Grand Challenges
for Ocean Sciences Research

BY ALAN BRANDT

THE PURPOSE of this note is to provide a framework for establishing Grand Challenges for ocean research that would inspire the research community, our national legislators, and the general public. Such challenges should provide a middle ground between previously proposed long-term studies that encompass virtually all aspects of ocean research, and studies that address specific near-term issues that confront the contemporary world, such as tsunami prediction and disaster mitigation. Toward this end, specific issues are identified that can pose a challenge to the research community over the next decade while making a significant contribution to problems of national concern.

The recent reports of the U.S. Commission on Ocean Policy (2004) and the Pew Oceans Commission (2003) provide a framework for ocean policy, stewardship, and governmental leadership for the 21st century. Even in these times of national stress, these reports and the associated congressional testimony have led to a revitalized effort to review and restructure our national ocean policy toward achieving a sustainable ocean environment. As a result, President Bush submitted a "U.S. Ocean Action Plan" (Council on Environmental Quality, 2004) to Congress in December 2004. These efforts provide a long overdue top-level perspective for ocean stewardship. The Council on Environmental Quality is at present undertaking the development of an Ocean Research Priorities Plan (ORPP), the Administration's next step in its ocean policy. When completed, the ORPP will be an interagency planning document describing the vision and priorities for U.S. ocean science and technology for the next 5–10 years (for more information and to download the draft report, see http://ocean.ceq.gov/about/sup_jsost_prioritiesplan.html).

Policy, stewardship, and governmental leadership needs are indeed paramount for the salvation of the world's waters. However, it is also of great import to provide guidance to focus the basic and applied research needed to provide a foundation for management of our ocean resources. Bob Gagosian eloquently made this case to the general public in an article (Gagosian, 2004) that acknowledges that we are now in a new age of oceanography, one that can reap tremendous societal benefits.

To foster such major advances in ocean research in this new age, we need to adopt as a goal *specific* Grand Challenges for a nominal ten-year period, as has been successfully accomplished in the Human Genome Project (more information available at http://www.ornl.gov/sci/techresources/Human_Genome/home.shtml). A similar endeavor resulted from the inspiring message that led to sending astronauts to the moon, viz.: "…I believe that this nation should commit itself to achieving the goal, before this decade is out, of landing a man on the moon and returning him safely to the earth" (Kennedy, 1961). Both of these earlier challenges resulted in concerted (and adequately funded) efforts that achieved their goals. We can do the same for the important challenges in the ocean sciences that will also reap far-ranging benefits.

ESTABLISHING RESEARCH PRIORITIES AND GRAND CHALLENGES

In a published shortened version of a talk in the early 1990s, John Knauss (Knauss, 1994) observed that while many of the same fundamental questions regarding the ocean's behavior indeed still remain, scientific advances were the result of ad-

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vances in technology (i.e., our ability to observe and measure the ocean's properties). He was also optimistic regarding the future of ocean research due to societal needs for understanding ocean pollution, weather forecasting, economic use of the oceans, marine archeology, and issues related to global warming.

To direct the ocean research community toward achieving these desired advances, a specific list of ocean challenges has frequently been posed, for example, the National Research Council's review of the National Science Foundation (NSF) ocean program (NRC, 2000) provided not only a comprehensive review of NSF's accomplishments over the last fifty years, but also a comprehensive assessment of the research issues for the ensuing years. Issues were identified in each of the areas of oceanographic research, and, while insightful, they are overly broad and all-encompassing. In contrast, the list of research needs made by Larry Clark in the December 2005 issue of *Oceanography* (Clark, 2005) in response to the catastrophic hurricanes Katrina and Rita, are more specifically focused on the effects of hurricanes, storms, and floods.

RESEARCH PRIORITIES

In the past decade there has been a significant evolution in national and international awareness regarding environmental protection (including the ocean) and national security/homeland defense. These issues set the stage for identifying current research priorities and for defining potential Grand Challenges. Paramount current concerns regarding the ocean include: anthropogenic effects on the environment, pollution, and global warming; the devastating effects of natural disasters due to increased habitation of coastal areas (as dramatically evidenced by the tsunamis and hurricanes of 2004 and 2005); the critical role of the coastal ocean in the viability of the national economy; and the increasing need for military (naval) operations and homeland protection activities in the littoral.

Based on these current national concerns, I propose the following list of research priorities for the ocean sciences (with deference to the greater ocean community that has previously put forth other listings and almost certainly, will currently have additional or other priorities).

1. Understanding and modeling global climate evolution and humankind's effects.
2. Understanding and predicting long-term effects of natural and anthropogenic modifications on oceanic ecosystems.
3. Identification of precursors to natural catastrophic environmental events, such as floods, storm surges, tsunamis, and hurricanes.
4. Understanding the long-term effects of short-term, episodic events (e.g., storms, hurricanes, spring freshet) on littoral bio-physical processes.
5. Understanding the mechanisms governing diverse, non-standard life forms discovered in the remote regions of the ocean, such as those present in the vicinity of hydrothermal vents and in submerged Antarctic lakes.
6. Development of viable sources of energy and resources from the sea.

IDENTIFYING GRAND CHALLENGES IN THE OCEAN SCIENCES FOR THE EARLY 21ST CENTURY

The nature of a "true" Grand Challenge is such that it represents an effort that is of high national priority, high impact (and likely high risk), captures the imagination of the research community as well as the general public, and has a finite duration with a clear identifiable goal. Regarding duration, I would suggest a ten-year effort, as this is a period in which goals can be reasonably conceived and programs can be developed within a realistic political, economic, and funding horizon.

Viable Grand Challenges for the ocean sciences must address issues of current concern, such as the research priorities listed above. These challenges should also focus on enhancing understanding of the ocean beyond immediate concerns and contribute to allaying these concerns. Casting the research priorities in a

Grand Challenge #1: Provide direct linkages between specific natural and anthropogenic emission sources and global climate evolution.

form that meets the criteria for a Grand Challenge is a formidable task, especially the criterion for establishing a specific identifiable goal, one that will clearly signify completion and success.

A review of the research priorities in response to the suggestion of one of the anonymous reviewers has lead to the selection of two candidate Grand Challenges that are put forth for consideration by the ocean community at large. These result from Research Priorities 1 and 6.

Grand Challenge #1: Provide direct linkages between specific natural and anthropogenic emission sources and global climate evolution.

This Challenge involves identifying and prioritizing specific sources of emissions that are absorbed into the atmosphere and the ocean (e.g., CO_2, ozone, nitrogen oxides) and correlating these sources with global warming, sea-level rise, air-sea exchange, ozone-layer alteration, and other phenomena. Relevant studies would include monitoring and modeling to gain an understanding of phenomenological transformations. Solid evidence of the linkage between emission source and climate evolution would provide a firm basis for sociological, political, and economic policies.

Grand Challenge #2: Develop economically viable sources of energy from the sea sufficient to provide ten percent of the U.S. energy needs.

Continued reliance on fossil fuels will increasingly lead to political and economic uncertainties. Compared to terrestrial resources, the store of resources in and below the ocean have barely been investigated. Utilization of oceanic resources could provide a basis for sustaining a high quality of life worldwide. This Challenge would necessitate a further understanding of the nature of ocean processes in order to make energy sources, such as tidal power, ocean thermal energy conversion, mineral extraction, and others, economically viable.

IMPLEMENTATION—INFRASTRUCTURE REQUIREMENTS

Addressing these or other Grand Challenges will require studies that encompass the full range of oceanographic scales—greater than ten orders of magnitude—from ocean-basin scales of the order 10^4 km to sub-millimeter scales characteristic of microstructure and plankton. It is widely acknowledged that advances in oceanography have and will inevitably come from advances in measurement methods (Knauss, 1994) (i.e., platforms and instrumentation). Moreover, considering the status of current knowledge and the increasing focus on multi-scale and interdisciplinary processes, it will be necessary to collect data that would provide information on high-frequency and small-scale processes over long time periods; processes that transpire during storms and other short-term/abrupt events wherein significant transformation occurs during short periods; simultaneous measurements of interdisciplinary dynamical processes; and coordinated spatial coverage over contiguous ocean regions, especially in the littoral.

To obtain the data required on these broad temporal and spatial ranges, it will be necessary to provide research platforms that can support these broad-based measurements. Thus, it will be necessary to continue the currently increasing emphasis and reliance on coastal observatories and satellite observations. In addition, a new type of research platform is proposed: a Quasi-Permanent Ocean Platform (QPOP). A platform of this type would provide a means for obtaining high-frequency, long-duration, interdisciplinary data at sites of particular interest (e.g., coastal upwelling regions, the edge of the Gulf Stream, areas of hurricane genesis), similar to the Tropical Ocean Global Atmosphere (TOGA) program array. Such a QPOP would be not unlike the Floating Instrument Platform (FLIP) (more information available at http://www.mpl.ucsd.edu/resources/flip.intro.html) with, however, a longer on-station residence time (say one to two years at a specific site) in order to provide data on the long-term annual variability of both the large- and small-scale interdisciplinary processes. A semi-submersible platform (for example, go to http://www.offshore-technology.com/projects/visund/visund5.html), possibly purchased or donated to the research community as "retired" naval ships often are, could be used for this purpose. Such a QPOP could serve as a base for *in situ* surface, subsurface, and meteorological instruments as well as a base for unmanned/autonomous underwater vehicles (UUVs/AUVs) and small research vessels.

CONCLUDING REMARKS

In this early part of the 21st century, there has been a realignment of national concerns and priorities. Moreover, ocean research is entering a new era, one in

which the study of ocean processes will include controlled experiments—perturbations of the ambient ocean to explore the ocean's behavior. As a result, key aspects of oceanography in the early 21st century will encompass the study of a significantly broader range of temporal and spatial scales; an increasing focus on interdisciplinary, bio-chemical-physical interactions; an increasing emphasis on littoral/coastal processes; continuing advances following from advances in measurement techniques; increased knowledge resulting from ocean "experiments;" and the need to develop platforms and instrumentation to record data at high sampling rates for long periods.

In keeping with these changes, a list of research priorities and suggested examples of potential Grand Challenges for ocean research for the early 21st century has been proposed. The Grand Challenges provide a middle ground between the broad and generally all-encompassing listings of research needs and listings of efforts needed to address specific current issues, while addressing current research issues of significant societal benefit that are potentially achievable within a nominal ten-year timeframe. Proposals from the ocean research community for additional or alternative research priorities and Grand Challenges are encouraged and, in fact, expected. Such a dialog would, I believe, be constructive and beneficial.

Prior advances leading to new and unique capabilities in the early 21st century offer to the oceanographer, manager, and citizenry an opportunity to observe in unprecedented ways the evolving ocean weather and climate and its impact on humankind. For the first time, we should be able to make responsible decisions on how to respond to the natural and manmade changes in the nature and behavior of the ocean. The research priorities and Grand Challenges presented, while only a personal perspective, hopefully will stimulate others to provide alternative views and thus aid in fostering the movement toward enhancing the base for enhanced ocean research recently initiated by the congressionally mandated U.S. Commission on Ocean Policy (2004).

Grand Challenge #2: Develop economically viable sources of energy from the sea sufficient to provide ten percent of the U.S. energy needs.

ACKNOWLEDGEMENTS

The preparation of this paper was supported in part under a J.F. Hafsted Fellowship awarded to the author by the Johns Hopkins University Applied Physics Laboratory. The author acknowledges and appreciates the review and contributions provided by S.E. Rennie and D.L. Porter, and E.P. Brandt. The nature of this article was such that it inevitably engendered extremely diverse reviews, all of which had merit. Thus, I am also appreciative of the comments provided by the anonymous reviewers and E.S. Kappel, editor of *Oceanography*.

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