into the stratosphere and thermosphere, and a strict quantitative association between global lightning activity and the amplitude of the Schumann resonance. Atmospheric chemists have long been interested in global lightning activity as a source of NO_x, and measurements of the Schumann resonance at a single point yield a precise measure of globally integrated lightning: this is the *only* known means of measuring a significant globally integrated quantity at a single point and is thus a valuable monitor of global change.

We note that in spite of our current ability to detect cloud-to-ground lightning in very near real time, the National Weather Service does not yet issue specific lightning warnings. There is a real question about whether such warnings could reduce loss of life, given that many fatalities appear to be associated with isolated discharges. (Storms that are obviously electrically active tend to drive people to safety.) We believe that *it is time to perform an analysis of the type of electrical storms that kill people*. This would serve as a basis for a possible warning system.

Lightning is also of concern to aviation, the forest service, and electrical utilities. We need a better understanding of the potential benefits of lightning reports and alerts to these interests.

It also appears that lightning may serve as an indicator of dynamical transitions of certain storm systems. There is evidence of a hiatus between primarily negative and mostly positive cloud-to-ground lightning during the tornadic phase of severe thunderstorms, and recent research has turned up a striking correlation between hurricane intensification (but not intensity, per se) and eyewall lightning activity. Thus, access to real-time lightning data may prove to be of some assistance to severe storm forecasters. We also believe that advances in understanding the physics of lightning may result from analysis of a suitable combination of NEXRAD and ground discharge data. There is some concern about whether lightning data are being archived in a way that will be usable by researchers.

The physics of lightning continues to be enigmatic. Basic research will lead to better and more complete use of lightning data as an indicator of storm dynamics and better forecasts of electrical activity. Such research would benefit from improved cloud microphysical data as well as from radar and ground current detectors.

d. Orographic influences

The earth's terrain is known to cause or modify many types of atmospheric phenomena, including

topographically enhanced rain and rain shadows
torrential rain and flash floods
forest fire storms
shear lines controlling tornado formation
sheltering of leeside locations from strong winds
severe downslope and channel winds
gravity waves that remotely interact with larger-scale flows
cold-air damming
modification of fronts and cyclones
diurnal control of thunderstorms
valley pollution and long-range pollution transport
clear-air turbulence

There are three fundamental difficulties facing researchers and practitioners dealing with meteorology in mountainous areas.

1) *The continuous scales of the earth's topography.* Atmospheric scientists have traditionally divided the earth's terrain into two categories: large-scale mountains and small-scale roughness. The airflow disturbance generated by large-scale mountains has been explicitly analyzed, while the small-scale roughness has been parameterized. This division is physically inappropriate. The earth's orography actually has a continuum of scales with no natural dividing scale. Even as the resolution of numerical models has improved from 400 to 100 to 25 km and beyond, there has remained an artificial division between resolved and unresolved orographically generated phenomena. Furthermore, there is a partially resolved range of scales near the grid size of the numerical model. These partially resolved scales cannot be treated accurately by parameterization or by direct computation. Among other things, the internal waves excited by flow over topography often break in the troposphere and lower stratosphere, providing a net drag on the large-scale flow. Weather prediction models prove sensitive to how this is formulated, and it is clear that progress in basic research on flow over topography with a continuum of scales is necessary before internal wave breaking can be adequately represented in models.

   Over the next two decades, the issue of terrain scale will provide challenges for the theoretician and the numerical modeler. The improved models will begin to capture the horizontal topographic scales of 100 km down to 1 km that contain the gravity wave spectrum of the earth's atmosphere. The interaction of gravity waves with the larger scales of flow, scales that are already resolved, will bring new physical and numerical problems into our research and applications.
2) *Predictability and triggering.* The question of whether atmospheric phenomena are more or less predictable in mountainous terrain is now thought to have a double answer. On the one hand, terrain can anchor flow systems in both space and in time of day. On the other hand, mountain airflow patterns exhibit their own instability and triggering characteristics. Slight changes in ambient wind speed, wind direction, or wind shear can lead to sudden reorganization of the airflow and precipitation patterns. For this reason, ensemble forecasting and probabilistic methods will be useful in problems related to orographic influence.
3) *Model development and verification.* The numerical simulation of mountain-induced mesoscale phenomena has advanced enormously over the past two decades. The current interest in this subject and the predicted advances in computer technology suggest that this field will continue to move ahead. There remain, however, fundamental questions about numerical techniques and surface boundary conditions. The choice of vertical coordinate in numerical models will continue to be discussed, especially in relation to the horizontal diffusion of moisture and the applicability of small-scale parameterization schemes. The degree to which surface roughness and evapotranspiration should be included in mountain-flow models will require further examination.

While there is a growing confidence that high-resolution numerical models can accurately describe mesoscale orographic phenomena, there is less confidence in our ability to verify model output against real data. This problem has to do with the wide spectrum of topographic scales and the full four-dimensionality (space and time) of orographic airflow fields. The application of existing measurement technology and new observational tools will be required for verification of models in mountainous regions.

e. Extratropical cyclones and associated phenomena

There has been enormous improvement in numerical weather prediction of synoptic-scale systems over the last two decades. The application of improved models and numerical methods together with advanced data assimilation techniques has advanced numerical weather prediction to the point where many forecasts of synoptic-scale distributions are rapidly approaching reality (though there are still notable forecast "busts").

These improvements do not always yield accurate weather forecasts, however. Many "perfect" synoptic-scale forecasts do not resolve mesoscale phenomena that dramatically affect local weather conditions. A good example of this was the "Storm of the Century" in March of 1993, which affected the entire eastern third of the United States. Although the numerical weather forecasts of the synoptic-scale and mesoscale structure and evolution of this event were excellent, local predictions of precipitation were in some places badly off the mark, owing to a series of mesoscale internal gravity waves that strongly affected precipitation. Another important example is the development of mesoscale wave cyclones on frontal boundaries. These are seldom predicted well by numerical models and often cause radical changes in local conditions.

Better forecasts out to 3 days are critical for a number of important forecasting problems, such as snowfall, precipitation type, high winds, etc., and these will depend largely on better upstream observations and improvements in understanding and capturing mesoscale phenomena. But current numerical weather prediction techniques may not be uniformly applicable at the mesoscale. A big issue that must be faced is the initialization problem. Most current techniques are designed to filter out phenomena such as gravity waves and upright and slantwise convection—the very phenomena we wish to forecast on the mesoscale. *We believe that better weather forecasts on timescales less than 1 day will require much improved understanding of mesoscale phenomena such as gravity waves, slantwise convection, and frontal cyclones, together with advanced numerical weather prediction techniques, such as dynamic and diabatic initialization that preserves real internal waves and condensational heating.*

Forecasting at all timescales on the U.S. West Coast and beyond a day or two on the East Coast is seriously impaired by lack of usable data over the Pacific. But there is a paucity of research on the effects of these data voids. *We strongly recommend that OSSEs making full use of adjoint techniques be undertaken to estimate the effect of oceanic data voids on medium-range numerical weather prediction and similarly to estimate the influence of potential new data sources on numerical forecasts.* Another intriguing technique that should be explored is to use ensemble forecasting methods to make *a priori* estimates of the distribution of sensitivity to observational error, so that programmable observation platforms, such as unmanned aerial vehicles or programmed deployment of dropsondes from commercial aircraft, can be directed to focus on sensitive regions. Adaptive observational strategies may serve to help optimize observations in aid of numerical weather prediction.

While the dynamics associated with advection of potential vorticity are relatively well handled by forecast models, diabatic and frictional sources and sinks of potential vorticity are neither well understood nor represented in the models. *We need to better understand and simulate the processes that act as sources and sinks of atmospheric potential vorticity.* This may be of great importance in understanding and simulating the origin of short-wave troughs.

The tropopause is the seat of many important dynamical processes in the atmosphere. The potential vorticity gradients at the tropopause are major sources of the potential vorticity anomalies that interact with lower-tropospheric potential vorticity anomalies and surface temperature gradients to produce baroclinic cyclones and anticyclones. Dynamical processes at the tropopause also play a major role in the net exchange of mass, water, heat, and chemical trace species with the stratosphere. We need to pay more attention to the possible benefits of more data and higher model resolution near the tropopause.

f. Tropical cyclones

Although the worst natural catastrophes in U.S. history have been associated with tropical cyclones, the recent modernization of the National Weather Service has done very little to improve the observational basis for hurricane forecasting and warning. Though satellites have vastly improved our ability to *detect* tropical cyclones, accurate forecasts of their movement and intensity change continue to rely on relatively sparse in situ measurements, and research has shown conclusively that data from dropsondes launched from aircraft greatly improve forecast skill.

Here, perhaps more than anywhere, the use of hurricane forecast models in suitably designed observing system simulation experiments could delineate a superior mix of observations necessary for accurate forecasts of tropical cyclone movement. There can be little doubt, however, that *improved measurements of the synoptic environment of hurricanes offers perhaps the best opportunity for improved forecasts that would lead to reduced loss of life and property damage.* Platforms that should be considered in estimating an optimal mix of data sources include satellite-borne sea surface scatterometers, Special Sensor Microwave Imager, passive water vapor measurements, and active radar and Doppler lidar systems as well as in situ and dropwinsonde measurements from manned and unmanned aircraft.

A previously convened panel of hurricane experts concluded that in spite of some increase in the skill of hurricane motion forecasts, there appears to be virtually no skill in present forecasts of hurricane intensity change except as occurs during hurricane landfall or passage over cold water. The limiting intensity of hurricanes is fairly well known from theory and observations, and varies with sea surface temperature and atmospheric conditions, but it is also known that very few real storms reach this limiting intensity. Real storms are usually prevented from reaching their potential intensity by as yet poorly understood interactions with dynamical and thermodynamic processes in the surrounding atmosphere, and by interaction with the underlying ocean. Progress in understanding both of these interactions is severely hampered by lack of data in the upper ocean and overlying atmosphere. Hurricanes may also undergo intensity change owing to internal natural variability as manifest, for example, by concentric eyewall cycles. The dynamics of such phenomena are not completely understood.

Recent research with coupled air–sea models has shown a rather strong negative feedback on hurricane intensity by cooling of the sea surface owing to turbulent mixing of cold water through the seasonal thermocline. This effect depends on the depth of the unperturbed ocean mixed layer and the size, intensity, and speed of movement of the hurricane. But there is no near real-time information about ocean mixed-layer conditions in advance of hurricanes, so that incorporation of ocean interaction effects must now rely on often dubious climatological mixed-layer data. *Expendable bathythermograph measurements of the upper ocean before, during, and after hurricane passage are indispensable for understanding and validating models of hurricane–ocean interaction and may prove to be a highly desirable routine measurement of the hurricane environment in aid of real-time intensity forecasts.*

Observations of tropical cyclones leave little doubt that their intensity can be strongly affected by interactions with their atmospheric environment, but these are not even understood to the extent that empirical rules are regarded as working well in practice. Recent research shows that the application of dynamical principles based on potential vorticity to hurricane–environment interactions have great promise, but here again lack of data is a major impediment. The weight of the evidence points to the upper troposphere and perhaps the lower stratosphere as the locus of important interactions, but these altitudes are well above those accessible to the current fleet of hurricane research aircraft. *High-altitude (15–20 km) research aircraft are essential for making measurements that will allow understanding and forecasting of hurricane intensity and structure change by environmental interactions.*

The solution of the tropical cyclogenesis problem remains elusive, although there has been some progress in recent years. Though not considered a critical forecast problem, the basic understanding of genesis may lead to fundamental advances in the general understanding of the interaction between convection and large-scale circulations. Such advances would no doubt lead to improvements in physical parameterizations used in numerical weather forecast models.

Hurricanes are sustained by fluxes of sensible and latent heat from the ocean surface and are thus sensitive to the magnitude of these fluxes and, in particular, to the ratio of the enthalpy and momentum exchange coefficients. Little is known about the behavior of these coefficients at high wind speed, and field programs designed to understand this problem might lead to major advances that would aid future numerical forecast models in which tropical cyclones are explicit features.

Finally, landfallen tropical cyclones often cause major inland flooding and associated damage and loss of life. A recent example of this was the extensive flooding in central Georgia resulting from the stalled remnants of Tropical Storm Alberto in the summer of 1994. Very little is known about the dynamics of landfallen tropical storms, and so prediction remains problematic. *We recommend that enhanced research on the dynamics of landfallen tropical cyclones be encouraged.* Perhaps the research aircraft fleets of NOAA and the National Center for Atmospheric Research could be directed toward an investigation of landfallen tropical cyclones.

g. Oceanic and coastal weather and sea-state forecasting

Commercial shipping and fishing as well as recreational boating depend on accurate weather forecasts over the oceans. Each year, there is considerable loss of life in weather-related boating and shipping accidents, and weather also influences the distribution of fish. Coastlines are subject to damage from storm surges and from high winds and waves. But forecasting of the marine and coastal environment is strongly impeded by lack of data for initialization and validation of numerical weather forecasts. *Efforts should be made to improve observations of the maritime atmosphere.* Aircraft observations of storms while far out at sea and as they approach and interact with coastal topography will be needed to address important marine and coastal forecasting problems. Such observations will also benefit forecasts at all timescales in the western United States and forecasts beyond 2–3 days in the east. Here again we emphasize that satellite observations alone cannot at present fulfill the need for integrated observations.

Interaction of atmospheric flows with coastal topography is known to strongly influence coastal weather, but understanding of such interactions is relatively incomplete. An example is the interaction of the marine layer with topography in southern California. The details of this interaction determine whether the Los Angeles airport might close because of fog or whether an air pollution alert might be necessary.

Parameterization of surface fluxes of heat, water, momentum, and chemical trace species is far from a solved problem, particularly at high surface wind speeds, at which spray and bubble formation greatly affect the fluxes. Better representation of surface fluxes is necessary in the attack on a broad set of problems, ranging from fog and haze formation and dissipation to climate variability. *We see a need for specialized field experiments aimed at quantifying surface fluxes from the ocean at moderate and high wind speeds.*

Finally, the set of phenomena known generically at "polar lows" are of great concern to maritime interests but have received attention from researchers only relatively recently. These small, intense vortices are threats to shipping over much of the high-latitude oceans, especially in the Gulf of Alaska and the Labrador Sea. They are marginally resolvable by the present generation of fine-mesh forecast models but appear to be sensitive to representations of convection and surface fluxes. Better representations of these processes, together with improved observations of the maritime atmosphere, as discussed previously, will be necessary before accurate forecasts of polar lows will be possible.

h. Aviation forecasting

More than perhaps any other industry, aviation is dependent upon weather information. Indeed, without weather guidance, both commercial and general aviation activities would, by law, come to a halt. Historically, the weather services provided to aviation depended on *synoptic-scale* conceptual models and numerical guidance. At first glance, this may seem satisfactory since the single biggest expense for the aviation industry is fuel, and synoptic-scale forecasts normally are able to provide optimal route guidance for minimizing fuel consumption. However, it is well recognized that most aviators, especially those in general aviation, fly in a mesoscale world—a world composed of thunderstorms, fronts, clear-air turbulence, wind shear, and icing. Understandably, ameliorating the threat to safety created by these phenomena supersedes the issue of fuel cost. Thus, there is a clear need for accurate observations and forecasts of mesoscale phenomena.

Recent new observational capabilities coupled with advances in communication technology will soon put comprehensive information pertaining to the current state of the atmosphere into the cockpit. Such information, however, requires careful interpretation and conversion to a graphical format that facilitates rapid decision making. This is a challenging problem. However, by far the most difficult aspect of providing aviation guidance is how to generate accurate short-term (0–6 h) terminal and route forecasts of aviation-sensitive weather parameters such as ceiling, visibility, icing, wind, wind shear, and turbulence. It is envisioned that such forecasts must come from deterministic high-resolution ($\Delta x \leq 10$ km) numerical models and that the numerical model forecasts must then undergo statistical post processing to 1) compensate for model biases, 2) account for small-scale effects not resolvable or physically represented, and 3) convert the output into a probabilistic form amenable to optimizing the forecast system's efficiency and the air traffic safety.

Developing such a short-term forecast system will require advances in several major areas. Chief among these are the following.

- *Improved representations of the physical processes governing the distribution of water substance in numerical models.* Of special importance is that the models accurately represent microphysical processes, including their sensitivity to aerosols. These processes are of paramount importance in predicting precipitation type (e.g., rain, snow, sleet, freezing drizzle, etc.) and therefore visibility and icing. As stated earlier, research on these processes will require in situ aircraft data as well as radar observations.
- *Improved data analysis and four-dimensional data assimilation techniques.* It is essential that model initial conditions accurately describe the mesos-

cale state of the atmosphere. This is because the requirement for very short-term forecasts will not provide time for the models to "spin up" mesoscale structures and circulations. Consequently, four-dimensional data assimilation techniques that incorporate asynoptic and nonconventional data from a variety of remote and in situ sensors must be developed.

- *New statistical postprocessing techniques.* It is questionable whether or not deterministic models can predict such parameters as wind shear, ceiling, and visibility on the terminal scale. Thus, it is envisioned that statistically based postprocessing of deterministic-model forecasts may be necessary. These techniques will compensate for model biases and account for local effects not resolvable or physically represented in the deterministic models.
- *Prognostic statistical models.* For forecasts less than 1 h, statistical point models are likely to be competitive with postprocessed deterministic model output. Relatively little has been done in this area due to a lack of data documenting atmospheric conditions at *and in the near vicinity* of airport terminals. New remote sensing capabilities provide an exciting opportunity to develop more sophisticated and more accurate point models than ever before possible.

The items listed above outline the major elements of what could be considered a predictability experiment focusing on cloud-scale and mesoscale phenomena. Success in this very short term forecast arena likely hinges on both technological and conceptual advances. In particular, prediction would benefit immensely if we were able to find economical ways to continuously observe, with appropriate detail, the mass (temperature) and moisture fields. Research to develop such an observational capability should thus be strongly supported. Concomitant with these efforts is the necessity to advance our understanding of the microphysical processes that affect cloud-scale and mesoscale atmospheric structures. For example, a particularly challenging problem is to develop microphysical formulations that will permit a numerical model to explicitly predict precipitation type. Among other things, such formulations would have to simulate the absence and/or degree of the ice crystal sedimentation processes that occur during natural seeding of low-level cloud layers composed of supercooled droplets. This means that numerical models will be required to differentiate among ice crystal habits, growth rates, fall rates, etc. Understanding and accurate representation of these structures and processes will provide the platform from which explicit prediction of such aviation-sensitive parameters as icing and visibility would be possible.

i. Fire weather prediction

Forest fires create large losses of timber, property, and sometimes life throughout the western United States. Weather information and forecasts on a variety of timescales are a vital component of fire forecasting and management, both before and after fires have begun. Current forecast models provide reasonable guidance for predicting areas at risk for fires. If the forest is dry, the synoptic meteorological conditions favoring fire formation are low relative humidity, high temperature and winds, and thunderstorms creating cloud-to-ground lightning with little precipitation. Overall improvements in short-range forecast accuracy will also better identify areas of fire risk.

However, *the greatest opportunity for scientific progress in the next 10 years may lie in developing and refining computer models of fire spread in mountainous terrain, which can be used to develop an optimal control strategy for a given fire.* The meteorological component of such a model is a nonhydrostatic airflow model with a relatively fine horizontal resolution of less than 1 km, which is used to simulate the immediate vicinity of the fire. This model could be nested in a mesoscale model that provides appropriate larger-scale boundary conditions for the fire-affected area itself. Such models are already well developed. The scientific challenge is to integrate this with a fire-spread model that uses the meteorological conditions to predict local fire spread and fuel burn rate, the heat from which modifies the airflow around the fire. There have been promising pilot demonstrations of this idea in which many features of an observed fire were qualitatively simulated. Current fire-spread models are very primitive and may need to be improved to take full advantage of this modeling approach. With current computer technology, it is becoming possible to run such a model in near real time on a portable computer. This could aid in deciding where to deploy fire fighters or to drop fire retardant, and in minimizing the risk to personnel. Even in control burns deliberately set to ameliorate later fire hazards, lives are sometimes lost when a burn goes awry. Prior modeling can be a valuable adjunct to the heuristic rules and human judgment usually used in all these cases, due to the complexity of the airflow that often develops.

Larger-scale mesoscale models can also be used to predict the distribution of smoke plumes from fires. Occasionally, such smoke may be thick enough to significantly affect the temperatures by several degrees over a large area, mainly by cutting down on incoming solar radiation. Conceivably, this effect could be included in a forecast model.

j. Seasonal climate prediction

Seasonal forecasting occurs at the boundary between mostly deterministic forecasts at the 1–10-day timescale and climate prediction at timescales beyond several years. Although there has been some notable success in seasonal predictions of weather associated with specific phenomena such as El Niño, it is not yet clear to what extent it might be possible to make seasonal predictions that show some skill or what mix of deterministic and statistical methods should be brought to bear on the problem.

Several outstanding issues must be addressed in connection with seasonal prediction.

- How far forward can ensemble techniques be pushed? Traditional application of predictability theory has been based on exponentially growing instabilities in the atmosphere, but recent ideas on algebraic growth of atmospheric disturbances give some hope that quasi-deterministic techniques might be pushed further. Recent research demonstrates that the interval of validity of deterministic forecasts can be extended in practice to equal that of the best forecast in a series. There is also some hope that deterministic prediction of long waves might be successful even after the decay of short-wave predictability.
- Better understanding of low-frequency modes of the coupled atmosphere–ocean and atmosphere–land systems, and better measurements of the land surface component of the system, might lead to much improved seasonal predictions. Examples showing definite seasonal forecast skill include weather anomalies associated with El Niño and seasonal forecasts of Atlantic hurricane activity based on long-period fluctuations such as El Niño and the quasi-biennial oscillation as well as land surface conditions in sub-Saharan Africa. Sea ice and snow cover on land may also prove to be significant components of the coupled system on seasonal timescales.
- The influence of high-frequency but extreme events on low-frequency coupled atmosphere–ocean and atmosphere–land surface phenomena must be addressed. For example, a hurricane moving into a region experiencing drought may end the drought by changing the soil moisture distribution.
- The degree of seasonal predictability is likely to depend on the initial conditions. For some initial conditions the forecast may be relatively insensitive to small errors, while for others it is very sensitive. The degree of fragility needs to be quantified so that confidence bounds can be placed on seasonal forecasts.
- The sensitivity of the nonlinear global ocean–land–atmosphere system to small perturbations in boundary conditions is likely to be linear in the perturbations, and this sensitivity can be probed by observing the system's response to naturally occurring fluctuations. One way of proceeding is to use the fluctuation–dissipation relation to find the equivalent transfer function. A seasonal prediction model should be consistent with the observed fluctuation–dissipation relation.
- External influences on short-term climate change need to be better understood. These include small fluctuations in solar output and volcanic eruptions.
- Observing systems in aid of seasonal forecasting must be global. The extent to which soil properties can be observed adequately by satellite is uncertain at this time.

5. Summary

Basic and applied research in atmospheric science has yielded dramatic improvements in weather forecasts and warnings over the past several decades and is now poised to make even more spectacular advances. The main stumbling block to realizing significant progress in basic research and operational meteorology is the need for better measurements of the atmosphere, oceans, and land surface, as well as the need to better understand and delineate optimal combinations of measurement systems for specific forecast problems. Our nation has invested heavily in environmental satellites, and this investment has been paid back many times in improved understanding of the atmosphere and better warnings of hazards ranging from hurricanes to severe thunderstorms and tornadoes. But observing systems of equal or even greater importance and that cost far less have not improved or even deteriorated. Examples are the research Doppler radar facilities, global rawinsonde coverage, and high-altitude research aircraft. In some cases, we have just begun to realize the potential benefits of certain types of measurement, measurements, such as of soil moisture and of the detailed structure of the tropopause. We need to stand back and take a hard look at the costs and benefits of *all* existing and proposed measurement systems, from the perspective of basic scientific progress and societal need and with a blind eye toward the objectives and budgets of individual federal agencies.

If we elect to take a rational and well-thought-out view toward observations in support of basic research and operational objectives, there is every reason to believe that we shall realize large potential advances in understanding and prediction. A proper accounting of land surface physics and irreversible processes in the atmosphere may lead to large increases in the skill of seasonal forecasts. Better measurements of atmospheric water vapor and of cloud microphysical processes, particularly those involving ice, may allow us to solve a number of outstanding problems such as the prediction of the development and movement of mesoscale convective systems and the response of atmospheric water vapor and cloud cover to climate change. Advanced applications of adjoint techniques to numerical weather prediction may reveal, in near real time, those parts of the atmosphere that are particularly susceptible to initial error, allowing us to target such regions for observational scrutiny and thereby greatly reduce numerical forecast errors. Better in situ observations in the environment of hurricanes may lead to dramatically improved forecasts of the motion and intensity of these great hazards. These are but examples of what we can expect to achieve in the coming decades if we take the right approach now.

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