



A Review of the Draft Ocean Research Priorities Plan: Charting the Course for Ocean Science in the United States
Committee to Review the JSOST Research Priorities Plan, National Research Council

ISBN: 0-309-66783-6, 74 pages, 8 1/2 x 11, (2006)

This free PDF was downloaded from:
<http://www.nap.edu/catalog/11789.html>

Visit the [National Academies Press](#) online, the authoritative source for all books from the [National Academy of Sciences](#), the [National Academy of Engineering](#), the [Institute of Medicine](#), and the [National Research Council](#):

- Download hundreds of free books in PDF
- Read thousands of books online, free
- Sign up to be notified when new books are published
- Purchase printed books
- Purchase PDFs
- Explore with our innovative research tools

Thank you for downloading this free PDF. If you have comments, questions or just want more information about the books published by the National Academies Press, you may contact our customer service department toll-free at 888-624-8373, [visit us online](#), or send an email to comments@nap.edu.

This free book plus thousands more books are available at <http://www.nap.edu>.

Copyright © National Academy of Sciences. Permission is granted for this material to be shared for noncommercial, educational purposes, provided that this notice appears on the reproduced materials, the Web address of the online, full authoritative version is retained, and copies are not altered. To disseminate otherwise or to republish requires written permission from the National Academies Press.

A Review of the Draft Ocean Research Priorities Plan:
Charting the Course for Ocean Science in the United States

Committee to Review the JSOST Research Priorities Plan

Ocean Studies Board

Division on Earth and Life Studies

NATIONAL RESEARCH COUNCIL
OF THE NATIONAL ACADEMIES

THE NATIONAL ACADEMIES PRESS
Washington, D.C.
www.nap.edu

THE NATIONAL ACADEMIES PRESS 500 Fifth Street, N.W. Washington, DC 20001

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 study was supported by a contract between the National Academy of Sciences and OCE-0602432 award/grant number from the National Science Foundation. Any opinions, findings, conclusions, or recommendations expressed in this publication are those of the author(s) and do not necessarily reflect the views of the organizations or agencies that provided support for the project.

Library of Congress Cataloging-in-Publication Data

or

International Standard Book Number 0-309-0XXXX-X

Library of Congress Catalog Card Number 97-XXXXX

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

Copyright 2006 by the National Academy of Sciences. All rights reserved.

Printed in the United States of America

THE NATIONAL ACADEMIES

Advisers to the Nation on Science, Engineering, and Medicine

The **National Academy of Sciences** is a private, nonprofit, self-perpetuating society of distinguished scholars engaged in scientific and engineering research, dedicated to the furtherance of science and technology and to their use for the general welfare. Upon the authority of the charter granted to it by the Congress in 1863, the Academy has a mandate that requires it to advise the federal government on scientific and technical matters. Dr. Ralph J. Cicerone is president of the National Academy of Sciences.

The **National Academy of Engineering** was established in 1964, under the charter of the National Academy of Sciences, as a parallel organization of outstanding engineers. It is autonomous in its administration and in the selection of its members, sharing with the National Academy of Sciences the responsibility for advising the federal government. The National Academy of Engineering also sponsors engineering programs aimed at meeting national needs, encourages education and research, and recognizes the superior achievements of engineers. Dr. Wm. A. Wulf is president of the National Academy of Engineering.

The **Institute of Medicine** was established in 1970 by the National Academy of Sciences to secure the services of eminent members of appropriate professions in the examination of policy matters pertaining to the health of the public. The Institute acts under the responsibility given to the National Academy of Sciences by its congressional charter to be an adviser to the federal government and, upon its own initiative, to identify issues of medical care, research, and education. Dr. Harvey V. Fineberg is president of the Institute of Medicine.

The **National Research Council** was organized by the National Academy of Sciences in 1916 to associate the broad community of science and technology with the Academy's purposes of furthering knowledge and advising the federal government. Functioning in accordance with general policies determined by the Academy, the Council has become the principal operating agency of both the National Academy of Sciences and the National Academy of Engineering in providing services to the government, the public, and the scientific and engineering communities. The Council is administered jointly by both Academies and the Institute of Medicine. Dr. Ralph J. Cicerone and Dr. Wm. A. Wulf are chair and vice chair, respectively, of the National Research Council.

www.national-academies.org

COMMITTEE TO REVIEW THE JSOST RESEARCH PRIORITIES PLAN

ROBERT DUCE, *Co-Chair*, Texas A&M University, College Station
NANCY TARGETT, *Co-Chair*, University of Delaware, Lewes
DENISE BREITBURG, Smithsonian Environmental Research Center, Edgewater, Maryland
DAVID CONOVER, State University of New York, Stony Brook
CORTIS COOPER, Chevron Energy Technology Company, San Ramon, California
CATHERINE CUNNINGHAM BALLARD, Michigan Department of Environmental Quality, Lansing
GERALD GALLOWAY, University of Maryland, College Park
ROBERT KNOX, Scripps Institution of Oceanography, La Jolla, California
WILLIAM KUPERMAN, Scripps Institution of Oceanography, La Jolla, California
ROGER LUKAS, University of Hawaii, Honolulu
JAMES SANCHIRICO, Resources for the Future, Washington, DC
ANDREW SOLOW, Woods Hole Oceanographic Institution, Massachusetts
DENISE STEPHENSON HAWK, The Stephenson Group, Atlanta, Georgia

Staff

SUSAN ROBERTS, Study Director
FRANK HALL, Program Officer
SUSAN PARK, Associate Program Officer
TONI MIZEREK, Christine Mirzayan Science and Technology Policy Graduate Fellow
JEFFREY WATTERS, Christine Mirzayan Science and Technology Policy Graduate Fellow
SARAH CAPOTE, Senior Program Assistant

OCEAN STUDIES BOARD

SHIRLEY A. POMPONI (*Chair*), Harbor Branch Oceanographic Institution, Fort Pierce, Florida
LEE G. ANDERSON, University of Delaware, Newark
JOHN A. ARMSTRONG, IBM Corporation (retired), Amherst, Massachusetts
WHITLOW AU, University of Hawaii at Manoa
ROBERT G. BEA, University of California, Berkeley
ROBERT DUCE, Texas A&M University, College Station
MARY (MISSY) H. FEELEY, ExxonMobil Exploration Company, Houston, Texas
HOLLY GREENING, Tampa Bay Estuary Program, St. Petersburg, Florida
DEBRA HERNANDEZ, Hernandez and Company, Isle of Palms, South Carolina
CYNTHIA M. JONES, Old Dominion University, Norfolk, Virginia
ROBERT A. LAWSON, Science Applications International Corporation, San Diego, California
WILLIAM A. KUPERMAN, Scripps Institution of Oceanography, La Jolla, California
FRANK E. MULLER-KARGER, University of South Florida, St. Petersburg
JOAN OLTMAN-SHAY, NorthWest Research Associates, Inc., Bellevue, Washington
ROBERT T. PAINE, University of Washington, Seattle
S. GEORGE H. PHILANDER, Princeton University, New Jersey
RAYMOND W. SCHMITT, Woods Hole Oceanographic Institution, Massachusetts
DANIEL SUMAN, Rosenstiel School of Marine and Atmospheric Science, University of Miami, Florida
ANNE M. TREHU, Oregon State University, Corvallis

Staff

SUSAN ROBERTS, Director
FRANK HALL, Program Officer
SUSAN PARK, Associate Program Officer
ANDREAS SOHRE, Financial Associate
SHIREL SMITH, Administrative Coordinator
JODI BOSTROM, Research Associate
NANCY CAPUTO, Research Associate
SARAH CAPOTE, Senior Program Assistant

Preface

Ocean science is complex and multidisciplinary. The committee congratulates the Joint Subcommittee on Ocean Science and Technology (JSOST) for undertaking, for the first time, a comprehensive planning activity that involved all federal agencies that support ocean-related research in the United States. The effort recognizes that synergies between and within agencies can enhance the outcomes and impacts of ocean science for the benefit of science and society. The task was challenging and difficult, and the final plan should be of great benefit to the ocean sciences community.

David Halpern (U.S. Office of Science and Technology Policy), Margaret Leinen (National Science Foundation), and Richard Spinrad (National Oceanic and Atmospheric Administration), co-chairs of the JSOST, approached the National Research Council Division of Earth and Life Studies in August, 2005, to assist with this research planning effort through a 3-phase process. In Phase 1, the National Research Council (NRC) staff provided summaries of recommendations from NRC reports published in the past 7 years that related to the themes, pillars, and cross-cuts identified by the JSOST. In Phase 2, an ad hoc NRC committee was appointed to review the draft Ocean Research Priorities Plan (ORPP) and provide recommendations for improvement. This document represents that review. The committee was given three months from the release of the complete draft to provide this review. In Phase 3, the committee is to provide an assessment of the final ORPP within 6 months of the release of the final plan.

The committee held three meetings and four conference calls during the preparation of its Phase 2 report. The first conference call was held on April 13, 2006, in preparation for the Denver workshop organized by JSOST to provide community input into the development of the draft research plan. The committee's first meeting was held on April 18–20 in conjunction with the Denver workshop. At this workshop, committee members observed the various breakout sessions that discussed the themes and cross-cut areas outlined in the planning document. The committee received the first draft of the Ocean Research Priorities Plan on July 28, 2006. The July draft did not include the near-term priorities, which were included in the complete draft, *Charting the Course for Ocean Science in the United States: Research Priorities for the Next Decade*, released to the committee on August 30, 2006.

The committee met twice following the April workshop to discuss the draft research plan and to write this report: on August 8–10, 2006, in San Francisco to discuss the plan made available on July 28, and again on September 25–26, 2006, in Houston to complete the review of the draft plan, including the near-term priorities.

ACKNOWLEDGEMENTS

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

deliberative process. We wish to thank the following individuals for the participation in their review of this report:

LEE G. ANDERSON, University of Delaware, Newark
KATHERINE ANDREWS, Coastal States Organization, Washington, DC
ROBERT G. BEA, University of California, Berkeley
PAULA COBLE, University of South Florida, St. Petersburg
RUSS E. DAVIS, Scripps Institution of Oceanography, La Jolla, California
EARL H. DOYLE, Shell Oil (retired), Sugar Land, Texas
PAUL G. GAFFNEY, Monmouth University, West Long Branch, New Jersey
EDWARD D. HOUDE, University of Maryland, Solomons
EDWARD LAWS, Louisiana State University, Baton Rouge
MOLLY MCCAMMON, Alaska Ocean Observing System, Anchorage
PETER J. MCCARTHY, Harbor Branch Oceanographic Institution, Fort Pierce, Florida
MARCIA K. MCNUTT, Monterey Bay Aquarium Research Institute, Moss Landing, California
ANTHONY F. MICHAELS, University of Southern California, Los Angeles
R. KEITH MICHEL, Herbert Engineering Corporation, Alameda, California

Although the reviewers listed above have provided many constructive comments and suggestions, they were not asked to endorse the conclusions or recommendations nor did they see the final draft of the report before its release. The review of this report was overseen by **Kenneth H. Brink**, Woods Hole Oceanographic Institution, Woods Hole, Massachusetts, and **Alexander H. Flax**, Consultant, Columbia, Maryland. Appointed by the National Research Council, they were responsible for making certain that an independent examination of this report was carried out in accordance with institutional procedures and that all review comments were carefully considered. Responsibility for the final content of this report rests entirely with the authoring committee and the institution.

The committee and its co-chairs are greatly appreciative of the significant support that we have received from the staff of the Ocean Studies Board. Their assistance facilitated the work of the committee and contributed to an enjoyable and productive working committee environment. In particular we thank Dr. Susan Roberts who served as study director, associate program officer Dr. Susan Park, and program officer Dr. Frank Hall who was involved with the early work of the committee. Ms. Toni Mizerek and Mr. Jeff Watters were a great help during their tenure with the National Research Council as graduate fellows for the Ocean Studies Board. We are also grateful to Ms. Sarah Capote for her superb skills in organizing the committee meetings and conference calls. The committee feels that the positive, accomplishment-oriented attitudes of each of these individuals enhanced the final outcome of the study.

Robert Duce, *Co-Chair*
Nancy Targett, *Co-Chair*

Contents

EXECUTIVE SUMMARY	1
SUMMARY	4
1 INTRODUCTION AND BACKGROUND	13
Origin of the NRC Study, 14	
Review of the Draft Ocean Research Priorities Plan, 14	
2 DEVELOPMENT OF THE OCEAN RESEARCH PRIORITIES PLAN	15
Addressing the Statement of Task, 17	
3 ASSESSMENT OF THE OVERALL PLAN	18
Introduction, 18	
Organization of the Draft Plan, 18	
Themes, 21	
Priorities, 21	
Time frame, 23	
Presentation, 23	
Addressing the Statements of Task, 24	
Recommendations, 25	
4 EVALUATING THEMATIC PRIORITIES AND CROSS-THEME INTEGRATION	26
Introduction, 26	
Recommendation, 29	
Stewardship of Our Natural and Cultural Ocean Resources, 31	
Clarity and Appropriateness of the Priorities, 32	
Appropriateness of the balance among substantive research areas, and between research activities such as observation, modeling, and communicating results, 32	
Recommendations, 32	
Increasing Resilience to Natural Hazards, 33	
Clarity and Appropriateness of the Priorities, 33	
Appropriateness of the balance among substantive research areas, and between research activities such as observation, modeling, and communicating results, 33	
Recommendation, 34	
Enabling Marine Operations, 34	
Clarity and Appropriateness of the Priorities, 34	
Appropriateness of the balance among substantive research areas, and between research activities such as observation, modeling, and communicating results, 35	
Recommendations, 36	
The Ocean's Role in Climate, 36	
Clarity and Appropriateness of the Priorities, 36	
Appropriateness of the balance among substantive research areas, and between research activities such as observation, modeling, and communicating results, 37	
Recommendations, 38	
Improving Ecosystem Health, 38	
Clarity and Appropriateness of the Priorities, 38	
Appropriateness of the balance among substantive research areas, and between research activities such as observation, modeling, and communicating results, 40	

Recommendation, 41	
Enhancing Human Health, 41	
Clarity and Appropriateness of the Priorities, 42	
Appropriateness of balance among substantive research areas, and between research activities such as observation, modeling, and communicating results, 42	
Recommendation, 43	
5 INTERDISCIPLINARY AND MULTI-MISSION OCEAN RESEARCH	44
The Need for Interdisciplinary and Multi-Agency Approaches, 44	
Implementation, 46	
Addressing the Statements of Task, 46	
Recommendations, 47	
6 EVALUATION OF NEAR-TERM PRIORITIES	48
Selection of Near-Term Priorities, 48	
General Comments, 48	
Comments on Specific Near-Term Priorities, 50	
Forecasting the Response of Coastal Ecosystems, 50	
Comparative Analysis, 50	
Sensors for Marine Ecosystems, 50	
Assessing MOC Variability, 50	
Addressing the Statements of Task, 51	
Recommendations, 51	
7 INFRASTRUCTURE AND INTELLECTUAL CAPITAL	53
Physical Infrastructure, 53	
Information Infrastructure, 54	
Intellectual Capital, 54	
Addressing the Statements of Task, 55	
Recommendations, 55	
REFERENCES	57
APPENDIXES	59
A Committee and Staff Biographies, 59	
B Acronyms, 64	

Executive Summary

The development of the draft Ocean Research Priorities Plan (ORPP), *Charting the Course for Ocean Science in the United States: Research Priorities for the Next Decade*, represents the first coordinated national research planning effort involving all federal agencies that support ocean science. The Bush Administration's U.S. Ocean Action Plan directed the National Science and Technology Council's Joint Subcommittee on Ocean Science and Technology (JSOST) to prepare the ORPP and Implementation Strategy. The JSOST asked the National Research Council (NRC) to review both the draft and final ORPP.¹ This activity has importance and value for opening lines of communication among and across government agencies, academia, non-governmental organizations, and industry that cannot be overstated. The draft plan succeeds in a number of important ways: (1) the central link between the ocean and society is clear and well articulated, (2) the six broad themes around which the report is organized succeed in capturing the main ocean-related issues facing society in a comprehensive and coherent way, (3) three important overarching opportunities are identified, (4) the role of research in improving technology, monitoring, management, and fundamental understanding of the ocean, coasts, and Great Lakes is recognized, and (5) the plan includes research priorities in the social sciences, a necessary component for improving ocean stewardship.

In this review, the committee identifies ways in which the draft ORPP may be improved. Most of these are highlighted in the Summary and detailed in the body of the report. The major recommendations of the committee are summarized below.

VISION AND CHALLENGES

The draft ORPP lacks a bold and compelling vision for ocean research in the next decade. The specific challenges for ocean science should clearly follow from the problems and opportunities facing society, but these connections are not clearly articulated in the current plan. The rationale and process for the selection of the individual priorities does not emerge from the supporting text. Additionally, the plan gives minimal reference to other major efforts to identify national priorities for ocean science and technology such as the U.S. Commission on Ocean Policy, the Pew Oceans Commission, and the Joint Ocean Commission Initiative. The draft plan thus misses an opportunity to build upon previous efforts and recognize the evolution of a consensus on the future direction of ocean science and technology to meet societal needs.

RECOMMENDATION: The Ocean Research Priorities Plan should provide a bold and compelling vision for the future of ocean science research. This vision should be placed near the front of the plan and referenced throughout to help integrate the discrete sections of the document. To provide

¹ Although the Implementation Strategy will be released with the final ORPP, it has not been released in draft form and is not part of the NRC review. The draft ORPP includes a brief description of the topics to be addressed in the Implementation Strategy in the section titled "The Next Steps."

Prepublication

a clearer connection between the research priorities and the underlying societal needs, the plan should identify a series of challenges for science and society under each theme.

LINKAGES BETWEEN THEMES

Major ocean research and management challenges facing our nation require multidisciplinary and interdisciplinary approaches that cut across the defined missions of individual government agencies as well as non-governmental organizations, academia, and industry. Because of the complexity of these challenges, sophisticated approaches that draw on strong expertise from a range of disciplines will be needed to acquire and apply knowledge for scientifically sound management strategies. The value of strong interdisciplinary and multi-agency approaches to ocean research has been highlighted in the U.S. Ocean Action Plan and other recent documents. While the importance of interdisciplinary science, as well as approaches that cut across agency missions, are acknowledged, the draft plan lacks a consistent approach across the themes that: (1) identifies the linkages among themes and intersections of the research priorities; (2) elucidates the mechanisms to foster collaborative research in those areas; and (3) emphasizes the need for strong interagency cooperation and collaboration.

RECOMMENDATION: The ORPP should provide a more comprehensive description of the needs and opportunities for multidisciplinary research, as well as research partnerships (multi-agency and agency/academic/industry/international), for each societal theme. The Implementation Strategy for the ORPP should evaluate the adequacy of existing mechanisms for interagency and agency-academia collaborative research to identify opportunities to improve collaboration among sectors.

IMPLEMENTATION AND METRICS

Issues central to assessing the feasibility of the plan and its responsiveness to the nation's needs are the level of funding and the strategy for implementation. Because the Implementation Strategy was not included in the draft research plan, it is difficult to assess either feasibility or responsiveness.

The ORPP does not provide a straightforward explanation of the scope of the plan. Is the ORPP designed to include all ongoing research activities, in addition to new programs to be initiated in the next 10 years? The document would be easier to interpret if it stated whether the plan incorporates existing research programs and, if so, identified the new initiatives associated with each of the themes.

In addition, the plan lacks metrics by which the plan will be judged. There is a huge range in the costs of conducting the various types of ocean research addressed in the priorities that logically will have an impact on the implementation of the research plan and progress towards achieving societal goals. Similarly, new capabilities in forecasting ocean processes (both physical and biological) will not benefit society unless there is a strategy for converting research programs into operational activities. Presumably, the JSOST's Implementation Strategy will specify the general funding assumptions under which the plan was developed. The ORPP lists items to be addressed in the Implementation Strategy including the roles and responsibilities of different agencies in implementing the plan, mechanisms for cooperation and coordination of agency activities, and performance measures. Since this information was not provided in the draft ORPP, it is difficult to evaluate if the research priorities can be achieved.

RECOMMENDATION: The ORPP should clearly state the goals, challenges, and research priorities of the plan and how these relate to existing programs and new initiatives. The implementation strategy should include a schedule by which these priorities could reasonably be addressed, a set of benchmarks by which progress could be assessed, and a strategy for maintaining new capabilities (made possible by advances in knowledge of processes, modeling, and technology) through the transfer from research to operational programs.

ORGANIZATION OF THE PLAN

The sections *Expanding the Scientific Frontier: The Need for Fundamental Science*, *Overarching Opportunities*, *Making a Difference*, and *Opportunities for Progress* are key pieces of the draft ORPP that define critical areas for science and technology efforts as well as for infrastructure and education needs. In a sense, the issues identified in these sections underlie or cut across all of the themes and research priorities. The placement in the current draft plan, with 3 sections at the end of the document, misses the opportunity to effectively integrate each of these cross-cutting areas into the ORPP.

RECOMMENDATION: The plan should be reorganized to include a discrete section devoted to cross-cutting elements that are central to the vision for ocean research. The concept of cross-cutting themes used in the Planning Document should be reintroduced and moved toward the beginning of the plan as a way to reinforce the importance of these elements in creating the foundation for progress on the societal themes.

Summary

INTRODUCTION

The draft Ocean Research Priorities Plan (hereafter referred to as the plan or the ORPP), entitled *Charting the Course for Ocean Science in the United States: Research Priorities for the Next Decade*, represents the first coordinated national research planning effort involving all federal agencies that support ocean science. The draft plan was prepared by the National Science and Technology Council's Joint Subcommittee on Ocean Science and Technology (JSOST) as called for in the U.S. Ocean Action Plan. This important effort has opened up valuable lines of dialogue between and across government agencies, academia, non-governmental organizations, and industry.

The purpose of the draft plan is to: "...develop and present ocean research priorities that address key interactions between society and the ocean." The draft plan identifies six themes that represent key areas of human interaction with the ocean:

- Stewardship of Our Natural and Cultural Ocean Resources
- Increasing Resilience to Natural Hazards
- Enabling Marine Operations
- The Ocean's Role in Climate
- Improving Ecosystem Health
- Enhancing Human Health

21 longer-term (~10 years) and four near-term (2-5 years) research priorities are defined with regard to these themes. There is no ranking of either the long-term or near-term priorities. Commonalities among the themes, particularly in the areas of infrastructure and education, are identified. The plan concludes by articulating a path forward that is defined by three overarching opportunities and the four near-term priorities. The plan also mentions the development of the Implementation Strategy, a document that will be released with the final version of the ORPP.

The co-chairs of the JSOST requested that the National Research Council (NRC) conduct a review of the draft plan. An ad hoc committee was assembled to perform this review, guided by the study's statement of task (see Box S-1). In Phase 1, the NRC staff provided summaries of recommendations from NRC reports published in the past 7 years that related to the themes, pillars, and cross-cuts identified by the JSOST. This report represents the results of phase 2 of this study. The findings and recommendations of this phase are summarized below.

Box S-1

Statement of Task

Phase 2

An ad hoc committee will review the draft plan for the “Ocean Research Priorities Plan” prepared by the JSOST with input from a public workshop in April, 2006. The review will address the following questions about the draft plan as a whole:

- (1) Is the plan responsive to the nation's needs for ocean research?
- (2) Does it effectively link proposed science and technology developments to benefits to the nation with regard to quality of life, safety and security, economic growth, environmental sustainability, and education?
- (3) (a) Are the priorities for each theme area clear and appropriate? (b) Is the time frame for attaining these priorities realistic?
- (4) Is there an appropriate balance (a) between short-term (2-5 years) and longer-term (5-10 years) priorities, (b) among substantive research areas, and (c) between research activities such as observations, modeling, and communicating results?
- (5) Does the document adequately identify multidiscipline and/or multi-mission issues?
- (6) Does the document identify the highest near term research priorities to address the goals and expected societal results?
- (7) Does the plan adequately consider the following resources: physical infrastructure, information infrastructure, and intellectual capital?

In its review, the committee will consider the scientific and stakeholder community comments at the April 2006 workshop and other comments received during the public comment period.

- (8) The committee will also evaluate whether the format of the workshop promoted the open exchange of ideas and suggestions for improvement.

Phase 3

In this phase, the committee will provide an overall assessment of the revised (final) plan with an emphasis on:

- How has the plan evolved in response to the NRC review and other community input?
- What specific improvements should be reflected in future planning efforts for ocean research?

ASSESSMENT OF THE OVERALL PLAN

General Considerations

The draft plan succeeds in five important ways. The critical link between the ocean and society is clear and well articulated. The overarching opportunities defined in the plan are appropriate and compelling. The six broad themes succeed in capturing the main ocean-related issues facing society in a comprehensive and coherent way, and the draft plan acknowledges the role of fundamental curiosity-driven research in meeting the nation's needs for ocean research and development. The plan includes research priorities in the social sciences, a necessary component for improving ocean stewardship.

The organization of the draft plan undercuts many of the valuable points that are made in the text. In particular, the sections *Expanding the Scientific Frontier: The Need for Fundamental Science*, *Opportunities for Progress*, the *Overarching Opportunities*, and *Making a Difference* all support the societal themes; however, these sections are not effectively presented in the current draft. The plan could

be revised in accordance with the JSOST's Ocean Priorities Framework (April 2005), which would address many of these shortcomings. The Framework document lists the following sections as part of the draft plan: Vision, Challenges, Principles and Critical Elements, Themes, Goals, Resources, and Evaluating Performance. Alternatively, the use of cross-cutting themes, similar to the approach in the Planning Document (April, 2006) but placed early in the ORPP, would give these issues greater emphasis and illustrate many of the connections among the themes.

The plan is not successful in translating the link between society and the ocean into a bold and compelling vision for ocean research in the next decade. There is a need to draw a clearer connection between the problems and opportunities facing society and the specific challenges for ocean science, and between these specific challenges and the research priorities identified in the plan. A stronger connection to earlier reports, such as those by the U.S. Commission on Ocean Policy (USCOP), the Pew Oceans Commission, and the Joint Ocean Commission Initiative, would underscore the coherent evolution of thinking about the critical role that ocean research plays in addressing the pressing problems and opportunities facing society, and put the draft plan into a larger context.

RECOMMENDATIONS:

- **The Ocean Research Priorities Plan should provide a bold and compelling vision for the future of ocean science research. This vision should be placed near the front of the plan and referenced in the discussions of the theme priorities. This would help to integrate the discrete sections of the plan.**
- **The plan should be reorganized to include a discrete section devoted to cross-cutting elements that are central to the vision for ocean research. The concept of cross-cutting themes used in the Planning Document should be reintroduced and moved toward the beginning of the plan as a way to reinforce the importance of these elements in creating the foundation for progress on the societal themes. In particular, the section *Expanding the Scientific Frontier: The Need for Fundamental Science* should be included as one of these cross-cuts and revised to strengthen the rationale for basic research.**

Themes and Priorities

The draft research priorities plan is organized around six societal themes. The themes successfully capture the main ocean-related issues facing society, have an interdisciplinary focus, and would benefit from a collaborative approach among federal, state and local agencies to integrate efforts. However, the draft plan is less successful at elucidating the linkages among these themes. These connections provide opportunities for identifying research priorities in addition to the ORPP's "Overarching Opportunities." Most of the thematic discussions address at least some issues that are linked to other themes. It would be helpful to enhance this discussion by taking a more consistent approach across the themes that: (1) carefully considers the interfaces between themes that intersect the key areas identified in the document, and (2) states the research needs and the mechanisms that will foster collaborative research in those areas.

The priorities within each theme are intended to guide effort toward the most important scientific objectives that must be reached to achieve the larger societal goals. This intention is only partially met. There are three primary concerns about the research priorities as they have been identified in the draft plan:

- The priorities that start with "Understand" sound like goals (not research priorities) while the priorities that start with "Apply" sound more like activities. This terminology communicates neither the value of ocean research nor the exciting challenges of the research which could then be used to develop milestones. In addition, many of the priority statements are so all encompassing that they do not indicate areas of research that should take precedence.

- The wording of some research priorities suggests that they do not involve research but rather implies that they are activities or operations that have no clear research component (e.g., Priorities 11 and 16).²
- The priorities do not convey the degree of difficulty or challenge in achieving them, making it difficult to realistically address the feasibility and time frame for their likely success.

To provide a clearer connection between the scientific priorities and the underlying societal needs, it would be useful if the plan articulated a series of “challenges for science and society.” These challenges would facilitate the identification of more tightly formulated research priorities under each theme and, importantly, would galvanize the ocean research and policy communities around the plan.

The draft plan identifies specific criteria by which research priorities were selected. However, the connection between these criteria and the specific priorities that were selected is not clear. Many of the priority statements are too broad to provide useful guidance. Also, it is unclear how the priorities relate to existing programs versus new initiatives. In some cases, the science described in the supporting text is too prescriptive with regard to the type of research activities required to address the priorities. In addition, the plan lacks both a schedule for addressing the priorities and specific benchmarks for measuring progress.

RECOMMENDATIONS:

- **The ORPP should clearly state the goals, challenges, and research priorities of the plan and how these relate to existing programs and new initiatives. The challenges should provide a more directed and inspiring rationale for the research priorities.**
- **Linkages among the themes should be clearly and consistently delineated in the supporting text for the research priorities. This could be accomplished through a simple statement that a given research priority will also forward the goals of other (specified) themes.**
- **The Implementation Strategy should include a schedule by which these priorities could reasonably be addressed, a set of benchmarks by which progress could be assessed, and a strategy for maintaining new capabilities (made possible by advances in knowledge of processes, modeling, and technology) through the transfer from research to operational programs.**

CONSIDERATION OF SPECIFIC THEMES

Stewardship of Our Natural and Cultural Ocean Resources

This theme encompasses stewardship of all aspects of the oceans and Great Lakes that represent commodities that fulfill essential human needs (e.g., goods and services like food and transportation) or values (e.g., recreation, tourism, preservation of culture). Although the plan discusses the need for taking an ecosystem-based approach, we suggest that the plan go one step further and use ecosystem-based management as the unifying concept to connect the numerous human uses of the ocean and research priorities. The committee also believes that the discussion of this theme fails to capture a sense of the imperiled status of our oceanic resources, the changes in human behavior necessary to achieve sustainability for renewable resources, and the difficulty of the science needed to solve these problems. In addition, the committee found it difficult to identify those resources and research priorities that the report classifies as cultural. The confusion stems from differences in how the plan discusses these resources. As it stands, the title of this theme implies that “natural” and “cultural” resources are of equal importance in terms of research priorities, yet none of the proposed priorities deal directly with cultural resources.

² See Table 4-1 for list of priorities.

RECOMMENDATIONS:

- **This theme should further emphasize the necessity of understanding all human impacts, not just fishing, and the feedbacks and cumulative impacts among them as the means of moving ocean governance to an ecosystem-based approach. In addition, a more compelling case should be made for these extremely important research priorities based on the greater scientific and public awareness of the decline in living resources and biodiversity.**
- **Research priorities for “cultural resources” should be identified or this topic should be removed from the theme.**

Increasing Resilience to Natural Hazards

This theme addresses societal risks and vulnerability to coastal and marine hazards, such as hurricanes and tsunamis. Emphasis is on understanding economic, environmental, social, and public health impacts from hazards, assessing and reducing risks and vulnerability, and making better forecasts of hazards. The priorities are appropriate, they address significant challenges, and there is a good balance among the priority areas. One deficiency in this theme is identification of the need for more process research to understand the nonlinear complexity of coastal inundation forcing, coastal erosion and sediment transport, and the health of coastal ecosystems has been highlighted in other reports, notably the report from the US COP. Hazards affect activity under all of the other themes and the discussion would be improved if these linkages were reviewed in brief.

RECOMMENDATION: The discussion of each of these priorities should be modestly expanded to provide greater specificity in the description of proposed research and to include research on coastal erosion and sediment transport.

Enabling Marine Operations

The draft plan defines marine operations to encompass commercial, recreational, and defense and security matters. The document provides justification for further research by correctly noting that marine operations can be expected to grow in importance in the future and therefore require a strong ocean science and technology base to maintain their vitality.

The priority statements in this section would be improved if made more specific and inspiring. The priorities, as presented, appear to be focused largely on marine transportation. Greater balance needs to be added by discussing how the proposed research will address marine operations related to defense, fishing and aquaculture, recreation, search-and-rescue, and energy and minerals exploitation. Some linkages to research within the other themes are identified but could be greatly expanded. Several of the suggested topics seem to involve operational issues that do not have a clear research component.

RECOMMENDATIONS:

- **Specific research requirements should be better identified and described.**
- **The plan should clearly distinguish operational activities from research goals. These goals should be broader than marine transportation and include areas such as national defense, fishing, and recreation.**

The Ocean’s Role in Climate

The title of this priority does not reflect the intent to address both the role of the ocean in climate *and* the impact of climate variability and change on the ocean. A more appropriate title might be “Oceans and Climate.” The priorities concern translation of basic research into practical use for societal benefits and address the required basic research components. Thus, while broad, they are complete and balanced. The priorities, as written, however, would be unlikely to capture the imagination and interest of non-scientists.

The critical needs for this theme are to expand sustained ocean observations and to conduct ocean process research required to improve numerical models for ocean-state estimation and prediction. It is not clear from the document how the pursuit of the thematic priorities will be coordinated to achieve this. The need for an integrated and sustained observing system is identified clearly. The importance of sustained and enhanced satellite observations for these climate priorities is called out, but the critical problem of ensuring continuity of existing and planned systems is not addressed. The required synergy between observations and models is not adequately addressed within the research priorities.

RECOMMENDATIONS:

- **Linkages with other themes should be improved.**
- **Priorities 12 and 14 should include discussions of sea level that explicitly recognize the importance of compounded sources of variability and change.**

Improving Ecosystem Health

This theme correctly addresses and acknowledges the complexity and importance of marine ecosystems, and suggests that there is much to be learned about the structure, function, and vulnerability of these systems. However, there are still some significant knowledge gaps that the plan misses, including: factors that control ecosystem stability and productivity, processes acting across interfaces, linkages between ecosystem types, and ultimately the relationship between marine ecosystems and the larger ocean-Earth-atmosphere system. Another important omission is research to develop indices that can be used to define healthy, resilient, and productive ecosystems. Some of the requirements that call for new and improved models implicitly require experiments and process studies. This could be made more explicit.

Productivity appears to be presented as the sole indicator of ecosystem health. However, by itself, productivity is a poor metric because marine systems can maintain stable levels of secondary production, set by nutrient loadings and physical conditions, even when severely perturbed by human activities. Ecosystem health needs to be assessed more broadly.

RECOMMENDATIONS:

- **A broader range of ecosystem responses (not just productivity) should be considered as measures for ecosystem health. Priority 15 would be improved by substituting wording such as the following: “Develop the capability to predict the impact of natural and anthropogenic processes on ecological systems.”**
- **Additional emphasis should be placed on increasing knowledge and understanding of factors contributing to maintenance and restoration of ecosystem health.**

Enhancing Human Health

This theme addresses a broad array of human health issues that relate to the ocean, with the major issues being pathogens, harmful algal blooms (HABs), contaminants, marine bioproducts, and biological models. These are appropriate and capture the most important research topics. The justification for this

theme is solid and should be easily understood by non-scientists, although the theme rationale could be strengthened by expanding beyond sickness and drugs to include the potential for major contributions to science in general. The research priorities capture a broad range of topics that are appropriate and will clearly advance science in this area. The priorities would be strengthened by clearer wording and by more explicitly describing important linkages with research priorities in other themes.

This theme includes an appropriate balance between substantive research areas, except for the inconsistent emphasis on basic research among the four research priorities. A strength is the recognition of the multidisciplinary nature of research needed to advance this theme.

RECOMMENDATION: More careful wording should be used so that the distinction between Priorities 18 and 19 is clear, the scope of recommended research is not unnecessarily constrained, and important linkages among research themes are described. The importance of both process studies and quantification of risk should be clearly emphasized.

INTERDISCIPLINARY AND MULTI-MISSION OCEAN RESEARCH

Major ocean research and management challenges facing our nation require interdisciplinary approaches and cut across the defined missions of individual government agencies. The complexity of these challenges should be matched by the use of sophisticated approaches that draw on expertise from a range of disciplines in order to enhance knowledge and to apply that knowledge to the development of scientifically sound management strategies. The major themes and “overarching opportunities” identified in the draft plan are interdisciplinary in nature and require expertise from many fields for substantial progress to be made. The draft plan rightly acknowledges the importance of interdisciplinary science, as well as approaches that cut across agency missions. The six research themes lend themselves to interdisciplinary efforts and collaboration, both within and across agencies. The descriptions of major research areas in the plan also indicate the need for integration, systems approaches, and collaboration in the conduct of research and the gathering of data. However, the identification of important linkages among the six major themes and multidisciplinary needs within research priorities was uneven, and in some instances quite weak. Pointing out these linkages throughout the report serves many important purposes, including more accurate identification of both the range of specific expertise needed to address the major themes and the scope of interdisciplinary and multi-agency efforts that will be required. The focus on interdisciplinary science starts off in the right direction through much of the draft plan. The real test of a commitment to move beyond historical disciplinary and organizational barriers will be determined by whether the Implementation Strategy presents a pathway to break down barriers between disciplines and agencies to facilitate multidisciplinary, multi-mission programs.

The emphasis on multidisciplinary, multi-mission efforts and collaboration above should not be interpreted as a recommendation to abandon or downplay the importance of disciplinary training. Expertise and training in specific disciplines will be required to develop effective multidisciplinary efforts. Also, advances in traditional disciplines contribute to the solution of important scientific challenges; hence, experts in these fields can help identify new concepts or technologies that will support multidisciplinary programs.

RECOMMENDATIONS:

- **The ORPP should provide a more comprehensive description of the needs and opportunities for multidisciplinary research, as well as research partnerships (multi-agency and agency/academic/industry/international), for each societal theme.**
- **The Implementation Strategy for the ORPP should evaluate the adequacy of existing mechanisms for interagency and agency-academia collaborative research to identify opportunities to improve collaboration among sectors.**

- **Disciplinary expertise and research should not be neglected in the description of important research and training needs presented in this document.**

EVALUATION OF NEAR-TERM PRIORITIES

Near-term priorities provide a bridge between the generalities of the themes, the focus of the 21 priorities, and the launching of more specific actions. It is necessary to focus the early research efforts where they will have the most impact and where they can be readily implemented. Given these constraints, the near-term priority areas selected appear to be appropriate and will advance the ocean research agenda, although they are not as clearly tied to the 21 long-term priorities as they might have been.

The basis for the selection of the near-term priorities is not clear. While criteria for selection have been identified, these criteria are not referenced in the discussion of the individual near-term priorities. Also, there are no milestones or metrics offered for tracking progress towards meeting the objectives behind these priorities. A few examples under each near-term priority would be valuable in illustrating potential outcomes and would provide more inspiration for the priorities. Also, it is not clear how the time frame for the near-term priorities meshes with the implementation of the 21 priorities. The ORPP does not explain how the implementation of the near-term priorities will affect the initiation of research efforts identified under the individual themes.

RECOMMENDATIONS:

- **The specific linkages of each of the near-term priorities to the 21 longer-term priorities should be identified. Milestones and metrics should be identified to measure accomplishment and progress.**
- **The near term priorities should include a description of the relationship between what is already being done in each area and how it will be expanded under the research plan.**
- **The time frames for conduct of near-term and longer-term research should be clearly defined with a discussion of the balance of effort between the two sets of priorities.**

RESEARCH AND HUMAN RESOURCE NEEDS

Physical Infrastructure

The draft plan successfully *lists* most of the types of physical infrastructure that will be required: vessels, earth-sensing satellites, buoys, unmanned vehicles, new sensors (particularly aimed at biological and chemical ocean variables), and more. It also *mentions*, correctly, various needs for more widespread deployments of many of these tools, and integrations of their measurements. However, the draft does not go beyond the stages of listing and mentioning. It does not provide goals and objectives that are connected to the stated priorities nor does it give specifics for implementation. Consequently, it is not possible to answer the question of whether the plan “adequately” considers these tools.

Information Infrastructure

This discussion essentially parallels the discussion of physical infrastructure. Plans exist that address needed information infrastructure for ocean sciences, but these plans were not written within the context of the ocean research priorities. Although discussions of the information infrastructure, in these

and other reports may be informative, they do not address “adequacy” for the purpose of the ORPP. Therefore, the existing plans only serve as guidance in defining information infrastructure not as the basis for determining the adequacy of the information infrastructure called for in the draft plan.

Intellectual Capital

The draft plan contains mention, but scant quantitative assessment, of the need for intellectual capital or the future workforce required to carry out the research and related work envisioned in the plan. Short shrift also is given to the social science workforce needs that are integral to the plan. The needs run from Ph.D. researchers to electronics technicians to science managers to experts in formal and informal education, outreach, social science, and economics.

RECOMMENDATIONS:

- **The final plan should move towards greater specificity in the area of infrastructure in order to command serious attention from its intended audiences.**
- **The ORPP should take heed of broader workforce issues and information sources in revising, clarifying, and improving its projections of needs in this area.**

1

Introduction and Background

In the Oceans Act of 2000, Congress called for the establishment of a presidential commission to undertake a comprehensive review of U.S. ocean policy. The U.S. Commission on Ocean Policy (USCOP) was established in September, 2001. The Commission's report, *An Ocean Blueprint for the 21st Century*, was released on September 20, 2004, and included over 200 recommendations directed at the President, the Congress, and executive branch agencies.

As required under the Oceans Act, the President released his official response to the Commission's report on December 17, 2004, titled the *U.S. Ocean Action Plan* (OAP). The OAP describes a number of current initiatives and planned actions that are consistent with the Commission's recommendations. In addition, the President issued an executive order that created an ocean governance structure led by a new cabinet-level Committee on Ocean Policy to coordinate ocean-related activities of the federal government. Within that committee's oversight is the National Science and Technology Council's Joint Subcommittee on Ocean Science and Technology (JSOST).

One of the first assignments of the JSOST was to develop an Ocean Research Priorities Plan (hereafter referred to as the plan or the ORPP) and Implementation Strategy by December 31, 2006. As described in the OAP, the ORPP "will seek enhanced collaboration, coordination, cooperation, and synergies, and will identify gaps and deficiencies along with related infrastructure needs ... [and] will be prepared in an open and transparent manner with advice from the ocean research community (government, academic, industry, and other non-government entities)" (Bush Administration, 2004). On April 5, 2005, the JSOST issued the Ocean Priorities Framework (OPF), which was intended to guide the development of the draft ORPP. On September 12, 2006, the JSOST announced the release of the draft ORPP entitled *Charting the Course for Ocean Science in the United States: Research Priorities for the Next Decade* in the Federal Register (FR Vol 71, No. 176, pp. 53685-53686). Details of the development of the ORPP are provided in Chapter 2 of this review. The draft plan is a multi-agency collaborative effort shaped by input from academia, industry, and non-governmental organizations via a public workshop and public comments. The purpose of the document is to:

"....develop and present ocean research priorities that address key interactions between society and the ocean. If acted upon, these priorities will result in considerable strides toward enhancing the quality of life and safeguarding the health of the open ocean, coasts, coastal watersheds, and Great Lakes." (JSOST, 2006)

The draft research plan identifies six themes that represent key areas of human interaction with the ocean. They are:

- Stewardship of Our Natural and Cultural Ocean Resources
- Increasing Resilience to Natural Hazards
- Enabling Marine Operations
- The Ocean's Role in Climate
- Improving Ecosystem Health
- Enhancing Human Health

Within each of the themes, research priorities are defined using a common set of questions as guides to help identify the most compelling research priorities. The draft plan further identifies opportunities for progress, that is, key infrastructure and education needs that are common to many of the themes. It concludes by articulating a path forward.

ORIGIN OF THE NRC STUDY

The co-chairs of the JSOST approached the National Research Council (NRC) Division of Earth and Life Studies (DELS) in August, 2005, to assist with the research planning effort for the ORPP through a 3-phase process. In the first phase, the NRC staff provided summaries of recommendations from NRC reports published in the past 7 years that related to the themes, pillars, and cross-cuts identified in the OPF. In Phase 2, an ad hoc NRC committee was appointed to review the draft ORPP and provide recommendations for improvement. That is the purpose of this report. In Phase 3, the committee is to provide an assessment of the final ORPP within 6 months of the release of the final plan. The statement of task for this study is provided in the summary chapter (Box S-1).

THE REVIEW OF THE DRAFT OCEAN RESEARCH PRIORITIES PLAN

This document reviews the draft ORPP according to the guidelines provided by the statement of task.³ *Chapter 2* assesses the format of the public workshop and evaluates the response of the plan to both the workshop summaries and to the public comments received on the planning document (Task 8). *Chapter 3* assesses the overall plan to determine whether it is responsive to the nation's need for ocean research and development (Task 1), whether it effectively links proposed science and technology developments to benefits to the nation with regard to quality of life, safety and security, economic growth, environmental sustainability, and education (Task 2), and whether the time frame for addressing the priorities is realistic (Task 3b). *Chapter 4* evaluates the proposed research agenda within each of the six thematic areas for clarity and appropriateness of thematic research priorities (Task 3a), balance among substantive research areas as well as among research activities such as observations, modeling, and communication of results (Tasks 4b and 4c), and degree of success in linking and integrating research activities across the themes. *Chapter 5* evaluates how well the draft document articulates and identifies the need for interdisciplinary and multi-mission ocean research (Task 5). *Chapter 6* assesses whether the document effectively identifies the highest near-term priorities to address the goals and expected societal results (Task 6), and the balance between short-term and longer-term priorities (Task 4a). *Chapter 7* considers infrastructure and human resource needs, evaluating how well the plan accounts for these needs in terms of physical and information infrastructure and intellectual capital (Task 7).

³ See Box S-1 for full statement of task and corresponding task numbers.

Development of the Ocean Research Priorities Plan

The committee would like to acknowledge and congratulate the individuals involved in the development of the draft ORPP, which represents the first coordinated national research planning effort involving all federal agencies that support ocean science. The committee cannot overstate the importance and need for these types of activities that open up lines of dialogue between and across government agencies, academia, non-governmental organizations (NGOs), and industry. With respect to the current plan, the committee's comments are meant to be constructive for the refinement of the plan and also to help guide future priority-setting efforts in ocean science.

Representatives of 25 agencies that populate the JSOST conceptualized and coordinated the development of the plan. Each section within the plan was delegated to subgroups of that committee and the output of each subgroup was integrated into the draft plan. Prior to its release, the draft plan was approved by all members of the JSOST and the Interagency Committee on Ocean Science and Resource Management Integration (ICOSRMI). During the development phase of the plan, the JSOST sought input from academic, industry, government, and NGO constituencies in two ways: via a public workshop and through formal public comment. The workshop, convened in Denver, CO, was structured initially to get comments on the draft ORPP. Shortly before the workshop, however, the goal of the workshop was changed from commenting on the ORPP to actually providing suggestions for setting the priorities.

Information about the Denver workshop was widely distributed throughout the ocean science community, including postings on the JSOST website; e-mail notification to members of relevant federal committees, advisory groups, and professional societies and organizations; postings to discussion lists; and print and electronic advertising in the journal *Science*, the *Chronicle of Higher Education*, and *EOS*. However, the workshop was convened with a relatively short lead-time; notification of the workshop began on February 26, less than two months before the workshop was to convene on April 18. This may have contributed to the relatively low attendance (231 people, not including the NRC committee members and staff), which was below the expected number of over 500.

Representation of the various sectors at the workshop was uneven and tended to be skewed toward federal employees (Figure 2-1). For this analysis, participants were placed in one of four categories based on affiliation listed on registration: U.S. government agency; academic or other non-governmental research institution; non-governmental organization; and industry. The committee acknowledges that many of the participants could fall under more than one category and has attempted to categorize participants in a consistent manner. NRC committee members and staff and meeting staff were not included in this analysis. Breakout session moderators met for an afternoon of training prior to the workshop. Despite this training, the session leaders had diverse interpretations of their charge. This resulted in considerable heterogeneity in the format of the breakout sessions and the outcomes. In some instances a moderator was also an author of the section of the document discussed in the breakout session. The dual role of author and moderator may have influenced the discussion and development of conclusions in those sessions.

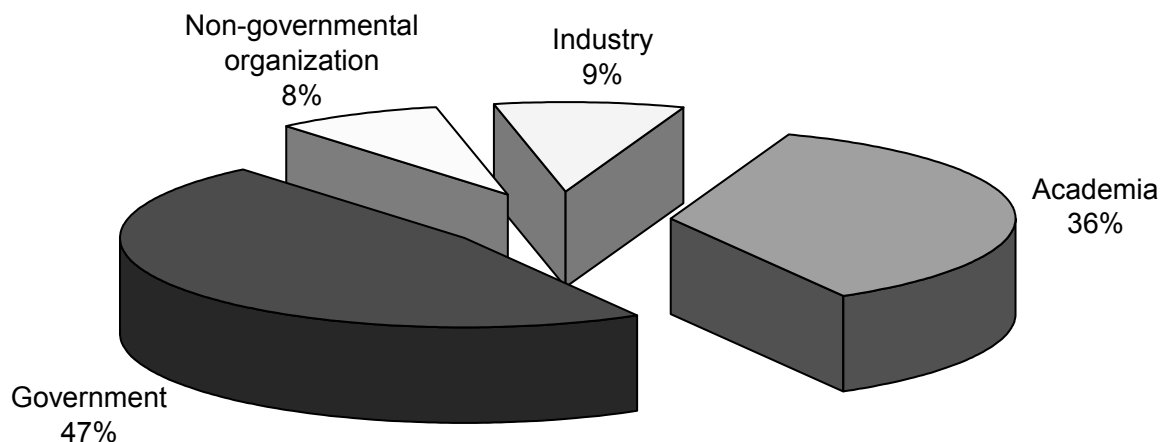


Figure 2-1. Workshop participants classified by affiliation (excluding NRC committee, committee staff, and meeting staff).

At the conclusion of the breakout sessions, the session moderators convened in groups according to theme. Each group then synthesized and condensed the results of the breakout session discussion. Several common themes and points of consensus emerged during breakout session discussions in each thematic area. There was no easy way to deal with orphan ideas in the summaries, but a special effort was made to ensure that all ideas articulated during the breakout sessions were captured and transcribed into the body of public comment. The moderators gave summary presentations in a closing plenary session that, while uneven in scope and approach, provided a valuable overview of the results of the many theme sessions. At the conclusion of the workshop there was a general expression of consensus and support among the participants for the progress made during the workshop, although there was also a sense that there was a lot of work ahead and many difficult issues to resolve in developing the draft plan.

The formal public comment period on the planning document was open from March 27 to May 15, 2006; extensive comments were submitted during this period. Comments were provided by 66 different organizations and individuals. The total length of the public comment document, made available on the JSOST website, was 183 pages. There were several common themes expressed in the public comments. One common refrain was the need to articulate the grand challenges in ocean science. The planning document was not effective in capturing either the urgency or the excitement that provides the rationale for developing a national plan for ocean science research. However, many of the public comments noted that the keynote address at the Denver workshop, given by Admiral James D. Watkins (ret.), chair of the U.S. Commission on Ocean Policy, provided a model for how to enliven the research plan. Additional common suggestions included improving connectivity and linkages between themes and disciplines, expanding the context to include international efforts, emphasizing the impacts of climate change and the role of humans in inducing climate change, and increasing focus on estuaries, coasts, and the Great Lakes as an integral part of U.S. oceans. There was a great deal of variation in the degree to which the JSOST succeeded in incorporating these comments into their revision. For example, many comments were made on the weakness of “science-to-policy” considerations in the planning document; with the addition of the section “Making a Difference” and its subsection “Information to Support Decision Making,” the draft plan made substantial improvements in this area.

In the months after the workshop and public comment period, a draft of the plan was developed from the original planning document presented in Denver. The full draft plan was released by the JSOST in August, 2006, and is the subject of this review. The August draft plan differed from the April planning document in three significant ways: (1) the organizational framework of the document was changed, (2)

one thematic area was dropped and the others were revised, and (3) the section devoted to cross-cutting themes (basic understanding of the ocean, research support through ocean observation and infrastructure, and expanded ocean education) was eliminated and these themes were resorted under different headings in the document.

ADDRESSING THE STATEMENT OF TASK

Statement of Task (8): Evaluate whether the format of the Denver workshop promoted the open exchange of ideas and suggestions for improvement.

The committee finds that while the format of the Denver workshop was designed to promote the open exchange of ideas and suggestions for improvement and did succeed to some extent in this regard, changes in the goals of the workshop shortly before it began; relatively low attendance at the workshop, particularly from non-governmental groups, including industry; and heterogeneity in the formats of the break-out sessions diminished the level of inclusiveness and strategic planning evidenced at the workshop. Following the Denver workshop there has been a significant effort by the JSOST leadership to reach out to the ocean science community through meetings and formal public comment. The committee commends the JSOST leadership for this series of public outreach meetings.

3

Assessment of the Overall Plan

INTRODUCTION

The draft plan succeeds in a number of important ways. First, the important link between ocean and society is clear and well articulated. Second, the committee agrees with the identification and the importance of the overarching opportunities (developing the understanding and capability to forecast ocean processes; collecting the scientific information needed to support ecosystem-based management of resources, especially those found in coastal and near shore ecosystems; and accelerating deployment of an ocean observing system that will, in turn, advance forecasting and management capabilities). Third, the draft plan is organized around six broad themes and these themes succeed in capturing the main ocean-related issues facing society in a comprehensive and coherent way. Fourth, the plan includes research priorities in the social sciences, a necessary component for improving ocean stewardship. Finally, the draft plan acknowledges the role of fundamental science in meeting the nation's needs for ocean research and development.

The plan does less well at translating the link between society and the ocean into a bold and compelling vision for ocean research in the next decade. The U.S. Commission on Ocean Policy (USCOP) final report, *An Ocean Blueprint for the 21st Century*, provides a useful example of such a vision starting on page 4 (“A Vision and Strategy for the 21st Century and Beyond,” USCOP, 2004). There is a need to draw a clearer connection between the problems and opportunities facing society and the specific challenges for ocean science, and between these specific challenges and the research priorities identified in the plan. These connections were clearly elucidated in several recent national reports, including those by the USCOP, the Pew Oceans Commission’s *America’s Living Oceans: Charting a Course for Sea Change* and the Joint Ocean Commission Initiative’s *From Sea to Shining Sea*. The JSOST draft plan mentions these reports only minimally and thus misses an opportunity to put the plan into a larger context. A stronger connection to earlier reports would underscore the coherent evolution of thinking about the critical role that ocean research plays in addressing the pressing problems and opportunities facing society.

ORGANIZATION OF THE DRAFT PLAN

A further concern of the committee is the current organization of the plan, which differs from the previous outlines in the Framework and Planning Documents. In particular, the committee is concerned with changes in the discussion of some of the original cross-cutting issues, especially fundamental science and education.

The draft plan does attempt to provide an overall vision and a sense of a grand challenge on pages 57-58 under the section “The Path Forward.” The sub-section on “Overarching Opportunities” in particular provides a vision of the future that is multi-faceted and from which numerous challenges and

related goals could be developed. This section could be strengthened significantly and moved to the front of the plan with other cross-cutting elements.

The draft plan draws a somewhat sharp distinction between basic and applied science. In doing so, the plan does not adequately reflect the nature of the scientific process in which basic process studies continually interact with more applied observational and modeling efforts, both to advance science and ultimately to inform and support policy decision making and societal needs. Improvements in models and observing systems – in terms of predictive ability and cost – will depend on continuing advances in basic knowledge. A consequence of separating fundamental from more applied science is the potential to undervalue the role of basic science in supporting all types of scientific research and application. Ocean research contributes insights that advance other fields of science in addition to addressing the science-based ocean policy and management needs. As one example, basic research in acoustic propagation, remote sensing, turbulence, environmental prediction, and novel vehicles has led to operationally important military systems. Similarly, experiments with marine communities and organisms provide insight into the underlying processes controlling the organization and functioning of ecological communities more generally. This point reflects the general connectedness of science in many ways across subject boundaries that at first glance may appear quite impenetrable. It is critical to recognize that basic and applied science represent a continuum, not discrete entities. The solid arrow in Figure 3-1 represents how societal goals can drive an applied research agenda, which is highlighted by the ORPP. The dashed arrows represent the nature of the scientific enterprise and the resulting linkages between societal needs, and applied and basic science. The presence of these feedbacks illustrates the dependence of one type of scientific endeavor on the other.

The committee suggests that the importance of wide-ranging fundamental research, while acknowledged in the plan, could be bolstered. The committee agrees with the JSOST view, that fundamental research is the crucial foundation for progress in *all* of the themes and should continue to “be driven by competitive, merit-based investigations” and not constrained to the subjects of the themes. This will allow for the continued infusion of curiosity-driven inquiry and novel findings, thereby supporting “unexpected breakthroughs” across many fields. As it stands, the plan ineffectively addresses the role of basic research in achieving the priorities identified in the plan. The discussion of fundamental science could be more effectively highlighted as a cross-cutting element, connected to the subsequent discussion of the themes by statements that provide examples of ways in which fundamental research could lead to progress in those theme priority areas. An associated modification of the thematic discussions would then serve to close this connection and emphasize the core importance of breakthroughs in fundamental science to advancing research priorities in the themes.

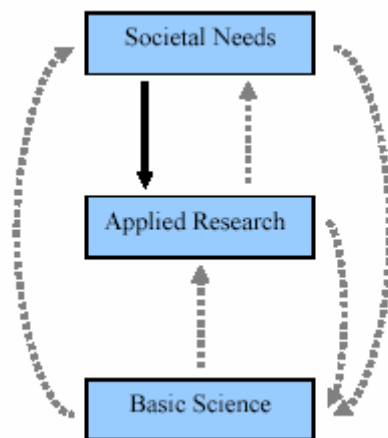


Figure 3-1. The interconnected nature of science and societal needs.

Clearly, two issues central to assessing the feasibility of the plan and its responsiveness to the nation's needs are the level and allocation of funding and the strategy for implementation. The draft plan is silent on these issues, making it difficult to assess either feasibility or responsiveness. Because many priorities rely on large infrastructure investments or will require establishment of new research programs, there is a need to spell out the general funding assumptions under which the plan was developed (as was done, for example by both the Pew Oceans Commission [2003] and the USCOP [2004] in their discussions of future ocean science support). The ORPP section titled “The Next Steps” provides an outline of topics to be addressed in the Implementation Strategy. The first bullet, “Roles and responsibilities of each constituent sector (e.g. federal agencies, state agencies, private sector, academia and non-governmental organizations) in planning, programming, budgeting and execution of the priorities” (p. 64, lines 8-10 of the ORPP), identifies issues that will be critical to ensure success of the plan. The coordination of effort among the agencies provides much of the justification for this research planning effort and cannot be overemphasized in the Implementation Strategy.

In addition, the committee has a specific concern that the operation of an integrated ocean observing system will degrade the nation’s research capabilities unless mechanisms are developed to transfer fiscal responsibility from research programs to operational entities. The plan correctly recognizes the importance of developing technologies into operational capabilities (p. 53, line 24), but one of the major stumbling blocks in this step is not mentioned – the need for a sustainable source of support for operations such as ocean observations. Enhancing ocean observations is clearly required to address the research priorities identified in the draft plan. If the observing system is explicitly designed, deployed, and operated to benefit both scientific and operational activities, it will be necessary to develop a cost-sharing approach for the operational and research programs. The need to plan for the transfer of research-oriented observation capabilities to operational observing programs has become a major concern, as indicated by the focus on this issue in several recent NRC reports (e.g., NRC, 2003a, 2003b).

Education is another element that does not receive sufficient emphasis, primarily because of the ineffective placement of the discussion towards the end of the plan. Education is integral to the entire ocean enterprise and its value and many contributions to the research enterprise should be emphasized as an essential, cross-cutting element of the ocean research plan, as illustrated in Figure 3-2.

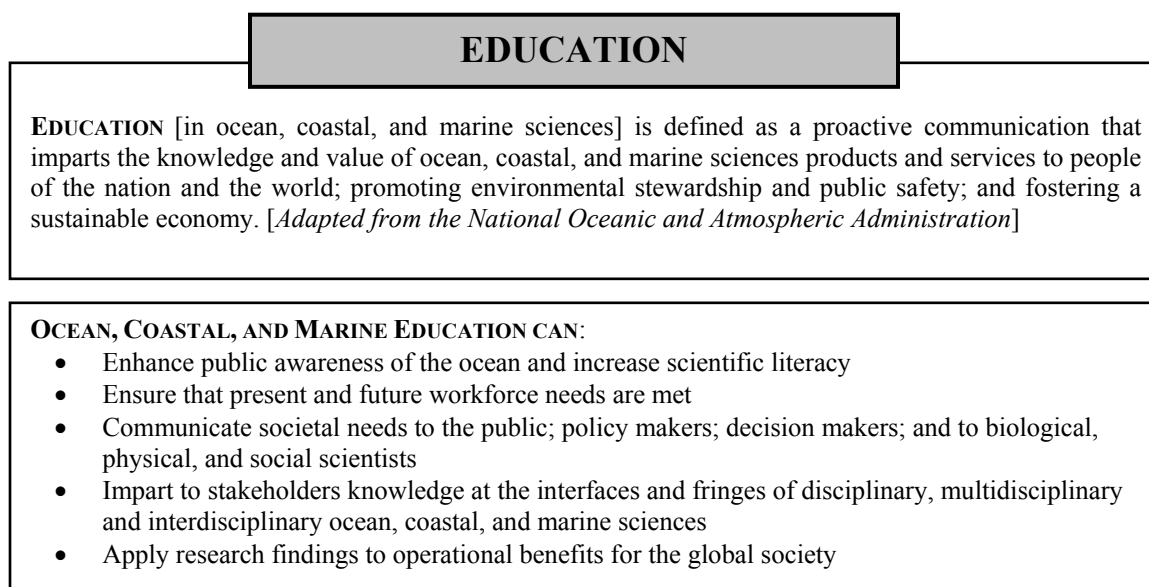


Figure 3-2. The contribution of education to the ORPP.

The committee notes that there are a number of times in this review where it is suggested that the draft ORPP does not provide sufficient detail to adequately describe or develop the arguments for a particular theme, priority, etc. The committee realizes that the JSOST needs to keep the ORPP as short and succinct as possible. However, even though the additional detail could make the plan a little longer, this added information will make the plan much more credible, useful, and scientifically acceptable. Some of the background discussion in the plan could be trimmed or concern about a longer plan could be allayed by a particularly succinct and readable Executive Summary.

THEMES

The draft plan is organized around six societal themes. The themes successfully capture the main ocean-related issues facing society, have an interdisciplinary focus, and would benefit from a collaborative approach among federal, state, and local agencies to integrate efforts. However, the draft plan is less successful at elucidating the linkages between and among these themes. These connections identify opportunities for collaborative research efforts in addition to the overarching opportunities. The process of connecting could also illuminate potential areas where the discussion of the science in the themes is inconsistent. For example, the text in the “Stewardship of Our Natural and Cultural Ocean Resources” theme discusses the need to expand data collection efforts to determine the “worth” of natural resources using current valuation methodologies, and the “Improving Ecosystem Health” section makes the point that new approaches are needed to evaluate the consumptive and non-consumptive use value of natural resources. More details on the linkages among themes will be provided in Chapter 4.

PRIORITIES

Within each theme, the draft plan identifies a small number of priorities intended to guide effort toward the most important scientific objectives that must be reached to achieve the larger societal goals. There is no ranking of the priorities either within or across themes. Because the research priority statements are phrased as broad, sometimes all encompassing, objectives rather than specific research needs, they only partially meet the intention to guide future scientific efforts. The terms “Understand” and “Apply” are more typically used for describing societal goals and are ineffective in conveying the scientific questions and technological innovations required to advance knowledge. Similarly problematic is the use of the word “priority.” By definition, “priority” implies precedence in importance, timing, or some other characteristic. However, because of their lack of specificity, the priorities do not clearly indicate a sense of value, urgency, or ranking. In other words, many of these priorities are too general to provide useful guidance. Three examples suffice to illustrate this point. A priority under the theme “Stewardship of Our Natural and Cultural Ocean Resources” is to “understand interspecies and habitat/species relationships as a basis for forecasting resource stability and sustainability.” The activities enumerated under this theme include “data collection, experimentation, and advanced modeling” with an additional four priorities listed for natural-resource modeling. It is fair to say that this priority essentially covers the entire field of marine ecology. A priority under the theme “The Ocean’s Role in Climate” is to “understand ocean-climate interactions across regions.” The specific issues raised under this priority include “the influence of the global tropical ocean phenomena (as demonstrated by El Niño, [the Pacific Decadal Oscillation] PDO, and [North Atlantic Oscillation] NAO events and monsoons in the Indian and eastern Pacific ocean)” and ascertaining “the role of the deep ocean ... particularly with regards to mitigating climate change (e.g., via carbon sequestration and heat storage).” Again, these are monumental scientific challenges covering a large part of physical and chemical oceanography. A priority under the theme “Enhancing Human Health” is to “understand human health risks associated with the ocean and the potential benefits of ocean resources to human health.” In large part, this is simply a restatement of the theme. By encompassing virtually the entire scope of research under a theme, many of

the priority statements do not suggest prioritization. The near-term priorities provide an example of true priority setting because they clearly give precedence with respect to implementation.

Although it may seem paradoxical, at the same time that many of the research priority statements are too broad to serve as a guide, some of the supporting text is overly prescriptive with regard to the type of scientific activity specified to address the priority. For example, supporting text under the priority statement “Understand human health risks associated with the ocean and the potential benefits of ocean resources to human health,” listed in the theme “Enhancing Human Health,” calls for the integration of ocean data and modeling “with epidemiologic studies to define exposures and refine risk assessments.” Although this integration may indeed be valuable, epidemiologic studies may not be the optimal tool in every circumstance. In one case, the wording of the priority statement under the theme Improving Ecosystem Health, “understand and predict the impact of natural and anthropogenic processes that govern the overall level of ecosystem productivity,” is too limited. Singling out productivity as a measure of ecosystem status is overly restrictive and captures neither the complexity nor the variety of ecosystems and the services they provide. The optimal mix of process, observational, and modeling studies will depend on the specifics of the research project and hence will be more effectively determined by scientists endeavoring to address these research priorities. This statement could be rephrased as: “Develop the capability to predict the impact of natural and anthropogenic processes on ecological systems.” A number of system characteristics, including organization, composition, complexity, and productivity, are important to consider both as measures of impact and as factors that ultimately influence the direction and magnitude of ecological responses. Particular attention should be paid to non-linear dynamics, threshold responses, and properties that influence the ability of systems to resist or recover from natural and anthropogenic stressors.

The ORPP identifies specific criteria by which research priorities were selected (presumably from a longer list of candidate priorities). However, the connection between these criteria and the specific priorities that were selected is not spelled out (see chapter 4 for additional discussion). In addition, the plan does not provide a schedule for addressing the priorities and lacks benchmarks for measuring progress.

The committee believes that some of the problems identified above could be alleviated by expanding the theme-priority hierarchy through the introduction of an intermediate level that identifies “challenges for science and society.” As it stands, the themes refer to societal issues, while the priorities are phrased as broad research goals for addressing these issues. Between these two levels, large-scale scientific challenges could be articulated to assist in the formulation of more tightly focused research priorities. Some examples are given in Box 3-1. These challenges would galvanize the ocean research and policy communities and more convincingly convey the need for an ocean research plan to address societal concerns. The examples in Box 3-1 are provided only to illustrate how challenges could be incorporated under the themes. Identification of “challenges” is an integral step in the development of the research plan and as such should be undertaken by the JSOST.

Box 3-1

Examples of Challenges for Science and Society

Theme: The Ocean's Role in Climate

Challenge for Science and Society: By 2020, determine environmentally safe limits and conditions for disposal of excess CO₂ in the ocean and develop economically viable disposal methods that meet those conditions.

Challenge for Science and Society: By 2020, assess the risk and prospects of abrupt climate change.

Theme: Stewardship of Natural and Cultural Resources

Challenge for Science and Society: Within the next decade, develop ecosystem-based models that accurately predict future abundances of all major harvested fish populations within a given ecosystem.

Challenge for Science and Society: Assess the potential for ocean energy and its likely ecological impacts by 2020.

TIME FRAME

The committee struggled with Task 3b (Is the time frame for attaining the priorities realistic?). Initially, the committee attempted to address this task for each of the priorities that were stated in the draft ORPP. Because of the linkages among these priorities, as discussed in Chapter 4, progress toward certain priorities will be affected by progress toward one or more of the others. It also became clear that because the 21 longer-term priorities were so broadly stated, the committee could not answer Task 3b without information about how the research priorities would be implemented. As noted in Chapter 4, the discussion of the priorities does not convey the degree of difficulty or challenge in achieving them, nor an assessment of where the ocean research community stands today, making it difficult to address realistically the likely time frame for success. There are no explicit, or even implicit, milestones.

PRESENTATION

The organization of the draft plan undercuts many of the valuable points that are made in the text. The current draft could be revised in accordance with the JSOST's *Ocean Priorities Framework* (April, 2005) which would address many of the shortcomings identified in this review. The Framework document lists the following sections as part of the draft plan: Vision, Challenges, Principles and Critical Elements, Themes, Goals, Resources, and Evaluating Performance. Alternatively, the use of cross-cutting themes, similar to the approach in the April, 2006, Planning Document but placed early in the ORPP, would give these issues greater emphasis and illustrate many of the connections among the themes. A possible new outline for the plan is provided in Box 3.2. Strong editorial attention is needed throughout the document to help to make it more exciting, dynamic, organized, and readable. The use of boxes, figures, tables, pictures, and other tools would help to highlight information by consolidating it and presenting it in a format that has a higher reader impact.

Box 3-2

Potential Outline

- 1) Vision
- 2) Approach
- 3) Crosscuts
 - a) Fundamental Science/Basic Research
 - b) Development of predictive models
 - c) Integrated Assessments
 - d) Ocean Observations
 - e) Information to support decision making
 - f) Ocean education – ocean literacy and workforce development
- 4) Themes #1-6
 - a) Challenges
 - b) Research Goals and Priorities
 - i) Timeframe
 - ii) Benchmarks
 - c) Tools
- 5) Near term priorities

ADDRESSING THE STATEMENTS OF TASK

Statement of Task (1): Is the plan responsive to the nation's needs for ocean research and development?

The committee finds that, at a broad level, the draft plan is generally responsive to the nation's needs for ocean research and development in addressing present problems and opportunities facing society. The draft plan is less responsive to the need for ocean research that, while not directly connected to today's challenges, will provide the basis for addressing challenges of the future. The draft plan also lacks a bold and compelling vision for ocean research and development over the next decade.

Statement of Task (2): Does the plan effectively link proposed S&T developments to benefits of the nation with regard to quality of life, safety and security, economic growth, environmental sustainability, and education?

The committee finds that, as a result of the use of vague statements to characterize research priorities, the plan does not effectively link proposed scientific and technical developments to benefits to the nation with regard to quality of life, safety and security, environmental sustainability, and education.

Statement of Task (3b): Is the time frame for attaining the priorities realistic?

Because the 21 longer-term priorities were so broadly stated, the committee finds that it cannot determine how realistic this time frame is without additional information specifying how the priorities will be implemented.

RECOMMENDATIONS

To address these shortcomings, the committee has the following general recommendations:

- **The Ocean Research Priorities Plan should provide a bold and compelling vision for the future of ocean science research. This vision should be placed near the front of the plan and referenced in the discussions of the theme priorities. This would help to integrate the discrete sections of the plan.**
- **The plan should be reorganized to include a discrete section devoted to cross-cutting elements that are central to the vision for ocean research. The concept of cross-cutting themes used in the Planning Document should be reintroduced and moved toward the beginning of the plan as a way to reinforce the importance of these elements in creating the foundation for progress on the societal themes. In particular, the section *Expanding the Scientific Frontier: The Need for Fundamental Science* should be included as one of these cross-cuts and revised to strengthen the rationale for basic research. Examples of the role of basic research in advancing the goals of the societal themes should be used to illustrate why this is a cross-cutting issue.**
- **The plan should identify challenges under each theme that provide a more inspiring rationale for the research priorities.**
- **Carefully formulated and well-justified research priorities should be identified that are clearly connected to scientific challenges. The Implementation Strategy should include a schedule by which these priorities could reasonably be addressed and a set of benchmarks by which progress toward this goal could be assessed.**

4

Evaluating Thematic Priorities and Cross-Theme Integration

INTRODUCTION

This chapter evaluates the proposed research agenda within each of the six societal themes. The evaluation criteria that will be used below are related to three of the tasks given to the committee. They include:

- The clarity and appropriateness of the thematic research priorities (Task 3a);
- The balance among substantive research areas as well as research activities such as observations, modeling, and communication of results (Task 4b and 4c); and
- The degree of success in linking and integrating research activities across the themes.

The draft plan identifies 21 research priorities (Table 4-1). The plan states that eight questions (Box 4-1) were used by the JSOST to guide the development of these priorities. From the narrative it is difficult to determine how the questions were applied to the six themes to produce the 21 priorities. For example, it is not clear if the questions were weighted equally, or if some were given more consideration than others. There is also no information regarding the number of potential priorities that were screened by these questions prior to arriving at the final 21 priorities. In that sense, it is difficult to assess whether specific areas were unintentionally or intentionally omitted as part of the priority-setting exercise. Further elaboration on the use of the questions in the development of the priorities would have been helpful in assessing them.

Table 4-1. Priorities identified by the JSOST*

1	Understand the status and trends of resource abundance and distribution through more accurate, timely and synoptic assessments
2	Understand interspecies and habitat/species relationships as a basis for forecasting resource stability and sustainability
3	Understand human-use patterns that may influence resource stability and sustainability
4	Apply advanced technologies to enhance the benefits of various natural resources from the open ocean, coasts, and Great Lakes
5	Understand the initiation and evolution of hazard events and apply that understanding to improve forecasts of future hazard events
6	Understand the response of coastal and marine systems to natural hazards and apply that understanding to assessments of future vulnerability to natural hazards
7	Apply understanding to develop multi-hazard risk assessments and to support development of model, policies, and strategies for hazard mitigation
8	Understand the interactions between marine operations and the environment
9	Apply understanding of environmental factors to characterize and predict conditions in the maritime domain
10	Apply understanding of human behavior to develop the information and tools necessary to carry out effective, safe, and secure marine operations
11	Apply understanding of marine operations to enhance the marine transportation system
12	Understand ocean-climate interactions across regions
13	Understand the impact of climate variability and change on the ocean, including its biogeochemistry and ecosystems
14	Apply understanding of the ocean to help project future climate changes and their impacts
15	Understand and predict the impact of natural and anthropogenic processes that govern the overall level of ecosystem productivity
16	Apply understanding of ocean-related socioeconomic activities to assess the ability of marine ecosystems to provide essential goods and services
17	Apply understanding of marine ecosystems to develop appropriate indicators and metrics for their sustainable and effective management
18	Understand, forecast, and reduce ocean-related risks to human health from pathogens, biotoxins, and chemical contaminants
19	Understand human health risks associated with the ocean and the potential benefits of ocean resources to human health
20	Understand how human use and valuation of ocean resources can be affected by ocean-borne human health threats and how human activities can influence these threats
21	Apply understanding of ocean ecosystems and biodiversity to develop products and biological models to enhance human well being

*The priorities are numbered here in the order that they are presented in the ORPP *only* to simplify referencing in the text. The numbers do not imply ranking by either the NRC committee or the JSOST.

Box 4-1

Criteria used to identify research priorities

The following questions were used to identify the most compelling research priorities for each theme, recognizing that the prioritization criteria for one theme may not be equally applicable to another:

- Is the proposed research transformational? (e.g., will the proposed research enable significant advances for insight and application, even with potentially high risk for its success; would success provide dramatic benefits for the nation?)
- Does the proposed research impact many societal theme areas?
- Does the research address high priority needs of resource managers?
- Would the research provide understanding of high value to the broader scientific community?
- Will the research promote partnerships to expand the nation's capabilities (e.g., contributions from other partners, including communities outside of ocean science, such as health science; unique timing of activities)?
- Does the research serve to contribute to or enhance the leadership of the United States in ocean science?
- Does the research contribute to a greater understanding of ocean issues at a global scale?
- Does the research address mandates of governing entities (federal agencies, state, tribal and local governments)?

There are three primary concerns about the research priorities as they have been identified in the draft plan:

- The priorities that start with “Understand” sound like goals (not research priorities). The priorities that start with “Apply” sound more like activities. This terminology fails to communicate that there are priority research activities from which milestones could be developed. In addition, many of the priority statements are so all encompassing that they do not indicate areas of research that should take precedence.
- The wording of some research priorities suggests that they do not involve research but rather implies that they are activities or operations that have no clear research component (e.g., Priorities 11 and 16).
- They do not convey the degree of difficulty or challenge in achieving them, making it difficult to realistically address the feasibility and time frame for their likely success (Task 3b).

In addition, the balance among substantive research areas, as well as research activities such as observations, modeling, and communication of results (Tasks 4b and 4c), is difficult to assess for some of the thematic priorities in the ORPP. In part, that is because the balance among activities will actually depend upon the implementation of the priorities. But, it is also due to the difficulty in discerning from the plan what research *will not be done* as a result of the choice of research priorities. In places where it is possible to comment sensibly on issues of balance, the committee does so.

The themes and their corresponding priorities would benefit from a stronger emphasis on several cross-cuts that underlie them.

- *Integrated Assessments.* The development of integrated assessment depends on information both from a comprehensive observing system and from models. In the context of physical climate variability, this is called state estimation. Such analyses are extremely useful in their own right, and they are essential to provide initial conditions for forecasts. Such baselines are essential for gauging ecosystem health trends as well. However, integrated assessments are only the first step in advancing the science and modeling capabilities. Within each theme, it would be very useful to articulate what the impediments are to generating high quality integrated assessments.

- *Basic Research.* The important role of basic research is not clearly addressed. Many of the research priorities identify a need for “understanding,” and it would be helpful, potentially inspiring, if some needed process studies were identified in the plan that could be conducted during the next decade. These inclusions could illustrate linkages among the priorities and the cross-cutting role of broad-ranging fundamental research as suggested in Chapter 3.
- *Predictive Models.* The complexity and difficulty of developing skillful integrated predictive models for use by managers and policy makers is not communicated in the descriptions provided in the “Necessary Tools” subsections within each theme, and by relegating the “Opportunities for Progress” to the back of the document. Within the themes, there are very different scientific challenges and levels of difficulty in approaching this cross-cutting objective. For example, ocean circulation and climate models have been used for operational prediction for some time, albeit with varying degrees of success, while the development of ecosystem models with sufficient predictive capacity to support ecosystem-based management are much less advanced. There are a number of models that are designed to contribute to ecosystem-based management, ranging from food web models to spatially explicit models of marine reserve design. However, there is much room for improvement of models and modeling approaches, and many models have yet to be rigorously tested against field data and experiments. In each theme, it would be very useful to articulate what the next major step is towards building a model with sufficient predictive skill to be of value to decision makers and researchers.

In addition to the cross-cuts, there are clear linkages between thematic areas (Figure 4-1). Most of the thematic discussions address at least some issues that are linked to other themes. It would be helpful to enhance this discussion by taking a more consistent approach across the themes that: 1) carefully considers the interfaces between themes that intersect the key problems identified in the document and 2) states the research needed and the mechanisms to be employed to foster collaborative research in those areas. A graphic that helps the reader to visualize the connections between thematic areas would also be useful.

RECOMMENDATION

Linkages between the themes should be clearly and consistently delineated in the supporting text for the research priorities. This could be accomplished through a simple statement that a given research priority will also forward the goals of other (specified) themes. This recommendation is echoed in the following discussion of and recommendations for specific thematic areas.

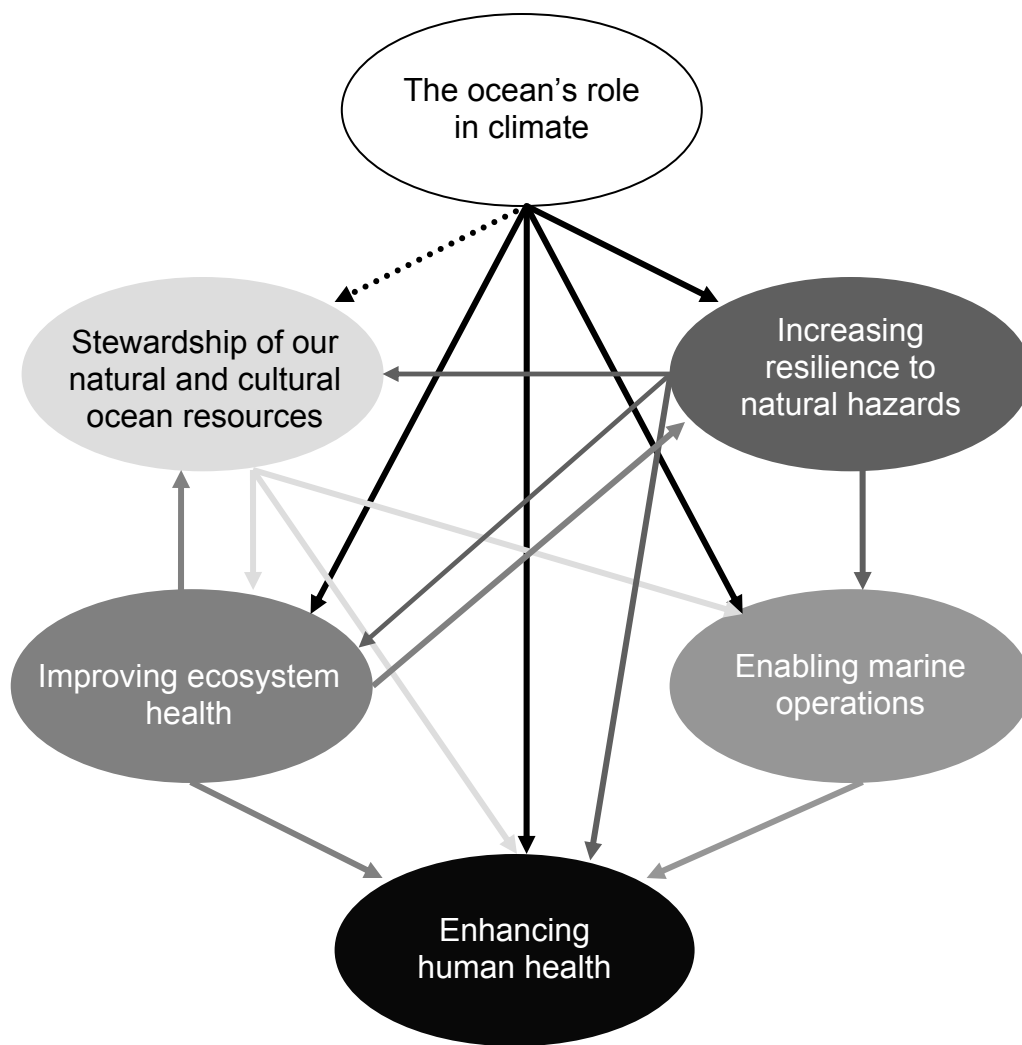


Figure 4-1. Visualizing the linkages among the themes. The figure illustrates how one theme may influence another (indicated by the direction of the arrows). Some themes are principally drivers – they describe processes that affect other societal themes; other themes are primarily receivers – they are affected by but have few impacts on other themes. For example, climate change is the fundamental driver in the system (arrows point outward). However, human health does not have a direct impact on the other themes but is instead affected by most of the themes (i.e., almost all arrows point toward it).

STEWARDSHIP OF OUR NATURAL AND CULTURAL OCEAN RESOURCES

The research priorities under this theme are:

- 1) *Understand the status and trends of resource abundance and distribution through more accurate, timely, and synoptic assessments.*
- 2) *Understand interspecies and habitat/species relationships as a basis for forecasting resource stability and sustainability.*
- 3) *Understand human-use patterns that may influence resource stability and sustainability.*
- 4) *Apply advanced technologies to enhance the benefits of various natural resources from the open ocean, coasts and Great Lakes.*

This theme encompasses stewardship of all aspects of the oceans and Great Lakes that represent commodities that fulfill essential human needs (e.g., goods and services like food and transportation) or values (e.g., recreation, tourism, preservation of culture). While this makes sense from the viewpoint of categorizing human dependence on the ocean, it suffers as a logical organizing principal for research priorities. Too many disparate types of resources that have little to do with one another in terms of the science needed to advance knowledge are grouped under this one heading. For example, the principles of assessing and managing living renewable resources are completely different from those involving mining of non-renewable, non-living mineral resources of finite quantity, and different from protecting cultural artifacts. In an effort to fold all natural and cultural resources under one tent, the four research priorities embedded in this theme – inventory, model, incorporate human dimensions, and apply technology – are very diffuse.

Although the plan discusses the need for taking an ecosystem-based approach, we suggest that the plan go one step further and use ecosystem-based management as the unifying concept to connect the numerous human uses of the ocean and research priorities. The interactive and cumulative effects of the many different sectors of our economy (energy, food, tourism, transportation) that draw resources from the ocean must be viewed in a holistic manner using an ecosystem-based approach. Underscoring the over-riding necessity to think in terms of the ecosystem when evaluating the effects of human activities could also be accomplished by drawing stronger connections between this theme and the one on ecosystem health.

The committee also believes that the rationale fails to capture a sense of the imperiled status of our oceanic resources, the changes in human behavior necessary to achieve sustainability for renewable resources, and the difficulty of the science needed to solve these problems. This is especially evident with respect to the stated research priorities as applied to living resources. For example, the approaches that have been employed to manage living resources have often had limited success. Recent widespread coverage in the public media has highlighted these failures and the serious declines in living resources and biodiversity of the ocean. The plan misses an opportunity to address public concern about the plight of our oceans as the motivation behind the research priorities.

The committee also found it difficult to identify those resources and research priorities that the report classifies as cultural. The confusion stems from differences in how the plan discusses these resources as illustrated in the statements on line 6-7, on page 21, “the ocean preserves a record of the past in the form of drowned cultural sites” and line 29-31 on page 21 “relict prehistoric landscapes, shipwrecks, and historic and living waterfronts along the nation’s coasts and Great Lakes all contribute to the national cultural heritage.” The latter reference includes marine dependent communities (e.g., fishing communities) and the former only references archeological sites such as shipwrecks. We recommend clarifying the definition along with highlighting the research priorities that relate to these resources. As it stands, the title of this theme implies that “natural” and “cultural” resources are of equal importance in terms of research priorities, yet none of the proposed priorities deal directly with cultural resources.

Clarity and Appropriateness of the Priorities

The priorities themselves are good and need only minor adjustments. One suggested adjustment is that the priorities provide a better sense of the difficulties and challenges inherent in the natural and social science. For example, the draft plan does not mention the changing conceptual frameworks and paradigms that concern the theory of resource exploitation of living resources. On the natural science side of the ledger, problems such as loss of biodiversity, fishing down food webs, shifting baselines, metapopulation dynamics and connectivity across populations and ecosystems, genetic alterations of populations and communities, and the failure of depleted populations to rebound are not discussed in this theme. Challenges in the human and governance dimension that might be highlighted are the role for different nuances of rights based approaches (NRC 1999b), managing recreational fisheries (Coleman et al, 2004; NRC 2006a), understanding the socioeconomic trade-offs inherent in ecosystem-based management especially with respect to marine mammals, sea birds, and harvested marine species, decision making under uncertainty, and the management issues at the land-sea boundary. And, finally, as the plan mentions there are many difficult questions that reside at the interface of natural and social science that relate to the nature of the coupling and the strength of the feedbacks.

The plan correctly highlights several technological issues that must be solved in the coming decade; i.e., the need to develop ocean aquaculture in an ecologically sustainable manner, the problem of species (mammals) sensitive to acoustic emissions, the need for bycatch reduction, and the need for enhanced mapping of continental shelf waters. These are important problems that research and/or technology can address, but they represent only a small subset of challenges that are faced in managing renewable resources in a sustainable fashion.

Appropriateness of the balance among substantive research areas, and between research activities such as observations, modeling, and communicating results.

The balance among research areas and activities is appropriate. The plan proposes to improve empirical data relative to resource status, build better models, provide better socioeconomic information, and develop new technology for a variety of purposes. The committee agrees that advances on all these fronts are appropriate and necessary goals.

The priorities in this theme have an extremely strong intersection with the priorities in “Improving Ecosystem Health” (see recommendations below), as the plan discusses. The plan misses the opportunity to point out other important linkages, including the impact of climate change and natural hazards on living resources and the connection between resource exploitation and marine operations.

Recommendations

- **This theme should further emphasize the necessity of understanding all human impacts, not just fishing, and the feedbacks and cumulative impacts among them as the means of moving ocean governance to an ecosystem-based approach.** Scientific consensus has already been reached regarding the need to use ecosystem-based approaches to managing living resources. To better manage the system and all of the parts, all human impacts need to be viewed through this holistic lens. Ecosystem-based thinking is the glue that can hold the numerous pieces of this theme together. Stronger ties to the “ecosystem health” theme should also be drawn.
- **A more compelling case should be made for these extremely important research priorities based on the greater scientific and public awareness of the decline in living resources and biodiversity.** The report is missing an opportunity to capitalize on growing public concern over the plight of the oceans.

- **Research priorities for “cultural resources” should be identified or this topic should be removed from the theme.** Cultural resources are certainly valued by society and should be protected. But is there a compelling research priority that must be accomplished to do so?

INCREASING RESILIENCE TO NATURAL HAZARDS

The research priorities under this theme are:

- 5) *Understand the initiation and evolution of hazard events and apply that understanding to improve forecasts of future hazard events.*
- 6) *Understand the response of coastal and marine systems to natural hazards and apply that understanding to assessments of future vulnerability to natural hazards.*
- 7) *Apply understanding to develop multi-hazard risk assessments and to support development of models, policies, and strategies for hazard mitigation.*

This theme addresses societal risks and vulnerability to coastal and marine hazards such as hurricanes and tsunamis. Emphasis is on understanding economic, environmental, social, and public health impacts from hazards, assessing and reducing risks and vulnerability, and on making better forecasts of hazards. Research under this theme would support assessment and reduction of risk; the saving of lives and property; and the improvement of mitigation, response, and recovery operations.

Clarity and Appropriateness of the Priorities

These priorities are appropriate and address significant challenges. While the stated priorities cover many aspects of coastal and marine hazards research, the explanation of the priorities would benefit from some additional detail. For example, under Priority 6, the discussion of secondary processes could be expanded to include the study of erosion and sediment transport, such as research to better understand erosion processes and sediment transfer; source, movement, volume, quality, and disposal engineering models; regional sediment budgets; impacts of human intervention (e.g., piers, groins); and fragmented uncoordinated management. This would also identify an area of research addressing the needs of the Great Lakes, which are conspicuously absent. The need for more process research to understand the nonlinear complexity of coastal inundation forcing, coastal erosion and sediment transport, and the health of coastal ecosystems has been highlighted in other reports, notably the USCOP report, but is absent from the draft ORPP. It might also include research emphasizing the importance (as learned from the Hurricane Katrina catastrophe) of preserving and enhancing “natural defenses” such as barrier beaches and wetlands, and in many cases adopting a strategy that emphasizes “nature first – engineered works second.” In Priority 7, the discussion might include identification of appropriate socioeconomic research topics such as policies, procedures, financial instruments, and incentives to avoid federal infrastructure investments and policies that inadvertently encourage development in vulnerable areas.

Appropriateness of the balance among substantive research areas, and between research activities such as observations, modeling, and communicating results.

There is an appropriate balance among the priority areas. Because, as indicated in the previous paragraph, the explanation of the priorities is relatively general, it is difficult to gauge the balance among research activities and applications and basic research. Research under these priorities would reflect a balance of activities.

Hazards affect activity under all of the other themes and the discussion would be improved if these linkages were reviewed in brief. Marine hazards influence ecosystems, so there is a need for a

connection to priorities within the “Improving Ecosystem Health” theme. Some marine hazards are influenced by climate variations and change (e.g., hurricanes) so this linkage is included in Priority 5. Obviously, hazards have human health and safety implications. Connections with the “Enhancing Human Health” theme also could be made more explicit. Given the broad nature of hazards research the committee also suggests that research linkages to the other themes be identified and the synergisms be briefly discussed.

Recommendation

The discussion of each of these priorities should be modestly expanded to provide greater specificity in the description of proposed research and to include research on coastal erosion and sediment transport.

ENABLING MARINE OPERATIONS

The research priorities listed under this theme are:

- 8) *Understand the interactions between marine operations and the environment.*
- 9) *Apply understanding of environmental factors to characterize and predict conditions in the maritime domain.*
- 10) *Apply understanding of human behavior to develop the information and tools necessary to carry out effective, safe, and secure marine operations.*
- 11) *Apply understanding of marine operations to enhance the marine transportation system.*

The ORPP defines marine operations to encompass commercial, recreational, and defense/security matters. The document provides good justification for further research by correctly noting that marine operations can be expected to grow in importance in the future and therefore require a strong ocean science and technology base to maintain their vitality.

The ORPP identifies the “rationale” for research in this theme as enhanced marine safety, protection of marine environments, and enabling of the operations of the marine industry. The first two are quite clear, but the last one seems to be a “catch-all” of loose ends that are never effectively addressed in subsequent discussions. For example, within the “Marine Industry” category, the ORPP states that “[t]here is a need to integrate natural resource requirements, data products, technological advances...” but this is never mentioned again in subsequent discussions. Finally, there is no substantial discussion of issues unique to Homeland Security. There is a box at the end of the section, but it seems to come more as an afterthought rather than being integrated into the section as an important topic.

Clarity and Appropriateness of the Priorities

The priority titles in this section would be improved if they were made more specific and inspiring. As is, it may take multiple readings of the supporting text for a reader to understand what the authors might have had in mind.

The discussion under Priority 8 does include some specific and intriguing research areas. It rightfully identifies the pressing issue of ballast water disposal, but it also describes research areas that are broad and open to subjective interpretation, e.g., “Areas of study include air, water, and sediment pollution...” The critical issue of the effects of sound on marine mammals, as highlighted in many reports including the USCOP, Pew Oceans Commission, and several NRC studies, is barely mentioned with only a parenthetical reference to “ocean acoustics.” Importantly, the discussion fails to explicitly call-out several important marine-related issues:

- understanding the environmental impacts of transportation in the Arctic, especially as new routes become available with reduction in sea ice, and
- quantifying the environmental impact of chronic leaks of oil and fuel in marinas and harbors (see recommendation from NRC, 2003c).

Priority 9 begins with what seems to be a call to “[enhance] environmental observation, characterization, and forecasting of ocean conditions across the global ocean.” It ends with an equally ambitious call for development of technologies to enhance data collection in all weather conditions. To address this concern the section needs much more specific suggestions (e.g., developing much better real-time operational current models that could be used for improving search and rescue, providing more efficient and safe ship navigation, and aiding in pollutant cleanup).

Priority 11 could be interpreted as intelligent implementation of various aspects of marine operations and not as research. The committee recognizes that there are legitimate research topics within this priority area, but they are not evident in the rather general discussion. Each of the topics within this priority needs to be expanded somewhat to identify the specific objective of the research to be undertaken.

Priority 10 suffers the same weaknesses already identified in the other three priorities. Because of the lack of specificity, the committee remains unclear as to exactly what would be involved and who would benefit. Perhaps, most importantly, several of the suggested topics seem to involve operational issues that are the responsibility of private industry (e.g., training of operators as opposed to development of more effective training materials).

Appropriateness of the balance among substantive research areas, and between research activities such as observations, modeling, and communicating results.

The priorities, as presented, appear to be largely focused on marine transportation. Greater balance needs to be added by discussing how the proposed research will address marine operations related to defense, fishing and aquaculture, recreation, search-and-rescue, and energy and minerals exploitation.

Some linkages to research within the other themes are identified but could be greatly expanded. For example, deepwater operations could be linked with the potential impacts on deepwater ecosystems. A tenuous link with “The Ocean’s Role in Climate” is made under Priority 8, but it needs a much more explicit linkage. Mention is made of “environmentally sensitive areas” in the discussion of Priority 8, and could be linked to the “Improving Ecosystem Health” theme.

The “Necessary Tools” section correctly identifies, among other things, the necessity for integrated databases and interoperability of the relevant technologies, but it fails to identify the key databases or shortcomings that need to be fixed with relevant technology. Finally, the ORPP does not mention the possible use of marine vessels as a component of the Integrated Ocean Observing System (IOOS) operation. This would certainly seem to be a topic worthy of considerable focus as marine vessels represent a potentially inexpensive, underutilized, and prolific source of ocean observations. As well, marine operations involved in deploying and maintaining the IOOS would benefit from the research under this theme.

Recommendations

- **Specific research requirements should be better identified and described.**
- **The plan should clearly distinguish operational activities from research goals.**
- **Research goals should be broader than marine transportation and include areas such as national defense, fishing, and recreation.**

THE OCEAN'S ROLE IN CLIMATE

The research priorities under this theme are:

- 12) *Understand ocean-climate interactions across regions.*
- 13) *Understand the impact of climate variability and change on the ocean, including its biogeochemistry and ecosystems.*
- 14) *Apply understanding of the ocean to help project future climate changes and their impacts.*

This section addresses the role of the ocean in the Earth's climate system, the impacts of climate variability and change on the ocean, and ultimately the impacts of climate variability and change upon human activities in, on, and around the ocean. The title, however, does not reflect the intent to address both the role of the ocean in climate *and* the impact of climate variability and change on the ocean. However, this latter objective is clearly recognized in the rationale for the theme, and it is included as an explicit priority. For example, an alternative title might be "Oceans and Climate."

Clarity and Appropriateness of the Priorities

Priority 14 concerns translation of basic research into practical use for societal benefit. Priorities 12 and 13 address the required basic research components. This is thus a complete, balanced, but broad, set of priorities. However, the committee believes that the priorities can be written in a more compelling way that would excite non-academics, including members of the political and managerial communities – those whose ultimate budget decisions will determine whether much of the proposed work will take place.

Priority 12 would be clear (though very broad) if "across regions" were not included. This phrase could be taken to mean global scale interactions only. Other themes require regional resolution of these interactions. Perhaps what is meant is "within and across regions." Understanding nonlinear time- and space-scale interactions is indeed a climate research challenge that deserves high priority, because of their importance for accurate forecasts (and quantifying uncertainty in forecasts), and because of the need to provide high resolution of those forecasts in time and space. For example, understanding the rate of sea level change, as highlighted in the rationale for this theme, requires explicit recognition of the importance of multiple, compounded sources of variability and change acting on different time and space scales within and across regions. This section would be improved by a brief discussion of process research that is required to advance understanding in this way.

Priority 13 is clear and highly appropriate to the needs of this and other themes. This priority will require fundamental research to develop statistical and/or numerical models that integrate physics, biogeochemistry, and ecosystems. However, the priority is all-encompassing, and it would be useful to identify some areas where there is a fundamental lack of understanding. For example, important aspects of biogeochemistry and ecosystems depend on diffusive processes, and a challenging and essential goal would be to understand climate-related variations of these processes, such as ocean eddies and mixing, and their impacts.

Priority 14 is also clearly relevant and highly appropriate considering society's current concern about accurate and timely climate-related forecasts. However, this priority as written does not reflect the requirement for some useful level of skill or accuracy in relation to the prediction and projection

objectives. For example, it is not at all obvious that “coupled climate models will also allow improved short-term predictions (e.g., hurricane intensity).” It is necessary to quantify the “predictability” of a system before an intelligent approach to generating skillful short-term predictions and longer-term projections can be developed, let alone implemented for use by managers and policy makers. Also, sub-models may need to be developed to enable useful predictions of certain variables from coupled climate models. For example, ocean surface wave statistics are needed for translating the impacts of climate variations into coastal ecosystems and into marine operations, yet they are not now included in climate models.

Appropriateness of the balance among substantive research areas, and between research activities such as observations, modeling, and communicating results.

The critical needs for this theme are to expand sustained ocean observations and to conduct ocean process research required to improve numerical models for ocean state estimation and prediction. It is not clear from the document how the pursuit of the thematic priorities will be coordinated to achieve this.

The need for an integrated and sustained observing system is identified clearly. The importance of sustained and enhanced satellite observations for these climate priorities is called out, but the critical problem of ensuring continuity of existing and planned systems is not addressed. While some of the related issues could be deferred to an implementation plan, assessing the feasibility of meeting the ocean observing requirements for the priority research articulated in the plan does depend strongly on what is assumed about satellite missions. For example, the current skill in estimating the ocean’s physical state is very dependent on the global coverage and integrating nature of altimetric measurements of sea level, and the objective of making useful predictions of climate variations may not be achievable without these satellite observations. The recent NRC report, *Earth Science and Applications from Space: Urgent Needs and Opportunities to Serve the Nation* (NRC, 2005) made the following observation: “Given the long lead times (up to a decade) required to identify user needs and develop instrument capabilities, it is essential to have a prioritized science mission strategy based on compelling scientific issues and societal needs and opportunities... [the current] process is completely inadequate to meet established needs of Earth science or society.” The report specifically mentions the loss of the wide-swath ocean altimeter due to budget shortfalls. The altimeter would have enabled measurement of phenomena needed to improve ocean circulation models and to support marine transportation and fisheries research and forecasts. The requirement for high-resolution coupled climate models is clear to climate scientists, but could be made more compelling in the plan if it were related to the requirements of other themes. Linkages of this theme with the “Enhancing Human Health” and with the “Improving Ecosystem Health” themes would be useful in this regard. The lack of a connection made with “Enabling Marine Operations” and “Stewardship of Our Natural and Cultural Ocean Resources” themes is also apparent. The importance of the combined effects of rising sea levels with storm surges and high wave episodes represents an important linkage between this theme and the “Increasing Resilience to Natural Hazards” theme that could be discussed.

The required synergy between observations and models is not adequately addressed within the research priorities. Improving model parameterizations of unresolved processes is an essential and fundamental research requirement, while improving methods for data assimilation into models is a compelling applied research requirement. There is a hint of these elements in “Opportunities for Progress” (subsection on “Developing the Tools”), but they need to be explicitly and clearly recognized within the research priorities.

Recommendations

- **Linkages with other themes should be improved.** The linkages with “Enhancing Human Health” and with “Improving Ecosystem Health” are provided in the rationale. Connections with “Enabling Marine Operations” are not made here, though some are identified within that theme. There are currently no apparent connections made with the “Stewardship of Our Natural and Cultural Ocean Resources” theme.
- **Priorities 12 and 14 should include discussions of sea level that explicitly recognize the importance of multiple, compounded sources of variability and change.** For example, climate-related changes in surface wave characteristics need to be understood. The superposition of storm surges and high wave episodes on rising sea levels represents an important linkage between this theme and the “Increasing Resilience to Natural Hazards” and the “Enabling Marine Operations” themes. This relationship between short time scale variability and long-term change is clearly identified in the former theme. The larger challenge of understanding the interaction of processes across time- and space-scales could be articulated more clearly in priority 12.

IMPROVING ECOSYSTEM HEALTH

The research priorities under this theme are:

- 15) *Understand and predict the impact of natural and anthropogenic processes that govern the overall level of ecosystem productivity.*
- 16) *Apply understanding of ocean-related socioeconomic activities to assess the ability of marine ecosystems to provide essential goods and services.*
- 17) *Apply understanding of marine ecosystems to develop appropriate indicators and metrics for their sustainable and effective management.*

This theme correctly addresses and acknowledges the complexity and importance of marine ecosystems, and suggests that there is much to be learned about the structure, function, and vulnerability of these systems. However, there are still some significant knowledge gaps that the plan misses, including: factors that control ecosystem stability and productivity; processes acting across interfaces (e.g., sea surface and bottom); linkages between ecosystem types; and, ultimately, the relationship between marine ecosystems and the larger ocean-Earth-atmosphere system. Another important omission is a definition of what constitutes a healthy ecosystem. The committee suggests that the plan would benefit from such a clarification.

Clarity and Appropriateness of the Priorities

Priority 15 emphasizes the incorporation of existing physical, chemical, and biological knowledge across different temporal and spatial scales to address ecosystem questions. However, existing information will not be sufficient to support the development of the dispersal models or next generation of trophic models called for under this priority. Model development will require additional process studies to improve ecosystem forecasting. This could be made explicit and clearer in the text.

The goal for this priority is to predict the impact of processes, natural and anthropogenic, that govern ecosystem productivity. In this context, the term productivity appears to be the sole indicator of ecosystem health. Productivity is one dimension of ecosystem health. However, it alone is a poor metric because marine systems can maintain stable levels of secondary production, set by nutrient loadings and physical conditions (NRC, 2000b; Nixon and Buckley, 2002), even when severely perturbed by human activities. One example is seen in coastal ecosystems that receive high levels of anthropogenic nutrient input. Productivity in such systems is often elevated, showing, for example, highly productive fisheries

and high fisheries landings, (e.g., Nixon and Buckley 2002, Figure 4-2), but they also often experience the negative symptoms of eutrophication such as seasonal hypoxia. This example illustrates the difficulty of trying to use a single metric such as productivity to define ecosystem health. It also highlights the need for a clear definition of ecosystem health. Other important system characteristics to consider include structure, function, and complexity.

As articulated in the rationale, there is a critical need to develop and enhance our understanding of the limits to system resilience, threshold responses, and movement and transport between and through systems. Priority 15, however, only calls for the incorporation of existing knowledge (e.g., physical, chemical, biological) across different temporal and spatial scales to address ecosystem questions and does not address these gaps. Ecosystems exhibit a nonlinear complexity that is relevant to the determination of a system's intrinsic predictability. Much of this complexity is still a black box and a better delineation of the potentially predictable versus generally unpredictable components will be necessary to enhance ecosystem forecasting skill. Monitoring to establish baselines for forecasts and predictions is necessary to understand the relationships between various ecosystem components, and can help to inform and parameterize the models that will be used for ecosystem-based management. However, the emphasis on monitoring should not come at the expense of process-oriented studies. Both approaches are needed, and the plan could seek ways to more clearly integrate the approaches that evaluate ecosystem status via continuous monitoring (data collection) with approaches that are process-oriented and/or hypothesis driven.

Priority 16 seems to suggest that modeling of social and economic factors will predict the impact of human society on marine ecosystems, but not enough is known about the ecological dynamics and system vulnerabilities such as trophic interactions, species-habitat functionalities, and thresholds to build such models. The supporting text acknowledges the need for better models and more information, but this is not clear from the wording of the priority statement.

The supporting text for priority 16 calls for new approaches to evaluate consumptive and non-consumptive use of resources and the need to take into consideration the rights of future generations. In addition to developing new approaches, there is a need to collect finer scale spatial and temporal data on consumptive and non-consumptive use of ocean resources. The statement on the rights of future generations does not describe a research question. However, a clearer statement on the need for better cost-benefit approaches and discounting procedures to assess the true value of sustainable management practices would be appropriate.

Priority 17 calls for the development of approaches to identify one or more indicators that can be used to assess the health of a given ecosystem. When tightly linked to the scientific results supporting priorities 15 and 16, indicators can provide an important source of information on the trends and future threats to an ecosystem. However, this section is particularly vague and will benefit from a clearer definition of what is needed to define a healthy ecosystem.

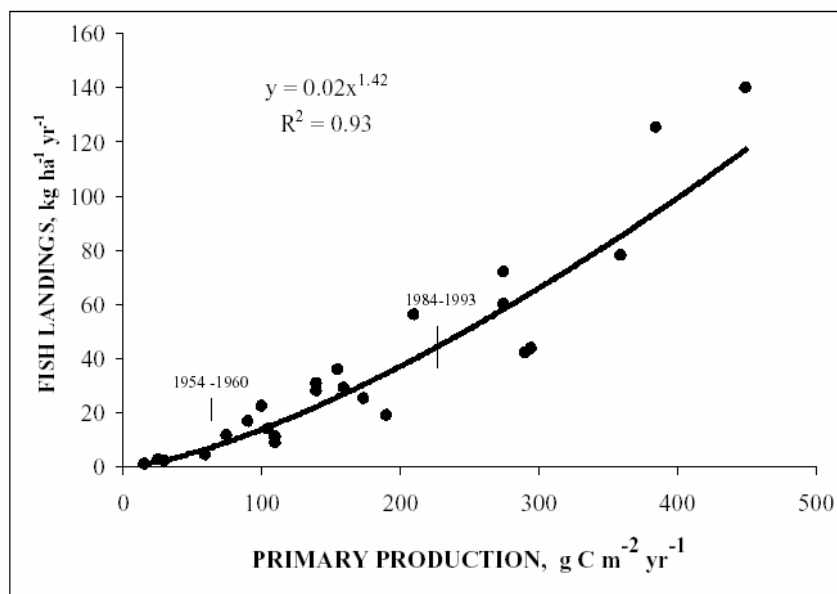


Figure 4-2. Cross system comparisons indicate that primary production increases with nitrogen loading and that fisheries landings are positively related to primary production. As a result, marine systems receiving the highest levels of anthropogenic nutrient loadings also tend to have the highest fisheries landings even if they also experience negative effects of nutrient enrichment such as algal blooms, hypoxia and loss of aquatic macrophytes. Source: Nixon and Buckley, 2002; courtesy of the Estuarine Research Federation.

Appropriateness of the balance among substantive research areas, and between research activities such as observations, modeling, and communicating results.

There is a strong emphasis on observations under this theme, which is appropriate for studies that are relevant to ecosystem health and, ultimately, ecosystem-based management. There is less emphasis on the requirements for developing “effective, reliable ecosystem models” for assessment, prediction, and management, though these requirements are listed. As noted earlier in this section of the committee’s report, the emphasis on monitoring could be balanced with discussion of needed process studies. Process studies are necessary to identify mechanisms that underlie patterns captured in observational studies and are essential for model development. This is a cross-cutting issue could be effectively presented if discussed as a call out box before the individual priorities.

The acquisition of large data sets is acknowledged as clearly important for studies that are relevant to ecosystem health and ultimately ecosystem-based management. However, the need for integrating this information (scientific and socioeconomic) into broadly available databases is not addressed in the plan. Improved sensors and instrumentation generate data in ever finer temporal and spatial scales, allowing important ecological questions to be addressed at scales not previously approachable. Improvements in computational power and increasing sophistication of data assimilation and modeling techniques provide the ability to utilize these large datasets, particularly for research that combine biological and physical processes.

Recommendations

- **The plan should seek ways to more clearly integrate the approaches that evaluate ecosystem status via continuous monitoring (data collection) with approaches that are process-oriented and/or hypothesis driven.**
- **Priority 16 should be revised as follows: “Develop socioeconomic models for application to ocean and coastal issues to help evaluate the impact of multiple human uses on marine ecosystems.”**
- **A broader range of ecosystem responses (not just productivity) should be considered as measures for ecosystem health. The priority would be improved by substituting wording such as the following: “Develop the capability to predict the impact of natural and anthropogenic processes on ecological systems.”** A number of system characteristics, including organization, composition, complexity, and productivity, are important to consider both as measures of impact and as factors that ultimately influence the direction and magnitude of ecological responses. Particular attention should be paid to non-linear dynamics, threshold responses, and properties that influence the ability of systems to resist or recover from natural and anthropogenic stressors.
- **Additional emphasis should be placed on increasing knowledge and understanding of factors contributing to maintenance and restoration of ecosystem health.** There are large gaps in our understanding of how marine ecosystems are organized and function, and how the individual biotic, chemical, and physical components of marine systems respond to both natural and anthropogenic forcings, which are not addressed in the current plan. Information should be translated into a form that is useful to resource managers and policy makers.

ENHANCING HUMAN HEALTH

The research priorities under this theme are:

- 18) *Understand, forecast and reduce ocean-related risks to human health from pathogens, biotoxins, and chemical contaminants.*
- 19) *Understand human health risks associated with the ocean and the potential benefits of ocean resources to human health.*
- 20) *Understand how human use and valuation of ocean resources can be affected by ocean-borne human health threats and how human activities can influence these threats.*
- 21) *Apply understanding of ocean ecosystems and biodiversity to develop products and biological models to enhance human well being.*

This theme addresses a broad array of human health issues that relate to the ocean. Both problems and opportunities are included. The major issues highlighted under this theme – pathogens, harmful algal blooms (HABs), contaminants, marine bioproducts, and biological models – are appropriate and capture the most important research topics. Pathogens, HABs, marine bioproducts and biological models were highlighted in the 1999 NRC report, *From Monsoons to Microbes: Understanding the Ocean’s Role in Human Health*. The other topic included in that report – natural hazards – is included in another theme in the plan. The addition of contaminants in this theme is appropriate and important.

In general, the justification for this theme is solid and should be easily understood by non-scientists. The theme rationale can be strengthened, however, by expanding beyond sickness and drugs to include the potential for major contributions to science in general. In addition, fact-checking will be critical. The estimate of the number of individuals that become ill from shellfish in the U.S. seems much too high. The accuracy of the estimate needs to be checked, the causes of illness (e.g., seafood storage and handling vs. illness related to HABs, pathogens, and contaminants within the marine environment) identified, and a citation provided for the number of individuals affected.

Clarity and Appropriateness of the Priorities

The four research priorities under the “Enhancing Human Health” theme capture a broad range of topics that are appropriate and will clearly advance science in this area. The research priorities for this theme include the most critical issues in the area of oceans and human health, identified at the Denver workshop and in *From Monsoons to Microbes: Understanding the Ocean’s Role in Human Health* (NRC, 1999a). The priorities would be strengthened, however, by increased consideration of the need for basic research, by clearer wording, and by more explicitly describing important linkages with research priorities in other research themes.

Consistent inclusion of the need for basic research on the range of topics within each priority would emphasize the value of an increased knowledge base that could contribute to meeting societal needs. For instance, Priority 21 could encourage research that provides improved understanding of molecular mechanisms underlying the action of natural marine toxins and beneficial compounds, not just “discovery, testing and development” of ocean bioproducts. Similarly, Priority 19 could encourage research to improve methodology and increase the accuracy and sophistication of epidemiological studies that the document recognizes as important to understanding and predicting effects of ocean processes on human health.

The breadth of research recommended in the plan is also unnecessarily constrained by the choice of wording. For example, the statement, “the use of marine species as mechanistic models for the study of diseases, toxicology and biochemical processes relevant to human health” can be altered so that potential contributions to other important fields such as genetics and neuroanatomy are not excluded. The scope of recommended research can be expanded by simply rewording the phrase to “the use of marine species as mechanistic models for a range of processes relevant to human health, including the study of diseases, toxicology, biochemistry and other important processes.”

An important problem with research priorities included in this theme is that the distinction between Priorities 18 and 19 is not clear. It appears that the document may be trying to distinguish between the need for underlying process studies (Priority 18), and quantification of risks and benefits (Priority 19). Clearer wording is needed. The need for process studies to improve the understanding of important underlying mechanisms for all highlighted topics could be more strongly emphasized, whether or not it was the intent in Priority 18 discussed above.

The wording in Priority 20 may also unnecessarily limit the scope of recommended research into economic consequences of real and perceived threats to human health. A full understanding of how real and perceived threats to human health affect fisheries and tourism is not limited to cases where fisheries or beaches are closed.

Appropriateness of the balance among substantive research areas, and between research activities such as observations, modeling, and communicating results.

This theme includes an appropriate balance between substantive research areas, except for the uneven discussion of basic research among the four research priorities. A strength of the priorities is the recognition of the multidisciplinary nature of research needed to advance this theme. The theme also recommends a mix of process studies, observations, and modeling. Not enough detail is provided in this brief section of the plan to assess the mix of these activities.

Linkages to other themes could be made more explicit by using clearer wording to convey the intent of the document. For example, a statement such as “research is needed to better understand and predict the relationship between ecosystem health and the effects of oceans on human health” provides much clearer direction on research needs than the current wording in Priority 18, which states that “these studies should also incorporate research being carried out on ecosystem health.” In addition, connections

between human health and critical issues discussed under other themes could be highlighted to emphasize the importance and opportunities for interdisciplinary and interagency collaboration. Examples of important linkages among themes that could be added include:

- “Improving Ecosystem Health” and “Enhancing Human Health”: The relationship between factors that influence marine biodiversity and the sources of novel marine compounds; and
- “The Ocean’s Role in Climate” and “Enhancing Human Health”: Potential effects of climate change on the distributions and pathogenicity of disease-causing agents, the distributions and toxicity of HABs, and opportunities for bioprospecting.

Recommendation

More careful wording should be used so that the distinction between Priorities 18 and 19 is clear, the scope of recommended research is not unnecessarily constrained, and important linkages among research themes are described. The importance of both process studies and quantification of risk should be clearly emphasized.

5

Interdisciplinary and Multi-mission Ocean Research

THE NEED FOR INTERDISCIPLINARY AND MULTI-AGENCY APPROACHES

Major ocean research and management challenges facing our nation require interdisciplinary approaches that cut across the defined missions of individual government agencies. The complexity of these challenges should be matched by the use of sophisticated approaches that draw on strong expertise from a range of disciplines in order to enhance knowledge and to apply that knowledge to the development of scientifically sound management strategies. Interdisciplinary studies not only increase the breadth of research possible, but also provide the opportunity for scientific advances that result from the transfer of technology, approaches, and ideas from one field to another. Basic research in the marine sciences contributes to other scientific disciplines while advances in other fields yield opportunities for progress in ocean research. For example, recent advances in genomics and bioinformatics have only just begun to be applied to marine science, but already there have been dividends in terms of the ability to characterize marine microbial communities and to provide a greater range of microbial diversity to understand the fundamental properties of microbial genomes. Similar synergies exist for studies in complex dynamical systems, nanotechnology, robotics, resource economics, and co-management governance structures, among others.

Coordinated and collaborative efforts can also reduce unnecessary duplication of efforts and allow the best use of limited available funding. The need for strong interdisciplinary, multi-agency approaches to ocean research has been highlighted in the U.S. Ocean Action Plan, in congressional testimony by NOAA Administrator Vice Admiral Conrad Lautenbacher (2006), and in *Grand Challenges in Environmental Sciences* (NRC, 2001).

The major themes and “Overarching Opportunities” identified in the draft plan are interdisciplinary in nature and require expertise from a variety of fields for substantial progress to be made. It is important to understand how ecosystem health affects human health, and how climate change will affect both the basic functioning of the ocean ecosystem and the services that the ocean provides to human populations. “Increasing Resilience to Natural Hazards,” ecosystem-based management, and many of the other critical topics identified in the draft require collaborations of a range of scientists, including climate scientists, ecologists, physical oceanographers, engineers, and economists and other social scientists.

The committee was encouraged by the acknowledgement in the draft plan of the importance of interdisciplinary science, as well as approaches that cut across agency missions. The six major research themes identified in the draft plan lend themselves to interdisciplinary ocean research efforts (Figure 4-1) and to collaboration both within and across agencies. The selection of the three overarching opportunities identified as key efforts that “must be pursued” – forecasting ocean processes, ecosystem-based management, and the ocean observing system – indicates recognition of the importance of multidisciplinary and multi-mission goals and approaches. Pursuing these opportunities will contribute to

a range of disciplines and to several identified themes, and cut across mission agencies. The opportunities will also require the diverse skills of broad interdisciplinary teams.

The committee was also encouraged that descriptions of major research areas identified the need for integration, systems approaches, and collaboration in the conduct of research as well as in the gathering of data. However, the extent to which general language on these topics was fleshed out to identify important linkages among the six major themes and multidisciplinary needs within research priorities was uneven, and in some instances quite weak (see chapters 3 and 4 for additional discussion of linkages). For example, the “Enhancing Human Health” theme generally missed the opportunity to emphasize linkages, including those emphasized in the NRC report *From Monsoons to Microbes: Understanding the Ocean’s Role in Human Health* (NRC, 1999a) such as:

- The relationship between ecosystem health and the contribution of oceans to human health and disease;
- The relationship between marine biodiversity and the provision of novel marine compounds;
- How climate change effects on the oceans may alter the effects of oceans on human health and disease; and
- How an enhanced understanding of physical and chemical ocean processes may improve predictions of effects of oceans on human health and disease.

The importance of the combined effects of rising sea levels with storm surges and high wave episodes, along with their impacts on coastal erosion, represents an important linkage between “The Ocean’s Role in Climate” and “Increasing Resilience to Natural Hazards” theme that is not discussed. Similarly, the draft plan does not make an explicit connection between “Enabling Marine Operations” and “Stewardship of Our Natural and Cultural Ocean Resources.” This is especially important because of the inclusion of the living marine resources element of the “Stewardship” theme. Although the draft plan is organized into distinct themes, these challenges do not occur in isolation, and in fact, affect each other. Pointing out cross-theme linkages throughout the plan is important because it more accurately identifies both the range of specific expertise needed to address the major themes and the scope of interdisciplinary and multi-agency efforts that are of high value.

Stovepiping of research into distinct themes without clearly defined interconnections can also result in both duplication of efforts and missed opportunities for multi-agency collaboration. Planned redundancy and overlap in agency research efforts can improve the quality of data collection. However, unplanned redundancy utilizes resources that could otherwise be designated to increase the scope and complexity of approaches used to address pressing scientific challenges. For example, the NRC report, *A Geospatial Framework for the Coastal Zone: National Needs for Coastal Mapping and Charting* (2004), found that “at least 15 federal agencies are involved in the primary collection or use of coastal geospatial data... This has resulted in a chaotic collection of charting products that can frustrate the efforts of users to take advantage of existing datasets and build on past studies.”

The use of “promotion of partnerships among disciplines and activities” as stated criteria to prioritize research needs in the draft plan is commended. The committee agrees with the statement in the “Next Steps” section of the draft that these partnerships and collaborative activities should reach beyond U.S. federal agencies and academic institutions. Scientific expertise and advances are greatly expanding, both in the industrialized and developing world. Taking fullest advantage of opportunities and benefits for multidisciplinary and multi-mission programs to meet the challenges identified in this document requires reaching beyond the borders of the U.S., to the private sector, and to other governmental and non-governmental entities whenever taking that step will strengthen science and stewardship. The European Union (EU), in particular, has greatly expanded the number and scope of large multidisciplinary research programs in ocean sciences and related fields. An even broader range of interdisciplinary and international scientific efforts are represented by several research efforts of the International Council for Science (ICSU), including the International Geosphere-Biosphere Programme (IGBP), the Scientific Committee on Oceanic Research (SCOR), and the World Climate Research Programme (WCRP). All of these efforts have very extensive ocean components that include significant involvement of U.S. scientists

and institutions. Partnerships between these EU and ICSU efforts and U.S. efforts should be encouraged, and lessons learned from the structure and organization of these research programs should be applied to similar U.S. efforts. An expanded consideration of international research collaborations would improve the final plan.

IMPLEMENTATION

The focus on interdisciplinary science starts off in the right direction through much of the ORPP. However, the real test of a commitment to move beyond historical disciplinary and organizational barriers will be determined by whether the Implementation Strategy presents a pathway to break down barriers between disciplines and agencies to facilitate multidisciplinary, multi-mission programs. This could be most effectively accomplished by developing the Implementation Strategy in an open and transparent manner. As implementation planning proceeds, it will also be important to ensure that the separation of research challenges into six separate themes does not result in a de-emphasis of the interconnectedness of the challenges, or a narrowing of approaches to address them.

The “Next Steps” section of the current document includes statements endorsed by the committee that may help guide the implementation of the current plan in a positive direction, but that also raises some concern. The committee encourages the major emphasis on coordination and collaboration among the broadly defined research and management communities. However, we question whether restricting collaborative efforts to “existing mechanisms” will impede the pace or scope of scientific progress. A thorough investigation of the current regulations that affect the ability of agency scientists to participate in collaborative research efforts is beyond the scope of this report. Nevertheless, there are examples of impediments to agency-academia and multi-agency collaborative research that indicate that reliance on “existing mechanisms” may sometimes be problematic. For example, restrictions that limit participation by agency scientists in the preparation of proposals can ultimately limit their participation in research relevant to agency missions, and can affect the scope and quality of research contained in proposals. Conversations with agency scientists also suggest that streamlining red tape required to conduct interagency collaborative research would increase the frequency and extent of such collaborations. An examination of whether new mechanisms to foster multi-agency research efforts would improve the quality and scope of interdisciplinary, multi-agency science may be warranted. A starting point for such an investigation may be the discussion of problems and potential solutions surrounding funding of multi-agency and agency-academia collaborative research in *Bridging Boundaries through Regional Marine Research* (NRC, 2000a).

ADDRESSING THE STATEMENTS OF TASK

Statement of Task (5): Does the document adequately identify multidiscipline and/or multi-mission issues?

The committee was encouraged by the emphasis the current document places on the importance of multidisciplinary research and multi-mission issues. However, the committee did not believe the current document adequately incorporates or highlights multidisciplinary and multi-mission research needs within the descriptions of research themes and priorities.

The emphasis above on multidisciplinary, multi-mission efforts and collaboration should not be interpreted as a recommendation to abandon or downplay the importance of development of strong expertise within disciplines. Scientific advances, training, and expertise within disciplines are required for effective multidisciplinary efforts. Advances in narrowly defined fields can be critical to solutions of important scientific challenges, and can help identify the most important contributions from other

disciplines. The importance of strong professional training programs in disciplines that contribute to ocean research is also highlighted in Chapter 7 of this report.

RECOMMENDATIONS

- **The ORPP should provide a more comprehensive description of the needs and opportunities for multidisciplinary research, as well as research partnerships (multi-agency and agency/academic/industry/international), for each societal theme.** A more thorough description of the needs and opportunities for multidisciplinary collaborative efforts in the current document should set the model for the implementation phase of this planning effort.
- **The Implementation Strategy for the ORPP should evaluate the adequacy of existing mechanisms for interagency and agency-academia collaborative research to identify opportunities to improve collaboration among sectors.** Barriers that result from burdensome red tape as well as those resulting from specific prohibitions should be considered.
- **Disciplinary expertise and research should not be neglected in the description of important research and training needs presented in this document.** Such expertise is critical to progress in all areas of science, and it is necessary for the success of multidisciplinary research programs.

6

Evaluation of Near-Term Priorities

SELECTION OF NEAR-TERM PRIORITIES

Near-term priorities (efforts) provide a bridge between the generalities of the themes, the focus of the 21 priorities, and the launching of more specific actions under the umbrella of the ORPP. The committee appreciates the necessity to focus the early research efforts where they will have the most impact and where they can be readily implemented. Given these constraints, the priority areas selected by the JSOST appear to be appropriate, and, although they are not as clearly tied to the 21 priorities as they might have been, will advance the ocean research agenda. The following sections provide the committee's general comments on the near-term priority selection process and specific comments on each of the four near-term priority selections.

GENERAL COMMENTS

Between publication of the first draft and release of the near-term information, the title of the section changed from near-term efforts to near-term priorities. The committee would suggest that calling the four topics "priorities" creates confusion with the 21 priorities and that the plan would be better served by labeling these as research efforts or research packages since they incorporate work within many of the 21 priority areas.

The basis for the selection of the near-term priorities (this term will be used since it is the term used in the plan) is unclear. The introduction to the near-term priority section identifies criteria, but these criteria are not referenced in the discussion of the individual near-term priorities. For example, partnership was introduced as a criterion, but no examples of partnership were provided. More support for the priorities could be gained by a short explanation as to why each was selected.

There is no discussion of the linkage between the near-term priorities and the 21 priorities and six themes. In a quick exercise, the committee attempted to map the linkages, and identified seven longer-term priorities that might be addressed in part by the four near-term priorities. The committee's review would indicate that a case could be made for the following linkages between short- and long-term priorities:

Short-Term Priority	Long-Term Priorities
1	5, 6, & 7
2	15, 16, & 17
3	Overarching Opportunities (observing systems)
4	14

Simple but explicit discussion of where the JSOST sees these linkages would benefit understanding and eventual management of the program. The discussion below of meridional overturning circulation (MOC)

variability highlights the need to explain the relationship between the selected priorities and on-going research efforts.

The discussion indicates that these near-term priorities incorporate aspects of each of the three “overarching opportunities” – forecasting ocean processes, scientific support for ecosystem management, and deployment of elements of the ocean observing system. While such an approach serves many of the 21 priorities, it does establish accomplishment of the “overarching opportunities” as a priority itself. It would be useful for the plan to speak to the role of the “overarching opportunities” versus the themes and priorities.

The near-term priorities are stated without any reference to milestones or metrics that might be used to manage or measure progress towards meeting the objectives behind these priorities. These milestones and/or metrics do not have to be extremely detailed, but should be sufficient to let the readers understand that some thought has been given to measurement of progress.

It is not clear in this section whether or not these near-term priorities will be undertaken with additional resources or whether these research areas will require drawing resources from other programs. If these are truly new efforts rather than continuation of ongoing programs, the context for evaluating the prioritization will be dependent on its impacts on ongoing programs that are important to progress under the 21 longer-term priorities. For instance, will work on the MOC in the North Atlantic result in a diminution of efforts to understand climate variability in the Pacific?

The discussions accompanying the description of the four near-term priorities, especially the last two, are uninspiring. As noted earlier in this report, other ocean commissions (U.S. Committee on Ocean Policy, Pew Oceans Commission, and Joint Ocean Commission Initiative) have made compelling arguments for the need for action to move ocean science forward to support critical societal needs. A few examples under each near-term priority would be valuable in illustrating what might have happened or could happen if the research proposed for the priority is or had been carried out. For example, the discussion of forecasting the response of coastal ecosystems can be tied directly to legitimate scientific questions about Hurricane Katrina and its impacts and how better information would have informed sounder decisions in the days preceding Katrina’s arrival on the coast.

The committee understands that the objective of the ORPP is to define a 10-year research program. The committee is concerned that the execution of the 21 priorities is a major undertaking, and that the near-term work as described represents only a small part of the total research challenge developed within the 21 priorities. The committee understands that support for the near-term priority selections reflects the capability to initiate work in the selected priority areas in Fiscal Year (FY) 2007 or FY 2008, or represents continuation or expansion of FY 2006 work. However, starting with efforts that seemingly relate to less than half of the 21 priorities could leave too much to be accomplished in the later years. The remaining work on the 21 priorities not captured or started in the first effort would represent a gargantuan task, suggesting that perhaps more should be done to bring work on other priorities into the near-term. On the other hand, if the near-term priorities represent a refocus of effort to those activities that will lead to full implementation of the 21 priority efforts in the later years (FY 2009 and beyond), then these near-term priority selections represent a logical path to full implementation of the ORPP. The committee would clearly support such an approach, assuming that ongoing research efforts that are ultimately essential to achieving the longer-term priorities are not disrupted without careful consideration.

The descriptions of the near-term priorities tend to be rather prescriptive in some areas where the scientific community needs greater flexibility and where it might well develop the methodologies to be followed. (An example is given in the discussion of the comparative analyses priority below.) While the development of the overall priority list was based on some interaction with the scientific community outside the government, it is not obvious that the near-term priorities reflect much input from this community. There is a clear need for existing and future agency programs to mix the ideas of researchers who are working at the edge with the challenges faced by agency science personnel.

COMMENTS ON SPECIFIC NEAR-TERM PRIORITIES

Forecasting the Response of Coastal Ecosystems

The substance of this near-term priority merits its inclusion. As noted above, the support for this priority would be improved by some reference as to why it was selected and its ties to ongoing research in this field.

Comparative Analyses

While the priority area appears to be appropriate and is a good candidate for immediate work, the proposed approach appears to be too limited and prescriptive by specifying only comparative analysis. In addition to comparative analysis, there is also an urgent need to improve our understanding of marine ecosystem organization and function. Although it is important to have modeling frameworks, by adopting “consistent modeling frameworks,” the resultant research might be limited to a select group of modeling approaches, stifle innovation, and lock agencies into a single family of models when science dictates the consideration of multiple approaches. The research for this priority would optimally include process studies and address restoration and management strategies.

Sensors for Marine Ecosystems

The topic appears appropriate and a good candidate for immediate action. The narrative description of the research could provide a more compelling discussion of the need for this research and the benefits that this research can bring to the table.

Assessing MOC Variability

While this near-term priority may be appropriate and reasonable, the description lacks discussion of why research in this specific region is a priority and how the proposed research ties into ongoing similar activities. Because of concerns about the description, the committee undertook a more detailed discussion of this priority.

If understanding, assessing, and predicting U.S. climate is considered in the prioritization process, an argument could easily be made for a focus on decadal variations in the North Pacific, which provides “upstream” ocean influences to North American weather patterns and to west coast ecosystems. In terms of potential impact on European weather and climate (and on Atlantic Ocean ecosystems), a rapid decrease of the Atlantic meridional overturning circulation represents a substantial hazard. According to paleoclimate records and climate models, such a collapse has a low probability of occurrence. Because of recent melting of ice sheets, there is heightened concern that the possibility of such a change is increasing (NRC, 2002). Models can potentially quantify the risks, but existing models have to be improved because there are considerable differences among them. And, any model projections have to be initialized accurately with a comprehensive ocean state estimation. Monitoring the branches of the Atlantic MOC is a wise thing to do in any case.

Efforts on the MOC near-term priority are already well under way. The Climate Variability and Predictability (CLIVAR) Atlantic program focus on the MOC has resulted in considerable progress on process studies to improve climate and ocean general circulation models, and the United Kingdom RAPID program in partnership with the National Science Foundation (NSF) has funded and deployed a monitoring array of 22 moorings across the North Atlantic at 26.5°N.

The MOC near-term priority links to several longer term research priorities listed under the “The Ocean’s Role in Climate,” “Increasing Resilience to Natural Hazards,” and “Improving Ecosystem Health” themes, but these connections are not elucidated in the ORPP.

ADDRESSING THE STATEMENTS OF TASK

Statement of Task (6): Does the document identify the highest near-term research priorities to address the goals and expected societal results?

The committee believes that the near-term priorities selected by the JSOST address areas that are of major concern to society and on which substantial progress can and should be made during the next few years. However, a clear rationale is not provided for selecting these four topics as the absolute highest priorities; it is possible that other, equally meritorious selections could have been made. In addition, the selected near-term priorities are not as clearly tied to the 21 priorities as they might have been.

Statement of Task (4a): Is there an appropriate balance between short-term (2-5 years) and longer-term (5-10 years) priorities?

The discussion of the near-term priorities does not provide a clear assessment of the starting point for determining progress towards priority accomplishment or milestones on this path, and the balance of effort between near- and longer-term efforts is not described. These issues may be left for the implementation strategy, but they frustrate a determination of feasibility of achieving the near-term priorities within the 2-5 year time frame. Also, it is not clear how the time frame for the near-term priorities meshes with the timing of the 21 priorities – are they meant to be sequential or parallel efforts?

RECOMMENDATIONS

- **The specific linkages of each of the near-term priorities to the 21 longer term priorities should be identified.** Identification of these linkages would more clearly identify the relationships among priority efforts and the path that near-term research will follow over the long-term.
- **Milestones and metrics should be identified to measure accomplishment and progress.** Such an explanation would better identify the relative magnitude of the envisioned programs and establish a basis for determination of progress.
- **The near-term priorities should include a description of the relationship between what is already being done in each area and how it will be expanded under the research plan.** This should provide significant information that will help determine the time frame for attaining each near-term priority
- **The ORPP should identify how the near-term priorities on the MOC relate to on-going Atlantic and Pacific research efforts. This would help identify possible synergisms and facilitate effective use of research resources.** Both Atlantic and Pacific efforts are important to understanding the ocean’s role in abrupt climate change and its impacts, and further explanation of their relationship would clarify the future path for this research area.
- **Research priorities should not include text that appears to prescribe the types of research to be used to achieve the goals.** Wherever possible, the descriptions of research should provide researchers with the opportunity to determine the best research approaches.

- **The time frames for conducting near-term and longer-term research should be clearly defined with a discussion of the balance of effort between the two sets of priorities.** A sequential approach to these priorities within a 10 year research period would not be adequate to address the research needs identified in the 21 longer term priorities.

Infrastructure and Intellectual Capital

This chapter considers whether the draft ORPP adequately addresses the three broad categories of resources – physical infrastructure, information infrastructure, and intellectual capital – needed to ensure that the priorities delineated under the six themes of the ORPP are achieved.

PHYSICAL INFRASTRUCTURE

In general, the draft plan successfully *lists* most of the types of physical infrastructure that will be required to achieve the ORPP priorities: vessels, earth-sensing satellites, buoys, unmanned vehicles, new sensors (particularly aimed at biological and chemical ocean variables), and more. It also *mentions*, correctly, various needs for more widespread deployments of many of these tools, and integrations of their measurements. However, as it stands, the draft does not go beyond the stages of listing and mentioning. It does not provide goals and objectives that are connected to the stated priorities nor does it delve substantially into implementation-related specifics such as numbers of sensors or vehicles, deployment patterns, and spatial and temporal sampling coverage with respect to the six themes and priorities. Consequently, it is not possible, at this stage, to answer the question of whether the plan “adequately” considers these tools (Task 7).

To define the adequacy of the physical infrastructure requires that the ORPP provide the following information *or* state the need for incorporation of the following information within the implementation plan that would be aligned with the ORPP.

- Delineate the *specific* objectives to be accomplished for each ocean research priority, to include targeted spatial and temporal scales *and* acceptable margins of error for each objective;
- Define the state of the science associated with each objective. This is needed to serve as a baseline and springboard for planning and prioritizing ocean research directions; and the subsequent need for physical infrastructure, whether existing or non-existing, to address each direction;
- Specify the interdisciplinary and multidisciplinary purposes for physical infrastructure across and within research objectives. This determination is warranted to avoid overlap and redundancy of physical infrastructure within and across the ocean priorities; and to make judicious and cost-effective use of existing and planned data and information inventories; and
- Inventory existing physical infrastructure, its adequacy, condition, and long-term viability for the purpose of accomplishing the ocean research priorities and objectives. Existing surveys should be used to facilitate the development of this inventory.

Unless this information is made available within the ORPP or its Implementation Strategy, it will not be possible to accurately determine the adequacy of existing physical infrastructure.

INFORMATION INFRASTRUCTURE

Research needed to address the recommended ocean research priorities will require infrastructure to model, assimilate, analyze, and broadly exchange data and information at levels that currently do not exist. This information infrastructure includes the requisite hardware; software, to include models and visualization tools; and personnel to ensure that massive amounts of disciplinary and interdisciplinary ocean sciences data are available for purposes of research, decision and policy making, and public use. To ensure the capability to store massive amounts of data for long periods of time, decadal and beyond, the required information infrastructure will need to be expandable in architecture.

The discussion here essentially parallels that given above regarding physical infrastructure. Plans exist which address needed information infrastructure for ocean sciences, including state-of-the-science computational resources and an ocean observing system designed to meet the current needs of the nation (e.g., NRC, 2000c; OITI Steering Committee, 2002); however, these plans were not written within the context of the ocean research priorities outlined here. While a discussion of the infrastructure recommended in these and other reports may be informative, it cannot address the committee's specific task (Task 7) regarding the "adequacy." Therefore, the existing plans can only serve as guidance in defining information infrastructure and not as the basis to determine the adequacy of the information infrastructure cited in the ORPP, for reasons discussed in the previous section.

INTELLECTUAL CAPITAL

Education was highlighted as an important and necessary component of a visionary ocean plan by the U.S. Commission on Ocean Policy in *An Ocean Blueprint for the 21st Century* (2004). In the cover letter of *An Ocean Blueprint*, addressed to the President of the United States, the Majority Leader of the U.S. Senate, and the Speaker of the House of Representatives, it states that, "formal and informal ocean education should be strengthened to better engage the general public, cultivate a broad stewardship ethic, and prepare a new generation of leaders to meet future ocean policy challenges." While the objective of educating the people of this nation regarding the ocean is widely acknowledged and accepted, and it is mentioned under each of the six ORPP themes, the draft ORPP is less than clear on the intellectual capital required to understand, manage, steward, and sustain the oceans; and to benefit from them. It is clear, however, that substantial scientific, technological, workforce development, and educational expertise aimed at each theme will be needed once specific goals, objectives, and time lines are established.

As in the preceding two sections, the draft plan contains mention, but scant quantitative assessment, of the need for intellectual capital or the future workforce that will be required to carry out the research and related work envisioned in the plan. Short shrift also is given to the social science workforce needs that are integral to the plan. The needs run from Ph.D. researchers to electronics technicians to science managers to experts in formal and informal education/outreach, social science, and economics, and the overall adequacy or inadequacy of existing preparation for these needs is surely different from type to type.

Much larger national and international issues regarding the enhancement and competitiveness of the science and technology enterprise, well beyond the ORPP, intersect here. Global competitiveness, reviewed in the 2006 NRC report, *Rising Above the Gathering Storm*, involves the technical sophistication of the U.S. workforce, and the ocean sector of that workforce is no exception. The ocean-related workforce draws upon, but is a small fraction of, the national workforce that will need substantial scientific and technological capabilities.

While a discussion of the myriad existing reports regarding the need for intellectual capital in this nation may be informative, it cannot address the specific question regarding the adequacy of the intellectual capital infrastructure for reasons discussed in the previous sections. It is known, however,

that the existence and expansion of the U.S. H1-B⁴ visa program is an indicator of this nation's present need for additional intellectual capital, capital which currently does not exist in this country. Therefore, additional statements and recommendations regarding intellectual capital await the development of the implementation strategy, including its goals, objectives, timetable, and budget.

ADDRESSING THE STATEMENTS OF TASK

Statement of Task (7): Does the plan adequately consider the following resources: physical infrastructure, information infrastructure, and intellectual capital?

Because a definitive statement regarding the adequacy of infrastructure and intellectual capital cannot be made without an examination of the implementation strategy, the ocean research priorities plan does not adequately consider physical infrastructure, information infrastructure, and intellectual capital. An implementation plan typically includes goals, objectives, needs assessments, and budget – elements that are essential for evaluating whether resources are adequate to address the research priorities.

To provide the information needed to determine “adequacy,” a clear and focused delineation of goals and objectives as aligned with the themes, priorities, and near-term priorities is needed. In addition to the statement of objectives, further information is required to detail the specific requirements of each objective, to include spatial and temporal scales of data and information and their “acceptable” margins of error, and the time period during which the objectives are to be accomplished. This level of information was not provided in the draft ORPP.

RECOMMENDATIONS

- **The final plan should move towards greater specificity in the area of infrastructure in order to command serious attention from its intended audiences.** The plan, to be credible, needs to move beyond mere lists of types of infrastructure required. Many prior thoughtful and more detailed plans and reports on important aspects (e.g., fleet renewal, earth sensing satellites, national and global ocean observing systems, computational or information technology requirements and intellectual capital) exist that should be taken into account for current status, trends, and needs/requirements. This should be done notwithstanding the fact that improving the plan in this manner necessarily involves matters of implementation that currently lie outside the draft plan.
- **The ORPP should take heed of broader workforce issues and information sources in revising, clarifying, and improving its projections of needs in this area.** Relevant prior work and reports pertaining to physical and information infrastructure exist largely within the ocean or earth science communities. The same is now true for information on the status and trends of the broad national technological, scientific, and social scientific workforce from which the varieties of “intellectual capital” implicitly required under the ORPP will have to be drawn (e.g., NRC, 2006).
- **The ORPP should place greater emphasis on education as one of the cross-cutting themes of the ORPP.** The committee has previously recommended (in chapter 3) that the concept of cross-cuts be reintroduced in the ORPP and feels that education should be included as one of these cross-cuts. Education is fundamental to the development of the intellectual capital required to meet the needs of the ORPP and underpins the overarching ability to successfully achieve the ocean research priorities. Without the examination of education as an integral interdisciplinary

⁴ The H-1B visa program allows noncitizens into the U.S. for temporary employment in a specialty occupation, including engineering, mathematics, physical sciences, social sciences, medicine and health, and education.

and multidisciplinary component of the implementation strategy and budget for the themes and priorities, it is impractical to engage in a discussion regarding the adequacy of infrastructure and intellectual capital.

References

- Bush Administration. 2004. *U.S. Ocean Action Plan: The Bush Administration's Response to the U.S. Commission on Ocean Policy*. [Online]. Available: <http://ocean.ceq.gov/actionplan.pdf> [October 6, 2006].
- Coleman, F.C., W.F. Figueira, J.S. Ueland, and L.B. Crowder. 2004. The impact of United States recreational fisheries on marine fish populations. *Science*. 305: 1958-1960.
- Federal Oceanographic Facilities Committee (FOFC). 2001. *Charting the Future for the National Academic Research Fleet: A Long-Range Plan for Renewal*. [Online]. Available: http://www.geo-prose.com/projects/pdfs/fleet_report_lo.pdf [October 6, 2006].
- Joint Ocean Commission Initiative. 2006. *From Sea to Shining Sea: Priorities for Ocean Policy Reform*. [Online]. Available: <http://www.jointoceancommission.org/assets/seareport.pdf> [November 27, 2006].
- Joint Subcommittee on Ocean Science and Technology (JSOST). 2006. *Charting the Course for Ocean Science in the United States: Research Priorities for the Next Decade*. Draft, August 30, 2006. [Online]. Available: http://ocean.ceq.gov/about/docs/jsost_chartcourse_083006.pdf [October 6, 2006].
- Lautenbacher, C. 2006. Hearing on "Status of the Oceans – 2006." Written Testimony of Vice Admiral Conrad C. Lautenbacher, Jr., U.S. Navy (Ret.) Under Secretary of Commerce for Oceans and Atmosphere and NOAA Administrator, National Oceanic and Atmospheric Administration, U.S. Department Of Commerce, Before the Committee on Commerce, Science, and Transportation, Subcommittee on National Ocean Policy Study, United States Senate, August 3, 2006. U.S. Congress, Washington, DC. [Online]. Available: <http://www.legislative.noaa.gov/Testimony/lautenbacher080306.pdf> [November 10, 2006].
- National Research Council (NRC). 1993. *Ocean-Atmosphere Observations Supporting Short-term Climate Predictions*. National Academy Press, Washington, D.C. 25pp.
- National Research Council (NRC). 1999a. *From Monsoons to Microbes: Understanding the Ocean's Role in Human Health*. National Academy Press, Washington, D.C. 144pp.
- National Research Council (NRC). 1999b. *Sustaining Marine Fisheries*. National Academy Press, Washington, D.C. 164pp.
- National Research Council (NRC). 2000a. *Bridging Boundaries through Regional Marine Research*. National Academy Press, Washington D.C. 128pp.

National Research Council (NRC). 2000b. *Clean Coastal Waters: Understanding and Reducing the Effects of Nutrient Pollution*. National Academy Press, Washington, D.C. 428pp.

National Research Council (NRC). 2000c. *Illuminating the Hidden Planet: The Future of Seafloor Observatory Science*. National Academy Press, Washington, D.C. 160pp.

National Research Council (NRC). 2001. *Grand Challenges in Environmental Sciences*. National Academy Press, Washington, D.C. 106pp.

National Research Council (NRC). 2002. *Abrupt Climate Change: Inevitable Surprises*. National Academy Press, Washington, D.C. 244pp.

National Research Council (NRC). 2003a. *Enabling Ocean Research in the 21st Century: Implementation of a Network of Ocean Observatories*. National Academies Press, Washington, D.C. 240pp.

National Research Council (NRC). 2003b. *Exploration of the Seas: Voyage into the Unknown*. National Academies Press, Washington, D.C. 228pp.

National Research Council (NRC). 2003c. *Oil in the Sea III: Inputs, Fates, and Effects*. National Academies Press, Washington, D.C. 280pp.

National Research Council (NRC). 2004. *A Geospatial Framework for the Coastal Zone: National Needs for Coastal Mapping and Charting*. National Academies Press, Washington, D.C. 168pp.

National Research Council (NRC). 2005. *Earth Science and Applications from Space: Urgent Needs and Opportunities to Serve the Nation*. National Academies Press, Washington, D.C. 58pp.

National Research Council (NRC). 2006a. *Review of Recreational Fisheries Survey Methods*. National Academies Press, Washington, D.C. 187pp.

National Research Council (NRC). 2006b. *Rising Above the Gathering Storm: Energizing and Employing America for a Brighter Economic Future*. National Academies Press, Washington, D.C.

Nixon, S.W. and B.A. Buckley. 2002. "A strikingly rich zone" – nutrient enrichment and secondary production in coastal marine ecosystems. *Estuaries* 25: 782-796.

Ocean Information Technology Infrastructure Steering Committee (OITI Steering Committee). 2002. *An Information Technology Infrastructure Plan to Advance Ocean Sciences*. [Online]. Available: http://www.geo-prose.com/projects/pdfs/oiti_plan_lo.pdf [October 6, 2006].

Pew Oceans Commission. 2003. *America's Living Oceans: Charting a Course for Sea Change*. A Report to the Nation. May 2003. Pew Oceans Commission, Arlington, Virginia.

U.S. Commission on Ocean Policy (USCOP). 2004. *An Ocean Blueprint for the 21st Century*. Final Report. Washington, DC.

A

Committee and Staff Biographies

Robert Duce (Co-Chair) earned his Ph.D. in inorganic and nuclear chemistry in 1964 from MIT. He is a Distinguished Professor of Oceanography and Atmospheric Sciences and retired Dean of the College of Geosciences at Texas A&M University. Dr. Duce was also Dean of the Graduate School of Oceanography at the University of Rhode Island. His research focuses on atmospheric and marine chemistry, including the global cycling of trace elements, the role of air/sea exchange processes in this cycling, and the impact of atmospheric substances (particularly iron and nitrogen) on ocean productivity. He has been an Ocean Studies Board member since 2001 and has served on several NRC committees. Dr. Duce is a Fellow of the AGU, AMS, AAAS, and the Oceanography Society. He is past president of the Oceanography Society, and the International Association of Meteorology and Atmospheric Sciences. He is also the past president of the Scientific Committee on Oceanic Research (SCOR).

Nancy Targett (Co-Chair) earned her Ph.D. in oceanography in 1979 from the University of Maine. Dr. Targett is Dean of the College of Marine and Earth Studies at the University of Delaware and Director of the Delaware Sea Grant College Program. Her expertise is in biological oceanography and her research focuses on marine chemical ecology, that is, organismal interactions mediated by naturally occurring metabolites, including: plant/herbivore interactions, predator/prey interactions, detoxification of allelochemicals, chemoattraction, and biofouling. She has been an officer in the International Society of Chemical Ecology, an associate editor for the Journal of Chemical Ecology and an Aldo Leopold Leadership Program Fellow. From 1994-2000 she held an appointment to the Mid Atlantic Fisheries Management Council where she chaired several of their committees. She has also served as a member of the Ocean Studies Board (2001-2003).

Denise Breitburg earned her Ph.D. in biology in 1984 from the University of California, Santa Barbara. Dr. Breitburg is a Senior Scientist for the Smithsonian Environmental Research Center. Her expertise is in coastal community ecology and the intersections with human communities. She is currently a co-principal investigator on studies to evaluate the nonnative oyster *Crassostrea ariakensis* as a potential sink or reservoir for pathogens of humans and shellfish. Dr. Breitburg's research focuses on how multiple stressors (such as nutrients, hypoxia, fishing mortality and toxic trace metals) related to human activities influence coastal systems, affecting phytoplankton growth, gelatinous zooplankton population dynamics, and fish growth and survival. She has served as the vice chair of the Chesapeake Bay Program Scientific and Technical Advisory Committee, has been on the Governing Boards of the Estuarine Research Federation and American Society of Limnology and Oceanography, and was a member of the NRC Panel on River Basin and Coastal Systems Planning.

David Conover earned his Ph.D. in 1982 from University of Massachusetts. Dr. Conover is a Professor in and Dean and Director of the Marine Sciences Research Center at Stony Brook University. His expertise is in ecology and evolutionary biology, particularly in the area of fisheries science. He has

researched all aspects of fish and fisheries, from population dynamics of bluefish to ecosystem-based management of fisheries. He seeks to understand the adaptive significance of reproductive, behavioral, physiological, or life history traits in fishes and to extend this knowledge to fundamental problems in resource management. Dr. Conover also maintains an active research program in applied ecology as it pertains to fisheries science and ecosystem ecology. His position as Dean of the Marine Sciences Research Center keeps him involved in a broad range of marine science issues. Dr. Conover's recent paper topics include: harvest selection, genetic correlations, and evolutionary changes in recruitment; ecosystem-based fishery management; and Darwinian fishery science: lessons from the Atlantic silverside.

Cort Cooper has been actively involved in ocean research and development since receiving his BSc and MSc in Engineering at MIT in 1977. He later returned and obtained his PhD from the University of Maine in 1987. Dr. Cooper is an oceanographer in the energy technology company of Chevron. He is also a Chevron Fellow, one of 15 scientists and engineers chosen for their technical contributions to the company. His research efforts have included leading the first comprehensive velocity surveys of the Loop Current in the Gulf of Mexico in the early 1980s and developing a hurricane current model whose results were later adopted as the industry standard. Dr. Cooper has initiated and lead six Joint Industry Projects (JIP) one of them included 32 companies and another 25. These JIPs have successfully resolved major technical questions and established industry standards in some cases. He has been a contributing author of three books, published 14 journal articles, and 28 conference papers. A former member of the Ocean Studies Board, he served on the NRC Committee on Oil in the Sea: Inputs, Fates, and Effects; and has been a frequent advisor to government agencies including NOAA, USGS, U.S. Navy, and the Minerals Management Service (MMS).

Catherine Cunningham Ballard holds a B.S. in Resource Development from Michigan State University (gained in 1986) where she also attended graduate school. She is Chief of the Michigan Coastal Management Program, Michigan Department of Environmental Quality. As the Program Chief, Ms. Ballard develops policy and distributes funds for protecting, restoring, and promoting appropriate, sustainable use of Michigan's Great Lakes coastal resources. Ms. Ballard has a strong interest in land use and growth management issues at the state and national level. Currently, she is President of the Michigan Association of Planning (Michigan Chapter of the American Planning Association), on the Executive Committee and Chair of the Coastal Water Quality and Land Use Committee, Coastal States Organization, on the Board of Directors for the Land Information Access Association, a non-profit that encourages public participation and access to geo-spatial information and technology. She also serves on the Advisory Committees of the Great Lakes Water Studies Institute and Great Lakes Nonprofit Institute at Northwestern Michigan College, Board of Directors of the Michigan Lighthouse Project and Michigan Lighthouse Fund, and Alumni Board of Directors, College of Agriculture and Natural Resources, Michigan State University. Recent past service includes participation in a NOAA Coastal Services Center Blue Ribbon Panel. In 2003, Ms. Ballard received the "Outstanding State Official" Award from the Michigan Association of Regions.

Gerald E. Galloway earned his B.S. in Civil Engineering from the U.S. Army Military Academy in 1957, a Masters of Science in (Civil) Engineering from Princeton in 1962, a Masters in Public Administration from Pennsylvania State University in 1974 and his Ph.D. in Geography (specializing in Water Resources) from the University of North Carolina at Chapel Hill in 1979. He is the Glenn L. Martin Institute Professor of Engineering and an Affiliate Professor of Public Policy at the University of Maryland, College Park. In 2004, Dr. Galloway was elected to the National Academy of Engineering for distinguished leadership in the management of sustainable water resources and education in environmental engineering. His research focuses on the development of U.S. national water policy in general and U.S. national floodplain management policy in particular. Dr. Galloway seeks to develop sound approaches to integrated water resources management. Gerry is a member of the National

Research Council Water Science and Technology Board and currently serves on the NRC Committee on U.S. Geological Survey Water Resources Research as well as the Committee on River Science at the U.S. Geological Survey.

Robert Knox holds a B.A. from Amherst College and in 1971 earned a Ph.D. in Oceanography from Massachusetts Institute of Technology-Woods Hole Oceanographic Institution joint program. He is the Associate Director of the Scripps Institution of Oceanography for Ship Operations and Marine Technical Support as well as a research oceanographer and lecturer. Dr. Knox's research interests are equatorial ocean circulation and dynamics and in acoustic remote sensing of ocean circulation. Dr. Knox chaired the University-National Oceanographic Laboratory System (UNOLS) Council and served as a member of the Ocean Studies Board. He participated in several National Research Council studies, including serving as the U.S. co-chair of the NRC/Academia Mexicana de Ciencias committee on U.S.-Mexico collaboration in ocean science and as chair of a NRC panel on continuation of operational ocean observations in the post-TOGA (Tropical Ocean/Global Atmosphere) era. He is a National Associate of the National Academies.

William Kuperman earned a Ph.D. in physics from the University of Maryland, an M.S. in physics from the University of Chicago, and a B.S. in physics from the Polytechnic Institute of Brooklyn. He is the director of the Marine Physical Laboratory (MPL) at Scripps Institution of Oceanography, University of California, San Diego, where he oversees a large multidisciplinary research program focused on exploratory and technology-based research and development of unique underwater sensor systems. Prior to joining the Scripps faculty in 1993, Dr. Kuperman spent much of his career as a researcher at the U. S. Naval Research Laboratory. He is a major figure in ocean acoustics – his pioneering work on time reversal in observed ocean acoustic fields is one of several major contributions to research in marine acoustics, now being applied to other fields including medicine and acoustical imaging. Dr. Kuperman holds a Secretary of the Navy/Chief of Naval Operations Oceanography Chair in Oceanographic Science. He was past President and a Fellow of the Acoustical Society of America, the former associate editor of *The Journal of the Acoustical Society of America*, co-author of the textbook *Computational Ocean Acoustics* and was awarded the 1995 Acoustical Society of America's Pioneers of Underwater Acoustics Medal. Dr. Kuperman became a member of the National Academy of Engineering in 2004 and is currently serving on the Ocean Studies Board. He has served on several NRC committees, including the Committee on Global Change Research.

Roger Lukas obtained his Ph.D. in oceanography from the University of Hawaii in 1981 and is currently a Professor at the University of Hawaii, School of Ocean and Earth Science and Technology. His research focuses on ocean-atmosphere interaction, seasonal-to-interannual climate variability, tropical ocean currents, equatorially-trapped waves and the distribution of oceanic water mass properties in the tropics and subtropics. Dr. Lukas was a member of the Ocean Studies Board and has participated in several NRC studies, including two studies related to ocean observing systems.

James Sanchirico received his Ph.D. in agricultural and resource economics from the University of California at Davis. He is currently a Senior Fellow with Resources for the Future. He is a natural resource economist, who primarily focuses on the economic analysis of marine policies, especially the effects of individual transferable quotas and marine protected areas. Dr. Sanchirico is currently a member of the editorial council at the *Journal of Environmental Economics and Management* and a past associate editor of *Marine Resource Economics*. He is an Adjunct Professor in the Department of Economics at Georgetown University and he has served as a consultant to international nongovernmental organizations. He is a member of the Association of Environmental and Resource Economists, and the American Agricultural Economics Association. His merits include honorable mention for the "2000 Quality of Research Discovery Award" from the American Agricultural Economics Association for his article "The Bioeconomics of Spatial Exploitation in a Patchy Environment."

Andrew Solow earned his Ph.D. in geostatistics from Stanford University in 1986. Dr. Solow is a Senior Scientist and Director of the Marine Policy Center at Woods Hole Oceanographic Institution. His research experience involves environmental statistics, time series analysis, spatial statistics, Bayesian methods, statistical biology, and ecology. Dr. Solow has authored or co-authored some 120 scientific publications on topics that range from biological diversity, El Niño, to empirical analysis on volcanic eruptions. In addition to his work in environmental and ecological statistics, he has worked on problems connected to the value of scientific information. Dr. Solow is a former member of the NRC's Commission on Geosciences, Environment, and Resources, and the Committee on a Review of A Plan for a New Science Initiative on the Global Water Cycle.

Denise Stephenson Hawk earned her Ph.D. in Geophysical Fluid Dynamics from Princeton University in 1986. She also gained her M.A. degree in geophysical fluid dynamics from Princeton University and completed her M.S. degree in environmental modeling from The George Washington University; and B.A. degree in mathematics from Spelman College. She is Chairman and CEO of The Stephenson Group, LLC, a consulting firm in Atlanta, Georgia. The Stephenson Group, LLC is engaged in a variety of assignments within government, business and academia which provide strategic, operational and tactical planning in areas of the environment and education. Dr. Stephenson Hawk has served as an atmospheric scientist for the National Aeronautics and Space Administration (NASA); ocean systems analyst in areas of underwater acoustics for AT&T Bell Laboratories; and as provost, department chair and professor within academia. She currently serves on the science advisory board for the Department of Interior's Minerals Management Service; steering committee for the National Climate and Weather Commission; and advisory board for the Southeast Center for Ocean Science Education Excellence. Dr. Stephenson Hawk is a former member of the science advisory boards for the National Oceanic and Atmospheric Administration, NASA, National Science Foundation, and the Ocean Research Advisory Panel for the National Oceanographic Partnership Program.

STAFF

Susan Roberts (Study Director) became the Director of the Ocean Studies Board in April 2004. Dr. Roberts received her Ph.D. in Marine Biology from the Scripps Institution of Oceanography. Dr. Roberts' past research experience has included fish muscle physiology and biochemistry, marine bacterial symbioses, and developmental cell biology. She has directed a number of studies for the Ocean Studies Board including *Nonnative Oysters in the Chesapeake Bay* (2004); *Decline of the Steller Sea Lion in Alaskan Waters: Untangling Food Webs and Fishing Nets* (2003); *Effects of Trawling & Dredging on Seafloor Habitat* (2002); *Marine Protected Areas: Tools for Sustaining Ocean Ecosystems* (2001); *Under the Weather: Climate, Ecosystems, and Infectious Disease* (2001); *Bridging Boundaries Through Regional Marine Research* (2000); and *From Monsoons to Microbes: Understanding the Ocean's Role in Human Health* (1999). Dr. Roberts specializes in the science and management of living marine resources.

Frank Hall received his Ph.D. in Oceanography from the University of Rhode Island in 1991. His dissertation research involved Quaternary paleoceanographic reconstructions of the high-latitude Atlantic and Arctic Oceans. In 1994, he was awarded a Ford Foundation Postdoctoral Fellowship to study at the Institute for Arctic and Alpine Research, University of Colorado. In 1998, Frank joined the faculty at the University of New Orleans as a geoscience educator, focusing on the preparation of preservice and inservice grades K-12 science teachers. Prior to joining to the Ocean Studies Board, he served as a Program Officer in the Division of Elementary, Secondary, and Informal Education at the National Science Foundation.

Susan Park received her Ph.D. in oceanography from the University of Delaware in 2004. Her dissertation focused on the range expansion of the non-native Asian shore crab *Hemigrapsus sanguineus*. In the summer of 2002, she participated in the Christine Mirzayan Science and Technology Graduate Policy Fellowship with the Ocean Studies Board. During her fellowship, she worked on the study *Nonnative Oysters in the Chesapeake Bay*. Since returning to the Ocean Studies Board full-time in 2006, Susan has worked on the following reports: *Review of Recreational Fisheries Survey Methods* and *Dynamic Changes in Marine Ecosystems: Fishing, Food Webs, and Future Options*.

Toni Mizerek is currently working toward her Masters at San Diego State University in the Quantitative Conservation Ecology lab. She is working on a population modeling project to assess the combined effects of both harvesting and habitat fragmentation on blue crab populations in Chesapeake Bay. In the summer of 2006, she participated in the Christine Mirzayan Science and Technology Graduate Policy Fellowship with the Ocean Studies Board. Toni's conservation interest began during her undergraduate education at the University of California, Santa Barbara. There, in one of the world's biodiversity hot spots, Toni became aware of many local issues of conservation concern and learned how essential a role policy can play. Though the projects she was involved with included a variety of systems, Toni was pleased to focus her attention to the marine realm. She has lived along the coast most of her life and has recognized many serious issues and anthropogenic impacts present in the oceanic environment. She hopes that her time at the National Academies will strengthen her understanding of the policy and science interface enabling more effective strategies to conquer current scientific concerns. Toni loves outdoor activities and sports like snowboarding and softball and is looking forward to discovering what the east coast has to offer.

Jeffrey Watters received his M.S. in limnology and marine science from the University of Wisconsin-Madison in 2006. His thesis examined the interactions between commercial longline fisheries and endangered sea turtles species in the Hawaiian Pacific. In the fall of 2006, he participated in the Christine Mirzayan Science and Technology Graduate Policy Fellowship with the Ocean Studies Board. During this fellowship, he worked with the committee to Review the JSOST Research Priorities Plan, assisted with the development of the of a study reviewing the baseline monitoring plan for an open-rack, liquefied natural gas deepwater port in the Gulf of Mexico, and assisted with the development of a study reviewing sea turtle population assessment methods.

Sarah Capote gained her B.A. in history from the University of Wisconsin-Madison in the winter of 2001. She is a senior program assistant with the Ocean Studies Board. During her tenure with the Board, Ms. Capote worked on the following reports: *Exploration of the Seas: Voyage into the Unknown* (2003), *Nonnative Oysters in the Chesapeake Bay* (2004), *Future Needs in Deep Submergence Science: Occupied and Unoccupied Vehicles in Basic Ocean Research* (2004), the interim report for *Elements of a Science Plan for the North Pacific Research Board* (2004), *A Vision for the International Polar Year 2007-2008* (2004), *Marine Mammal Populations and Ocean Noise: Determining When Noise Causes Biologically Significant Effects* (2005), *Final Comments on the Science Plan for the North Pacific Research Board* (2005), *Understanding Oil Spill Dispersants: Efficacy and Effects* (2005), *Managing Coal Combustion Residues in Mines* (2006), and *Mitigating Shore Erosion along Sheltered Coasts* (2006).

B

Acronyms

CLIVAR	Climate Variability and Predictability Program
DELS	Division on Earth and Life Studies
ENSO	El Niño/Southern Oscillation
EU	European Union
HABs	harmful algal blooms
ICOSRMI	Interagency Committee on Ocean Science and Resource Management Integration
ICSU	International Council for Science
IGBP	International Geosphere-Biosphere Programme
IOOS	Integrated Ocean Observing System
JSOST	Joint Subcommittee on Ocean Science and Technology
MOC	meridional overturning circulation
NAO	North Atlantic Oscillation
NOAA	National Oceanic and Atmospheric Administration
NRC	National Research Council
NSF	National Science Foundation
NSTC	National Science and Technology Council
OAP	Ocean Action Plan
OPF	Ocean Priorities Framework
ORPP	Ocean Research Priorities Plan
PDO	Pacific Decadal Oscillation
SCOR	Scientific Committee on Oceanic Research
USCOP	U.S. Commission on Ocean Policy
WCRP	World Climate Research Programme