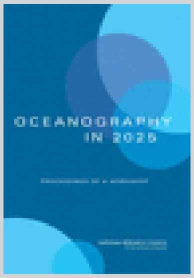

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OCEANOGRAPHY IN 2025

PROCEEDINGS OF A WORKSHOP

Deborah Glickson, *Editor*

Committee on Oceanography in 2025: A Workshop

Ocean Studies Board

Division on Earth and Life Studies

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Preface

On January 8 and 9, 2009, the Ocean Studies Board of the National Research Council (NRC), in response to a request from the Office of Naval Research, hosted the "Oceanography in 2025" workshop. The goal of the workshop was to bring together scientists, engineers, and technologists to explore future directions in oceanography, with an emphasis on physical processes. The focus centered on research and technology needs, trends, and barriers that may impact the field of oceanography over the next 16 years, and highlighted specific areas of interest: submesoscale processes, air-sea interactions, basic and applied research, instrumentation and vehicles, ocean infrastructure, and education.

To guide the white papers and drive discussions, four questions were posed to participants:

- What research questions could be answered?
- What will remain unanswered?
- What new technologies could be developed?
- How will research be conducted?

Four keynote speakers, chosen for their diversity of opinions, presented their vision of future needs in oceanography from observation, modeling, and/or societal viewpoints. We wish to thank Dr. Chris Garrett, University of Victoria; Dr. Russ Davis, Scripps Institution of Oceanography; Dr. Kelly Benoit-Bird, Oregon State University; and Dr. Raffaele Ferrari, Massachusetts Institute of Technology. In addition, we wish to thank Rear Admiral David Titley, Commander of the Naval Meteorology and Oceanography Command, for his introductory comments to the workshop participants.

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 NRC's Report Review Committee. The purpose of this independent review is to provide candid and critical comments that will assist the authors and the NRC in making the published report as accurate as possible and to ensure that the content of the proceedings is relevant to the workshop. The review comments and draft manuscript remain confidential to protect the integrity of the deliberative process. We wish to thank our reviewers for the time and effort they put into this review. We also wish to thank Cheryl Logan, NRC Christine Mirzayan Science and Technology Policy Graduate Fellow, for her work copyediting this document.

The workshop proceedings should not be confused with a National Academies consensus report. The proceedings do not contain findings or recommendations endorsed by the National Academies or the National Research Council. Any advice, findings, conclusions, or recommendations in these proceedings are strictly those of the author(s) and do not reflect consensus of the workshop participants.

The agenda and participant list are reprinted in Appendixes A and B, respectively.

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Introduction and Goals

Linwood Vincent[*]

In 2008, the Office of Naval Research (ONR) formulated a series of strategic roadmaps for the Navy's major research focus areas to promote planning for the next series of budget requests. In this planning process, a longer term vision for each focus area is necessary to understand how the detailed short term (2-5 year) research fits within larger strategic goals. An additional aspect of this process is the integration of the efforts of ONR with that of the Naval Research Laboratory (NRL).

The Physical Oceanography Program at ONR is within the Operational Environments Focus Area and has a corresponding effort at the NRL. Both organizations devised and then integrated roadmaps for oceanography in some detail out to 2015 and to a lesser degree of detail to 2025. The program managers based the roadmaps on their perception of potential progress in the science and the overarching needs of the Navy and the Marine Corps. Since in such a process it is impossible to foresee future funding trends, efforts were made to emphasize what should be possible technically. Everyone involved in the process felt unsure about reaching out to 2025 since so many unknown factors may have an influence on the path of achievement at that time.

During the same period, the Oceanographer of the Navy's office embarked on a similar quest directed at a vision of where operational oceanography could be in 2025. This effort spanned not just oceanography, but all the major programs of the Oceanographer's office (weather

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[*] Office of Naval Research

and ocean forecasting, charting, etc.). Although the emphasis was on technology and operations, an important element was to envision what scientists in various fields needed to have accomplished by the 2025 timeframe. The participants in this effort were mainly drawn from the operational commands with participation from the ONR/NRL research community.

In reviewing the processes for the two Navy studies it is clear that one group that has not been formally engaged in this process are the research scientists who, to a large part, will create the new science from now until 2025. ONR asked the Ocean Studies Board to hold a workshop to address Oceanography in 2025 from the research perspective. Realizing that few people have perfect foresight about the future, the goal was to solicit the viewpoints of a wide range of scientists with the expectation that the sum of the viewpoints, unfiltered, may better span the set of possibilities than a consensus.

The workshop proceedings will be useful as a comparison to the visions expressed in both the ONR roadmaps and the Oceanographer's vision statement. Additionally, the Ocean Studies Board has been tasked by the Navy, the National Science Foundation (NSF) and other members of the Joint Subcommittee on Ocean Science and Technology (JSOST) to address two related studies: ocean research infrastructure needs out to 2030, and the evolution of the U.S. academic research fleet over the next few decades. Both of these studies inherently depend upon some vision of oceanography in 2025, and will utilize the input provided by this workshop. Finally, by making the proceedings widely available, the oceanographic community as a whole is invited to consider the research trajectory for the next 16 years.

Integrated Oceanography in 2025

*John J. Cullen**

OCEANOGRAPHY NEEDS TO DEFINE ITS ROLE IN A RAPIDLY CHANGING WORLD

Rapid technological advances in ocean observation, modeling and information systems provide the potential for nearly limitless expansion of marine research as the field of oceanography emerges from its data-limited foundations. Now, the challenge is to define the best strategies for exploiting new capabilities while justifying the required investments when resources are limited. Oceanographers can address this challenge by conducting their research in a new and much more immediate context of science serving society's need to observe, understand and predict changes in their local, regional and global environment. This leads to a proposal: Oceanography should become part of a profoundly cross-cutting Global Environmental Portfolio that must be developed if humanity is to meet the challenges of climate change and increasing human impacts on the planet.

The ocean environment is under increasing stress. In addition to the threats of greenhouse-gas-driven climate change—rising global sea levels, disappearing Arctic sea ice and global ocean acidification—society's reliance on the ocean for sustenance is increasing, resulting in overexploitation of marine resources and increased reliance on aquaculture. Meanwhile, marine biodiversity decreases worldwide with uncertain implications. There is also a global migration of the human population to the coast that is putting more pressure on the coastal zone to support

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*Dalhousie University

increased commercial and recreational activity while satisfying growing demands on its natural resources, even as levels of pollution rise. Quite simply, the ocean and human society's relationship with it are changing profoundly and very rapidly; in response, society must develop effective strategies for stewardship and management of the ocean using a multidisciplinary approach that takes into account the ecosystem's numerous interconnected components and also the human dimension. To do this oceanography must join with other disciplines and sectors (commerce, management, policy) to become part of an integrated oceans element of an even broader Global Environmental Portfolio. This movement cannot be led by oceanographers, but we can contribute to it.

The challenges of climate change and increasing human impacts on the ocean will drive ocean research during coming decades. However, research alone cannot do the job. Ocean researchers must work across disciplines to provide policy makers, and the public they serve, with clear and understandable assessments of the state of the ocean and its sensitivity to climate and human influences in coming decades of change, if not environmental crisis. The challenge extends beyond finding the answers to technical and scientific questions: the results of scientific research must be validated and conveyed to a broad range of users, quickly and effectively. New forms of communication will be key—among disciplines, across sectors, and with the public. Rapid and broadly accessible communication of the state of the ocean, and its future role in the biosphere, will be a primary justification and goal for ocean research.

LONGSTANDING QUESTIONS ABOUT THE OCEAN WILL BE ANSWERED

Physical forcing of the ocean by weather and climate, the resultant responses of marine food webs, and their combined influences on the chemistry of the ocean and atmosphere, are intimately and inextricably linked. Consequently, the role of the ocean in global climate change, and the effects of climate variability on living marine resources including fisheries, can be understood only by observing, describing and ultimately predicting the state of the ocean as a physically forced ecological and biogeochemical system. What has been lacking until recently is the capability of integrating the study of physically forced ecosystem dynamics and biogeochemical cycling across scales, from:

- The mesoscale (with spatial scales of order 10-100 km and temporal scales of order 10 to 100 days)—on which pelagic ecosystem structure responds to changes in ocean circulation and mixing,
- To the regional and seasonal scales—on which relationships

between ocean circulation and nutrient distributions determine patterns of primary productivity and fisheries production,
- To the basin scale—on which the oceanic inventories of carbon dioxide and fixed nitrogen, major drivers of climate, are determined over centuries and longer.

The missing element has been the capability for vertically resolved observations of physical forcing and ecological-biogeochemical responses in the ocean interior (e.g., nutrients, oxygen, components of the plankton and indicators of their physiological status) to describe how submesoscale processes contribute to critically important mesoscale variability. By 2025, this capability will be mature (deployed on gliders and profilers, and complemented with direct observations of genes and gene expression), and years of data from across broad expanses of ocean will be available. These observations will form the link between detailed oceanographic process studies, surveys using advanced biogeochemical analyses, and paleoceanographic reconstructions of the relationships between climate and ocean biogeochemistry. In 2025, we will have the data to test comprehensively the 20th century hypotheses about how ocean systems work (e.g., the influences of environmental variability on pelagic food web structure), and we will certainly develop new hypotheses to explain previously unobserved phenomena.

MARINE SYSTEMS WILL BE MUCH MORE PREDICTABLE

Next-generation numerical models will directly incorporate interdisciplinary data from ocean observing systems (satellites, gliders, profilers, moorings) to guide forecasts of a broad range of state variables (concentrations of nutrients, oxygen and different components of the plankton, including some species). In addition, the models will assimilate, directly from sensors, information on biological and chemical rate processes, including photosynthesis and a range of biochemical transformations. Measurements of inherent optical properties will provide quantitatively grounded proxies for physical, chemical, and biological constituents as well as some rate processes. We will thus be able to predict the variability of key physical, chemical, and biological properties and processes, with measurable skill. But we will never be able to describe fully the complexity of ocean ecosystems.

Importantly, by 2025 we will appreciate the limits to predictability of ocean ecology on scales from days to years. This will be fundamental to our evaluation of predictions of changes for decadal time scales and longer, which no doubt will become increasingly important as society grapples with environmental challenges in a rapidly changing world.

Oceanography in 2028*

Mark Abbott[†]

Predicting the future is an interesting, if somewhat futile, exercise, but at the least it can provoke us to think about where we are and where we might be headed. We can (a) project forward our wishful thinking, (b) assume that there will be little or no change in how we operate—or (c) assume that no one will remember our forecasts, so what we say is of little importance. I intend to follow a different direction and consider the evolutionary pressures that have brought us to our present state and how these forces will likely change.

WHERE WE ARE TODAY

Although the basic federal funding model has persisted for several decades, there is increasing dominance by NSF in academic funding, with a shrinking level of support from ONR and a short-term (roughly 10-year) flowering of support by the National Aeronautics and Space Administration (NASA) in the 1990s. This change in the funding portfolio has had subtle, but significant, impacts on the field. As noted by Wunsch (1989), the traditional three-year, competitive grant cycle favored by NSF presents significant obstacles to the development of ocean instrumentation. Such high-risk activities often do not fare well in the peer review system

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*Excerpted from "Oceanography in 2028," originally published in *Oceanography*, Vol. 21, No. 3.

[†] College of Oceanic and Atmospheric Sciences, Oregon State University

because the outcomes are less certain than research-focused proposals. Sustaining long time series is another challenge within the NSF funding environment. Eventually, such programs seem more akin to "monitoring" and more appropriate for a mission agency such as the National Oceanic and Atmospheric Administration (NOAA).

FORCES FOR CHANGE

Change in the nature of science questions is a hallmark of all science, not just oceanography. Forty years ago, physical oceanographic research focused on issues relevant to military concerns, such as ocean mixing, sound scattering, and mesoscale eddies, which would affect the ability to detect submarines. But tomorrow's concerns and challenges will require a far higher level of integration of science across the Earth system, including the oft-neglected human dimension. Our community has adapted in the past and will in the future to the changing nature of the science, the issues of importance to society, and the analytical and observing tools that are available. But, have we become ossified to the extent that it is difficult to create new institutions and organizations and nearly impossible to eliminate old ones? Have our research institutions (and even our culture) become so deeply invested in their present structures that they will fail before they recognize and respond to a changing world? Or, will our response to these changes lose sight of our underlying purpose and values?

More oceanographers are chasing after funds that are unlikely to grow at a rate that will even satisfy academic population growth (which is about 10% per year for new Ph.D.s in ocean sciences). With universities placing ever-increasing importance on grants and publications for promotion decisions, the field will experience an even harsher competitive environment. Universities will not be able to accommodate their needs even with a growing federal budget, and they will begin to pursue new sources of revenue such as state governments, corporations, private foundations, and even foreign governments. Unconstrained, high-risk research will be even more of a rarity, although the inherent conservativeness of the peer review system has already limited the success of such proposals (Braben 2008).

In the area of cyberinfrastructure (CI), there have been several important shifts over the last decade. First, the near-ubiquitous deployment of a range of networks linking a wide variety of devices has transformed our model of a personal computer (PC) or a computer terminal linked to a mainframe into something much more dynamic and transitory. If research can be conducted effectively by accessing networked resources (e.g., sensors, digital libraries, collaborators), then how can universities justify their indirect cost structures if faculty (or individual, nonaffiliated scientists) are working outside the university? Second, nearly every component of CI has moved into the commodity marketplace. Users must know how to evaluate and integrate these updated technologies; there will be little pre- or post-sale support. The rapid innovation cycle will stress both academics and funding agencies. Third, the traditional balances of control and authority have shifted. The relatively slow, well-defined process of data collection, analysis, manuscript preparation, peer review, and publication has been transformed, not simply replaced, with an online process.

A NEW WORLD FOR THE OCEAN SCIENCES?

Three forces will change the environment for our field over the next 20 years. First, federal funding will not grow at a rate sufficient to accommodate our needs or even the growing number of oceanography faculty. Second, even if new sources of funding become available, they will bring new expectations and new requirements. Third, the interconnected forces of globalization and CI open up new opportunities as well as challenges for our institutions and culture.

Budget and political pressures will lead to a restructuring of our institutions by 2028. The smaller oceanography programs will likely continue much as they are today, focused primarily on teaching with some summer salary for faculty research. Some of the mid-size and very large programs will be absorbed into larger schools and colleges that will emphasize basic science education at the undergraduate level, and these education programs will look different from today's discipline-based majors. A small number of the very large programs will persist largely unchanged although their programs will be under continuing and increasing financial pressure.

Some will develop new business models, perhaps with university faculty running all of their grants through the private company but retaining a tenure-based "safety net" at a university for teaching. Such new, nonacademic organizations may bring much-needed flexibility compared to the traditional, discipline-bound and individual-focused academic promotion and tenure process. These organizations might even be more appealing to young scientists than the harsh competition and insecurity of a tenure track faculty position. By 2028, I expect that there will be far more corporate interest in some types of ocean research at academic institutions, not just scientific consulting firms. Along with expanded interests in ocean resources (e.g., open ocean aquaculture, deep sea resource extraction, wave energy), new opportunities will appear, such as iron fertilization as a carbon offset and phytoplankton as an energy source. Philanthropic support will also increase, but most of these new funds will be targeted towards specific research programs or advocacy and education programs.

As the existing oceanographic programs and institutions consolidate, restructure, or transform themselves, there will be significant impacts on the research fleet. We are on a trajectory today where there will be significantly fewer ship days available; there will be increasing emphasis by the agencies to further reduce operating costs. Observing systems will become more standardized, with a greater reliance on gliders, cabled observatories, and other autonomous systems that need much less ship support. Although this approach clearly has many advantages, it does represent a fundamental shift from scientists who develop their own tools and approaches to scientists who are "consumers" of standard data products. While, at some level, this shift to science consumers is a good thing, it does have a set of potentially negative impacts. For example, who will develop the next generation of science instruments? Are we training students in the appropriate way to balance the need to create new techniques and approaches with the need to be able to use standardized tools and data? Will CI that is developed to meet the needs of consumers and the entertainment industry continue to meet the needs of the scientific community? Will the sense of community engendered through the use of shared facilities be disrupted as the costs of these facilities compete for funds at the expense of support for principal investigators?

Here are five actions that we could begin now to take control of our future:

- We need to move beyond the traditional discipline-based model of oceanography graduate education (which grew largely out of the Sverdrup, Johnson, and Fleming textbook [1942]) and infuse oceanography into an undergraduate science curriculum that relies on hands-on research experience. Oceanography could serve as a framework for teaching the fundamental sciences (physics, chemistry, and biology) and mathematics.
- We need to develop new business models for oceanographic research that are not frozen in the present structures of tenure-based academic institutions or purely soft-money research businesses. Broadly based, interdisciplinary research teams do not fit comfortably within the rigid department-based environment of tenure, but they do need long-term stability and persistence to enable high-risk science that is often lacking in consulting firms driven by short-term needs for profitability.
- We need to engage the funding agencies as well as the private sector to develop new instrumentation that leverages modern design tools and capabilities, rather than simply tweaking the

approaches of the past 30 years. The ocean is a harsh environment, but this is not an excuse for continuing to rely on traditional instrument designs.

- We must work as a community to develop principles and establish processes that can balance the needs of community-based facilities and individual and team-based science. The only mechanism in place now is the constraint of funding availability.
- We must position ocean research and education within a larger context of the Earth as a system. This construct does not mean abandoning the unique vision and capabilities of oceanography. We must demonstrate both our willingness to understand the larger environmental issues facing society and our ability to inform decision-making with the best science.

We will experience as many changes over the next 20 years as we have over the past two decades. Who could have foreseen the dramatic decline in sea-going oceanography just as the World Ocean Circulation Experiment (WOCE) and Joint Global Ocean Flux Study (JGOFS) were beginning in the mid-1980s or that an Earth observing system, which was in the midst of coming to fruition in the mid-1990s, would now be on the cusp of an "observational collapse" (NRC 2007)? The changes in the next 20 years will be more profound and more uncertain. How we as individual oceanographers, as individual institutions, and as a community choose to recognize and respond to these changes will set the course for our field for many decades.

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The Changing Relationship Between Humans and the Ocean

*J. G. Bellingham**

INTRODUCTION

The coming century will see an explosion of activity in the ocean as terrestrial resources are depleted and advanced technologies drop the cost of ocean access. The growing importance of the ocean to the global economy will simultaneously create a demand for improved understanding of the ocean, provide resources for developing new technologies, and will create a more complex political landscape for addressing ocean issues. Developments we might anticipate include:

- The ocean will play a growing role as a source of renewable energy
    - Wind farms will be increasingly placed offshore as winds over the ocean are higher than over land
    - Wave energy generation will be a useful source of power for some regions
    - Most solar radiation falls on the ocean
- The depletion of fisheries will continue to drive the growth of aquaculture, making far more effective use of the ocean for production of food, but also creating serious environmental risks.
- The advent of carbon markets in some form will create enormous economic incentives to engage in industrial-scale activity to sequester carbon in the ocean.

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*Monterey Bay Aquarium Research Institute

- The largest remaining oil and gas discoveries will occur in the deep ocean, and this domain will become increasingly important as a source of oil.
- Mining of metal ores from the seafloor, already attracting substantial private investment, may become an important source of resources for developing countries.
- Transportation of goods by sea may be transformed as climate changes modify trade routes (opening of the Northeast Passage, increasing severity of weather).

Beyond economic considerations, the security of our country will be directly dependent on the ocean. Some examples of security issues of the future include:

- Abrupt climate change involving the ocean could cause serious disruption to economies of both developed and developing countries, creating the potential for political instability. While the probability is hopefully low, the damage could be catastrophic and global, and abrupt climate change must therefore be taken seriously.
- The competition for ocean resources could be a catalyst for conflict.
- The growth of industrial activity at sea will create a need to protect critical United States (U.S.) infrastructure in comparatively remote regions of the world ocean.
- As many nations gain access to advanced submarines, antisubmarine warfare will become an important naval capability again.
- Asymmetric threats will multiply as mines and mobile autonomous platforms inhibit and/or deny access to critical waterways and threaten U.S. interests.

While it is a truism that in many respects we know less about the bottom of the ocean than we do about the far side of the moon, it is also true that today few taxpayers care about that discrepancy. This apathy towards the state of the ocean will need to change.

OCEAN SCIENCE ON A FRAGILE PLANET

The ocean sciences are likely to be increasingly driven by the need to understand the ocean’s role in shaping global climate. Science has identified environmental risks which could have catastrophic consequences for the world and for the U.S., and many of these risks are oceanic in nature (e.g., thermohaline circulation modification, sea level rise, severe storms, ocean acidification, depletion of fisheries, melting sea ice, etc.). At present, our ability to evaluate the probability of these risks is poor, which is largely due to the lack of intensive programs to understand the ocean's role in climate and to put in place monitoring systems with advance warning capability.

TECHNOLOGY AS ENABLER

Technical advances on a number of fronts promise to dramatically improve our ability to work in and on the ocean. However, progress is slow, and, at present, there is no concerted national effort—other than perhaps for satellite systems and some specific military needs—to develop ocean technologies that address existing and emerging societal needs.

Techniques for precise identification of species in the laboratory and detection of organisms in the field will be developed. This includes both genomic methods and other techniques which use the morphology, optical, and/or acoustic characteristics of organisms. Methods to measure the state of organisms (e.g., photosynthetic efficiency) will be increasingly important as we attempt to characterize the rates of change of key biological indicators.

Improved sensors for directly measuring chemical properties of the ocean will become available for key nutrients and tracers. As these systems become smaller and consume less power, they will enable a much more detailed understanding of ocean processes on small space and time scales.

Robotic platforms which conduct observations and simple tasks with little or no human supervision are being rapidly adopted. However, these systems are mostly 'first generation' platforms and much greater capability is possible. Over the next decade, new and more capable platforms will be introduced. Infrastructure for delivering power and communication to remote instruments and platforms in the ocean interior will enable a continuous, interactive presence in remote locations.

Tools for managing, exploring, and accessing data which allow sophisticated analysis of observations and the development of predictive systems will enable cross-disciplinary research. Physic-based models will become increasingly sophisticated, testable, and, at the same time, more accessible.

However, to achieve these advances efficiently and rapidly, more effective funding mechanisms are needed for ocean-science-driven technology and engineering activity.

Societal Implications for Ocean Research in 2025

*Matthew Alford**

The past few decades of ocean research have moved our field from a young, exploratory science to a mature one. This transformation has been heralded by tremendous advances both in our understanding of the ocean and our ability to observe it. An extreme believer would even argue that the degree to which we now understand the ocean-atmosphere system has saved our planet, by enabling observations that detected climate change and bolstered a coherent argument for its anthropogenic origin.

2025 will be a different world than today—one with a much greater population and squarely in a post-peak oil era. The global economy by that time will be profoundly affected, and possibly driven, by carbon, food supply and energy issues. All nations will fully appreciate by then the reality and dangers of climate change. Most will resist changing business practices and energy policy for purely altruistic or environmental reasons. However, by then nations and businesses will be required to mitigate their carbon emissions by either using renewable energy, purchasing carbon credits, or through sequestration techniques.

The ocean will sit prominently in the limelight in this not-too-distant-future economy for several reasons:

- A much greater portion of our energy will come from offshore. While I suspect that wave and tidal energy will not pan out to be significant sources of energy, offshore wind/solar farms and algal biodiesel harvesting have much greater promise, but will present significant ocean engineering challenges.

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*Applied Physics Laboratory, University of Washington

- Likewise, the increasing role of wild and farmed fish for feeding the Earth's growing population will necessitate greater and greater understanding of ecosystems and their interaction with the changing physical environment.
- More and more people will continue to live near the sea, increasing the economic and societal impact on humans of increased storms and higher sea levels associated with a warmer climate. Prediction of these will elevate ocean research and regional monitoring to ever-higher importance. Regional prediction of ocean states will continue to be a vital part of the U.S. Navy's operations.

Improved prediction will also be vital on the global scale, since evaluating the efficacy of various carbon mitigation strategies will have real economic impacts. That is, ocean models will be used by governments on a daily basis to determine the dollar amount of carbon credits, as well as by companies to determine the most economical way of proceeding (e.g., purchase green power versus paying carbon taxes).

The ocean plays a central role in each of these themes; hence, funding for ocean research will be significantly greater than it is today. Showing here my natural tendency for audacious hope, I predict that NSF will have a much larger budget than today—possibly obtaining the fabled doubling that has been spoken of for some time. ONR's future is difficult to forecast, but it seems certain that private donors (foundations and companies) will fund much more ocean research than today, owing to its elevated and more tangible and immediate importance to the tangible economy. This will include funding for both ocean technology and ocean engineering, as well as basic research to improve models. These increases in funding will allow much more research per year; however, owing to the growing number of oceanographers being trained now (a 10% per year increase; Abbott 2008) that will be seeking jobs then, the funding climate for each principal investigator (PI) may not become easier than today; and possibly the opposite. Indeed, this demographic trend has the potential to create grave problems for our field without this increased funding.

Much of the research will necessarily take the form of "monitoring," in the form of coastal observing systems such as those part of the Integrated Ocean Observing Systems (IOOS or OOSs), global drifting arrays such as the Argo array, and open ocean buoys and regional cabled systems such as those in the Ocean Observing Initiative (OOI). These assets, particularly Argo with its global coverage and good spatial resolution, will be recognized as vital time series and expanded. Hence, many will analyze data and contribute greatly to our understanding of ocean processes. Given the increased number of researchers just mentioned, this is a good coincidence. Fewer will go to sea, but many will be able to address exciting and difficult questions on the global scale with these data.

In addition to this, there must continue to be a strong cadre of seagoing oceanographers. We as a community must continue to realize the value of this work, which though it is certainly more expensive per PI, is necessary to the success of the other efforts. As models improve, there will always be new physics discovered as parameterizations are tested more rigorously. Creative and exploratory seagoing efforts will be required to explore these processes. These are expensive and often risky operations, often requiring the development of new instrumentation. For these investigators to have the expertise to develop nonstandard observational tools (besides moorings, acoustic Doppler current profilers [ADCPs], and conductivity-temperature-depth sensors [CTDs]) that may someday become the new observational workhorses, we must find a way to continue to fund high-risk instrument development projects—traditionally funded by ONR and more difficult to get funded by NSF. Perhaps ONR's downward trend will reverse, or alternatively private donors will rise to the challenge?

Specific problems likely to be still extant in 2025 include the need to understand processes with nested scales. For example, fronts are a persistent area requiring better understanding, as they are the loci of many poorly understood physical, chemical and biological processes. Yet frontal research will continue to be plagued by the difficulty of simultaneously resolving the smallest and largest scales with a single ship. Perhaps fleets of gliders, or even small autonomous vessels capable of deploying towed instruments and ADCPs, that are deployable from research ships will be developed.

As a second example, unraveling the tough problems that really affect ecosystems will require more collaboration with biological, chemical and physical oceanographers. The advances being made in biological oceanography with genetic techniques are astounding—soon maps of species and even population abundance will be possible using *in situ* sequencing. The physical environment must play a central role in these distributions; yet, close interaction across fields will be required for progress.

Though some changes will be sweeping, oceanography will be recognizable in 2025. The eternal optimist, I believe it will be an exciting time, provided we can stay true to our shipboard roots while embracing new autonomous technologies. Maintaining a healthy balance between monitoring (routine but vital for long time series) and exciting new technologies and experiments will be key to our success.

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Oceanography in 2025: Responding to Growing Populations on a Rapidly Changing Planet

*Scott Glenn**

APPROACH

Where will our field of physical oceanography be in 2025? As an ocean forecaster, I find comfort starting with observations of where our field is today, determining what is changing, and what is staying the same. Next, I look at the forcing functions, specifically what will be required of us, and what new technologies will enable us to meet these needs. Then, based on this information, I try to project forward.

WHAT IS STAYING THE SAME?

- The ocean is still multiscale and complex, as well as a difficult place to work. Observing, understanding and modeling the cascade of time and space scales will remain a challenge.
- Despite significant investments in ocean science, applications and observational infrastructure, the ocean is and will remain vastly undersampled. Our prized glimpses of the well-sampled times and locations will continue to inform model development.
- Models will continue to improve, growing more complex and interdisciplinary as computational capabilities continue to increase.
- Even with 30 years of satellite oceanography and more recent advances in remote and autonomous systems, oceanographers

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*Rutgers University

still go to sea on ships and below the surface in submersibles. This continues despite the increase in operating cost.

- Somehow we will continue to find clever ways to cobble together support to maintain critical time series datasets.

WHAT CHANGES ARE OBSERVED?

- Coastal ocean observatories are now a sustainable reality leveraging multi-agency support. NOAA is constructing a National High Frequency (HF) Radar network. Gliders are being transitioned into the operational Navy. U.S. IOOS has formed 11 Regional Associations. The NSF OOI includes a coastal component. Department of Homeland Security (DHS) Centers of Excellence are leveraging these resources. Energy companies are contributing to accelerate both scientific discovery and operational support.
- High-resolution nested atmospheric forecasts are an operational reality. A growing ensemble is run every day by academic, industry and government agencies in many regions around the country. The locally generated regional forecasts enhance the coarser resolution global and national forecasts.
- Physical ocean forecast models now work. There is still plenty of room for improvement and operational hurdles to overcome, but they do provide guidance when the dedicated effort can be applied.
- Coupling of biological, chemical, and geological models to the physical oceanographic models is accelerating. This is enhanced by the use of physical circulation models and new observations to separate observed changes into physical transports and biological, chemical, or geological transformations.
- The growth of interdisciplinary science. More funding is going to interdisciplinary science teams. More multi-author papers are being published. More papers acknowledge multiple funding agencies.
- The emergence of campaign-style science. We often go to sea on single ships. We now see an increasing trend to also go to sea in coordinated fleets.
- Interest in observatory datasets, scientific understanding, and the resulting predictive models has grown well beyond the scientific community. The same data and forecasts used by the scientists benefit government agencies and private industry. The reverse is also true, resulting in a broader funding base.
- Oceanography is becoming more integrated with other disciplines. This is enabled by some oceanographic institutions

being purposely located on the main campus of larger research universities.

- While marine education is still dominated by the graduate schools, undergraduate education, once vehemently opposed by many oceanographers, is a growing reality. The need to promote a more ocean-literate public and invigorate the K-12 pipeline has been recognized.

WHAT ARE SOME OF TODAY'S GLOBAL DRIVERS?

- The growing global human population will reach about eight billion by 2025, with most of the growth in less developed countries. Growth and migration result in the largest increases in coastal regions. Growing populations require more food, water and energy.
- Less developed coastal countries are more reliant on fisheries than the well-developed coastal countries. Many of our world fisheries are already at capacity, but pressure for food for people and feed for domesticated animals grows. Aquaculture is growing to fill some of the gap.
- Energy need is synonymous with climate change. The need to reduce carbon inputs to the atmosphere while meeting the increasing global energy needs will require the full range of responses, including greener energy sources and new approaches to carbon sequestration in the ocean.
- While coastal populations grow, so do urbanized centers. The local effect of megacities with human and industrial outputs, high volume ports for goods and energy, and highly impacted fisheries will require research on urbanized watersheds, estuaries and coasts for more informed management. Research in complex, heavily trafficked coastal regions is required in the U.S. and exportable to other countries.

WHAT ARE SOME OF THE KEY ADVANCES THAT WILL ENABLE THE NEXT GROWTH STEPS?

- Ocean remote sensing will continue to make advances. Key is the development of new algorithms for ocean color, new active radar satellite sensors, and sustained coastal HF radar networks. Like ships of opportunity, inexpensive remote sensing systems will be deployed on aircraft of opportunity.
- Mobile autonomous platforms are fundamentally transforming our ability to sample the subsurface ocean. They can be deployed

for sustained operations well beyond the endurance of ships, are cheap enough to be assembled and flown in coordinated fleets or swarms, are rugged enough to sample through severe storms, and are unmanned, so they can be deployed on riskier missions in extreme environments. They will continue to evolve, and become more complex in their options and capabilities for propulsion, energy utilization, communications, sensor payloads, and automated control.

- Communications are key to adaptive sampling, assimilation, and the development of collaborative communities. The internet on land, improved two-way global satellite communications, broader shore-based cell phone networks, and research on underwater communications will continue to develop into a ubiquitous communication grid.
- Autonomous platforms require sustained power to remain effective. Battery technology, driven by broader needs than oceanographic, will produce safer, higher energy density batteries, both primary and rechargeable. Research on power harvesting systems will continue. Offshore wind farms are excellent sampling and communication platforms. Wave energy systems are already being built and deployed.
- New biological, chemical and sediment particle sensors will introduce new capabilities that can be miniaturized, automated, and made more energy efficient for deployments on autonomous platforms.
- Platform control systems. Control of autonomous platforms is still very rudimentary, and in all but a few cases still includes a human in the loop. As we increase the number of platforms and sensors, new automated control software will be developed that increases the platform's ability to make decisions on its own, to operate as a coordinated swarm, and to interact with datasets and people on shore.
- Data visualization. The ability to visualize multivariate datasets and model output by a distributed community for scientific analysis, adaptive sampling and decision-making will evolve.

WHAT WILL THE FUTURE LOOK LIKE BASED ON THESE EVOLVING NEEDS AND CAPABILITIES?

- The need for ocean research will remain strong, including the impacts of climate change as it progresses, research on green energy generation, carbon sequestration or geoengineering to reduce the rate of climate change, research on sustainable fisheries and aquaculture to provide food, and research on the balanced management of urbanized coasts. While the need for broad interdisciplinary research will increase, proceed without fear. Even though other ways to succeed in academic ocean science beyond the present disciplinary model will develop, the need for single discipline PI research will remain.

- Satellites were the transformational technology of the 1980s, changing the way we viewed the ocean. Ocean observatories are the transformational technologies of the 2000s. By the 2020s, interdisciplinary transport and transformation models will be the transformational technology. Specific development projects will focus on improved parameterizations for the unresolved mixing scales, and the interdisciplinary ocean model components, of the coupled atmosphere-ocean-land models. And, as with the other transformational technologies, you will no longer need to be an ocean modeler to use ocean models.
- We will build new scientific alliances that go beyond institutional walls to accomplish greater goals. Coastal alliances will form to cover the spatial scales of the world's many Large Marine Ecosystems. Global alliances will form to address problems in remote and extreme environments such as the poles and the southern oceans. The teams that form will likely be assembled in the continuing spirit of the National Ocean Partnership Program (NOPP), pulling from the greater pool of researchers.
- The number of individuals in oceanography and breadth of their experience will grow. The definition of an oceanographer is already different for today's students than it was for us. Today's students have a wider variety of degrees and careers to choose from as they pursue their interest in ocean science.

Some Thoughts on Physical Oceanography in 2025

*Ken Melville**

Projecting forward to 2025 is not so large a leap: 16 years, perhaps 40% of a typical post-doctoral working career, three Ph.D.s, a few El Niño-Southern Oscillation (ENSO) cycles and a few Intergovernmental Panel on Climate Change (IPCC) assessment reports. Looking back to 1993, could we have guessed where we would be today?

For physical oceanography, some of the technological advances that have revolutionized the field in the intervening period have turned out to be: radar altimetry from the TOPEX/Poseidon mission launched in 1992; profiling floats that became operational in the early 1990s and now constitute the 3000-float global Argo system; gliders that became operational almost a decade ago and now are about to be mass produced; computational power that has permitted ever more realistic global physical models while also permitting ever higher resolution for local process studies. All of the elements of these technological advances, and others one could cite, were in place in the early 1990s.

By and large it has been the physical oceanography (PO) community that has led the way in developing autonomous platforms for measuring the traditional variables in the ocean: currents, temperature and salinity. It is to be expected that extreme weather (e.g., hurricanes, Southern Ocean), climate variability, marine ecosystems and Navy needs will remain strong drivers of oceanography over the coming decades. As detailed comparisons of numerical models and observations move from the mesoscale

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*Scripps Institution of Oceanography, University of California, San Diego

to submesoscale to microscale, it is inevitable that the need for a better understanding of coupled physical, biological and chemical processes will be needed. Interpretation of chemical and biological variability in the ocean will require a foundation in physical oceanography.

For example, ocean acidification and its impact on marine organisms is not just an issue for chemical and biological oceanographers, but also for physical oceanographers, since the air-sea fluxes of CO_2, which are poorly understood, depend on physical processes in the lower atmosphere, the upper ocean and at the air-sea interface. Implicit in current parameterizations of gas transfer velocities based on just the wind speed is the assumption that the turbulence that supports the fluxes depends only on the wind speed. This assumption is untenable in non-stationary wave, current and turbulence fields. Since bubble-mediated gas transfer may be significant, acoustical oceanographers may also be expected to play a role in developing improved air-sea flux measurements and models.

The inference to be drawn from this example, and there are others, is that the capability of platforms and instrumentation will have to be expanded to include small, low power, low maintenance chemical, biological and acoustic sensors along with the payload and power to support them. Recent attempts have been made in this direction with bio-optical and carbon chemistry sensors on profiling floats or gliders, but much more needs to be done so that the Argo array and perhaps glider arrays can expand their global capability into these areas.

With the success of the TOPEX/Poseidon mission, plans are now underway at NASA to develop higher resolution radar altimetry that will resolve sea surface height (SSH), ocean winds and wave measurements down to the submesoscale. While this would permit snapshots of submesoscale processes, satellite altimetry is still limited by the O(10)-day repeat cycle, which may be much longer that the time over which these processes evolve. This and other aspects of orbital remote sensing highlight the need to supplement the global coverage of satellite remote sensing with suborbital or airborne remote sensing capabilities for submesoscale process studies over shorter timescales.

Access by the oceanographic community to research aircraft is very limited, with few aircraft and funding a more explicit consideration than it is with getting access to UNOLS (University-National Oceanographic Laboratory System) vessels. For process studies, radar or Light Detection and Ranging (LIDAR) altimetry and related measurements can be made from aircraft. For air-sea flux studies in the lowest layers of the atmosphere, manned flight at O(10) m above the surface is risky in all but the most benign conditions and this points to the need for Unmanned Aircraft Systems (UASs) to undertake such measurements. For example, with the technical difficulties of deploying and maintaining air-sea flux moorings in the Southern Ocean, and with the need to provide areal as well as temporal coverage, UASs could play an important role there, as they are beginning to do in hurricane research. Ship-launched and recovered UASs would significantly enhance the capabilities of research vessels. Ship-based UASs have already been used for marine geomagnetic measurements.

Another potentially important role for aircraft, whether manned or unmanned, is in the air-deployment of instruments. Traditionally, airborne expendable bathythermographs (AXBTs) or Global Positioning System (GPS) dropsondes have been deployed, but smaller shorter-lived air-deployable profiling floats or gliders with physical, biological and chemical sensors would significantly complement the on-board remote sensing capabilities of the aircraft.

In the field of air-sea interaction, we do not have a good understanding of surface-wave processes. However, we know that the sources of turbulence and mixing on both sides of the surface are dependent on the wave field. Increasingly, and not surprisingly, it is being found that air-sea fluxes of mass, momentum and energy depend on the wave field, effects that were hidden in the large scatter of earlier measurements. However, with our increasing capability to undertake field measurements that approach laboratory quality, and with the constraints that the basic conservation laws impose, there is reason for optimism about the progress to be made by 2025. It is also the case that the synergy between Large Eddy Simulation (LES) numerical models and measurements will propel the field forward at an accelerating pace. The results of this research will ultimately lead to an improved understanding of air-sea fluxes with this understanding finding its way into improved subgrid-scale closures for coupled atmosphere-ocean models, and climate models.

Last year marked the 50th anniversary of the CO_2 record (The Keeling Curve) at Moana Loa, and this year will mark the 60th anniversary of the California Cooperative Oceanic Fisheries Investigations (CalCOFI). The value of long continuous time series in climate research is no longer in dispute, but their maintenance over 50 years or more has from time to time required great tenacity and fortitude on the part of individuals and institutions to maintain the flow of funding in the face of competing priorities and budget shortfalls. If we are to understand climate variability, the time scale over which these observations need to be maintained stretches indefinitely into the future, and we need to find a better way of making sure it happens. We need planning and funding on the decadal and longer time scales to match those of the climate itself.

While the capability of numerical models has expanded significantly in recent years, we must avoid the mistake of assuming that those advances can be maintained without new and improved data to better understand the processes being simulated, and to test and improve the models. In a time of increasing pressure on research budgets, the temptation to fund more PIs to do modeling rather than fewer PIs to do more expensive instrument development and field work will be great. This issue is also reflected in our educational programs, where more expensive lab classes have tended to decline as educational budgets have come under pressure.

There is a larger educational question, and that is how do we attract and educate the coming generations of oceanographers so they have the motivation and skills to make the discoveries needed for us to better understand the ocean and its impact on society? The fresh postdoc in 2025 is about 11 years old now. How do we convince a middle schooler that our science is fun? How do we ensure that the high schooler and undergraduate see oceanography as a career option and ultimately get the science background needed for graduate school? And how in graduate school do we provide breadth across the subdisciplines of oceanography, while providing depth in specific areas? I do not think we will solve these problems in two days, but I do think we should begin to address them, both at the institutional level and as a community.

The Next-Generation Coupled Atmosphere-Wave-Ocean-Ice-Land Models for Ocean Research and Prediction

*Shuyi S. Chen**

VISION

Natural science in coming decades is likely to focus on better understanding and protecting the global environment and resources, which require a fully integrated multidisciplinary approach. Oceanography is and will continue to be a key component of the fully integrated global climate system.

In 2025, oceanography will no longer be viewed as "elite" toys, but rather a "utility" for public use. Decision making that matters to public and government operations will be linked directly to ocean and marine weather forecasts from hours to weeks. It will have added value to asset allocation and risk assessment and management.

The ocean and atmosphere will be viewed as a fully coupled system. Coupling between the atmosphere and ocean through surface waves at the air-sea interface and coupling between the physical and biological/chemical processes in the ocean will be significantly advanced in terms of understanding and numerical modeling. The high-resolution, fully coupled model prediction of the ocean eddies, fronts, and sea state will be accurate enough for practical usage. The coupled global ocean-atmosphere models will be capable of climate prediction with reduced uncertainty.

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*Rosenstiel School of Marine and Atmospheric Sciences, University of Miami

CHALLENGES AHEAD

- Innovative and untraditional approaches will meet resistance and need time to mature.
- Basic understanding of physical processes at the air-sea interface, especially the role of surface waves in the air-sea fluxes, may be limited by the lack of observations, especially in extreme conditions such as winter storms and tropical cyclones.
- Computer models will continue to advance with increased grid resolution (~1 km or less) and better model physics. However, observations of high spatial and temporal resolution will not be possible in the foreseeable future. The gap between the computer models and observations will be a major challenge for evaluating and validating coupled model predictions.
- Assessing and understanding uncertainties in the ocean prediction will be a challenge for both research and educating general public and stakeholders.

A WAY FORWARD

Educating and training of the new generation of scientists to have not only a solid physics/mathematics background but also a broad knowledge of the ocean-atmosphere system that is different from the traditional oceanography or atmospheric science. We need innovative colloquium development and new educational programs/systems to foster the new ways of thinking.

Rapid development in computational science and computing power will continue in the next 10-20 years. We need to take advantage of the new technology to produce the most advanced ocean prediction and data assimilation system that is capable of providing the level of detailed description of the ocean, which can help us to understand the system in a way we may not even be able to imagine at present time.

We need to develop a system to ensure a smooth and seamless transfer of knowledge and technology from research to operations in a timely manner. This may require a major culture change in both the current research and operational communities. It takes leadership and resources to encourage and support such activities.

Science in Action, Episode 1: Exploring Boundaries

*Meghan F. Cronin**

PEAK OIL IN A WARMING WORLD

In 2025, oil limitation and climate change will reshape every aspect of our lives including oceanography. In addition, oceanography will be reshaped by the collapse of fisheries due to ocean acidification and overfishing. New technologies will emerge for harvesting energy and observing the oceans and numerical models that resolve eddies and fronts will become powerful enough to run for centuries.

TEAMWORK IN 2025

Oil limitation and recognition of the influence of anthropogenic CO_2 on climate will limit usage of ships for oceanographic research. As a consequence, research will become highly leveraged. Every available platform (ships, buoys, drifters, floats, gliders . . .) will be multitasked to carry a suite of miniaturized sensors. Single PI fieldwork will be a distant memory. Instead, the measurements will be made by a team of scientists and will serve several communities (biogeochemical, ecosystem, physical, meteorological . . .) which had previously been independent. In 2025, the lines between these fields will become blurred and more scientists will become fluent in several disciplines. In particular, after the carbon market is put in place, everyone from Main Street to Wall Street will become versed in the ocean carbon cycle. In 2025, most oceanographic research

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*Seattle, WA

will in some way be directed towards monitoring and better understanding ocean uptake of carbon. The public will be vested and interested in oceanography. Indeed, oceanography will become a bit of a team sport, with the public tuning into "Science in Action": Humans vs. Nature. Who wins?

Although there will still be those who collect and work with "raw" data, most will use gridded products that integrate data from various sources. There will be significant effort and care generating these products, particularly since many of the products will have commercial value due to carbon cap and trade and wave energy harvesting efforts. While many of the products will be generated at national model centers, there will also be products developed at universities, governmental research laboratories, and some of the many new startup private companies focused on geophysical systems. An oceanographer could have a creative and successful career developing, improving, and maintaining these products, and would most likely be a user of the product as well as developer. Most products, but not all, will be freely available. Likewise, most data collected will be made publicly available in near-real time so that it can be ingested into the products.

THE DEVIL IS IN THE DETAILS

In 2001, a curiously sharp front was observed by SeaSoar measurements near the equator at 95°W: the transition from cold tongue to the warm water north of it was compressed into a 1-km wide region. In the intervening years between 2001 and 2025, we will find that fronts are ubiquitous and these "wall-like" fronts are relatively common. As the oceans are probed in ever more detail, more complexity will be observed. The observed and modeled frontal structures will challenge our fundamental understanding of ocean dynamics. Basic principles such as Ekman dynamics will need to be reconsidered for frontal regions. Boundaries will be the new frontier. In 2025, most physical oceanography graduate students will be focused on boundaries caused by fronts, continental margins, and the air-sea interface.

Resolving the frontal structures will pose a challenge to the observing system and numerical models. Some Argo floats will forego their parking depth current measurements in order to provide greater spatial and temporal resolution CTD measurements in frontal regions. Gliders will become a popular tool, with a number of universities hosting centers with "glider pilots." The passage of fronts will be studied in the long, high-resolution time series from fixed moorings. Much of the technologies of late 20th and early 21st century, however, will have been inadequate to observe the structure in these fronts and boundaries. For example, while conventional drifters can observe the near-surface currents, the near-surface shear is not observed. ADCPs mounted on ships and moorings during the late 20th and early 21st centuries almost always had a blank spot in the ~20 m directly below the air-sea interface. Likewise, new technologies will need to be developed to measure turbulent mixing above 20 m. This layer, where active air-sea interaction occurs, will continue to be the focus of considerable research in 2025.

NEW TECHNOLOGY IN 2025

Sensors will become smaller and lower powered, as will our cars. Some high-powered oceanographic sensors will draw power from waves and solar panels. Wave energy will be used for oceanographic measurements, military installations, and will feed into the power grid for island and remote communities including Hawaii and the Aleutian Islands. Due to the expenses associated with ship time, transmission cables, and buoy maintenance, however, it is unlikely that wave energy will grow beyond these niche markets.

Passive acoustic listening (PAL) technology will likely become widely used for meteorological, physical, fishery, and ecosystem studies. PAL rainfall measurements will be the robust *in situ* measurements used to correct biases in satellite rainfall products. There will be considerable effort made to combine the PAL wind, wave, and bubble measurements with buoy hull measurements (e.g., sea surface temperature), in order to eliminate the need for a tower for buoy measurements of air-sea transfers of heat and CO_2. Relative humidity and air temperature will prove to be the most challenging aspect of the flux measurements. Thus while CO_2 flux will be able to be monitored by small towerless buoys, high quality heat flux measurements will still require buoys with towers. These buoys with towers will also allow many other surface and atmospheric boundary layer measurements. Reliance upon wind farms for the general power grid will lead to rapid advancements in atmospheric boundary layer technology. Flashlight-sized LIDAR systems, which measure cloud structure and wind profile with a bottom bin as low as 10 m, will become part of the standard suite of meteorological sensors for buoys and voluntary observing ships (VOS). Golfers, mountain climbers, and sailors will impress their friends and competitors with these gadgets sold at outdoor enthusiast stores.

Real Time Decision Support Everywhere

*Nathaniel G. Plant**

ASSESSMENT OF OCEANOGRAPHY IN 2025

Oceanography has reached a mature stage wherein we have reliable observation, modeling, and forecasting capabilities. In the next decade, we can take advantage of these capabilities to advance both scientific discovery and the societal impact of oceanographic research. Exciting scientific discoveries can be maximized by expanding the number of opportunities to compare predictions to observations: observations differ significantly from expectations when unknown processes or interactions of processes are important. The societal impact of oceanography can be maximized by increasing the availability of oceanographic knowledge to human decision-making processes. Since oceanography encompasses a wide range of temporal and spatial scales, there is a very broad "audience" for this knowledge. For instance (focusing on coastal oceanography), forecasts of wind, waves, and storm surges that impact coastal areas are already incorporated in planning decisions regarding short-term evacuations and longer-term land use. Forecasts of climate-change scenarios are now included in longer-term planning discussions.

Other opportunities exist for oceanography to become a utility for daily decision making. An approach to exploiting these opportunities is through quantitatively connecting oceanographic processes with many other interrelated processes. The goal is to increase the number of opportunities to answer, "How is ocean process/variable X related to societal

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*Center for Coastal & Watershed Studies, United States Geological Survey process/variable Y?" For instance, coastal water quality (i.e., sediment or nutrient load) is related to river influx. Forecasts of coastal water quality can be made using forecasts of weather (rainfall, temperature) as well as sources (amount of fertilizer sold at hardware stores?). This example is meant to suggest that oceanographers can utilize widely available, non-traditional data. Again, comparison of predictions and observations—even if they include variables that are outside the set that is typical of oceanography—will improve our understanding of predictive capabilities, increase societal impact of applied oceanography, and lead to discovery of new interactions and mechanisms.

A number of challenges must be overcome in order for oceanography to reach its potential in 2025. Simultaneous prediction and observation of a diverse set of variables is required. Where are the computing platforms and sensors to support this? We live in an age of smart technology where observation, computing, and decision-making are co-located. The iPhone™ processes real time signals (from a GPS) and user objectives ("where is location Q?") to provide decision guidance ("Turn left!"). What are the oceanographic analogues to this operational scenario? Autonomous vehicles apply this strategy by adapting the sampling mission to a real time analysis of the environment. Perhaps fathometers on commercial and recreational boats will sample wind, wave, depth, and temperature data that will support real time updates of environmental models. These updates will be used to make decisions. Will a fisherman's fathometer be used to evaluate impacts of global warming?

Other challenges will arise from the need to guide the responsible use of detailed and accurate oceanographic information. For example, we must usefully convey uncertainty when answering specific questions such as, "Will my house be destroyed by a hurricane?" We will need to address uncertainty with increased spatial and temporal resolution. Furthermore, attaining an ability to address important societal questions will generate new conflicts of interest and moral dilemmas. For instance, if model predictions of fish populations were 95% skillful, how do we ensure that this information is used responsibly?

Sampling the ocean remains a challenge. Continuous data input and model updating can be used to determine where more information is needed to test particular hypotheses or to address applications. When operational data-assimilation programs are in place, we gain information to determine the value of observational resources and can improve deployment strategies for both research and applications. There will be continued competition for these resources, but the data-assimilation formalism exists to distribute them equitably.

Finally, who will participate in the research? It will be conducted by academic institutions, industry, federal agencies, and individuals. Common communication interfaces are needed (such as Google Ocean™). Will these interfaces self-organize or have central organization? Will they support oceanographic applications that minimize the adverse impacts of climate change and major hurricanes? Will these communication tools support the education and efforts of the next generations of oceanographers? I hope so, because my prediction for 2025 is that nearly everyone will be an oceanographer!

CONCLUSIONS FROM THE WORKSHOP

The section above is an evaluation of the state of oceanography now and a vision for how oceanography will evolve in the next decade. The evaluation and vision were used to initiate discussions during the "Oceanography in 2025" workshop. Here, I summarize my conclusions from some of these discussions. Starting with the white papers, it was possible to identify five approaches to drive, motivate, and rationalize future investment in oceanography.

- We could argue that oceanography should be more focused on solving applied problems. This strategy is easy to support (see next paragraph).
- We could invest in increased observational capabilities. This is justifiable by itself only if there are important new phenomena to be discovered. Biology has a strong case.
- We could focus effort on understanding specific oceanographic processes. This is justified where improved understanding of a small set of processes has broad impact on applied problems or can lead to breakthroughs in understanding other processes (e.g., interaction of migrating organisms with acoustics).
- We could focus investment in developing oceanographic technology. This approach is hard to argue on its own (build it and they will come?) but it is certainly a key component of future oceanographic investment.
- We could emphasize exploitation of numerical models. This is defended by arguing that past investment has given us skillful capabilities that can and should be used more widely. All of these approaches will be used in the future. However, it may be necessary to choose one to be a primary driver that is used to organize and support the need for other components.

A primary conclusion from the workshop was that an increased focus on applied problems is necessary and inevitable for oceanography. Some pitfalls exist along this path. For example, conflicts of interest may result when major policy decisions depend on interpretations of uncertain scientific knowledge. An increase in applied oceanography will require resources to be spent to support operational activities—perhaps at the expense of scientific missions. And, more effort will be required to communicate applied problems to researchers (RADM Titley attempted this at the workshop, Figure 1). It is likely that the pitfalls associated with increasing applied emphasis will be outweighed by benefits. For instance, we can expect that increased applied efforts would lead to overall increases in oceanographic funding, broader visibility of oceanography as a societal utility, increased attractiveness to the next generation of oceanographers who want to have a societal impact, and increased probability of identifying knowledge gaps and new science directions.

A secondary conclusion from the workshop was that broad application of "state of the art" oceanographic technology is at risk because the knowledge required to make it work is isolated in a small number of technicians. Research programs, both at individual institutions as well as at a national level, are at risk if this knowledge cannot be transferred. Development efforts using web groups (wiki pages) might help address this issue. Also, we discovered that high-impact topics (climate change, energy) were not interesting enough to the assembled oceanographers to warrant intense discussion at the workshop. Why? A likely answer is that these topics are too broad to be addressed by oceanographers alone. Per-

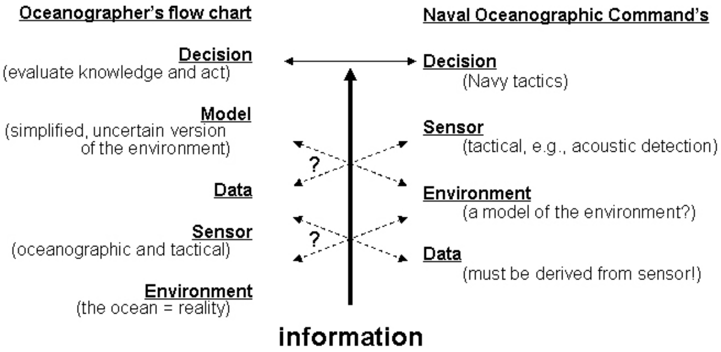

FIGURE 1 Applied oceanography. The flow chart on the right represents the organizational framework used by the NAVO. Clear communication of the role played by technology (sensors), observations (data), and models of the environment is required in order to increase the impact of oceanographic research on solving applied problems resulting in human decisions. This figure suggests a mismatch between how a research oceanographer views applied problems compared to NAVO's view.

haps we recognized this and deferred discussion to a time when the other required disciplines (atmospheric scientists, geologists, and engineers) are adequately represented.

ACKNOWLEDGMENTS

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Trends in Oceanography: More Data, More People, More Relevance

*J. Thomson**

In 2025, the world's oceans will be largely unchanged from today. They may be more acidic, or warmer, but those will be minor shifts in comparison with the coincident evolution of society. Changes in oceanography, as a scientific discipline and as a commercial interest, will thus be driven by human factors. Three current trends are likely to continue: first, several areas of oceanography are becoming data-rich for the first time. Second, there are more people producing and consuming oceanographic knowledge. Third, oceanography has an expanding global relevance in studying the changing climate, and, possibly, in meeting some of the growing energy demands.

MORE DATA

Technology developments are giving oceanographers access to ever-increasing amounts of information; often well beyond what can be observed from a single ship or gained from direct calculation. The most prominent examples are the products from remote/autonomous sensing and from computational modeling. Both are the result of accelerated technologies, such as solid-state memory, lithium batteries, and multi-core processors, which are dramatically improving sampling via a multitude of platforms and are enabling models of realistic complexity. The resulting datasets and simulations are being used to test hypotheses and

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*Applied Physics Laboratory, University of Washington

advance science, as well as provide data products to a growing audience. The wealth of data is encouraging science that is more comprehensive, such that results from specific locations and conditions can now be placed within larger contexts.

There are fundamental limitations to these new technologies, however, and care must be taken to gather all the data necessary to answer a given set of scientific questions. Specifically, the oceans remain opaque to all forms of radiation, and thus satellite remote sensing remains a surface-biased tool. Computational models remain under-resolved in space and time, even with the rapid acceleration of processor speed. Thus, the conventional tools of oceanography (i.e., shipboard surveys, laboratory experiments, and theoretical derivations) will remain important for assimilation into model results and new observing systems. As such, the infrastructure to teach conventional oceanography must be maintained. Fundamentally, the wealth of data from observing systems will only be useful in so much as the underlying processes can be understood. That will require focused experiments carefully designed by expert researchers—people who are producers of scientific knowledge, not solely consumers of data products.

MORE PEOPLE

There are more producers and consumers of oceanographic content every year. The number of trained oceanographers, measured by U.S. doctorates, is increasing at an annual rate of 10%. While this is promising as a solution to the influx of new data, it is daunting for the U.S. agencies tasked with funding oceanographic research. There is a real danger that high-risk, exploratory research will be shelved in favor of short-gain projects with nearly guaranteed outcomes. Conversely, there is a tremendous opportunity to expand oceanography beyond the tradition of federal funding. Industrial and corporate partnerships may sprout as oceanic resources are explored and integrated into the global economy.

Enhanced public and grassroots support may emerge as well. For example, as instrumentation costs become lower, a network of citizen observations, similar to the Meteorological Assimilation Data Ingest System (MADIS) Mesonet for weather observations, will be possible by utilizing private vessels, docks, and facilities. Such projects would continue the progress towards a data-rich state, while attracting public interest. The pending launch of Google Ocean™ (a similar geo-exploration software to the popular Google Earth™) is an excellent example of such data-based outreach. In this way, "more people" may be expanded well beyond "more doctorates"—and may be one of the most promising trends for oceanography.

MORE RELEVANCE

The oceans are now acknowledged as fundamental to the earth system and anthropogenic effects on that system. Given this new motivation and expanding datasets, several large science questions are ripe for study. For example, exchanges between the ocean and the atmosphere must be quantified to provide realistic input in Global Circulation Models (GCMs) and climate forecasts. One of the key quantities is energy, which is input via winds and tides and somehow dissipated via mixing and turbulence. This process is poorly understood on both local and global scales, yet is crucial to the dynamics of the overall system, as well as the accuracy of the GCMs. Furthermore, ocean energy is of practical interest as a potential power source; clearly, to understand the possible effects extracting power, the natural system must be better understood.

Other basic topics, such as biophysical coupling and coastal evolution, are poised for breakthroughs. While progress accrues in the basic science, predictive models in these areas are becoming valuable tools for resource management. Already, physical models of circulation, impressive in their richness of phenomena and fidelity to data, are being challenged to include biology at multiple scales. In well-tuned cases (which require data-rich backgrounds), the ability to reproduce coincident data shows great potential. The ability to predict future scenarios is enticing, but must always be constrained by observations and grounded in process.

The next 16 years will mark only 10% of the time passed since the voyage of the H.M.S. Challenger (1872) and the naissance of oceanography. Surely, the curve is steeper now, and these next 16 years will witness more change than the first. The relevance is greater, the expectations are higher, and the pace is quicker. Yet to make real progress, emphasis on the basic scientific methods and measurements of the previous years must prevail, even as data products and applications become more comprehensive.

Future Developments to Observational Physical Oceanography

*Tom Sanford**

Forecasting is one of the most difficult intellectual endeavors. If I could do this well, I'd have salvaged more of my retirement investments! However, from the perspective of my 45 years in oceanography, I can offer some expectations for 2025. I experienced the transition from purely mechanical instruments, such as current meters and Nansen bottles, to electronic versions, navigation from Loran A to GPS and hand graphing of observations to onboard digital computing and display. The advent of low-power electronics, microprocessors, solid-state memory, batteries and communication links has had an enormous impact on ocean instrumentation, field observations and data analyses. Some components and systems, however, have changed little, such as pressure cases and underwater connectors. Research has become much more interdisciplinary. Clearly, we have engaged more in coupled atmosphere-ocean investigations. Other changes include the growth of graduate education programs, comprehensive numerical models and the research vessel fleet. Even the attitudes and helpfulness of vessel crew have changed greatly for the better! There is more emphasis on outreach.

Between now and 2025 we need to avoid some obvious pitfalls. First is funding, the lifeblood of oceanography. Oceanographic sensor development and field operations are expensive in terms of personnel and facilities. For many of us, we rely mostly on ONR and NSF and have experienced increased difficulty and uncertainty in obtaining adequate

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*Applied Physics Laboratory, University of Washington

support. State and municipal governments should support local environmental studies. Alternative models for funding should be explored, such as by private individuals and foundations.

Second, research opportunities are changing. Already there is a trend away from individually directed research to multidisciplinary, multi-investigator projects. NSF's Science and Technology Centers and ONR's Departmental Research Initiative (DRI) projects are examples. This trend does not encourage innovative, PI directed research. Access to facilities may be limited because ship time is becoming more expensive and less available. Vessel clearances for work in foreign Exclusive Economic Zones (EEZs) may become more difficult. Restrictions on the scientific use of acoustic transmissions, such as from acoustic modems, ADCPs, and acoustic backscatter systems, are hampering oceanographic research.

Third, human resources may dwindle. We lack recruitment and retention of enough graduate students and ocean engineers, and fewer post-graduate and initial professional employment opportunities. Oceanography in 2025 will require new ideas, methodologies and innovative technologies. However, there are too few junior ocean engineers to support maximum advances (a related impediment is the rapid obsolescence of electrical and mechanic components). Few graduate students conduct dissertation research at sea or even have any sea experience. Some graduate students may view the fragility and uncertainty of funding as disincentives for a career in experimental oceanography. The field is graying and needs to develop a plan to recruit and retain young oceanographers. More foreign students fill slots in our graduate programs. Recent ads for postdocs received few or no replies from U.S. citizens. One remedy that could change this trend is to reinstate the ONR Graduate Fellowship program.

A forecast for 2025 should reflect responses to present and looming challenges and new opportunities. What are these? We need greater and more reliable support and research opportunities for graduate and post-graduate students and junior oceanographers. A careful balance between curiosity-driven and sponsor-directed investigations must be maintained. We need more comprehensive, more representative, longer duration, and cost-effective ocean observations. New research facilities, new vessels and development of new technologies are vital to the advancement of physical oceanography in 2025. I can speculate on some topics. My perspective emphasizes basic research in physical oceanography.

I think there will be more emphasis on research that is interdisciplinary and has relevance for society. Global climate change will continue to require extensive observational and numerical studies. There will need to be oceanographic methods to monitor the global ocean circulation. In particular, the Atlantic Meridional Overturning Circulation (AMOC) must be observed routinely because it may be a precursor to significant climate change. Another example is hurricane prediction, even mitigation. Carbon sequestration in the ocean and ocean acidification are important research topics of considerable societal importance. Anoxic zones require mitigation. Certainly, sea level rise will deprive millions of people of their livelihoods and force migrations, some of which will be strongly resisted by potential host countries. Piracy and terrorism may jeopardize oceanographic fieldwork and national security. Many of these problems will prompt interest in extensive coastal observing systems, such as cabled observatories, autonomous oceanographic instruments and satellite sensors. Wave and tidal energy conversion is likely to increase as societies search for alternative energy sources. Estuarine research will be needed to understand the ecology and cope with increased fishing and pollution. Of course, there will be the process studies that determine the physics of ocean phenomena, such as ocean mixing, storm responses and frontal processes. Observational work will rely on autonomous instruments. Swarms of autonomous underwater vehicles (AUVs) will provide truly synoptic observations of ocean regions and processes. Spatially integrating observations, such as those based on ocean acoustics and motional induction, will increase.

How will we achieve more comprehensive ocean research of longer duration and larger spatial coverage in 2025? We are likely to rely more on autonomous and remotely-operated technologies, satellites, vessels, shore-based and cabled observatories, bottom stations, gliders/drifters with acoustic modems, and more comprehensive computer models. A potentially important development is the cooperation of the maritime industry to provide platforms for VOS. More progress will be made with enhanced collaborations among oceanographers and ocean engineers.

Areas where scientific and societal needs should promote new methodologies include:

- low-power and high-density electronics, larger capacity solid-state memory and faster microprocessors,
- higher capacity energy sources (batteries, EC and ultra capacitors),
- *in situ* power generation (e.g., wave, microbial fuel cell),
- pressure-tolerant electronic components (i.e., operates without pressure case),
- improved high-baud, bi-directional communication links (e.g., Iridium follow-on),
- AUVs to make observations in hostile areas and piratical states,
- larger AUVs for delivery of bottom instruments or operation as mobile gateway nodes,

- more satellites with traditional and new Earth observing sensors,
- more VOS usage for instrument deployments and observations,
- faster computers and peripherals,
- improved numerical models with strong data assimilation,
- new sensors and sensor systems for:
  - velocity (low-frequency ADCP, turbulence, vorticity, profilers),
  - gliders, drifters and floats,
  - cabled observatories with profilers and bottom sensors (e.g., inverted echo sounders),
  - fast CTD profilers for operation on under way ships (both research vessels and VOS),
  - air-sea fluxes (e.g., gases, momentum, enthalpy),
  - *in situ* pH and gas concentration sensors with long-term stability.

Although progress will occur in most of the science topics and sensor developments, it is certain that many gaps will remain and new challenges and opportunities will arise. Will we have a comprehensive AMOC monitoring system, improved hurricane intensity and track prediction, extensive coastal pollution mitigation, accurate sea level prediction? Doubtful. I think progress will be made in many areas, however much new research and development will be required to achieve these goals in later years. It is likely that significant new financial resources will be hard to obtain under the current circumstances. I trust that compelling cases can be made for many important oceanographic undertakings.

Prospects for Oceanography in 2025

Michael Gregg[*]

SCIENTIFIC OPPORTUNITIES AND NEEDS

Continental shelves, slopes, and fronts are proving to be far more complex than typical open-ocean mesoscale features that occupied much of our attention in previous decades. Because these domains may not be major influences on climate dynamics, justifying intensive research there can be a “hard sell” to NSF. But, because our Navy is likely to operate in these places, they should remain a focus of ONR for several decades.

At best, process studies near coasts and fronts catch glimpses of important dynamics for a few days or weeks, with hints of much more variability than was captured. Adequate understanding will require observations both more dense and more sustained than anything attempted to date. Fleets of autonomous vehicles, some propelled, some not, offer the only hope. ONR’s Persistent Littoral Undersea Surveillance Network (PLUSNet) program is a prototype of what can be done, but, to obtain scientific results, an academically-based facility is needed, similar to the National Center for Atmospheric Research’s (NCAR) fleet of research aircraft. The task is too much for individual PIs. And the level of coordination needed during measurements is not likely to result from groups of PIs coming together for a few weeks. Combining engineers, technicians and oceanographers, this group should be run by an experienced oceanographer, with oversight by a board of peers chartered by and reporting to ONR.

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[*] Applied Physics Laboratory, University of Washington

INSTRUMENTATION

Driven by ONR, advances in vehicle technology are one of the major successes of the past decade. Lacking so far is a corresponding development of small, robust sensors to take full advantage of these platforms. MEMS (Micro-Electro-Mechanical Systems) technology is advancing rapidly in many other fields (e.g., inkjet printers, accelerometers for deploying airbags in cars and stabilizing images on small cameras, and tiny gyroscopes to trigger dynamics stability controls). Systematic effort is needed to compare capabilities of MEMS and nanotechnologies with needs and opportunities for sensors on autonomous ocean vehicles.

As one example of sensor development, microstructure probes have not improved significantly for thirty years. Thinistors, combining the sensitivity of thermistors with the speed of cold films, are badly needed for use on vehicles moving at one knot or faster. I tried and failed several years ago, and now Ray Schmitt has a grant. If that is not successful, the lessons learned should be used to try again; the task is eminently feasible. Velocity microstructure is sensed using a dwindling supply of bimorph beams made for phonographs. Replacements are badly needed, and they must be far more resistant to water leakage than present probes to work for months on gliders or profiling floats. Finally, because high-frequency vibrations are endemic to vehicles, especially ones with propulsion, tiny but sensitive accelerometers are needed that can be mounted much closer to the probes than at present to remove vibration signatures.

On somewhat larger scales, direct measurements of salinity and density are likely the only way of solving problems posed by “salinity spiking” resulting from mismatches in dynamic responses of temperature and conductivity probes. Smaller probes will also permit mounting more types of sensors on front ends of vehicles. Improvements are beginning, but there is a long way to go to take full advantage of the new vehicles. The effort will require teams of oceanographers and engineers working together for considerably longer than the three- to four-year spans of typical grants.

PEOPLE

Who will do the work? Present trends show declining interest in physical science by American undergraduates. In particular, we are not graduating many students experienced in developing new instruments; success is uncertain, and oceanography faculties want Ph.D. students to demonstrate that they can do science rather than engineering. Worrying in themselves, these trends are particularly ominous for the Navy.

Last year several of us met with RADM Landy, the previous Chief of Naval Research, asking him what we could do to help naval oceanography. He responded with a question: "How can 6.1 research, oceanographic in our case, give the U.S. Navy a competitive edge?" My conclusion is that it presently offers much less competitive advantage than previously; our results are published in open journals, immediately available over the Internet, and the large number of foreign students and faculty ensure that advances in the art of understanding the ocean are rapidly transmitted abroad. The only prospect for a competitive edge that I can see lies in our results being picked up more rapidly by our applied Navy labs and contractors than by similar foreign groups. This, in turn, is also under stress. There is a shortage of U.S. citizens with oceanographic Ph.D.s to staff applied groups, such as NRL, and my sense is that the gap between these groups and the academic community has widened during the last decade or more. For example, participation of NRL personnel in joint work at sea is much less than it had been. Several steps can be taken by ONR to address RADM Landy's question.

- ONR can require that a significant fraction of the graduate students it supports be U.S. citizens. Provided that PIs can count on student support, this will require them to recruit undergraduates rather than filling their needs with foreign students.
- ONR can facilitate joint work and exchanges of personnel between academia and Navy labs. The Department of Defense (DOD) has recently developed a limited program along these lines, but ONR is in a position to make much more targeted impacts on ocean-related work by establishing joint workshops and field programs. Some elements of these exist, but the scope should be expanded to be effective. One aspect is facilitating academic oceanographers in getting security clearances to understand naval problems in enough depth to contribute to solutions. This, of course, will only work if some academic oceanographers are willing to devote some of their time to helping the Navy in ways not leading to open publications. Owing to the general tightness of funding, linking 6.1 funding to helping Navy labs and programs would be a strong inducement.

Oceanography in 2025

*John Orcutt**

Oceanography today is characterized by an increasing trend toward multi-institutional operation and management of seagoing resources that will accelerate in coming years for a variety of reasons. Examples today include UNOLS, Argo, OBSIP (U.S. National Ocean Bottom Seismometer Instrument Pool), HiSeasNet, OOSs, and the OOI. The effective management of such facilities is a major challenge; over a number of scientific and engineering disciplines, these collaborations have been characterized as Virtual Organizations.

Statistics and anecdotes both support this growth in virtual organizations. There are about 200 research universities in the country and publications in science and engineering involving multiple institutions increased by 48% between 1988 and 2001. Between 1990 and 2000, nearly 16% of all scientific publications involved international institutions, doubling over the decade 1990-2000. The rationale for this increased extra-institutional collaboration includes the trend toward more multidisciplinary research and the need to broaden the expertise to address increasingly complex problems. Practically, agencies and program managers often require multi-institutional collaboration. An NSF-sponsored study of virtual organizations, however, suggested that projects involving multiple universities produced fewer knowledge outcomes than those involving a single institution. While there are many reasons for these changes, finding effective methods for managing virtual organiza-

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*Scripps Institution of Oceanography, University of California, San Diego

tions is critical and must be considered more deeply than in the past. The NSF now often requests management plans for some programs and very extensive plans for projects such as OOI with budgets on the order of $400M. Another major driver in coming years for oceanography will be climate observations for both scientific purposes and in support of new treaties seeking to reduce the introduction of greenhouse gases and to mitigate changes associated with these gases.

The next major summit to develop the follow-on to Kyoto will begin on 30 November 2009. President Obama and his transition team believe that the replacement for Kyoto should not be a "Feel Good" treaty (an oft-used term to describe previous treaties like the United Nations Framework Convention on Climate Change). One of the hallmarks of a proper treaty is a monitoring regime to collect the data necessary to ensure that not only is the U.S. complying with the treaty, but that the other signatories are bearing their share of the burden. Monitoring systems for nuclear testing treaties are an example of previous efforts in this regard although the technical challenges for a climate treaty dwarf those encountered with nuclear testing! Significantly expanded monitoring on land and at sea will have to be undertaken, requiring new methodologies and broad collaborations between disciplines. Sixteen years lie between 2009 and 2025 and, with some certainty, it's possible to estimate the capabilities that will be available in computing and digital communications to support the growing need for observations, observatories and related modeling.

Today, chip density on central processing units (CPUs) is doubling about every 18 months, network speeds double every eight months, and disk capacity doubles nearly annually with little or no increase in cost for a physical unit. For example, it's now possible to buy a 1.5 TB disk drive for $166.00. In 2025, the same drive at the same cost will store 99 PB of data. The highest speed commercial network available today moves data at 10 GBps; in 2025 this will be 170 Pbps. An Apple Pro 8™ core desktop computer can compute at 24 billion floating-point operations per second (GFlop). In 2025 the same kind of machine, if it still exists, will provide 24 TFlop. This past March Intel showed off an 80 core CPU and similar technologies may continue to push the envelope in 2025. In terms of supercomputing, the TeraGrid includes computers that can sustain 8 TFlop computations. Today, petascale computers are beginning to become available, a culmination of the TeraGrid that began eight years ago. If the NSF continues this pattern, there will be two more transitions before 2025, first to exascale and then to zettascale computing, 10^9 times faster than today's TeraGrid. Geoscience models in 2025 will be computed at scales much more physically interesting than today and the management of the parent environmental data will be straightforward. The workflows needed to support the integrated management of modeling, data, data assimilation and observational network control, however, will be a major challenge. Oceanography will only be able to occasionally make use of high-speed fiber optical networks, especially at a global scale.

The state-of-the-art in satellite communications today is the NASA Tracking and Data Relay Satellite System (TDRSS) that can support communications including a ground segment at speeds in excess of 150 Mbps using Ku-Band technology. Boeing™ and Lockheed Martin™ are now bidding on the Transformational Communication Satellite Program (TSAT) that will provide future communications for DOD. Like TDRSS, this is a multi-satellite, geosynchronous system that can relay data in space around the planet. Each satellite must be capable of providing multiple RF connections at >45 Mbps and laser communications at 10-100 s Gbps. These systems bound the upper end of communications available to oceanography. Today HiSeasNet, which I started a few years ago, provides shore->ship communications using C and Ku-Band antennae at 256 kbps to as many as five ships and individual ship->shore at 96 kbps. The highest data rate achieved by HiSeasNet was 19 Mbps although the bandwidth is limited by available funding and not by current technology. The system is presently installed on 16 UNOLS ships. Individual buoys, gliders or profilers generally use Iridium satellites with data rates of 2.4 kbps or multiples thereof. Oceanographers should seriously consider the use of commercial C-Band with spread-spectrum technology to provide continuous data rates significantly larger than this and at a fraction of the cost of Iridium.

Finally, long-term measurements of climate-scale phenomena are seriously lacking. If a 30-year climate time scale is used, several cycles must be observed to understand the complex system. The longest instrumental record may be the central England temperature composite going back to 1659. Useful global atmospheric records have been taken since World War I, global ocean measurements began in the 1990s, adequate ice measurements started five years ago, synoptic sea surface temperature measurements from satellites began 30 years ago, atmospheric CO_2 measurements are fifty years old, satellite altimetry measurements started 15 years ago and deep ocean physical measurements have only begun. Too few scientists understand how to build long-term instruments capable of addressing climate questions, funding is generally limited, and faculties don't reward such work. Finally, even fewer scientists and engineers understand how to extend measurements made with a single instrument and a single investigator to a global network required by science and policy over the coming 16 years.

Thoughts on Oceanography in 2025

*Daniel Rudnick**

OBSERVATIONS, MODELING, AND SCALES

Oceanography has tended to be an observational science in the sense that phenomena have been observed before they were predicted theoretically. An open question is whether this view will have to evolve as numerical models become more realistic, and predict features in greater detail than can be easily observed. A growing emphasis on prediction will continue until 2025. Observations will have at least two fundamental roles as prediction becomes better. First, observations will continue to be needed to validate models, and to provide ground truth for initialization and assimilation. Second, observations will be essential to develop parameterizations for mixing, where mixing is used as a general term for all processes of smaller scale than can be simulated directly by the model. What we call "mixing" should be understood as purely operational: as computers get faster and spatial grids get finer, the unresolved processes are themselves of finer scale. The observational focus should then be what is now commonly called the submesoscale or finescale, smaller than mesoscale eddies of order tens of kilometers, and larger than the microscale of centimeters. Many autonomous platforms are well suited to such observations, and their use will certainly expand.

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*Scripps Institution of Oceanography, University of California, San Diego

CLIMATE CHANGE

A major focus of the next twenty years must be the study of the ocean's role in climate. Climate change has entered the public consciousness to an extent unparalleled by any scientific issue in recent times. How we respond to climate change is the scientific challenge of our generation. How can oceanographers respond to this challenge? We will certainly be called upon to document change as it occurs, and to evaluate attempts at remediation. Research on alternative energy sources will intensify, but energy derived from the ocean is likely to be significant only in certain locations, not as a global solution. Global observations of the ocean will continue to improve, with moorings, floats, and gliders forming the backbone of the system. A fundamental challenge to improved understanding is the long time scales involved: time series long enough to achieve definitive answers may be too far in the future to help us solve problems in time.

PHYSICAL/BIOLOGICAL INTERACTIONS

The next two decades will see the solution of many problems straddling the boundary of physics and biology. A driving force will be the development of new and better biological sensors. Increasing numbers of biological variables will be measured at the same resolution as physical variables. The physical processes that supply nutrients to the euphotic zone will be quantified to the extent that reliable predictions of primary production will be possible. Distributions of zooplankton will be observed and modeled better than ever before, so predictions of biomass will be at the same stage that predictions of salinity and temperature are today. The ultimate result of these advances will be better stewardship of fisheries, as ecosystem observation, modeling and prediction reaches maturity in 2025.

THOUGHTS ON EDUCATION

In the past, most graduate students had a bachelor's degree in a basic science, engineering or mathematics before beginning study in oceanography. As programs in environmental sciences sprout across the country, students who would have followed the traditional path by taking a basic science will receive more interdisciplinary training. As the number of courses taken by an undergraduate is necessarily finite, this increased breadth must come at a cost of depth of knowledge in a particular discipline. We are already seeing this effect in physical oceanography, as incoming students with interdisciplinary undergraduate degrees are notably less capable at math than typical graduate students of the past. As educators, we must change how we teach, and even what we teach, to achieve the best results with the new generation of students. A positive outcome will be future oceanographers who are better at crossing disciplinary boundaries, and explaining scientific results to non-specialists. A potential problem may be scientists without a core expertise. How will the next generation gain the depth of knowledge to design the next great oceanographic instrument, or make a fundamental breakthrough in theory?

HOW SCIENCE GETS DONE

An approach to achieving the breadth of expertise needed to solve outstanding scientific problems is to form teams of scientists from different disciplines. Teams like these can be intellectually rewarding for participants, and can lead to products unrealizable by individuals. However, in the future as now, scientists working long solitary hours in offices and labs will make the most fundamental advances. While inspiration can be drawn from a variety of sources, originality comes from within. A graduate student working in obscurity now will be the emerging leader of 2025.

UNSOLVED PROBLEMS

The most exciting unsolved problems of 2025 are likely, as always, to be the problems uncovered by solving today's problems. I will not attempt to divine these, which is a prediction an order of magnitude more difficult than the guesses I have made so far. There will be, however, current problems that will remain unsolved in 2025. The fundamental limitation on the durability of oceanographic instrumentation will continue to be corrosion and biofouling. Whatever advances will be made in improved sensors and energy sources for ocean observations, electrons will always move and life will find a way. In this sense the ocean is an observational frontier unlike land, atmosphere, or space.

The Role of Observations in the Future of Oceanography

*Raffaele Ferrari**

Galileo Galilee first pointed out that progress in physical sciences proceeds in three steps: observation of a new natural phenomenon, formulation of a hypothesis to explain the phenomenon, and an experiment to test the hypothesis. This implies that scientific progress is achieved only when observations of new phenomena become available. This has indeed been the case in oceanography since its early days. New observations have led progress in the field. An assessment of where the field might be fifteen years from now must therefore start from an evaluation of what observations are likely to become available in that time span. I will restrict my considerations to a few technologies that I believe are most likely to advance our field, and will leave it to others to attempt a comprehensive review of the future of physical oceanography.

Lacking a crystal ball or clairvoyance, the past is our best guide to predicting the future. I entered the field fifteen years ago, when satellite oceanography was taking its first steps. In those years, satellite measurements of SSH and sea surface temperature (SST) provided the first global view of the time dependent nature of the ocean circulation. The Argo float program complemented surface measurements with thousands of vertical profiles of temperature and salinity continuously repeated across the whole ocean. The old paradigm of an ocean circulation dominated by large scale steady gyres (~5000 km) was abandoned. A new view emerged, where the circulation is dominated by turbulent mesoscale

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*Massachusetts Institute of Technology

eddies with scales of ~100 km, much like cyclones and anticyclones in the atmosphere. In parallel to these novel observations, ocean models also progressed from coarse and highly dissipative mesh grids to finer eddy-admitting grids. We are now able to produce simulations of the present state of the ocean that compare increasingly well to observations. However, the skill of models in making long range predictions of the ocean is still very limited, because they lack a physically based representation of the physics at the submesoscale, i.e., scales of 1-100 km. Dissipation of momentum is achieved through enhanced viscosities and drag laws with little physical validation. Turbulent transport of tracers like heat, salt, carbon and nutrients is represented with unphysical constant eddy diffusivities.

The first observational breakthrough is likely to come from an upcoming generation of radar altimeters. Conventional nadir-looking radar altimeters have an alongtrack resolution of ~100 km, barely sufficient to resolve the largest mesoscale eddies. The technique of radar interferometry, recently demonstrated by NASA's Shuttle Radar Topography Mission, offers an approach to mapping SSH at 10 km resolution over a wide swath of 100 km. NASA and the French space agency CNES (Centre National d'Études Spatiales) plan to use this technology in the Surface Water Ocean Topography (SWOT) mission to be launched sometime after 2016. SWOT will provide the first global observations of the surface mesoscale field and a large fraction of the submesoscale field. This kind of resolution is crucial to assess whether the turbulence generated by high resolution ocean models is realistic—mesoscale eddies contain 90% of the ocean's kinetic energy and submesoscale eddies and fronts dominate the vertical velocities. Biogeochemically, these eddies set the physical and chemical environment of ocean ecosystems on space scales of kilometers and time scales of days, through their stirring of tracers and nutrient supply control by vertical motion across the euphotic zone. Indeed, one can think of the mesoscale as an ocean life "evolutionary hot-spot" in time and space. Just as it is not a coincidence that elemental ratios in seawater are the same as those in life, so the lifecycle of phytoplankton is in synchrony with finescale physics. These turbulent flows may well be key determinants of the structure and function of the entire marine food web. Moreover, the average structure of marine ecosystems will reflect the integrated, and rectified, effects of the finescale processes, modulating primary production and community structure and hence the export of organic carbon to the interior ocean with obvious implications for climate.

Seismic reflection profiling, a technique that has been used for decades to image the solid earth beneath the ocean, could also become a revolutionary tool for oceanographic studies. Boundaries between bodies of water have a very faint sonic signature, which the oil industry used to treat as noise. But in 2003, a team led by W. Steven Holbrook of the University of Wyoming adopted the technique and created unexpectedly clear acoustic images of density boundaries in the ocean with an outstanding resolution of 10 × 10 meters. The information in these images is not quantitative (it is difficult to invert for density from sound), but it provides detailed pictures of turbulent structures in the ocean on scales from hundreds of kilometers down to a few meters. Much like schlieren images used to study turbulence in the laboratory, the 2D seismic sections might shed light on how energy is transferred from geostrophic eddies to 3D turbulence. This is an essential question because the pathway of energy from the large to the dissipation scales sets the equilibration of the ocean circulation. Numerical models must accurately represent this energy transfer if they are to be believed in their forecasts.

Last, but not least, in the next fifteen years the oceanographic climate record will be substantially extended. Record lengths of SSH and scatterometer winds will be doubled. Monitoring of ice sheets from space will extend to a couple of decades. The Argo observing system, which has just become operational, will provide the first glimpse of ocean variability below the sea surface. The combination of these measurements is not only crucial to monitor the anthropogenic effect on Earth’s climate, but it is also essential to test the skill of ocean models in reproducing natural climate variability. Ocean models are often run for centuries to study climate change, but it is not clear whether they have any skill on those time scales. Fifteen years from now, it will finally be possible to test model skill vs. data.

The list of observational techniques presented is not meant to be comprehensive. Rather, I emphasize techniques that will sample temporal and spatial scales that are not yet observed but are crucial to improve our understanding of the role of the oceans in climate. Climate change has been called the defining issue of our era. The future trajectory of the Earth and its management in the coming century can only be informed by numerical models that couple atmosphere, ocean and cryosphere, as well as the complex interaction and co-evolution of its physics, chemistry and biology. The chemical and physical environment of life in the ocean is controlled by oceanic turbulence, which draws heat from the surface into its interior and, through its interaction with life, draws carbon to depth. The observations I described will move us toward a full understanding of ocean turbulence and its impact on life. This will be a milestone achievement towards our goal of predicting climate and climate change.

The Future . . . One More Time

*Rob Pinkel**

In discussing visions of the future, it is important to distinguish between what one desires to happen and what one predicts will probably happen. I'll attempt to focus on the former.

The process of "planning for 2025" is now entering its second decade.[†] In the years remaining, there is sufficient time to spin up one new Tropical Ocean-Global Atmosphere (TOGA) or WOCE sized research program, but not two. With the OOI spinning up over the next five years, there is no dominant vision for the next new research thrust.

In terms of changing science, I am most aware of developments near my own field of interest. Presently, there is a growing appreciation that the mesoscale and larger currents loose energy and mix scalars at rates that only partially depend on their own flow properties. Sites of high turbulent mixing are found near topography that is tuned to the baroclinic tides and also near flow structures that can refractively trap near inertial waves. The space-time geography of these mixing sites can be inferred from relatively few key process-experiments. Does an "eddy viscosity" fueled by breaking internal waves indeed extract energy from the mesoscale as would a true viscosity? In the atmosphere, low-latitude waves are thought to drive eastward flow at mid-latitudes. Ideally, the numerical models of 2025 will have a dissipative scheme that incorporates external geographic-fortnightly modulated mixing and the refractive trapping of

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*Scripps Institution of Oceanography, University of California, San Diego

[†] Using the FOFC fleet renewal plan as a starting point.

inertial waves. Field experiments to support this modeling effort will be great fun.

In terms of ship usage, numerous enhancements of scientific capability are possible. High-frequency (~50 kHz) multibeam swath sonars should be installed on research vessels and used for the routine mapping of scattering layers in the upper 600 m of the ocean. On a moving ship, a swath sonar provides a 3D view of plankton patchiness that can be recorded whenever the ship is underway (Figure 1). The midwater zooplankton community is relatively inaccessible to net sampling. Acoustic surveying can help to focus *in situ* sampling campaign. The lateral slope of plankton layers is a measure of isopycnal slope, a quantity of direct interest to physical oceanographers. The recent development of seismic refractive imaging of deep ocean density structures should be exploited. A wealth of information can be derived from ship-mounted Coastal Ocean

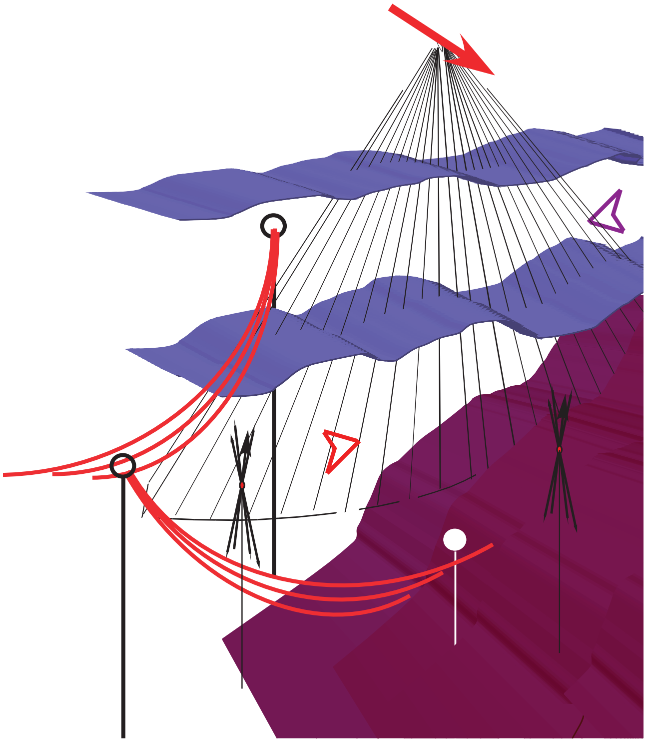

FIGURE 1 Schematic of a survey where the ship (arrow) is using multibeam sonar to map zooplankton layering in the water column, assisted by a constellation of AUVs and all operating within an acoustic tomographic array.

Dynamics Applications Radar (CODAR). By steaming repeatedly around a fixed path, tidal and inertial currents can be separated from sub-inertial signals.

Pooled AUV resources should be expanded, such that a ship user has the ability to operate a small constellation of remote sampling platforms. A central survey ship and surrounding AUVs could conduct small scale tracer release experiments, frontal studies, etc., with great efficiency. The development of low-speed long-range gliders has radically improved existing observational capability. The three present versions are all variations on the "virtual mooring" theme. In terms of platform capability, huge range of parameter space remains to be explored. Larger gliders/AUVs can carry more energy relative to their drag. By 2025 one hopes that an assortment of designs are available, each excelling at a different task. In the present community, the operators of pooled AUVs/ROVs also carry on platform development, in an ongoing response to customer demand. This has the inadvertent side effect of killing the development of alternative systems by independent PIs at "non-subsidized" sites. The surviving AUV/ROV development groups in the U.S. are each associated with a source of "fenced" financial support. A way must be found to "broaden the gene pool" while still pooling the hardware.

Incremental technical advances in selected areas can lead to greatly expanded scientific capabilities. Development of extremely low-powered versions of basic sensors such as current meters and CTDs will enable much broader bandwidth experiments. The creation of improved batteries will contribute to this bandwidth increase. A significant increase in the veracity of commonly used sensors is also required. Since 1995 (t_0–15 years), there has been relatively little progress. A next-generation family of standard tools should be deployed by 2025 (t_0+15 years). A focused program by NSF and/or ONR in this area would be catalytic.

Data transmission on global scales is presently accomplished by Iridium. The cost per bit must be enormously reduced if Iridium is going to serve as the primary remote data-link for the community. A transformative development would be the creation of planar phased-array antennas that would enable HiSeasNet broadband communications using relatively small surface buoys. Modularized solar power/battery storage systems and HiSeasNet antennas/servers should be developed and managed as a pooled resource.

The Role of Acoustics in Ocean Observing Systems

Peter Worcester and Walter Munk**

In the summer of 1940, Walter Munk asked Harald Sverdrup, then the Director of Scripps Institution of Oceanography, if he could ". . . be admitted for study towards the Ph.D. Degree in Oceanography." Munk reports that "Harald Sverdrup gave my request his silent attention for an interminable minute, and then said that he could not think of a single job in oceanography that would become available in the next ten years." World War II (WWII) and the Cold War changed all that.

"Business as usual" is not, from the historical perspective, necessarily what is now considered "business as usual." This observation is not meant to imply that a return to the prewar environment in which basic oceanographic research was conducted in the U.S. is likely. Two forces have been at work that are leading to significant changes in how oceanographic research is and will be conducted, however.

Since the end of the Cold War nearly 20 years ago, the Congress and President have not had an easy metric to decide how much funding to provide basic science in general and oceanography in particular. As a result, institutional and organizational arrangements that were developed during the Cold War are unlikely to be able to continue unchanged. It is becoming more and more obvious, for example, that oceanography will be unable to continue to support the numbers of individuals funded solely by grants to perform basic research that has been the norm in the

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*Scripps Institution of Oceanography, University of California, San Diego

years since WWII. One implication is that the size of oceanographic institutions performing basic research will shrink. Another implication is that oceanography will have to become ever more involved in undergraduate teaching to ensure hard-money positions.

The second force at work is technology. Oceanography is an observational science. Many, if not most, of the advances in our understanding of the ocean depended on the development of the tools needed to make measurements in the ocean environment. A triad of observational tools—satellite altimetry, hydrography performed by profiling floats and gliders, and acoustic remote sensing—combined with the development of ever more realistic ocean modeling and data assimilation capabilities, have revolutionized and continue to revolutionize our ability to observe the physical state of the ocean.

One possible outcome is that there may well be a shift toward the paradigm of meteorology, if society comes to believe that operational ocean observation systems are justified in terms of products useful to society, as operational atmospheric observational systems are now. The looming specter of anthropogenically-induced climate change may well lead society to this conclusion, although it is not the only aspect of the ocean in which society has a vital interest. The atmosphere and the ocean form a coupled system that determines the climate of the planet. We as a society have a vital interest in measuring how the earth's climate is changing, regardless of the cause. We also have a responsibility to measure what we are doing to the planet and to assess the effectiveness of any measures that we take to mitigate these impacts.

In meteorology, those running operational monitoring and prediction systems are distinct from those performing basic research, with different reward systems. There is already some movement in this direction in oceanography, as is evident in the implementation of the equatorial Tropical Atmosphere Ocean (TAO) project and IOOS. Those running the observational meteorological systems are rewarded for providing data products in near-real time, rather than for publishing papers. If oceanography moves in this direction, academic oceanographic institutions will have to decide whether or not to participate in the operation and maintenance of operational, as opposed to research, ocean observing systems. Those choosing to participate in the operation of OOSs, as some already are (at least in part to make up for the inadequacy of other funding sources), will likely find it necessary to modify the academic reward systems currently in place in order to suitably reward those who maintain and operate the observing systems.

Given the importance of the ocean to society, it seems that there is a reasonable likelihood that the current movement toward operational ocean observation systems will continue, although perhaps with different degrees of emphasis on the coastal and open oceans. These systems will evolve over time as the effectiveness and costs of various possible ocean measurement technologies become clearer. Of the triad of observational tools available to physical oceanographers, active and passive acoustic systems have not yet been widely applied in ocean sensing systems, however, in spite of the fact that they have capabilities that are difficult or impossible to obtain using other technologies.

OCEAN ACOUSTIC THERMOMETRY/TOMOGRAPHY

Long-range acoustic transmissions have the ability to measure gyre-scale changes in ocean temperature with unparalleled precision and temporal resolution (Figure 1). The acoustic travel times are inherently spatially integrating, which suppresses mesoscale variability and provides a precise measure of range- and depth-averaged temperature. The measurements are nearly instantaneous and can be made at any desired sampling rate at essentially no additional cost. Unlike other sensors, the acoustic methods are not subject to calibration errors, because the fundamental measurement is one of travel time.

Climate change is an important, but not the only, issue. It is necessary to differentiate between the ability to measure large scale changes in ocean properties and the ability to measure the smaller-scale variability that impacts ocean ecosystems. Profiling floats, as implemented in the Argo program, form (by design) an incoherent array that is not well suited to providing information on smaller-scale ocean variability. Acoustic systems may well be able to help fill this gap. The ability of acoustic methods to resolve mesoscale variability with high temporal resolution may well make important contributions to the study of the interactions between the physical and biological environments. After a century of measuring what turned out to be ocean climate, physical oceanographers are finally developing the tools, including acoustic methods and gliders, needed to measure ocean variability on the time and space scales important to ocean ecosystems.

TRACKING OR UNDERWATER GPS

Relatively crude acoustic tracking of neutrally buoyant floats (Sound Fixing And Ranging [SOFAR] and RAFOS) is being and has been used extensively to make Lagrangian measurements of ocean circulation. Modern tracking systems using broadband signals can provide much higher accuracy positioning not just for neutrally buoyant floats, but for gliders, AUVs, and profiling floats. The potential benefits might well rival those provided by GPS.

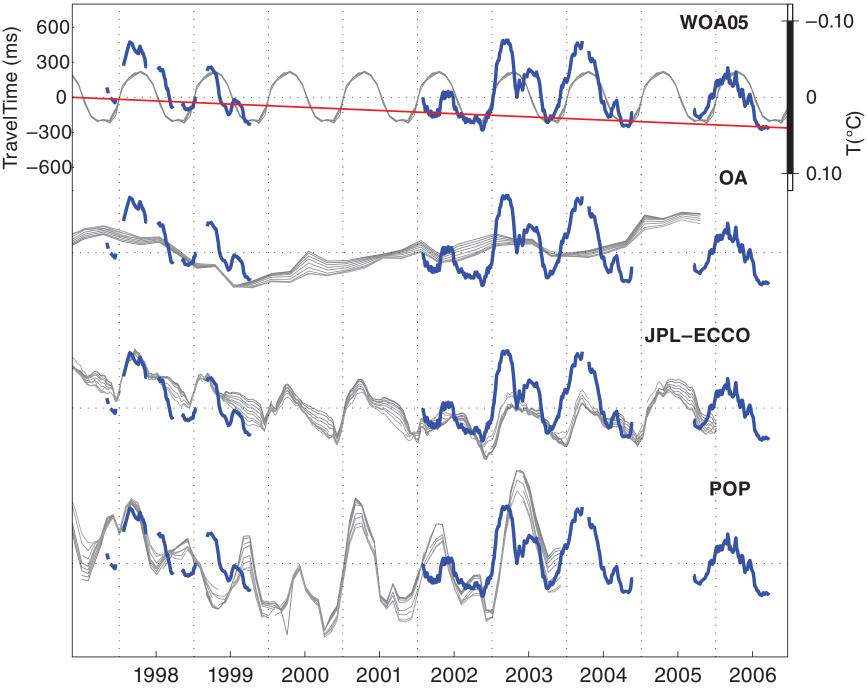

FIGURE 1 Measured travel times for transmissions from an acoustic source near Kauai to a bottom-mounted receiver approximately 3500 km distant to the northwest of Kauai (bold) are compared with travel times derived from four independent estimates of the North Pacific (light): (i) climatology, as represented by the World Ocean Atlas 2005 (WOA05), (ii) objective analyses of the upper ocean temperature field derived from satellite altimetry and *in situ* profiles (OA), (iii) an analysis provided by the Estimating the Circulation and Climate of the Ocean project as implemented at the Jet Propulsion Laboratory (JPL-ECCO), and (iv) simulation results from a high-resolution configuration of the Parallel Ocean Program (POP) model. The time means have been removed from all of the time series. The nominal travel-time trend corresponding to a warming of 5 m°C per year on the sound-channel axis is also shown (straight line). Approximate ray-average temperature perturbations corresponding to the measured travel-time perturbations are shown on the right-hand axis.

MONITORING OF UNDERWATER SOUND

Monitoring ocean sound is valuable for a variety of reasons, ranging from detection of underwater earthquakes, to detecting clandestine explosive tests (e.g., Comprehensive Nuclear-Test-Ban Treaty Organization [CTBTO] hydrophone arrays), to measuring wind and rain, to long-term monitoring of biological processes (cetaceans, fish, invertebrates), to determining trends in the "climate" of ocean sound levels.

OCEAN MODELS, DATA ASSIMILATION, AND OPERATIONAL OCEANOGRAPHY

Ocean acoustic tomography has been successfully used to measure ocean temperatures and currents on a wide variety of scales for thirty years, but acoustic data still seem unfamiliar to many oceanographers. As the normal use of ocean data becomes assimilated into ocean models to estimate the present (and future) state of the ocean, acoustic data will be placed on an equal footing with other data. The issue will become the ability of the various measurements to effectively constrain ocean models.

There also seems to be a perception that acoustic methods are inordinately expensive. Oceanography has not in the past been sensitive to the issue of life-cycle versus capital costs, as this issue does not arise in short-term, process-oriented basic research. Life cycle costs are critically important in gauging operational observation systems, however. Consideration of true life-cycle costs has the potential to make systems with higher capital costs, but lower operational and maintenance costs, such as acoustics, more attractive. My conclusion is that given the unique capabilities of acoustic sensing systems and their relatively low operational costs, it would seem that the wider application of acoustic methods to measuring the ocean will ultimately be inevitable.

Oceanography in 2025

*Walter Munk**

Let me start with a backward look. What are the important developments in physical oceanography in the last few decades? I would rank as foremost: (i) mesoscale dynamics, the recognition of ocean "weather" and its implication to ocean modeling, (ii) satellite remote sensing.

I consider TOPEX/Poseidon the most successful ocean mission ever. It has served to solve dozens of ocean problems that were previously not understood. Yet when John Apel came to Scripps to sell us on a satellite altimetry mission, there was scant interest. He spoke to a leading oceanographer who told him: "If you gave me the data, I would not know what to do with it." John had a similar reception in Woods Hole. Our track record for predicting important developments is not very good.

We are very short on oceanographic time series in our highly variable environment. I will predict that drifters and gliders will take over these expensive (and boring) missions.

Without wind stress the global ocean would be a stagnant pool of pollution. Yet we do not have a physical model of wind stress. I am somewhat concerned that the success and sexiness of modeling activities keep us from solving some fundamental problems. I am even more concerned that a reduced emphasis on sea-going observations will keep us from solving some fundamental problems.

We are desperately in need of observations extending over at least a century. John Colosi and I (Colosi and Munk, 2006) have recently pub-

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*Scripps Institution of Oceanography, University of California, San Diego

lished an analysis of the Honolulu tide gauge which goes back to imperial times, yet we were NOT able to detect an expected climatic drift. Too short! It is hard to request many 100-year time series by 2025, but we shall have made a start. We need to think what it takes in our society to commit for long-term observations.

REFERENCE

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Physical Oceanography in 2025

*Chris Garrett**

Physical oceanography will continue to advance using new observations and more powerful computers. It will contribute increasingly to interdisciplinary problems. Based on my own narrow experience and prejudices, I have three main recommendations. These are that we:

- Devote more attention to practical issues that fall somewhere in the middle ground between physical oceanography and ocean engineering.
- Continue to recognize the value of simple models.
- Consider seriously the education and recruitment of our successors.

The present state of the world is one of the reasons for suggesting more attention to practical problems. It could even be argued that we are in a situation similar to that of 1941, facing serious threats that demand the focused attention of the scientific community. Many of today's threats are aspects of rapid global change, with some of them being associated with the by-products of human energy consumption.

The oceanographic community is currently devoting considerable attention to research aimed at improving predictions of the future climate. This is admirable, but I suggest that (i) these predictions will remain sensitive to small scale processes that we will never be able to understand and

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*University of Victoria

parameterize precisely, so that our efforts will lead merely to a reduction in the error bars on our predictions, and (ii) there is already enough evidence to suggest that the probability of unacceptable climate change is high enough to warrant drastic changes in human activities.

It can thus be argued that major attention to such things as non-greenhouse gas emitting energy sources is warranted. If these are not found and adopted, then attention must also be paid to adaptation, particularly to things like rising sea level. In both of these areas there will be a need for our community to contribute at the interface between physical oceanography and engineering, and, of course, the members of our community in 2025 will include people currently in the early stages of their education.

I would like to give a simple personal example of an investigation in which the viewpoint of a physical oceanographer was brought to bear on a practical problem in renewable energy. The topic, while comparatively trivial and unimportant, will also serve to illustrate my second point about the value of simple models. The topic is that of placing turbines in strong currents to generate electrical power. It turns out that, subject to a couple of reasonable approximations, there is a very simple general formula for the maximum available power, well supported by detailed numerical models and very different from the engineering formula in common use. An overview can be found in Garrett and Cummins (2008).

The message of this example is that the appreciation of physical understanding and simple models is deeply rooted in the physical oceanographic community but not always so obvious in approaches to practical problems. I could provide several similar examples, as I'm sure many other physical oceanographers can. Our traditions need to be maintained and we need to be prepared to contribute more to practical issues where our approach is valuable and complements that of other communities. Overall, it probably takes physical oceanographers to point out that power from the ocean is unlikely to be globally significant.

By “we,” I mean the physical oceanographic community. In 2025 it will no longer consist of the same individuals. Where will the new members of our community come from? Will they have the same strengths as us, and can we help them avoid any weaknesses from which we suffer? We could start with a questionnaire for those already in the field, with questions such as:

- What was your educational background?
- How did it prepare you a) well, and b) badly, for a career in physical oceanography?
- What attracted you into physical oceanography?

- How can we be sure that by 2025 we will have people entering our field who are even better prepared than we have been?

In answering the last question, we need to recognize that someone who will graduate with a Ph.D. in 2025 is maybe in grade five now. How can we help to ensure that such a student will have an adequate school education in mathematics and science? What do we recommend for university study? I suspect that it is a strong physics background that we most appreciate and I am personally concerned by a) the diversion of good students into calculus-free university programs in environmental science, and b) the narrow-mindedness of most North American physics departments.

We all need to work at our own, or affiliated, institutions to develop courses and programs that will attract students who are talented in mathematics and physics but who want to find fields that are both intellectually challenging and societally valuable. What could be better than "the physics of the environment"?! We can also benefit from exposure in semi-popular journals read by physics faculty and maybe undergraduates. *Physics Today* is one such journal, with frequent articles on our kind of physics and with several members of the editorial staff who are sympathetic. We need to cultivate them!

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A Vision of Future Physical Oceanography Research

*James J. O'Brien**

Despite great advances in understanding the physical ocean, I envision much new knowledge to be gained in the next 25 years. Most oceanographers still think about steady-state balances despite all the knowledge about variability—whether the topic is eddies or internal waves, etc. Almost every oceanographer describes the ocean in a steady-state manner (present reader is an exception, of course).

The atmosphere is heated from below, the ocean from above. In the lower atmosphere, coherent structures last a few weeks, while in the ocean, coherent structures created by internal ocean dynamics or atmospheric forcing lasts for months to decades. I know of only a few who understand this. This special behavior of the ocean means that blue water oceanography cannot be modeled very well without an adequate observational program.

HOW WILL THE RESEARCH BE CONDUCTED?

As to be expected, oceanographers will use every possible technique—*in situ*, satellites, drifting buoys, smart flyers, etc. Oceanographers have to give up their hoarding of data, paid to be collected by the federal agencies. In addition, deployment of resources has to be done wisely by testing hypotheses and considering understanding—called "experimental design"—which is rarely used in physical oceanography.

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*Florida State University

WHAT QUESTIONS WILL BE ANSWERED?

By smart folks is the answer. The entire community must begin to embrace numerical models. There is currently an obsession with climate change but extreme events are more important. For example, meteorologists can predict where a hurricane can go but they cannot tell you how strong it will be tomorrow. The answer must be, at least partly, due to the lack of ocean data and understanding. This is an important oceanic problem. There are many others.

WHAT QUESTIONS WILL REMAIN UNANSWERED?

I don't know. It depends on the investment made in oceanography. It is clear to me that great importance must depend on understanding the variability of the ocean. We need at least two scatterometers, but as of 2009 we cannot expect these until 2020. We need at least two altimeters, maybe four. These won't be available soon. The first salinity satellite will not be flown until 2015.

Some Thoughts on Logistics, Mixing, and Power

*J. N. Moum**

I can think of several things that are likely to be true in 2025. Some are definitely not good for the field of oceanography, some offer debatable benefits and, as has always been the case, technical developments that have nothing to do with oceanography will offer the potential to revolutionize it. These raise some questions about our capability to observe and understand future ocean circulation.

We are on track to have fewer scientific research vessels in the near future and beyond. It also looks like we will be concentrating a significant portion of our observational and intellectual resources at a very few locations—observatories. At the same time, climate scientists have been telling us that, under reasonable warming scenarios, there may occur significant changes in ocean circulation. If significant changes occur, will we have the resources available to properly observe them?

I know that the wonderful satellite-based measurements we now all have access to will steer us toward shifts in major current systems, but they only tell us what's happening at the sea surface. And while the governing equations aren't going to change and numerical simulations are getting progressively better, they still don't deal with the subgrid scales very well, and they won't in 2025, or 2100 for that matter. As one example, mixing parameterizations that appear to work reasonably well in the Denmark Overflow do not work at the equator, even though scales of shear and stratification are pretty much the same. This is probably

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*College of Oceanic and Atmospheric Sciences, Oregon State University

because the scales at which the parameterizations are applied are simply not appropriate for predicting the mixing. At the equator there is clearly an important intermediary between the mixing scales and the resolved scales in the form of an energetic narrowband internal gravity wave field that we are only just beginning to understand.* In the event of a climate change-induced reorganization of ocean circulation, what happens if (for example) the vertical structure of the Gulf Stream intensifies to equatorial-strength shear and stratification? Does the mixing there need to be re-parameterized? Will we have the resources to properly measure this on the range of time and space scales necessary to determine the subsurface structure and how this influences what we observe from satellite? Or will we be spending our resources servicing fixed observatories elsewhere?

On a different note, technical developments over the past decade or so have allowed deployment of relatively inexpensive, high data-rate, battery-powered instrumentation for extended periods. These developments include low-power surface mount electronics (originally developed for the space program), and great improvements in battery capacity and data storage for the digital camera industry. We have found that we can power dual thermistors, pressure sensor, 3-axis accelerometers and compass on a TAO mooring for a year at data rates of 7 Mb/h. As part of our deployments of mixing meters on equatorial moorings, we have had to determine the motions in the frequency band 0.001-100 Hz. Any conventional mooring with a surface float is forced at surface wave frequencies. Is it possible to harvest this energy in order to extend deployment lifetimes?

Small, very efficient power generators have recently been developed to harvest biomechanical energy with the idea that this can be used to power prosthetic limbs and other portable medical devices. The demonstration was a generator built into a knee brace used to power an iPod™ (the generator was used only to assist the muscles in decelerating rather than requiring them to be an energy source). This delivered 5 W average, which is at least 20 times greater than needed for our mixing meters.

I guess it is possible to adapt small generators to take advantage of the available energy in mooring motions—the energy available is orders of magnitude greater than that from a decelerating knee joint. Perhaps gliders and other AUVs can take advantage of this while at the sea surface. This suggests the potential to extend deployments, an important factor for an agency like NOAA, for example, which is attempting to maintain its vast equatorial mooring arrays with reduced ship availability.

Related to this, long-range power transmission is grossly inefficient.

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*This brings up a separate but fundamental issue that has yet to be decided—does each flow require its own parameterization?

Many who have thought about reducing carbon emissions suggest that *local* power generation will play an important role. Cloud computing centers are being clustered near inexpensive power sources, such as Columbia River hydro-electric dams. Interestingly, Google™ has applied for a patent for ship-based computing centers, to take advantage of both the available cooling water as well as the energy available from the "natural motion of the water." It seems that there is considerable effort from industry to eliminate long-range power transmission; maybe small, efficient power generators will be of help to oceanographers. Cabled observatories require long-range power transmission. Wouldn't it be ironic if, once observatories are cabled, it won't be necessary to transmit power through those cables?

Ageostrophic Circulation in the Ocean

*Peter Niiler**

To understand ocean circulation from observations, large international projects have been undertaken in the past 50 years to measure the temperature, salinity and satellite sea level. These data are used to compute horizontal pressure gradients and the corresponding geostrophic circulation on 400 km scales over the globe and 1-10 km on scales locally. When direct near surface velocity observations are added to the momentum balance during this computation a 50 km spatial scale distribution of dynamically balanced sea level, or geostrophic circulation can be computed (Figure 1).

Every ocean GCM (OCGM) in operation today can 'smoothly' assimilate these temperature, salinity and sea level data and produce a 'geostropic' circulation from a balance of local horizontal pressure gradient and the Coriolis force over most of the water column.

But theory and observations both demonstrate that the circulation is not in geostrophic balance along lateral ocean boundaries, in straits and overflows and in the upper 200 m nearly everywhere. In this upper ocean column where momentum and vorticity is imparted by the wind stress, the principal exchanges of thermal energy and fresh water takes place and where most of the biological productivity occurs, the circulation is quite different as implied by the geostrophic contours of Figure 1.

A 'streak line' map of the 15 m-depth circulation can be constructed from the integration or drifter motion (Figure 2) that shows dramatic

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*Scripps Institution of Oceanography, University of California, San Diego

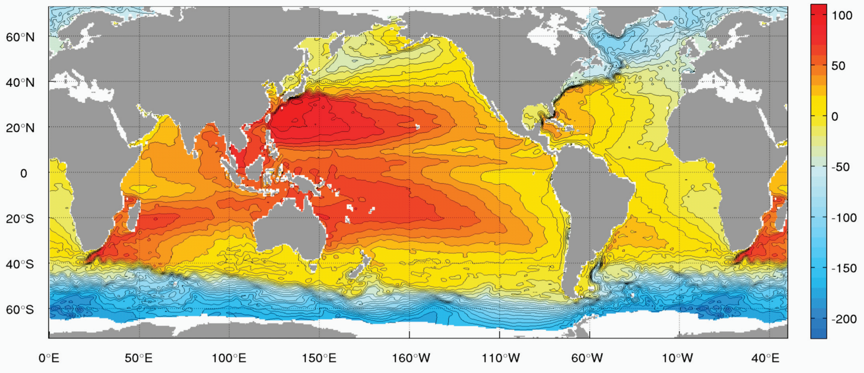

FIGURE 1 The 15 m-depth dynamic topography computed from application of the horizontal momentum balance to satellite sea level and drifter velocity observations. The geostrophic currents flow along contours of constant sea level, contoured in 10 cm intervals. From Maximenko et al., 2009.

departures from the geostrophic streamlines. The most notable is that while the geostrophic circulation moves water from the mid-latitudes toward the equator, the streak lines move water toward the pole. In the subtropical North Atlantic and North Pacific the ageostrophic velocity component to the north is at least twice as strong as is the geostrophic

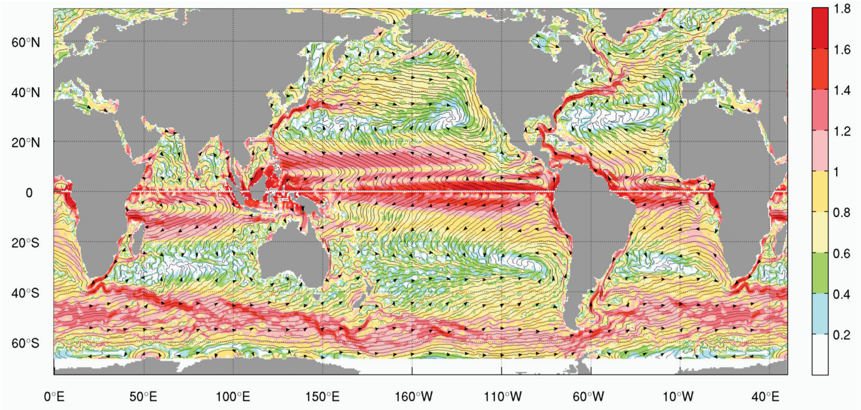

FIGURE 2 Streak lines of the 15 m-depth velocity derived from the Lagrangian motion of drifters. The shade indicated the speed on a logarithmic scale and black arrows mark the direction of flow. Note the large scale convergent vortices on the both the northeast Pacific (a well known region of plastic accumulation) and a similar, and more stable vortex, in the southeast Pacific (from where no water property data has been garnered). From Maximenko et al., 2009.

velocity component to the south. The vertical structure of the ageostrophic velocity is not well measured in general and each OGCM will produce a vertical structure depending upon its vertical and horizontal turbulence models and the spatial resolutions in which these are applied. The ageostrophic component of the horizontal circulation contains the largest horizontal divergence, and hence vertical velocities, and thus every model will present a different vertical circulation.

Secondly, the greater part of the ocean contains both stationary and transient mesoscale features that commonly have local Rossby numbers of 0.1-0.2. Model calculations demonstrate that in such circumstances it is not appropriate to construct a map of upper ocean circulation from the arithmatic sum of a local Ekman Current and local geostrophic current. An ageostrophic current, or a secondary three-dimensional flow pattern, results from the non-linear interaction of the local wind-forced flow and the vorticity structure of the underlying mesoscale (Figure 3). This verti-

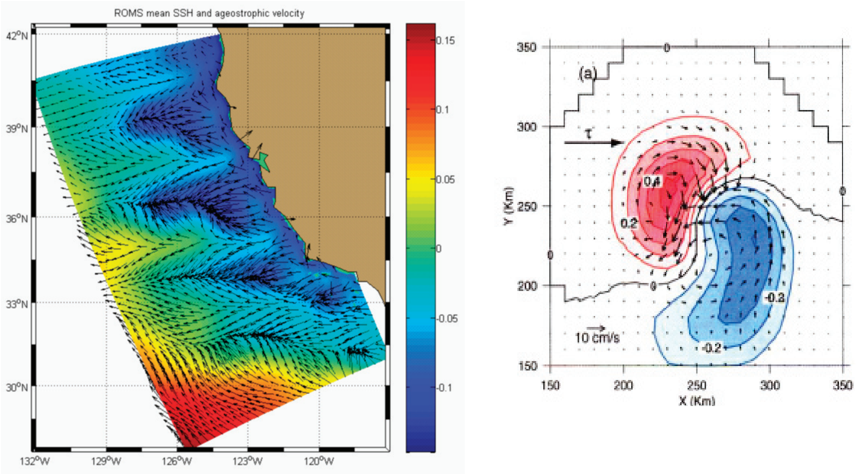

FIGURE 3 The 9-year mean sea level height and ageostrophic velocity at 15 m-depth of 5 km horizontal resolved Regional Ocean Model System (ROMS) of the California Current System (CCS) (left panel). As also observed, ROMS produces four semi-permanent cyclonic meanders of the sea level, or standing cold 'geostrophic' eddies in the CCS, even when driven with large scale COADS monthly mean winds. The ageostrophic velocity forms convergent and divergent patterns that are tied to the meanders. The right panel shows the ageostrophic surface velocity (black arrows) in a model of a symmetric cold eddy of the strength and vertical structure commonly observed in the CCS that is forced by a uniform wind. Note the similarity of the surface ageostrophic velocity in ROMS of the CCS and model of a single cold eddy-wind interaction. The contoured shades are changes in SST (C°) caused by this interaction. From Centurioni et al., 2008.

cal circulation, which extends to over 200 m depth, is produced from the ageostrophic horizontal velocity convergence. Model diagnostics show that this vertical circulation is a strong function of parameterizations of both horizontal and vertical mixing in the model.

Oceanography of 2025 will require observations and realistic modeling of the circulation patterns that contain the vertical motion of the upper 200 m. Models will be compared not by how well they assimilate or replicate the sea level or reproduce the geostrophic velocity, but rather by how their internal vorticity and thermal energy and fresh water balances maintain ageostrophic velocity structures and the associated vertical circulations. This task calls for development and implementation of continued new methods and instruments for direct velocity observations of the oceans.

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The Future of Ocean Modeling

Sonya Legg, Alistair Adcroft,* Whit Anderson,* V. Balaji,* John Dunne,**
Stephen Griffies, Robert Hallberg,* Matthew Harrison,* Isaac Held,**
Tony Rosati, Robbie Toggweiler,* Geoff Vallis,* Laurent White**

In this white paper we explore the possible trends in ocean modeling, to complement other white papers in this volume which focus on observational trends. Numerical models and the computers they run on are now integral to ocean science. Models can be used for both short term prediction and long term projection, as low cost testbeds for observational scenarios, and to integrate and synthesize observations, as well as to explore the fundamental physics and biogeochemistry of the ocean.

In the future, oceanography as a whole is likely to become much more applied, with increasing demand to apply oceanographic understanding to societal and commercial needs. Ocean models will likely be used by non-experts interested in a variety of different applications from coastal fisheries to storm surge prediction. Ideally the best ocean models available should be used—usually such models are created in the academic/public research sector, with an open-source structure which encourages continual improvement. To make these models accessible to applied users they will have to be more flexible and easier to use as black boxes, perhaps running on a remote computer through a web-based service. A technological development which would make models easier to tailor for different applications is the standardization of code to make model modules interchangeable. A user would then be able to take model components "off the shelf" and put together the right combination of

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*Atmospheric and Ocean Sciences Program, Princeton University dynamical core, parameterizations, and data assimilation infrastructure for a particular application.

In the past few decades great advances in ocean modeling have been enabled simply by the increase in resolution possible as computer power increased. It seems likely however that a wall is being reached in terms of the speed of individual processors. Increased resolution will now be achieved largely through the use of greater numbers of processors. Distributed computing, running different components of the model at different sites, is not likely to help increase resolution, due to the problems of latency. The addition of more components to the modeling system (i.e., biological and chemical, as well as physical) will require dramatically greater computational resources and improvements in data management.

With the likely limitations on raw computing power, new numerical techniques will be investigated to allow better representation of the ocean. An example is the use of unstructured meshes, which have now reached the stage of being used for idealized process study simulations. In 2025 there will likely be global circulation models employing such techniques, just as the once experimental isopycnal vertical discretization is now being employed in climate models today. However, it is unlikely that adaptive grids will be used in global climate models due to lingering concerns about their use with complex subgridscale parameterizations. Instead adaptive grids are more likely to be useful in local area models, such as in coastal storm surge forecasting and hurricane forecasting. Continued improvement in vertical coordinates and advection schemes will help to reduce numerical errors.

Techniques for bridging between global and smaller scales are likely to be in more widespread use in the future, so that for example global climate predictions can be applied to scales where user communities (e.g., ,fisheries) need information. These include techniques for nesting different models within global models and for locally enhancing the resolution of global models.

Data assimilation will become a more indispensible part of ocean modeling, including the coupled assimilation necessary for decadal predictions, and employing real time use of subsurface data (e.g., from autonomous platforms and other new observing technology as it comes on line). Continuation of observing and monitoring technologies which have proved their worth for oceanography and climate science (e.g., satellite altimetry, Argo profilers, TAO moorings) is essential for modern coupled data assimilation.

Model biases will likely continue to be a significant challenge for coupled assimilation as well as simulation and prediction, providing motivation for continued process studies. Process studies will likely focus on still smaller scales as model resolutions increase, and process studies will be strongly integrated with models. In turn model improvements are likely to be strongly linked to the improved physical understanding of processes provided by such process studies. As model parameterizations become more physically based, they will incorporate fewer arbitrary dimensional constants. The remaining non-dimensional constants will be determined from a combination of observations, laboratory experiments and LES modeling. In this way model credibility will no longer be confined to one realization (e.g., the current climate) and simulations of both future and paleo scenarios will become more credible.

In the future, observational oceanography will likely make much greater use of models in planning of observing programs and interpretation of observational data. Real time communication between models and observing systems will become more important, for example, in using model predictions to guide intelligent observing platforms.

KEY SCIENCE QUESTIONS

As global ocean modeling becomes both more interdisciplinary and higher resolution, questions such as the role of mesoscale eddies in biogeochemical cycles, air-sea interaction and climate will be able to be examined. New observations such as global measurements of the spatial and temporal variability of turbulent mixing, measurements of currents and mixing under ice sheets, and continuous measurements of the fluxes into and out of geostrophic eddies, will stimulate modeling studies of the role of the ocean on the ice sheets, and the importance of tidal mixing and mesoscale eddies to the global circulation. As models begin to employ parameterizations without tunable dimensional parameters they will be able to be applied to paleooceanographic problems such as deglaciation and CO_2 variations. With a longer observational record, better coupled model initialization systems and higher resolution coupled models, we will have a much better, although probably still incomplete, understanding of the processes involved in decadal variability.

EDUCATIONAL NEEDS

New trends in ocean modeling will require more recruits with training in computational fluid dynamics techniques, as well as software engineers who can develop the web interfaces to make models accessible to a broader user base. More scientists well versed in interdisciplinary work, at the interface between physical and biological oceanography, will be needed. At the same time the academic and research community will need to learn to communicate with applied scientists using models for practical purposes. One recruitment strategy is to encourage teaching of upper-level oceanography classes to physical science/engineering majors, something which will require the cooperation of the faculty of those departments. All these needs have to be balanced against maintaining rigor in fundamental oceanographic understanding, and the reward structure of the research and academic institutions.

Towards Nonhydrostatic Ocean Modeling with Large-eddy Simulation

*Oliver B. Fringer**

Ocean models are limited in the physics they can resolve by the available computing resources and the available time a user has to wait for a result. At one end of the spectrum, resolution is compromised in favor of speed and efficiency in order to obtain predictive solutions faster than real time. For example, a coupled ocean-atmosphere hurricane prediction model must run much faster than real time if predictions of hurricane tracks are to inform the public on potential safety hazards. In these large scale models, grid resolution dictates the physics that can be resolved and in turn determines the physics that must be parameterized. In general, a coarser grid leads to a faster prediction, but at the expense of resolving less physics and placing more emphasis on the underlying parameterizations of unresolved processes. Most ocean models require parameterizations of unresolved physics and are known as unsteady Reynolds-averaged (URANS) simulations, whereby the higher-frequency motions are filtered out of the governing equations and parameterized.

At the other end of the spectrum of ocean models are those that resolve as much of the processes as possible with grid resolutions that are dictated by available computing resources. If all of the turbulence is resolved by the grid, the simulation is termed a direct numerical simulation (DNS) and no parameterizations are required. DNS is prohibitively expensive because the number of grid cells required in a three-

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*Stanford University

dimensional simulation is given by $Re^{9/4}$, where $Re = UL/\nu$ is the characteristic Reynolds number and U and L are characteristic velocity and length scales, respectively. As an example, a DNS of a breaking internal wave for which $Re = 10^6$ ($U = 0.1$ m s^{-1}, $L = 10$ m) would require roughly 3×10^{13} grid points, or 10,000 Tb of memory in a typical simulation code, an intractable problem at least for the next two decades. Because the rate at which energy is lost to dissipation can be considered uniform over scales within roughly one order of magnitude of the smallest dissipative scale, it is possible to relax the resolution requirement and perform a so-called large-eddy simulation (LES) under the premise that a bulk of the energetics is accounted for by the large, energy-containing eddies, while the smaller, or subgrid-scale eddies, can easily be parameterized without incurring significant parameterization errors. The fundamental difference between URANS and LES is that LES reverts to DNS with enough grid refinement, while the parameterization in URANS is independent of the grid resolution. While LES is much more tractable than DNS, LES of ocean processes is still computationally intensive. For example, LES of the turbulent dynamics in an upwelling front over one inertial period in a 1 km by 1 km by 500 m domain using 1 m resolution (sufficient for LES) would require a simulation with 500 million grid cells using 512 processors on a supercomputer for roughly two weeks.

In addition to the computational requirements associated with a large number of grid cells, LES models must also compute the nonhydrostatic pressure, which roughly doubles the computational overhead relative to a hydrostatic simulation. Strictly speaking, all LES models must be nonhydrostatic. However, all nonhydrostatic models do not necessarily require LES, since numerous problems require solution of the nonhydrostatic pressure without requiring the grid resolution associated with LES (internal solitary-like waves, for example, are simulated well with nonhydrostatic URANS formulations). While some ocean models incorporate LES-style parameterizations for horizontal turbulent fluxes, there are no ocean models based strictly on the LES formulation for turbulent fluxes in all three directions, since all models possess some form of a RANS-type parameterization for the unresolved vertical turbulent fluxes, particularly at solid boundaries and at the free surface.

The most optimistic projections of supercomputer performance based on Moore's law and recent trends in parallel computing indicate that parallel computer performance will increase by a factor of 10,000 by 2025. This implies that the highest-resolution simulations of oceanic processes will increase by a factor of 100 in each direction if the grid is refined only in the horizontal, while three-dimensional simulations could be increased by a factor of 20 in each coordinate direction. Even with these substantial increases in grid resolution, it is unlikely that LES will take center stage in the ocean modeling community, since an increase in the grid resolution by an order of magnitude in all three directions will still not enable LES-type simulations that fare better than URANS parameterizations in regional and larger scale ocean models, particularly at boundaries. While regional scale hydrostatic URANS models will likely not be converted to LES models, the increase in computer performance will lead to an increase in the speed of the smaller-scale nonhydrostatic URANS models, and these models could potentially be run as LES-type models. For example, at present, it takes two months for a nonhydrostatic URANS model to compute the internal wave field in Monterey Bay over a fortnight with a resolution of 20 m (using unstructured grids). In 2025, if the projections are correct, it may be possible to compute this same problem with a resolution of 1 m in each direction, which would certainly come close to LES. Even with this grid resolution, however, turbulent fluxes at the free surface and bottom boundaries will still need to be parameterized using URANS-type parameterizations.

Because advances in computer power by 2025 will likely not lead to a regional scale LES-type ocean modeling capability, and beyond that time it is not clear whether computing power will continue to increase at a rate that justifies implementation of LES-type ocean models, the ocean modeling community will need to focus more on model nesting and coupling, whereby higher grid resolution is achieved via nesting of progressively finer grids within a simulation, and the grids are coupled to one another via transfer of information at the boundaries or within the individual grids. Although one-way nesting is quite common in ocean modeling, the notion of two-way coupled ocean modeling is far from mainstream and few models incorporate real two-way nesting. With increased computer power, the refined grids in two-way coupled simulations will run as LES-type models and hence will require less parameterization, and these will in turn provide the necessary subgrid-scale fluxes to the coarser grids and will effectively act as the turbulence parameterizations for those grids. Ultimately, only parts of a regional-scale domain will be resolved as an LES, while the remainder will continue to run using URANS parameterizations. These parameterizations will in turn have significantly improved by 2025 due to findings from highly resolved LES and DNS process studies.

In summary, computing power will advance enough by 2025 such that nonhydrostatic, large-eddy simulations of coastal-scale problems may be tractable. However, computers may never be fast enough to achieve large-eddy simulations of regional and larger scale oceanic domains. The alternative is to focus on two-way model coupling that will enable subgrid-scale physics as computed by LES-type models in specific regions of interest to be fed back into the larger-scale URANS models.

Simulations of Marine Turbulence and Surface Waves: Potential Impacts of Petascale Technology

*Peter P. Sullivan**

PETASCALE COMPUTING

Large scale parallel computing is a potential boon to the scientific interests of the geoscience communities and in particular oceanography. Over the next decade, computing systems will routinely attain peak speeds of one petaflop and more (1015 floating point operations per second) with memory capacities of order one petabyte (1015 bytes of information). This is at least an order of magnitude increase compared to the current generation of parallel machines (e.g., the IBM SP6 and Blue/Gene machines, see http://www.top500.org/ for current trends in supercomputing as well as the current ranking of the most powerful machines). An example of a peta-class system under development is the “Blue Waters” Project (http://www.ncsa.uiuc.edu/BlueWaters) being pursued by the National Center for Supercomputing Applications. Blue Waters aims to achieve sustained petaflop performance utilizing 106 computational cores and is projected to come online by 2011. By embracing petascale computational technology (recent developments in both hardware and software are described at http://www.image.ucar.edu/Workshops/TOY2008/focus2) early in its development phase oceanography will be in an advantageous position to advance its science and applications as even more powerful computational systems are developed.

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*University Corporation of Atmospheric Research

SCIENCE AND APPLICATIONS

Current computational systems and algorithms allow DNS and LES of turbulent boundary layers utilizing $O(1024^3 \sim 2048^3)$ gridpoints (see Figure 1). The majority of these calculations are posed in idealized settings with imposed external forcings (i.e., with no feedback or coupling to the larger scales). These calculations are interesting and enlightening, but do not adequately capture the full set of scale interactions. Future petascale computations are likely to use as many as $O(10{,}000^3)$ gridpoints and thus couple a wider spectrum of scales. These meshes will allow DNS and LES of marine boundary layers with higher Reynolds number incorporating numerous physical processes in larger domains. Strongly stable boundary layers (see Figure 2) are an important flow regime not currently addressed by either DNS or LES. Weak highly intermittent turbulence cannot be sustained in low Reynolds number DNS and in the current generation of LES with subgrid-scale (SGS) models that are too dissipative to capture the stochastic dynamics of high Reynolds number strongly stratified turbulence. At least an order of magnitude increase in Reynolds number along with further developments in SGS models are required to increase the fidelity of DNS and LES of stable boundary layers. Increases in computer power will also allow turbulent simulations over 3D topography, moving surface wave fields, and ocean boundary layers driven by hur-

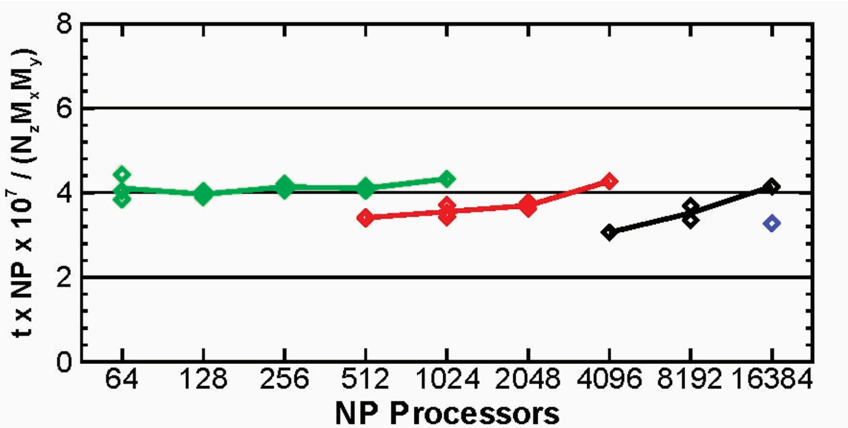

FIGURE 1 Computational time per gridpoint for LES of a convective atmospheric boundary layer on a Cray XT4. a) left line and symbols problem size 512^3; b) center line and symbols 1024^3; c) right lines and symbols 2048^3; and d) right diamond symbol 3072^3. The parallelization is accomplished using 2D domain decomposition and the Message Passing Interface (MPI), results from Sullivan and Patton (2008).

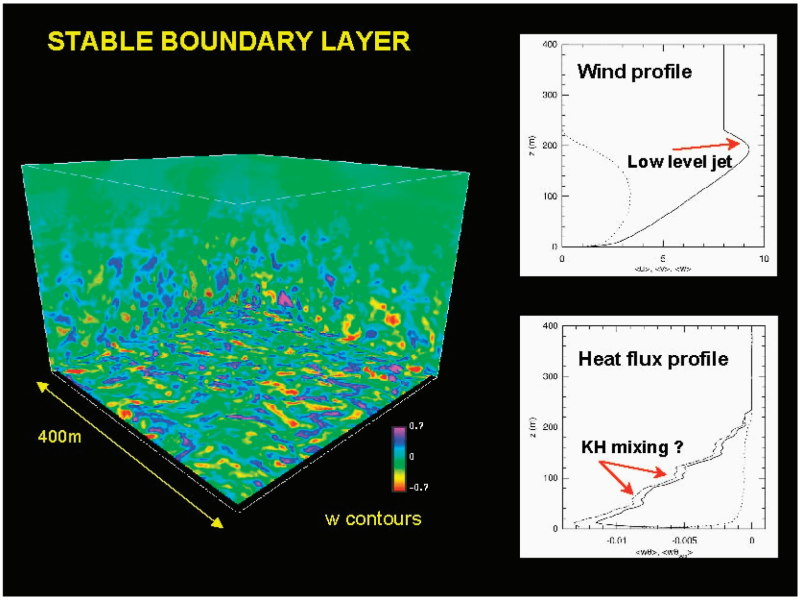

FIGURE 2 LES of a nocturnal stable (atmospheric) boundary layer with mesh resolution ~ 2 m in all directions. The boundary layer depth z_i ~ 200 m and the stability measure z_i/L ~ 1.2 indicates a weakly stable regime (L is the Monin-Obukhov length). The 3D visualization of the vertical velocity field shows that the flow is dominated by numerous small scale structures. The vertical profile of the mean wind shows the formation of a super geostrophic jet near the top of the boundary layer and the stair-step structure in the vertical heat flux profile is suggestive of Kelvin-Helmholtz overturning. Strongly stable boundary layers, $z_i/L > 2$, with intermittent turbulence are not adequately simulated with current LES and DNS.

ricane winds (see Figure 3). All these simulations will ultimately improve climate and weather forecasts.

Air-sea interaction and in particular the coupling of winds, waves, and currents at scales ranging from centimeters to kilometers is a fundamental scientific problem that is also likely to benefit from petascale computing. In order to more faithfully capture the dynamics of air-sea interaction the community should devote energy towards developing large-wave simulation (LWS) technology. LWS is viewed as a more complete cousin to today's LES. A 3D time-dependent LWS model of the air-water interface would naturally capture interactions between winds, waves, and currents. An LWS model is cast in physical space by applying a spatial filter to the governing Navier-Stokes equations for an air-water medium including the proper kinematic and dynamic boundary condi-

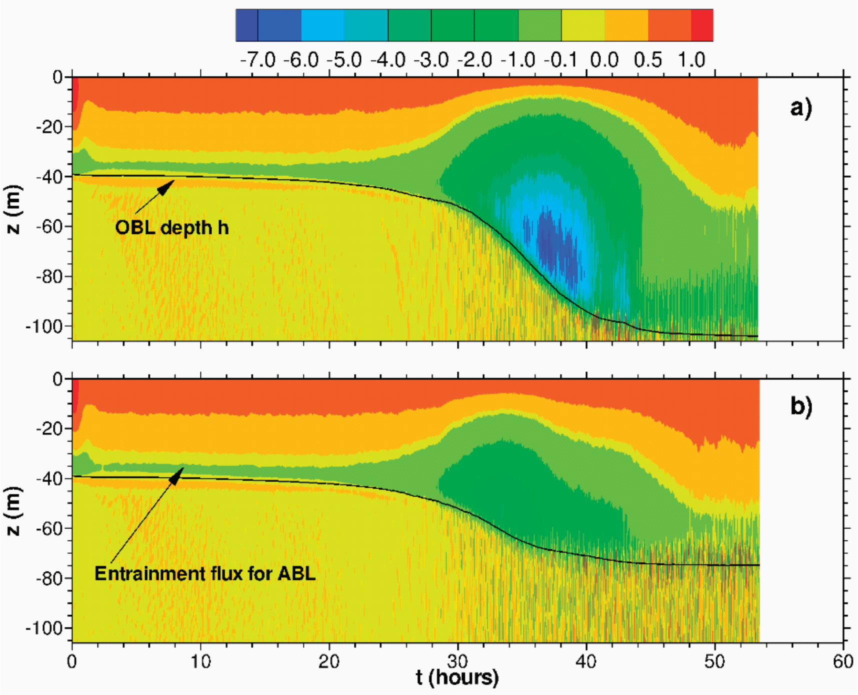

FIGURE 3 Variation of the vertical profile of turbulent scalar flux ‹$w'\theta$› (z,t) from LES of an ocean boundary layer driven by Hurricane Frances winds and stresses. LES domains are located on the resonance (panel a) and non-resonance (panel b) sides of the storm track. At each time, the scalar flux is normalized by its imposed surface value. In these figures the ocean boundary layer (OBL) depth h, determined as the location of the maximum vertical temperature gradient, is shown as a heavy black line. The arrow shows the normalized entrainment flux (~–0.2) for a classical daytime convective atmospheric boundary layer (ABL). The contours of scalar flux show that the bulk of the temperature decrease in the ocean boundary layer is induced by entrainment cooling. The rapid oscillations in the scalar flux below the thermocline result from a complex system of internal waves excited by the strong wind forcing. The LES mesh is 500 × 500 × 160 gridpoints and the timestep Δt varies from 15 s to 0.5 s over the length of the simulation. The total number of timesteps > 150,000.

tions at the air-water interface similar to LES. The filtering step introduces new unknown SGS terms, both turbulence-turbulence and turbulence-wave correlations, that need to be parameterized in terms of resolved winds, waves, and currents. Modeling these correlations is a challenging task and requires guidance from both laboratory and field measurements-spatial measurements of turbulence and waves in two different media are needed to validate and construct the needed SGS parameterizations. High resolution idealized DNS will be used to gain insight into the variations of these SGS correlations at lower Reynolds numbers. Efficient (and parallel) Poisson solvers are required to make LWS viable.

LWS will replace the current generation of spectral wave models which are largely built with heavy doses of empiricism for the wind input and dissipation source functions. LWS will answer fundamental questions as to how waves grow and the dependence on wave age and wave slope for a spectrum of waves. LWS might also prove useful in hurricane simulations. Also, LWS will permit testing of the fundamental interactions between waves and currents (e.g., Craik-Leibovich asymptotics can be tested). It will also provide insight into wave breaking dynamics, in particular the intermittent spatial and temporal distribution of breaking, and the generation of currents. A key aspect of LWS is the ability to integrate the equations of motion over sufficiently long periods so that waves grow, break and interact (i.e., the integration period is sufficiently long to permit significant wave-wave interactions). LWS will be complementary to simpler free surface calculations and will shed light on the important aspects of gas transfer related to micro-scale breaking.

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Computational Simulation and Submesoscale Variability

*James C. McWilliams**

In the way of an oracle, I offer the following remarks about the future of physical oceanography:

> Because of broadband intrinsic variability in currents and material distributions and because of the electromagnetic opacity of seawater, the ocean is severely undersampled by measurements and likely to remain so. (Surface remote sensing makes a wonderful exception.) The most important instrumental advances will be ones that improve on this situation.

Computational simulation of realistic oceanic situations is steadily growing in capacity and usage. Given the first remark, this is a very good thing. It supports a dual strategy of using measurements to inspire and test models, and using models to design experiments and extend the scope of measurements. So far, planning for field experiments that embody this duality is still rarely done well. In 1997 NSF convened a similar futurism workshop (APROPOS), and I contributed a white paper describing practices and trends in numerical modeling that still seems apt (McWilliams 1998). I would now add two further remarks. First, accumulating experience supports the hypothesis that such simulations—even if sometimes remarkably like nature in their emergent patterns, phenomena, and multivariate relationships—have an inherent, irreducible imprecision compared to measured quantities in turbulent (chaotic) regimes (McWilliams 2007). This extends to irreproducibility among different

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*University of California, Los Angeles

model codes putatively solving the same problem (*n.b.*, the persistent spread among global-warming simulations). The imprecision is due to the model composition with its non-unique choices for numerical algorithms, parameterizations, and couplings among different processes. Testing and digesting this hypothesis and acting on its implications are strongly recommended. Second, there is a serious, unsolved infrastructure problem in oceanic modeling, *viz.*, how to increase and depersonalize model documentation, calibration, and availability in support of widespread usage without impeding the necessary, continuing evolution of what is still a young technology. How many published model results are reproducible by a reader, hence verifiable? How can we facilitate the interfaces between model makers and users? How can anyone other than the IPCC go about deploying an ensemble of different models to understand the spread of their answers for a range of problems? (Even the IPCC's is an inadvertent ensemble.)

For reasons that have a lot to do with measurement undersampling and model immaturity, oceanography is only now moving into a bloom of discovery about distinctive types of variability within the submesoscale regime (10s–1000s m; hours–days). This is an awkward scale regime for the usual measurements: small compared to most remote-sensing footprints; large compared to a ship's range; and subtle to distinguish from inertia-gravity waves in point time series. There is an emerging, provisional paradigm for non-wave submesoscale variability. Its primary energy source is mesoscale eddies and currents, which confounds the theoretical (and computationally confirmed) prediction of up-scale energy transfer in geostrophic, hydrostatic flows. It is manifest in frontogenesis, frontal instability, coherent vortices (including the notorious "spirals on the sea" often seen in reflectance images but never measured *in situ*), "mixed-layer" instability, unstable topographic wakes, "arrested" topographic waves, ageostrophic instability of geostrophic currents, spontaneous wave emission by currents, temperature and material filaments, horizontal wavenumber spectra with shallow slopes, probability density functions with wide and skewed tails (e.g., near-surface cyclonic vorticity and downwelling velocity), and acoustic scattering patterns of lenses and layers (e.g., in geoseismic surveys). It affects a forward cascade of both kinetic and available potential energy and thus provides a route to dissipation for the general circulation (via mesoscale instability) that is probably globally significant. This cascade supports a microscale interior diapycnal material mixing that sometimes may be competitive with breaking internal waves. It also induces density restratification (an apparent vertical unmixing!) that is especially effective around the surface mixed layer. It provides important material transport between the surface mixed and euphotic layers and the underlying pycnocline and nutricline, and it sometimes provides important horizontal transport. As yet, only a few flows have been simulated, only a few theories devised, and only a few regions measured for their submesoscale variability. This family of phenomena deserves a lot of attention in the coming decades.

The disciplinary borders of physical oceanography are increasingly indefensible with respect to both scientific content and the education and recruitment of new researchers. This view should be embraced in our institutional homes.

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Ocean Measurements from Space in 2025

*A. Freeman**

OVERVIEW

Ocean measurements from space have advanced significantly since the first sensors were flown on NASA satellites such as Seasat in the 1970s. New technologies have opened the door to new, unforeseen scientific questions and practical applications, which, in turn, have guided the next generation of technology development—a fruitful, mutual coupling between science and technology. Recent advances in modeling of ocean circulation and biochemistry are now also linked to improved measurement capabilities.

Since the ocean is largely opaque over much of the usable electromagnetic spectrum, ocean measurements from space are largely confined to surface properties such as SSH, SST, surface wind vectors, sea surface salinity (SSS), ocean color, and surface currents. In some cases properties of the ocean beneath the surface can be inferred from such measurements, the most striking example being the determination of bathymetry from sea surface height measurements made by altimeters. Measurements of variations in the Earth's gravity fields (e.g., by NASA's Gravity Recovery and Climate Experiment [GRACE]) mission are somewhat a special case, and have been used to infer ocean bottom pressure, for example.

With the release of the 2007 decadal survey for Earth Science and

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*California Institute of Technology

Applications from Space (NRC 2007), we are poised on the brink of a series of improvements in ocean measurements from space that will revolutionize oceanography from space in the next decade—as big an advance or bigger than the advent of ocean altimetry with TOPEX/Poseidon. This white paper looks forward to that timeframe and beyond, towards the type of measurements we should expect in 2025, and the science questions that we should be able to ask.

OVERARCHING SCIENCE QUESTIONS

The scientific and practical questions that are likely to drive developments in the 2025 timeframe are:

- Oceans as part of the coupled ocean-atmosphere-ice-land-biogeochemical system
  - Most current ocean models that assimilate data are presently run in 'forced' mode; they do not affect the atmosphere. Most atmospheric models that assimilate data use an overly simplified representation of the oceans (a mixed layer) or worse, only sea surface temperature. These are due to computational expense, a barrier that is fast receding.
  - CO_2 uptake, hurricanes, ENSO, ice shelf disintegration and ice sheet advance are all examples where the coupling between the ocean and in these cases either the atmosphere or the cryosphere are critical.
- Description and prediction of the global water cycle in the context of global climate change can only be fully realized when the marine branch of the hydrological cycle is considered.
- Increased spatial and temporal resolution in ocean observations, ocean models, and climate models.
  - Spin up / spin down time scales in the oceans depend on eddy (~ 100 km or less) parameterization. These time scales are essential for climate forecasts. Thus climate models need to resolve or parameterize properly ocean eddies for realistic climate simulations (Marshall, personal communication, 2008).
  - In ocean models, dissipation of momentum is achieved through enhanced vertical viscosities and drag laws with little physical validation. Turbulent transport of tracers like heat, salt, carbon and nutrients is represented with unphysical constant eddy diffusivities in numerical ocean models. Ocean models running at sufficient resolutions to address submesoscale (1-100 km) dynamics have just begun to emerge (Capet et al. 2008).

  Global observations at these scales are needed to constrain the models.
  - For coastal work, forecasting for navigation, inundation, and marine resources critically depends on short length scales, controlled by the shallow ocean depth.
- Need to forecast with increasing accuracy and shorter time delays both short time scales (navigation, harmful algal blooms) and long ones (climate) for societal benefit.

To address these questions, we believe a progressive improvement in measurement capability is necessary, across a broad range of parameters, as outlined in Table 1 and the discussion below.

TECHNOLOGICAL ADVANCES

The kind of technology advances that will enable the improvements in ocean measurements from space described above include:

- Miniaturized, more efficient radar components to reduce mass/power needs of radar electronics
- Efficient, high-power transmitters at shorter wavelengths (especially Ku- and Ka-Band)
- Increased onboard processing and/or downlink capability, allowing data acquisition at higher spatial and temporal resolution.
- Larger deployable antennas in the 6-12 m range, particularly employed in a conical scan mode, enabling higher resolution radiometry and scatterometry.
- A scanning interferometer pair of antennas, rotating through an azimuth scan of 360 degrees to provide along-track interferometry measurements of surface currents at high resolution.
- Precision formation flying, to enable bistatic wide-swath sea surface height measurements from two platforms flying in formation, and gravity measurements from multiple platforms.
- Laser interferometry to improve the accuracy of gravity measurements from future GRACE-like missions.
- Wide field of view imaging spectrometers with improved stability and signal to noise (SNR) and atmospheric correction capabilities, enabling global ocean biosphere measurements at moderate resolution (~1 km) on a daily basis and on fine resolution (60 m) on synoptic basis.
- Deployment of ocean color imagers on geostationary platforms to sample the highly dynamic processes of coastal ecosystems.
- The spaceborne implementation of active remote sensing of bio-

TABLE 1. State of Ocean Measurements from Space in 2009, in 2017, and in 2025

Measurement from Space	2009	2017[a]	2025
Sea Surface Height (SSH)	100 km spatial scales; 10 day revisit (TOPEX/Jason series)	10 km spatial scales; 10 day revisit (SWOT)	10 km spatial scales; 1 day revisit (Multiple SWOT satellites)
Ocean Vector Winds (OVW)	25 km spatial scales; 1-2 day revisit (Quikscat/ASCAT)	3-25 km spatial scales; 6 hour revisit (XOVWM/ASCAT/Oceans II)	3-6 km spatial scales; 6 hour revisit (XOVWM and follow-ons)
Sea Surface Salinity (SSS)	0.2 psu; 150-200 km spatial scales; 30 day time scale (SMOS 2009; Aquarius in 2010)	0.2 psu; 40 km spatial scales; 10-30 day time scale (SMAP)	0.1 psu; 20 km spatial scales; 7 day time scale (Aquarius follow-ons)
Surface Currents	Geostrophic only—100 km spatial scales; 10 day revisit (Topex/Jason series) No ageostrophic.	Geostrophic cf. SSH Ageostrophic in coastal zones—< 1 km (DLR Tandem-X)	Geostrophic cf. SSH Ageostrophic globally—< 1 km; 1-2 day revisit (Scanning ATI)
Gravity	400 km spatial scales; monthly updates (GRACE)	Improved precision; 400 km spatial scales; monthly updates (GRACE II + GOCE)	Improved precision; <400 km spatial scales; weekly updates (GRACE follow-ons)
CO_2 Flux at the Surface	1000 km spatial scales; 0.4 $gCm^{-2}yr^{-1}$ flux; monthly updates (AIRS/OCO)	100 km spatial scales; 0.4 $gCm^{-2}yr^{-1}$ flux; weekly updates; day/night (ASCENDS)	10 km spatial scales; 0.1 $gCm^{-2}yr^{-1}$ flux; daily updates (ASCENDS follow-ons)

continued

TABLE 1. Continued

Measurement from Space	2009	2017[a]	2025
Sea Surface Temperature (SST)	1-2 km spatial scales; 1-day revisit; no visibility thru' cloud (MODIS) 40 km spatial scales; 1-day revisit; all-weather (AMSR-E); DT = 0.3 – 0.7 K	1-2 km spatial scales; <1-day revisit; no visibility thru' cloud (VIIRS on NPOESS) 40 km spatial scales; 1-day revisit; all-weather (AMSR-E); DT < 0.3 K	1-2 km spatial scales; <1-day revisit; no visibility thru' cloud (VIIRS on NPOESS) 1-2 km spatial scales; 1-day revisit; all-weather (Next-gen Microwave radiometers); DT < 0.1 K
Ocean Color/ Biogeochem	< 1 km spatial scales; 1 day revisit (CZCS/Seawifs/ MODIS/MERIS)	< 1km spatial; <1 day revisit (VIIRS on NPOESS) 50 m spatial; 17-day revisit (HyspIRI) .25 km spatial; 15 min revisit (GEOCAPE) 1 km spatial; 1 day revisit (ACE)	.1–1 km spatial scales; 15 min revisit globally (GEO network)
Sea Ice Area and Type	3 day revisit (Radarsat/ Envisat)	1 day revisit (DESDynI)	1 day revisit (DESDynI follow-ons)
Sea Ice Thickness	Freeboard @ < 1 km scales (Icesat II + Radarsat/Envisat) Snow accumulation (Cryosat)	Freeboard @ < 1 km scales (Icesat II + DESDynI) Snow Accumulation (TBD)	Ice thickness @ < 1 km scales (TBD)

[a] The projections for 2017 assume that the relevant missions in the National Research Council's decadal survey for Earth Science and Applications from Space (NRC 2007) are implemented on schedule.

chemical constituents of the ocean, including fluorescence spectroscopy instruments (UV/visible) and lasers at the blue end of the visible spectrum to measure mixed layer depth, as input to biochemical models.

- Increased computational power will allow more complex coupled models to be run at higher resolutions, and data assimilative models to assimilate data.

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Future of Nearshore Processes Research

*Rob Holman**

BACKGROUND

The nearshore, generally defined as depths less than 10 m, is an energetic, wave-forced region whose dynamics are driven by the propagation of a random wave field over a shoaling bathymetry. The bathymetry, in turn, responds to these overlying wave motions, introducing a strong feedback and resulting rich system behavior such as complex sand bar systems. Predictions can be partitioned by time scale. Nowcasts, for which bathymetry is unchanging, are a physical oceanography problem with the mobility of the sediments introducing only small boundary layer effects. Predictions of the short-term system evolution of a specific bathymetry, akin to short-term weather forecasts for the atmosphere, can be carried out using coupled models of fluid and sediment response. Predictions for time scales beyond the prediction horizon (perhaps weeks), akin to the climate case, are not simply achievable through integration of weather models.

COMPLICATING FACTORS

Progress has been slowed by several characteristics of the nearshore problem:

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*College of Oceanic and Atmospheric Sciences, Oregon State University

- Time scales of important processes span about ten orders of magnitude from interannual to breaking- or bottom-induced high frequency turbulence.
- The location of the bottom, a sensitive boundary condition for wave dynamics, changes at O(1) on time scales of days.
- Feedbacks between fluid motions and bathymetry are strong, driving the formation of patterns ranging from bottom ripples to rips channels and complex sand bars.
- The response time scale of sand bars, days to weeks, is somewhat longer than those of external wave forcing so that the system is constantly in dynamic pursuit of equilibrium.
- Depth goes to zero within the domain creating a singularity by definition.
- *In situ* sampling in the nearshore is difficult due to the harsh climate and rapidly changing bathymetry.

DIRECTIONS OF PROGRESS

For time scales shorter than the prediction horizon, progress will involve improvements in measurement capabilities, in dynamics and in data assimilation procedures.

Due to the harsh nature of the environment and the rapid evolution of variables such as bathymetry, remote sensing will play a growing role in both research and applications. A renewed focus on the physics of electromagnetic scattering from the surface and interior will allow us to exploit previously empirical relationships between remote sensing signatures and geophysical variables, some of which will have no *in situ* measurement analog. For example, research into the dynamics of breaking-induced bubble populations and their signatures to optical, infrared and radar polarimetric sensors will allow estimation and understanding of nearshore radiation stress gradients, the primary driver of nearshore flows. Multi-sensor methods will be developed that exploit variations of response among sensors to improve measurement capabilities. For example, breaking waves, foam and a non-breaking sea surfaces all yield different signals at optical, infrared and radar frequencies with additional differences depending on polarization.

With the explosive growth of unmanned aerial vehicles (UAVs), there will be a proliferation of available platforms for overhead remote sensing. Improvements in small navigation systems, in miniaturized sensors and in light-weight computing will make UAV-based imaging very powerful once air traffic control and image co-registration issues are solved. Research methods developed for fixed camera systems like Argus for remotely measuring currents, wave spectra and evolving bathymetry will become operational for mobile platforms like UAVs and will be key to operational predictions.

The rapid commercial sector improvements in computing power, particularly in small packages with powerful object-oriented toolboxes, will allow substantial improvements in intelligent instrumentation. Imaging sensors will become smart and situationally aware, automating many of the tedious details such as distortion, gain correction, georeferencing and the calculation of derivative image products such as polarimetry images. Networks of sensors will be integrated easily.

Increasing computational power will also benefit *in situ* instruments. However, the logistics of deploying and maintaining instruments in the nearshore will always be daunting and we will likely see a growth in the use of small, cheap Lagrangian sensors that could measure surface waves and flow, bottom boundary physics and potentially depth. Water column tracer use will continue to expand and we will continue to learn more from infrared signatures.

The explosive growth in computing power will have obvious payoffs to nearshore modeling work. Previously parameterized processes will be increasingly resolvable and run-time reductions will allow greater use of ensemble-based methods. Recognizing that the limitations in nearshore predictive capability lies more with limited data and nonlinear feedback behavior than with limitations in understanding (excepting the dynamics of wave breaking), there will be major progress in data assimilation methods, particularly those that work with the remote sensing data that is increasingly available. Methods should be developed that explicitly exploit non-traditional measurements such as the width of the surf zone.

Hopefully we will discover simplifying principles to some of the vexing components of the nearshore problem. For example, bottom bed roughness may respond to overlying flows according to some macroscopic principle that simplifies bottom stress calculations (akin to turbulence principles). However, unlike turbulence, our models will need to recognize that time-variations in forcing mean that we are always in pursuit of equilibrium (if equilibrium states even exist). Overall, our largest problem is learning to deal with coupled feedback systems and their resulting complex behavior. We will need to discover appropriate statistical variables, for example to represent complex sand bars simply, and we will need to determine to what extent variability is a consequences of the basic feedbacks and is robust rather than sensitive to details in the physics.

Future Directions in Nearshore Oceanography

*H. Tuba Özkan-Haller**

ASSUMPTIONS ABOUT THE STATE OF AVAILABLE TECHNOLOGY AND THEIR EFFECT ON RESEARCH METHODS

The thoughts outlined herein are based on a few assumptions about the state of resources available in 2025. These assumptions affect oceanography as a whole, although I will discuss nearshore oceanography in particular. Note that within this context, the nearshore region includes any part of the ocean that is affected by the presence of surface gravity waves. It is assumed that between now and ~2025,

- Increases in computational power will continue, though the architecture may be dictated by the entertainment industry. Advances in wireless communication technologies and increases in available bandwidth are also assumed to continue, although paradigm shifts in the way scientific computations are carried out or data is gathered may need to occur to take advantage of all advances. Nonetheless, herein it is assumed that computational power or bandwidth issues are not the limiting factors.
- Remote observations (video, radar, infrared [IR], LIDAR, etc.) will mature over the next few decades and will provide high resolution synoptic observations. These will likely produce standard data products (perhaps similar to satellite data products) available to the research community.

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*College of Oceanic and Atmospheric Sciences, Oregon State University

- Numerical models that were developed over the last decade will mature to the point where routine predictions (similar to weather predictions) can be made (e.g., waves at navigational inlets).

In the past, the development of high performance computing was significantly influenced by the needs of the scientific enterprise. Recently, advances in computational power have been driven primarily by the entertainment industry through the popularization of video games and the associated advances in graphics cards. This trend will likely become more pronounced; hence, the scientific community will need to adapt to the available new technologies through the use of different programming platforms.

As remote sensing technologies mature, they will likely begin producing standardized data products and the raw data may not be available to the scientist. However, the products will likely be available to a broad cross-section of the scientific community.

Finally, routine forecasts of local wave conditions are just now coming online although the accuracy and reliability of such predictions still needs to be rigorously assessed. Nonetheless, it is likely that local wave and circulation forecasts (e.g., waves near navigational inlets, rip current forecasts at selected beaches) may become routine over the next few decades. Such efforts will produce long term data sets of model predictions that may be readily accessed by the scientific community.

Note that all these potential changes will enable rapid advancements in the science. However, they may necessitate paradigm shifts in scientific programming, data processing and use, and may require targeted investment of time and funds.

FUTURE DIRECTIONS

Discipline-based research will likely continue as part of all oceanographic subfields. Within nearshore oceanography, such work will likely be related to, for example, details of wave breaking, details of the turbulence generated due to the wave breaking process, or small scale sediment transport processes. Studies such as these will likely involve highly intensive direct numerical simulations using the Navier-Stokes equations. Such computations may need to take advantage of computational power that is arising due to the rapid evolution of the entertainment gaming industry.

Although progress in discipline-based science is important, it is also becoming evident that feedbacks in the ocean exist that involve processes that are traditionally covered by different disciplines. For example, biological-physical interactions in the nearshore zone affect larval transport and recruitment. Chemical-physical interactions affect oxygen penetration into sandy sediments on the inner shelf and influence the morphology of the ocean bottom. Inner shelf and nearshore areas may be more interconnected than previously thought. Wave-structure interaction studies involving non-deforming bodies may need to be applied to newly emerging controllable wave energy extraction devices (that significantly affect that surrounding wave field). This will involve consideration of device control, structural dynamics and physical oceanography.

The current funding climate makes obtaining funding for cross-disciplinary research difficult (targeted programs are a notable exception). Yet much of the relevant science is maturing rapidly and the science will soon likely be in a position to start disentangling the complexity in the oceans. Once this dilemma is resolved increased productivity in cross-disciplinary research may follow.

Science Strategies for the Arctic Ocean

*Mary-Louise Timmermans**

Observational evidence suggests the Arctic is undergoing significant climate change; records show increasing atmospheric and ocean temperatures, ocean freshening, rising sea levels, melting permafrost and decline of sea ice. The rapid loss of permanent sea ice suggests emphasis is needed on sustained, uninterrupted Arctic observations and focused analyses to understand and predict Arctic change on seasonal, interannual, and decadal time scales. Some research suggests atmospheric circulation, rising global temperatures and ice-albedo feedbacks will lead to ice-free summers in the Arctic Ocean in as little as 10 years from now, while other studies indicate that strong natural variability of the Arctic system will inhibit further loss of summer sea ice. The next two decades will be of great significance in Arctic research.

The following are specific questions motivated primarily by the direct need to understand and predict the state of Arctic sea ice. Where and how is the heat that is transported to the Arctic Ocean from lower latitudes lost, and what role does the ocean play in the mass balance of sea ice? How might the Arctic Ocean internal wave field change with reductions in sea-ice extent, and what feedback mechanisms might then arise as a result of higher mixing? What types of feedbacks are associated with the observed general freshening and strengthening of the stratification of the upper Arctic Ocean (for example, in terms of ocean heat loss or the structure of ice formed from a fresher ocean)? What mechanisms cause storage

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*Woods Hole Oceanographic Institution

and release of large volumes of fresh water and ice in the Arctic Ocean on seasonal, interannual and decadal time scales?* What intermittent and spatially variable processes in the continental shelf and slope regions (e.g., eddies, cross-shelf exchange driven by buoyancy flux in the seasonal ice zone, winter shore leads and polynyas) are important to ventilation of the deep Arctic Ocean? How can regional Arctic and global models be formulated and used more effectively (for example, by improvements in ice rheology and ridging dynamics, ice-ocean friction parameterizations, better treatment of the basin boundaries, and proper validations of ocean stratification, circulation and seasonality). How might natural variability of climate parameters, such as the Arctic Oscillation index, impact the Arctic Ocean and ice cover on decadal and multi-decadal time scales, and can we identify the interplay between natural and anthropogenic forcing? How will Arctic ecosystems change with reduced ice cover and shorter winters, and to what extent will these changes be irreversible?

Considered measurements of the Arctic system are needed to answer these questions. Study of the Arctic Ocean is restricted both by limited opportunities for access and by the lack of appropriate instrumentation. Standard observational practice to sample in August-September (when the sea-ice coverage is at its seasonal minimum and the Arctic is accessible by research icebreakers) and April-May (using aircraft when the sea ice is sufficiently strong and there is adequate daylight) does not capture seasonal and shorter time scale variability and provides only limited spatial coverage. In recent years, advances in our understanding of the Arctic have been made through the use of autonomous Ice-Based Observatories (IBOs). IBOs combine suites of different sensors mounted in the drifting permanent Arctic ice pack, providing (via satellite) year-round automated measurements of the ocean, ice and atmosphere.

Advances in IBO instrument design and capability are required to both improve long-term functionality and to return additional oceanographic information. For example, in the coming years velocity sensors will be incorporated on ocean profiling IBOs to provide the capability of measuring deep ocean features such as internal waves and eddies, as well as smaller-scale flows, and thus heat transport, in the ocean beneath the ice (surface ocean velocity measurements are already being made by the Naval Postgraduate School's [NPS] Autonomous Ocean Flux Buoys). Direct velocity measurements from an extensive array of IBOs will allow us to quantify ocean dynamics and upward heat fluxes over a substantial

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*It is speculated that heat and fresh water exchange between the Arctic Ocean and the North Atlantic depends upon both the process of Ekman pumping associated with the climatological atmospheric circulation over the Arctic Ocean, and on seasonal sea-ice transformations in the Arctic, leading to complex seasonal variability.

fraction of the ice-covered Arctic Ocean and over all seasons. A particular focus will also be placed on specializing and adding biogeochemical sensors to IBOs, and developing reliable processing techniques, to monitor properties such as dissolved oxygen levels, phytoplankton biomass, and dissolved organic matter concentrations. In addition to developing sensor technologies, modifications to IBOs will be required if existing measurement techniques are disrupted by the emerging changes in sea ice; these adaptations would include increased floatation, enhanced buoy design for survival over freeze-up and variable under-ice tether lengths.

Additional shelf observatories based on autonomous vehicles and bottom-mounted instruments will be employed to investigate shallow shelf regions where sea ice is seasonal, and where winter ice cover destroys conventional instrumentation. Arctic researchers at Woods Hole Oceanographic Institution (WHOI), the Applied Physics Laboratory at University of Washington (UW-APL) and elsewhere are in the early stages of designing floats, gliders and autonomous vehicles for long-term use under ice to provide broad spatial coverage, and high-resolution measurements of, for example, Arctic Ocean circulation, under-ice roughness and seafloor topography along critical sections in both the seasonal ice zone and the central Arctic basin. The instruments will be integrated with basin-scale acoustic navigation and communications systems incorporated in IBOs to provide navigation data to autonomous platforms by relaying their position via acoustic data link. IBOs will be made capable of acting as communication relays for data passed to them from passing vehicles or to relay commands and data from shore to visiting vehicles. Other advances in Arctic observing capability will include remote calibration technologies, greater resolution due to increased data storage density, advanced battery chemistries, and lower power consumption.

Federal research funds are required not only for advances in Arctic instrumentation and new field techniques (for example, through collaborative NSF Science and Technology Centers), but for associated process-oriented studies which emphasize collaboration between engineers, modelers, observationalists and theoreticians, as well as interdisciplinary connections between physical oceanographers, chemists and biologists. In the coming years, process and climate studies to interpret extensive new observations of the Arctic Ocean and answer the specific questions outlined above will include analyses of: fresh water and heat accumulation and release; dynamics and evolution of water mass fronts; mixing mechanisms; evolution of the surface layers and ice-ocean interactions; seasonal and higher-frequency biological processes; and property fluxes.

Submesoscale Variability of the Upper Ocean: Patchy and Episodic Fluxes Into and Through Biologically Active Layers

Daniel Rudnick, Mary Jane Perry,† John J. Cullen,‡*
Bess Ward,§ Kenneth S. Johnson‖

THE UPPER OCEAN

The upper ocean, defined roughly as the upper few hundred meters, will continue to be the subject of intense research through 2025. As a practical matter, almost all human interaction with the ocean is in the upper ocean, including transportation, fishing, and national defense. Communication with the atmosphere takes place through the upper ocean, so studies of air-sea interaction of all scales from squalls to climate require knowledge of upper ocean processes. Finally, most ocean life and its variability is concentrated in the upper ocean because of the availability of light for photosynthesis, making the upper ocean a focus for problems spanning the boundaries of physics, biology and chemistry. It is a fair bet that significant progress will be made by 2025 on these problems.

LAYERS, LAYERS EVERYWHERE AND HARDLY TIME TO THINK

The upper ocean is often described as composed of layers, each defined by some set of properties. These layers may be coincident, may overlap, or may not exist at all under some circumstances. Because of

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*Scripps Institution of Oceanography, University of California, San Diego
†School of Marine Sciences, University of Maine
‡Dalhousie University
§Princeton University
‖Monterey Bay Aquarium Research Institute

their prevalence in the literature, and likely in future research, some definitions of layers are in order. The mixed layer is bounded by the surface, and is usually defined as being uniform in such properties as temperature, salinity, or velocity. Although observations have shown these properties are not always uniform over the same depth interval, the concept of a strongly turbulent slab mixed layer has spawned a number of useful models. Observations show that the actively turbulent layer does not always coincide with the mixed layer. The mixing layer is the region bounded by the surface in which turbulent dissipation is strong. Biologically influenced tracers, such as optical estimates of particle concentration, can reveal when and where active mixing slows, confining phytoplankton near the surface. Recent research has focused on the transition layer, the region between the mixed layer and the weakly turbulent interior ocean. The transition layer is both turbulent and stratified, so fluxes of momentum and other properties are strong. As the transition layer often overlaps with the nitracline and the subsurface chlorophyll maximum, processes in the transition layer are crucial to the biogeochemistry of the upper ocean. The subsurface chlorophyll maximum layer is the region of high chlorophyll concentration that is generally found near the base of the euphotic zone below a nutrient-depleted mixed layer, with a thickness of five to 20 meters. Phytoplankton within this layer typically have higher cellular chlorophyll concentrations that compensates for low light; consequently this layer may or may not also be a particle maximum layer. The subsurface chlorophyll maximum layer is a persistent feature of tropical and subtropical oceans and is a seasonal feature in many mid- to high-latitude oceanic and coastal regions, developing after nutrients in surface waters are depleted by the phytoplankton spring bloom. Planktonic thin layers are features with thickness on the scale of several centimeters to several meters, persisting from hours to days, and often with distinct species assemblages and very high concentrations of chlorophyll. Mechanisms for thin layer formation include straining by shear and active aggregation of the organisms by buoyancy regulation or swimming behavior. These layers are often associated with sharp gradients of nutrients and density, and thus they represent hot spots of physical, chemical and biological interaction.

FLUXES: A WAY OUT (OR IN)

The layers defined above have certainly been observed, and have proven to be conceptually valuable. What we really need to know, however, are the mechanisms of their formation and maintenance, and consequently the fluxes of materials and properties into and through them. In turn, a thorough understanding of fluxes would allow an explanation for the formation of any layer, and would lead to prediction as fluxes are either directly resolved or parameterized in numerical models. The vertical turbulent fluxes in the transition layer are an important topic for research in the coming years. This region is challenging, as both internal waves and turbulence are strong, its depth changes as it follows the undulations of the mixed-layer base, and any strict assumption of one-dimensionality is unlikely to be satisfactory. A long-standing problem in biological oceanography is that turbulent fluxes, determined over the years from physical measurements, have never seemed sufficient to describe the observed production. For example, throughout the oligotrophic ocean dissolved inorganic carbon is depleted in the mixed layer each summer by biological processes, yet there are almost no detectable nutrients to support the carbon consumption. Do episodic events that are difficult to sample by conventional, shipboard programs control this biological production, which represents a major component of the ocean carbon cycle? With the continued improvement in biogeochemical sensors, fluorescence measurements and molecular probes that assess short term physiological responses to nutrient pulses, the near future holds promise for the solution of this problem, as biological and chemical variables are measured on the same scales as physical variables (centimeters to meters vertically, over deployments long enough to detect episodic events that may occur on time scales of seconds to minutes at intervals of hours to days).

THE SUBMESOSCALE: WHERE THE ACTION IS

The most important vertical advective fluxes occur on scales smaller than the energetic mesoscale, whose study has dominated past decades. The submesoscale, horizontal length scales of order kilometers and smaller, is populated with ageostrophic processes causing vertical flows. The ageostrophic circulation cells at fronts and on the edges of eddies are a particular focus. At issue is an ongoing debate about what causes eddies to be sites of enhanced productivity. Exactly geostrophic dynamics do not include vertical flows, so ageostrophy is required, but what is the relevant process? Wind forcing of the eddy and instability at the eddy's edge are candidate processes. The seasonal restratification of the mixed layer is a submesoscale phenomenon, and as it occurs near the same time as the spring bloom, the coupling of biology and physics is a distinct possibility. The very fact that such properties as nitrate and chlorophyll have fundamentally different structures, sources and sinks than temperature and salinity make them potentially valuable tracers for understanding submesoscale processes, especially considering that new sensors can reveal physiological properties of microbes that reflect recent nutrient perturbations. As model resolution improves, the processes that are unresolved will continue to shrink, so the submesoscale will be a focus for theoretical studies aiming to improved parameterizations.

SUMMARY

The fluxes causing the layered structure of the upper ocean are likely be quantified within the next 15 years. Observations using new biological and chemical sensors will pave the way, as biogeochemical and physical variables are resolved at the same length and time scales. Submesoscale dynamics will be a focus, as ageostrophic processes cause the relevant vertical fluxes.

Who's Blooming? Toward an Understanding of Why Certain Species Dominate Phytoplankton Blooms

Mary Jane Perry, Michael Sieracki,* Bess Ward,† Alan Weidemann‡*

WHAT QUESTIONS REMAIN UNANSWERED?

A critical question in biological oceanography is "what controls the species composition of phytoplankton assemblages?" While the general patterns of development of mid- and high-latitude spring blooms, upwelling blooms, and storm-induced blooms are reasonably well known—at least with respect to biomass—the specifics that determine which species first become abundant, as well as the species that replace them over time, are not. Because phytoplankton respond directly to physical forcings, it is likely that both species composition and timing of blooms will change in response to climate change. While ecosystem models have evolved from parameterization of phytoplankton as total biomass to functional groups and individual species, observational assessment of phytoplankton species on appropriate space and time scales remains a technological challenge.

WHY CARE ABOUT PHYTOPLANKTON SPECIES?

Some phytoplankton species are toxic—either to humans (harmful algal toxins are concentrated by filter feeding bivalves), copepods (some

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*School of Marine Sciences, University of Maine
†Princeton University
‡Naval Research Laboratory

diatom aldehydes lead to birth defects in some copepods), or birds and marine mammals (domoic acid is transferred up marine food webs). Some phytoplankton species provide better nutrition to zooplankton and fish than other species; nutrition plays an important role in successful growth and development of larval fishes. Some phytoplankton species are more efficient in transporting carbon to the benthos, by virtue of heavier cell coverings (e.g., silica) that results in more rapid sinking. Some species scatter light more effectively as a consequence of spines or calcite coccoliths, thereby changing the relationship between chlorophyll concentration and light attenuation.

WHAT CONTROLS THE SPECIES COMPOSITION OF PHYTOPLANKTON BLOOMS?

Part of the answer lies in understanding how phytoplankton are introduced (or inoculated) into a local surface mixed layer. Horizontal transport is an important mechanism for introduction of species into a local water mass. Does interannual variability in the advective introduction of species, either due to changes in source waters or relative transport rates of currents, lead to interannual variations in the species composition of blooms? In shallow waters overlying continental shelves some phytoplankton species, particularly diatoms and dinoflagellates, can form resting stages or cysts that lie dormant in the sediment for months to years. These cysts and resting stages can be either triggered to germinate by exposure to very low levels of light at the end of the winter or can be reintroduced into the euphotic zone by vertical mixing events.

The second part of the answer to what controls the species composition of phytoplankton blooms lies in relative net growth of individual species:

$$dP = P(\text{gain} - \text{loss})\, dt$$

where P is abundance of a phytoplankton species, gain is primarily growth rate, loss is primarily grazing rate, and t is time (from Gordon Riley). If the identity of species that might bloom were better known, it would be possible to carry out focused laboratory experiments to determine how growth rates of individual species are controlled by light, temperature, nutrients and mixing. Grazing rates depend on who the consumers are, whether grazer abundances vary interannually, and if grazers are selective for or against certain species of phytoplankton. Again, with knowledge of potential bloomers, grazer selectivity experiments could be carried out.

WHAT NEW TECHNOLOGIES COULD BE DEVELOPED AND WHAT QUESTIONS ANSWERED?

Although current technologies can provide highly resolved time series of total phytoplankton biomass (typically as chlorophyll a concentration) prior to and during blooms in both Eulerian and Lagrangian frames of reference, there are very few observations of phytoplankton species collected either in a Lagrangian frame of reference or with sufficient temporal frequency to resolve changes in organisms that can double population size on the order of once per day. The notable exceptions are measurements with flow cytometers and FlowCAMs from ships and a few shallow water moorings.

In order to truly understand bottom-up regulation of marine ecosystems (i.e., to what extent species composition, abundance and timing of the primary producers exert control over the rest of the food web), the field needs to move beyond observing and assessing phytoplankton primarily as chlorophyll. To understand why certain species become abundant, the capability to easily assess phytoplankton species on the appropriate space and time scales needs to be developed.

WHAT TECHNOLOGIES ARE REQUIRED?

To address the questions raised above, systems for unattended measurement on both mobile (Lagrangian) and fixed (Eulerian) platforms need to be developed. Two likely methodologies are optical imaging and molecular analysis. Early examples of both of these technologies exist today, but will require serious and significant technological investment if easy assessment of species is to be enabled by 2025. Optical imaging of individual phytoplankton-size particles today is laser based, with image analysis of larger cells (flow cytometry and FlowCAM). Molecular technologies, including microarrays, are rapidly developing and need to be coupled with water sampling and microfluidics. In addition to the need for serious reduction in sensor size, other issues include sensor robustness, depth rating, power consumption, battery life, sensing frequency including conditional sampling, sensing duration of weeks to months, on-board manipulation of water samples, on-board data analysis and compression, data storage and transmission, and ability to be integrated into mobile platforms and moorings.

HOW WILL THE RESEARCH BE CONDUCTED?

Experiments could be conducted with a combination of platforms that move actively (e.g., long-duration AUVs or combination gliders), covering potential source water downstream of an area of interest, and platforms that follow the water (e.g., Lagrangian mixed layer floats). High frequency sampling would provide a picture of what really happens during a bloom—similar to a walk through the garden to see what plant species are there and who grows the fastest; high frequency identification of species would provide an answer to who's blooming and the beginning of the answer to why.

Understanding Phytoplankton Bloom Development

Bess Ward and Mary Jane Perry*[†]

WHAT QUESTIONS REMAIN UNANSWERED?

Historical biogeographical data sets document global and regional patterns in phytoplankton and zooplankton species distributions. Although correlations with, for example, temperature and nutrient concentrations, are strong, they remain descriptive. At a very fundamental level, we still do not know what controls the species composition of phytoplankton assemblages and what key environmental variables determine the success or failure of different groups under different conditions. Even biogeochemical models that include functional groups do so with low resolution and depend upon simple variables such as size to distinguish groups. Although size sounds like an objective variable, it's not obvious that actual phytoplankton fall into ecologically meaningful size categories. Thus we need to link observations of phytoplankton species to measured and model outcomes to determine what factors matter to phytoplankton, and thence to predictive power in response to anthropogenic changes such as nutrient enrichment and global warming.

WHY FOCUS ON PHYTOPLANKTON BLOOMS?

Molecular ecological investigations, first of prokaryotic plankton and more recently of eukaryotic phytoplankton, have revealed a vast diversity of species in natural assemblages. The diversity at the molecular level is

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*Princeton University

[†] School of Marine Sciences, University of Maine

even greater than can be determined by the best microscopist, and modern methods such as DNA/RNA microarrays can evaluate diversity and abundance rapidly in relatively high throughput mode. The fact remains, however, that despite the large background diversity, only a very small number of types ever reaches really high abundance in blooms. Irigoien et al. (2004) showed that the highest biomass/production occurs mostly in high biomass blooms and these blooms are due to only four phytoplankton types: diatoms, coccolithophorids, Phaeocystis and dinoflagellates. Thus blooms, although rare and geographically small, are inordinately important to overall marine productivity and are dominated by a few types.

WHAT CONTROLS THE SPECIES COMPOSITION OF PHYTOPLANKTON BLOOMS?

The answer to this question depends on the factors that cause a high diversity, low abundance assemblage to develop into a low diversity, high abundance bloom. We suspect that the critical events occur long before the typical oceanographic measurement—chlorophyll or some other measure of biomass—can detect changes in the assemblage. The important responses that allow some species to win and cause others to lose must occur very soon after conditions change to allow a bloom: introduction of new nutrients by advection or upwelling, cessation of mixing due to surface warming, etc. Responses to environmental changes occur at the level of gene expression, probably often in genes that we have not yet identified or whose ecological significance we have not yet grasped. Thus fundamental molecular biological research is required to identify targets for response assay development.

WHAT NEW TECHNOLOGIES COULD BE DEVELOPED AND WHAT QUESTIONS ANSWERED?

In order to investigate the early development of phytoplankton blooms, it is necessary to begin sampling even before there is indication that changes are occurring. This can be done in manipulative experiments with large volume incubations at sea or on land, or by using remote observations to predict likely bloom development and then undertake *in situ* sampling. In order to determine the appropriate time scales for investigation of phytoplankton responses, some additional work with pure cultures is probably warranted. Then both experimental and *in situ* sampling can be scaled to catch, e.g., changes in gene expression that can occur on the order of minutes to hours. The development of large phytoplankton blooms depends partly on the absence or lag of grazing, and overall production is thus subject to top-down control. The initial response at the genetic level that allows some species to take advantage of episodic changes in growth conditions, however, is fundamentally bottom-up. We must be able to identify and interpret such responses long before food web interactions become obvious.

WHAT TECHNOLOGIES ARE REQUIRED?

The two basic needs are development of technology to evaluate gene diversity and expression for many different phytoplankton species rapidly and specifically, and to deploy those methods on samples collected at appropriate time and space scales. It's hard to imagine being able to carry out the biological measurements required to assess early bottom-up responses outside the laboratory, but some progress has been made. *In situ* quantitative polymerase chain reaction (Q-PCR) and hybridization methodologies are under development (John Paul, Chris Scholin). By selecting a few well-characterized genes, very specific assays can be developed to identify major phylotypes and assess gene expression among them. Functional gene microarrays offer a way to analyze the relative abundance and level of gene expression of many different kinds of genes from a multitude of different organisms simultaneously. At the very least, frequent sample collection and preservation can be done so that laboratory based analyses can be linked to remotely measured chemical and physical variables. Remote sampling and sample processing are engineering challenges beyond my insights. Clearly it's not just technology, but communication between e.g., molecular biologists and engineers, which will make important advances possible.

HOW WILL THE RESEARCH BE CONDUCTED?

In the analysis of actual blooms, high frequency sampling and relatively rapid analysis is required. This probably requires a combination of remotely operated vehicles, ships and laboratory analysis. Real time analysis is not necessary for everything as long as real time sampling can be coordinated with the necessary physical and chemical measurements. In order to develop molecular assays for the key genetic responses involved in bloom development, it is also necessary to support laboratory based molecular biological research. This can go on in parallel, as genetic samples can often be reanalyzed when new tools become available.

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From Short Food Chains to Complex Interaction Webs: Biological Oceanography in 2025

*Kelly J. Benoit-Bird**

Interaction webs, models that show which species in a community interact with each other and how strongly, are considered to be a cornerstone of modern ecology. Since interactions among species are a fundamental component of how communities and ecosystems function, interaction webs are central to a large number of ecological questions. Questions that have been addressed using interaction webs in benthic, terrestrial and lake ecosystems include:

- Are large communities more or less resilient to environmental stress than small communities?
- What is the maximum number of trophic levels likely to occur in any community?
- Do changes in species abundance or composition at one trophic level create cascading effects to other trophic levels?
- In a community subjected to anthropogenic perturbations, do decreases in the densities of some species lead to compensatory increases in the densities of functionally similar species, thereby preserving the ecosystem services performed by the community?

The emphasis in ecology on networks has highlighted the impor-

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*College of Oceanic and Atmospheric Sciences, Oregon State University

tance of the community on species-pair interactions. However, studies of pelagic marine ecosystems have emphasized primarily pairwise comparisons of species sometimes between species that are not adjacent in the food web, making interpretation of results difficult. This focus on a few components in a system can be attributed to several factors:

- the 3-dimensional nature of pelagic ecosystems;
- the mobility of plants and all of the animals in these systems and thus the lack of a fixed reference point;
- the specialized tools required to bring to the surface, image, or remotely sense the organisms, their characteristics, and their habitat; and
- the inherent difficulties in making measurements and performing manipulative experiments in the oceanic environment. As a result, comparisons in oceanic systems are often made between organisms that can be measured rather than those that are thought likely to interact.

Continued development of new techniques and equipment to overcome these challenges is necessary, but not sufficient to move biological oceanography forward in the next few decades. In order to move from studies of short chains of organisms to ecological questions that can be addressed with the interaction web conceptual framework, several challenges need to be overcome. Because measurement of each type of organism in the ocean currently (and likely always will) requires a specialized suite of techniques, few investigators are capable of making measurements of multiple trophic levels, which are quite different from the single-investigator studies that have proven effective at addressing these problems in other ecosystems. For example, phytoplankton are typically studied optically while fish are often investigated acoustically, two very different approaches requiring specialized equipment and data analysis skills. Studies of ecological questions at more than a single trophic level thus typically require multiple investigators. The academic reward system (e.g., promotion and tenure system) must be able to properly evaluate the contributions of members of these collaborative teams and reward members for this work equally to work carried out individually. While some progress in this area has been made, particularly in the historically interdisciplinary field of oceanography, these changes are not as obvious in other disciplines (such as ecology) or at higher levels of academic administration. For example, if a department values and rewards collaborative research how is this commitment to interdisciplinary study communicated to authors of external review letters and to committees in the dean's or president's offices? How are papers with multiple authors evaluated? Do committees give credit for the time spent leading large research teams separately from paper output? These, and many other questions, need to be addressed in order to encourage young scientists to tackle large ecological problems requiring many investigators.

The need for collaborative research to address important ecological problems is not a new one. Several large, ecosystem-scale studies have been funded in the last several decades, leading to significant advances in our understanding of a variety of pelagic marine ecosystems. However, even in these big programs, investigators tended to emphasize their single organism of interest and small groups of investigators focused on comparisons of a few groups of organisms. The truly integrative studies needed to look at more than a few trophic levels simultaneously have proven exceptionally difficult both in planning and data collection and analysis. While specialized researchers will always be required to carry out specific measurements, interpret their data, and move techniques forward, to overcome these difficulties we also need broadly trained scientists focused on integration of results. To be effective, these individuals would need to understand the principles of the various instruments and data types, but would not be able to be technical experts in each. These individuals would play an important role in designing experiments so that comparable data could be collected on a variety of ecosystem components, would facilitate communication between groups that have their own specialized vocabulary, and would lead the synthesis stage of data analysis. This need for team leaders that specialize in synthesis presents two problems. First, how do we, a field primarily of specialists, effectively mentor these broadly trained students? Addressing this requires we rethink our educational paradigms and our traditional one-on-one apprenticeship approach to training graduate students. This is not an insignificant obstacle. Second, once we have trained these scientists, will they be able to find and succeed in research positions? This returns us to concerns about academic reward structure.

Integrated studies of multiple types of organisms in terrestrial, benthic, and freshwater ecosystems have revealed how natural communities are organized and have provided statements about whole communities that are important for interpreting measures of a single species, understanding community processes, and predicting the effects of climate change. In studying the processes and patterns of communities in the pelagic ocean we face unique challenges, but they are not insurmountable. Some challenges in integrated ecosystem studies will inevitably be addressed by the forward progress of instruments and techniques. There are, however, cultural barriers in academic reward structure and graduate education that will not be overcome without a concerted effort from within the field of oceanography.

The Interface Between Biological and Physical Processes

*Mark Abbott**

NEW APPROACHES TO ECOSYSTEM MODELING

The present structure of our nitrogen/phytoplankton/zooplankton (NPZ) models has been unchanged for over 60 years. Although we have added more components, the basic model assumes that everything can be based on a single element (nitrogen) and that the basic interactions are analogous to chemical reactions where the components can be represented as continuous fields of reactants. The models are basically plumbing with various reservoirs and complex functions that represent the flow of nitrogen from one reservoir to another. As our knowledge of ecosystems has improved, we have added more reservoirs (e.g., adding the microbial loop, accounting for fixation of nitrogen), but once we establish the basic structure of the ecosystem, then it is difficult to model how the system will adapt to changes in environmental forcing. Doney et al. (2004) proposed that a new class of models be developed that are based on ecosystem functions rather than trophic levels. Such functions could include processes such as nitrogen fixation, recycling, etc., where the structure would resemble models of genetic regulation that link environmental conditions with the expressions of particular genes (or in this case, functions). For example, could we express the export of surface carbon as a function that would be triggered by spring bloom conditions? If so, what

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*College of Oceanic and Atmospheric Sciences, Oregon State University

are the environmental processes that initiate blooms? Such new modeling approaches could lead us beyond the problems of parameter estimation and model tuning which are inhibiting the development of models that represent the complexity of ocean ecosystems, especially in the context of climate change.

COUPLING OF VERTICAL VELOCITIES TO NUTRIENT FLUXES

Most circulation models have difficulties simulating vertical velocity. Because of a range of factors, model estimates tend to be smoother than the observations as conditions of strong vertical velocity are restricted to small scales in time and space. For calculations of physical quantities such as heat and momentum fluxes, highly smoothed estimates are likely acceptable. However, for biogeochemical processes such as nutrient uptake and photosynthesis, the nonlinear nature of these processes amplifies the response, such that small changes in the predicted light field have significant biological impact. Thus errors in vertical velocities that may be small and inconsequential in the context of physical processes may have enormous impacts on biogeochemical and ecological models. Regions such as the Polar Front are dominated by localized upwelling and downwelling, and incorporating these processes in coupled models will be a significant challenge over the next 20 years.

UPTAKE OF CO_2 BY THE OCEAN

Most biogeochemical models assume that the ocean will continue to take up and sequester about two gigatons/year of atmospheric CO_2. However, ocean CO_2 uptake may diminish in response to changes in ocean pH and in climate forcing. For example, phytoplankton blooms might be less frequent in a warmer ocean, slowing down an important pathway for the downward flux of organic carbon. Because of the complexity of the relationships between climate forcing, biogeochemistry, and ecosystem response, this question will be an important issue for the next 20 years.

MECHANISMS UNDERLYING CLIMATE OSCILLATIONS

Coupled models still have difficulties reproducing major oscillations in the ocean/atmosphere system such as the ENSO, the Pacific Decadal Oscillation (PDO), and the North Atlantic Oscillation (NAO). As these processes can serve as natural laboratories to observe responses of ocean ecosystems to changes in physical forcing, it is important that our models begin to capture these important components of the Earth system.

THE ROLE OF "RARE" SPECIES

In any sample of surface ocean water, the phytoplankton biomass is dominated by a handful of species with the rest of the sample composed of species that are represented by only a few individuals. These species never dominate the phytoplankton community, instead only occurring in small numbers. We do not understand their role in the ecosystem or biogeochemical cycling. Moreover, we do not understand how they persist in oligotrophic environments where there may be significant competition for nutrients.

IMPACTS OF GEOENGINEERING

With increasing economic and political pressures for carbon sequestration (e.g., cap and trade for carbon) as well as climate change mitigation, there may be both government and private sector efforts to fertilize the upper ocean through iron additions, artificial upwelling, etc. At-sea aquaculture could also be considered a type of geoengineering. The point is that the oceans will become more "managed" rather than simply exploited.

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Research on Higher Trophic Levels

Daniel P. Costa, Yann Tremblay,† Sean Hayes‡*

Our understanding of the mechanisms responsible for biophysical coupling in marine ecosystems has developed significantly over the last two decades, but is limited to the mechanisms that relate physical oceanographic processes to primary production and primary consumers (zooplankton). In contrast, our knowledge of the linkages between biology and physics of higher trophic levels remains quite descriptive at best. This is unfortunate because higher trophic level species are increasingly under threat of extinction along with a loss in marine biodiversity. This is occurring as we are becoming aware of the importance of upper trophic levels in structuring marine communities due to both their role as predators (Estes et al. 1998; Myers and Worm 2003) and as they transport nutrients across and within the water column (Smetacek and Cloern 2008).

As the first Census of Marine Life (CoML, www.coml.org) ends in 2010 we will have gained significant insights and developed new tools to study a wide variety of marine habitats, from the coastal margin to the deep sea. However, these studies were separated in space and time. Imagine what could be accomplished if this research was applied in an integrated manner, providing a seamless transition from the benthos through the water column to the intertidal. With measurements of the movement patterns of top predators coupled with the abundance of

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*University of California, Santa Cruz

†Institut pour la Recherche et le Développement, France

‡Fisheries Ecology Division, Southwest Fisheries Science Center

zooplankton. Such an integrated effort would need to be focused on a number of regions where existing infrastructure is in place or locations that are representative of critical marine habitats. Such an integrated effort would provide not just a onetime snapshot of the biodiversity of a marine habitat, but would provide a dynamic view into the processes that maintain biodiversity and a better understanding of how it can be protected. A critically important aspect of this is that we will be able to monitor how life in the ocean is changing in response to climate change. Such information will be critical to policy makers to provide them with the information necessary to mitigate such impacts.

Models will be an important component of such an effort, as much of the data collected will be descriptive. While NPZ models have proven quite informative for lower trophic levels, they do not scale up to higher trophic levels. Individual Based Models (IBMs) can represent the movements of a single marine animal and can create an energy budget that incorporates the costs of movement and acquiring prey. Such a model would be spatially explicit, and influenced by environmental and other relevant factors affecting animal behavior. A suite of these IBMs can be released into a model to represent a population of a given species. The movement patterns relative to oceanographic features and prey availability can then be modeled along with information on species interactions. The development of such models would require a mechanistic understanding of the habitat utilization patterns of higher trophic levels. Electronic tags can be used to help elucidate the habitat utilization patterns of marine organisms and provide data that are appropriate for incorporation into IBMs. Integration of oceanographic data with marine animal distribution and behavior can be used to build models that describe the interrelationships of marine animal movements to their physical and ecological habitat. Such a modeling approach would provide an "experimental test bed" to examine the processes that determine animal distributions, local abundance and movement patterns.

Under the auspices of the CoML, a variety of technologies have been developed, among them is the use of electronic tagging that has been deployed on a large scale in an integrated manner to track the movements and behavior of large marine vertebrates, in the Tagging of Pacific Pelagics (TOPP) program (www.topp.org) and in the Pacific Ocean Shelf (POST) tracking project program (www.post.org). The primary methods for tracking marine organisms include: GPS, ARGOS satellite, acoustic and archival data storage tags (Figures 1 and 2). Over the last decade the capability of electronic tags has increased considerably. However, there are a number of technological advances that need further development, including novel ways of powering the tags, increased sensor capabilities (including oceanographic sensors and animal behavior and/or physiol-

FIGURE 1 Southern elephant seal with a Sea Mammal Research Unit CTD tag attached to its head. These tags transmit information on the animal's surface track (Figure 2A), dive behavior (Figure 2B) and temperature and salinity profiles (Figure 2C).

ogy), better attachment methods, miniaturization of tags, and alternative methods of data recovery. While new higher capacity batteries may be developed, an alternative would be to develop other methods of obtaining power. For example, these animals move through the water and some undergo considerable changes in pressure. Conceptually, this seems very straightforward, but the development of reliable power harvesting systems has not begun. Other sensors that could be added to the tags include

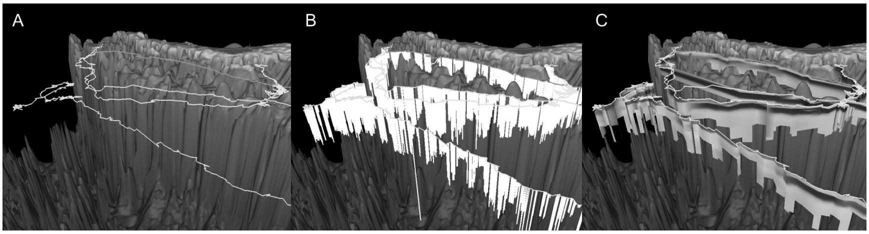

FIGURE 2 Tracks of southern elephant seals showing the range of data that can be derived. A) surface track only, B) surface track with underwater behavior, and C) track with CT profile along route of the animal.

such important oceanographic measure as O_2, pH, CO_2, and chlorophyll, as well as important measures of animal behavior as 3-axis acceleration, feeding and heart rate and possibly active sonar to measure prey fields in front of the animal. Finally, novel methods of data recovery would greatly enhance the range of species that these tags could be deployed on. Currently, archival tags have to be recovered to obtain the data. This is done when the animal returns to a rookery (seals and birds), the tag is released and floats to the surface where it transmits a subset of the information (pop up tags), or the data are transmitted via ARGOS when the animals come to the surface (air-breathing vertebrates and some sharks). A major advance would be achieved if the data obtained by electronic tags could be telemetered underwater via an acoustic modem. The data could be collected when the animal swims past an acoustic receiver such as being proposed by the Ocean Tracking Network program (OTN; www.oceantrackingnetwork.org).

As these tools evolved, they reached a sophistication and reliability where the data collected were equivalent to the industry standards for oceanographic sampling tools. For example, elephant seals can sample the water column 60 times a day reaching depths of 1000 m under their own power across broad expanses of the ocean that are difficult to reach by ships or other conventional means (Figure 3) (Boehlert et al. 2001). The research subjects became research tools and can provide oceanographic data for a fraction of the costs and can provide coverage in regions where conventional methods do not work such as polar regions (Charrassin et al.

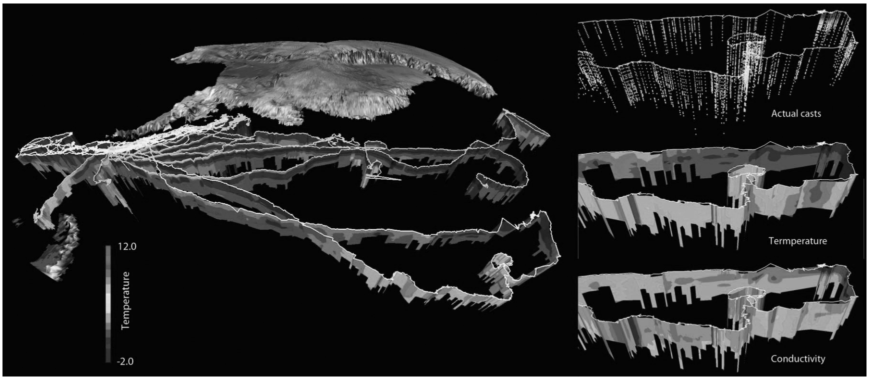

FIGURE 3 Left: tracks of 12 southern elephant seals instrumented with ARGOS linked CTD tags. Right Top: a close up showing the actual profiles data collected; Right Middle: a close up of the temperature profiles that can be interpolated from those casts; Right Bottom: a close up of the conductivity profiles that can be interpolated from those casts.

2008; Costa et al. 2008). At the same time technologies have been improving to study the movements of smaller fish species at sea. Instrument size currently limits satellite telemetry to the largest fish species such as sharks and tunas.

Archival tag technology has become sufficiently miniaturized so that juvenile fish less than 100 g can be tagged without significant increases to their mortality. However, for juvenile salmon which reliably return to a river of origin where they can be predictably captured, marine survival rates are only 2-5%, making the cost of deploying archival tags prohibitive. As a result, acoustic technologies have moved to the forefront of marine fisheries movement research. In the North Pacific alone thousands of fish from over a dozen species are now being tagged with small, relatively inexpensive acoustic transmitters, and their movements are being monitored by a growing network of acoustic arrays led by the OTN and POST. These networks are providing new insights into the movements of fish past fixed listening arrays in the ocean without the need for tag recovery. Unfortunately, these data have two limitations over the archival and satellite tag technologies. The first is a lack of oceanographic habitat sensors to collect data in the environment where the fish is found and second is array deployment limited to the continental shelf. These limitations could be overcome by deploying "business card tags" (BCTs) on larger marine animals such as elephant seals. BCTs are capable of alternating between transferring and receiving data from other BCTs and regular acoustic pinger tags when they come within range. As more tags are deployed there would be a high probability of regular encounters between a BCT tagged elephant seal and other acoustically tagged species. While one might consider the ocean to be vast, marine organisms are likely to converge on the same oceanographic features, dramatically increasing the probability of encounters. An added advantage is that larger marine organisms could not only carry the larger BCT tag, but could carry additional sensors that would provide information on the physical environment (e.g., CTD).

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Marine Biogeochemistry in 2025

Kenneth S. Johnson[*]

Developments in the past five years have enabled a remarkable shift in measurement capabilities that will revolutionize our approach to observing ocean biogeochemistry on a global scale. A suite of chemical and biological sensors can now be deployed for years in the ocean on profiling floats and return data with no detectable drift in sensor response. These systems are becoming sufficiently affordable that it is possible to envision biogeochemical sensor networks with hundreds of nodes or more, similar to the current Argo network of 3000 profiling floats. This will allow the development of basin-scale and, ultimately, global-scale observing systems. These sensor networks will permit ocean scientists to quantitatively observe fundamental biogeochemical processes such as rates of nutrient supply, net community production, physical controls on bloom development (e.g., the Sverdrup Hypothesis), dynamics of oxygen minimum zones and their impacts on denitrification, and carbon export throughout the ocean with a level of detail hitherto impossible. The spatial and temporal responses of these processes to climate oscillations and greenhouse gas forcing will be observed with a resolution that is simply not possible when observations are limited to ships. An integrated observing system that combines *in situ* sensors deployed on long endurance platforms with satellite sensors and data-assimilating, biogeochemical-

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[*] Monterey Bay Aquarium Research Institute

ecological models would provide previously unachievable constraints on the carbon cycle and its sensitivity to a changing climate. It would transform ocean biogeochemistry.

Today, our primary sources of information on temporal variability of biogeochemical processes within the ocean come from a few, ship-based time series programs at single points (e.g., Hawaii Ocean Time-series [HOT], Bermuda Atlantic Time-series Study [BATS], Carbon Retention In A Colored Ocean [CARIACO], European Station for Time-series in the Ocean, Canary Islands [ESTOC]) and from satellite ocean color measurements. In addition, a few programs, such as the Atlantic Meridional Transect (AMT), involve repeat transects over broad regions at near annual scales. Ship-based sampling at time series sites is generally monthly, at best, which misses high frequency processes. Satellites excel at providing global coverage at higher frequencies in cloud-free areas, but ocean color data is generally limited to one optical depth (<30 m in much of the ocean), the data do not resolve vertical structure and many high latitude areas are not cloud-free. The result is that we have little understanding of how ocean biogeochemistry is changing in response to natural climate oscillations such as El Niño and PDO or to anthropogenic climate changes driven by burning fossil fuels. Even the processes that control regular, annual events such as the spring bloom at high latitudes are not always well understood.

Oxygen sensors are now being deployed on profiling floats for multi-year periods with little or no drift in sensor response (Kortzinger et al. 2005; Johnson et al. 2007). These sensors are being used to study ocean ventilation (Kortzinger et al. 2004), the balance of net community production in oligotrophic regions (Riser and Johnson 2008), and carbon export (Martz et al. 2008). Remarkable precision has been attained with oxygen sensors deployed for years on profiling floats (Kortzinger et al. 2005; Riser and Johnson 2008). Bio-optical sensor technologies have also advanced rapidly. There have now been a number of studies using sensors on profiling floats (Mitchell et al. 2000; Bishop et al. 2002; 2004). The data reported by Boss et al. (2008a; 2008b) show measurements of chlorophyll fluorescence from 400 m depth to the surface in the North Atlantic for three years (Figure 1). These observations clearly resolve the annual cycle with no apparent drift in sensor response at depth and the data show remarkable events driven by mesoscale processes. Optical nitrate sensors (Johnson and Coletti 2002) are currently deployed on profiling floats and have operated successfully for more than one year with little sensor drift (Figure 2). The power budget implies that they can operate for 4 years with 60 nitrate measurements from 1000 m to the surface at a cycle time of 5 days. Plans to deploy pCO_2 sensors on floats are under way (Kortzinger, personal communication). Long-endurance pH sensors based on

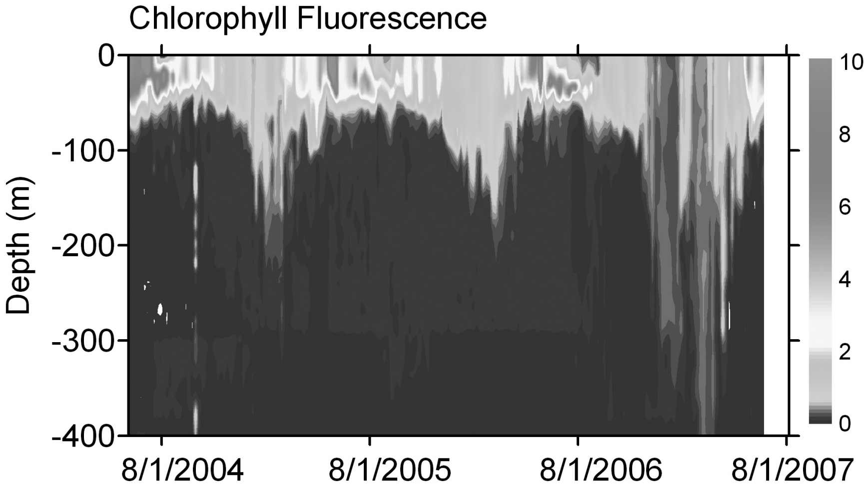

FIGURE 1 Chlorophyll fluorescence observed over three years with sensors on an Apex profiling float in the North Atlantic. The spring bloom is clearly resolved each year and a remarkable export event is seen in mid- to late-2006. Data from Boss et al. (2008a).

Ion Selective Field Effect Transistor technology are being adapted for use on profiling floats. Optical particulate inorganic carbon sensors are in development with an eye towards deployment on profiling floats (Guay and Bishop 2002).

By 2025, we can expect that the ocean will be populated with a dense array of biogeochemical sensors on platforms that have evolved from the current design of profiling floats. These sensors will allow ocean scientists to monitor significant components of the carbon cycle, ranging from primary production to carbon export, without leaving their office. The availability of this array will greatly shift the way biogeochemistry of the ocean is studied. Numerical models will continuously assimilate this biogeochemical data and offer real time assessments of biogeochemical processes throughout the ocean. Shipboard research will focus primarily on process studies that are conducted within the framework of the background sensor array. These studies will refine our understanding of detailed environmental impacts on processes observed with the global array. Development of the array will enable scientists from around the world, who do not have direct access to the ocean, to participate in biogeochemical science that is on the leading edge.

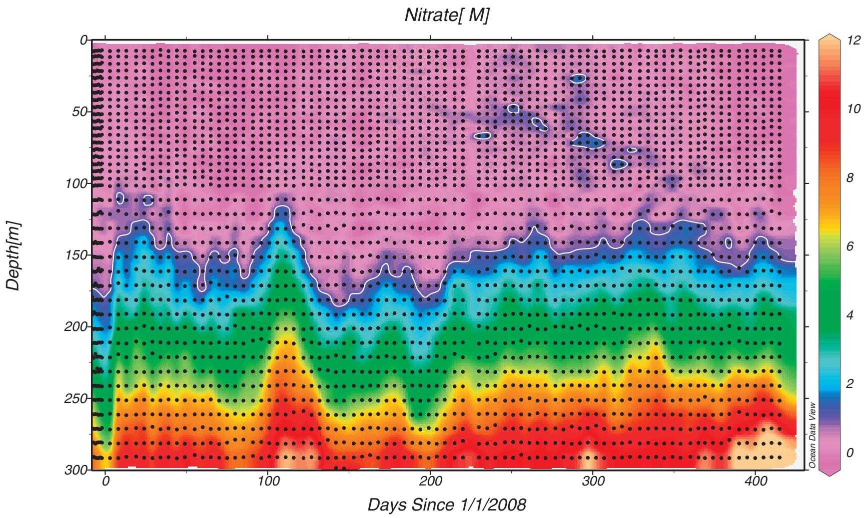

FIGURE 2 One year of nitrate measurements made with an ISUS nitrate sensor (Johnson and Coletti, 2002) deployed on an Apex profiling float near Hawaii. Real time data is available at http://www.mbari.org/chemsensor/floatviz.htm. Data from K. Johnson and S. Riser.

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Next-Generation Oceanographic Sensors for Short-Term Prediction/Verification of In-water Optical Conditions

*Mark L. Wells**

Assessment and prediction of in-water optical conditions for coastal waters and inshore seas remains an important but elusive goal for ONR, NSF and NASA research. Layered upon small scale spatial variability in optical characteristics in coastal regions is the temporally dynamic coupling among physical, chemical and biological parameters that regulate ocean optics. Predictive models built upon our incomplete understanding of these linkages provide general hindcast capability for environmental conditions, but prove to be largely unreliable for accurate forecasting in these dynamic environments due to insufficient data streams of critical parameters. Improving forecasting accuracy will require information based on widely distributed, sensor-based observing systems capable of measuring multiple parameters simultaneously at high temporal and spatial resolution. Implementation of these sensor networks, particularly beyond the narrowly focused physical infrastructures of ocean observatories, awaits the development of next-generation oceanographic sensors.

Environmental sensors can be broadly categorized as measuring physical, chemical, or biological properties. Of these, physical sensor technology is the most mature, with well-established field-deployable sensors for a number of basic oceanographic parameters (e.g., temperature, pressure, salinity, light, turbidity). In contrast, there exist only limited capabilities for field-deployable chemical sensors (e.g., dissolved oxygen, pH, redox state) and there are essentially no specific biological

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*School of Marine Sciences, University of Maine sensors (other than chlorophyll fluorescence) to provide key information on the production, structure, and composition of biologically influenced ecosystems in real time. Though much insight can be inferred from the current spectrum of sensor capabilities, accurate assessment of the broader chemical and biological parameters will be essential for accurate forecasting of in-water optical conditions. Moreover, none of the current sensor technologies provide logistically feasible capabilities for generating in-water, real time, high-density spatial and temporal data streams from remote coastal regions.

Fundamental to meeting these needs are the requirements that future sensors be robust, require low power, have fast response, require no maintenance (e.g., disposable), are adaptive (meaning that they can alter sampling strategy based on power needs, detection of an ‘event’, etc.), and, perhaps most importantly, that they comprise small, inexpensive units. For example, were the physical dimensions of current sensor packages to fall by three orders of magnitude, one could readily envision deployment of disposable adaptive sensor “swarms” by ocean currents, air, or other means that would report back real time data on the ocean field. While some opportunities certainly exist for sensing improvement by linking novel current capabilities, entirely new technological approaches will be crucial to significantly advance sensor capabilities. It is necessary that oceanographers begin to think differently about sensor research and development.

Nanotechnology arguably offers the most promising venue for achieving new advances in sensor development. Nanotechnology is a highly interdisciplinary science and engineering field that explores and exploits the unique phenomena occurring at the atomic, molecular and supramolecular scales to create materials, devices and systems with unique properties and functions. There are both “bottom-up” processes (such as self-assembly) that create nanoscale materials from atoms and molecules, as well as “top-down” processes (such as milling) that create nanoscale materials from their macro-scale counterparts. Nanoscience and nanoengineering offer a unique, largely untapped resource for new sensing modes that take advantage of unique interfacial energy properties, communication schemes, and even energy scavenging approaches to power longer-term sensor operations at the nanoscale. The unique properties of nanomaterials give them novel electrical, catalytic, magnetic, mechanical, thermal, or imaging features that are highly desirable for applications in commercial, medical, military, and environmental sectors. There exist current application examples of extreme miniaturization prototypes of complex systems (e.g., a rice-sized gas chromatograph, a microscopic and motile wireless oxygen sensor), and industrial advances in nanofabrication currently enable the high-volume production essential to support the enormous spatial sampling demands for integrative sensor capabilities. Moreover, nanotechnology offers a unique foundation for transformative science and engineering strategies of sensor design to attain the chemical and biological resolution needed to understand and predict the coupling of biology and ocean optics. But a substantial new intellectual investment towards developing next-generation sensors will be essential to achieve this objective.

The observation that developing nanotechnologies hold enormous potential for sensors has been noted in numerous previous workshops and study reports. However, nanoscience so far is largely unrealized as a foundation for environmental sensors, particularly field-deployable sensors. The central roadblock is that the nanoscience and nanoengineering expertise and research-supporting infrastructure is largely unknown and inaccessible to most oceanographers. There is a need to bring a different set of players to the table; representatives of potentially synergistic fields that would not otherwise cross paths. Herein lay a major stumbling block. Fundamental differences in research culture, knowledge base, and funding source and expectations create a barrier between oceanographers and nanoscience and nanoengineering. NSF currently is being lobbied to develop a new initiative in Geosciences to help break down the boundaries between science and engineering in the research community. The intent of this initiative would be to enable and promote a strong collaborative atmosphere between geoscience and nanoscience engineering for developing novel and transformative field-deployable sensors to empower existing and future global observing networks. Establishing a broad cross-agency support for this new collaboration would enable substantial advances in next-generation sensor capabilities by 2025.

To summarize, the challenge of populating current and future environmental observing networks with relevant sensing capabilities will not be met using current technology. Nanotechnology provides a major, largely untapped avenue for the transformative research and engineering needed to attain full utilization of these global observation networks, but oceanography lacks a critical mass of researchers who bridge with nanoscience, nanoengineering and industry. This situation will not change substantially without specific steps to lower the barrier between these distant research spheres, while also incorporating the industrial participation essential to linking sensor research to commercially available products.

Evolution of Autonomous Platform for Sustained Ocean Observations

*Russ E. Davis**

In the 16 years until 2025 I believe changes to oceanography will be substantially incremental. This is particularly true for ocean observing where even relatively modest technical developments take a decade. Rather than give broad (and unreliable) prognostications about oceanography over these years, I prefer to focus on one specific evolutionary trend.

The worldwide Argo program is showing how significant scientific results can be derived from proliferated long-term sampling with autonomous vehicles. Argo floats were designed to minimize the cost of routine observations with a very limited set of sensors (mainly a CTD) and as a consequence, these relatively inexpensive floats have long design lives, no redundancy, and can support only a few of the lowest-powered sensors.

While appropriate for general-circulation studies, Argo floats are not well matched to the upper ocean where comprehensive, expensive, and energy consuming sensor suites are needed. The complex interactions of air-sea fluxes, ocean mixing, primary production, biogeochemistry, marine optics, marine acoustics, and fauna in the upper ocean demand sustained observation with comprehensive sensor suites. The questions to be answered are myriad and of practical and academic interest. For example, we do not understand the main mechanisms supporting air-

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*Scripps Institution of Oceanography, University of California, San Diego

sea fluxes under high winds, or those responsible for stirring within the mixed layer or with the well stratified ocean below, nor how these processes affect the distribution of passive or living material in the ocean. Models that seek to predict variability of currents, ambient noise, rates of atmospheric CO_2 exchange, ecosystem evolution, optical properties, acoustic propagation, and even the oceanic consequences of global change all parameterize these processes, often using simple hypotheses about mechanisms that are calibrated with small data sets from a limited range of conditions. For simple questions like how deep a mixed layer will be, this leads to manageable quantitative errors. For complex questions, like ocean optical structure or nutrient cycling through the food web, it can lead to larger qualitative errors.

There are many ways to improve the factual basis for ocean models. New, more comprehensive, sophisticated and accurate sensors are needed. Improved data assimilation procedures for using data to test and improve models are essential. But the complexity of upper ocean biophysical coupling and the spectrum of associated time scales also demand many multi-year, multi-variable time series from available sensors deployed in a range of locations to isolate and quantify the key processes and parameterize them. Ships and moorings will often be the right platforms, but they are too expensive for proliferated long-term use. Profiling floats are economical, but today's platforms neither carry the energy nor provide the reliability to properly support comprehensive sensor suites. I believe a new class of platform will become a focus of future ocean observations. Characteristics of this class of vehicles follow:

- They will be free-drifting to avoid expensive deployment and recovery operations, will cycle vertically like Argo floats so a single sensor generates a profile, and will communicate only at the surface. Unlike Argo floats, they will carry comprehensive high-value sensor systems, will include system redundancies for reliability, will be re-used, and will have flexible mounting systems for varied sensors and a modular approach to power management, data recording and relay, and real time control of the vehicle and sensors.
- Relatively extensive sensor suites will generate profiles when the vehicle cycles to depth and back. Typical sensors to be carried include CTD, multiple wavelength fluorescence, optical backscatter and/or transmission, Laser Doppler Velocimeter or thrust probe for turbulence, Laser Optical Plankton Counter, chemical sensors for oxygen, pH, CO_2 and/or nutrients, and an optically sensed and occasionally flushed sediment trap. ADCPs for abso-

lute currents, multi-frequency sonars for acoustic backscatter and biological remote sensing, and/or accelerometer-based surface wave sensors could be used during extended surface intervals.

Additional characteristics that cannot be implemented today include:

- Novel energy sources or energy renewal from solar, wind or wave harvesting at the surface;
- Lightweight or remotely sensing instruments for wind measurements, air temperature humidity, and atmospheric optical properties that might profile upward;
- A combination of acoustic detection and optical identification that could provide reproducible measures of large-plankton and fish abundances.

What is needed now is not so much a specific instrument as a line of scientific and technical developments leading to a class of multi-use autonomous instruments.

Toward an Interdisciplinary Ocean Observing System in 2025

*Eric D'Asaro**

The changes in our ability to observe the physical aspects of the ocean over the last 30 years have been remarkable. I can now go to the Internet and get estimates of the ocean stratification, currents and atmospheric forcing anywhere in the world. These products are constructed primarily from climatologies, Argo float data, satellite observations and numerical model systems that assimilate both oceanographic and meteorological data. These estimates and the associated predictions are imperfect in many ways, but their skill demonstrates the remarkable success of physical oceanography in measuring and understanding the dynamics of ocean circulation. This observing system is dramatically changing the way that physical oceanography is conducted and will continue to do so. I hope, and expect, that similar changes will occur in the study of ocean biogeochemical processes by 2025. This note outlines my thoughts on the driving forces toward this change, possible pitfalls and the resulting social changes in the field. I will focus on the autonomous ocean component, since that's what I know, while ignoring the important satellite and modeling components.

The key force driving this change will be sensors. A crucial development behind physical oceanography's switch from water sampling to electronic sensors has been Seabird Electronics sustained efforts to honestly meet the WOCE standards for CTD accuracy. As a result, relatively inexpensive temperature and salinity measurements can be made by

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*Applied Physics Laboratory, University of Washington

almost any observational group with accuracies sufficient for all but the most demanding applications. There is a vast potential for similar sensor development in chemical and biological oceanography, particularly given the large number of potentially measurable quantities. At present, sensors for oxygen, optical properties, and some nutrients are becoming sufficiently accurate and easy to use. Carbon system sensors will follow shortly. In the longer term we can hope, for example, for genomic measurements of species distributions.

The second key element is platforms. The current technology of Argo floats is quite mature; gliders are rapidly maturing. Given the potentially large number of biogeochemical sensors, there is probably a need for vehicles with a larger payload capability than the current Argo floats. We have demonstrated the possibilities in the recent North Atlantic Bloom (NAB) experiment. Another missing piece is an AUV with the ability go long and slow, like a glider, as well as occasionally go fast when necessary, and be able to navigate in very shallow water (i.e., drive up to the dock). There is no reason why such a propeller driven vehicle cannot have the same endurance as a glider.

The third key element is communications, which is also probably in good shape. Argo has demonstrated the great utility of even a very poor communications system (i.e., ARGOS). Iridium is much better and the company appears to have a solid and growing customer base. There appears to be enough global demand for their voice and data services to support this business. The Iridium satellites will need to be replaced before 2025 and the company has been moving forward with plans to do this.

There will be many ways to stifle this change with good intentions. We are still in the early stages and the rate of innovation is high. There is a significant danger of freezing the design of a global observing system based on today's technology, and thus shutting out tomorrow's technology. For example, some call for a global system to observe oxygen from Argo floats. This is an excellent idea, but any plans to implement it should not preclude future measurements to observe the carbon system when these sensors become available. We do not yet know how to build an observing system for global ocean biogeochemistry. The ocean's biogeochemical system is very complex, both in the number of variables and in its high degree of spatial and temporal variability. An observing system cannot measure everything, but must measure many things in many places. Figuring out how to do this right is a challenging and important task, with considerable "intellectual merit."

Managing the balance between science and engineering will also be challenging. Ocean science clearly needs good engineering, but keeping the engineering relevant to science requires constant attention. Fortunately, the task of designing new sensors, adding them to autonomous platforms and using them in creative ways can occur on the scale of a small group of PIs and engineers funded by grants and working with industry. If funding for this type of activity is available, it is easy to imagine several groups of this type maintaining a period of "transformational research" in which new ways of sampling the biogeochemical system are developed, tested and, through industrial partners, made available to the broader community.

This new technology will change the way that ocean science is conducted. Already, the large stream of data freely available on the Internet has allowed many creative and productive physical oceanographers to operate with little direct connection to the process of ocean measurement. This is good for science, since it brings more brains to bear on the important problems, and good for education, because it allows faculty and students across the world to participate in research. Oceanography is becoming much more like meteorology, with oceanographers distributed more widely across academia, government and industry and with a significant quantity of applied work. As in meteorology, research programs can now be firmly set within a synoptic context defined by the global observing system. We can, for example, study the links between productivity and ocean physics not just "in the Sargasso Sea," but also at chosen locations within its eddy field.

It is inevitable that autonomous platforms will compete with the research fleet. Floats, gliders and moorings now allow us to sample the global ocean in ways that are impossible with ships. Two days of global ship time now cost roughly the same as a glider or 4-6 Argo floats. Today, most biogeochemical measurements are done from ships, thus providing a strong constituency for the maintenance of the fleet. As more and more biogeochemical oceanographers get their data from the web, rather than on cruises, this constituency will decrease. However, ships will always be able to measure more things in more detail than floats or gliders so there is plenty of room for creatively combining the two methodologies.

I hope that these new observing technologies will promote interdisciplinary research within oceanography and between it and the broader earth systems sciences. Physical oceanography has had remarkable success by focusing on the dynamics associated with the spatial and temporal variability of the ocean. The new sensors will allow physics and biogeochemistry to be sampled on the same space and time scales and thus lead to large advances the understanding their interconnections. Just as the observational tools of physical oceanography applied to geophysical fluid dynamics resulted in today's observational system, we can hope that the new biogeochemical tools applied to rigorously test and improve today's primitive biogeochemical models will also result in a system for understanding and predicting the ocean's biogeochemistry. Such advances will occur only through the formation of effective interdisciplinary teams spanning the ocean and earth science disciplines.

Small Scale Ocean Dynamics in 2025

*Jonathan Nash**

Major advances in oceanography result from methodical sampling fortunate enough to capture details of new processes at work. While web-accessible observatories will provide one source of ocean data, small scale physical dynamics will continue to be elucidated using novel instrumentation and well-planned, intensive studies. These are key if internal waves and the resultant turbulence are to be generalized and identified within sparse datasets or parameterized in imperfect models. However, the changing infrastructure and technological advances in electronics, energy, and computational power by 2025 will change the way these studies are conducted. Together these will permit real time integration of process-driven experimentation, ancillary observatory data and numerical modeling. By 2025, we will have new instruments, more powerful computers, and more efficient access to ancillary data. But the discoveries will still be made by inquisitive scientists interpreting real data that streams to us either while at sea or from afar. To move these discoveries to the next level, we will continue to need advanced, efficient vehicles (ships?) with long-range acoustics, rapid profiling capability, etc.

From a technological standpoint, oceanographic instrumentation will benefit from the same advances in high-capacity, low-power electronics that now enable a year's worth of turbulence data to be acquired with a small battery pack, miniaturized electronics and penny-sized storage

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*College of Oceanic and Atmospheric Sciences, Oregon State University

devices. By 2025 we will see routine use of high data-rate sensors in both autonomous roving platforms and moored applications. Instrument suites previously restricted to lab or tethered applications may see routine long-endurance, remote usage as cabled observing systems become a reality.

But new energy systems may provide the biggest change. By 2025, lithium batteries may have gone the way of the phonograph; new energy technologies and/or efficient propulsion systems may power propelled autonomous vehicles for many months instead of many hours. Imagine if energy capacity were to no longer limit mission length, data transmission rates, internal computations, etc. The possibilities for remote, *in situ* sampling would be almost endless. High-speed autonomous vehicles with high-power acoustics and other sensors could sample in ways almost unimaginable today. Could these eliminate the need for manned ships for physical sampling? More realistically these advances will be incremental. But crises inspire change; even increased efficiency will change our capabilities.

A proliferation of enhanced, long-endurance autonomous platforms could provide a globally distributed set of turbulence and internal wave measurements. Through a combination of routine and targeted experiments, these would capture the dynamics of events that occur both frequently and infrequently, under extreme conditions (hurricanes, high seastate) and in remote locations (high latitude winters). To date, these dynamics have been grossly undersampled due to our general desire to stage experiments in easily accessible regions and when seas are calm. By 2025, I believe we will have made substantial progress towards both quantifying these processes and incorporating their effects into our modeling framework.

Oceanography in 2025

*Dana R. Yoerger**

Underwater vehicles and related *in situ* sensors will advance significantly in the next 16 years in terms of operating range, endurance, and in the types of measurements they can make. By vehicles, I am referring to powered autonomous underwater vehicles, remotely operated vehicles, gliders, and floats.

While AUVs are presently operational, the vehicles in everyday use today do not use their limited energy supplies to best effect, with the possible exception of gliders. As a result their range and operating speeds are limited. Vehicle drag is often dominated by external appendages such as hydrophones, antennas, and recovery aids rather than the hull form itself. Significant gains in the practical efficiency of propulsors and significant reductions in "hotel" loads (control system, sensors, etc.) are also possible. Many of these improvements can be obtained through hard-nosed, competent engineering rather than fundamental invention. AUV research and development groups around the world are actively involved in such developments, and we can expect that new or improved vehicles taking advantage of these efforts will be coming online in the next few years and will be very mature by 2025. A several-fold improvement in power consumption is nearly certain.

In the next 16 years, our present energy sources (lithium primary cells, lithium-ion secondary cells, for example) will most likely be completely surpassed by new developments. Possible near-term energy

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*Woods Hole Oceanographic Institution

sources include lithium-seawater batteries and several types of fuel cells. These developments will be driven by applications outside oceanography. Our present battery technologies available for AUVs were driven by the needs of relatively small devices such as laptop computers and cell phones; the next generation of energy sources will be driven by the storage needs of systems with much larger energy requirements in response to larger societal needs such as distributed power generation (solar, wind, etc.) and electric vehicles. A developer of lithium-seawater batteries projects nearly a factor of 10 increase in energy density over present lithium primary cells. These improvements would totally reshape our use of both powered AUVs as well as gliders, where the power needs of the sensor packages impose fundamental limitations.

To take advantage of the combination of reduced power usage and increased energy supplies, the vehicles must be reliable, be able to localize their position without aid from a vessel or mission-specific seafloor beacons, and have sufficient intelligence to deal with unanticipated events during the mission. No doubt our capabilities in these areas will be substantially improved by 2025. While methodological breakthroughs are likely, pragmatic engineering progress will allow consistent progress on all these fronts.

These developments, properly assembled into well-designed operational systems, will permit a revolutionary new approach to many oceanographic problems. The combination of improved power use and significantly improved energy sources can increase the present range of vehicles (presently on the order of 100s of km) many fold, perhaps by a factor of 20 to 50. Can we imagine how we would take advantage of an AUV with 5000 km range?

In situ sensing technologies are on the brink of a revolution that can fundamentally change our understanding of a variety of important ocean processes. As an example, compact, low-powered mass spectrometers that can make laboratory-grade measurements even at great depth are coming online now. Likewise newly emerging *in situ* genomic sensors can assess the presence and even abundance of specific organisms. These powerful instruments can allow us to measure many quantities *in situ* for which we must presently secure samples for analysis in the laboratory. The change from laboratory analysis to *in situ* sensing not only dramatically lowers the cost per measurement, it also enables vastly improved spatial coverage as well as long-term time series observations that conventional sampling and laboratory analysis cannot possibly accommodate.

AUVs with such capabilities will enable fundamentally new operational paradigms. Present-day operational AUV costs are dominated by the cost of the vessel and the support personnel that must accompany the vehicle. But AUVs with reliable long range capabilities could operate with full autonomy provided other technical issues such as localization could be solved, greatly reducing costs since the vessel and support crew could go "off the clock" shortly after the vehicle is launched, either returning to port or go to work on a different task. The combination of increased range and new *in situ* sensors will allow spatial coverage and data densities unthinkable today. Likewise, multiple-vehicle operations using cooperative control schemes will further enable us to capture dynamic features over large areas.

A recent paper by Davis and McGillicudy (2006) illustrates the power of *in situ* sensing operating on a submerged platform with long range. They towed the Video Plankton Recorder (VPR), an *in situ* microscope, behind a research vessel in an undulating pattern between the surface and 130 meters along a continuous track over 5500 km in length. The resulting images, classified using automated techniques amenable to on-vehicle processing, showed unexpected widespread populations of the N_2-fixing colonial cyanobacterium *Trichodesmium*, leading to a fundamental revision of our understanding of nitrogen fixing in the world's oceans and resolving a conundrum that had not been resolved by conventional sampling methods. By 2025, we could have multiple long-range AUVs operating over such ranges in cooperating teams, each equipped with suites of *in situ* sensors such as the VPR, mass spectrometers, and a variety of genomic sensors. Such observations, combined with our improving ocean modeling capabilities, have the potential to fundamentally rewrite our understanding of many ocean processes.

Remotely operated vehicles are likely to evolve significantly in the near future as well. Taking advantage of many AUV technologies, self-powered remotely operated vehicles communicating with light optical fiber tethers, acoustic communications, or optical links will enable direct human control or at least human supervision without the heavy cables required to transmit power. By eliminating heavy winches, these vehicles will be more portable, will be able to work from smaller vessels, and may not require dynamic positioning. These qualities will increase the pool of candidate vessels and make the new vehicles applicable to event response or for cruises that require intervention or sampling capabilities but don't require a full ROV spread.

Despite the advances with AUVs and gliders, our success will always depend on ships. Expanded AUVs equipped with new *in situ* sensors will not eliminate the need for sampling, and many types of sampling will remain in the domain of human-occupied vehicles, remotely operated vehicles, and specialized facilities (such as the new long core system on *Knorr*) that require capable vessels. Many of these sampling efforts are crucial elements of programs where oceanography is most relevant to pressing societal needs such as understanding past and present climate variability. Undoubtedly laboratory instrumentation will continue to evolve and the resulting capabilities will always exceed those of *in situ* sensors. For example, *in situ* mass spectrometers will enable many new types of investigations; but they will not have sufficient performance to replace accelerator mass spectrometer facilities to determine the ventilation age of seawater. AUV capabilities will certainly improve for some types of sampling, but for the foreseeable future the most demanding types of sampling (gathering hydrothermal fluids, retrieving fossil corals, or taking cores, for example) will require skilled human intervention. The role of our ships may change, as their capabilities as tenders for launching vehicles, floats, and gliders will become much more important. But new ships well matched to this role will certainly be required in my view.

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The Research Vessel Problem

J. N. Moum, Eric D'Asaro,† Mary-Louise Timmermans,‡ Peter Niiler§*

The workhorses of the US oceanographic research fleet are the 12 Global, Ocean and Intermediate Class vessels that are presently operated by UNOLS. Of these, six (*Melville*, *Knorr*, *Oceanus*, *Endeavor*, *Wecoma*, *New Horizon*) are well past their projected 30 year service lives, with midlife refits > 10 years ago. Another (*Seward Johnson*) is 25 years old. The remaining five ships have projected service lives to about 2025. The Navy plans to fund two new Ocean Class vessels, to be launched in the next five plus years. With these two new vessels, the US oceanographic research fleet goes from 12 vessels to seven vessels long before 2025 under present planning guidelines, although five of these seven will be past the ends of their service lives by then.

At the same time, the oceanographic community has incurred new obligations in assuming responsibilities for global, regional and coastal observing systems. These include moored, autonomous and drifting assets. While these are commonly billed as replacements for shipboard observations (notwithstanding the need for ships for servicing), their much smaller payload and power capabilities will mean that ships will almost always be capable of more sophisticated, difficult and novel measurements. Ships can go faster than autonomous platforms, can carry

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*College of Oceanic and Atmospheric Sciences, Oregon State University
†Applied Physics Laboratory, University of Washington
‡Woods Hole Oceanographic Institution
§Scripps Institution of Oceanography, University of California, San Diego

heavier packages and support a wider range of simultaneous measurements. New sensors and methods, even those destined for autonomous platforms, will inevitably be tested on ships. New autonomous platforms will be tended by ships before being set off on their own. Novel sensors on autonomous platforms will need to be calibrated and verified by more traditional measurements made on ships. Hydrographic programs that catch the water from surface to seafloor must continue and become more complex with the addition of many more biochemical compounds and molecules to be sampled—these require ships. The Southern Ocean is beyond the reach of autonomous probes, necessitating shipboard monitoring.

By 2025, we envision that ships will continue to be a vital part of oceanography, but that their role will have shifted. They will still do the heavy lifting of deploying moorings, servicing long-term arrays and making intensive short-term surveys or hydrographic lines using large, heavily instrumented packages. They will deploy, recover and tend mobile autonomous platforms, although some of this work may be better accomplished from smaller, faster vessels than we have today. Most importantly, we expect that intensive research programs aimed at new understanding of ocean processes will be conducted by a mix of ships (including necessary multi-ship experiments) and other platforms. Intensive ship surveys measuring, for example, a full suite of biogeochemical properties for ecosystem studies, or high-resolution 3D towed and acoustic surveys of ocean density and microstructure for mixing studies, will be supplemented by autonomous platforms placing these intensive ship measurements in a larger space and time context.

For example, the most important problem in physical oceanography today is a resolution of the subgrid scales of ocean circulation models. These scales occupy a broad range in time and space and have great geographical variability. We know that present parameterizations of the physics at these scales are incorrect to varying degrees and that this leads to unknown and unpredictable uncertainties in projections of future global climates, a matter of significance as our planet undergoes large and rapid climate change. Because of the complexity of the processes at subgrid scales, they are not simply studied by routine observation. Rather they require fixed, focused and intensive observations from mobile platforms. To observe such flows requires real time analysis of incoming data from multiple sensing devices by scientists at sea, *supplemented* by measurements on fixed and autonomous platforms.

In a 1997 article in *Oceanus* RADM Pittenger, former Oceanographer of the Navy, summarizes the 15-year modernization of the fleet to 1997 that included AGORs 23-25 and several midlife refits, and states, “There is a very important lesson here . . . replacement of ships is a decadal process.

Given a nominal life of about 30 years, planning for ship replacement must begin before the ships to be replaced are 20 years old."

The addition of the Navy's two new Ocean Class Vessels is welcome and important, but these are not enough. Oceanographers are in danger of not being able to respond to the challenges of the 21st century. Not yet, but soon.

"Ocean Mapping" in 2025

*Larry Mayer**

It is both exciting and intimidating to speculate about where our field will be 16 years from now. While I will not be so bold as to try to address the entire field let me start my comments with some overarching statements and then drill down and focus on those areas that I have most experience with—namely seafloor mapping and the use of acoustic and other sensors to quantify the deep-sea environment and understand deep-sea processes. In the broadest sense I firmly believe that over the next few decades we will see a greatly increased permanent presence for instruments and sensors in the ocean. This will be facilitated by both NSF's research observatory efforts (OOI) and NOAA's more operational observatory system (IOOS). The combination of the observatory infrastructure and advanced remote sensing tools with evolving modeling and visualization capabilities will revolutionize our predictive capabilities. Most importantly it will probably demonstrate how little we really understand about the small scale temporal and spatial variability of ocean processes. Lessons learned from these large scale experiments will inevitably be transferred to smaller scale efforts (and vice-versa—e.g., we are presently building a "mini-observatory" to monitor Portsmouth Harbor and innovations developed in our and other small scale efforts may also be adopted in the larger programs). Along with this I believe we will see a rapidly increasing role for autonomous vehicles (both powered and gliders and both large and very small) providing a cost-effective

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*Center for Coastal and Ocean Mapping, University of New Hampshire way to compromise between spatial and temporal coverage. Finally, I also strongly believe that with increased satellite communication capability we will be able to transfer much of the processing, interpretation and even decision-making load from the ship (or AUV) to shore-based facilities. We are experimenting with this now through "Telepresence Consoles" which provide multiple high bandwidth audio, video and data channels to a shore-based facility. In our work with NOAA's Ocean Exploration program, multibeam sonar data is transmitted in real time to our lab (where we stand watches as if we were on the vessel), where it is processed and 3D maps are returned to the ship in near-real time. The potential of this capability for support of many types of sea-going operations is tremendous.

With respect to my own field of "ocean mapping," our goal is to make the ocean as transparent as possible, yet we are faced with the fundamental limitations of an optically opaque medium and the tradeoffs between acoustic resolution and propagation. Over the past 25 years, concomitant advances in sonar design, positioning systems, computing power, and visualization capabilities have led to the development and application of multibeam sonar technology, and with it, an evolution from sparse 2D profiles of the seafloor to full coverage 3D images that have revolutionized our understanding of a range of seafloor-interacting processes. We are now poised for the development of the next generation of swath mapping systems. These systems will evolve from current narrow-band systems to broad-band, multi-frequency or chirp-based systems. The increased bandwidth of the next generation of swath mapping systems will increase spatial resolution but more importantly will provide a "multi-spectral" look at the seafloor (and water column) and with this the possibility for the remote derivation of quantitative seafloor or mid-water (e.g., fish) properties (think of this as the difference between a black and white satellite image and a full-color image). Along with broader bandwidth, the next generation of swath mapping systems will use multiple phase detections within each beam footprint, evolving to the point where lateral resolution will be limited by the pulse length rather than the current limitation of beam footprint. Additionally, these new systems will use 2D arrays that will allow multiple beams to be formed in both the along-track and across-track direction. Finally, the new generation of systems will allow information to be collected from the entire water column (not just the seafloor), resulting in an evolving real time high-resolution 3D image (when combined with appropriate real time visualization software) of the seafloor and targets in the water column (e.g., fish, oceanographic fronts—including Doppler measurements, or other targets of interest). Many of the technological advances described here are underway in some form or another (Figure 1), but it will take at least 10 years bring them to

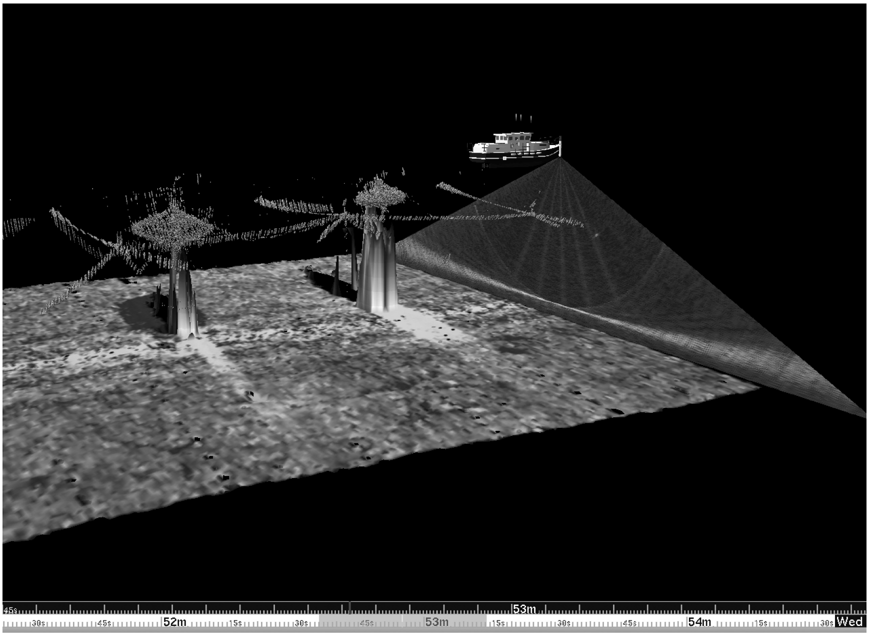

FIGURE 1 Interactive 3D visualization of multibeam sonar collected both seafloor and mid-water data. Midwater targets are open-ocean aquaculture cages and anchor lines.

full fruition. These efforts would benefit greatly from research leading to the development of efficient and very broad-band acoustic transmitters.

While the developments described above will greatly aid in the ability to characterize both the seafloor and the water column, resolution of the finest scale features will always require bringing the sensor closer to the target. Remotely operated vehicles offer one approach to bringing the sensors close to the target (with little constraint on power and data transmission), but ROV surveys are time consuming and costly. AUVs (both powered and gliders) offer a tremendous opportunity to deliver sensors anywhere in the medium and while power and data transmission compromises are inevitable, they open up a vast new range of opportunities to monitor and capture ocean processes at a range of scales (as well as stealth). Thus I hope to see in the coming decade rapid evolution of AUV capabilities. Critical amongst needed developments will be more efficient power supplies, sensors that require less power, and development of more sophisticated control software that will allow adaptive behavior of

AUVs depending on mission requirements (e.g., modify mission plan in response to detection of particular target or parameter). I also suspect we have only begun to scratch the surface on capabilities for non-powered AUVs (e.g., gliders) or devices that use solar-, wind-, or wave-power as an aid. For an AUV, intermittent visits to the surface have many advantages including boosting power, transmitting data and updating position.

A constraint on many AUV-based surveys is the loss of high resolution positioning capability once submerged. Long-baseline transponders are the current solution but these are expensive and time consuming. An area ripe for progress is the development of "hybrid" modes of high-resolution positioning for AUVs. Current kinematic GPS capability allows cm-level positioning as long as the satellites can be viewed. The combination of free-floating kinematic GPS buoys and some sort of acoustic ranging system may offer the possibility of greatly extending the positional accuracy of AUV-based surveys in an unconstrained (geographically) and cost-effective manner. An important research area associated with this will be improved underwater acoustic communications. Once again—increased bandwidth (along with clever compression algorithms) will be the key.

Seismic Oceanography: Imaging Oceanic Finestructure with Reflection Seismology

*W. Steven Holbrook**

INTRODUCTION: OLD DOG, NEW TRICK

Seismic oceanography (SO) is a new approach to studying interior ocean structure by applying an old tool—marine seismic reflection profiling. Reflection seismology is a standard technique used in industry and academia for imaging the solid earth using reflected sound waves. We have recently discovered that finestructure—temperature variations at vertical scales of meters to tens of meters caused by internal waves, intrusions, and mixing processes—can be imaged quite well with seismic reflections at 10-150 Hz—the frequency range commonly used in seismic reflection profiling. Our results (e.g., Holbrook et al. 2003) show spectacular images of thermohaline finestructure in the ocean (Figure 1); features such as intrusions, internal waves, and mesoscale eddies are clearly visible. These images show the ocean in a way it has never been seen before.

The past several years have seen rapid progress in defining this new tool. We have achieved a basic physical understanding of the origin of the acoustic reflections (predominantly temperature finestructure at the 10 m vertical scale). The physical basis for SO is the presence of "boundaries" in the ocean caused by strong vertical gradients in either density or sound speed. The strength of reflections from a "sharp" discontinuity can be described by the "reflection coefficient," $R = (\rho_2 c_2 - \rho_1 c_1)/(\rho_2 c_2 + \rho_1 c_1)$,

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*University of Wyoming

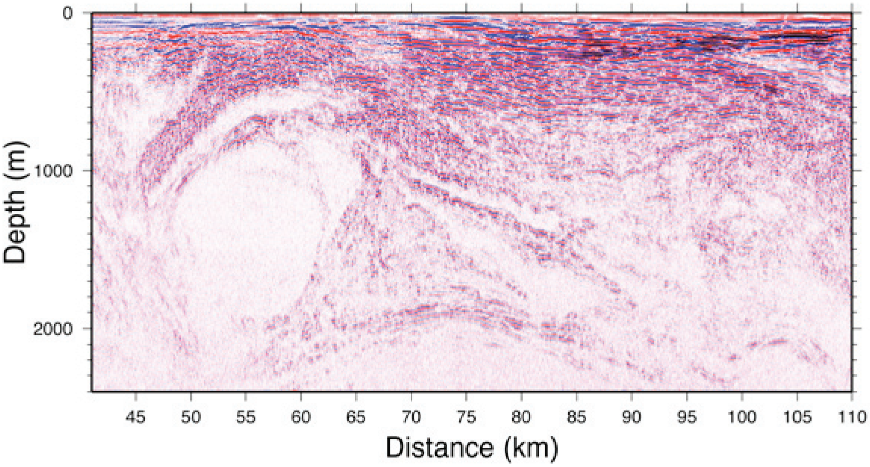

FIGURE 1 Image of a mesoscale eddy beneath the North Atlantic Front.

where ρ and c represent density and sound speed, respectively, and the subscripts represent layers (layer 1 overlies layer 2). Because sound speed (i.e., temperature) dominates, to first order these images can be thought of as maps of dc/dz at vertical scales of ~10 m.

Our group and others have produced fascinating images of finestructure in numerous ocean settings, including fronts, Meddies (Figure 2),

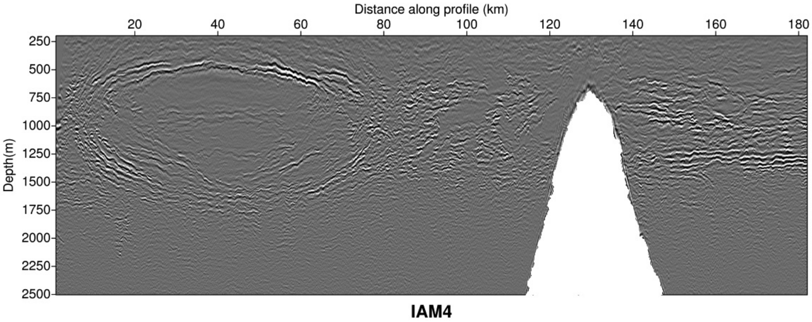

FIGURE 2 Image of a Meddy in the Gulf of Cadiz. The Meddy is visible as the prominent oval shape on the left side of the figure. Note the strong contrast in finestructure characteristics on either side of the Gorringe Bank (white protrusion centered at 130 km). Image courtesy of Berta Biescas, Marine Technology Unit—CSIC (Spanish National Research Council).

intrathermocline lenses, warm core rings, watermass boundaries, and thermohaline staircases, some of which raise unexpected questions about the processes controlling the distribution of oceanic finestructure. We have also shown that quantitative information on, for example, internal-wave spectra (Figure 3) and temperature can be gleaned from these data. These observations raise the intriguing possibility that seismic reflection profiling may become a tool of great usefulness to physical oceanographers in observing and characterizing ocean structure and dynamics.

Note that SO differs from traditional acoustic oceanography in several ways, including the dominant sound frequencies (and thus the resolution), the targets, and the acquisition methods. Reflection seismology uses much lower frequencies (10-200 Hz) than traditional ocean acoustics. The resolution is thus lower (vertical resolution O(5m)), which means that our targets are fundamentally different: rather than scattering from mil-

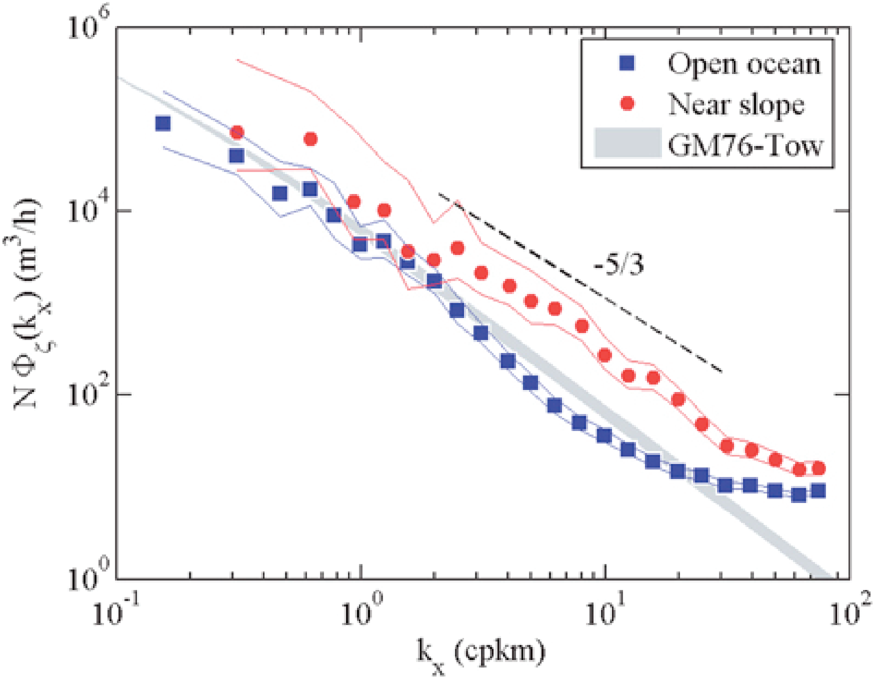

FIGURE 3 Horizontal wavenumber (Kx) spectra produced from seismic reflection images in the Norwegian Sea. Gray field is the GM76 tow spectra. Two sets of reflectors were tracked: open-ocean reflectors, which show good agreement with GM76, and near-slope reflectors, which show enhanced internal wave energy levels. From Holbrook and Fer 2005.

limeter- or centimeter-scale objects (e.g., plankton), we record specular reflections from temperature and density finestructure. Specialized equipment is necessary at sea; sound sources are usually pneumatic sources that release high-pressure air into the ocean, and the reflected wavefield is recorded on a kilometers-long hydrophone streamer towed behind the vessel.

ADVANTAGES AND DISADVANTAGES OF THE TECHNIQUE

The principal advance that SO offers is the ability to track oceanic finestructure laterally at relatively high spatial resolution: the typical lateral sampling of seismic images is 6.25 m. These images provide, first and foremost, a means of "flow visualization" (to borrow Larry Armi's description) akin to schlieren images, which show structural detail that can provide intuition into dynamical processes. Other advantages offered by reflection profiling as a complement to standard oceanographic measurements include the ability to simultaneously image large volumes of ocean, over full ocean depth, and the ability to do 3D and timelapse imaging. Especially when combined with *in situ* physical oceanography (PO) observations (either from expendables or from more detailed measurements), these images have the potential to add great value to traditional PO investigations of ocean mixing processes.

The principal disadvantage of SO is that it cannot provide information where finestructure is weak or absent. This means, for example, that the method is ill-suited to imaging the abyss, where the low stratification prevents gradients in c and ρ of sufficient magnitude to produce acoustic reflections.

WHAT MIGHT SO PROVIDE?

We are now poised to make several significant advances in seismic oceanography. Two developments are particularly promising. First, we now have the means to create the first 3D and timelapse 3D ("4D") images of oceanic finestructure, which enable 3D maps of, for example, internal wave energy. Second, recent work shows that reflection images have the potential to produce quantitative estimates of turbulence dissipation by applying the Klymak & Moum theory of horizontal wavenumber spectra. Because of the spatial density of such data, we have the possibility of producing maps of dissipation over large regions of the ocean. This approach is in its infancy and requires testing and truthing, but the potential applications are obvious.

SO is applicable to studying any process that creates, destroys, disrupts, or deforms finestructure in the ocean. Problems that can be addressed by SO thus include (but are not limited to):

- Where does mixing occur—in particular, where are mixing hotspots in the ocean?
- How is boundary mixing influenced by critical slopes and seafloor roughness?
- How, and where, do fronts and eddies shed energy into the internal wave field?
- What are the lateral length scales of oceanic finestructure, and what controls these length scales?
- How does isopycnal stirring create temperature variance in the ocean?
- What are the 3D shapes of internal wave packets in the ocean?
- What controls the generation of strong internal waves in places such as the South China Sea?

SEISMIC OCEANOGRAPHY IN 2025

Predicting the role of SO in oceanography in 2025 is quite difficult. By that time, SO could either be a historical footnote or (one hopes) a widely used technique in oceanography. The technique has much promise in imaging (and thus mapping) processes that have an expression in temperature/density finestructure. Fulfilling that promise will require:

- improved and continued collaboration and communication between the PO and seismology communities via workshops;
- successful development and testing of techniques to invert low-frequency acoustic returns for oceanic properties of interest (temperature, density, internal wave energy, and turbulence); and
- a willingness on the part of funding agencies to invest in this methodology by supporting focused field and laboratory work.

A large extant database of seismic reflection profiles contains useful information that should be mined, but substantial progress needs joint PO/seismic field programs that collect state of the art data at the same time and place.

RESOURCES

Holbrook's web page: http://www.steveholbrook.com/research/seismic_oceanography/

Recent ESF-sponsored workshop on SO: http://www.cmima.csic.es/sow/

EU-GO project: http://www.dur.ac.uk/eu.go/

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The Ocean Planet 2.0: A Vision for 2025

*Justin Manley**

Henry Stommel's visionary article of 1989, The Slocum Mission, anticipated fleets of autonomous vehicles roaming the ocean over long time scales and collecting unprecedented new oceanographic data. Now, 20 years later, Stommel's vision has been demonstrated, if not achieved. With the deployment of thousands of Argo floats, and an order for construction of a large fleet of gliders imminent, the idea of many autonomous systems monitoring the ocean is no longer a vision. It is today's reality.

But beyond new approaches to data collection, a vision for the future of oceanography must consider much broader data utilization. In 2009 much of the developed world has moved to a "data consumer" culture, relying upon myriad networked devices to download music and weather reports while uploading family photos and their latest thoughts to social networking sites. Meanwhile, developing economies are seeing rapid penetration of mobile phones with data services used, for example, to check crop prices at market to determine the best time to ship produce. Exchange of professional and personally generated data underlies many activities of modern consumers and producers around the globe.

New technologies are bringing this model to the ocean. In 2004, the NOAA Ship *Ronald H. Brown* deployed an ROV over the wreck of the RMS *Titanic*. High-definition video from the ROV was relayed to shore via a

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*National Oceanic and Atmospheric Administration, Ocean Exploration and Research; Batelle (This view is exclusively the opinion of the author. It does not represent the policy of NOAA or Battelle.)

high bandwidth satellite link and the public watched nine minutes of live footage of the wreck on the National Geographic Channel. In 2009 data from that cruise is now contained in the newly released Google Ocean™. Those who missed the 2004 broadcast can relive the exploration via freely available software, again from the comfort of their home. No seasickness required!

Also in 2009 the NOAA Ship *Okeanos Explorer* will complete its shakedown. This vessel will permanently employ ROV and telemetry capabilities like those used in the 2004 *Titanic* expedition. Ocean exploration is following the model of NASA's Mars rovers, sending home digital data and limiting the focus on physical samples. Once captured as easily replicated ones and zeros, rather than atoms and molecules, it is easy to envision new discoveries rapidly moving into tools like Google Ocean™.

This data flow will not be limited to pictures or bathymetric maps. Viewers of the popular television show "CSI: Crime Scene Investigation" have come to believe a sample of dust or liquid can yield its secrets to a machine composed of spinning tubes and clicking mechanical arms, linked to an apparently omniscient database. Such sophisticated lab analysis is moving from the bench to the ocean. In 2009, new instruments based on mass spectrometry and laser-Raman techniques have demonstrated the ability to measure dissolved gases or analyze the composition of subsea materials *in situ*. Other tools are combining powerful optical systems with advanced vision processing software to image and analyze phytoplankton in real time. Wet sample nets on deck are giving way to files emailed around the globe.

Today's technologies are revolutionizing the collection of oceanographic data. Distributed autonomous systems and ocean observing networks will yield rich new data sets and challenge the ability of users to manage and apply this resource. By 2025 the ocean community will be focused on consumption, and meaningful use, of ocean data rather than simply struggling to collect information. Some likely outcomes of broadly available ocean data include:

- Emerging tropical storms will be rapidly detected and continually compared to models providing superior warning to coastal communities.
- Pollution in fragile ecosystems will be rapidly identified and followed to its source for prompt enforcement and cleanup.
- Fisheries will be closely monitored, guiding fisherman to the most efficient catches and resource managers to truly sustainable natural resources.
- Beach goers, scuba divers, surfers and other coastal users will have immediate access to real time conditions and highly accurate

forecasts of winds, waves, currents, water temperatures and any hazardous conditions.

- Shipping traffic will be optimized for time, cost and emissions impacts yielding both economic and environmental benefits.

The abundant, and regularly updating, ocean data will be appreciated even far inland. A channel dedicated to climate, much like today's Weather Channel™, will broadcast three-dimensional models of the fluid (air and water) earth and viewers will come to recognize the flow of the Gulf Stream just as they do the Jet Stream in 2009. The cell phones, or wristwatches or bionic implants of 2025 will connect individuals to global and regional models. Perhaps Google™ will give way to online "goggles" allowing the public to immediately understand the planetary impact of their transportation and consumption choices. Citizens will visit their undersea national parks and monuments through immersive media experiences that will make today's virtual reality look like black and white television does in this age of high definition plasma screens. Through such experiences, and constant connection to the ocean and its impacts, society in 2025 will recognize what another visionary, Arthur C. Clarke, noted some time ago:

> "How inappropriate to call this planet Earth when it is clearly Ocean."

Force Projection Through the Littoral Zone: Optical Considerations

*Kendall Carder**

". . . Maritime supremacy is still the most effective means to project power" (Khine Latt, personal communication, 2008). Power projection or smuggling through the littoral zone will be enhanced by stealthy transit. What defenses may be needed by 2025?

NAVIGATIONAL AIDS

Active navigational aids such as sonar in coastal waters are noisy, while quiet, optical methods may be range-limited, especially for turbid waters. Risk of visual surveillance in clearer waters may limit opposition forces and smugglers to nocturnal operations even when other considerations (e.g., tides, currents, etc.) may support daylight transits. What methods might be employed by 2025?

Recent theory and applications (e.g., Fournier 2006 and refs. cited) suggest that ultra-fast (femtosecond) near infrared (NIR) lasers can reduce exponential light attenuation to only linear attenuation for pure water. Incredibly, absorption lengths in the linear mode could be kilometers. However, uncertainties remain with regard to attenuation (absorption and scattering) of fast pulses by phytoplankton and colored dissolved organic matter, delaying practical applications of this technique until much more research takes place.

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*SRI International

With a navigation or communication LIDAR always pointing below the critical angle, direct light passing through the surface for detection by aircraft would be negligible. Light reflecting off the bottom would become diffuse, reducing its perceptibility from above the surface. The marked extension of range provided by successful applications of femtosecond laser and LIDAR packages would be useful for improved navigation, mine detection, and optical communications.

OPTICAL COMMUNICATIONS

Even without the range extension expected using ultra-fast lasers, optics can play a role in quietly off-loading high-rate data (e.g., bottom imagery, mine searches) collected by AUVs. Circling around a submerged communication buoy, low power optical modems on the AUV can communicate with the buoy at visible wavelengths. The buoy itself can optically communicate directly with a loitering UAV using a very-thin, optical-fiber "periscope." It can also communicate directly with UAVs from below the surface at visible wavelengths.

3D OPTICAL MODELS

Entire 3D light fields have been calculated with a hybrid marine optical model (HyMOM) that calculates the response functions for various elements (e.g., water cubes, boundaries) and combines them into an environment. The environment is then modeled by inserting light at the air-sea interface and solving iteratively using a relaxation approach. The light field is calculated for all model directions at each cube face (e.g., every 25 cm). Higher resolution in directional and spatial dimensions greatly increases the memory requirements and computational speed needed for rapid solution of these problems, so simulating detection of smaller objects requires significant model enhancement in both angular and spatial resolution.

Mine-detection strategies can be assisted by optical models that simulate lighting and shadow effects. Various underwater vehicles, swimmers, and objects of interest (e.g., mines) can be inserted into a variety of model environments for optical simulation of their ease of detection from any direction and depth. Preliminary model examinations have been made of the light structure (e.g., shadows) associated with channels and ridges, and beneath ship hulls (Reinersman and Carder, 2004), and higher-resolution models of pilings have been developed (Carder et al. 2005) as a backdrop for evaluating problems associated with the search for underwater mines. The spectral character associated with vertical and horizontal light fields about a modeled coral head (Carder et al. 2006) has also been made to evaluate potential stress of UV-rich environments on coral and foraminifera.

Stealthy force insertion or smuggling from over the horizon using effective camouflage during daylight hours may be practical by 2025. Active camouflage permits adaptation to changes in the albedo of the background scene, whether in shallow water over a bright bottom or over a dark bottom. 3D optical modeling (Figure 1) has demonstrated that simple, active camouflage can reduce the contrast of an object over a bright, sand bottom by up to six-fold (Carder and Reinersman 2006). Shadows of underwater objects above bright bottoms significantly increase object detectability, suggesting how an insertion might best be detected. While it may seem counterintuitive, superior active-camouflage potential is found near the surface (Figure 2) because shadows on the bottom are much more diffuse for near surface versus near bottom transits on sunny days. Deeper transits may be appropriate on cloudy days, however.

By 2025 model simulations can be carried out to select the optimum

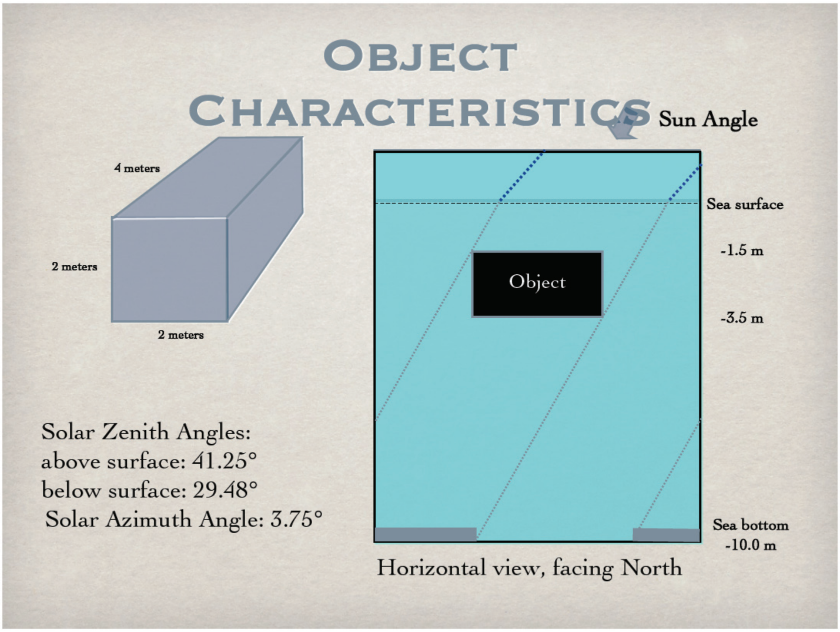
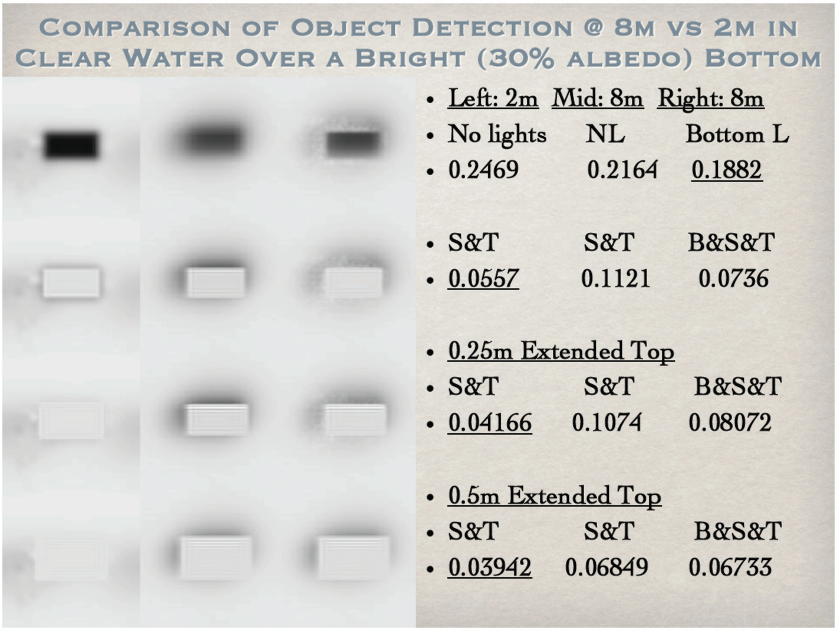


FIGURE 1 HyMOM model layout for a 2 m × 2 m × 4 m object in clear water at 1.5 m below surface over a bottom with a 30% albedo. The additional bottom shadow at right is from a virtual object in a box to the right caused by model wrapping (photons leaving one side re-enter from the opposite side to simulate an infinite ocean).



COMPARISON OF OBJECT DETECTION @ 8M VS 2M IN CLEAR WATER OVER A BRIGHT (30% ALBEDO) BOTTOM

- Left: 2m Mid: 8m Right: 8m
- No lights NL Bottom L
- 0.2469 0.2164 0.1882

- S&T S&T B&S&T
- 0.0557 0.1121 0.0736

- 0.25m Extended Top
- S&T S&T B&S&T
- 0.04166 0.1074 0.08072

- 0.5m Extended Top
- S&T S&T B&S&T
- 0.03942 0.06849 0.06733

FIGURE 2 Nadir view of model results for object at 2 m and 8 m depths with no lights (NL), side and top (S&T) lights, and bottom (B) lights. The numbers are the standard deviation-mean ratio for contrast. 8 m contrasts are twice 2 m values. Adding bottom lights and extending a transparent lighted top 0.25 m and 0.5 m reduces the contrast at 8 m, but it still exceeds that at 2 m. All images are contrast-enhanced.

routes and depths of transit expected for a variety of vehicles and environmental conditions. The power required to optimally camouflage any delivery package for each potential route can also be calculated to evaluate likelihoods for various threats. By then computer speed and memory will not be limiting for most of the problems being conceived today. Perhaps the most complex task of optical modeling in the future will be that of solving "inverse problems." What could be making that shadow? Is that a manta ray or a mine? Is that the primary signal, or a reflection, or multiple-path artifact? Are those stars being occluded randomly, or is there an intervening object? How would these problems be best addressed? Perhaps they will be addressed by decision trees designed by experts in artificial intelligence, expert systems, or machine logic. Those capabilities will, obviously, drive the need for high-speed sensors and networks as well as "reach-back" capabilities that would allow "intelligent" application of archived or historical data (to a real time problem).

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Large Scale Phase-resolved Simulations of Ocean Surface Waves

Yuming Liu and Dick K.P. Yue**

ABSTRACT

We envision a new-generation wave prediction tool based on direct large-scale nonlinear phase-resolved wavefield simulations that will augment existing phase-averaged approaches to provide heretofore unavailable modeling and prediction of realistic ocean wavefield evolutions. Upon integration with advanced *in situ* and/or remote wave sensing technology, the new tool is capable of incorporating such sensed data in phase-resolved reconstruction and forecasting of nonlinear ocean surface waves, providing information that could significantly enhance marine operations and safety. Such a capability also provides a useful framework for assisting in the optimal deployment and utilization of ocean surface sensing systems.

BACKGROUND

The accurate prediction of ocean surface wavefield evolutions is a challenging task due to nonlinearities in the wave interactions, the difficulties in modeling wave-breaking dissipation and wind forcing, and, in the context of coastal environment, effects of currents, bottom bathymetry and properties, and the presence of coastlines. Until recently, phase-averaged models such as WAve Prediction Models (WAM; for deep ocean) and Simulating WAves Nearshore models (SWAN; for nearshore

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*Massachusetts Institute of Technology

regions) are the mainstay of practical predictions. These models are developed based on the phase-averaged energy-balance equation with physical effects associated with nonlinear wave interactions, wind input, and wave-breaking/bottom dissipations modeled as “source” terms. While much progress has been made over the past decades in the basic approach and in the parameterizations of the model terms, the success has not been uniform, with predictions often falling outside the error band of the observations or in some cases outright failing. Given the basic phase-averaged assumption and the necessary simplifications in the models, further major advances could prove difficult in the present framework. Equally important, these phase-averaged models provide predictions only of the spectral characteristics of the waves, and are not as useful when detailed space-time phased-resolved information is of importance such as in the understanding of extreme wave dynamics.

NEW APPROACH

Over the past ten years, we have worked on the development of a new powerful capability, which we call SNOW (**s**imulations of nonlinear ocean **w**ave-field), for predicting the evolution of large-scale nonlinear ocean wavefields using direct physics-based phase-resolved simulations. With rapid development of computational capabilities and, more significantly, fast algorithms for nonlinear phase-resolved wave simulations, we believe that SNOW could be useful for wave predictions in spatial-temporal scales that would complement and possibly replace phase-averaged models for many practical applications.

SNOW is fundamentally different from existing phase-averaged models. It predicts the nonlinear wavefield evolution by direct simulation of the wave dynamics including nonlinear wave-wave, wave-current, and wave-bottom interactions, wind input, and wave-breaking dissipation. Where modeling is required, say in the wind forcing or capturing wave-bottom interactions, it can be directly physics-based and generally applied as boundary conditions on the field equation. Since phase information and wave profile are inherent in the simulation, this provides for opportunities for model calibrations, advances and refinements not possible in the phase-averaged context. In addition, spectral and statistical wave information from SNOW could provide valuable guidance to developing new models and parameterizations in existing approaches such as WAM and SWAN.

In terms of the intended spatial-temporal scales, the computational efficiency of an approach like SNOW is paramount. SNOW is based on a highly efficient pseudo-spectral approach, which solves the primitive Euler equations, follows the evolution of a large number (N) of wave modes, and accounts for their nonlinear interactions up to an arbitrary high order (M). Significantly, SNOW obtains exponential convergence and near linear computational effort with respected to N and M. Thus the scalar computation time is linearly proportional to the domain size and evolution time. Of equal importance, SNOW is highly parallelizable on modern high-performance computing (HPC) platforms, achieving almost linear scalability with the number of processors (utilizing $O(10^3)$ processors to date). At present, we are capable of direct simulations of an ocean wavefield of $O(10^3)$ km^2 propagating over a distance of $O(10^{1\sim2})$ km (utilizing $N\sim O(10^{3\sim4})$ per dimension, and $M = 3\sim4$). With further algorithm development and speedup, in conjunction with increases in HPC capabilities, in the foreseeable future (likely by 2025), SNOW is expected to be able to provide routine simulations of wavefields of $O(10^{4\sim5})$ km^2 propagating over distances of $O(10^{2\sim3}$ km). The main technical challenge here is computational, associated with algorithmic speedup and refinements in the context of massively parallel SNOW calculations. The real challenge however is likely not technical/computational, but scientific, in the modeling and capturing the myriad physics associated with the evolution of the wavefield, and in the availability of concurrent high-resolution measurements for calibration and validation, all in the phase-resolved context.

SAMPLE RESULTS

To date, we have used SNOW to obtain nonlinear wave-wave interactions in deep water and finite depth including current and complex bathymetry and bottom properties, with relatively simple phenomenological models for wind forcing and wave breaking dissipation. In a particular project to provide realistic/representative wavefields for ship motion analyses, we have computed an ensemble (the MITWAVE dataset) of 3D wavefields (of typical domain size of 30 km × 30 km) based on initial JONSWAP spectra. Figure 1 shows the distribution of exceeding probability of crest heights from MITWAVE wavefields with various spectrum parameters (spreading angle Θ and peak enhancement factor γ) compared with linear and second-order phase-averaged theoretical predictions. SNOW simulations have been used to identify and characterize the occurrence statistics and dynamical properties of extreme (rogue) wave events. Among other findings, we confirm that linear (Rayleigh) theory significantly under predicts the probability of large rogue wave events. Finally, we show an application where SNOW uses WAMOS II radar data to first reconstruct and then provide a forecast of the wavefield. Figure 2 shows the comparisons between SNOW and WAMOS radar data at the initial and a subsequent time corresponding to $t\sim T_p$.

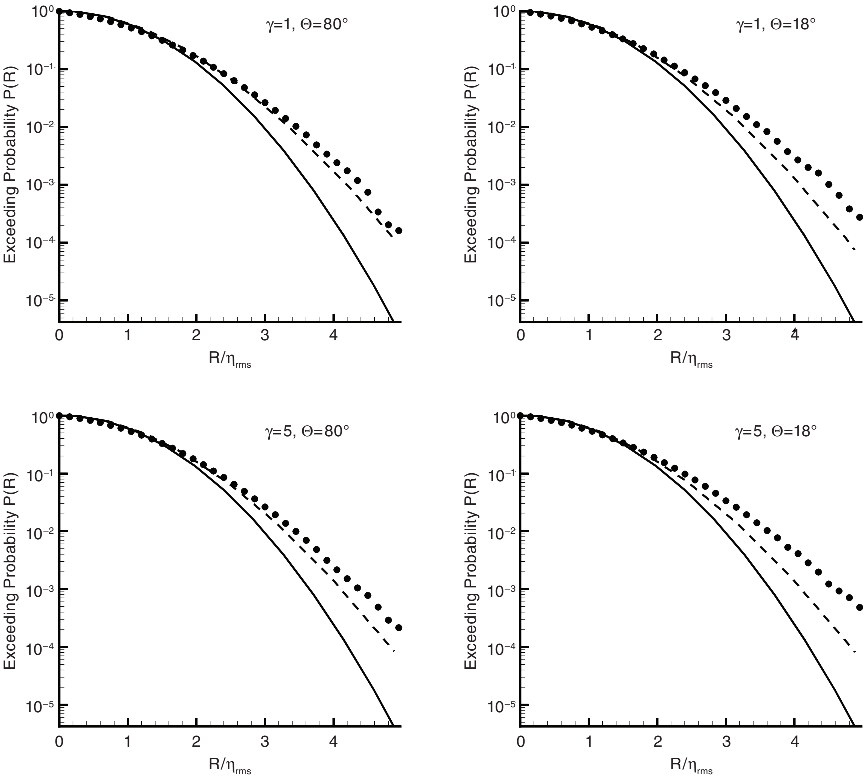

FIGURE 1 Comparison of exceeding probability of crest heights for various Jonswap wave spectrum parameters. The results are obtained from phase-resolved SNOW simulations in a domain of 30 km × 30 km after an evolution time of $t/Tp = 100$ for wavefields with significant wave height $Hs = 10$ m, peak period $Tp = 12$ s and four combinations of enhancement parameter γ and spreading angle Θ: $\gamma = 1.0$ and $\Theta = 80°$ (top left), $\gamma = 5.0$ and $\Theta = 80°$ (bottom left), $\gamma = 1.0$ and $\Theta = 18°$ (top right), and $\gamma = 5.0$ and $\Theta = 18°$ (bottom right). Plotted are the results by SNOW simulation (bullets); Rayleigh linear distribution (solid line); and Tayfun second-order distribution (dashed line).

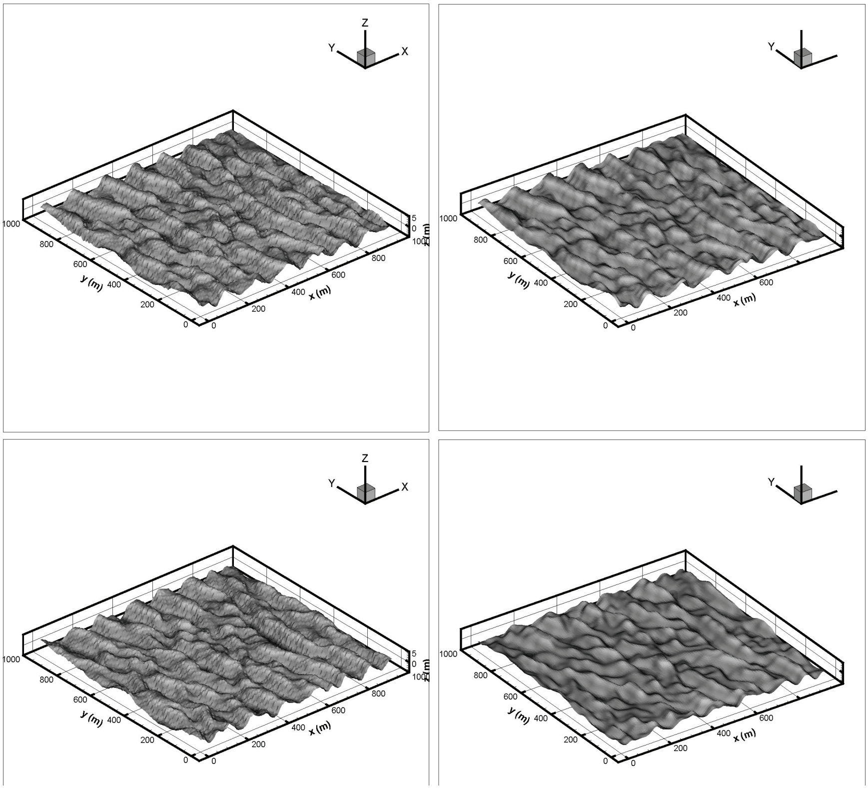

FIGURE 2 Comparisons of SNOW reconstructed ocean wavefield (top right) and WAMOS II radar sensed wavefield (top left) at time t = 0 as well as SNOW forecasted wavefield (bottom right) and radar sensed wavefield (bottom left) at t = 10 s. The domain of SNOW reconstructed and forecasted wavefield is 1 km × 1 km. The wavefield has a significant wave height Hs = 7.0 m and peak period Tp = 10 s. The radar is fixed on an offshore platform in North Sea.

Appendixes

A

Workshop Agenda

OCEAN STUDIES BOARD
NATIONAL RESEARCH COUNCIL
OCEANOGRAPHY IN 2025: A WORKSHOP

January 8-9, 2009

The Beckman Center of the National Academies
100 Academy Way
Irvine, CA 92617
(949) 721-2200
WORKSHOP BULLETIN BOARD—dels.nas.edu/osb/forum

Thursday, January 8, 2009

8:00 a.m.	Registration and Working Breakfast (Beckman Center Dining Room)

PLENARY SESSION

8:30 a.m.	Introduction and Workshop Goals Linwood Vincent, *ONR* Rear Admiral David Titley, *Navy* Dan Rudnick, *Chair*
9:10 a.m.	Participant Introductions
9:30 a.m.	3 Themes: Engineering, Simple Models, Education Chris Garrett, *Univ. of Victoria*
10:05 a.m.	Trends Shaping Oceanography in 2025 Russ Davis, *SIO*
10:40 a.m.	Break (Beckman Center Atrium)

BREAKOUT SESSION

11:00 a.m.	Breakout 1: What New Technologies Could Be Developed? How Will Research Be Conducted? (See Breakout List for Room Assignment)

12:30 p.m.	Working Lunch (Dining Room)
2:00 p.m.	Breakout 2: What Research Questions Could Be Answered? What Will Remain Unanswered?
4:00 p.m.	Break (Atrium)

PLENARY SESSION

4:30 p.m.	Plenary Session
5:30 p.m.	Discussion: Themes for Day 2 Breakouts
6:00 p.m.	Working Dinner (Dining Room)

Friday, January 9, 2009

8:00 a.m.	Working Breakfast (Dining Room)

PLENARY SESSION

8:30 a.m.	Day 2 Workshop Goals Dan Rudnick, *Chair*
8:50 a.m.	From Short Food Chains to Complex Interaction Webs: Biological Oceanography in 2025 Kelly Benoit-Bird, *OSU*
9:25 a.m.	Oceanography at the Beginning of the New Millenium Raffaele Ferrari, *MIT*
10:00 a.m.	Break (Atrium)

BREAKOUT SESSION

10:30 a.m.	Breakout 3: Topics TBD
12:00 p.m.	Working Lunch (Dining Room)
1:30 p.m.	Breakout 4: Topics TBD
3:00 p.m.	Break (Atrium)

PLENARY SESSION

3:15 p.m.	Plenary Session and Concluding Remarks
4:30 p.m.	Workshop Adjourns

B

Workshop Participants

STEERING COMMITTEE

Holman, Rob
Oregon State University
holman@coas.oregonstate.edu

Pearlman, Jay
The Boeing Company (ret.)
jay.pearlman@ieee.org

Perry, Mary Jane
University of Maine
perrymj@maine.edu

Rudnick, Dan
Scripps Institution of Oceanography
drudnick@ucsd.edu

INVITEES

Abbott, Mark
Oregon State University
mark@coas.oregonstate.edu

Alford, Matthew
University of Washington
malford@apl.washington.edu

Bellingham, James
Monterey Bay Aquarium Research Institute
jgb@mbari.org

Benoit-Bird, Kelly
Oregon State University
kbenoit@coas.oregonstate.edu

Carder, Ken
SRI, International
kcarder@monty.marine.usf.edu

Chen, Shuyi
University of Miami
schen@rsmas.miami.edu

Costa, Dan
University of California, Santa Cruz
costa@biology.ucsc.edu

Cronin, Meghan
NOAA PMEL
Meghan.F.Cronin@noaa.gov

Cullen, John
Dalhousie University
john.cullen@dal.ca

D’Asaro, Eric
University of Washington
dasaro@apl.washington.edu

Davis, Russ
Scripps Institution of Oceanography
redavis@ucsd.edu

Ferrari, Raffael
Massachusetts Institute of Technology
rferrari@MIT.EDU

Freeman, Tony
California Institute of Technology
anthony.freeman@jpl.nasa.gov

Fringer, Oliver
Stanford University
fringer@stanford.edu

Garrett, Chris
University of Victoria
cgarrett@uvic.ca

Glenn, Scott
Rutgers University
glenn@marine.rutgers.edu

Gregg, Michael
University of Washington
gregg@apl.washington.edu

Holbrook, Steve
University of Wyoming
steveh@uwyo.edu

Johnson, Ken
Monterey Bay Aquarium Research Institute
johnson@mbari.org

Legg, Sonya
Princeton University
Sonya.Legg@noaa.gov

McWilliams, Jim
UCLA
jcm@atmos.ucla.edu

Melville, Ken
Scripps Institution of Oceanography
wmelville@ucsd.edu

Moum, Jim
Oregon State University
jmoum@coas.oregonstate.edu

Munk, Walter
Scripps Institution of Oceanography
wmunk@ucsd.edu

Nash, Jonathan
Oregon State University
nash@coas.oregonstate.edu

Niiler, Peter
Scripps Institution of Oceanography
pniiler@ucsd.edu

O'Brien, James
Florida State University
jim.obrien@coaps.fsu.edu

Orcutt, John
Scripps Institution of Oceanography
jorcutt@ucsd.edu

Ozkan-Haller, Tuba
Oregon State University
ozkan@coas.oregonstate.edu

Pinkel, Robert
Scripps Institution of Oceanography
rpinkel@ucsd.edu

Plant, Nathaniel
United States Geological Survey
nplant@usgs.gov

Sanford, Thomas
University of Washington
sanford@apl.washington.edu

Sullivan, Peter
University Corporation of Atmospheric Research
pps@ucar.edu

Thomson, Jim
University of Washington
jthomson@apl.washington.edu

Timmermans, Mary-Louise
Woods Hole Oceanographic Institution
mtimmermans@whoi.edu

Ward, Bess
Princeton University
bbw@princeton.edu

Worcester, Peter
Scripps Institution of Oceanography
pworcester@ucsd.edu

Yoerger, Dana
Woods Hole Oceanographic Institution
dyoerger@whoi.edu

Yue, Dick
Massachusetts Institute of Technology
yue@mit.edu

ADDITIONAL GUESTS

Harper, Scott
Office of Naval Research
scott.l.harper@navy.mil

Kiss, Ron
Webb Institute
RKKISS@aol.com

Manley, Justin
NOAA Office of Ocean Exploration
Justin.Manley@noaa.gov

Paluszkiewicz, Theresa
Office of Naval Research
terri.paluszkiewicz@navy.mil

Pittenger, Dick
Woods Hole Oceanographic Institution
rpittenger@whoi.edu

Rhodes, Robert
Naval Research Laboratory, Stennis Space Center
Robert.Rhodes@nrlssc.navy.mil

Salazar, LT Jenn
Naval Meteorology and Oceanography Command
jennifer.salazar@navy.mil

Titley, RDML David
Commander, Naval Meteorology and Oceanography Command
david.titley@navy.mil

Vincent, Linwood
Office of Naval Research
linwood.vincent@navy.mil

Winokur, Bob
Office of the Oceanographer of the Navy
robert.s.winokur@navy.mil

OSB STAFF

Roberts, Sue
Director, Ocean Studies Board
sroberts@nas.edu

Glickson, Deb
Associate Program Officer, Workshop Study Director, Ocean Studies Board
dglickson@nas.edu

Chiarello, Heather
Senior Program Assistant, Ocean Studies Board
hchiarello@nas.edu