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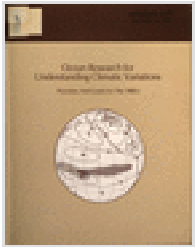

Ocean Research for Understanding Climatic Variations: Priorities and Goals for the 1980's (1983)

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Ocean Research for Understanding Climatic Variations

Priorities And Goals For The 1980's

Steering Committee for Ocean/Climate Research Strategies
Ocean Sciences Board
Commission on Physical Sciences, Mathematics, and Resources
National Research Council

Cover: Hypothesized global atmospheric response to sea-surface temperature anomalies associated with El Niño events in the equatorial Pacific during the Northern Hemisphere winter, based on observational studies and experiments with general circulation models. The hatched area denotes the region where sea-surface temperatures are in excess of 1° above normal; the shaded area denotes the region of enhanced precipitation and the contours show geopotential height anomalies at the jetstream level. [After Horel and Wallace (1981) and Rasmussen and Carpenter (1982)]

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NOTICE: The project that is the subject of this report was approved by the Governing Board of the National Research Council, whose members are drawn from the councils of the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine. The members of the committee responsible for the report were chosen for their special competences and with regard for appropriate balance.

This report has been reviewed by a group other than the authors according to procedures approved by a Report Review Committee consisting of members of the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine.

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TABLE OF CONTENTS

PREFACE xi

FOREWORD xiii

1. SUMMARY AND RECOMMENDATIONS 1

An Overall Strategy 2
The Global Ocean Circulation 2
Tropical-Ocean/Global-Atmosphere Interactions 3
Regional Process Studies 3
Long-Time Series Measurements 4
Paleoclimate Studies 5
Tools, Techniques, and Management 5

2. INTRODUCTION AND BACKGROUND 7

2.1 The Ocean in the Climate System 7
2.2 Ocean and Climate Research in the Past Decade 9
2.3 The National Climate Program 16
2.4 The World Climate Program 17
2.5 Future Directions 18

3. SCIENTIFIC ISSUES AND RECOMMENDED RESEARCH 21

3.1 General Ocean Circulation 22
3.2 Large-Scale Air-Sea Interaction in the Tropics 28
3.3 Ocean Heat Transport 33
3.4 Polar Ocean and Ice Interactions 37
3.5 Ocean Climate Through the Sediment Record 39
3.6 Schedule for Programs 42

4. TECHNIQUES, TECHNOLOGY, AND DATA MANAGEMENT 45

4.1 Long Time Series of Ocean Measurements 45
4.2 Empirical Data Analysis 47
4.3 Ocean Modelling for Climate Research 48
4.4 New Technology for Measurements 48
4.5 Requirements for Satellite Data Management 50

5. REFERENCES 53

LIST OF TABLES

TABLE 1 Documents on Ocean Research For Understanding Climate 10

TABLE 2 Proposed and On-Going Field Programs Related to Ocean Climate Studies 43

LIST OF FIGURES

FIGURE 1 The Ground Track Traced Out by a Satellite at an Altitude of 1300 km and With an Orbital Inclination of 64° During a Ten-Day Period of an Exactly Repeating Ten-Day Cycle 27

FIGURE 2 Recommended Lines for Closely Spaced Hydrography Superimposed on a One-Day path of a Satellite 27

FIGURE 3 The Southern Oscillation. Correlation of Annual Mean Atmospheric Pressure with Jakarta (after Berlage) 29

FIGURE 4 The Southern Oscillation as Given by the Differences of Atmospheric Pressure Between Easter Island and Darwin, Australia from 1949 to 1978 Relative to a Mean of 10.3 Millibars 29

FIGURE 5 Hypothesized Global Atmospheric Response to Sea-Surface Temperature Anomalies Associated with El Nino Events in the Equatorial Pacific During the Northern Hemisphere Winter 31

FIGURE 6 Boundaries for a Greenland-Iceland-Norwegian Sea Cage 40

PREFACE

Ocean Research for Understanding Climatic Variation - Priorities and Goals for the 1980's was prepared to start a dialogue among ocean scientists on the steps that must be taken in order to advance significantly our understanding of the role that the oceans play in climatic variation, and to identify ocean research needed in the context of the developing national and international climate research plans.

This report reviews and identifies the successes of the 1970's in ocean research programs related to climatic variation, and outlines the scientific issues to be addressed including techniques, technology, and data management with recommendations for new programs. It does not develop a detailed national strategy in the strict sense. The complexity of the interactions between the ocean and the atmosphere and our ignorance of the precise role that the ocean plays in the climate system means that it is not yet possible to devise a plan similar to that prepared for the Global Weather Experiment. However, major goals, many of the required elements, and some priorities are identified. The development of a detailed strategy is a longer-term objective.

Although this report was drafted by the committee chairman, it is the responsibility of the Steering Committee for Ocean/Climate Research Strategies. It reflects discussions at committee meetings, reports of national and international meetings on climate research strategy, and recent scientific literature. We gratefully acknowledge the contributions of the following individuals to the indicated sections: General Ocean Circulation--C. Wunsch and W.D. Nowlin, Jr.; Heat Storage and Transport--H. Bryden; Air-Sea Interaction--T. Barnett, J. Namias, P. Niiler, J. O'Brien, and J.M. Wallace; Polar Ocean and Ice Interactions--W.D. Nowlin, Jr., S. Martin, N. Untersteiner, and J.M. Wallace; the Sediment Record--J. Imbrie; Empirical Data Analysis--T. Barnett; and Long Time Series--F. Webster. We also appreciate the contributions of F. Bretherton, C. Collins, S. Hayes, J. Imbrie, J. McWilliams, F. Webster, W. Wooster, and V. Worthington. Financial support for this study was provided by Program Initiation Funds of the National Research Council and by additional support from the National Climate Program Office of the National Oceanic and Atmospheric Administration and the National Science Foundation.

D. James Baker, Chairman
Steering Committee on Ocean/Climate
Research Strategies

FOREWORD

Climatic variation is of major importance to all nations. It is now clear that the ocean plays a major role in that variation. In order to understand that role, a major program of basic research is necessary. This report, Ocean Research for Understanding Climatic Variation - Priorities and Goals for the 1980's, sets forth goals for such research programs and identifies research needs at national and international levels. It is intended for use by the ocean science community and federal agencies concerned with the oceans and with climate.

This report is an example of one way that the Ocean Sciences Board has identified requirements for future developments in ocean science and examined the limitations on these developments in terms of human and technical resources. Other examples of reports that have helped develop strategies for large-scale ocean research programs commensurate with critical scientific questions are An Oceanic Quest, 1969 and The Continuing Quest, 1979.

There is a continuing need to clarify and propose strategies to address important and exciting scientific problems as well as to define the specific information requirements and practical limitations of such strategies. A reasonable balance between these intrinsic and extrinsic aims must be maintained. I am confident that the newly established Board on Ocean Science and Policy will consider these aims in terms of broad and basic questions in oceanography as well as in the context of particular federal agency requirements.

I am grateful to the Steering Committee for Ocean/Climate Research Strategies for the preparation of this report.

John H. Steele, Chairman
Ocean Sciences Board

1. SUMMARY AND RECOMMENDATIONS

Understanding climatic variation is of major importance to all nations. The ocean plays a critical role in climatic variation and, in order to understand that role, a major program of basic research is necessary. This report, Ocean Research for Understanding Climatic Variation-Priorities and Goals for the 1980's, (1) summarizes the current state of understanding of how the ocean interacts with the atmosphere to cause climatic variation, (2) describes the World Climate Research Program and its oceanographic components, (3) identifies the major elements and goals of a United States research program to address unsolved issues, and (4) recommends a set of major research programs. In doing so, it focuses on the ocean research necessary to understand the role of the ocean in climatic variation on time scales of months to centuries. This range of time scales is consistent with the plan for the World Climate Research Program (WCRP) which covers those periods from a few weeks (where the ocean may be considered as a quasi-steady boundary condition to the atmosphere) to decadal and longer time scales (where the atmosphere and the ocean must be considered as a self-consistent, coupled system).

We, the authors of this report, have identified and provided recommendations on an overall strategy and on five research objectives. The research objectives are:

(1) to describe the global ocean circulation and understand the processes necessary for its maintenance and variability;

(2) to study the tropical-ocean/global-atmosphere interaction, initially focussing on the Pacific Ocean;

(3) to conduct regional process studies of heat transport in the ocean and of air-sea transfers;

(4) to establish new and to continue existing stations for long time series of ocean measurements in order to support the studies of global circulation, tropical air-sea exchanges, and regional processes; and

(5) to study paleoclimate data that will provide climate information on periods from decades to centuries. In addition and more generally, we have addressed the needs for tools and techniques. Much of the new research will depend critically on the availability of satellite-based measurements of the ocean.

o An Overall Strategy

An overall strategy to advance understanding of the ocean's role in climate variation is needed to guide and focus the efforts of the concerned scientific and federal communities.

We recommend that the Board on Ocean Science and Policy, in cooperation with the Board on Atmospheric Science and Climate of the National Research Council, develop such a strategy, based on a consensus within the scientific community as to the priorities for investigating the ocean's role in climatic variations and working in cooperation with federal agencies and others as appropriate (page 19).

o The Global Ocean Circulation

We endorse the basic concept of a World Ocean Circulation Experiment (WOCE) (page 22) and note the urgent need for a U.S. commitment now to the required satellite measurements of ocean surface topography and surface wind stress. As critical parts of WOCE

we recommend that the elements of the proposed ocean Topography Experiment (TOPEX) program, including spaceborne altimeter, scatterometer, and geoid measurements, and the necessary in situ measurements from drifting and moored arrays be supported. In addition, we recommend a five-year mission life-time in order to obtain as much information as possible on interannual variability (page 25).

To support these recommendations, we stress that understanding the general circulation of the ocean is fundamental to understanding the ocean's role in climatic variation. The basic parts of a global circulation study are satellite altimetry, ocean density measurements, and in situ measurements of sea level and velocity by conventional techniques and new acoustic averaging techniques. Much work must be done before such a global study can be undertaken, but the required planning now will help assure that future regional programs contribute to the broader, global ocean study.

The existing measurement technology and projected resources (both national and international) point towards a satellite-based global experiment of five-year's duration that could take place as early as the late 1980's. National planning is already underway for an Ocean Topography Experiment (TOPEX), and international planning for the World Climate Research Program, including a World Ocean Circulation Experiment (WOCE) that would incorporate a number of national satellite programs. Thus, we strongly endorse the national plans for altimetry, scatterometry, and geoid measurements from space.

As part of the global circulation study, measurements are required to complete the basic physical and chemical description of the ocean, particularly temperature, salinity, density, and chemical tracer measurements. In addition, arrays of moored instruments will be required to monitor density changes. Because of its promise for monitoring ocean density, (a major need in describing the general ocean circulation as part of WOCE), we recommend the continued development of the technique of acoustic tomography (page 26). In addition, we recommend that hydrographic sections be taken in areas of the world ocean that are currently undersampled because such sections are crucial to describing the general circulation (page 26).

Chemical tracers, particularly tritium, have demonstrated the extent of exchange between upper and lower ocean layers. However, further study of mixing processes and time scales involved is required in order to understand how the ocean responds to increasing atmospheric carbon dioxide. Thus, we recommend that the chemical tracer measurements proposed by the Transient Tracers in the Ocean Study be carried out (page 28), and urge that liaison be maintained between the planning for these studies and the on-going planning for the WOCE.

o Tropical-Ocean/Global-Atmosphere Interactions

The strong climatic signal in the tropical oceans and the recent developments in the understanding of possible links between the tropical ocean and the global atmosphere indicate that a large-scale, atmosphere-ocean study in the tropics should be undertaken soon. Previous and current studies focusing on the Southern Oscillation/El Niño phenomenon have shown that the atmospheric effects are global. Therefore, a global scale study must be carried out eventually. Such a global-air sea interaction study must include a study of the origin and dynamics of sea-surface temperature anomalies and their effects on atmospheric forcing. The continuation and extension of existing studies together with large-scale measurements of ocean and atmospheric circulation, sea-surface temperature, heat-storage, and air-sea exchange in the tropical and subtropical Pacific would be essential elements of such a large-scale study. Satellite measurements of wind stress by scatterometry are crucial and satellite-based altimetry studies discussed above for WOCE would be an important and unifying feature. United States planning to date has been central to the development of international interest in a Tropical Ocean/Global Atmosphere (TOGA) study. Thus, recognizing the importance of the link between the interannual variability of the atmospheric climate and the tropical ocean, we endorse the concept of a large-scale tropical Pacific ocean-atmosphere study, note the importance of satellite wind stress measurements, and we recommend that full support be provided for the on-going planning for such a study (page 32).

o Regional Process Studies

There is a growing recognition of the importance of ocean heat transport in the global heat balance. Elements of a program to investigate ocean heat transport include: measurements for estimation of ocean heat flux in each ocean, studies of the variability of ocean heat transport across one section, and studies of the mechanisms of ocean heat transport. Process studies in the North Atlantic, North Pacific, and in the Antarctic Circumpolar Current are identified. Such studies include satellite measurements of winds, sea surface temperature, and air sea transfers, as well as more conventional in situ oceanographic measurements. There are on-going international discussions sponsored by the WCRP on the feasibility of large-scale budget experiments.

We endorse the general concept of a program to understand processes of heat transport in the ocean, and stress that the elements of such a program could be carried out now. Therefore, and because of the close connection between circulation and heat transport, we recommend that further planning for large-scale studies be carried out in the context of and in collaboration with the WOCE planning (page 37). We emphasize the importance of the Earth Radiation Budget Experiment. Current planning has scheduled this experiment to start before the satellites proposed for WOCE can be in orbit. Simultaneous operation is preferred for maximum information.

It appears possible that a useful ocean-atmosphere-ice climate study could be carried out in the enclosed region of the Greenland, Iceland, and Norwegian Sea. The excellent atmospheric observation net and the presence of a variable ice cover make this region a potentially viable trial region for larger budget studies. Therefore, in view of the potential importance of the Greenland, Iceland, and Norwegian Sea region for climate and water mass formation and the strong international interest, we recommend that a study on the feasibility of an energy-budget experiment be carried out (page 39). Moreover, because the areal extent of sea ice may be a good indicator of CO_2-induced warming of the atmosphere, we also endorse the continuance of long-term passive microwave measurements of sea ice cover in both polar regions (page 38).

o Long-Time Series Measurements

Long-time series of ocean measurements are essential to support the studies above as they seek to describe the ocean's behavior on climate time scales. Thus we fully support the continuation of existing time series, whose data can be used as indices of the general circulation, and the establishment of new time series as required (page 45). Data from long-term stations are the only record of ocean behavior on climate time scales. It is essential that this data record be maintained intact and expanded. Thus, noting the need for long time series measurements to support the global scale ocean circulation, tropical ocean-global atmosphere studies, and heat flux process studies, we recommend that those agencies responsible for funding ocean measurements give high priority to the development of exploratory time series as well as to the continuation of existing long time series. In addition, we recommend the establishment of new stations at selected passages and island stations around the world (page 46). The phantom weathership concept needs further development and comparisons are needed with other techniques of gathering long-time series in the open ocean, such as ships of opportunity.

o Paleoclimate Studies

Paleoclimate data can provide information that is valuable both for model validation and for understanding ice-age processes and causes. Thus, we recommend, but with lower priority than the items above because the time scales are centuries and longer, that support be given to the follow-on to the SPECMAP (Mapping variations in Ocean Spectra) project, if it can be shown that data from the sediment record can fill the spectral gap between the instrumental record and the geological record (page 42).

o Tools, Techniques, and Management

We also considered the needs for tools and techniques. including analytical and numerical modelling, empirical data analysis, technology of both in situ and space-borne instruments, and data management. There are several recent reports on these subjects. A summary of their salient points is provided in Chapter 4. We make two recommendations, one on modelling and the other on data management.

We recommend that more and better modelling efforts and tools be developed because vigorous development of highly parameterized simple models leading to a hierarchy of models of increasing complexity is of special importance for understanding climatic variations (page 48). In addition, because data management is a particular problem, especially with new satellite systems coming on line, we endorse the recommendations of the recent Office of Technology Assessment 1980 report, "Technology and Oceanography" on this topic and we recommend that further joint efforts between oceanographers and meteorologists be undertaken to develop an adequate data management scheme (page 51).

2. INTRODUCTION AND BACKGROUND

2.1. The Ocean in the Climate System

Does the Gulf Stream warm Europe? Will increased CO_2 in the atmosphere cause coastal flooding? Do sea surface temperature anomalies in the tropics affect mid-latitude climatic variation? These are all questions relating to the ocean's role in the climate system. Advances in understanding during the past decades have led to a good understanding about how the atmosphere works, but we know less about the basic dynamics of the ocean, and still less about how these two are coupled. As a consequence, good scientific answers to these climate questions, and others like them, are still not available.

For purposes of short-term weather prediction, a knowledge of ocean dynamics is not required. The sea-surface temperature controlling the heat flux to the atmosphere may be regarded as a given, quasi-steady, boundary condition. Thus the predictive problem lies entirely in the atmosphere. However, for the monthly and longer time scales of interest to climate prediction, the role of the ocean and the atmosphere are essentially reversed. A predictive model of the ocean is now needed, and the relevant properties of the atmospheric circulation must be expressed in terms of averaged quantities (Hasselmann, 1977).

During the past decade, we have learned much about the ocean from in situ arrays and ship-borne instruments. In addition, much has been learned about how to model ocean dynamics. During the next decade it appears certain that new global views of the ocean's behavior will become available from satellite measurements. The new data from spaceborne instruments when joined with data from in situ measurements will yield a three dimensional and global description of the ocean ideally suited for developing the required predictive models of the ocean and its role in the climate system.

The purpose of this report is to identify the specific areas of research necessary in order to make progress towards understanding the role of the ocean in the coupled ocean-atmosphere system. In this context, the report of the U.S. GARP Committee (1975) provides a useful set of definitions.

There the global climate system is described as consisting of five physical components: atmosphere, oceans, ice and snow, land surface, and plants and animals. The climate is defined as the average of various elements of weather and the state of other components of the system. Climatic state is defined as the average of the appropriate variable over time intervals longer than the lifetime of individual weather systems (days) and the intervals over which the atmospheric behaviour can be theoretically predicted (weeks). Thus, we have monthly, seasonal, yearly, or decadal climatic states. Climatic variation is defined as the difference between two climatic states of the same kind, as between two July ocean surface temperatures during different years in the North Pacific. Climatic anomaly is defined as the deviation of a particular climatic state from the average of a large number of climatic states of the same kind, as the January 1977 ocean surface temperature anomaly at the equator.

With these definitions, the question of study of the ocean for understanding climate can be resolved into three main questions:

1. What is the climatic state of the ocean?
2. How do the ocean and the atmosphere interact to produce the climatic state, climatic variation, and climatic anomalies in the ocean and in the atmosphere?
3. How are the living resources in the ocean and the sediments of the sea floor affected by and how do they respond to the climatic state, variation, and anomalies of the ocean?

Most past and current studies of the ocean contribute to the first question in some way. Clearly, the more information that we have about the physical variables and their distribution in the ocean, the closer we are to having an adequate description of the climatic states of the ocean. This report, however, focuses on those studies of the climatic states of the ocean that have the most relevance to the understanding of the second question about climatic variation and climatic anomalies.

Those ocean processes that are believed to be the most important in the understanding of the coupled system include ocean circulation, transfers of heat and momentum across the boundary, and heat storage and transport. We discuss these here. In addition, we include sea ice (whose variability affects the planetary albedo) and mixing across the ocean thermocline as it affects the transport and ultimate fate of CO_2. Ocean chemistry is discussed primarily in its role in understanding the general ocean circulation. The record of climate from the sea floor as recorded in the sediments and remains of living organisms is included primarily because such records provide proxy data for extending the data record for validation and interpretation of climate models.

The choice of subjects and time scales of importance is dictated by the emerging understanding of the ocean's role in the climate system. Current evidence indicates that the weather patterns on an earth without oceans but with a moist surface would have monthly, yearly, decadal, and longer time scales of variation (Gates, 1979). However, these fluctuations would reflect only the inherent instabilities of the atmosphere. The existence of a large deep ocean on our earth, which absorbs and transports a significant amount of the solar heat, adds a potential controlling factor to the lower atmospheric boundary. It is possible that this slowly varying part of the system could make the changes in the total climate system of the earth more predictable than the hypothetical earth without oceans.

The argument for predictability is based on heat storage and transport. Significant amounts of heat can be stored in the ocean with small changes of temperature. For example, the heat required to change the mean temperature of the atmosphere by 1 degree Celsius could be supplied by a change in the mean temperature of the oceans of only 0.001 degree, or the same 1 degree change could also be supplied by a change of only 0.5 degrees in the mean temperature of only the upper 30 meters of the North Pacific Ocean (a change which is below our current observational capability). This heat can then be given back to the atmosphere at different times and at different places. The advection and storage of heat by the ocean and the transfer of heat to the atmosphere thus provide a slowly varying driving force for the atmosphere. Empirical data and modelling studies have shown suggestive correlations, both simultaneously and with lags, between sea-surface temperature change and subsequent climatic changes, although these do not occur at all seasons. Thus, it is possible that a significant fraction of atmospheric fluctuations on monthly to decadal (and longer) time scales are related to oceanic fluctuations.

The particular importance of the ocean in the climatic issues involved with increasing atmospheric CO_2 was emphasized by the report of an ad hoc study group on Carbon Dioxide and Climate (Climate Research Board, 1979) and reaffirmed by a second group in 1981 (Climate Board, 1982). The ad hoc study group noted that one of the major uncertainties was the transfer into the oceans of the increased heat from the warming of the atmosphere. They suggested that the capacity of the intermediate waters of the oceans to absorb heat could delay atmospheric warming by several decades. Thus the actual warming at any given time could be appreciably less than that calculated on the assumption that thermal equilibrium is reached quickly. They note that we may not be given a warning until the CO_2 loading is such that an appreciable climate change is inevitable. Clearly, studies of mixing processes and time scales in the main thermocline are important here, as are measurements of ocean storage of CO_2. Global sea level may be a good indicator for CO_2 warming (see Chapter 4).

2.2 Ocean and Climate Research in the Past Decade

A number of documents have appeared in the past decade to address one or more of the ocean/climate issues. Table 1 includes a representative set of reports that deal in the broadest sense with the ocean and climate. A brief review of those documents and their recommendations helps set the context for future work.

The 1970s were the International Decade of Ocean Exploration. In 1969, an Oceanic Quest published by the National Academy of Sciences (Committee on Oceanography and Committee on Ocean Engineering, 1969) recommended new large-scale studies of mineral resources, living resources, environmental prediction, environmental quality, environmental forecasting, seabed assessment, and living resources.

TABLE 1. Documents on Ocean Research for Understanding Climate

Date	Title	Sponsoring Organization
1969	An Oceanic Quest--The International Decade of Oceanic Exploration	Committee on Oceanography National Research Council Committee on Ocean Engineering National Research Council
1973	Southern Ocean Dynamics--A Strategy for Scientific Exploration	Polar Research Board National Research Council
1974	The Ocean's Role in Climate Prediction	Ocean Sciences Committee National Research Council
1974	U.S. Contribution to the Polar Experiment POLEX-GARP (North)	Joint POLEX Panel National Research Council
1975	U.S. Contribution to the Polar Experiment POLEX-GARP (South)	Joint POLEX Panel National Research Council
1975	Understanding Climate Change--A Program for Action	U.S. Committee for the Global Atmospheric Research Program National Research Council
1975	The Physical Basis of Climate and Climate Modeling	GARP Joint Organizing Committee World Meteorological Organization International Concil of Scientific Unions, Global Atmospheric Research Program, Publication #16
1977	A U.S. Climate Program Plan	Interdepartmental Committee for Atmospheric Sciences
1977	Report of the Panel on Monitoring	SCOR Working Group 48 Ocean Climate Fluctuation
1977	Proposed NASA Contribution to the Climate Plan	NASA
1978	The Polar Sub-programme	GARP Joint Organizing Committee WMO/ICSU GARP Publication #19
1978	Elements of the Researc Strategy for the U.S. Climate Program	U.S. Committee for the Global Atmospheric Research Program
1979	The Arctic Ocean Heat Budget	SCOR Working Group 58
1979	An Ocean Climate Research Plan	Environmental Research Laboratories National Oceanic and Atmospheric Administration

TABLE 1. (Continued)

Date	Title	Sponsoring Organization
1979	The Continuing Quest--Large Scale Ocean Science for the Future	Ocean Sciences Board National Research Council
1980	A Strategy for the National Climate Program	Climate Research Board National Research Council
1980	Technology and Oceanography	Office of Technology Assessment U.S. Congress
1980	The National Climate Program Five-Year Plan	National Climate Program Office National Oceanic and Atmospheric Administration
1980	Report of the SCOR/JOC/IOC Pilot Ocean Monitoring Study Planning Meeting, Miami, 1-15 Oct. 1979	GARP Joint Organizing Committee WMO/ICSU WCRP Publication Geneva
1980	Ocean Models for Climate Research: A Workshop	Climate Dynamics Panel U.S. GARP Committee
1981	Satellite Altimetric Measurements of the Ocean	TOPEX Science Working Group NASA
1981	Technical Studies Related to the Development of a System for Ocean Climate Monitoring	NOAA Office of Ocean Technology and Engineering Services
1981	Large Scale Transport of Heat and Matter in the Ocean	NATO Advanced Research Institute Sept. 20-24, 1981, Chateau de Bonas, France
1981	Report of the Meeting on Coordination of Plans for Future Satellite Observing Systems and Ocean Experiments to be organized within the WCRP (Chilton, U.K., 26-31 January 1981)	JSC/CCCO World Climate Research Program, WCP-8
1981	JSC/CCCO Meeting on Time Series of Ocean Measurements (Tokyo, May 11-15, 1981	JSC/CCCO World Climate Research Program, WCP-11
1982	Papers Presented at the Meeting on Time Series of Ocean Measurements (Tokyo, May 11-15, 1981)	JSC/CCCO World Climate Research Program, WCP-21
1982	The "Cage" Experiment: A Feasibility Study	JSC/CCCO World Climate Research Program, WCP-22
1982	Scientific Opportunities Using Satellite Wind Stress Measurements over the Ocean	Report of the NASA Satellite Surface Stress Working Group

An Oceanic Quest was firm in its conviction that joint studies of the ocean and atmosphere related to climate were necessary. The report states: "Today our livelihood and prosperity are inextricably bound to that of peoples on other continents. The state of the rice crop in Indonesia, the monsoon rains over the farmlands of India, and years of bad fishing off Japan all affect us. Many of these fluctuations in livelihood and prosperity are linked to vagaries of weather and climate which if anticipated and predicted could be allowed for. Thus our need to measure, monitor, and understand the weather has grown to global proportions, and it is beginning to dawn on us that on this global scale the atmosphere and the ocean are as closely linked as two coats of paint on a croquet ball."

Recommendations for large-scale studies were also made by the Committee on Polar Research of the National Research Council for a program in Southern Ocean Dynamics, covering the areas of dynamics of the Circumpolar Current, Antarctic Bottom Water formation, and exchange processes and overall budgets (Committee on Polar Research, 1974) and for heat budget studies in the Arctic basin and Greenland-Iceland-Norwegian Sea Region (Joint POLEX Panel, 1974, 1975).

The recommendations of an Oceanic Quest and the Southern Ocean Dynamics reports led to the formation of several new large programs of oceanic research that are relevant here. These programs were GEOSECS (Geochemical Sections), a global survey of the distribution of a large number of geochemical and physical tracers; MODE (Mid-Ocean Dynamics Experiment), a study of the dynamics of mesoscale eddies and their role in the general ocean circulation; CUEA (Coastal Upwelling Ecosystems Analysis), a descriptive and modelling study of the physics, chemistry, and biology of coastal upwelling and its forcing; NORPAX (North Pacific Experiment), a study of the oceanic fluctuations of the north and tropical Pacific Ocean and their relation to the atmosphere; and ISOS (International Southern Ocean Studies), a study of the transport, dynamics, and air-sea coupling of the Antarctic Circumpolar Current.

Although the goals of the IDOE programs were not restricted to understanding climatic processes, the results give us a better knowledge of the ocean on the time and space scales of relevance to understanding climate and climatic variation. Only NORPAX, which has a significant meteorological component, addressed specifically the issues of the ocean's role in climatic variation. Significant advances in understanding large-scale ocean-atmosphere interaction in the tropical and North Pacific have resulted from that program.

The Barbados Oceanographic and Meteorological Experiment (BOMEX) and the Atlantic Trade Wind Experiment (ATEX), both of which took place in 1969, set the stage for the 1970's in air-sea interaction programs. During the 1970's, the major IDOE air-sea interaction program was the Joint Air-Sea Interaction Project (JASIN) coordinated and led by the U.K. After a series of preparatory experiments, the major study, in which nine nations, including the U.S., participated, was held in 1978. The aims of JASIN (which were largely met) were to observe and distinguish between the processes causing mixing and transport in the boundary layers and to relate these processes to mean properties of the layers, and to examine and quantify aspects of the momentum and heat budgets in the atmospheric and oceanic boundary layers and fluxes across and between them. One of the important results of JASIN was to provide ground truth for SEASAT microwave sensing of ocean surface wind vectors, and to demonstrate the validity of the satellite techniques.

Several other air-sea interaction projects were carried out in the late 1970's including INDEX, GATE, MILE, and STREX. The Indian Ocean Experiment (INDEX) was designed to determine the response of the ocean to the large-scale monsoonal forcing and to relate it to theory. The GARP Atlantic Tropical Experiment (GATE) was a test ground for and precursor of the Global Weather Experiment. On the oceanographic side, GATE scientists studied salt and heat budgets, internal waves, mixed layer and shallow frontal development, and the Atlantic equatorial current system. The Mixed-Layer Experiment (MILE), carried out in the North Pacific in the late 1970's, was an attempt to elucidate dynamics of processes in the upper layer. The Storm Transfer and Response Experiment (STREX), carried out at station PAPA in the North Pacific in 1979-1980 studied the effects of storms on air-sea interactions and upper ocean dynamics.

In the mid 1970's, many of these programs were either on-going or in the advanced planning stages. At the same time, the Global Atmospheric Research Program was gathering momentum towards the 1979 Global Weather Experiment, also called the First GARP Global Experiment (FGGE). In order to make sure that interaction of oceanographers and meteorologists would be as fruitful as possible in the GARP context, the Ocean Sciences Board of the National Research Council conducted two workshops in 1973 on the role of the ocean in climate prediction (Ocean Sciences Committee, 1974).

These workshops were designed to provide an overview of the present and projected large projects related to the GARP objective of understanding the physical basis of climate, to formulate specific questions vital to understanding the role of the ocean in climate prediction, and to indicate where the FGGE could provide opportunities for better understanding the ocean-atmosphere climate problem. The report of the workshops identified the major questions and problems in developing a climate predicting system, and summarized the character of climatic variation in the Pacific and the Atlantic oceans as seen through the relatively meager data sets available. It noted that the greatest need in the Atlantic is analysis of temperature in studies of basin-wide data sets, whereas the greatest need in the Pacific is for extending the length of the existing series by continued monitoring. These needs remain today. Problems noted included the fact that the observations tend to be concentrated in fishing areas, which are not necessarily the areas important to climate change. The features and interactions of the major projects were summarized, and the prospects for new remote sensing techniques were outlined. Although the need for a wider collection of routine data was emphasized, this need has not been met yet.

The report focused on the use of ships of opportunity to advance the state of ocean monitoring. While recognizing that ship-of-opportunity tracks are determined commercially and may be biased against stormy weather, the report pointed out that, even so, they offer the only real alternative to the ocean weather stations. Because the weather stations have become expensive and are harder to justify with the recent availability of satellite data, there has been a drastic reduction in the network. The report noted that, without disrupting the current commercial ship reporting program, a part of the required sampling effort could be reshaped to provide a constant frequency of subsurface sampling at specified locations. This is the Phantom Weather Ship program which is laid out in great detail in the subsequent Report on Monitoring Ocean Climate Fluctuation (SCOR Working Group 48, 1977). Although the program is cost effective, the recommendations of the SCOR panel have still not yet been implemented by any country. A possible next step is recommended in Section 4.1.

The U.S. Committee for the Global Atmospheric Research Program continually identified the need for ocean research related to climate in their documents published through the decade, beginning with the Plan for U.S. Participation in the Global Atmospheric Research Program (1969). This was followed by a report by this group on the second objective of GARP, to understand the physical basis of climate, entitled Understanding Climate Change. That report recommends efforts in the broad general areas of monitoring, process experiments, and modelling. In addition, it points out the need for ocean monitoring to determine sea surface temperature, heat storage and transport, the sub-surface temperature structure of major current systems, salinity (particularly in high latitudes), sea level, and chemical composition. However, no specific program was recommended, and no new monitoring programs were undertaken. Process studies were encouraged, particularly in the areas of upper layer dynamics, mid-ocean eddies, thermohaline circulation, and intense western boundary currents. Some of these studies have been and are being carried out by oceanographers, but generally not in the context of climate research.

General statements about the need for ocean studies in the climate context were also included in the U.S. Climate Program Plan published in 1977 by the Federal Coordinating Committee for Science, Engineering, and Technology (Interdepartmental Committee for Atmospheric Sciences, 1977). This plan led to the National Climate Act, passed by Congress in 1978, and the National Climate Program Five-Year Plan, discussed below.

Perhaps the most detailed look at requirements for ocean studies in climate research was put together during this period by the Joint Organizing Committee of GARP (1975) in their document, The Physical Basis of Climate and Climate Modeling, the report of the International Study Conference in Stockholm, Sweden, July 29-August 10, 1974. Recommendations are made in the area of modelling, oceanographic atlases, studies of surface fluxes and atmospheric sensitivity to changes in ocean surface properties, monitoring, and studies of paleoclimates. Recommendations focused more on identifying opportunities in existing oceanographic programs rather than recommending new programs, thus recognizing the need for basic research in oceanography. The need for coordinating existing oceanographic data into a standard format useful for climate studies was also underlined.

In 1978, the Climate Dynamics Panel of the U.S. GARP Committee summarized the essential elements of the overall research strategy for the U.S. Climate Program. The panel recommended initial experimental ocean monitoring by ships of opportunity, phantom weather stations, island stations, moored arrays, and repeated hydrographic sections across major currents and gyres; and identified as important a number of process experiments in tropical ocean-atmosphere interaction, mid-latitude thermal climatic anomalies, ocean-atmosphere exchange at high wind speeds, meridional heat flux, ocean-ice interaction, and geochemical exchanges.

The general recommendations of the Climate Dynamics Panel led to the more detailed study carried out by the Environmental Research Laboratories of NOAA and reported in An Ocean Climate Research Plan (1979), which made specific recommendations for NOAA support of ocean programs in climate. Three of the programs recommended there have been funded: The Equatorial Pacific Ocean Climate Studies (EPOCS), the Storm Transfer and Response Experiment (STREX), and the Sub-Tropical Atlantic Climate Study (STACS). In addition, NOAA began the planning for monitoring the ocean for climate by contracting with UCAR for a strategy report on a system for ocean climate monitoring (NOAA Office of Ocean Technology and Engineering Services, 1981). That report, which identified issues in monitoring, is now available. Next a long-term monitoring strategy needs to be developed by NOAA in order to support the large-scale experiments on ocean circulation and on air-sea interaction being proposed as part of the World Climate Research Program (see Section 4.1).

In the summer of 1979, a review of the preliminary U.S. National Climate Five-Year Plan was made by the Climate Research Board (1980). The resulting report, A Strategy for the National Climate Program, identified and stressed the specific need for describing and understanding the global ocean circulation, and recommended that the feasibility of such a program be studied immediately. It was suggested that the United States propose such a study as a major international initiative to begin sometime in the late 1980's. The proposed TOPEX (Topographic Experiment) study and the plans for the international World Ocean Circulation Experiment (WOCE) discussed in Chapter 3 have grown from these recommendations.

The TOPEX planning and the Satellite Surface Stress working group report (1982) all show how during the 1970's the emerging feasibility and improving accuracy of global synoptic coverage of important oceanic and air-sea interaction parameters by satellite became a major factor in planning for climate-related experiments. Successful data communication from drifting and moored buoys and measurements of wind stress at the surface, sea-surface topography, and sea-surface temperature all began to show the potential of satellite measurements for understanding the ocean. This focus on satellite measurements is reflected in the recommendations of the documents written at the end of the decade. Satellite measurements play a major role in the recommendations of this report.

It is instructive to conclude this brief review of the recommendations of the 1970's with a summary of the report from the Ocean Sciences Board, The Continuing Quest--Large Scale Ocean Science for the Future (1979). The document considers the continuation of the large-scale cooperative approach to ocean investigations after the IDOE. Significantly, a strong interest in climate and its manifestations pervades this report which is written from the point of view of ocean research. The report notes that the ocean plays a leading role in the development of many types of forecasts and uses the critical role of the ocean in climate as an example. In each disciplinary section of the report, we see the orientation of climate. In terms of biological oceanography, we have the question of how temporal climatic changes are reflected in variations in the kind, quantity, and fate of phytoplankton. Since phytoplankton is the basis of the marine food chain, changes in other marine populations ultimately depend (at least in part,) on the link between atmospheric climate and the ocean. From marine geology and geophysics, we have the questions of the causes of the quaternary ice ages, and how comparison of paleoclimatic observations with the quantitative predictions of physical theory can contribute to our understanding of how the ocean affects climate. The fundamental question asked by physical oceanography is, how the ocean interacts with large-scale low frequency atmospheric fluctuations.

The set of recommendations in Chapters 3 and 4 of this report directly responds to these basic oceanographic interests as well as the needs of the newly-formed National Climate Program and the World Climate Research Program.

2.3 The National Climate Program

Thus, we see that the 1970's end with vigorous ocean research programs mainly oriented to understanding the ocean rather than specifically to understanding climate. One goal of the National Climate Program and the World Climate Research Program is to embrace many of the elements that were recommended by the various groups and provide support for that ocean research necessary to understand the climate system.

The National Climate Program (National Climate Program Office, 1980) focuses on the improved use of existing climate knowledge, the exploration of the economic benefits that might be derived from more effective use of climate information in decision making and on improved systems for delivery of climatic information to a wide range of users as well as on the research necessary for the determination of the sensitivity of climate to manmade perturbations, such as increasing atmospheric carbon dioxide content and the resultant effects on seasonal and interannual fluctuations of climate. These latter periods are of most concern to farmers and others dependent on climate.

The research part of the plan identifies the determination of solar and earth radiation and the role of the ocean in heat transport and storage as principal areas of study. Initial programs would build on the existing studies in the North Atlantic and North and Tropical Pacific and be coordinated with the international efforts. The program outlined in Chapter 3 is intended to be a contribution to the National Climate Program.

2.4 The World Climate Program

The World Climate Program has four components: Research (WMO/ICSU), Impact Studies (UNEP), Data (WMO), and Applications (WMO). The program was formally established following the World Climate Conference in February 1979, by the 8th World Meteorological Congress. Here we focus on the research component, the World Climate Research Program (WCRP) (World Climate Research Program, Joint Scientific Committee, 1980), that is planned and coordinated by a WMO/ICSU Joint Scientific Committee (JSC). The major objectives of the WCRP are to determine to what extent climate can be predicted and to establish the extent of man's influence on climate. In order to achieve these objectives, the program notes the need for improvement of our knowledge of global and regional climates and their temporal variations, and our understanding of the mechanisms responsible for climatic variation. The program will: assess the evidence for significant trends in global and regional climate; develop and improve physical models capable of simulating and assessing the predictability of the climate system over a range of space and time scales; and investigate the sensitivity of climate to possible natural and manmade stimuli in order to estimate the changes in climate likely to result from specific disturbing influences.

The WCRP is primarily concerned with time scales varying from months to centuries. This range of time scales is consistent with the availability of comprehensive data sets and the practical aspects of numerical modelling. Some requirements such as paleoclimate reconstructions will involve larger time scales. They are especially important for model validation and comparison.

Of the many climatologically significant processes in the entire climate system, two--the effects of clouds and the effects of the oceans--have been singled out as requiring particular attention because of their fundamental nature and the long lead time needed for the organization of experimental programs for their study. Clouds are important because of their effects on the radiation energy budget of the climate system. The oceans are important because of the effect of their dynamics and thermodynamics on the global cycles of heat, water, and chemicals (particularly carbon and nitrogen) in the climate system.

The proposed initial research strategy of the WCRP for improving understanding of the ocean for climate includes improving models of ocean processes and circulation, including both hydrodynamic models and diagnostic models; creating a new base of oceanic data to support work with ocean models; developing new observing techniques for gathering the data; undertaking global circulation studies; and undertaking regional and local experiments where needed (for example, remote sensing of the water flux through the surface due to evaporation and precipitation).

SCOR and IOC through their joint Committee on Climatic Changes and the Ocean (CCCO), are working with the WCRP to develop an oceanographic component of that program. Two major ocean observation programs and a supporting and continuing ocean monitoring program have been identified by the JSC and the CCCO as especially important for the World Climate Research Program: a world ocean circulation experiment (WOCE), a tropical ocean-global atmosphere experiment (TOGA), and selected long time series of measurements in major ocean circulation regions. These major studies are discussed in more detail in Chapter 3. In addition, the uncertainty in precipitation and evaporation data over the ocean is noted as a major problem for closing the hydrological cycle which leads, in turn, to a residual uncertainty in the water balance and heat flux estimates for both the atmosphere and the ocean.

Sea-ice studies recommended include development of improved methods for the treatment of sea ice in climate models; study of the physical processes affecting interactions between air, ice, and sea at the sea-ice margin, sensitivity studies of the response of climate models to sea ice; and continued monitoring by satellite of the sea ice edge and its total extent.

2.5 Future Directions

The recommendations of the United States national and world climate programs are consistent with the reports and recommendations that have been made throughout the 1970's. The encouragement of the developing climate programs has been helpful to oceanographers in gaining support for basic ocean research. But we still do not have answers to many of the basic questions of ocean dynamics and thermodynamics that will be required to understand climate change.

Although most of the fundamental climate questions raised in the documents of the late 1960's remain unanswered, it can be stated with some certainty that we know now better what information we need from the ocean and where some of the climatically significant interactions take place. Thus, the tropics have been identified as such a region ripe for study now. We also have an emerging notion of how to make the necessary measurements. Satellite-remote-sensing technology, as it has developed in the decade of the 1970's, and new acoustic techniques for large-scale measurements are central to this idea. Although we know how difficult it is to set into practice routine long term measurements, the parameters to be measured and the technology for doing so can be identified. We also recognize the difficulties in developing truly joint ocean-atmosphere research programs. Although there were some attempts to make such joint experiments as part of GARP, they were generally not successful from the oceanographer's point of view. The different state of knowledge, the different sampling needs, the different degrees of instrument development, and the generally different ways of doing things between oceanography and meteorology, all mean that the two groups need to be brought together at the beginning. This problem is beginning to be recognized and dealt with in the programs now ongoing and proposed.

Overall, the importance of the role of the ocean in climatic variation becomes evident from two different directions. In basic science it is seen as a fundamental problem that integrates across the disciplines. The practical issues relating to food, energy, and water resources are focused on the need for a better understanding with a view towards prediction.

The need for understanding has been identified and important advances are imminent. Thus it is essential that the scientific community develop now a consensus strategy for the investigation of the ocean's role in climatic variation. In the following pages the steering committee has identified the probable elements of such a strategy. However, the strategy itself must be developed in close cooperation with the international planning now being carried out through the CCCO and its various panels and working groups. We recommend that the Board on Ocean Science and Policy, in cooperation with the Board on Atmospheric Science and Climate of the National Research Council, develop such a strategy, based on a consensus within the scientific community as to the priorities for investigating the ocean's role in climatic variations and working in cooperation with federal agencies and others as appropriate.

3. SCIENTIFIC ISSUES AND RECOMMENDED RESEARCH

To understand the interaction of the ocean and the atmosphere requires study of a full range of climate-related processes: the basic dynamics and thermodynamics of the ocean and the atmosphere, the sensitivity of the climate system to climatic anomalies in different oceanic regions, and the dynamics and underlying causes of climatic anomalies. Key to all the climate interactions are the underlying physical processes in the ocean that transport, mix, and store heat and radiatively important gases, and that are responsible for the transfer of heat, mass, momentum, and gases to the atmosphere.

During the 1970's several of the IDOE programs achieved results of importance in understanding the ocean's role in the climate system. In particular, NORPAX studies established the existence of large sea-surface temperature anomalies in the North Pacific and demonstrated correlations between the anomaly patterns and climatic variations over North America. In the equatorial regions, convincing evidence was found to indicate that the El Niño phenomenon is just one part of a global interaction of the atmosphere and ocean. High correlations continued to be found between tropical winds and tropical ocean circulation and between the southern oscillation and the El Niño phenomenon. New physical mechanisms involving Kelvin wave and Rossby wave propagation were demonstrated.

The CUEA program demonstrated large-scale remote atmospheric forcing of local upwelling, and recognized that coastal processes are tied to equatorial processes in the eastern equatorial Pacific. ISOS results revealed a relation between large-scale circumpolar winds and local currents in the Drake Passage, and a large eddy transport of heat with strong spatial dependence. The MODE and POLYMODE studies, in addition to providing an important description of eddies in the North Atlantic region, have shown that eddy heat flux is relatively weak in the Sargasso Sea area.

Where do we go from here? The most recent recommendations of the SCOR/IOC Committee on Climatic Changes and the Ocean, as developed at a study conference on Large-Scale Oceanographic Experiments in the World Climate Research Program (Tokyo, May 1982), give high priority to four main areas of study: (1) a global study of the general ocean circulation using satellite techniques to determine surface forcing, density distribution, geostrophic flow, eddy statistics, and water mass conversion (tentatively called the World Ocean Circulation Experiment--WOCE); (2) a tropical ocean-atmosphere study of global scale to be based on the developing understanding of the El Niño Southern Oscillation processes (tentatively called the Tropical Ocean-Global Atmosphere--TOGA study); (3) a study of heat flux processes in the North Atlantic and Pacific oceans; and (4) a continuing set of observations at key locations in the ocean for long-term climate-related data sets (tentatively called the Pilot Ocean Monitoring Study--POMS) designed to support the global studies.

We agree with these recommendations. In terms of immediate priorities, we rank the World Ocean Circulation Experiment and the Tropical Ocean-Global Atmosphere study highest because these programs are crucial. WOCE will yield the knowledge of the general circulation needed for all ocean climate studies and the technology is now ready. TOGA focuses on the only area of large-scale air-sea where significant background information is available for the design of significant experiments. Certain elements of the heat flux processes studies can be carried out effectively now. We recommend that further planning for large-scale heat flux studies be carried out in the context of planning for WOCE since heat flux and circulation are closely linked. Each of these general subject areas is discussed in more detail below in this chapter. Ultimately, a full strategy needs to be constructed for ocean monitoring, but there are needs that can be met now (see Section 4.1).

Polar ocean and ice interactions and paleoceanography studies provide information on longer time scales. Although such studies are of particular use in establishing climatic scenarios, they are of a lower priority in the context of the World Climate Research Program. However, since many of the resources, funds, and manpower, which would be needed for these studies come from different sources, we have also identified these as useful studies.

3.1 General Ocean Circulation

The redistribution and subsequent return of heat to the atmosphere from the ocean involves certain time scales of response which depend on the internal climate of the ocean, its circulation, and mixing. The POMS (Pilot Ocean Monitoring Study) Report (GARP, JOC and SCOR, 1980) has emphasized that any plausible decadal climate plan must include as a goal the determination of the climatic state of the ocean as well as that of the atmosphere. One important aspect of the climatic state of the ocean is what is usually called the "general circulation."

The general circulation of the ocean is not uniquely defined. It is the flow which produces the time-averaged transport of momentum, mass, heat, salt, and other tracers. To a first approximation, it is usually assumed that the transport fields are largely determinable by the geostrophic relation. The extent to which the study of different quantities, momentum, heat, etc., leads to different circulation patterns is currently the subject of much research activity.

Major obstacles to understanding the ocean circulation lie in both observations and modelling. The ocean is difficult to observe properly. Our picture of the general circulation of the ocean and its variability is largely dependent on two distinct types of observations. The first includes the record of the distribution of water density in the ocean that has been built up over the past 50 years from measurements made from ships and sea level measurements from coasts and islands. Treating the density data as though they were taken almost simultaneously, we have a picture of the gross structure of the circulation that is aliased to an unknown degree. For example, we don't know to what extent the density field on the large scale varies interannually.

The sea level data give us a measure of the variability of ocean circulation in certain regions, with the tropical Pacific particularly well covered. However, there are certain regions of the ocean which are not adequately covered with either modern hydrographic data or sea level stations. We know so little about the zonal circulations at sub-surface depths, even qualitatively, that little can be inferred from the present data bank of hydrographic sections primarily along latitude lines. The greatest limitation is in our knowledge of the circulation of the major mid-ocean gyres. Further in situ measurements in the major gyres are required and an expansion of the existing sea level network is necessary.

The second type of measurement involves arrays of moored and drifting instrumentation deployed for periods up to two years. Data from these cluster-type arrays have shown a very energetic variability--mesoscale eddies--in the ocean circulation with periods of weeks to months and spatial size ranging from tens to hundreds of kilometers. The report of the Workshop on Ocean Models for Climate Research (U.S. GARP Committee, 1980) emphasizes that modelling of the general ocean circulation is severely complicated by the existence in much of the world's oceans of these mesoscale eddies.

The eddies may be thought of as the oceanographic counterparts of the atmospheric synoptic scale, but there are important differences. The major part of the oceanic kinetic energy is bound up in the eddy scale, and this scale of motion may play an important role in the meridional transport of heat. Although the eddies are small relative to the ocean basin size, because eddies play an active role in the energy budget and are suspected to be important in heat transport, their resolution and parameterization is being actively pursued in ocean general circulation studies.

To measure fully the circulation of the ocean and its variability and to provide the necessary data for modelling, we need to fill the gap between the information obtained over periods of years by large-scale, basin-wide surveys using ships and that obtained by cluster-type experiments, which can give instantaneous pictures of the circulation but over only extremely limited areas and in regions of comparatively low currents. Because there are many analogs between the ocean circution and the circulation of the atmosphere, we can see clearly now that to carry our understanding of the ocean at least to a first approximation of our understanding of the atmosphere, we will need the equivalent of both the meteorologists' global observation network and their regional network.

The major new step envisaged for the late 1980's and early 1990's in study of the general circulation is the use of basin-wide measurements from satellites and large-scale acoustic arrays (Munk and Wunsch, 1982). Current technical developments have created the possibility of an observational system consisting of satellite altimeters and scatterometers, geoid measurements, drifting buoys communicating by satellite, in situ acoustic arrays for obtaining information on the general circulation, and sufficient basin-wide surveys to complete our description of the density field of the ocean. Therefore, by the early 1990's we can expect to be able to observe the ocean and its driving forces as a whole.

The surface topography and geoid measurements will allow construction of global maps of the surface geostrophic velocity and its variations on space scales from 25 km to entire ocean-basin widths and on time scales from days to years (Wunsch and Gaposhkin, 1980). The scatterometer can define the surface vector wind field which represents the major driving force of the ocean circulation (Satellite Surface Stress Working Group, 1982). These two measurements, by themselves, would provide oceanographers with the equivalent of the meteorological surface pressure and surface forcing fields.

In order to complete the description of the density field, a set of closely-spaced, long ocean sections is necessary in areas currently undersampled as shown in Figure 2. For in situ measurements, in addition to the required hydrographic stations and sections, we can look to both surface drifters and various acoustic and electromagnetic averaging techniques for new advances in the 1980's. The in situ measurements will complete the observational network. The continuing numerical modelling efforts in general circulation and coupled ocean-atmospheric circulation models will find a useful data base in the resulting set of measurements.

The general question of a global satellite-based experiment is under study by the NASA/TOPEX (Topographic Experiment) Science Working Group and the JSC/CCCO World Ocean Circulation Experiment (WOCE) steering group.

The JSC/CCCO have developed a set of goals for a World Ocean Circulation Experiment. These are:

1) to determine the three-dimensional general circulation of the ocean over a period of several years, with adequate spatial and temporal resolution to understand the dynamics and kinematics of the movement of water;

2) to describe and understand the major elements of the oceanic flux of heat and fresh water and their interchange with the atmosphere;

3) to make possible better determination of the rate of absorption of carbon dioxide by the oceans;

4) to measure and understand the annual cycle and long term variability of the ocean circulation;

5) to derive more precise estimates of the conversion rates of water masses within the ocean; and

6) to arrive at an understanding of the requirements for efficient and economically feasible long-term measurement and monitoring of the ocean.

The JSC/CCCO anticipates that a program directed at these goals will involve many elements, including satellites, ships, in situ observation systems of many kinds, and numerical and analytical models. Cooperation from both national and international agencies will be required. A nominal observation period of about five years is envisaged.

The TOPEX Science Working Group prepared a plan for satellite altimetry to contribute to WOCE. The program could begin in the late 1980's. The recommendations of the TOPEX group provide details on the satellite instrumentation, the orbit and resulting ground track (see Figure 1), the necessary geoid and surface wind stress information, and the data system. The proposed accuracy of the altimeter is two centimeters, and the satellite track is to repeat within one kilometer every ten days. In terms of timing, because both the in situ observations and the global winds are important elements of the TOPEX experiment, it is important that TOPEX have maximum overlap with related oceanographic and meteorological experiments now in the planning phase. Satellite scatterometer data, for global surface wind stress, is an essential part of the program. The improved knowledge of the geoid sought from a special gravity field mission (Committee on Geodesy, 1979) need not precede the launch of TOPEX because the data can be used retrospectively. Nonetheless, for most efficient interpretation of the data, it is highly desirable that the proposed six-month long gravity field mission be flown sometime before the end of the proposed five-year TOPEX mission.

We recommend that the elements of the proposed ocean Topography Experiment (TOPEX) program, including spaceborne altimeter, scatterometer, and geoid measurements, and the necessary in situ measurements from drifting and moored arrays be supported. In addition, we recommend a five-year mission life-time in order to obtain as much information as possible on interannual variability. We note that the JSC/CCCO planning for the satellite measurements in the World Ocean Circulation Experiment is drawing on the United States TOPEX studies in its preparation of an international plan involving altimetric satellites from different nations. We urge that liaison be maintained between the United States working groups and the international planning for WOCE.

For in situ measurements, it appears that large-scale acoustical arrays (Munk and Wunsch, 1979) could monitor the density field. Using the techniques of acoustic tomography, it may be possible to monitor fluctuations in the thermal structure of the mid- to deep-ocean by measuring perturbations in acoustic travel time (and received intensity) between sources and receivers placed at long distances from each other in the deep sea. An application of geophysical inverse theory allows the deduction of density perturbations in the mesoscale and larger, potentially over entire ocean basins. A major advantage of tomography over conventional systems is that the information gained grows geometrically, rather than linearly, with the number of moorings. A 1981 study on a 300 km by 300 km square southwest of Bermuda shows that the acoustic arrays could resolve the eddy field (Ocean Tomography Group, 1981). If the acoustic array were expanded, it could be capable of resolving the full three-dimensional density field of the ocean over entire basins every few days. Together with altimetry and surface drifters to determine the surface velocities, the total system appears to be capable of direct measurement of the geostrophic and non-geostrophic flow field from top to bottom every few days. We recommend the continued development of the technique of acoustic tomography because of its promise for monitoring ocean density, a major need in describing the general ocean circulation.

Equally as important as the array measurements is basic, large-scale oceanographic data coverage in data-sparse regions. As discussed above, several long sections or "long lines" of accurate data must be carried out as part of any general circulation experiment. These stations would include vertical sampling at about 1° spacing in latitude from the surface to near-bottom of temperature, salinity, oxygen, and nutrients. CTD (Conductivity-Temperature-Depth) profiles should be taken, and must be supplemented by bottle data adequate to define the major features in water mass and density structure. An example of some of the sections required as determined by our current lack of knowledge is indicated on Figure 2. We recommend that hydrographic sections be taken in areas currently undersampled and urge support for such studies, as they are crucial to understanding the general circulation.

Prior to GEOSECS, comprehensive global models of chemical properties of the ocean were not possibe. As a consequence of that major study during the decade of the 1970's, information has become available on the global distribution of stable and radioactive isotopes. Of particular interest to climate studies are the documentation of the distribution of species of the carbon dioxide system in the ocean and the demonstration from tritium and ^{13}C distributions of the rapid renewal rate of deep water masses in the Atlantic Ocean where the characteristics of most of the world's deep water are determined. Tritium and other fallout products in association with the deep overflow water in the Denmark Strait at $63^{\circ}N$ have already penetrated to $38^{\circ}N$ in the western Atlantic. The tritium data show that more than just the upper ocean is involved in air-sea exchange.

The measurements of the distribution of transient tracers (anthropogenic [H^3, He^3, C^{14}, Kr^{85}, Freons, etc.] and natural [Ra^{228}, Ar^{39}, etc.]) give us an important and complementary view of ocean circulation not feasibly obtainable in any other way. For vast regions of the oceans, the chemical studies may be the only way that we will have of obtaining the time scales of the circulation on a large scale. Moreover, the large injections of the anthropogenic tracers produced during nuclear testing will not soon, if ever, be repeated. In addition, the mixing and fate of CO_2 remains an issue. For these reasons, we need a research program to follow the transient tracer distributions over the next decade in order to get information on the

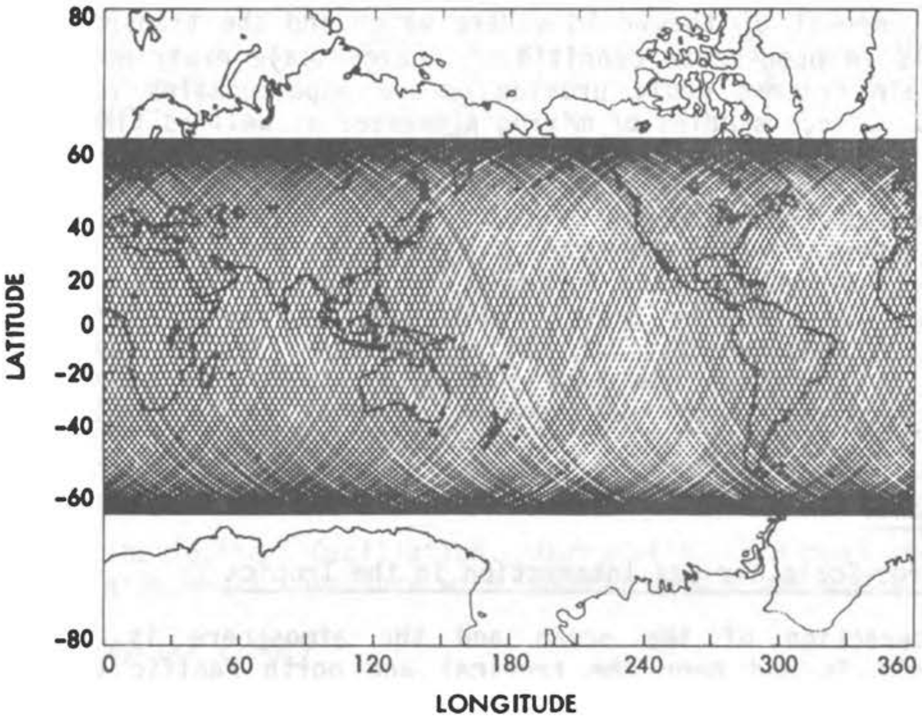

Figure 1. The ground track traced out by a satellite at an altitude of 1300 km and with an orbital inclination of 64^0 during a ten-day period of an exactly repeating ten-day cycle.

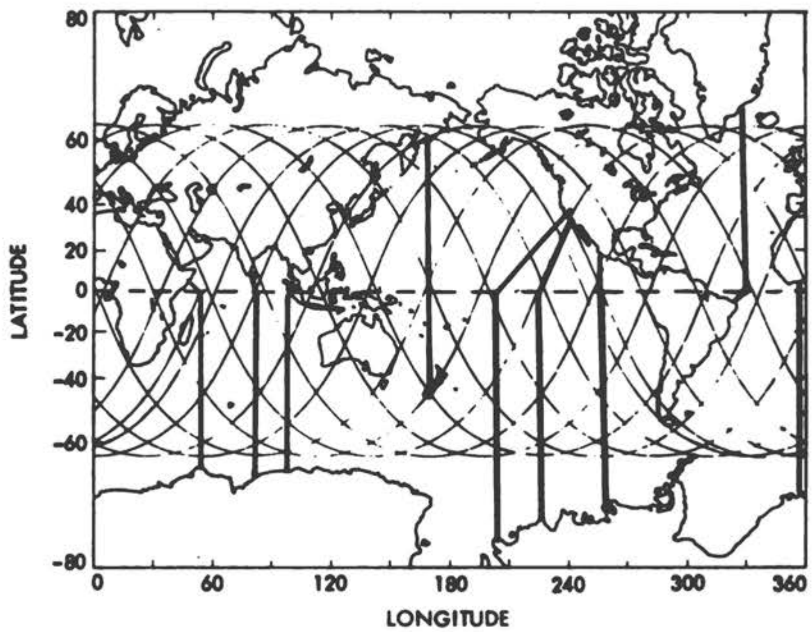

Figure 2. Recommended lines for closely spaced hydrography superimposed on a one-day path of a satellite as above.

rate of renewal of the world's deep water and the transport rates of chemicals in open-ocean conditions. Large-scale experimental release of certain tracers could provide unique opportunities for diffusion studies. Direct studies of mixing processes as well as time scales are important in this context. Such a program could contribute to an evaluation of the ocean's capacity to absorb CO_2 and other atmospherically transported pollutants.

The current plans for the Transient Tracers in the Ocean (TTO) study include measurements in the North Atlantic, Equatorial and South Atlantic, and then the South, Equatorial, and North Pacific oceans. With these measurements, the entire Atlantic, Pacific, and Antarctic sectors of the ocean would be covered by 1988. We endorse the TTO study, recommend that the proposed chemical tracer measurements be carried out, and urge that liaison be maintained between the TTO chemical studies and the on-going planning for the World Ocean Circulation Experiment.

3.2 Large-Scale Air-sea Interaction in the Tropics

The interaction of the ocean and the atmosphere is demonstrated vigorously in and over the tropical and north Pacific Ocean, where evidence is growing for the existence of physical links between global scale atmospheric fluctuations and alternating episodes of warm and cool sea surface temperatures. Recent reviews of this topic can be found in Namias (1975), Namias and Cayan (1981), Barnett (1978), Hamon and Godfrey (1978), and Newell (1979).

In recent years, the large-scale Southern Oscillation, which has been known since the 1920's (see, e.g., Walker, 1923; Bjerknes, 1969; Julian and Chervin, 1978; Horel and Wallace, 1982; Rasmussen and Carpenter, 1982), has come to be viewed as only one of a number of manifestations of such global-scale teleconnections. For example, apparently Arabian Sea anomalies can affect the intensity of the monsoon rainfalls over the Indian subcontinent (Shukla, 1975). In the equatorial Atlantic and Pacific, a clear connection between winds and currents has been observed (Wyrtki, 1973, 1979; Hurlburt et al., 1976; Moore et al., 1978; Philander, 1981). In the South Pacific, a recent study by Fletcher et al. (1982) of 120 years of historical data show fluctuations of ocean temperature and atmospheric wind anomalies of global scale. Figures 3 and 4 from Wyrtki (1982) show the global-scale correlation of mean annual atmospheric pressure with the pressure at Jakarta and the time scales. The interannual variability is clear. There are many more examples.

The global scale and the persistence of the patterns suggest that knowledge of the ocean processes would help long-range forecasting. The long-range weather forecasting work of Namias and his coworkers has shown much correlation and many connections between the atmosphere and the ocean. In spite of this enormous amount of work, the success of long-range forecasting has been modest, except when compared with either geophysical predictions or economic forecasts. However, as Namias (1980) notes, we do not yet have reliable long-range forecasts.

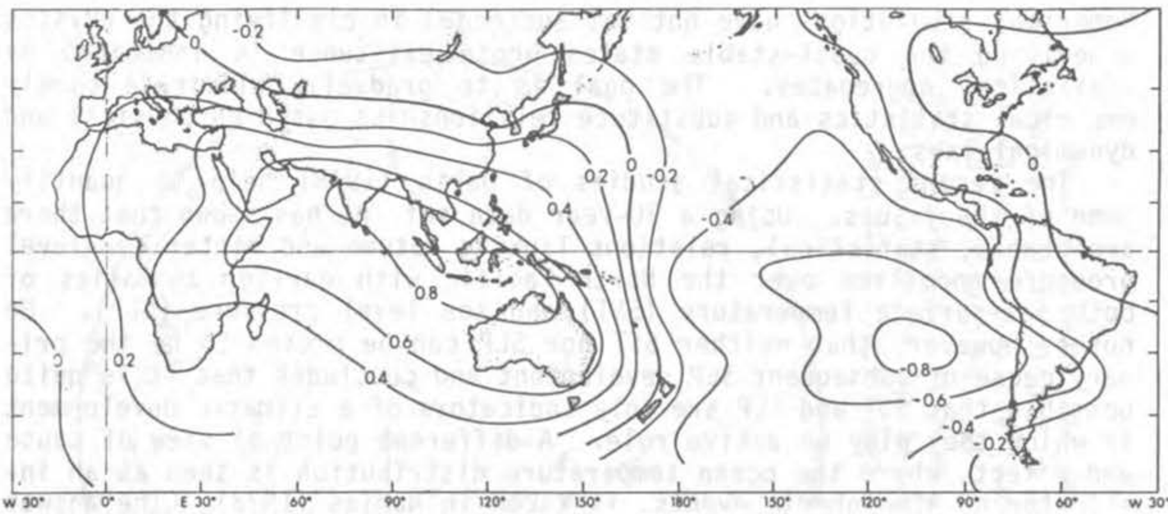

Figure 3. The Southern Oscillation. Correlation of annual mean atmospheric pressure with Jakarta (after Berlage).

Source: Wyrtki (1982)

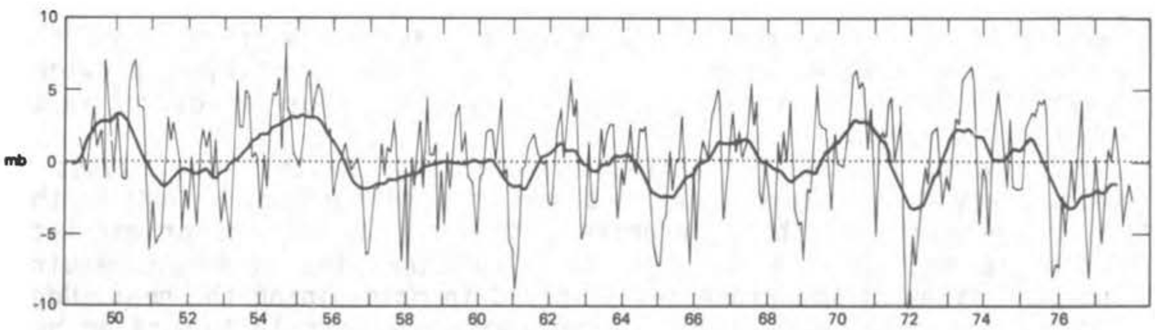

Figure 4. The Southern Oscillation as given by the difference of atmospheric pressure between Easter Island and Darwin, Australia from 1949 to 1978 relative to a mean of 10.3 millibars. The thin line gives monthly means, the heavy line the 12-month running mean.

Source: Wyrtki (1982)

Numerical simulations have not yet succeeded in clarifying the physics underlying the quasi-stable states whose existence is indicated by statistical aggregates. The goal is to gradually eliminate purely empirical statistics and substitute relationships based on physical and dynamical laws.

The recent statistical studies of Davis (1978) help to quantify some of the issues. Using a 30-year data set, he has shown that there are stable, statistical, relations linking autumn and winter sea-level pressure anomalies over the North Pacific with earlier anomalies of both sea-surface temperature (SST) and sea level pressure (SLP). He notes, however, that neither SST nor SLP can be proved to be the primary cause of subsequent SLP development and concludes that it is quite possible that SST and SLP are only indicators of a climatic development in which they play no active role. A different point of view of cause and effect, where the ocean temperature distribution is seen as an instigator of atmospheric events, is taken in Namias (1973). The answer to this question can be resolved only by a combination of long time-series data and process-oriented experiments.

A number of recent observational, modelling, and theoretical studies provide a dynamical interpretation of the teleconnections between tropical sea-surface temperatures and mid-latitude circulation patterns. A synthesis of observational results from the work of Rasmussen and Carpenter (1980), Horel and Wallace (1981) and a number of other authors is presented in Figure 5. Similar patterns of atmospheric response have been obtained in a number of recent general circulation modelling experiments (e.g., see Shukla and Wallace [1983], and Blackman et al., [1983]). The pattern of response resembles the steady state solutions of Hoskins and Karoly (1981) of the linearized barotrophic vorticity equation with Rossby-wave propagation of a sphere with forcing from the tropics. These new dynamical insights provide a physical foundation for the statistical relationships discovered in the empirical studies.

In spite of the identified correlations, neither the causes of sea-surface temperature anomalies nor the mechanisms by which they alter the state of the atmosphere either in the tropics or mid-latitudes are yet fully known. It is clear that the research required includes study of the processes involved in maintaining the heat budget and the general circulation. Recent work suggests that a major priority in the ocean research should be a basin-wide study in the tropical and subtropical Pacific, building on the results obtained to date. Such studies must include work on the dynamics of the mixed layer and the upper ocean, since the upper ocean heat budget is crucial to the development and transport of anomalies in the tropics.

In the past decade, our knowledge of the mid-latitude mixed layer has increased considerably through studies by NORPAX, JASIN, MILE, STREX, etc. This is due to measurements of temperature and velocity microstructure, from which turbulence level and dissipation rates can be estimated; and by acoustic measurements by which internal waves and lower-frequency ocean response can be described. With these data it

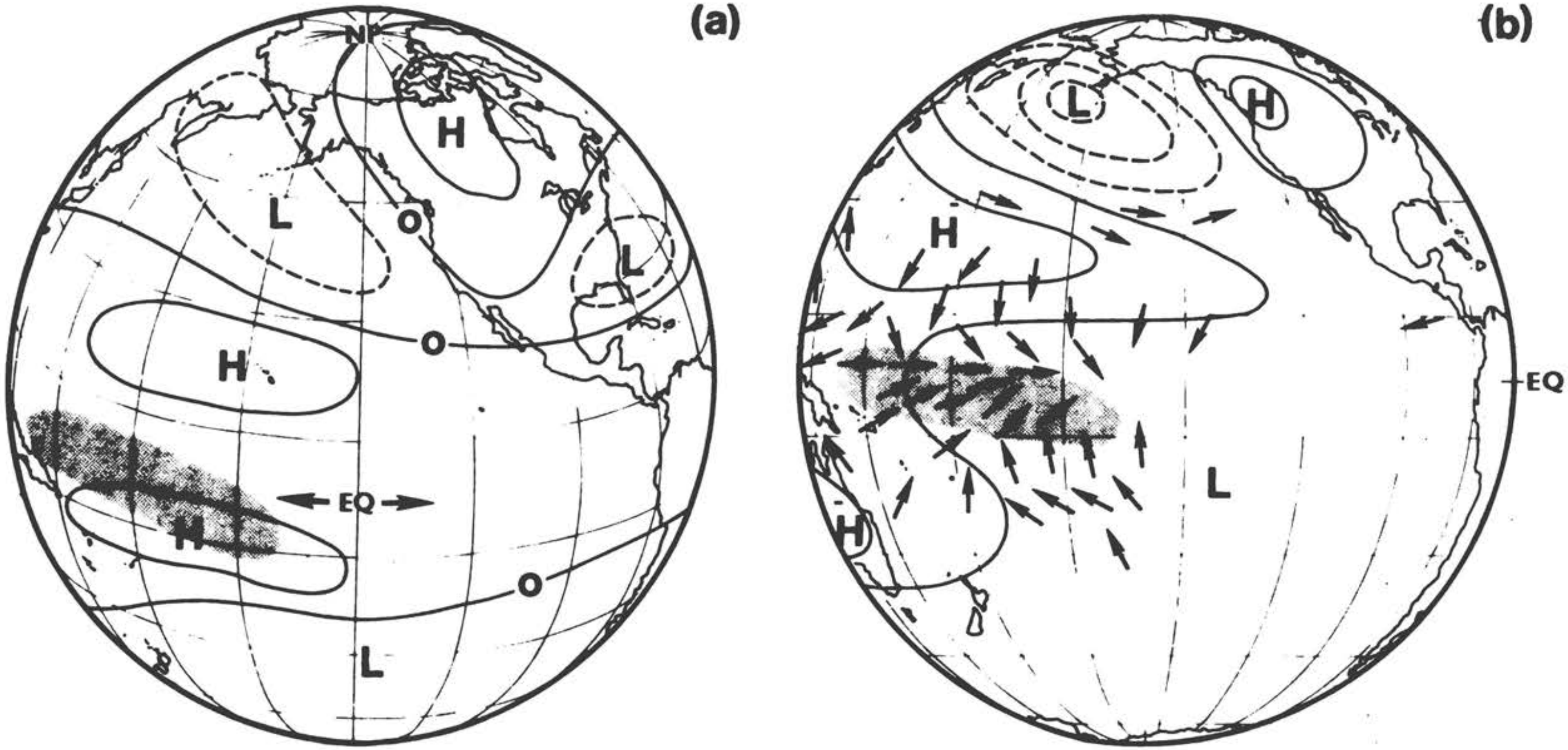

Figure 5. (a) Hypothesized global atmospheric response to sea-surface temperature anomalies associated with El Nino events in the equatorial Pacific during the Northern Hemisphere winter, based on observational studies and experiments with general circulation models. The shaded area denotes the region of enhanced precipitation and the contours show geopotential height anomalies at the jetstream level [after Horel and Wallace (1981) and Rasmussen and Carpenter (1982)]. (b) Shading denotes enhanced precipitation, arrows represent surface wind anomalies, and contours represent sea-level pressure anomalies from Shukla and Wallace [(1983)].

has been discovered that some one-dimensional, or rather simply inferred three-dimensional, dynamical balances are very effective in accounting for the relationship between surface fluxes and changes in the mixed layer.

In contrast, the low-latitude mixed layer has not been studied extensively due to the complex three-dimensional physics and the relative difficulty of making and interpreting measurements in the strong three-dimensional flow patterns which occur there. The equatorial mixed layer, however, is as thick as the main thermocline, and its relative importance in advection and storage of heat, momentum, and vorticity is most likely greater there than in mid-latitudes. Furthermore, through it the ocean feeds latent heat back to the tropical atmosphere, and it is important for air-sea modelling to have physically realistic models of the oceanic processes in the mixed layer. Such a program to study the dynamics of equatorial mixed layers would most logically build on the results of the PEQUOD and EPOCS programs and would be a part of the basin-wide study mentioned above.

The many recent developments in the study of the tropical Pacific which demonstrate its interaction with the tropical and mid-latitude atmosphere suggest that the time is ripe for a large-scale experiment there. Existing programs (e.g., PEQUOD, EPOCS, and the United States/France XBT programs) are producing results and additional international programs are under discussion. By the middle of the decade, it is likely that there will be sufficient understanding of the oceanographic processes in these areas so that a large-scale experiment can be begun. Equatorial Atlantic studies, including the United States SEQUAL (Seasonal Equatorial Atlantic) experiment, planned to begin shortly, will also produce results of importance in designing studies of global scale.

The general features of the required ocean studies have been identified by the Equatorial Pacific Ocean Climate Studies program, which focuses on the origin and dynamics of sea-surface temperature anomalies and their effect on the atmosphere, and by the Pacific Equatorial Ocean dynamics program which focuses generally on the response of the tropical ocean to changing atmospheric forcing. The continuation and extension of these studies together with measurements of circulation, sea-surface temperature, and heat storage in the sub-tropical Pacific would be the essential elements of such a large-scale study. Satellite measurements of wind stress by scatterometer are crucial to this study. The satellite based altimetry studies discussed for WOCE would be an important and useful unifying feature, but because of the failure of the geostrophic relation at the equator, direct measurements of currents will also be an essential feature. However, it is possible that part of the tropical study could be carried out by the use of drifting buoys and other *in situ* techniques before the TOPEX satellite is in orbit.

We endorse the concept of a large-scale tropical Pacific ocean-atmosphere study, note the importance of satellite wind stress measurements, and we recommend that full support be provided for the on-going planning for such a study. We urge that liaison be maintained between the United States working groups and international planning for a Tropical Ocean-Global Atmosphere program which will involve studies in the tropical Atlantic and Indian Oceans as well.

3.3 Ocean Heat Transport

It is only during the last decade that ocean heat transport has been appreciated as a significant factor in maintaining the present global heat balance. For most of the century, it was generally thought that the atmosphere accomplished the bulk of this poleward transport. Recent studies, however, suggest that the ocean is the predominant transporter of heat in latitudes equatorward of 30 degrees. In addition, it apparently makes a significant contribution to the heat transport even in polar latitudes, especially in the southern hemisphere (Vonder Haar and Oort, 1973; Trenberth, 1979).

This polewards transport of energy mediates the climate of the earth. The total flux of energy by atmosphere and oceans appears to be determined by global constraints (Stone, 1978): the solar constant, the size of the earth, the tilt of the earth's axis, and the hemispheric mean albedo. These constraints suggest a strong negative feedback between oceanic and atmospheric energy fluxes. A full answer to the question of partitioning is not available, but Newell (1974) has speculated that changes in this relative fraction could lead to dramatic changes in climate, even ice ages.

Certainly it is true that, for example, the wintertime climate of western Europe is ameliorated by the warm Atlantic waters that flow northward past the British Isles and Norway. Stommel (1979) points out that in December, at ocean weather station Juliette, which lies at the latitude of Birmingham, Amsterdam, and Berlin, the sea receives on the average only 50 watts of sunlight per m^2, but gives up to the atmosphere some 224 watts per m^2, which the warm waters have carried from lower latitudes. This vertical flux of heat must be supplied by a horizontal convergence of heat in the upper layers of the ocean. Direct measurement of this convergence would involve an extensive array of moored instruments recording velocity, temperature, and depth over several years time and is not presently technically feasible. Stommel has calculated the geostrophic velocity using the beta-spiral technique and shown that if the net heat transport there extended only over 55 percent of the full width of the North Atlantic Ocean it could account for the full global meridional ocean heat flux called for by Vonder Haar and Oort.

Traditionally, ocean heat transport has been estimated indirectly either from charts of air-sea energy exchange based on bulk aerodynamic formulae or by subtracting atmospheric energy transport from the combined atmosphere-ocean transport derived from radiation studies. Ocean heat transport estimated by either of these methods has large uncertainties. Recently, it has been shown that, given appropriate measurements, accurate estimates of ocean heat transport can be made directly from the distribution of currents and temperature in subtropical regions of the ocean (Bryden and Hall, 1980). The appropriate measurements include: direct current and temperature observations in western boundary currents (such as the Gulf Stream), transoceanic hydrographic sections, and wind stress values across the section. The direct method has been most successfully applied in the North Atlantic, which is the only ocean basin for which direct current measurements in western boundary currents have been made.

One element of a program to investigate the ocean's role in the global heat budget then should be to make appropriate measurements so that ocean heat transport can be estimated in each ocean basin. Such data is available in the North Atlantic, but direct current measurements are required, particularly in the Kuroshio in the North Pacific Ocean, in the Brazil Current in the South Atlantic, and over the relatively shallow New Zealand Plateau in the South Pacific. For comparison of North Atlantic and North Pacific, a transoceanic hydrographic section across the North Pacific at about 30°N is also required. From these and existing measurements, heat transport as well as the transport of other properties such as fresh water or nutrients could be estimated directly in the North Pacific, South Atlantic, and South Pacific Oceans.

A second element of such a program should be to explore the temporal variability of ocean heat transport across one section, for example, across 25°N in the Atlantic where the direct method has already been used. Indirect methods suggest a large annual cycle in ocean heat transport across 25°N which ought to be resolved by a time series of hydrographic stations at carefully selected sites on both sides of the section and by monitoring the currents and temperature of the Gulf Stream flow through Florida Straits. The hydrographic station on the western side of the ocean is a logical U.S. contribution to the program; on the eastern side of the ocean, such a station could be operated by a number of countries including the United Kingdom, Federal Republic of Germany, and France.

A third element of the program should be to understand the mechanisms of ocean heat transport. While it is often difficult to distinguish them in practice, oceanographers generally consider three mechanisms of poleward heat transport: a horizontal circulation in which warmer water flows poleward in a western boundary current and colder water returns equatorward in the mid-ocean; a vertical-meridional circulation in which warmer, shallower water flows poleward and colder, deeper water returns equatorward; and an eddy heat flux in which temporally and spatially varying currents act to transport heat poleward with no net time- or space-averaged flow. In the direct estimate of heat transport across 25°N in the Atlantic, the dominant mechanisms are the heat transports due to the horizontal circulation and to the vertical-meridional circulation.

While the heat eddy transport is small at 25°N, and it appears that it is small in all subtropical mid-ocean regions, there is evidence that eddy heat flux is the dominant mechanism of heat transport across the polar front in the Antarctic circumpolar region (Bryden, 1979; deSzoeke and Levine, 1981). This evidence suggests the hypothesis that eddy heat flux is likely to be important in regions of large eastward flowing currents such as the Antarctic Circumpolar Currents and the eastward extensions of the Gulf Stream and Kuroshio. Because oceanic eddies have small horizontal scales relative to the length of transoceanic sections, it would be difficult to measure eddy heat flux across a transoceanic section. A more reasonable approach is to make several local sets of measurements in these eastward currents in an attempt to relate eddy heat flux to the larger-scale properties of the circulation. Such studies should eventually allow eddy heat fluxes to be parameterized in terms of the large-scale properties so that the eddy-heat flux contribution could be reliably estimated.

Determination of air-sea fluxes is also important for large-scale interaction studies. The equatorial and mid-latitude western boundary current regions are the areas where most of the heat exchange through the sea-surface takes place. Air-sea interactions are also particularly intense during storms, and these special periods may account for much of the exchange that causes sea-surface temperature anomalies and water mass formation events. For example, one day of a 70 m/sec wind causes as much exchange as one year of the 10 m/sec trade winds (since energy exchange is proportional to velocity cubed). Because of the temporal intermittence of these flux processes, the ability to observe frequently and with geographic coverage is essential and satellite sensing is required.

Determination of the air-sea fluxes, heat storage, and heat transfer by eddies and circulation is needed to understand the heat budget of the ocean in the upper ocean in the different regions of the world. Determination of all of these is beyond the capability of present oceanographic research tools. Existing in situ techniques are too expensive and have too short a lifetime. Extended geographical coverage, particularly in the southern hemisphere, as well as more data on fluxes and currents are needed.

In the Southern Ocean, the exchanges of heat and water between the atmosphere and the ocean lead to the formation of the major intermediate and bottom waters of the world ocean. The climate scale fluctuations in the formation of these waters and of fluxes between the Southern Ocean and other oceans is a potential source of climatic variability to the ocean-atmosphere system. The largest flux of heat from the abyssal waters of the world ocean takes place here due to water-mass modification and heat exchange. For this reason alone, the region is a major link between the oceans and changes of the earth's climate.

In addition, the Southern Ocean is the source for Antarctic Intermediate Water. This water mass (which is probably formed at least in part by seasonal sea-ice interactions) spreads out into lower latitudes of both hemispheres just below the upper thermocline of the tropical ocean. It is expected that the resulting interaction between this water mass and the surface layers is a process which can transmit the climatic fluctuations of high southern latitude to the rest of the world ocean--and subsequently to the atmosphere through sea-air interactions in the tropical and equatorial region.

Pilot studies by the International Southern Ocean Studies program in the Southwestern Pacific near New Zealand have shown a significant poleward heat transport and large eddy variability in this region. The mean advective geostrophic heat flux is near zero. But the eddy heat flux seems large, at least from two isolated measurements. Further study of mechanisms of heat transport would be valuable here.

But there is a stronger reason. The recent historical data study of 120 years of sea-surface temperature anomalies and wind anomalies in this region by Fletcher et al. (1982) shows the possible existence of long period oscillations of basin-wide scale. The wind anomalies appear to develop first in high latitudes, perhaps reflecting changes in the thermal forcing in the Antarctic regions. We noted earlier the many previous studies that show significant correlations between tropical time series and northern hemisphere upper air circulation patterns. It is entirely possible that the tropical patterns are linked in turn to high southern latitudes, so that what we are seeing is truly global interaction.

A program to address these problems would include: SST satellite measurements, moorings for heat and momentum transport, and surface drifters to track the surface flow. In this way the generation, development, and advection of SST anomalies could be studied, and the mechanisms of heat flux could be further elucidated. Such a focused program would be done most logically in conjunction with a circumpolar set of tide or shallow pressure gauges on islands which monitor the large-scale circulation.

In view of all the points noted above, in the first session of the SCOR/IOC Committee on Climate Change and the Ocean (CCCO), a proposal was made to consider an experiment to measure the meridional heat transport by the ocean with greater accuracy than is given by the current estimates. The shorelines on each side of the ocean would form the eastern and western boundaries while transocean sections approximately along two parallels of latitude separated by 20° to 50° could form the northern and southern boundaries. Incoming and outgoing radiation would be monitored by satellite. Because the measurements would thus form a cage over the atmosphere and ocean, such experiments are called "Cage" experiments.

A JSC/CCCO Working Group, in looking at the feasibility of such "Cage" studies, has considered the vital issues which must be addressed before any large-scale heat budget ocean-atmosphere heat budget experiment can be carried out (JSC/CCCO, 1982). These are addressed in the context of estimating the heat flux in different ways for comparison, under the principle that the best experiment allows a comparison of techniques.

The working group considered three techniques: (1) direct oceanographic measurement of ocean fluxes at the northern and southern boundaries of the cage; (2) estimating ocean heat flux from residuals of the total global heat flux less the atmospheric heat flux from satellite measurements; and (3) area integration of fluxes across the sea-surface. It concluded that only the first two are potentially viable. These two estimates can be compared with the best available estimate of the sea-surface heat fluxes obtained by standard methods. Accordingly, the working group believes that it is feasible, with a reasonable amount of effort, to approach the accuracy limits required for a Cage experiment in the North Atlantic Ocean.

A number of specific preliminary studies are considered essential by the working group before a full-scale study could be carried out. These include development of Kuroshio and Gulf Stream transport measurements, process-oriented studies to understand mixing in the main thermocline, air-sea flux budget studies to reduce the uncertainties in the individual flux terms, and modelling and observing systems simulation experiments to identify gaps in the existing meteorological networks necessary to meet specified error bounds. The deployment of the Earth Radiation Budget Experiment (ERBE) system for the calibration of the earth's radiation balance was considered crucial, as was the development of atmospheric water vapor and precipitation measurement programs in mid-latitudes.

We endorse the general concept of a program to understand processes of heat transport in the ocean and note that the elements of such a program and a number of specific studies as outlined generally above could be carried out now. We recommend that further planning for large-scale studies be carried out in the context of and in collaboration with the WOCE planning in the light of the close connection between circulation and heat transport. We emphasize the importance of the Earth Radiation Budget Experiment, noting that current planning has this experiment scheduled for operation before the satellites for TOPEX and WOCE can be in orbit. Simultaneous operation is preferred for maximum information.

3.4 Polar Ocean and Ice Interactions

As our understanding of the global climate system has increased, the significant role of polar regions in the dynamics of the ocean and the atmosphere at the longer time scales has become more apparent. This was noted early in the planning for the Global Atmospheric Research Program and is a central theme of GARP Publication Number 19, the Polar Sub-Program. A set of climate related studies is recommended in that document.

Ocean heat flux and air-sea interaction are linked in the polar regions where they work together to determine the ice edge position, ice concentration, cyclogenesis, and SST anomalies. Recent estimates (Oort and Vonder Haar, 1976) show that at high northern latitudes the atmosphere transports more heat northwards than does the ocean, but that seasonal changes in heat storage appear to be much larger in the ocean than in the atmosphere. Both positive and negative feedbacks are possible, and many of these are discussed in the documents listed above.

We focus here on two areas of particular importance: a satellite-based ice monitoring program for both poles, and a study of oceanic and ice circulation and transport in the Greenland-Iceland-Norwegian Sea.

The extent of sea ice determines the solar radiation absorbed and hence the heat balance of a hemisphere. In addition, the presence of sea-ice alters the exchanges of heat, moisture, and momentum between the ocean and the atmosphere. On the seasonal time scale, it is possible that some of the factors that influence climatic anomalies such as the winter of 1977 are related to the regions of maximum cyclogenesis which are also the margins of the wintertime ice packs.

The dynamic and thermodynamic pack ice models developed thus far have suggested that the Arctic ice pack may respond dramatically to a general warming. We know that both the Arctic and the Antarctic exhibit large interannual variations in temperature and extent of snow and ice. When considering the possibility of a global warming due to increased CO_2 in the atmosphere in this century, both modelling and observations suggest that the warming in the polar regions will probably be three to five times greater than the global average.

The effect of such a warming on the cryosphere is still unclear. Floating pack ice in the Arctic and Antarctic would probably decrease, and some modelling studies suggest that the pack ice in the Arctic Ocean could disappear entirely. Such an event could create a condition in the Arctic Basin that has not occurred for the past several million years or more. Continual monitoring in this region is required because apparently our first warning of CO_2 warming will come from the polar regions. We endorse the continuance of long-term passive microwave measurements of sea-ice cover.

The heat budget in the Arctic Ocean has been studied by SCOR Working Group 58 (1979). Since the passage between Greenland and Spitsbergen, (Fram Strait) is the only deep water entrance to the Arctic Basin, the net heat flux through the Strait is a crucial element in the heat budget of the Arctic surface. Thus this flux is one of the controlling factors in the pack ice extent and distribution. The working group recommends continued and expanded study of the fluxes of both sensible (oceanic) and latent (ice) transport through the Strait. Of particular importance is the study of the time scale of the major fluctuations and their causes. This area is the northern boundary of the Greenland-Iceland-Norwegian Sea, and thus studies of the currents in this region will be required so that we know enough to determine the heat flux at the northern boundary.

The Greenland-Iceland-Norwegian Sea is of interest for another climatic reason. Bottom water is formed there annually by processes which are not fully understood. Part of this dense cold water flows southward into the North Atlantic and thence into the rest of the oceans. Slightly warmer and less saline deep water is found in the Iceland area and overflows Denmark Strait. Some of this bottom water is also advected northwards through the Fram Strait, and it fills the Arctic Basin to within about 800 m of the surface. Conceivably, changes in the transport or temperature of bottom water flowing into the Arctic Ocean could change the ice cover in that region. Feedbacks are possible whereby the resulting change in climate could lead to a differing production rate of bottom water. Worthington (1970) has speculated that the replacement by warm water of cold water flowing out of this region is one of the reasons that Europe is warmer than its latitude would indicate.

Thus it appears that a very logical and important climatic study for the north polar region would be the measurement of the volume and heat transports through the Fram Strait, the volume and heat transport of the dense overflows into the North Atlantic, and the volume and heat transport of the warm water that enters the Greenland-Iceland-Norwegian Sea through the Faroe-Shetland Channel. The difference in heat transport between all these must be roughly the heat given off to the atmosphere, which could be monitored by satellite and drifting buoys. Year-to-year variations in these fluxes should be studied in relation to the climate of northern Europe. An overall heat budget study of the Greenland-Iceland-Norwegian Sea (GINS) could contribute to the large-scale budget studies ("cage experiments") discussed above. Such a "GINS-cage" could be a test site for larger cage programs and some of the data could be used as a northern boundary condition for North Atlantic heat budget studies. The program has the advantage of easier logistics than the entire North Atlantic, and it has all of the same processes involved in the heat budget including in addition the variable ice cover. The southern boundary of this GINS-cage would be a logical northern boundary for the cage being considered by the JSC/ CCCO. Aspects of this smaller cage could bear on processes and logistics for the larger one.

The area of the possible programs is indicated on Figure 6. The northern boundaries would be the Greenland-Spitsbergen Passage and the Spitsbergen-Norway line along approximately 20°E longitude. The eastern and western boundaries would be the Denmark Strait and the Iceland-Scotland Passage.

We recommend that, in view of the potential importance of this region for climate and the strong international interest, a study be carried out on the feasibility of an energy-budget experiment, the study to draw on the on-going plans for satellite ice monitoring.

3.5 Ocean Climate Through Chemistry and the Sediment Record

Climate models suffer from a lack of a long-term instrumental record for validation and comparison. To fill this need, a climatic data set from noninstrumental (proxy) records was assembled during the 1970's by the CLIMAP (Climate: Long-range Investigation, Mapping, and Prediction) project. This research had two principal objectives: first, to reconstruct the global pattern of sea-surface temperatures, fronts, and currents at the time of the last glacial maximum, 18,000 years ago; and second, to obtain time series at key sites which document the behavior of the ocean and the cryosphere over the past 700,000 years with time scales of 5,000 to 100,000 years.

The first objective was achieved by the production of charts of surface ocean temperatures for an average August and an average February during the last ice age (CLIMAP, 1976, 1981). These reconstructions are based on statistical analysis of the distribution of temperature-sensitive marine micro-organisms found at appropriate

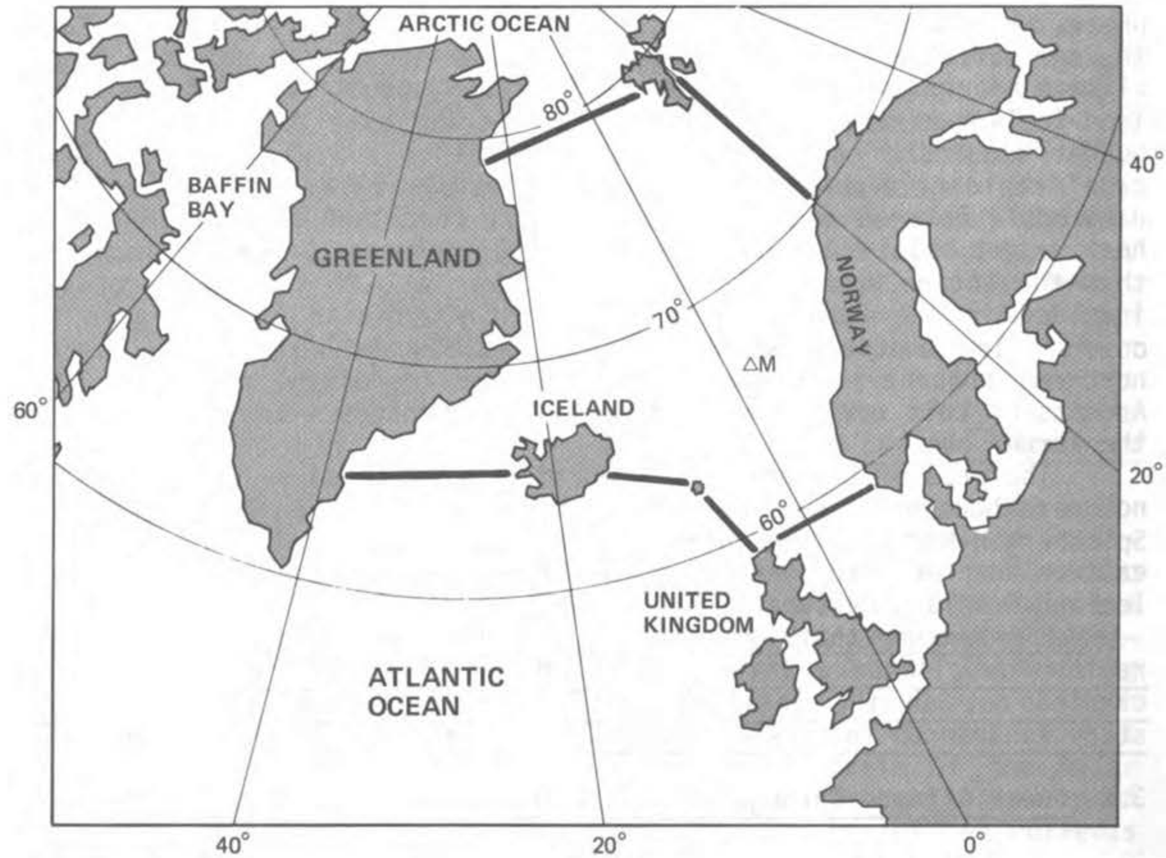

Figure 6. Boundaries for a Greenland-Iceland-Norwegian Sea Cage. "M" is weathership Mike.

levels in deep-sea cores. In general, the ice-age map is characterized by a compression of climatic belts toward the centers of the subtropical oceanic gyres, which thus form the thermally and geographically stable parts of the climate system.

CLIMAP also assembled new data on the global distribution and thickness of ice-age ice sheets and major mountain glacier systems, ice-age sea level, and on the distribution of the albedo of land-, snow-, and ice-covered surfaces. These data are precisely those required for global models of the general atmospheric and coupled ocean-atmospheric circulation. Initial results of these simulation experiments have been reported by Gates (1976a; 1976b) and by Manabe and Hahn (1977). These experiments suggest, among other things, that the continents were drier during the ice age, and that the weakening of the Asian ice-age monsoon was due mainly to the effect of increased continental albedo.

The CLIMAP project also found good agreement between climatic periodicities inferred from deep-sea cores and those predicted by the Milankovitch theory ice-ages based on variations of the earth's orbit around the sun (Hays et al., 1976; Kominz and Pisias, 1979; Morley and Hays, 1981; Ruddiman and McIntyre, 1981; Imbrie and Imbrie, 1980). This agreement is of particular interest because the new generation of radiation-balance models, recently applied to the problem, has yielded results that support the astronomical theory (Suarez and Held, 1976, 1979; Weertman, 1976; Pollars, 1978). These theoretical developments, combined with advances in paleoceanography, suggest that research should now aim to identify the mechanisms by which different parts of the climate system, including sea-ice and the surface and deep waters of the ocean, respond to changes in radiative boundary conditions. Except for studies of the annual cycle, we know of no other problem in climate dynamics where the changes in primary external forcing terms are known so precisely.

As summarized above, investigations by the CLIMAP project emphasized oceanic variations on time scales from 5,000 to 100,000 years. New paleoceanographic studies for climate should give emphasis to shorter time scales. Recommendations made in the 1978 report on elements of a research strategy for the United States climate program by the U.S. GARP Committee Climate Dynamics Panel argue for studies of two time scales: 10 to 100 years, and 100 to 1000 years. The first would cover the past 1000 years, the second, the past 30,000 years. It is clearly desirable to augment our present meager knowledge of oceanic variations on the shorter of these time scales, and we note that a Panel on High-Resolution Paleoclimatology has been formed by the CCCO to promote and organize such research. Unfortunately, the number of sites from which such high-resolution oceanic records can be obtained is rather small, and they are generally located in coastal areas. We also note that the SPECMAP project, begun in 1979, plans to document oceanic variation at time scales of 1000 years and longer at a number of open-ocean and ocean-margin sites.

The advent of a new technique for sampling deep-sea sediments--hydraulic piston coring--has opened up new opportunities for investigating the history of the ocean over a wide range of frequencies. In essence, this technique makes it possible to obtain undisturbed, deep high-resolution piston cores. These cores show for the first time detailed records of oceanic change for intervals over the past 100 million years (Prell et al., 1980). Since it is known that the geometry of the ocean basin changed radically during this period, it is possible to investigate in detail how the ocean responded to these changes in boundary conditions. Evidence is mounting that this resolution is marked by step-like shifts in equilibria, each regime being characterized by a characteristic set of means, amplitudes, and frequencies.

Although the direct relevance of these investigations to climatic studies on the scale of decades or less is small, they nevertheless offer valuable opportunities for testing the ability of models to simulate the oceanic and atmospheric response to boundary conditions strikingly different from those on which the models were tuned.

We recommend that support be given to the follow-on to the SPECMAP project if it can be shown that the data from the sediment record exists for filling spectral gap between the instrumental record on time scales of 10 years to the geological record on time scales of 5000 years. The support of collection of new sediment data for such studies is of lower priority than the field studies recommended in earlier sections because of the inherently longer time scales involved.

3.6 Schedule for Programs

Table 2 presents a sequence of on-going and planned ocean climate related programs.

TABLE 2. Proposed and On-Going Field Programs Related to Ocean Climate Studies

Program	Time								
	1978	1979	1980	1981	1982	1983	1984	1985	1986
<u>Pacific</u>								TOGA →	
	EPOCS →								
			PEQUOD →						
				XBT Monitoring					
<u>Atlantic</u>				STACS →					
						SEQUAL →			
				XBT Monitoring					
				TTO (Atlantic)					
<u>Global Circulation</u>									WOCE →
									TOPEX →
						Hydrographic sections →			Gravity field mission →
								TTO (Pacific)	
<u>Polar</u>	ISOS →						GINS-Cage (tentative)		
							Antarctic Heat Flux (tentative)		

4. TECHNIQUES, TECHNOLOGY, AND DATA MANAGEMENT

In this section, we cover four topics that have broad application to the field of ocean research for climate. Long time series of ocean measurements are rare, but essential to the description of changes relevant to the time scales of interest here. Empirical data analysis and theoretical modelling are integral to the specific topics discussed in Chapter 3, but are also singled out here for special emphasis. New technology for climate-related measurements will be required, and a review of needs is presented here. Data management, particularly satellite data, remains a difficult problem, and must be addressed soon if we are to deal successfully with new satellite data streams.

4.1 Long Time Series of Ocean Measurements

The steering committee sponsored a three-day meeting in La Jolla, February 23-25, 1981 on the general subject of long time series for ocean measurements. The data from existing long term stations was discussed, a number of problems with the data and data products were identified, techniques for long term monitoring were considered, and new areas for long time series were identified.

The information from the La Jolla meeting was used as a U.S. contribution to the JSC/CCCO sponsored meeting in Tokyo, May, 1981 (JSC/CCCO, 1981b, 1982b). A number of recommendations were made at the the Tokyo meeting; those specifically relevant to long time series are summarized below:

The Tokyo meeting participants recognized that long time series of ocean measurements are extremely rare and that their value increases nonlinearly with longevity. For this reason, the participants strongly recommended that the highest priority be given to the continuation of existing long time measurements of the ocean. Of particular importance to the United States are the existing stations Paulirus (off Bermuda), PAPA (off Western Canada), CALCOFI (off California), BRAVO (Labrador Sea) and the previous station GOLLUM (off Hawaii). In addition, the participants noted the importance of archiving with the time series data the full details of the observing techniques, sensor situations, and data reduction algorithms used during the life of the series. High priority was given to including sea-level data, data from drifting platforms and compressed CTD/STD data sets in oceanographic data centers. The group urged that national data centers abstract all available CTD/STD profiles for use in climate study and make them available for international exchange.

In terms of expansion of the existing network of stations, the Tokyo meeting participants endorsed the concept of "exploratory" time series--essentially test stations in potentially crucial areas--and recommended that the velocity, temperature, and salinity in the major straits and passages of the world ocean be monitored because of their importance as indices of the general circulation of the ocean, and because existing technology makes it possible to obtain useful measurements now. Straits and passages of particular importance include the Florida Straits, the Straits of Tokahara (south of Kyushu), the Denmark Straits, the Straits of Gibraltar, and the Drake Passage. In addition, it was recommended that the non-coastal islands of the world be instrumented for sea-level, sea-surface temperature, and a meteorological suite of measurements including near surface wind, air temperature, and surface air pressure. The techniques successfully developed by K. Wyrtki (1979) in his Pacific Ocean monitoring need to be applied more widely. This group recommended that the existing XBT network be extended to a basin-wide coverage in all oceans, and that other indices of importance including satellite cloud drift winds throughout the tropics as a indicator of surface stress, and precipitation activity be derived from satellite infrared imagery and microwave radiometry.

The particular importance of salinity measurements for the understanding of long-term oceanic changes was noted and it was recommended that surface water samples be collected using various ships of opportunity and that improved instruments be developed to record valuable salinity data. An example of the latter is the expendable CTD.

The meeting also addressed the issue of detecting CO_2 changes with long time series of measurements, noting the importance of finding time series where the background of interannual variability is relatively low. Sea-level appears to be a particularly sensitive indicator of CO_2 warming effects (Barnett and Baker, 1982). The reasons for this are that a change of 0.1 percent of the global land ice cover will result in a sea-level change of about 5 cm, and increases in ocean temperature will presumably accompany increases in atmospheric temperature. A change of 0.5°C in the upper 200 m of the ocean would increase sea-level by roughly 2 cm. Both of these effects would be global in nature and it may be easier to detect a signal which is coherent in all the oceans than to identify one that is regional.

We recommend that those agencies responsible for funding ocean measurements give high priority to the development of exploratory time series and to the continuation of existing long time series as indices of the general circulation of the ocean. The recommendations from the La Jolla and Tokyo meetings should be used in the continuing development of a system for ocean climate monitoring. We recommend the establishment of new stations at selected passages and island stations around the world. A full systems and cost analysis needs to be carried out in the context of existing knowledge. The phantom weathership concept (see Chapter 2) needs further development and comparison with other techniques of gathering long time series in the open ocean such as ships-of-opportunity.

4.2 Empirical Data Analysis

Much of what we currently know about climate variability has come from past empirical studies of data. Most of these studies have been hampered in one way or another by a lack of uniform data coverage. Thus, it is not unusual to find the studies concentrating on relations between simple variables or between a given climatic field, e.g., SST and other climate variables.

Recently, much more effort has been put into the derivation and analysis of fields of climatic variables. For instance, the Pacific trade wind field and its relation to the underlying oceanic variability has been studied extensively. New, more powerful statistical methods of analyzing these data have also been brought into the scientific studies. Some initial efforts have been made to relate the outputs of various general circulation models to fields of actual observed climatic variability.

Perhaps one of the most fundamental trends that appears to be developing in current data analysis is toward the use of what can be called "secondary" or "derived" data fields. In this case, instead of working simply with sea-surface temperature or wind velocity, scientists have used these quantities and others to estimate air/sea heat exchange or heat advection by ocean currents. Thus, the data analyses are being used to understand and evaluate the physical processes that might cause climate change as opposed to simply describing these changes.

The future in empirical data analysis for climate purposes looks promising for a number of reasons. First, the attempts to work with derived data fields that represent physical processes is only in its infancy. Although much work remains to be done in this area, many of the primary data fields needed for these computations are, or will soon be available. Much of the problem in the past has been due to the difficulty of assembling the necessary global data fields and of handling problems that occur in data-void areas, e.g., much of the southern oceans. It is expected that these problems will be at least partially resolved over the next decade.

The analyses that are applied to the data noted above will become increasingly sophisticated. The statistical methods will be considerably more substantial than the simple correlation analyses. Furthermore, the future holds promise of a blend between statistics and dynamics related to the derived fields noted above. Eventually, many may come to view the climate problem in a statistical dynamical framework since a large portion of the observed climate data consists of what appears to be noise.

4.3 Ocean Modelling for Climate Research

A workshop on this subject was sponsored by the Climate Dynamics Panel of the U.S. Committee for the Global Atmospheric Research Program in late 1979 (U.S. GARP Committee, 1980). The recommendations of that report are current and valid; we will summarize the major ones here. The reader is referred to the report for further details. The recommendations cover two areas: those activities that show the most promise for solving problems of immediate interest and new areas that are required in ocean modelling. The workshop report noted that primitive equation models are relevant and should continue to be used where there are no alternatives. In addition, efforts should be made to develop non-diabatic quasi-geostrophic models of greater efficiency than primitive equation models. Systematic comparison between the two types of models should be carried out, and the feasibility of intermediate models with increased efficiency should be explored, particularly for use in equatorial modelling. The quasi-geostrophic models should be extended to allow for greater vertical resolution, and efforts should be continued to parameterize sub-eddy scale processes important to mixing, diffusion, and dissipation in eddy-resolving models.

The report also recommended that efforts be increased to understand the physical basis for parameterizations of the eddy scale and to improve the parameterizations now used in coarse grid models. For mixed-layer modelling, the group recommended that mixed-layer models be embedded into regional and general circulation models and that the improved general circulation models be tested against field data in climatically important regions.

We endorse these recommendations, and recommend the vigorous development of highly parameterized simple models leading to a hierarchy of models of increasing complexity because such models are of special importance for understanding climate interactions.

In terms of new resources for modelling, the workshop participants estimated that computer usage per year in the next few years for climate related research will require the equivalent of a dedicated CRAY-1 computer. This means that the ocean modelling requirements could be roughly equal to the present atmospheric climate modelling requirements.

4.4 New Technology for Measurements

In this area a recent review of technology needs for oceanographic research and monitoring was made by the Office of Technology Assessment (OTA) (1980) of the United States Congress. The report, Technology and Oceanography, was prepared at the request of the National Ocean Policy Study of the Senate Committee on Commerce, Science and Transportation. The report assessed the status of ocean technology in use today and analyzed future problems and opportunities in a variety of areas, including climate-related research.

The OTA report notes that in compliance with the National Climate Act of 1978, there are a variety of planning and feasibility studies and field experiments designed to study ocean processes for climate, but that these studies are generally not adequately funded in the area of technology development. It is probable that substantial increases in technology funding for climate studies will be required by the middle to late 1980's if the research plans gain agency and congressional support.

The National Climate Program Office, set up to coordinate interagency activities, does not have the resources necessary to assure this coordination, to initiate action to fill the inevitable gaps (particularly in technology) between the diverse agency programs, nor the authority to direct a coordinated research program.

It was noted that there is no comprehensive statement of the technology needs for climate research in the ocean. Four general areas of future climate technology needs were identified. Under the heading of platforms, the report noted the need for organization of a worldwide ships-of-opportunity program including a satellite data network and centralized data processing center; the need for substantial improvements and cost reductions in expendable buoys and probes that can be deployed from a variety of platforms; and some commitment to fixed, long-term moorings to replace the weathership stations that are slowly being phased out.

In the area of in situ sensors, the report emphasized the need for new types of upper ocean current meters for use in circulation and heat flux studies; for improved, cost effective sensors for integrating temperature, salinity, and velocity from fixed moorings; and for improved, cost effective sensors for measuring profiles of temperature, salinity and velocity as part of the worldwide ships-of-opportunity program. It also pointed out the importance of new technology for measuring the humidity content of the atmosphere near the ocean surface and the need for sensors to monitor trace gas constituents such as CO_2.

A satellite-based remote-sensing capability was considered crucial to the program. The report noted that the satellites are the only way to get global coverage for surface topography, global wind fields, and sea-ice extent. It stressed that the current errors in estimating sea-surface temperature are as large as the air-sea temperature difference which controls the sensible heat flow (Barnett, et al. 1979). The need for remote measurement of humidity near the ocean surface was emphasized as was the need for measurements of precipitation over the ocean. Improved synoptic coverage of sea-ice location, thickness, and age is also required. Satellite systems for communication of in situ measurements were noted as also important overall to the climate program.

In data management, the report stressed that the anticipated flood of data from satellite sensors will necessitate major efforts to upgrade data management and handling capabilities in order to retain existing satellite data, merge historical data of various types with satellite data, and provide easy and economical access to data bases. In addition, since current and future climate studies involve a mix of data of highly variable quality from several different sources, one or more dedicated centers and a dedicated computer will probably be required for the distribution and processing of future climate related data. The overall mix of sensors and platforms will necessitate a good data collection and coordination system, some of which is already in place.

4.5 Requirements for Satellite Data Management

Most recent reports emphasize the need for adequate management systems for the handling of oceanic data, particularly the satellite data streams. The OTA report notes that the existing data are voluminous but poorly managed in available archives. Since the volume of data from satellite and other platforms is overwhelming, it is critical to invest in competent, well-managed archives. Data-formating, distribution, and management must be carried out in such a way that academic, government, and private users can have timely access to the data they want at minimum cost. This management structure does not now exist. In addition to developing the management structure, there must be a long-term commitment to carry through the climate program. In summary, the OTA report recommends retention of satellite data for climate studies, improved management systems, well-managed data archives, and efficient data distribution networks.

NASA workshops have identified the need for a well organized, easily usable data base of satellite observations. The content of such a base must be readily available to the average scientific user who will be interested not only in type and distribution of data but also details associated with its derivation from calibration algorithms, characteristics of the instruments that acquired the data, orbital information, etc. In order to prevent the further loss of space-based climate data due to inadequate archiving and to initiate as soon as possible the accumulation of a global data base adequate to support a wide range of climate research, the NASA Workshop on Air-Sea Interaction (1980) recommended that procedures for developing third generation (images, parameters, and statistical summaries) and fourth generation (the above plus information on variables derived by combining information from two or more sensors) archives be developed, and that both surface and satellite based data should be included in order to preserve essential information about all spatial and temporal scales in the data.

Also recommended was a center for climatic data analysis that would develop new techniques for collection, analysis, archiving, display, and distribution of the satellite data; analyze and archive conventional and satellite data, and that would use models to develop assimilation techniques and analyze the raw data.

It is clear from all of these reports that a joint effort between oceanographers and meteorologists is required to establish a capability for data planning, processing, and distribution. Such a system will require a major computing facility and be cooperative with governmental laboratories. It is essential that the user community have input on how the system is designed and operated. One possible mechanism would be to have a board of experts on an ocean data utilization system with the members drawn from the oceanographic and meteorological community.

It is important to realize that such planning should start immediately. Data from SEASAT, NIMBUS, TIROS, and GOES would be far more useful and usable by scientific and commercial users if it were processed in both a real-time mode and retrospectively with data products and data links available on-line from a computer rather than through hard copy only.

We recognize the difficulties that have been identified by the OTA report and NASA reports and we recommend that further joint efforts between oceanographers and meteorologists be undertaken to develop an adequate data management scheme. In particular, the current efforts now underway at JPL/Cal Tech are an important first step in this direction. This program, called the "Ocean Pilot System," provides for a user interface, extensive display and data manipulation capabilities, access to data bases from SEASAT, JASIN, and NIMBUS-7 data, and networking for remote users. The lessons from this effort will be invaluable for design of a comprehensive satellite data management system.

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