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AN OCEAN CLIMATE RESEARCH STRATEGY

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PREFACE

This report is my view of where ocean climate research should be heading based on the situation in 1983. It was prepared during a period spent as a senior fellow with the National Research Council. From literature reviews, meetings, and discussions, I have tried to put the various activities in ocean climate research together into a cohesive whole. Based on this, I present a strategy, directed to the National Science Foundation (NSF), for ocean research that will support the development of our ability to predict climate fluctuations. I do not identify specific research that NSF might support. Rather, I provide guidelines that I hope will aid NSF and the research community in setting priorities and establishing procedures.

When I started my review, I was critical of ocean climate research programs and plans: it seemed to me that there were too many plans, that they were too poorly defined, and that the hard choices had not been made. I finish this study as an enthusiast for the potential benefits of an ocean climate research program. I hope I have retained a critical distance in the process: I have tried to set forth the pros and cons straightforwardly.

This report is directed to NSF managers, to climate program managers in other federal agencies, and to university scientists involved in climate research. I hope to present the consensus on ocean climate research and to give guidelines on strategy in such a way that all involved will recognize the common problems that must be dealt with. Those of us who seek to understand climate may have different ideas about where to begin to seek understanding, but we need to share ocean climate research objectives. We may disagree on which projects should be supported, but we need to agree on the criteria for support.

Many reports have been written about research on the role of the ocean in climate--perhaps too many. A clear direction is hard to find, and priorities have not been set. One report, however, has been a great help to me: Ocean Research for Understanding Climatic Variation: Priorities and Goals for the 1980's, prepared by a committee of the Ocean Sciences Board of the National Research Council under

1 SUMMARY

This report is intended to guide the National Science Foundation (NSF) in its lead role in planning and administering ocean climate research within the National Climate Program. Several national and international oceanographic research programs have been proposed to aid in developing our ability to predict natural climate fluctuations. These programs are summarized in this report, and a recommendation on a strategy for NSF in regard to each is presented.

The ocean plays a central role in the variability of global climate (see Chapter 2): it is the principal global reservoir of heat and appears to be as important as the atmosphere in the global transport and redistribution of heat. The principal climatic effects on man are produced by the atmosphere, and it is thus atmospheric climatic variations that we seek to predict. However, in order to do this we need to describe and understand the variability of the ocean, its mean state, and its large-scale interactions with the atmosphere.

The Interannual Variability of the Tropical Ocean and the Global Atmosphere Program (TOGA), a proposed large-scale ocean-atmosphere experiment (see Chapter 3), would be an exciting opportunity to study a strong natural climate signal, involving, among other things, the Southern Oscillation, the El Niño phenomenon, and wintertime climate anomalies over North America. The economic benefits of predicting these climate anomalies could be great. NSF should support TOGA planning in anticipation of U.S. participation in a large-scale international experiment.

The present state of the ocean itself, as well as its variability, must be understood if we are to understand climate. Reliable global observations of the state of the ocean and the time changes of that state are needed. Recent technical developments have made it feasible to consider carrying out a World Ocean Circulation Experiment (WOCE; see Chapter 4) to understand the role of ocean circulation in climate. Planning for WOCE is still in an early stage. NSF should support research to aid in that planning, though the extent of NSF commitment to WOCE should be determined following a fuller definition of the program.

Because the transport and storage of heat by the ocean is central to all theories of the role of the ocean in climate, a number of programs have been proposed to study ocean heat flux (see Chapter 5). One, the Cage experiment, was suggested to examine the long-term mean, the annual cycle, and the interannual variability of the North Atlantic heat flux and to establish the validity of the measurement techniques. After considerable scientific review, it has been decided to propose Cage and other heat flux experiments as parts of WOCE and TOGA rather than as stand-alone programs.

Monitoring of the ocean and atmosphere, that is, the collection of regular observations for long periods of time, will be necessary for an ultimate understanding of climate variability (see Chapter 6). There is so far no U.S. commitment to establishing long-term ocean climate monitoring. NSF-supported ocean climate research will need the base of observation that can be provided by monitoring, and NSF should work with other agencies to achieve it. The next steps in developing ocean climate monitoring can be simple ones, such as sea-level observations, exploratory time series, and phantom weather ships.

Ocean climate research that falls outside the accepted national and international programs is reviewed (see Chapter 7). NSF should remain flexible enough to support good climate research even when it is outside the "approved" framework. Examples of programs that might be considered are given. The United States can benefit by cooperating with other countries in their climate research programs.

Programmatic issues with federal agencies other than NSF and with other countries are reviewed (see Chapter 8). On the national level, a favorable consensus from U.S. oceanographers and a commitment by capable scientists to participate are needed if these large ocean climate research programs are to go forward. Recommendations for NSF action in its lead federal role are proposed.

The report concludes (see Chapter 9) with a summary of the recommendations contained herein for NSF strategy and action.

2 INTRODUCTION

The focus of this report is ocean research that will lead to or support our ability to predict year-to-year natural variations in climate one season in advance. The report summarizes what we already know about the role of the ocean in climate variability. There is a list of references that includes many key papers.

Against this background a discussion is presented on what ocean research should be done to help develop an ability to predict climate variations, particularly in agricultural regions where the economic benefit of such predictions would be great.

The variability of climate on scales longer than the interannual, and long-term climate change are briefly considered, but are not the principal concern of this study.

Oceanographers and meteorologists have proposed a number of national and international research programs designed to enhance our understanding of climate. Some parts of these programs are already under way. This report briefly describes and then evaluates the proposals for two large-scale oceanographic research programs: the Interannual Variability of the Tropical Ocean and the Global Atmosphere (TOGA) and the World Ocean Circulation Experiment (WOCE). In addition, planning for large-scale ocean heat flux experiments is reviewed. (A fuller description of these programs can be found in the references.) This report offers advice on strategies for each for NSF use.

In a report of this kind, where general strategies are considered, it is probably unwise to attempt to spell out specific research programs that should be supported. Nevertheless, the conflicting needs for support must be resolved; that is, we need to set priorities. This is usually difficult because all the proposed programs appear to be exciting. But if we try to support everything, we risk failure by spreading the resources too thin. In order to begin to resolve this problem, principles (or a strategy) for research will be established herein whereby priorities can be set.

This report also looks at possible ocean climate research that is not explicitly included in the existing proposals for large-scale experiments. Has something important been left out? Not all good scientific ideas fit into the framework that has been established. Some scientists have not succeeded in selling their ideas about ocean climate research to their colleagues. Others prefer to work alone. The need to support some kinds of ocean climate research that may not have the blessing of the "establishment" is therefore considered.

The proposals for ocean climate research seem to exceed the resources available. To go forward now with the programs proposed, significant new monies would probably have to be found, or ocean research might swallow up support that now is going to other disciplines.

Some of the large-scale ocean climate research programs being proposed lack a full definition of objectives and tasks. Because of this, the resources needed to accomplish them have not yet been determined. This makes the setting of priorities even more difficult. Where priorities are indicated here, they should be open to modification as changes in objectives or resource levels occur.

The principal objective of the ocean climate research considered here is the development of a predictive capability for forecasting interannual seasonal climate variations. The research involved falls into three categories:

1. Research is needed to obtain a description of the ocean-atmosphere system. We need to improve our knowledge of ocean climatology, and we need to define large-scale ocean-atmosphere correlations.

2. Research is needed to understand ocean processes that relate to climate variability. For example, what is the role of the ocean in heat transport and what are its variations? Can we construct models that simulate the important physical processes?

3. Tests of our predictive capability are needed. Models will be developed whose purpose is to predict climatic variations. Validating and testing these models will be important activities. Thus, in planning for ocean climate research, we should look for occasions to develop and test our predictive skill.

The large-scale ocean experiments discussed here do not fit neatly into these categories. Each combines description, process studies, and models and has some elements related to prediction. Nevertheless, in assessing these programs as part of setting priorities, we should ask how each contributes to improving the description of the ocean-atmosphere system, to defining the natural processes that affect climate variability, and to improving predictive ability.

Another approach to setting priorities is to ask what factors are impeding progress. Are certain key elements acting as obstacles, such as the lack of an observing technology, or a missing link in the chain of knowledge about interacting natural processes? If a piece of knowledge is missing, sometimes it is difficult to know exactly what it is that holds us back. In other cases, there may be general agreement that a certain technique (such as an altimetric satellite) is needed before we can proceed. Those factors that appear to be critical if we are to advance will here be identified.

The National Science Foundation has been identified as the lead federal agency for the Ocean Heat Transport and Storage "principal thrust" of the National Climate Program. The National Climate Program Plan (U.S. NCPO, 1980) designates six principal thrusts, two of which deal with research. A principal thrust has high priority, is of major importance to the goals of the program, and promises significant opportunity for progress. This report considers possible ocean climate research roles of the concerned federal agencies and indicates where work supported by each agency might fit into the context of the total national ocean climate research program.

Finally, the report considers the international setting. Many countries are participating in the planning of a global research program that addresses all aspects of the problem of climate: the World Climate Research Program (WCRP). The WCRP plans for ocean climate research components are in varying states of development. This report reviews these plans and advises on U.S. involvement, that is, how American ocean climate research activities can fit within the world program, can aid it, and can benefit from it.

3 THE ROLE OF THE OCEAN IN CLIMATE

This report is principally concerned with understanding the role of the ocean in interannual climate variability. That is, 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, and abnormal frosts? If it does, can we understand the processes whereby this occurs? Can we develop a capability for predicting climate change?

This chapter provides a review of what is known of the role of the ocean in the earth's climate system. The ocean influences the mean climate state of the earth. There is growing evidence that the ocean has, as well, a major influence on climate variations.

This review will not be exhaustive, since many existing documents and papers present the scientific background. This chapter simply sets the stage for the rest of the report with a review of the scientific basis for studies of climate variability and the role of the ocean. The list of references will provide guidance to the extensive literature.

Terms such as "climate state" and "climate variability" are used throughout this report. These terms have been defined by the U.S. Committee for the Global Atmospheric Research Program (1975) and are reproduced here in Appendix B.

MEAN CLIMATE STATE

The ocean plays a major role in determining the mean climate state of the world. It covers 70 percent of the surface of the earth and is a source of moisture for the atmosphere. As such, it is critical in controlling global patterns of precipitation and evaporation.

The heat capacity of the ocean is huge. The upper 3 m of the ocean can contain as much heat as the entire atmosphere. 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 heat inertia of the ocean.

The poleward flux of heat in the ocean is of the same order of magnitude as that in the atmosphere, but the processes of the oceanic transport are not well understood. Any attempt to understand the mean climate state of the world must take account of the role of the ocean in establishing and maintaining the global heat balance.

The mean climate state of the ocean is not now well understood. Unless we can define oceanic variability in terms of its departure from some mean state, we may be unable to explain the role of the ocean in maintaining or modifying global climate.

CLIMATE VARIABILITY

Both ocean and atmosphere show climate variability on time scales of months to centuries. The annual or seasonal cycle is generally large, but the nonseasonal variability can exceed the seasonal in some regions, particularly in some oceanic areas.

Ocean heat storage, transport, and transfer to the atmosphere are variable. It may be that such variations are the principal oceanic factor controlling climate variability. Thus an understanding of the uptake, transport, storage, and release of heat by the ocean may lead to an understanding of global climate variations. This report reviews plans for large-scale ocean heat flux experiments, such as Cage.

Models of the atmosphere with and without a moving ocean show that oceanic effects influence the mean atmospheric temperature distribution (Manabe and Bryan, 1969; Spelman and Manabe, 1983). The circulation of the ocean appears to affect climate variability on all scales. Thus there are proposals to study the general circulation of the ocean and the climate state of the ocean. In this report the proposal for a World Ocean Circulation Experiment (WOCE) is reviewed.

Ocean heat transport and storage processes have lifetimes that are long in comparison with those of atmospheric processes. Atmospheric predictability may inherently be limited to a week or two. But the chain of events involved with the Southern Oscillation, a global-scale atmospheric and oceanic climatic anomaly, has a duration of about 18 months. Though the ocean and atmosphere interact, this long time scale seems to be dominated by the high thermal and mechanical inertia of the ocean. Thus long-range climate forecasting probably must take ocean processes into account.

The largest nonseasonal variable climate signal is the interannual, which, particularly in some tropical regions, may be larger than the annual or seasonal signals. Year-to-year variations in the earth's climate are of great economic importance. Effects such as unusual rainfall, drought, or heat waves can have significant agricultural impacts. Oceanic thermal variations related to climate variability can affect marine fisheries. Thus, there are economic incentives to seek to develop a predictive ability.

Variations in climatic conditions can affect marine fisheries. Changes in temperature, light, and ocean currents can affect such factors as the reproduction, feeding, and location of fish in the sea. Regions of advection, upwelling, and convergence can be correlated with the abundance, or lack thereof, of fish.

The subject of climate and its impact on the living resources of the sea is scientifically and economically interesting and deserves further study. It is, however, not directly within the scope of this study.

As has already been mentioned, some interannual climate signals are particularly strong in the tropical ocean. The strongest of these signals, known best as the Southern Oscillation and discussed in the next chapter, is an interannual atmospheric and oceanic signal that has global dimensions (Wright, 1978). Sea-surface temperature anomalies, changing wind patterns, excessive precipitation, and continental cold spells are among the manifestations that sometimes are associated with the Southern Oscillation. In this report, plans are reviewed for a study of this phenomenon and the interaction of the tropical ocean and the global atmosphere (the TOGA study).

Climate variations having scales of approximately a decade are known to exist but are less well documented than those of annual time scale. Their economic effects are also less well documented, but they can be important. The "dust-bowl years" were possibly a result of decadal climate variations.

There is evidence that the ocean plays a role in decadal climate variability, and some proposals for large-scale ocean experiments to understand decadal variability have been made. The consensus seems to be that to develop a predictive capability, research on interannual climatic phenomena should have first priority. At this time, no plans for ocean studies explicitly directed to decadal climate scales have emerged in the World Climate Research Program (WCRP).

Long-period climate variability, having time scales between decades and centuries, is not well documented (Hecht, 1981). The study of such phenomena obviously requires a long-term commitment. The economic impact is uncertain. It is even unclear how we would today make use of the knowledge of long-term climatic variation if it were available.

CLIMATE CHANGE

The paleoclimatic record shows evidence of climate change over the millennia. In addition to these natural effects, the climate may be changing as a result of man's activities. Such inadvertent modification of climate has received considerable public attention. In particular, the climatic impact of changing carbon dioxide levels has been the subject of extensive discussion.

The issue of possible changes in the earth's climate as a result of man-induced changes in the level of carbon dioxide has been and is being reviewed elsewhere (Climate Board, 1982; Carbon Dioxide Assessment Committee, 1983) and is outside the scope of this study. A major scientific problem is that of distinguishing anthropogenic climate changes from those that would have happened naturally.

The ocean must be considered in any examination of the carbon dioxide question. The large thermal inertia of the ocean will affect the response of the atmosphere to warming. Thus the actual warming at any time could be less than that calculated on the assumption that thermal equilibrium is quickly reached. The ocean will, as well, absorb carbon dioxide and provides a long-term geochemical buffering of the atmospheric carbon dioxide. Ocean warming could melt polar ice and trigger feedback effects that in turn could modify the atmospheric thermal response. The conclusion of the Climate Board (1982) is that "the role of the ocean in time-dependent climatic response deserves special attention in future modeling studies, stressing the regional nature of oceanic thermal inertia and atmospheric energy transfer mechanisms."

Carbon dioxide is not the only radiatively important trace substance that can affect the earth's radiation, and hence heat, balance (Chamberlain et al., 1981). Substances such as ozone, oxides of nitrogen, and water vapor can have an effect similar to that of carbon dioxide. The total influence of all these other substances could be as great as that of carbon dioxide, though the requisite studies to determine the actual effects have yet to be made. Again, the ocean plays a role in global storage and transport.

Sediments and remains of living organisms on the sea floor can give us a record of past climates. The study of paleo-oceanography is an important component of the study of climate variations on scales of centuries or longer (Geophysics Study Committee, 1982). These investigations, however, are generally not directly related to the oceanic processes involved in climate variability at the scales we are considering here. (An interesting exception is the use of radiocarbon measurements of corals in the Galapagos Islands to construct a time series of El Niño occurrences (Druffel, 1981).) The knowledge we gain about natural processes at such long time scales (millennia) may be more closely related to solar phenomena. Though important to an overall understanding of the global climate, this subject is not within the scope of this study.

4 THE INTERANNUAL VARIABILITY OF THE TROPICAL OCEAN AND THE GLOBAL ATMOSPHERE

The Southern Oscillation, a family of naturally occurring, interacting phenomena in the ocean and atmosphere that produces climate anomalies, provides us with the opportunity to carry out experiments in interannual climate forecasting and to develop a climate prediction capability. The phenomena that make up the Southern Oscillation family (e.g., anomalies of sea-surface temperature, atmospheric pressure, precipitation, and temperature) are found in the tropical ocean and global atmosphere. In addition, the Southern Oscillation, centered in the Pacific Ocean, may have analogues in the other tropical oceans. A study of these phenomena, their properties, their linkages, and their climate consequences holds promise of providing us with a predictive capability that far exceeds what we are likely to achieve through atmospheric studies alone.

THE SOUTHERN OSCILLATION

The Southern Oscillation is a large-scale exchange of atmospheric mass in the atmosphere between the eastern and western hemispheres in the tropics. It can be detected in sea-level atmospheric pressure records as a see-saw 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 a couple of years and may occur at 2- to 10-year intervals. It is the most obvious instance of interannual climate variability.

The Southern Oscillation comprises a global family of climatically varying phenomena. Among these are sea-surface temperature anomalies in the Pacific, Indian, and Atlantic oceans. The changes in the equatorial current system and the heat content of the Pacific Ocean are particularly marked. The largest oceanic oscillation is El Niño, an anomalous warming off the coast of South America. El Niño brings destruction to the fisheries off Peru and Ecuador. Plankton, fish, and birds, depending in a chain for nutrients provided by the upwelling of cold seawater off the coast, die. 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 South American coast.

The Southern Oscillation has climate significance because it is a strong signal and because of its time scale. Though the Southern Oscillation does not occur regularly, an occurrence has correlated manifestations that normally persist for nearly two years from first to last appearance. This duration offers the potential for us to develop a predictive capability of perhaps a few months. The stages of the oscillation are phase-locked to the annual cycle. That is, 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 we, with a better understanding of the Southern Oscillation, use it to predict wintertime climate anomalies over the United States a season in advance?

Let us review the evidence. Correlations between the Southern Oscillation and North American climate anomalies were first described in the 1930s by Sir Gilbert Walker. Since that time, there has been growing evidence of the reality of these correlations. Wintertime climate anomalies in the Northern Hemisphere are correlated with earlier atmospheric pressure anomalies over the South Pacific and with sea-surface temperature anomalies in the equatorial Pacific Ocean. Warm sea-surface temperatures are followed by high atmospheric pressure over Indonesia and Australia. Through a global-scale process of physical links, which has been called "teleconnection," these events are correlated with above-normal wintertime temperatures in the southeastern United States and below-normal wintertime temperatures in northwestern Canada.

During a normal event, an El Niño begins in January. During the year, warmer-than-normal sea-surface temperatures spread over vast areas of the eastern and central equatorial Pacific. By the following September, surface atmospheric pressure over Indonesia reaches a maximum. Wintertime temperature anomalies over North America may follow in December through February (Horel and Wallace, 1981). The chain of events in the ocean and atmosphere may be a basis for prediction. However, we must be careful not to overstate the case.

During the winter of 1982-83, the strongest El Niño event ever observed took place. It was not forecast, it was not 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 underlined the incomplete state of our understanding.

Correlations of North American temperatures with earlier Pacific sea-surface temperature anomalies are found only during the winter season and only over the northwestern and southeastern parts of the continent. Over most of the United States, the correlations are not significant. Where the correlations are significant, they account for considerably less than half the variance in those regions (Barnett, 1981). Nevertheless, Horel and Wallace (1981) suggest that the patterns of correlation may be blurred images resulting from the superposition of an ensemble of sharper patterns that correspond to the various states of the equatorial ocean and atmosphere. If so, then a sharper specification of the equatorial sea-surface temperature and of tropical rainfall might, with effective modeling of the processes of teleconnection, lead to better advance climate anomaly predictions over the United States than is now possible. This hope motivates the general excitement that today exists among oceanographers and meteorologists for a large-scale ocean-atmosphere experiment to explore the interannual variability of the tropical ocean and the global atmosphere (TOGA).

CLIMATE PROCESSES IN THE TROPICAL ATLANTIC AND INDIAN OCEANS

The link between the tropical Pacific Ocean and the atmosphere has attracted considerable scientific attention. The Atlantic and Indian oceans also provide interesting but different examples of large-scale interactions between the tropical ocean and the global atmosphere.

Atlantic sea-surface temperature anomalies correlate with droughts in Brazil. Those in the Indian Ocean correlate with variations in the Indian monsoon. As in the Pacific, tropical sea-surface temperature anomalies influence and in turn are influenced by the atmosphere.

An El Niño-like phenomenon may occur in the tropical Atlantic (Hisard, 1980; Merle, 1980), but the smaller dimension of the Atlantic basin may be the reason for the lack of clear evidence for its existence (Moore et al., 1978). The tropical oceans respond to changes in the atmospheric wind stress. The circulation in the ocean is forced by the winds in a way that is distinctive in each ocean. For example, the propagation time of planetary waves across the ocean basin is a critical factor. The Pacific Ocean, being wider than the Atlantic, responds differently to a similar wind forcing, and a strong El Niño thereby occurs.

The Indian Ocean region appears to play an important role in the Southern Oscillation. In addition, there is a large seasonal change in the Indian Ocean in response to the monsoon. The Somali Current, for instance, reverses its direction 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.

THE TOGA PROPOSAL

A large-scale ocean-atmosphere experiment to study the Southern Oscillation family of phenomena has been proposed. It is called the Interannual Variability of the Tropical Ocean and the Global Atmosphere Experiment (TOGA). (Other names for components of the same investigation are the El Niño and the Southern Oscillation Experiment (ENSO) and the Ocean Atmosphere Climate Interaction Studies (OACIS).) The basic aims of the TOGA studies are as follows (TOGA Study Group, 1983):

1. to determine the nature of the interannual variability of the tropical oceans and global atmosphere, and
2. to understand the mechanisms that determine the interannual variability and the predictability of the variations.

These aims encompass the principal planetary-scale aims of a related atmospheric research program, the Monsoon Climate Program.

TOGA as proposed today would include these elements:

* a description of the time table and chain of events in the Southern Oscillation and El Niño,
* the relationship of the Southern Oscillation to the

regular annual cycle,

* the fluxes of heat across the Pacific,
* the coupling between the oceanic mixed layer and the deeper waters,
* the relationship between equatorial convection and precipitation and sea-surface temperature anomalies,
* the conditions that lead to a major versus a minor El Niño,
* the relationship between sea-surface temperature anomalies and atmospheric planetary wave patterns,
* the relationship between sea-surface temperature anomalies and higher-frequency fluctuations such as storm tracks and atmospheric blocking, and
* the observing system needed to predict climate change associated with the Southern Oscillation.

The Climate Research Committee (1983) has proposed the following scientific and operational objectives for ENSO:

1. To develop an improved understanding of the in situ local and remote atmospheric forcing of and response to fluctuations in equatorial Pacific sea-air transfers of moisture and sensible heat due to sea-surface temperature anomalies.
2. To identify the processes that control the development and time evolution of the thermal anomalies associated with the Southern Oscillation and the El Niño in the equatorial Pacific Ocean.
3. To understand the large-scale ocean-atmosphere interactions responsible for much of the short-term interannual fluctuations of the coupled climate system and to determine the predictability of the system. Of particular interest are relationships between the Southern Oscillation phenomenon and (1) climate fluctuations in mid-latitudes, particularly North America, and (2) interannual variations in the Asian monsoon, including both its regional characteristics and its relationship to the planetary circulation.
4. To develop improved schemes for prediction of short-term climate variability.
5. To design the optimum operational observing system required to provide the data base for such predictions.

TOGA is an exciting opportunity. The Southern Oscillation is a strong climate signal. The economic benefits that could be derived from predicting some of the associated climate anomalies could be great. A number of excellent scientists are enthusiastically working on the problem. Progress is being made in data analysis, field experiments, and theoretical work.

On the negative side, there is as yet no comprehensive theoretical framework for TOGA. The first fragments of a theory exist, and some linking physical mechanisms have been hypothesized. However, we do not yet have a strong enough base of theory to be able to design a full TOGA experiment with assurance.

Correlation does not always indicate causality. In a system where processes are known to be tied to the annual cycle, it is particularly risky to infer causality simply from correlations and timing. Statistically significant correlations can be found among the Southern Oscillation Index, equatorial Pacific sea-surface temperature anomalies, the El Niño, and wintertime temperature and precipitation anomalies over parts of North America. These correlations do not imply causal links, however. We cannot say that the Southern Oscillation causes the other observed climate variations. Because the events in the Southern Oscillation family are each linked to phases in the cycle of annual variation, we cannot say that event A causes event B, simply because A usually precedes B. Both A and B could be due to other causes. What is needed is a plausible physical mechanism to explain why A is usually followed by B. Then experiments can be designed to test that hypothesized mechanism. We must seek a testable theoretical framework to explain why and how the Southern Oscillation, the El Niño, and the temperate latitude climate anomalies are physically linked.

A TOGA STRATEGY FOR NSF

NSF should support TOGA planning with an eye to implementing a large-scale oceanographic and atmospheric experiment. Oceanic and atmospheric sciences will have to collaborate, and the scale of the problem will require strong coordination between NSF, NOAA, and NASA. The effort required to design and implement TOGA is justified by the benefits that might be derived. A prediction of regional climate changes over North America, for example, could have great social and economic rewards.

Within the United States, the subject of TOGA is receiving considerable attention. Parallel programs that had been developing separately (El Niño and the Southern Oscillation Experiment (ENSO), being prepared under the auspices of the Climate Research Committee of the National Research Council, and Ocean Atmosphere Climate Interaction Studies (OACIS), being prepared by NOAA) are being merged. The merged program will likely be a major U.S. contribution to TOGA.

In addition, several ongoing and proposed American ocean research programs are related to TOGA: the Equatorial Pacific Ocean Climate Studies (EPOCS), the Sub-Tropical Atlantic Climate Study (STACS), the Pacific Equatorial Ocean Dynamics Experiment (PEQUOD), the Seasonal Response of the Equatorial Atlantic (SEQUAL), and the Tropic Heat Experiment. These programs can serve as components of a U.S. contribution. Even more importantly, these programs provide a reservoir of scientists prepared to work on the climate links between the tropical ocean and the atmosphere.

A decision will have to be made concerning the extent of U.S. commitment to TOGA. Planning is proceeding actively for a Pacific-based program to study the Southern Oscillation (ENSO, OACIS). However, our position with regard to research in the Atlantic and Indian oceans is not clear. A large-scale U.S. program, the Seasonal Response of the Equatorial Atlantic (SEQUAL), is based in the Atlantic but does not address all the broad issues of atmospheric and oceanic climate linkages. NSF should participate with other agencies and the universities in a discussion of our overall involvement with TOGA.

Can the United States afford to be involved in all three oceans? The question of setting priorities becomes more difficult if the U.S. program is to be global. Or should U.S. involvement be principally in the Pacific Ocean, where the Southern Oscillation studies are centered? If the United States does concentrate its efforts and resources in one area, what should be the extent of U.S. collaboration with other countries who may, for their own reasons, need to concentrate their efforts somewhere else?

As TOGA develops, NSF and the other agencies should focus on first obtaining answers to a set of basic questions as a prelude to a full-scale program. Some of these questions are, What is the chain of events in the ocean and the atmosphere and what imposes the time scale on the Southern Oscillation? How are the component elements linked to the annual cycle? What differentiates a major El Niño from a minor one?

These basic questions will not all be answered in the early stages of TOGA. Keeping them in mind, however, will help to focus the program to provide a physical understanding leading to an improved predictive skill.

INTERNATIONAL INVOLVEMENT IN TOGA

As this report was being prepared, a statement of TOGA's international objectives was also being prepared. The Committee on Climatic Changes and the Oceans (CCCO) and the Joint Scientific Committee for the World Climate Research Program (JSC) have established a TOGA Steering Committee that is developing a scientific framework.

Among the questions that need to be answered is, Should TOGA include all tropical oceans? A three-ocean TOGA has some attractive aspects. Each ocean has its special processes, and it may be that large-scale ocean-atmosphere interactions can best be understood by examining the contrasts between the three ocean basins.

Some countries may be prepared to participate in TOGA if they can do so in the Atlantic or Indian oceans, whereas a TOGA confined to the Pacific Ocean might not be acceptable to them. This was, in fact, the case for some countries in the First GARP Global Experiment. The United States should seek a balance between scientific and political factors in considering the question of the scope of TOGA. Particularly if the United States should decide to restrict its TOGA program to the Pacific Ocean, it would probably be unwise to take a firm international position before exploring the consequences.

An El Niño-like phenomenon sometimes occurs in the tropical Atlantic, but our observations are not adequate to be certain about its existence. In general, the Atlantic provides an opportunity to study large-scale tropical air-sea interactions without the complicating effects of a strong El Niño. If a program were to be developed simply to look at equatorial oceanic processes, the Atlantic might be a better choice than the Pacific, first, because the annual cycle is more regular and, second, because the region to be covered is smaller.

Large-scale experiments in the Atlantic Ocean are already under way. The U.S. program SEQUAL and the French program Français Océan Climat Atlantique Equatorial (FOCAL) are working cooperatively to understand the oceanic response to seasonally varying winds. A study of the corresponding atmospheric, and hence climate, response to the changing oceanic conditions is not an explicit part of these programs. The potential importance of SEQUAL and FOCAL to understanding global climate variability is great, but some effort will have to be made if the results of these programs are to be channeled effectively into the climate research program stream.

The Southern Oscillation extends into the Indian Ocean, where the monsoon provides a sharp alternating sea-surface forcing. The Southern Oscillation appears to have origins in Southeast Asia (though appearances may be deceiving). There may be significant oceanic exchange of mass and heat between the western Pacific and the eastern Indian ocean. Thus it may be scientifically valuable to include the Indian Ocean as part of TOGA, to complement a major Pacific Ocean TOGA experiment.

No U.S. ocean climate program is currently under way in the Indian Ocean. Some American oceanographers are cooperating in a French program, SINODE (Surface Indian Ocean Dynamic Experiment), whose purpose is to study the seasonal and interannual variability of the currents and heat content of the upper layer of the northwest Indian Ocean.

French research activities in the Indian Ocean are providing benefits to American research. If the tropical Indian Ocean is included within the U.S. TOGA program, the benefits from the work of other countries could outweigh the additional expenses that might be incurred.

To summarize, the Southern Oscillation, centered in the Pacific, presents an exciting natural signal that seems to promise a predictive capability for climate variations in temperate latitudes. The opportunity to study this phenomenon should not be missed, and the United States should support a major TOGA experiment in the Pacific. At the same time, complementary TOGA research activities should be supported in the Atlantic and Indian oceans, though it may be that other nations will play the principal role there.

5 THE WORLD OCEAN CIRCULATION EXPERIMENT

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

A large-scale oceanographic experiment to examine global ocean circulation and ocean climate processes is being proposed. The World Ocean Circulation Experiment (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.

THE GENERAL CIRCULATION OF THE OCEAN

The role of the ocean in climate change can be well understood only if we understand the present climate state of the ocean itself, including its circulation and the transient processes with which it interacts. Thus we need reliable global observations of the state of the ocean and of the changes of that state with time.

Reliable observations of a natural system can only be made if the variability is first understood. This variability will govern the sampling procedures needed to obtain a representative description. If the sampling procedures are improperly designed, the processes that we are trying to observe could be overlooked or misidentified. Our current description of the ocean, based on decades of observations, may be adequate in some places and for some oceanic processes. In many places, however, our description is not adequate, and the classic set of oceanographic measurements does not define all of the processes occurring in the ocean that are important in the climate system.

Recent oceanographic studies have exposed the existence of a number of processes that could be important to the ocean's role in climate variability: mesoscale eddies, tropical waves, isopycnal mixing, the seasonal variation of the mixed layer, microscale mixing in the interior of the ocean. We have also developed computer models of the large-scale ocean circulation. The models underline the importance of some of the processes just listed. Thus, to observe and understand the climate of the ocean, we need to describe the small-scale processes in the ocean in enough detail to model them realistically.

A major obstacle to obtaining observations of the ocean is the difficulty of obtaining measurements over long time scales and over great distances. But 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 the wind stress on the sea surface. If these observations are combined with subsurface remote sensing, it may 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 idea of a world ocean circulation experiment has encountered some resistance. Physical oceanographers have a tradition of research programs that are regional in scope. Some of them are suspicious of any program with "world" or "global" in its title. The personal cost of working with others in a large program can be great. Some oceanographers who have tried it prefer to avoid the hassles and stick to programs that are small enough that they can retain control of their lives.

Some meteorologists are suspicious of a world ocean circulation experiment because they view it as an attempt to use the World Climate Research Program (WCRP) as a justification for large-scale oceanographic research that may not have direct links to climate.

In spite of these hesitations, the consensus is that understanding global ocean circulation is essential to understanding climate. A global program will complement regional oceanographic programs and will provide observations that will benefit them. The climate state of the ocean is as much a part of global climate as the climate state of the atmosphere. Certainly, the atmosphere has more direct impact on humans and their activities than does the ocean. However, if we want to understand how the climate system works, we cannot continue to treat the climate of the ocean as secondary, as something that can simply be parameterized or treated with bulk formulas.

The challenge, then, is to define the global ocean experiment that is justified by the needs of the climate research program and that will provide the understanding of the ocean that we so critically need.

An unresolved question is that of the depth to which the ocean interacts with the atmosphere on time scales relevant to climate variability. On one hand, it is often assumed (mostly by meteorologists) that the upper 100 to 300 m of the ocean is sufficient to define the sea-air interactions, at least on time scales shorter than decades. On the other hand, some (oceanographers) contend that this is not sufficient. They believe that all of the ocean needs to be taken into account in order to understand climate variability on scales of a year or longer.

Quantitative estimates of the depth of the ocean interacting with the atmosphere are hard to come by. One measure uses the time it would take to warm the ocean in the case, for example, of the global warming that might be produced by rising levels of carbon dioxide in the atmosphere. Thermal time constants were calculated (Climate Board, 1982) using downward mixing modified by the changing sea-surface equilibrium as the ocean warms. The results show that for a new equilibrium temperature 3°C higher, the ocean would warm to a depth of 50 m in 3 years, to 500 m in 30 years, and to 5000 m in 300 years. This type of calculation has been cited as evidence that climate models need only look at the upper 200 m of the ocean for time scales shorter than decades. Those that counter this type of calculation point out that mixing downward is not uniform. Tritium measurements, for example (Östlund et al., 1976, Figure 2), show mixing down to 5000 m at high latitudes in the North Atlantic within a few years.

THE WOCE PROPOSAL

A stated objective of WOCE is "to describe and understand quantitatively the general circulation of the ocean, in order to assess within the WCRP the sensitivity of the climate system to changes in external forcing, whether natural or anthropogenic, on time scales of decades to centuries" (CCCO-IV, 1983). The proposal for WOCE has three types of scientific objectives:

1. To describe the general circulation of the ocean.
2. To understand the rates and processes of water-mass transformation.
3. To describe the spectrum of seasonal and broad-band ocean variability.

The WOCE Scientific Steering Group has been charged (CCCO-IV, 1983) with developing a clear description of the WOCE observational and modeling program.

WOCE objectives have yet to be specifically defined, and the existing documents give only a general indication of what WOCE will consist of. Curiously, the Tokyo Study Conference in its definition of WOCE observational needs does not highlight the need for determining the general circulation of the ocean. Nor does it emphasize the need for such techniques as global satellite altimetric measurements of ocean surface currents. This oversight was corrected at CCCO-IV (1983, Appendix III), and the primary observational work defined for WOCE includes studies of the following:

1. The global circulation of the ocean.
2. The fields of surface forcing: wind stress, net surface heat flux, and net surface moisture flux.
3. Oceanic temperature and salinity distributions.
4. The statistics of mesoscale eddies in order to characterize lateral mixing.
5. Determination of ventilation times and water-mass conversion processes.
6. Large-scale surveys of signatures of water-mass conversion.
7. Large-scale aspects of the seasonal cycle.
8. Broad-band inherent variability.

The observations needed for this list of studies include measurements of wind and wind stress, sea level, solar radiation, diffuse attenuation coefficients, and total precipitable water. Also needed are deep density sections, tracer samples, and extensive in situ temperature, salinity, and velocity measurements.

To provide the basis of knowledge to understand the state of the ocean, we must describe the mean circulation of the ocean over several years as well as the space-time variability on time scales of months to years. This might in part be done as a global experiment lasting 5 to 10 years. In addition, special studies could focus on processes that would elude an experiment of this duration.

It is evident from the above summary that we are not yet fully beyond the shopping-list stage. There is broad agreement that the circulation of the ocean has a strong influence on climate and therefore must be understood. The next step must be to define the circulation measurements that are needed in quantitative terms. A WOCE workshop, to be held in the summer of 1983 under the auspices of the National Research Council, should be an important step in clarifying plans for the U.S. component of WOCE. Today's sketchy WOCE plans should mature into a critical component of the climate research program.

A WOCE STRATEGY FOR NSF

WOCE planning has not yet begun in detail, and the documents so far prepared (CCCO-III, 1982; WOCE Design Options Study Group, 1982; CCCO-IV, 1983) do not define what WOCE will be. There is, in fact, still some ambiguity about the primary objective: is it to examine global oceanic and atmospheric processes or is it to describe global ocean circulation? This state of affairs is probably normal at this stage of planning for a large undertaking. The uncertainty in the definition of WOCE points to the work that remains to be done in developing a large-scale ocean experiment.

NSF may wish to support some research to aid in planning for WOCE. A large-scale commitment to WOCE should await a definition of what the experiment comprises and an international agreement on the scope of the program. WOCE is intended to be a global experiment, and hence international agreements are essential to its planning. NSF should note that a large-scale WOCE is not necessary for some of the observational studies listed in the previous section. These might be carried out as separate projects whether or not WOCE is developed.

The stated WOCE objectives have some similarities to those that have been presented for Cage (see the next chapter). The proposed mode of implementation is not the same, however: WOCE proponents talk of generic studies, while proponents of heat flux experiments (sometimes the same individuals) have advocated specific estimates of heat budgets. Nevertheless, both involve estimating surface fluxes of heat and moisture, determining water-mass conversion processes, and measuring solar radiation and atmospheric flux divergences. Thus the recent decision (CCCO-IV, 1983) to undertake heat flux estimates as part of the precursors to and observations for the main WOCE experiment is a logical step in planning.

NSF should anticipate further sorting-out of WOCE (and heat flux) objectives. The component generic studies ought to be more fully defined and defended. Are all equally necessary? If we were to support a sequence of studies as part of WOCE or as a precursor to WOCE, how should that sequence be ordered? That is, where should we begin? NSF should seek to have these questions resolved before making a full-scale commitment to WOCE, not because WOCE is of uncertain value, but simply because many of the basic steps in program definition have yet to be taken. WOCE is still young. The ideas and objectives need to mature.

A common thread in many WOCE component studies is an earth-orbiting satellite that measures sea-surface elevation by altimetry and surface wind stress by scatterometry. Such a satellite (TOPEX Science Working Group, 1981) would provide a framework for a broad range of climate studies. Sea-surface elevation can define the field of surface geostrophic currents. With complementary measurements, such as of the density field in the interior of the ocean, the circulation of the ocean might be determined. Drifting and fixed buoys could also provide complementary measurements. An intriguing possibility is to combine satellite observations of altimetry and wind stress with ocean acoustic tomography (Munk and Wunsch, 1982) as a means to provide an ocean-observing system. This might be a major step in providing the kind of synoptic information in the ocean that we have long taken for granted in the atmosphere.

Proposals within NASA for an altimetric satellite have not been accepted so far by the administrator. This may in part be due to lack of a perceived consensus need for such a satellite. It may be that WOCE can go ahead without a U.S. oceanic topographic satellite. The European Space Agency is planning a SEASAT-like satellite that will measure altimetry, to be launched in 1987. Japan may launch a satellite with an altimeter in 1990. The precision of these satellites may not be as great as that proposed for a U.S. altimetric satellite (TOPEX), but they could allow WOCE to proceed.

If WOCE goes ahead without a U.S. satellite for sea-surface elevation and wind stress, U.S. oceanographers could be at a disadvantage. The policies of the foreign space agencies with regard to data availability are not clear, but it is possible that U.S. researchers might have to wait to obtain access to the data until after foreign scientists have had the right to first use.

Satellite altimetry and scatterometry are essential for WOCE, for ocean climate monitoring, possibly for heat flux studies, and possibly for TOGA. NSF thus should note the vital need for an earth-orbiting satellite to provide information on surface ocean currents and the wind stress on the sea surface for future ocean and climate research. NSF senior management should discuss this issue with NASA and NOAA. If we are to proceed with large-scale ocean experiments in the next decade, we will soon need to make commitments for satellites to support those experiments. NSF should be aware of what NASA is likely to do in order to make its own plans.

INTERNATIONAL INVOLVEMENT IN WOCE

WOCE is in need of technical definition and planning. Its present state of development is insufficient to define the activities or the level of support that will be needed. We need to establish an international program office that can plan and that can deal with the issues on a technical level. Adding to existing secretarial activities or creating an office that is part of a secretariat (like that of the Intergovernmental Oceanographic Commission) will likely not result in a group that is technically capable of scientific and technical planning.

6 HEAT TRANSPORT STUDIES

Transport and storage of heat by the ocean are central to all theories of the role of the ocean in global climate and thus central to our hope for developing the skill to predict climate variations. In this chapter the problem of estimating heat transport processes in the ocean is considered, and proposals for large-scale experiments that focus on this question are reviewed. Though these experiments are now likely to take place as components of WOCE and TOGA, they are here discussed in a separate chapter because of their importance and the extensive scientific review they have received.

OCEAN HEAT TRANSPORT AND STORAGE

The ocean dominates the energy storage of the combined ocean-atmosphere system. Heat can be stored in the ocean for periods that are long in comparison with atmospheric residence times. The ocean can transport this heat and can give it up to the atmosphere far from the place where it was received. Oort and Vonder Haar (1976) estimate that the ocean has a heat transport poleward from the tropics to mid-latitudes as large as or larger than the corresponding mid-latitude atmospheric transport.

As was discussed in the chapter on TOGA, we have found correlations between tropical sea-surface temperature anomalies and subsequent climate variations in temperate latitudes. Such correlations have been known for some time (e.g., Bjerknes, 1969), but recent work (e.g., Horel and Wallace, 1981) provides a hypothesis of the physical processes that bring this about. The correlations between the tropical Pacific and Northern Hemisphere extratropical latitudes are not strong. But the correlations do suggest that variations in oceanic heat storage and transport may modulate atmospheric fluctuations on time scales that are tied to the oceanic processes.

Heat flux is a central variable in all ocean climate models. For model testing, we need to be able to determine experimentally the poleward transport of heat by the oceans and its variations with time. Techniques for estimating ocean heat transport are subject to uncertainty. Before we can confidently deal with the question of ocean heat transport, we will have to develop means for measuring it so that we can have assurance in our estimates. Our aim should be to measure the ocean's role in heat flux in order to understand the magnitude of poleward heat transport by the ocean and the atmosphere, the distribution of this transport by region, and the processes that control the magnitude and time scale of the transport.

THE CAGE PROPOSAL

Ocean heat flux experiments have been proposed nationally and internationally to explore the storage, transport, and transfer of heat by the ocean. The Cage experiment was proposed by a group led by F.W. Dobson (Bretherton et al., 1982) to examine the long-term mean heat flux, the annual cycle, and the interannual variability over the North Atlantic.

The Cage objective is to compare the results of three different techniques for estimating the heat flux from the ocean to the atmosphere over an ocean basin. The three methods are as follows:

1. Estimation using the distribution of temperature and velocity within the ocean (the Hall and Bryden (1982) method).
2. Estimation by area integration of the heat transfer across the air-sea interface (the Budyko (1974) and Bunker (1976) method).
3. Estimation from the net radiation at the top of the atmosphere together with estimates of atmospheric flux divergence and oceanic heat storage (the Oort and Vonder Haar (1976) method).

All three methods are subject to major uncertainties. We need to know how well we can determine ocean heat flux before we can begin to consider its interannual variability. We want to know the random and systematic errors associated with each. By comparing the three techniques over an ocean basin, it might then be possible to estimate the interannual variability of heat transport and its sensitivity to long-term climate change. These experiments would be useful if each of the three methods could estimate the heat flux within an uncertainty of 10 percent. At least 5 years of measurement have been proposed over an ocean-wide region bounded east and west by continents and north and south by transoceanic sections. The goal of Cage would be to determine the mean heat flux to the atmosphere over the North Atlantic to a desired 20 percent accuracy.

The Cage proposal was controversial. Uncertainties in the individual components of the sea-surface flux budget are about 30 to 40 percent. With measurement techniques now available, only by taking global averages can these uncertainties be reduced. Critics say that to obtain an estimate of heat flux that is accurate within 20 percent, we must average extensively (over the entire ocean basin, over all depths, over long periods of time). This much averaging will produce an integral result that will not resolve the time- and space-dependent processes that control the poleward flux of heat.

A further problem is that satellite radiation measurements, such as might be made by an Earth Radiation Budget Experiment (ERBE) satellite, may not be available during Cage. Should Cage go ahead without such a satellite? A knowledge of the radiative fluxes at the top of the atmosphere is fundamental to obtaining the heat balance over an ocean basin. Without ERBE, Cage will be seriously weakened.

To estimate the heat flux from the net radiation at the top of the atmosphere, we will need estimates of the atmospheric divergence of latent and sensible heat and of potential and kinetic energy over an ocean basin. We do not know if such measurements can be made with the requisite accuracy (less than 10 W/m^2). Furthermore, we do not yet have a procedure whereby we can test such estimates for bias.

At the Tokyo Study Conference (CCCO, 1983), it was suggested that, at least in the Atlantic, the interannual variability in heat flux is about 10 W/m^2. If this is the case, it is contended, all components of an Atlantic Cage experiment need not be done simultaneously. In particular, satellite measurements could be made at some later stage, when a satellite becomes available.

It was thus proposed at the Tokyo Study Conference that the North Atlantic Cage experiment be redefined into a step-by-step process. This would begin with a study of the interannual variability of each of the heat budget components. If this study shows that the interannual variability of these components is greater than is now apparent, it might be necessary to return to a simultaneous Cage experiment.

Finally, at the fourth session of CCCO (CCCO-IV, 1983) the issue of an explicit Cage experiment was addressed. The committee felt that intercomparisons between different techniques for estimating heat flux were required. Only by this means can systematic differences in the estimates produced by the techniques be identified. However, rather than planning for a single comprehensive experiment, opportunities for pairwise intercomparisons should be exploited in WOCE and TOGA, and possibly in special regions such as enclosed ocean basins, particularly in the North Atlantic.

The committee urged work to improve our techniques for developing atmospheric assimilation models and for obtaining direct estimates of surface fluxes. The heat transport estimates recommended by the Cage Study Group should be undertaken as part of the precursors to and observations for the main WOCE experiment.

THE PATHS PROPOSAL

The Pacific Transport of Heat and Salt (PATHS) Program has sometimes been referred to as a "Pacific Cage," but as presented by the Pacific Cage Study Group (1983), led by G.A. McBean, it has significant differences in objectives and approach. The principal objective is to understand the horizontal processes of large-scale transport of heat and salt in the mid-latitude North Pacific Ocean. The study should investigate the short-term climatic variability in the distributions of heat and salt in both the subarctic and subtropical gyres of the North Pacific, on time scales of months to years, for a period of approximately 10 years. The measurement should give the net flux of heat and water from the ocean to the atmosphere. The principal focus, however, is internal heat redistribution by horizontal processes.

Another objective is to estimate transports and air-sea fluxes from time-dependent measurements of heat and salt storage. The southern boundary could be 30°N. Together with the Atlantic studies, this would close off the globe north of 30°N and permit a zonally averaged estimate of the meridional heat transport.

A positive feature of Pacific heat flux studies is the extensive existing data base. The Japanese in particular have collected regular measurements in the western Pacific for several decades and produce analyses based on them. In this country as well, programs such as the North Pacific

Experiment (NORPAX) and the North Pacific ship-of-opportunity program known as TRANSPAC have accumulated a significant data base. These programs are ongoing, and coordination of them (by CCCO) is mainly what is needed.

However, if the Pacific heat flux is to be estimated to the same accuracy as that proposed for Cage in the North Atlantic (say, within 0.2×10^{15} W), the observational demands may be more stringent. The surface area of the Pacific north of 25°N is about 35 percent greater than that of the Atlantic. Thus the corresponding average surface flux of heat will be less, perhaps about 5 W/m^2. Measurements accurate to this level will be difficult, and it is not yet clear that they are technically feasible.

The PATHS plans include a number of component activities. In the Kuroshio region, the Japanese are planning several experiments. The Ocean Heat Transport Experiment (OHTEX) will observe the Kuroshio with a current meter array for one year. The Ocean Mixed Layer Experiment (OMLET) will monitor the variability of the mixed layer, measuring the processes that are involved in heat fluxes and developing the techniques needed to carry out the measurements. An American program, Heat Advection Investigations in the Kuroshio (HAIKU), will observe the Kuroshio southeast of the Ryukyu Islands, a region that may be analogous in ocean-basin heat transport to the well-studied Florida Straits. PATHS also proposes that a transoceanic section be made to measure the heat transport analogous to the estimate of Hall and Bryden (1982) in the Atlantic.

The PATHS Study Group has proposed a set of component programs within a broad framework. The resultant program appears to lack the overall strategic flavor that characterized the Cage proposal. If Pacific heat flux plans are further developed, they may come to resemble more closely those of Cage.

North Pacific interannual variability is less well known than that in the Atlantic, but appears to be relatively large compared to the mean. Thus, unlike Cage, a nonsimultaneous PATHS experiment has not been proposed.

For PATHS as with Cage, CCCO-IV (1983) recommended that the heat transport work be done as part of WOCE and TOGA. The committee noted that PATHS has a strong resemblance to many aspects of WOCE. Experience gained in PATHS could contribute to WOCE planning. Thus PATHS might be considered a precursor to WOCE.

THE GINS CAGE PROPOSAL

The Greenland, Iceland, and Norwegian Seas Cage Experiment (GINS Cage) is a proposed program to study climate processes in the northern polar seas. To begin, a study of sea-air-ice processes in the Greenland Sea is proposed. The region of the Greenland Sea is favorable for studies of the interactions among air, sea, and ice. There is vigorous exchange of energy between the ice-covered and the ice-free ocean. The seasonal and interannual variations of ice cover are large and provide a strong signal for study. The data base is large because of extensive shipping in the region.

The Greenland Sea area is well configured for observations: there are narrow passages to north and south, there is a coast to the west, and there is an unconfined ice boundary to the east. Since the dominant surface flow is nearly unidirectional, drifters can be used efficiently. Finally, the region is accessible and is already surrounded by a relatively dense network of meteorological observing stations.

Plans are still being developed, but the essential research elements can be summarized: ice dynamics, the role of the ocean mixed layer in the heat balance of the bottom surface of the ice, and large-scale convection and its connection to lower latitudes through deep flows into the North and South Atlantic. After 5 years of monitoring, the work could be extended from its Greenland Sea base to include the Norwegian Sea. The proposal is tentative, pending further studies of ocean transport data across the passages of the GINS Cage area.

Reaction to the GINS Cage idea has been mixed. On the positive side, the region is nicely defined, the signal is large, and the role of ice in global climate is worthy of study. On the negative side, a Greenland Sea study followed by a GINS Cage experiment would take considerable effort. Many of the Cage enthusiasts would rather spend that much effort on a full-blown North Atlantic Cage experiment. Furthermore, because uncertainties in measurement are reduced by area integration, a Cage-type experiment becomes more difficult the smaller the area. Though the heat flux signal is large in the GINS area, we do not yet have a careful estimate of the feasibility of a Cage experiment in that region.

Oceanographers and meteorologists with an interest in the polar regions are not numerous, and thus the enthusiasm for GINS Cage at scientific meetings such as the Tokyo Study

Conference is not great. Furthermore, a critical question is whether there is a sufficient number of committed scientists. If the problems are worthy of study, we must be assured that enough qualified researchers are available and willing to take them up. The issue is further complicated by organizational factors. In the funding agencies and in the NRC, polar matters sometimes are separately considered. Common development of research plans in polar regions thereby requires an additional effort to assure coordination.

A HEAT FLUX STRATEGY FOR NSF

The question of strategy to be followed here is a sticky one. Ocean heat flux may be a key factor in controlling global climate variability. However, many oceanographers question our ability to design and carry out an adequate large-scale heat flux experiment in this decade. Before making a commitment to such experiments, NSF should be assured that all three methods for estimating the heat flux are technically feasible within the desired accuracy, that information on processes and not simply integral results will be obtained, and that any proposal for a nonsimultaneous Cage-type experiment will be documented with quantitative estimates of the interannual variability of the component processes.

If technically feasible, should we carry out both North Atlantic and North Pacific experiments? Can the United States or the international climate research community afford to do both? If we can only afford to do one, which one? There are arguments in favor of each ocean basin. The poleward oceanic heat flux appears to be greatest in the North Atlantic. As a consequence, the radiation levels per unit area are greater than over the North Pacific, and a precise estimate will be easier to obtain, an argument in favor of the Atlantic. The North Pacific, on the other hand, may be a better choice because it is not complicated by strong interchanges with the Arctic basin, as is the North Atlantic. It is, in effect, closed off to the north, thus simplifying the geography and the heat flux. Deep and bottom water formation is absent in the North Pacific. On the negative side, the poleward heat flux may be less in the Pacific. Because the area of the North Pacific is considerably greater than that of the North Atlantic, with a smaller total heat flux, the surface radiation levels in the North Pacific must be considerably less and thus the air-sea interchanges per unit area must also be less. Hence a program of measurements with 20 percent accuracy could be more difficult to achieve in the Pacific.

Doing Atlantic and Pacific heat flux experiments at the same time would have some advantages. The complete Northern Hemisphere ocean and atmosphere heat budget could be determined. This would, in effect, put a cap on the earth north of 25°N. Since the Atlantic and Pacific differ in the dynamics of their heat transport, concurrent observations might shed light on their contrasts. Finally, a heat flux experiment becomes more attainable the larger the area over which one averages. Thus, covering the Atlantic and Pacific together could make the desired accuracy of 20 percent more achievable.

One factor that works against the idea of doing a heat flux experiment over both the Atlantic and the Pacific is the scale of operations that would be needed. Could we meet the expense? Are there enough interested scientists to carry out the work? Do we have enough ships and scientific equipment? An earth-orbiting radiation satellite is needed, here, at least, a hemispheric experiment would impose only a small additional expense.

If we do carry out heat flux studies over both ocean basins, we may do so within a different context than that proposed for Cage and PATHS at the Tokyo Study Conference (Bretherton et al., 1982, Pacific Cage Study Group, 1983). For example, a WOCE that incorporates heat flux process studies could, because of its generic nature, deal with all ocean basins. If this comes to pass, however, the heat flux experiment may evolve considerably beyond the Cage and PATHS proposals. NSF should anticipate that large-scale heat flux experiments will be proposed in some form. NSF should be prepared to support such work because of its importance in understanding climate change. Nonsimultaneous heat flux studies might provide the opportunity to incorporate heat flux experiments as part of WOCE and TOGA. If this should happen, NSF may be able to support heat flux studies as part of or precursors to the other programs.

Many of our ideas about North Atlantic heat flux are stimulated by the direct estimate of Hall and Bryden (1982) of the poleward heat flux across 25°N latitude in the Atlantic. A corresponding estimate for the Pacific does not exist. In fact, we do not even yet know the order of magnitude of the Pacific poleward heat transport across 25°N latitude. Collecting such a measurement will be much easier to do than the large-scale experiments. Though not a quick and easy task, it is a lot easier to justify as a start than an ocean-basin-scale experiment. NSF should consider supporting a trans-Pacific-Ocean poleward heat flux measurement when a suitable proposal appears. We are being held back in developing our ideas about the North Pacific because of the lack of such an estimate.

NSF might support other heat flux research activities whether or not we go ahead with full-fledged ocean-basin-scale programs. Existing expendable bathythermograph (XBT) data could be analyzed to estimate heat storage over time scales important to climate. A number of techniques need to be developed to provide a capability for measuring heat fluxes. We should collect radiation measurements in situ to compare with estimates from satellites. We should carry out design studies for heat flux and water-mass conversion experiments. We should continue broad-based satellite radiation measurements. We might undertake limited simulation experiments of an observing system. The aim should be to estimate the extent to which we are able to estimate heat and energy fluxes in the ocean and atmosphere, and to define what improvements are needed to reduce errors to acceptable levels.

The GINS Cage experiment could be a valuable contribution to understanding the global heat balance. However, a study of the scientific feasibility is first needed, and we ought to clarify the issue of the scientific resources and manpower that could be called upon to support GINS Cage. NSF should insist that the issue of the role of polar heat fluxes be addressed as part of the justification for either an Atlantic Cage or the GINS Cage experiment.

INTERNATIONAL INVOLVEMENT IN CAGE

Cage and PATHS have been the subject of international studies sponsored by the CCCO. The Cage Study Group (Bretherton et al., 1982) and the Pacific Cage Study Group submitted reports to the Tokyo Study Conference that were the basis for recommendations (CCCO, 1983) that were submitted to the CCCO. The Pacific Cage Study Group (1983) was able to revise its draft following the Tokyo meeting to reflect the changing ideas on the feasibility of large-scale heat flux experiments.

The recommendations to the CCCO have been eclipsed by later evolution in the thinking of those scientists who are active in planning heat flux experiments. The Tokyo Study

Conference recommended that a heat flux steering committee be established for "organizing and guiding the development of Cage and PATHS studies. . . ." A series of precursor studies were listed, though the issue of a full commitment to the programs was sidestepped.

7 OCEAN CLIMATE MONITORING

Monitoring, that is the collection of regular observations of the ocean and atmosphere over large regions for long periods of time, has been frequently cited as a necessary element for progress in understanding climate variability. Yet, as we shall review, advocates have been unable so far to obtain a commitment to establishing large-scale ocean climate monitoring programs, particularly in the ocean. In this chapter, the case for ocean climate monitoring is reviewed.

MONITORING: BUILDING A DESCRIPTION FOR UNDERSTANDING

A description of oceanic processes, particularly of ocean heat transport and storage, is needed to evaluate the role of the ocean in climate variability. The long time scale of oceanic heat anomalies may be an important factor for climate forecasting, but the length of time needed to describe and understand these anomalies presents a problem in experimental design.

Furthermore, events like the Southern Oscillation occur sporadically (typically at 2- to 10-year intervals) and have a cycle length of about 2 years. Such large-scale ocean-atmosphere interactions must be described over several events because of their complex nature. A description of a single event would not be sufficient to understand the phenomenon because each occurrence is different. An ensemble of descriptions is needed to separate out possible overlapping events and to define a characteristic occurrence of the phenomenon. (See, for example, the compositing technique used by Rasmussen and Carpenter (1982) to study the Southern Oscillation/El Niño.) The time needed to describe and understand the climatic influence of the Southern Oscillation is long.

It is thus important to have some means for ocean climate monitoring that can give regular, reliable, and repeated oceanic and atmospheric observations over the course of many years.

Our challenge in experiment design is to decide what set of observations will suffice. We do not need to instrument the oceans on a fine grid to test our oceanic general circulation models. The cost of establishing a long time series, even at one point, can be great. Long series of measurements that are now being taken should be continued (WCRP, 1981). Because of cost, however, we should be sure of our need before establishing new stations.

Before we can define the optimal set of measurements, we need to have at least a crude estimate of the variability of the ocean. Therefore exploratory time series of limited duration should be taken wherever feasible, particularly in conjunction with major oceanographic experiments.

As our description of ocean variability improves, so will our ability to define the optimal system for ocean climate monitoring. The optimal system in this sense has been defined as the minimal set of observations required to define the critical ocean climate indices (NOAA Office of Ocean Technology and Engineering Services, 1981). In optimal design the scale of observations should match the complexity of the phenomenon we want to observe. The problem we face is that for most oceanic climate processes, our knowledge is so rudimentary that we cannot specify the optimum set of observations. Before we can, we must explore. Then, armed with a preliminary description of the phenomenon, we can begin to define what is needed for effective monitoring.

The next steps in extending our monitoring of the ocean for climate need not be elaborate. Many proposals for doing this have been made. The Ocean Science Committee (1974), in a series of workshops led by Henry Stommel, recommended the establishment of "phantom weather ships." In this program, commercial ships would collect measurements as they passed certain designated points in the ocean. The resultant time series of observations would provide regular samples at fixed locations and function much as the Ocean Weather Stations did but without the great expense of maintenance.

The phantom weather ship idea was reiterated (Scientific Committee on Oceanic Research, 1977) by an international panel led by R.R. Dickson. Since that time there has been no move toward implementation. The obstacle seems more to be a lack of coordination than a lack of money. Particularly internationally, we seem to have no effective means for getting this program under way. We will discuss this problem below under "Pilot Ocean Monitoring Studies."

Yet another relatively inexpensive source of climate information could be gained by extending the global network of sea-level observation. This would be particularly effective if extended to isolated islands. Such stations can be set up relatively inexpensively. A long-term commitment to their maintenance needs to be made. The scientific value of long-term global sea-level measurements could be high in comparison to cost. For example, much of our knowledge of the processes involved in the El Niño phenomenon (Wyrtki, 1977) comes from sea-level measurements in the western Pacific.

Sea-level measurements can also serve as an indicator of the effects of increasing atmospheric carbon dioxide (Baker and Barnett, 1982). Increases in ocean temperature will presumably accompany increases in atmospheric temperature. The resulting increase in the heat content of the upper layers of the ocean will cause sea level to rise. The effects could be global, and detection may be enhanced by the coherence of the signal.

If significant information on world climate variability can be gained through relatively inexpensive means (e.g., compared with satellites), what is holding us back? Proposals for sea-level observations go back many years; Stommel's workshops (OSC, 1974) recommended that they be made. Again, international coordination is an issue. In addition, the collection of simple sea-level measurements over many years is not perceived as an attractive activity: the payoff is distant, the technology is not glamorous, and the program is difficult to defend from the budget trimmers, who often mistrust long-term commitments. Nevertheless, in advancing our knowledge of the ocean's role in the global climate system, sea-level measurements remain important and effective.

Though the question of long-term climate change has been considered to be outside the scope of this study, a parallel need for ocean monitoring for this purpose as well is worth noting. In a recent study on the effects of atmospheric carbon dioxide chaired by Joseph Smagorinsky (Climate Board, 1982), an ocean monitoring system for early detection of climate change was recommended. The report states, "The operational monitoring of the ocean's response to climatic change may provide an early indication of climate change. Of particular value appear to be such indices as potential temperature and salinity changes on isopycnals in the wind-driven gyres."

EXPLORATORY TIME SERIES

To develop an effective ocean climate monitoring methodology, we first need estimates of the space and time spectrum of oceanic variability for many regions of the ocean. Further, trial time series can explore the possible benefits of and practical means for monitoring various regions of the ocean. Following the Tokyo Study Conference (CCCO, 1983), we call such short-term observational programs "exploratory time series."

An exploratory time series is intended to be of limited duration and to provide a first estimate of the climate-relevant oceanic variability. If appropriate in special cases, an exploratory time series may evolve into climate monitoring.

An inhibiting factor in the establishment of ocean monitoring has been the tendency to argue that once established, an ocean-monitoring time series should not be terminated. Because of the seemingly endless advance commitment this demands, there has been a reluctance to accept ocean monitoring. As a consequence, we may have been too cautious in collecting climate time series. We need a means to extend our knowledge of oceanic variability through time series measurements without feeling guilty about terminating the series. The answer: exploratory time series.

Exploratory time series should be designed to assure that the spectrum of variability is resolved, to examine the feasibility of observational techniques, and to assess the benefits that might be obtained from future monitoring. They should be geographically dispersed, incorporated into large-scale oceanographic experiments, and used as a preliminary to ocean climate monitoring. Research scientists will normally design and establish the exploratory time series and analyze and review the results. Ocean climate monitoring, on the other hand, will normally be an operational activity, just as monitoring now is in the atmosphere.

Although exploratory time series are a useful preliminary step, ocean climate research programs will need a reliable source of routine global data. There thus ultimately must be a commitment to ocean climate monitoring.

A MONITORING STRATEGY FOR NSF

The need for long time series of oceanic and atmospheric measurements to describe long-time-scale and intermittent climate anomalies poses a particular problem for NSF. The collection of long time series (monitoring) of ocean variability is hard for NSF to support in the face of competing proposals for short-term focused science. Furthermore, most oceanographers believe that NSF should not be supporting climate monitoring; that is seen as NOAA's role. Monitoring, as such, probably cannot be defended as an activity for creative scientists. Yet a commitment to monitoring is necessary if we are to obtain the description of the ocean that is needed for understanding and prediction. Such a commitment is likely to be made only by agencies other than NSF, whose mission involves environmental observation.

To encourage ocean climate monitoring, NSF should work closely with the other agencies (NOAA and NASA) whose responsibility it will be to carry it out. A possible first step would be to draw up a formal interagency plan. The research sponsored by NSF will need the base of observation that can be provided by monitoring. If NOAA and NASA are to commit to the development and operation of a monitoring system, they need assurance that it is needed and encouragement to act.

Recent experience indicates that a commitment to ocean climate monitoring may not be easy to achieve. The attempt should be made, however, since the need is great. The issue is broad enough that perhaps NSF should seek the involvement of the National Climate Program Office. Without the identification of ocean climate monitoring as a national need, it may be impossible to marshal the needed resources and to obtain the long-term commitment.

Some support should go as well to the development of techniques and instruments for ocean monitoring. Techniques should include means for measuring the annual cycle and interannual variability of the upper-ocean heat budget, for volumetric surveys of water-mass properties as a means for determining trends, and for measuring the fluxes of mass, heat, salt, and tracers associated with the circulation of water and its exchange with the surface layers. These techniques should be considered for development and use in ocean climate research experiments. As such, they could be supported by NSF. As new techniques develop for obtaining consistent observations over many years, NSF should work with other agencies, particularly NOAA, to encourage their operational implementation. This might be done in part by joint support of the operational use of prototype techniques in the final phases of development programs.

PILOT OCEAN MONITORING STUDIES

Pilot Ocean Monitoring Studies (POMS) is today more an acronym than a program. The idea came from a 1978 meeting (Global Atmospheric Research Programme (GARP), 1979) chaired by R.W. Stewart of Canada. A follow-up meeting on POMS a year later (GARP, 1980) considered a comprehensive range of topics: existing data bases, improving ocean circulation models, network design, large-scale experiments, altimetric and hydrographic surveys, national and institutional programs, instruments and methods, and data management. Subsequent to that meeting, however, significant additional steps have not been taken. Although ocean monitoring is generally conceded to be essential to obtaining a description of the ocean's role in climate variability, we are not moving toward the creation of an effective program, either nationally or internationally.

Internationally, the Integrated Global Ocean Service System (IGOSS), jointly sponsored by the Intergovernmental Oceanographic Commission (IOC) and the World Meteorological Organization (WMO), would appear to be the appropriate operational mechanism. IGOSS has worked to coordinate existing national ocean-observing programs. The progress here has been slow but real. IGOSS has not, however, sparked the development or even the planning of an ocean climate monitoring program. This may be because IGOSS still does not have a clear identity and objective, either in the minds of the sponsoring international agencies or in those of the member states. Some frustrated scientists have suggested setting up an ocean climate observing network independent of IGOSS, perhaps through bilateral arrangements or through the Scientific Committee on Oceanic Research (SCOR). Nevertheless, an intergovernmental framework will ultimately be needed for global climate monitoring, and this points us back to the IOC, the WMO, and IGOSS.

The United States has energetically supported IGOSS through the IOC and the WMO. Further increasing the level of American support would make our contribution even more lopsided and could be ineffective. Perhaps IGOSS could be invigorated to support ocean climate monitoring through a CCCO initiative. CCCO could sponsor a definition of the need for ocean climate monitoring, the implementation of a pilot network, and the use of the system to develop prototype products. If this is done in cooperation with IGOSS, it might encourage IGOSS to take over and run whatever is developed.

8 OTHER OCEAN CLIMATE RESEARCH ISSUES

So far this report has reviewed the large-scale programs that seem to have the blessings of national and international planners. In this chapter, some ocean climate studies that may fall outside this framework are first reviewed. Then the advantages of bilateral (as opposed to broad international) ocean climate research work with other countries are considered. The chapter concludes with a discussion of common observational and modeling needs.

OCEAN CLIMATE RESEARCH OUTSIDE THE BIG PROGRAMS

The large, internationally sanctioned ocean climate programs receive most of the attention, here as elsewhere. Yet a number of competent ocean scientists concerned with the climate variability problem are not convinced that they should work within the big programs. Oceanography has a tradition of independence. Some oceanographers interested in climate are reluctant to relinquish that independence in order to work within the large programs.

Ocean climate research is concerned, by definition, with global scientific questions. Can they be effectively addressed by independent studies? The easy course of action, and one that is not hard to defend, is to insist that oceanographers (and perhaps meteorologists) work together in addressing world climate and the world ocean. Nevertheless, many ocean scientists who lack the taste for big programs have good ideas with the potential to make progress in understanding climate. These ideas should not be excluded because their authors prefer not to work in big science. Whatever decisions are made about the big programs, NSF should continue to be flexible enough to support good ocean climate research ideas even when they are outside the "approved" framework.

Some oceanographers contend that global ocean climate planning is overblown and perhaps even unrealistic, that it does not take account of the difficulties in obtaining reliable data in the field, and that a better description of the ocean's structure and circulation is needed before we can move on to understanding the ocean's role in climate. They argue for simpler field programs.

There is concern too, that we must not begin a large costly global program before we are scientifically and technically ready to carry it off well. If we were to try prematurely and fail, it is likely that the funds to do it right would be a long time in coming.

A number of ideas for ocean climate research outside of the official programs have been presented that could improve our knowledge of climate variability. Some of these ideas may end up as components of the big programs (like WOCE) as the planning for these programs evolves. Among the ideas are the following:

* Make a few long, deep hydrographic sections intended to provide a base of information about interior low-frequency ocean movements and dynamics (see the discussion of the Long Lines program below).
* Maintain and perhaps extend island tide stations in the western Pacific Ocean.
* Maintain the Pacific XBT monitoring program, TRANSPAC (White and Bernstein, 1979).
* Carry out some small experiments to understand the physical processes that are important components in climate, such as air-sea fluxes of heat, water, and momentum.

Some ocean studies that may be important are less fashionable. Few oceanographers are studying the polar regions. Is the role of the ice-covered regions in climate variability receiving enough attention? This question has been reviewed (ICEX Science and Applications Working Group, 1979), but there has been little follow-up.

Some large-scale oceanographic studies have been developed outside the climate program, and their results could benefit our understanding of climate variability. In a sense, most of physical oceanography could be tied to climate research. We have to decide where to draw the line. By including too much under the climate umbrella, we risk weakening the focus of the ocean climate research program. Yet we should note the importance to climate research of many ongoing and proposed ocean research programs, such as the following:

<u>Transient Tracers in the Ocean</u> (TTO) has as its primary objective to determine the flux of fossil fuel carbon dioxide into the ocean and to predict its distribution through an understanding of ocean circulation.

Using chemical tracers such as tritium (hydrogen-3) and carbon-14, TTO will study ocean circulation and mixing. The results should test the ability of numerical ocean circulation models to predict ocean transport on time scales of decades and longer.

Studies of climate variability on interannual time scales will benefit from an enhanced understanding of ocean circulation. TTO should improve our estimates of ocean heat flux. For example, we should learn more about whether or not a two-gyre circulatory system exists in the western North Atlantic, about the role of the western boundary undercurrent in exchanging water with the Sargasso Sea, and about the heat and mass interchanges between the North Atlantic and the atmosphere.

TTO is not being proposed as an activity within the climate research program. It does, however, hold promise as ancillary research. As WOCE plans are developed, it may be evident that chemical tracers are an essential tool for understanding ocean circulation. TTO and WOCE should be coordinated, and NSF should be aware of the added climate research benefits when evaluating the TTO proposals.

Long Lines is the name given to an informal proposed program of hydrographic station sections. Not enough is known of the distribution of temperature, salinity, and density in many areas of the ocean. We are unable to describe the large-scale circulation. We are particularly weak in meridional (i.e., north-south) sections that would allow us to describe the subsurface zonal (i.e., east-west) circulation. Our ignorance may even lead our models astray, since most existing sections are zonal.

Even a few meridional sections, if well located, might resolve some questions of ocean circulation: Does the Gulf Stream extend east of the Mid-Atlantic Ridge? Does the Gulf Stream recirculation begin in the eastern North Atlantic? What circulation produces the observed tongue of salty Mediterranean water that extends westward into the North Atlantic? We do not now have answers to these questions even though they concern the North Atlantic, the ocean we know most about.

The sections that have been suggested for Long Lines are more numerous in the Southern Hemisphere than in the Northern Hemisphere; there are fewer existing data there.

If a Long Lines program is begun, it will contribute to our fundamental knowledge of the large-scale physical oceanography of the world ocean. NSF probably will want to evaluate proposals on this basis. However, the program also could have an important benefit to our understanding the role of the oceanic general circulation in climate variability. Judged solely on the criteria of contributions to the climate research program, Long Lines might not have high priority. If, however, it is judged to be important to our knowledge of the physical structure and circulation of the ocean, the additional benefit to the climate research program should be weighed in assigning priorities.

WORKING WITH OTHER COUNTRIES

Ocean climate research activities of other countries have the potential of adding to the American effort. In some cases, cooperative work with other countries can produce benefits to the United States that are greater than the incremental expense.

French programs in the Atlantic and the Indian oceans have already been mentioned. Other countries are active in ocean research programs that augment or could augment American efforts. A comprehensive catalog of these is beyond the scope of this report, but a few examples may serve to indicate what is possible.

Japan has carried out regular ocean surveys for many decades. They have developed a number of products and services based on these surveys. Temperature, salinity, and velocity fields within about 500 km of the Japanese coast are routinely mapped. Meridional sections along 137°E and 155°E have been collected twice each year for the past decade. This series may be of value in estimating interannual ocean heat transport in the western Pacific.

Japan has the capability for first-rate ocean measurements. Two programs, the Ocean Heat Transport Experiment (OHTEX) and the Ocean Mixed Layer Experiment (OMLET), are being planned as contributions to the climate research program. American programs in the Pacific will benefit from collaboration with them.

The Soviet Union has been pushing for more than a decade for a program to make repeated oceanographic observations at fixed locations or sections. Though the rationale for their proposals has changed over the years, the substance has remained the same. As part of the climate research program, they propose a program known as the "Energetically Active Zones of the Ocean" (EAZO), also known under its older name of "Sections."

The Sections program has a number of attractive features: among them, four-times-a-year observations of dynamically active regions of the ocean. The Soviet Union has a research vessel capability that is enormous by the standards of any other country. Two of their locations ("polygons") are located off North America, in the Gulf Stream and off Newfoundland. It would thus seem to be to our advantage to cooperate with them.

Oceanographers in many countries have been reluctant to endorse the Soviet proposals. There are a number of reasons for this. Soviet oceanographers have a reputation for poor quality control of their data. In particular, their measurements of salinity and currents have often proved too poor for use. Western scientists find that working with Soviet colleagues is difficult because of their inability or unwillingness to communicate. Finally, there is widespread skepticism about Soviet motives.

At the moment, the EAZO program is carried as a "national program" within the climate research program (CCCO, 1983). If the Soviet Union were to show that it can collect reliable data at regular intervals, the attitude of American oceanographers would likely change. If that should happen, it could be to the advantage of the United States to work with the USSR.

The Federal Republic of Germany, the United Kingdom, and Canada are active in the North Atlantic. U.S. scientists generally find it easy to work with scientists from these countries, and there is consequently little formal intergovernmental structure. Cooperative climate programs with these and other countries should not be overlooked, however, since the benefits of working with competent scientists from these countries are great.

9 NATIONAL AND INTERNATIONAL COORDINATION

Most of the U.S. research in ocean climate will be supported by agencies of the U.S. government. The National Science Foundation is the lead agency for the Ocean Heat Transport and Storage principal thrust of the National Climate Program. (NSF's responsibilities in this role are summarized below.) This is the only ocean-oriented principal thrust in the National Climate Program and one of two dealing with research. NSF thus is the only agency having a specified lead designation for ocean climate research. This report, addressed to NSF, is intended to guide NSF in that role.

The ocean research program that should be implemented to understand climate change is too large to be supported by a single agency of the U.S. government. Several agencies will have important roles to play. However, a review of climate documents that have emanated from the agencies reveals that agencies often do not have a clear image of their role. One sometimes gets the impression that no clear criteria have guided an agency's choice of work. In this chapter, suitable agency roles in ocean climate research will be considered, as well as how those roles might fit together to provide a coherent national ocean climate research program. In addition, the agencies must develop a balanced program of support for ocean climate research, based on the views of a broad spectrum of the oceanographic community.

The United States will play a key role in the World Climate Research Program, and will likely support a considerable proportion of the major ocean climate programs. This chapter concludes with a review of international coordination, with some evaluation of possible U.S. positions.

NATIONAL CLIMATE PROGRAM OFFICE

The National Climate Program Office (NCPO), housed in NOAA, is responsible for administering the National Climate Program and assuring coordination among the agencies in the program. NCPO looks to NSF, as lead agency, for development of plans, budget requirements, agency responsibilities, and progress reports related to the Ocean Heat Transport and Storage principal thrust.

The role of the NCPO may be changing: there are signs that NCPO will play a more active role in coordinating the climate program among the agencies and in developing a focus for the fragmented climate program in NOAA. Research is only one aspect of the National Climate Program, and the NCPO thus has responsibility for maintaining a balance among the program components.

NATIONAL SCIENCE FOUNDATION

The NSF, as lead agency for Ocean Heat Transport and Storage, has the de facto responsibility for oversight of the national ocean climate research program. NSF has been doing this through informal meetings with representatives of other agencies and by making extensive use of the National Research Council (the Board on Ocean Science and Policy and the Climate Research Committee in particular). To date this has worked. If problems arise that involve the setting of priorities among the agencies, it may be necessary to set up a more formal steering mechanism.

NSF faces a problem in meeting its responsibilities as lead agency. All the lead agencies in the National Climate Program have had difficulty in coordinating their components. There is thus no good model for NSF to follow. NSF is the largest supporter of ocean climate research and thus has some credibility with the other agencies in its lead role.

Coordinating and leading a group of agencies is not something that NSF is accustomed to doing. In order to accomplish this, NSF may wish to consider having the coordination carried out by an outside organization, such as an oceanographic institution or a corporate body, such as the University Corporation for Atmospheric Research (UCAR) or the Joint Oceanographic Institutions (JOI), Incorporated. If this is done, the costs will increase without showing clear scientific return. No matter what procedure is used in coordinating ocean climate research, the expense of coordination should be faced. The National Climate Program is large and complex. If it is to succeed, the component programs must be coordinated. The issue of paying for this coordination should be addressed by the lead agencies and the National Climate Program Office.

NSF research programs related to climate have typically involved collaborative research projects from a number of institutions. These programs may have a duration of from 3 to 5 years. Such a mode of operation tends to yield results that respond to specific scientific questions but is not well suited to programs that require a continuing year-after-year commitment. Long-term programs need to be part of a climate research program, and hence there is a need for other agencies that can support them to play a role complementary to that of NSF.

NATIONAL OCEANIC AND ATMOSPHERIC ADMINISTRATION

The National Oceanographic and Atmospheric Administration (NOAA) has been supporting a substantial ocean climate research program (Environmental Research Laboratories, 1979). NOAA programs include the Equatorial Pacific Ocean Climate Studies (EPOCS), the Subtropical Atlantic Climate Study (STACS), and oceanographic components of the Global Atmospheric Research Program (GARP). NOAA also has the lead responsibility for the U.S. TOGA Program.

In addition to carrying out ocean climate research, NOAA has other responsibilities that are important to the climate program. NOAA is the lead agency for the principal thrust of the National Climate Program entitled "Generation and Dissemination of Climate Information." NOAA's Environmental Data and Information Service runs the National Climatic Center that manages meteorological data and the National Oceanographic Data Center that manages oceanographic data. As the climate program progresses, the management of data and information will be a factor in its success. Thus these elements of NOAA need to be involved in the planning for large ocean climate experiments.

NOAA's National Ocean Service (NOS) has responsibility for ocean monitoring. To date, NOS has exercised that responsibility chiefly in conventional mapping and charting activities. They have missed opportunities to support monitoring useful to ocean climate, such as the Pacific tide gauge network. A global study of the ocean's role in climate demands reliable ocean observations, analogous to those we take for granted in the atmosphere. NOS ought to be working toward developing an ocean service on a par with the atmospheric service provided by the National Weather Service. Although NOS has not so far given a high priority to developing this capability, perhaps the recent creation of the National Ocean Service, from what had been the National Ocean Survey, will lead NOAA, through NOS, to accept responsibility for the needed ocean climate monitoring. The climate program may provide an additional stimulus to NOS. NSF should, perhaps through the NCPO, make known the climate research need for ocean monitoring.

NOAA has given some priority to research that could improve the operational capabilities of its service elements. NOAA is the lead agency for the Climate Prediction principal thrust of the National Climate Program. Thus NOAA has supported some research that might lead to a predictive capability. NSF should maintain close ties with NOAA program managers to be sure that programs are complementary.

NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

The National Aeronautics and Space Administration (NASA) has the goal of developing spaceborne techniques for observing the ocean and thereby advancing our understanding of oceanic behavior. This objective embraces considerable activity related to ocean climate research. NASA's spaceborne oceanic observations are intended to study oceanic circulation, heat content, and heat flux. Such work involves the interaction of the ocean with the atmosphere and the effect of the ocean on climate.

NASA has focused on defining scientific questions addressable by specific earth-orbiting satellite oceanographic sensors. They have commissioned a series of studies that, though not specifically directed to ocean climate research, provide a valuable summary of satellite oceanographic capabilities and needs. The long-term ocean sciences satellite program was reviewed by the National Oceanographic Satellite System Science Working Group, chaired by Francis Bretherton (Ruttenberg, 1981).

Other studies completed or under way consider altimetry for studying ocean circulation (TOPEX Science Working Group, 1981), synthetic aperture radar for studying sea ice and surface waves in the open ocean (Jet Propulsion Laboratory, 1982), scatterometers for studying wind stress (Satellite Surface Stress Working Group, 1982), color scanners and their value in studying primary production (Ocean Color Science Working Group, 1982), and the type and quality of in situ observations needed to complement satellite ocean observations (in preparation). An earlier NASA study chaired by W.J. Campbell (ICEX, 1979) set forth the need for an Ice and Climate Experiment.

A World Ocean Circulation Experiment (WOCE) will depend critically on remote sensing by satellite of sea-surface elevation, surface wind stress, and meteorological variables. Thus something like the TOPEX satellite, with altimeter and scatterometer for global sensing of surface ocean currents and surface wind stress, is essential for WOCE. This should be supported by sea-surface temperature sensing by radiometer and satellite data links to in situ ocean instruments. To date NASA has not made a decision on this program, and the uncertainty is a major deterrent to the development of U.S. plans for WOCE.

NASA has lead agency responsibility for the principal thrust in Solar and Earth Ra iation. An Earth Radiation Balance Experiment (ERBE) wil be an essential part of ocean-atmosphere heat flux studies such as Cage. Though there are plans for an ERBE experiment in the near future, it may have been completed by the time the oceanographers are ready to carry out an ocean heat flux experiment that will need the ERBE measurements.

NASA should play a key role in the planning for and execution of large-scale ocean climate experiments. NSF must work with NASA on the timing of large-scale programs for which satellites are essential. In particular, WOCE and heat flux planning must take satellite availability and performance into account.

OFFICE OF NAVAL RESEARCH

The Office of Naval Research (ONR) does not now explicitly support ocean climate research. ONR does, however, support a number of process studies, particularly at the air-sea interface and in the surface mixed layer, that are relevant to climate. For example, work supported by ONR may be important in resolving the question of the sea-surface water-vapor flux. Our current understanding of ocean-atmosphere climate interaction owes a great deal to the results of the NORPAX program, which was supported for many years by ONR. ONR is also supporting the development of techniques in remote sensing that have direct application to ocean climate research experiments. Furthermore, naval operational activities need environmental information of the type that is important to climate research.

ONR has shown a willingness to work with other agencies in ocean climate planning, but it may not choose to accept a formal role in planning and coordinating ocean climate research. NSF, in its lead role, should maintain regular informal contacts with ONR, to ensure that relevant ONR-sponsored research is incorporated into ocean climate studies.

NATIONAL COORDINATION

An important ingredient in the practical implementation of the large-scale ocean climate research programs is a consensus from U.S. oceanographers that the experiments can be done and should be done. This is particularly needed for WOCE, since the definition of WOCE's objectives is still incomplete. A commitment by capable scientists to participate and to see that the experiments are successful is also needed. Without the consensus and the commitment, the federal agencies find it difficult to develop the new funding needed for supporting these experiments.

One necessary step in developing a consensus is to allay the fears of many oceanographers that all new funds will go to the large programs, like WOCE. This concern needs to be addressed. The federal agencies, and particularly NSF, must be involved. The climate program advocates in the scientific community cannot assure their colleagues in other ocean research disciplines that a proper balance will be found. Those controlling the money must give this assurance. Here is an opportunity for program managers in NSF and other agencies to seek the opinions of oceanographers of all stripes, not just those with climate research interests. What should be the appropriate balance of support for these programs? What is the view of biological and geological oceanographers (for example) about the need for strong support of ocean climate research? Answers to such questions might be sought through National Research Council committees.

INTERNATIONAL COORDINATION

U.S. work on global ocean climate studies can be augmented and supported through cooperation with other countries. The details of this cooperation have already been given in the chapters of this report that discuss the scientific programs. In this chapter the international framework within which planning is taking place is summarized.

International ocean climate research is a component of the World Climate Research Program (WCRP). The WCRP is jointly sponsored by the World Meteorological Organization (WMO) and the International Council of Scientific Unions (ICSU). Ocean climate research is coordinated by the Committee on Climatic Changes and the Ocean (CCCO) under the joint auspices of the Intergovernmental Oceanographic Commission (IOC) and the Scientific Committee for Oceanic Research (SCOR) of ICSU. CCCO in turn maintains liaison with the Joint Scientific Committee (JSC) of the WCRP. The CCCO was established to give the oceanographers an organization that is parallel to the JSC for the WCRP.

The United States has played a strong role in developing international plans for ocean climate research, and there is every indication that this will continue. We should not let the international planning dictate the content of the U.S. program, however. If we are aware of and participate in the international planning, we should benefit from the supporting research work that will be done by other countries in cooperation with us. NSF does not have explicit international responsibilities in ocean climate research, but should work through U.S. delegations and other bodies to ensure that the scientific point of view is considered in international planning.

The United States should be sensitive to the need for the CCCO to maintain independence from the meteorological (WMO) and oceanographic (IOC) operational organizations. Both offer the promise of a way to do things more efficiently. However, the oceanographers need an organization that can plan and implement an international research program and that draws upon both the research and the operational sector. The CCCO may develop into such an organization. In these early stages, as CCCO still is trying its wings, the United States should make the effort to support the CCCO as an independent entity by maintaining the SCOR link.

10 CONCLUSIONS

The recommendations for NSF action or strategy that are contained in this report are summarized here by chapter:

THE INTERANNUAL VARIABILITY OF THE TROPICAL OCEAN AND THE GLOBAL ATMOSPHERE (Chapter 4)

NSF should support TOGA planning with the possibility of U.S. participation in a large-scale international oceanographic and atmospheric experiment. The scale of the program will require strong coordination between NSF, NOAA, and NASA.

The first stage of TOGA should focus on answering basic questions as a prelude to a full-scale program. We need a physical and theoretical framework in order to design a full TOGA experiment with assurance.

The extent of U.S. participation in an international TOGA needs to be clarified. U.S. plans are proceeding for a Pacific-based program to study the Southern Oscillation. The United States should also participate with other countries in the Atlantic and Indian ocean parts of TOGA to complement the Pacific studies.

THE WORLD OCEAN CIRCULATION EXPERIMENT (Chapter 5)

Planning for WOCE is still in an early stage. NSF should support research to aid in planning for WOCE, though a full commitment should await a more complete definition of the program.

An earth-orbiting satellite to measure sea-surface topography and wind stress is an essential element of WOCE. There are no U.S. plans to launch such a satellite in this decade. NSF senior management should discuss the need for spaceborne oceanic measurements with NASA and NOAA. A commitment to an observational satellite is needed if we are to go ahead with WOCE.

HEAT TRANSPORT STUDIES (Chapter 6)

NSF should support heat flux studies within the TOGA and WOCE programs, or possibly as precursors to them. This should include opportunities for pairwise intercomparisons of techniques for heat flux estimation. NSF should consider supporting a trans-Pacific Ocean poleward heat flux measurement when a suitable proposal appears. NSF should also support studies to examine the extent to which we are able to estimate heat and energy fluxes in the ocean and atmosphere. If the GINS Cage experiment is seriously proposed, NSF should insist that the issue of the role of polar heat fluxes be resolved as part of the justification.

OCEAN CLIMATE MONITORING (Chapter 7)

NSF should seek the involvement of the National Climate Program Office to identify ocean climate monitoring as a national need and to obtain a long-term commitment to it. Then NSF should work closely with other agencies (NOAA and NASA) to establish an ocean monitoring system that will meet the needs of climate research programs.

OTHER OCEAN CLIMATE RESEARCH ISSUES (Chapter 8)

A number of valuable ocean climate studies fall outside the currently approved national and international framework. NSF should remain flexible enough to support good ocean climate research ideas even when they are outside the "approved" framework.

NSF should watch for additional benefits to the climate research program in the other oceanographic research proposals it receives, and, if possible, these benefits should be considered in reviewing and assigning priorities.

NATIONAL AND INTERNATIONAL COORDINATION (Chapter 9)

NSF should do the following:

1. Supply the National Climate Program Office (NCPO) with plans, budgets, interagency coordination and progress reports related to the Heat Transport and Storage principal thrust.

2. Establish ties with NOAA, NASA, and ONR to ensure that research related to ocean climate is complementary.

3. Consider establishing an ocean climate research co-ordinating center, possibly at an oceanographic institution or by using a corporate body such as UCAR or JOI.

4. Define, possibly through the NCPO, research needs for ocean monitoring that could be supplied by NOAA.

5. Work with NASA to define needs and establish timing for ocean climate research satellites.

Though it does not have specific international responsibilities, NSF should work through the appropriate U.S. delegations and bodies to do the following:

1. Support the CCCO as a body independent of both oceanographic (IOC) and meteorological (WMO) operational organizations.

2. Urge the establishment of a technically capable program office for WOCE.

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APPENDIX A: ACRONYMS

CCCO: Committee on Climatic Changes and the Ocean
CRC: Climate Research Committee (of the National Research Council)
EAZO: Energetically Active Zones of the Ocean Program (of the USSR)
ENSO: El Niño and the Southern Oscillation Experiment
EPOCS: Equatorial Pacific Ocean Climate Studies
ERBE: Earth Radiation Budget Experiment
FGGE: First GARP Global Experiment (also known as the Global Weather Experiment)
FOCAL: Programme Français Océan Climat Atlantique Equatorial (of France)
GARP: Global Atmospheric Research Program
GINS Cage: Greenland, Iceland, and Norwegian Seas Cage Experiment
HAIKU: Heat Advection Investigation in the Kuroshio
ICEX: Ice and Climate Experiment
ICSU: International Council of Scientific Unions
IGOSS: Integrated Global Ocean Services System (of the IOC and WMO)
IOC: Intergovernmental Oceanographic Commission (of the United Nations Educational, Scientific, and Cultural Organization)
JOI: Joint Oceanographic Institutions, Incorporated
JSC: Joint Scientific Committee for the World Climate Research Program and the Global Atmospheric Research Program
NASA: National Aeronautics and Space Administration
NCAR: National Center for Atmospheric Research
NCPO: National Climate Program Office
NOAA: National Oceanic and Atmospheric Administration (of the U.S. Department of Commerce)
NORPAX: North Pacific Experiment
NOS: National Ocean Service (of NOAA)
NRC: National Research Council
NSF: National Science Foundation

OACIS: Ocean Atmosphere Climate Interaction Studies
OHTEX: Ocean Heat Transport Experiment (of Japan)
OMLET: Ocean Mixed Layer Experiment (of Japan)
ONR: Office of Naval Research (of the U.S. Department of Defense)
OSB: Ocean Sciences Board (of the National Research Council)
OSC: Ocean Science Committee (of the National Research Council)
PATHS: Pacific Transport of Heat and Salt Experiment
PEQUOD: Pacific Equatorial Ocean Dynamics
POMS: Pilot Ocean Monitoring Studies
SCOR: Scientific Committee on Oceanic Research (of the International Council of Scientific Unions)
SEQUAL: Seasonal Response of the Equatorial Atlantic
SINODE: Surface Indian Ocean Dynamics Experiment (of France)
STACS: Sub-Tropical Atlantic Climate Study
TOGA: Tropical Ocean Global Atmosphere Experiment
TOPEX: Ocean Topography Experiment
TRANSPAC: North Pacific Ocean ship of opportunity XBT monitoring program
UCAR: University Corporation for Atmospheric Research
WCRP: World Climate Research Program
WMO: World Meteorological Organization
WOCE: World Ocean Circulation Experiment

APPENDIX B: DEFINITIONS

According to the U.S. Committee for the Global Atmospheric Research Program (1975), climate and climate change may be defined as follows:

Climate state. This is defined as the average (together with the variability and other statistics) of the complete set of atmospheric, hydrospheric, and cryospheric variables over a specified period of time in a specified domain of the earth-atmosphere system. The time interval is understood to be considerably longer than the life span of individual synoptic weather systems (of the order of several days) and longer than the theoretical time limit over which the behavior of the atmosphere can be locally predicted (of the order of several weeks). We may thus speak, for example, of monthly, seasonal, yearly, or decadal climate states.

Climate variation. This is defined as the difference between climate states of the same kind, as between two Januaries or between two decades. We may thus speak, for example, of monthly, seasonal, yearly, or decadal climate variations in a precise way. The phrase "climate change" is used in a more general fashion but is generally synonymous with this definition.

Climate anomaly. This we define as the deviation of a particular climate state from the average of a (relatively) large number of climate states of the same kind. We may thus speak, for example, of the climate anomaly represented by a particular January or by a particular year.

Climate variability. This we define as the variance among a number of climate states of the same kind. We may thus speak, for example, of monthly, seasonal, yearly, or decadal climate variability. Although it may be confusing, this definition of climate variability includes the variance of the variability of the individual climate states.