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Physical Oceanography for the Year 2000

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by
W. D. Nowlin, Jr.

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FOREWORD

This manuscript was prepared as a part of the Oceans 2000 exercise commenced by the Board on Ocean Science and Policy (BOSP). BOSP was reconstituted as the Ocean Studies Board in 1984. Although this overall project was not continued by the reconstituted Board, nonetheless this paper had already been submitted to the Ocean Studies Board, who felt that it was well worth publishing.

The ideas and views expressed herein are those of the author and do not necessarily represent those of the members of the Ocean Studies Board. Also, this paper should be considered as "Physical Oceanography in the Year 2000, as seen from 1985."

Physical Oceanography for the Year 2000

W. D. NOWLIN, JR.

Oceanography is poised on the doorstep of an exciting era. The next 10 to 20 years should bring revolutionary increases in our understanding of the ocean and its interrelationship with the atmosphere. Physical oceanography seems destined to lead the field into this era. This is fitting and expected because knowledge and understanding of the physical processes are prerequisites to increased understanding of the biological, chemical, and geological processes that they affect.

The technological and scientific developments of the last decades that have positioned oceanography on this threshold of new understanding include the following: (1) increased understanding of the nature of ocean circulation and the sampling procedures necessary to observe it; (2) instrumentation for making long time series studies at any depth or in any environmental regime; (3) numerical ocean models and the high-capacity computers to use them; (4) improved methods for measuring hydrographic variables, including chemical tracers; (5) improved knowledge of the processes responsible for water mass formation and for diapycnal mixing within the interior of the ocean; (6) satellite technology capable of observing both the primary atmospheric forcing and the oceanic response; and (7) the means for obtaining improved ocean measurements from drifters and vessels of opportunity.

The International Decade of Ocean Exploration of the 1970s encouraged and financed many of these developments and provided the milieu in which oceanographers, regardless of institutional affiliation, could work in teams to study oceanic phenomena that would not yield their secrets to individual scientific assaults.

The United States was the world leader in the International Decade of Ocean Exploration. In large measure due to that leadership and the farsighted federal bureaucrats of the 1960s who provided for the construction of the U.S. oceanographic research fleet, U.S. scientists remain at the forefront of oceanographic research. To maintain that position within the world scientific community, however, we must seize the opportunities offered by recent technical and scientific improvements. This requires identification of those programs that, because they are scientifically worthy and feasible, we wish to initiate, accelerate, or strengthen during the next 20 years. We must also identify those facilities, instruments, and human resources that are prerequisite to these programs.

Four thrusts in physical oceanographic research are identified here. Two are large in scale and are related to the climate problem: the World Ocean Circulation Experiment (WOCE) and the Interannual Variability of the Tropical Ocean and Global Atmosphere Experiment (TOGA). Another thrust is to better understand internal waves and microstructure, by which the ocean ultimately mixes mass and momentum. The fourth thrust focuses on the continental shelf and slope circulation, which is that part of the ocean most subject to development for recreation, food, and minerals.

In addition, it is important to continue emphasis on mesoscale process studies. It is well recognized by atmospheric climatologists that one of the primary mechanisms by which heat is transported poleward is the net statistical rectification of atmospheric storms. Mesoscale eddies in the ocean may play a similar role. Mesoscale features have received considerable study in MODE, POLYMODE, the Warm Core Ring Program, and in numerical modeling studies, but the processes and effects of these features are far from completely understood. While the mesoscale is an implicit part of WOCE, and global data sets describing ocean variability at the mesoscale will be accessible as a part of the experiment, mesoscale process studies per se are deserving of increased emphasis. Such processes are an important link between the large-scale, climatological pictures, and the small-scale dissipative processes.

Additional support for these thrust areas must not undermine the U.S. program of individual research support for general physical oceanography (though many scientists now supported by that program will likely reorient their efforts toward one of these thrusts). It is the undirected, general oceanography program that has traditionally spawned the technical and scientific developments on which broad new programs can be based.

The infrastructure needed to support the new thrusts, as well as the general research program, includes an ongoing program of ocean satellites, a program to replace and modernize the academic research fleet, continued funding for existing support groups (such as current meter, CTD, or chemistry), a technology development program focused on specific instrument needs, improved capability for numerical modeling and analyses of large data sets, a new look at oceanographic data archiving, and increased emphasis on training young scientists in the mathematical and computer skills needed in physical oceanography.

RECOMMENDED RESEARCH INITIATIVES

Climate-Related Studies

Many countries are participating in the planning of a global research program, the World Climate Research Programme (WCRP), to address aspects of climate variation. This program seeks the understanding that will allow us to answer questions with important economic and social implications. For example, what are the mechanisms, if any, by which the ocean influences year-to-year variations in the earth's climate? Does the ocean play a role in producing climate anomalies, such as droughts, floods, heat waves, or abnormal frosts? If it does, can we understand the processes whereby this occurs? Can we develop a capability for predicting climate change?

We know that the ocean plays a major role in determining the mean climate of the world. It is critical in controlling global patterns of precipitation and evaporation. The ocean absorbs energy from the sun and releases energy to the atmosphere at times and places distant from the point where the energy was received. The seasonal temperature range is reduced over land areas adjacent to the ocean because of the large thermal inertia of the ocean.

The poleward flux of heat of the oceans is of the same order of magnitude as that of the atmosphere, but the regional and temporal variations of the oceanic transport are not well known. To understand the mean climate state of the world, we must understand the ocean's role in establishing and maintaining the global heat balance. Unless oceanic variability can be defined in terms of its departure from some mean state, we may be unable to explain the influence of the ocean on global climate.

Models of the atmosphere with and without a moving ocean show that the ocean influences the mean atmospheric temperature distribution. The circulation of the ocean appears to affect climate variability on all scales. Ocean heat transport and storage processes have lifetimes that are long in comparison with those of the atmosphere. The ability to predict atmospheric variability from solely atmospheric models is now limited to 1 or 2 weeks. We know that significant variability occurs over much longer time scales, however. Ocean heat storage, transport, and transfer to the atmosphere are variable and may be the principal oceanic factors controlling these longer time scales of climate variability. Thus, long-range climate forecasting must take ocean processes into account.

Interannual climate variation is the largest nonseasonal climate variation, and it may be larger in some regions than annual or seasonal variations. Year-to-year variations in the earth's climate are of great economic importance. Unusual rainfall, drought, or heat waves can have significant impacts on agriculture. Oceanic thermal variations related to climate variability can affect marine fisheries. Thus, there are economic incentives for the development of a predictive ability.

Climate variations with scales of approximately a decade and that produce important economic effects are known to exist but are less well documented than those with an annual time scale. There is evidence that the ocean plays a role in decadal climate variability, and proposals for large-scale ocean experiments to understand decadal variability have been made. Long-period climate variability, with time scales between decades and centuries, is not well documented.

We wish to study climate variation through models of the coevolution of the ocean and atmosphere over time scales from months to geologic time. Oceanographers and meteorologists have proposed research programs to enhance our understanding of climate variability (Board on Ocean Science and Policy, 1983; Committee on Climatic Changes and the Oceans, 1983; Webster, 1984a). The two large-scale research programs are TOGA and WOCE. Parts of these programs are already under way, though plans for neither are complete. Much of the discussion of these programs in this report is taken from Webster (1984b).

Interannual Variability of the Tropical Oceans and Global Atmosphere Experiment

The Southern Oscillation is a large-scale exchange of atmospheric mass between the Eastern and Western Hemispheres in the tropics. It can be detected in sea level atmospheric pressure records as a see-saw cycle of high pressure in the South Pacific Ocean and low pressure in the Indian Ocean, alternating with the opposite conditions in the other phase of the cycle. It has a characteristic cycle length of several years and may occur at two- to ten-year intervals. It is the most obvious instance of interannual climate variability.

Associated with the Southern Oscillation are sea surface temperature anomalies in the Pacific, Indian, and Atlantic oceans. Changes in the equatorial current system and the heat content of the Pacific Ocean are particularly marked. The largest such oceanic anomaly yet documented is El Niño, a sea surface warming off South America which brings destruction to the fisheries off Peru and Ecuador. Plankton, fish, and birds, which are dependent on a chain of nutrients normally provided by the upwelling of cold seawater off the coast, die when El Niño's warming occurs. This has economic effects on the global markets for fish, poultry, and fertilizer. El Niño also brings heavy coastal rains that cause flooding and damage crops along the coast of South America.

Because of its strong signal and time scale, the Southern Oscillation has a significant effect on the climate. Though the Southern Oscillation occurs regularly, its occurrence has associated manifestations that normally persist for nearly two years from first to last appearance. This duration offers the potential to develop a predictive capability of perhaps a few months. The component phenomena of the Southern Oscillation normally occur at specific seasons of the year.

From the viewpoint of the United States, the correlations of the Southern Oscillation with North American climate anomalies present an intriguing challenge. Can the Southern Oscillation be used to predict wintertime climate anomalies over the United States a season in advance? Correlations between the Southern Oscillation and North American climate anomalies were first described in the 1930s by Sir Gilbert Walker. Since then there has been growing evidence of their reality. Wintertime temperature anomalies are correlated with earlier atmospheric pressure anomalies over the South Pacific and with sea surface temperature anomalies in the equatorial Pacific Ocean.

During the winter of 1982-1983, the strongest El Niño event ever observed took place. It was not forecast, nor was it generally recognized as an El Niño occurrence until it was well developed, and its subsequent evolution and duration were not anticipated. Considerable research has been stimulated by this event, which underlines the incomplete state of our understanding.

The link between the tropical Pacific Ocean and the atmosphere has attracted considerable scientific attention. Likewise, the Atlantic and Indian oceans provide interesting but different examples of large-scale interactions between the tropical ocean and the global atmosphere. Atlantic Ocean sea surface temperature anomalies correlate with droughts in Brazil. In the Indian Ocean, there is large seasonal change in response to the monsoon; for example, the Somali Current reverses seasonally. The Indian Ocean thus provides a unique location for studying some kinds of large-scale interaction between the ocean and the atmosphere. Indeed, the early evolution of the Southern Oscillation appears to occur in the atmospheric circulation over the southern Indian Ocean.

TOGA is principally an oceanographic program focused on the upper tropical ocean and overlying atmosphere. Its goals are to determine the nature of long-period fluctuations and their relationship to the global atmospheric circulation and to understand oceanographic and atmospheric processes that determine interannual variability. TOGA includes an oceanographic observation program aimed at describing the month-by-month variability of the upper layer of the tropical oceans over a 10-year period that began in 1985. One aim is to provide a 10-year data set of the atmosphere-ocean exchanges of momentum, heat, and moisture utilizing in situ and satellite-derived observations. The integration of these observational programs will be a modeling component aimed at developing tropical ocean models and assessing the sensitivity of the global atmosphere to forcing by the tropical ocean.

TOGA is an exciting research program. The Southern Oscillation is a strong climate signal that promises a predictive capability for climate variations in temperate latitudes. The economic benefits that could be derived from this capability would be great. A number of excellent scientists are enthusiastically working on the problem. Progress is being made in data analysis, field experiments, and theory. The first fragments of a comprehensive theoretical framework exist, and some linking physical mechanisms have been hypothesized. However, development of theory and improvement of the descriptive base must proceed in tandem to design a full TOGA experiment with assurance. The United States is supporting and should continue to support a major TOGA experiment in the Pacific Ocean. Complementary research activities in the Atlantic and Indian oceans also should be supported, although other nations may play the principal role there.

World Ocean Circulation Experiment

We must understand the global oceanic circulation to understand the ocean's role in maintaining the climate state and in influencing climate variability. Without this knowledge we are unlikely to be able to predict future climate variations.

WOCE is being planned to examine global ocean circulation and its relation to ocean climate processes. WOCE will be directed at describing the circulation of the ocean, defining the linking physical processes in the ocean-atmosphere climate system, and understanding the sensitivity of that system to forcing by changes in the atmosphere.

Recent oceanographic observations have revealed a number of processes that could be important to the ocean's role in climate variability: mesoscale eddies, tropical waves, isopycnal mixing, seasonal variation of the mixed layer, and mixing in the interior of the oceans. Computer models of the large-scale ocean circulation underline the importance of some of these. Thus, to observe and understand the circulation of the ocean, we need to describe the processes that are responsible in enough detail to model them.

A major obstacle to describing the ocean is the difficulty of obtaining measurements over long time scales and over great distances. Recent technical developments and new means of making measurements have made it feasible to consider carrying out a global experiment to understand the role of ocean circulation in climate. Orbiting satellites give promise of regular global measurements of sea surface temperature, surface currents, and wind stress on the sea surface. If these observations are combined with subsurface measurements, it should be possible to develop a description of the ocean that, for the first time, would begin to be as complete as our description of the atmosphere.

The WOCE is the oceanographic component of the study of long-term climate trends and sensitivity of the WCRP. It is being planned for the 1990s. At the international level, the formulation is being guided by a Scientific Steering Group under the auspices of the Committee on Climatic Changes and the Ocean and the Joint Scientific Committee of the WCRP. The goals for the international WOCE are (1) to collect the data necessary to develop and test ocean models useful for predicting climate change; (2) to determine the representativeness of the specific WOCE data sets for the long-term behavior of the ocean; and (3) to find methods for determining long-term changes in the ocean circulation. Since the focus is on the construction of ocean models and the collection of data sets necessary for demonstrating that these are useful models of the ocean circulation, the determination of what these data sets should be is the crux of the WOCE design.

The large-scale circulation of the ocean includes the mean and variability of the physical properties of the ocean: velocity, density, heat, and chemical constituents. In order to define a feasible experiment, priorities must be established among the many possible measurements.

Exchange with the atmosphere is important as a primary source or sink for most physical properties in the ocean. Atmosphere-ocean exchanges of momentum and density provide the primary dynamic forcing of the ocean. On the other hand, the forcing of the atmospheric general circulation by the ocean is primarily through exchanges of heat and moisture and, on longer time scales, radiatively active chemical constituents. Understanding the interaction of the ocean and the atmosphere is thus a significant part of WOCE.

Large scale means space scales ranging from eddies to basins and time scales from seasons to the length of the measurement period. Keep in mind, however, that some important smaller-scale processes may have to be included by either parameterization or direct measurement. Thus, any claim to understanding must include knowledge of the sources and sinks, or forcing and dissipation, and the redistribution by internal fluxes of these physical properties on these scales. Measurement techniques that encompass large scales are thus essential to WOCE.

WOCE will have a satellite observation program. This will include missions to measure sea surface topography by radar altimetry and surface wind stress by scatterometry. A global hydrographic and chemical survey by research vessels will provide data to define and constrain the global models. Chemical constituents, in addition to being components of the general circulation in their own right, have many close associations with the field of motion. (They can be tracers of ocean currents and mixing, constituents of ocean density, and quantities whose storage and exchange affect long-term changes in the atmosphere; thus, chemical constituents are considered to be physical properties of high priority for understanding the long-term circulation.) Also included in WOCE will be strategically located in situ instruments, for example, current meters and a global network of sea level tide gauge stations. Arrays of subsurface floats and surface drifters will contribute to the understanding of both surface currents and deep interior motions. Finally, measurements from volunteer observing ships will add necessary data on the thermal and velocity fields of the upper ocean and meteorological parameters for atmosphere-ocean exchanges.

The large-scale circulation is complex, involving many space and time scales. It is generally agreed that quantitative and accurate simulations and predictions can be achieved only with four-dimensional (three space dimensions and time) numerical models running on the fastest computers. Therefore, the development and evaluation of such models will be a fundamental integrating component of WOCE.

Planning for the U.S. component of WOCE is well under way. Within the U.S., the primary scientific objective is to understand the general circulation of the global ocean well enough to be able to model its present state and predict its evolution in relation to long-term changes in the atmosphere. The specific objectives are as follows:

1. to complete a basic description of the general circulation of the ocean;

2. to determine seasonal and interannual oceanic variability on a global scale and the effect of such variability on ocean measurement strategies and the coevolution with the atmosphere;

3. to improve the basic description of the surface boundary conditions and the exchanges of physical properties with the atmosphere, and to establish their uncertainties;

4. to determine the interbasin exchanges in the global ocean circulation;

5. to obtain quantitative estimates of the large-scale exchange of buoyancy and chemical constituents between the upper boundary layer and the ocean interior by adequately describing the properties of the surface layer, including its horizontal mass transport and divergence;

6. to determine oceanic heat transport and storage in relation to the heat budget of the earth;

7. to determine the large-scale transport capacity for ideal tracers in the ocean;

8. to determine the important processes and balances for the dynamics of the general circulation; and

9. to improve numerical models for the diagnosis, simulation, and prediction of the general circulation of the ocean.

The primary practical objective is to provide the scientific background for designing an observing system for long-term measurement of the large-scale circulation of the ocean as a basis for operational climate prediction. Specific objectives are the following: (1) to identify those parameters and indices that are essential for long-term measurement in a climate observing system, and (2) to develop strategies for a cost-effective operational system for describing the large-scale circulation, including approaches for merging satellite data with in situ information.

Internal Wave and Microstructure Studies

Models of water mass formation and modification in the world's oceans are very sensitive to the magnitude and nature of diapycnal mixing, despite its apparent slow mean rate in the ocean interior. A prime example of this is the abyssal circulation deduced from the classical Stommel-Arons theory. Variation of the dependence of the diapycnal mixing coefficient on the stratification can actually reverse the direction of the mean flow. Based mainly on theoretical conjectures, there appears to be significant regional and vertical variations in the types and rates of mixing. Some of the leading candidate mechanisms are (1) intermittent billow turbulence due to shear instability induced by internal waves; (2) intrusions from benthic boundary layers mixed by tides and breaking internal waves; (3) deep convective mixing in the wintertime mixed layers; (4) frequent billow turbulence in strong current shear; (5) double-diffusive convection, and (6) sloping convection across fronts. The difficulty is that there are few unambiguous observations of diapycnal mixing. The statistical approach through microstructure measurements is somewhat controversial. In some cases it has proved possible to diagnose the presence of mixing events from fine and microstructure measurements and even from large-scale (T and S) properties measured by CTDs. The following is a summary of emerging trends in internal wave and microstructure research adapted in part from D'Asaro and Müller (1984) and Caldwell (1983).

Internal waves are viewed as an important link in the overall oceanic energy cascade from the large scales of generation to the small scales of dissipation. Although the dominant sources and sinks for internal waves have not been identified, the following concept is generally accepted: Energy enters the internal wave field at large scales and cascades down to small scales by nonlinear wave-wave interactions. When the shear reaches a critical value, the waves break and generate small-scale turbulence and microstructure. At microscales, energy is dissipated by molecular processes. Research that led to this picture has been dominated by the concept of a universal internal wave spectrum, an idea introduced a decade and a half ago by Garrett and Munk (1972). This concept of a universal spectrum is being challenged, whereas the link between internal waves and microstructure has been substantiated.

Observed internal wave spectra usually fit the universal spectrum to within a factor of three for frequencies significantly above the inertial frequency and less so in the near-inertial band. The variations are likely the dynamic signatures of the sources, sinks, and internal transfers of the internal wave field. Numerous sources and sinks have been proposed. Observationally, however, no dominant generation or dissipation mechanism has been identified. Theoretical and observational evidence is emerging that the wind generates near-inertial frequency waves at large vertical scales and that internal waves and the mesoscale flow strongly interact. Classically, it has been assumed that the internal wave field dissipates its energy predominantly in the interior of the ocean, through small-scale turbulence. Calculations indicate, however, that the loss of internal wave energy at a sloping boundary might be substantial and could be the major energy sink of internal waves. Significant sinks of energy also may occur in critical layers when near-inertial waves become trapped within fronts or eddies. These losses would be concentrated at particular locations in the ocean and not spread uniformly throughout its volume.

Studies of the relationship between small-scale turbulence and the internal wave field require measurements of both the turbulence, using microstructure instruments, and the internal wave shear and density fields, using larger-scale measurements. Existing evidence suggests that the internal wave field is highly random and, therefore, many measurements are required. There is hope, however, that systematic relationships between fine-scale Richardson numbers and microscale dissipation rates can be established. Systems capable of repeated measurements of both microstructure and internal wave scales have only recently become available and some are limited to use in only the upper few hundred meters of the ocean.

Arguments developed in the last decade allow vertical diffusivity for mass to be estimated from microstructure parameters; however, a clear picture of the three-dimensional structure and evolution of these mixing events has not yet emerged. Generally, mixing is envisioned as being caused by Kelvin-Helmholtz billows, which have been extensively studied in the laboratory and observed at one location in the upper ocean. A variety of other stratified shear flow instabilities with structures distinct from Kelvin-Helmholtz billows, such as wave breaking and critical layer absorption, have been observed in the laboratory and may also occur in the ocean. Turbulence research in other fields has benefited greatly from flow visualization studies that aim to identify the dominant structures of the turbulent flow. Once the structures of a flow have been identified in this way, they usually can be seen in point measurements. It seems likely that similar studies using dye or high-frequency acoustics would likewise increase our understanding of oceanic turbulence.

One of the more enigmatic products of the direct microstructure measurement programs is the suggestion of a low level of turbulent mixing in the ocean. Effective vertical diffusivities estimated from microstructure data are of the order of 0.1 cm^2/s or less, a result consistent with predictions based on universal internal wave models. The puzzle arises when comparison is made with tracer data from the deep ocean which suggest that the effective diffusivity is 1 cm^2/s or larger (e.g., Munk, 1966). In fairness, it should be stressed that the direct microstructure observations have been collected in the upper 1 km of the ocean only, while the tracer study results reflect the abyssal water column.

In the absence of direct microstructure observations, several models have been put forward to account for the apparent higher level of mixing in the abyssal ocean than in the main thermocline. In one such model, it is argued that if the fine-scale shear field is limited by Richardson number, the effective diffusivity produced by shear mixing increases with weakening stratification. An effective diffusivity of the order of 1 cm^2/s may then be possible in the weakly stratified abyssal ocean. A second school of thought suggests that boundary mixing is the dominant mechanism for cross-isopycnal fluxes, either by breaking internal waves or boundary layer turbulence. Through a combination of strong vertical mixing at sloping boundaries and lateral mixing in the interior, it has been suggested that apparent vertical diffusivities of 1 cm^2/s are possible in the abyss. A challenge for the future is to collect direct microstructure and internal wave measurements in the deep ocean to test these ideas.

Internal wave research is presently undergoing a transition from a dominantly kinematic study of spectral slopes to a dominantly dynamic study of sources, sinks, and internal fluxes. The link between internal waves and oceanic turbulence is becoming more apparent, and the glimmers of a dynamic understanding are emerging. The parameterization of the internal wave and turbulent fluxes is a major goal of these studies. Further progress will come from simultaneous measurements of internal waves and microstructure and from a detailed comparison of experimental data with the results of numerical models. This work will require collaboration between workers in different areas of research.

Internal waves are only one candidate for generating small-scale mixing, and future attention can be expected to shift to several different convective mechanisms for mixing. One such topic will be the very intense mixing found in the deep, strongly cooled winter mixed layers. Warm core rings of the Gulf Stream have proven to be a good laboratory for studying this process. Recent work with rings has shown that the turbulence levels in actively cooled layers obey scaling laws similar to those of convective systems in the atmosphere, and that the heat and salt budgets of a cooling warm ring appear to require lateral mixing with surrounding waters. Hints of anticyclogenesis also are seen in association with convective cooling in rings. What are the dynamics of this process, and does it have implications for gyre-scale circulation? Since this process of deep convection is one that helps to establish the temperature and salinity of "mode" waters subducted into the thermocline, we must determine the relative roles of vertical entrainment and lateral mixing in our attempts to piece together a theory of the thermocline. In addition, much work will be required to discern the structure of convective plumes within these mixed layers. What are their morphologies and scales? Do they generate or interact with internal waves in the underlying thermocline? How is the horizontal momentum redistributed by these plumes?

Other convective systems under active investigation are found in thermohaline staircases in the main thermocline. These series of thin, high-gradient sheets separated by well-mixed layers some tens of meters thick are caused by double-diffusive mixing at the interfaces. This is a field which has been well served by laboratory experiments which, in many cases, demonstrated the phenomenon that was later to be discovered in the ocean. The salt finger process has attracted more interest because a great volume of the ocean is unstable in the fingering sense (both temperature and salinity decrease with depth). It appears that there are regions where the vertical exchange in the staircases is sufficiently strong to be the dominant term in the heat and salt budgets. In such areas (e.g., the Caribbean Sea and the Mediterranean outflow), the strong vertical mixing appears to cause rapid horizontal changes in water mass structure. However, these calculations are based on the extrapolation of laboratory flux laws down to the small salinity differences observed in the ocean. This could be a reasonable assumption, except for the fact that the ocean salt fingers must surely be affected by the ocean internal wave field. There is some evidence that the inertial wave field is much reduced in thermohaline staircases, but detailed measurements confirming this have yet to be made. In addition to field work in areas of strong fingering staircases, efforts can be expected to describe the weaker, more intermittent fingers found over great expanses of the gyres. Instrumentation is being developed that will be capable of detecting salt fingers on a more routine basis. As we increase our understanding of how fingers behave under distinct hydrographic conditions, we will be able to predict with some confidence the vertical exchange rates from simple T and S profiles alone.

The other form of double-diffusive instability arises when both T and S increase with depth. This is termed diffusive convection. Significant regions of the oceans near both poles are stratified in this sense, and the role of diffusive staircases in the heat budget of these areas may be significant. Most observations of diffusive steps have been made under the ice. The layers tend to be smaller than salt finger layers (a few meters versus a few tens of meters), and more modest vertical diffusivities are calculated (on the order of 1 cm^2/s versus 10 cm^2/s for the finger steps). Recent analysis suggests that it will be possible to model the mixing rate solely as a function of the ratio of the temperature and salinity gradients. Direct measurements of turbulence levels in such a system would be very valuable.

As our parameterizations of the double-diffusive processes become more accurate, we may be able to incorporate these processes into our numerical models. Simple one-dimensional models of the effects of salt fingers on the T-S relation already exist. Similar modeling efforts for the diffusive case can be expected. It should be possible to extend the models to two and three dimensions, and even up to the coarse vertical resolution of basin or global circulation models. Feedback from theoretical and numerical results will be an important check on the accuracy of suggested mixing rules.

We also can expect further progress to be made in laboratory experiments on the structure and behavior of double-diffusive systems. Many experiments have worked with more strongly driven convection than is found in the ocean, but there is no fundamental difficulty in achieving conditions that more closely match those in the ocean. The very sensitive microstructure instruments that have been developed for ocean work could be applied profitably to laboratory experiments.

Another double-diffusive phenomenon that recently has attracted much attention is the thermohaline intrusion. Laboratory studies have demonstrated the ability of double-diffusive mixing to drive horizontal intrusions from water mass fronts. A variety of fine and microstructure studies of intrusions have demonstrated some of the predicted effects. Double-diffusion interleaving appears to be an important cross-frontal mixing mechanism, providing the necessary link between the mesoscale and the microscale. While there are already reasonable models of the large-scale effects of thermohaline staircases, proper parameterization of the intrusion process will require a great deal of further work. It is clear that, in addition to the acquisition of regional statistics on the intensity of interleaving, a substantial effort must be made in exploring the dynamics of the intrusions themselves. This will include further laboratory, theoretical, and modeling work, as well as large, well-coordinated field experiments using the most advanced mapping and microstructure instruments. Such field experiments could be served well by the use of Lagrangian techniques such as neutrally buoyant floats and tracers, both natural and introduced.

In summary, internal gravity waves, direct convection, double diffusion, and small-scale turbulence are the motions by which the ocean mixes momentum and mass. The specific way in which this mixing is done has pronounced effects on geostrophic eddies, tracer distributions, and the general circulation. To understand these grander scales of motions, we must understand the smaller-scale mixing processes.

Continental Shelf and Slope Circulation

A regional approach is required to understand shelf and slope circulation regimes because the relative importance of the various physical processes that influence shelf circulation generally vary from region to region. Observations made with modern instrumentation during the last decade have resulted in much-improved descriptions of the dominant physical processes that influence circulation over continental shelves (e.g., Allen et al., 1983), but much research remains to be done to understand and model accurately the physical processes that govern these regional flow regimes. Continued analysis of existing data, development of improved theoretical and numerical models, and further field experiments will be required.

The circulation over the continental shelf and slope is forced primarily by surface wind stress, continental runoff, tides, large-scale currents, eddies, and wave motions from deep sea regimes which impinge on the continental margins. Continental shelves provide the sink for much of the ocean's tidal energy. In some regions tidal currents are sufficiently strong to cause local vertical mixing throughout the water column, to dominate sediment transport and the benthic community, and to drive vigorous residual currents. Continental runoff influences regional water properties and drives alongshore flows. Recent experimental programs have focused on the role of local wind stress, particularly the alongshore component, in driving energetic low-frequency currents over the shelf. Initial results indicate that the degree of coupling between local wind and local current may vary considerably on a regional and seasonal basis. The ability of the continental margin to act as a waveguide for coastally trapped waves has been verified observationally. These waves can be excited by several mechanisms and may propagate large distances away from the generation region.

The general subject of cross-shelf transports of, for example, mass, momentum, energy, salts, and nutrients remains poorly understood, though this is one of the most important aspects of shelf circulation. Cross-shelf surface Ekman transport driven by the alongshore wind stress is a fundamental element in dynamic models of the generation of wind-driven currents over the continental shelf, yet observations rarely show such a simple picture. Recent Eulerian and Lagrangian current observations obtained off California show a rich mesoscale structure in the flow field over the shelf, even during periods of strong alongshore winds and coastal upwelling (The CODE Group, 1983). The Lagrangian drifter observations suggest that the mesoscale circulation is much more energetic than previously imagined, and the estimated cross-shelf eddy diffusivity on the order of 10^7 cm^2/s indicates that cross-shelf eddy processes play a very important role in the cross-shelf flux of mass, heat, and nutrients (Davis, 1985). The coupling between flow on the outer shelf and the offshore current or eddy regime over the slope certainly needs additional study. Other important unknowns related transports are the effects of density fronts and topographic irregularities and the differences between such transports in the frictional surface layer and the underlying water column.

The alongshore variations on large (on the order of 100 km) scales of low-frequency, seasonal mean, and annual mean currents are likewise not well understood, but are of major importance. For some shelf regions, recent observations have yielded clear descriptions of the annual mean and seasonal variations in the alongshore currents. The driving mechanisms for even the mean circulation, however, have not been identified unambiguously. (For example, it is unclear to what extent the mean flow in the Middle Atlantic Bight is driven by an imposed offshore pressure gradient or an upstream source of shelf water.) Related to the dynamics of the mean and seasonal variation in alongshore flow are specific questions regarding the role in the alongshore momentum balance of the alongshore pressure gradient and bottom stress, two fundamental forces that are important in even the simplest theoretical and numerical models. Recent advances, both theoretical and experimental, in our understanding of the bottom boundary layer have changed our concept of the role of bottom friction. The generation of shelf currents by density differences caused by river runoff and the interaction of these density-driven currents with wind-driven currents are two related subjects that need additional study. In some cases, coastally trapped waves transport low-frequency kinetic energy along the margin. The effects of stratification, irregular topography, coastline orientation, mixing, and energy dissipation need further examination.

In summary, a few key questions were successfully pursued during the past decade: What is the physical mechanism of coastal upwelling? What role do continental shelf waves play in shelf circulation? By what mechanism is a steady-state pressure field maintained over an open continental shelf? What is the relative importance of wind, freshwater inflow, and coupling to deep water phenomena in driving shelf circulation? What is the role of a shelf-sea front in shelf-slope exchange processes? While none of these questions have been answered fully, the broad outlines of the answers are known as a result of the explosive growth of coastal oceanography. Follow-up work is essential in order to exploit fully the results of recent labors. Moreover, key questions remain to be answered; these have already prompted new research initiatives and are likely to guide the work of the near future.

It seems clear that the main emphasis in the next phase of coastal physical oceanographic research will be the interaction of shelf circulation with water movements on the upper continental slope. The most vigorous kind of interaction occurs when a western boundary current occupies the upper slope -- as is the case off the South Atlantic Bight and (intermittently) off the west Florida shelf. In the South Atlantic Bight much has already been learned. Generally speaking, the Gulf Stream imposes the pressure gradient which drives the outer shelf mean circulation. Important perturbations are caused by cyclonic eddies traveling along the Gulf Stream front. The properties of these eddies are known descriptively, but they are not dynamically understood. Similar eddies have been discovered in the

Gulf of Mexico. The circulation of the outer west Florida shelf is under the influence of these eddies, and it cannot be conceptualized without first understanding the cyclonic eddies. Large anticyclonic eddies (warm core rings) impinge on the continental slope in the Middle Atlantic Bight and in the western Gulf of Mexico. Their behavior over sharply sloping bottom and their interaction with shelf circulation are not understood. We do know that they generate strong currents ($\tilde{}$1 m/s), carry small cyclonic eddies on their perimeter (which on satellite pictures look like the arms of a spiral galaxy), and give rise to strong bursts of inertial oscillations in their neighborhood.

More generally, seas over continental slopes are likely to prove to be a unique oceanic environment because of the strong vorticity constraints that a steeply sloping seafloor imposes. One hypothesized consequence is the relative quiescence of the current over mid-slope (depth range, 1-2 km), except of course in the presence of a western boundary current. Quiescent mid-slope regions might well constitute an important worldwide sink for anthropogenic pollutants, as well as for the fine particulate matter that is naturally present in the ocean.

Improved descriptions of the foregoing phenomena and processes will likely require additional field measurements. To rationalize the descriptions, there are critical needs for the development of (1) improved theoretical and numerical models incorporating the different dynamic processes important in shelf circulation, and (2) objective methods to test model predictions against observations and to use observational data in numerical models. New models must include better representations of the surface and bottom boundary layers, and of frictional descriptive processes generally. Parametric studies with a coupled eddy-resolving basin model and shelf model, both of which have the necessary resolution pertinent to the dominant scales to be expected, should be attempted. No matter how clever one is with open boundary conditions, nothing new will be learned unless one actually allows for an adjoining basin with realistic dynamics. Such modeling studies coupled with related field experiments will allow an understanding of the interchange of momentum, vorticity, and other properties between the deeper-water region and the shelf. Some important questions are the following: Does the shelf act only in a passive sense of responding to excitation from the open sea, or can the shelf dynamics have a significant feedback on the open ocean dynamics? Does the shelf act as a sink for all oceanic wave phenomena (other than gravity waves) that are incident on it?

PREREQUISITES FOR RESEARCH

Several classes of prerequisites are essential for the success of the recommended research initiatives, as well as for the continued excellence of the general individual research program in physical oceanography. These prerequisites include new developments and replacement of the existing infrastructure to support research in this discipline.

Scientific Manpower

If we are to approach seriously the challenging problems of the next decades, we require new young scientists who are creative and farsighted. The number of subdisciplines of physical oceanography must be increased. Special emphasis must be placed on training in the application of mathematics to oceanography, especially in numerical modeling and the application of numerical and statistical techniques to oceanographic data analysis. Manpower is our shortest commodity in physical oceanography; thus, its acquisition should receive our highest priority.

Ocean Satellites

New technology providing global views of the oceans from space by satellite-borne instruments and new high-speed computers promises to allow major breakthroughs to be made in the description and understanding of the ocean. Research satellites have demonstrated a remarkable ability to measure several critical ocean parameters. Different types of radar measure sea surface topography, from which ocean currents and seabed shape can be deduced; ice shelf elevations, from which we can determine whether polar ice caps are melting; ocean wave heights, which are important for ship routing; sea surface winds, which are important for ocean circulation; and high-resolution sea ice imagery, which is important for supporting commercial operations in ice-infested waters. Detailed measurements of ocean color provide an estimate of biological productivity, which affects fish yield; infrared radiation from the ocean gives sea surface temperatures; and longer-wavelength radiation gives temperature, wind speed, rain rate, and sea ice concentrations under all weather conditions.

As an example of what can be achieved using satellite techniques, the global ocean wind field was acquired every three days by an instrument aboard the National Aeronautics and Space Administration's (NASA's) Seasat satellite in 1978. During the entire 100 days of this mission, as many individual measurements of wind speed and direction were collected as during the previous 100 years and more of shipborne observations.

To assess future impacts of the new satellite technology and set priorities for its effective use, a report was prepared by the Joint Oceanographic Institutions Inc. (JOI), Satellite Planning Committee (1984), which included representatives from 16 major U.S. ocean research institutions. The report stressed the immediacy of our nation's need for a better understanding of the oceans and showed how a significant part of that need can be met by satellite observing systems. Although existing weather satellites operated by the National Oceanic and Atmospheric Administration and the Department of Defense provide some routine surface observations, measurements of surface winds, ocean currents, biological productivity, and the gravity and magnetic fields of the earth are still needed. The JOI report (1984) presented a plan for acquiring these measurements in phases from space.

Four new satellite missions are proposed that will include an overlap of flight missions. The first is the Naval Remote Ocean

Sensing System (NROSS), which will carry a NASA scatterometer to measure winds and waves at the ocean surface. NROSS is awaiting approval for a 3-year mission, perhaps beginning in 1991. For the measurement of ocean surface topography (surface currents), the Ocean Topography Experiment (TOPEX/POSEIDON), which is joint with the French, has been approved and will have a 3- to 5-year mission to begin in 1991. NROSS and TOPEX/POSEIDON are crucial to the World Climate Research Programme; their timing must remain as scheduled to ensure their simultaneous performance with other complementary satelite and field activities of this international program.

NASA is currently investigating flight opportunities for ocean color measurements. In view of the importance of these measurements, the JOI report (1984) strongly recommended deployment of a satellite-borne Ocean Color Imager. For gravity and magnetic field measurements, the Geopotential Research Mission is proposed for a new start in 1988 and deployment in the 1990s.

Academic Fleet Replacement

First-class oceanographic research requires the availability of well-equipped research vessels. In large measure because of the quality of our academic research fleet and their associated instrumentation, the U.S. finished the decade of the 1970s in the forefront of ocean science. The preeminence of our academic research fleet, however, is now being challenged by research vessels from many other countries. Based on a 30-year age criterion, nine of the 20 vessels of our University-National Oceanographic Laboratory System (UNOLS) fleet should be replaced before the year 2000. Though age alone is not a sufficient criterion, replacements are warranted because of changes in the direction of science, technological obsolescence, operational cost escalation of older vessels, and needs for special-purpose vessels. To establish a prudent fiscal expenditure policy and to benefit from the experience gained from the operation of existing vessels, the U.S. should have a continuing program to improve the capability of our research fleet.

The UNOLS has commissioned a study leading to a coordinated plan for the replacement of the aging UNOLS fleet and the construction of new ships. This study consists of the following elements: review and verification of requirements for research vessels; status of current ships and the identification of needed capabilities and priorities; report of critical areas of ship replacement and specifications for priority replacements; conceptual design studies of several selected alternative platforms; community-wide review and discussion of the foregoing; development of a replacement plan incorporating desired fleet mix to meet requirements, priorities, time frame, and costs of new construction; and preliminary design of the vessel types needed to implement the early phase(s) of the replacement plan.

Multiagency agreement on, and backing of such a plan is essential to the continued health of oceanographic research in the U.S.

Support for Existing Technologies

Within the U.S. there are presently a number of groups providing specialized oceanographic services that help keep us in the forefront of research. These include capabilities in float and drifter technology, data transmission, analytical techniques, CTD, chemical sampling, and data analysis. The capabilities are essential and very difficult to develop; they require continuing funding adequate for not only ongoing operation, but the incorporation of technological developments as well.

Development of Sampling Technology

Though undirected and often unsupported, the development of technology for ocean research has provided physical oceanographers with an array of innovative, useful tools. Vector current meters; long-lived acoustic release systems; floats for remote tracking of currents near sound channel depths; expendable profilers of vertical shear of ocean current; systems for profiling salinity, temperature, and density from a conducting cable while obtaining water samples; and satellite-tracked ocean surface drifters are examples. Nevertheless, the technological developments that will be needed to support the new research thrusts of the 1990s and beyond are foreseen, and many either are not under way or are progressing too slowly to make their maximum contributions in terms of cost efficiency or under sampling capability.

New systems of subsurface drifters might internally record their positions for very long periods and then surface to report these positions by satellite. Rugged, all-environment communication links are needed between subsurface moored instrument arrays and satellites in order to monitor the status and records of ocean sensors.

Semi-intelligent vertical profilers are needed that operate without a connection to a surface vessel, alleviating the need for special winches and rapidly reducing ship time. These are only a few examples to illustrate the capabilities that are presently needed and within reach but that are yet undeveloped. A long-term program of technological development must be fostered.

Numerical Modeling and Data Analysis

Numerical ocean models presently are limited by the capability of available computers and by the numbers of trained numerical ocean modelers. Basin-wide and global numerical models with eddy-resolving capabilities must be implemented to take full advantage of the global data sets anticipated. Global-scale coupled atmosphere-ocean models must be realistically formulated for climate studies. A national ocean modeling initiative must have three thrusts: the development and availability of class seven computers and their descendants, academic access to present and future supercomputer facilities, and training of additional research scientists to develop and make effective use of vector codes.

Oceanographic modelers need to develop the capability of merging real data and dynamic principles into a meaningful tool, perhaps by using the objective analysis techniques that diagnostic meteorologists have employed and refined. One of the important steps in numerical weather prediction on long time scales is proper initialization, particularly if primitive equation models are employed. Oceanographers must be even more clever in initializing their models since the data not only contain gaps, are not simultaneous, and do not have the advantage of absolute pressure measurements, but they also consist of a variety of different types of data, for example, hydrographic data, Lagrangian drifter data, surface sensed temperatures, and chemical tracers. (It is hoped that satellite altimetry data soon will be useful for oceanographic signals of barotropic modes, given an accurate geoid and accurate knowledge of oceanic tides.) In any event, there is a real need for an objective method of blending all sources of data into a meaningful whole.

The ocean satellite data expected in the 1990s will require new concepts of data management. Requirements of the Interannual Variability of the Tropical Oceans and Global Atmosphere Experiment (TOGA) and the World Ocean Circulation Experiment (WOCE) for data reduction, dissemination, analysis, and archiving are now being addressed by the working groups planning these experiments. It seems likely that completely new data transmission links and distributed data-processing centers will be the effective solution.

Ocean Data Archiving

Dissatisfaction with the present system of archiving oceanographic data in the U.S. has been expressed by the ocean science community for at least a decade. However, the system capability has improved little, if at all, relative to the increase in volume and diversity of data. The entire archival system of ocean data within the U.S. requires a new look and drastic modification to accommodate even the present types and levels of data collected, much less those anticipated in the next decades. Consideration is needed of how to couple archived data, new *in situ* data, and satellite-derived data to processing and analysis centers.

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