Chapter 12

Towards the World Ocean Circulation Experiment and a Bit of Aftermath

CARL WUNSCH

INTRODUCTION

The World Ocean Circulation Experiment (WOCE) was the largest and most ambitious oceanographic experiment ever carried out. It was nearly 15 years in the planning, 10 years in execution, and the costs (depending upon what one counts) were of order U.S. one gigadollars spread over about 30 countries. Apart from the chapter by Thompson *et al*. (2001), comparatively little has been written about the origins of this unique program.

Here I will try to provide an informal, completely personal narrative of how WOCE came to be. I have read enough of the methods and concerns of professional historians to avoid making any claim that what is written here is any more than an anecdotal account, relying mainly upon my very imperfect memory, and incomplete records dating from 1977. I am looking back through the wrong end of the telescope.

Carl Wunsch, Professor, Massachusetts Institute of Technology.

Others who were involved from the beginning almost surely have a very different point of view. If a serious history of physical oceanography in the last quarter of the twentieth century is ever written, the material here should be regarded as at best a starting point.

At the outset, I note that much oceanography was conducted outside the WOCE framework, and it would be an error to claim that all of the advances that took place during the 1990s were attributable to it. But WOCE was surely the centerpiece, many observational and theoretical programs were put in place to take advantage of its existence, and the overlap of investigators working inside and outside the program was so great that attributing to WOCE much of the progress of that time is not a wild exaggeration.

ORIGINS

Background Science

One can trace the origins of what eventually came to be called WOCE to what, for a few of us, seemed to be an intellectual crisis in physical oceanography, circa 1975. In 1973, a field and theoretical program known as the Mid-Ocean Dynamics Experiment (MODE-1) had been carried out by a consortium of physical oceanographers from the United States and United Kingdom. This program, summarized by the MODE Group (1978), had exploited the then-new technologies of current meters, temperature recorders, bottom pressure sensors, XBTs, neutrally buoyant floats, and CTDs to demonstrate unequivocally the existence in the ocean of an intense eddy field. Prior to that time, the small-scale structures visible in hydrographic sections (e.g., Fuglister, 1960) had been regarded as a kind of fuzzy "noise" of no particular interest. A bit of information was available (e.g., Crease, 1962) suggesting from the primitive neutrally buoyant floats circa 1955, of remarkably intense, presumed transient motions at depth in the western North Atlantic. Fragmentary records existed from a number of comparatively brief measurements (see, for example, Monin *et al.*, 1977, Chapter 5). Physical oceanographers knew about internal waves, and were aware of the importance in the atmosphere of eddies [going back at least to Jeffreys (1933), and Victor Starr's work in the 1940s; see Lorenz (1967)]. But until about 1971, the technology simply did not exist to do more than speculate about the hypothetical importance in the ocean of time-dependent motions.[1]

---

[1] Soviet scientists had published a number of papers in their own literature that were interpreted by them as showing eddylike motions. Western scientists did not, however, take their results as seriously as they might have because of the language barrier; the primitive nature of the equipment (e.g., numbers were recorded by printing them onto a paper tape); and the locations such as the Black Sea. Monin *et al.* (1977, p. 133) list both Soviet and Western observations that, with hindsight, show eddies everywhere. (Note that in the Soviet literature, the term "synoptic" is used for the less proper Western adjective "mesoscale.")

Numerical modelling of the ocean had advanced greatly since the pioneering efforts of K. Bryan, G. Veronis, and a few others. But the slow, small computers of that era, combined with the very small deformation radius in the ocean conspired to prevent ocean models from being run in a high enough Reynolds number regime so as to become unsteady.

Between the limited observations and the sticky ocean models, the conventional picture of the ocean circulation was that of a laminar steady state. To this day, oceanographic textbooks still render the ocean circulation through pictures of large-scale scalar properties (e.g., temperature, salinity, oxygen) contoured and discussed as though the system is essentially steady and flowing only on the largest spatial scales—a geologist's view of the ocean. An analogy would be an atmospheric physics textbook that recognized only the mean, climate, state and failed to notice the presence of weather.

The results of MODE-1 and its troubled successor POLYMODE (see Collins and Heinmiller, 1989, for an account) showed that the ocean was likely an essentially turbulent fluid. Whether the turbulence had important dynamical and kinematic roles was unclear, but theory, and analogies with the atmosphere, suggested strongly than one could not simply assume it to be an annoying source of observational noise.

A parallel development, independent of oceanography, was the growing interest and concern about rising CO_2 levels. Several people, but notably Roger Revelle, were calling attention to the possibility of major climate change and insisting that the scientific community had to learn more about the implications. An indicator of the growing concern was the appointment of the so-called Charney Committee of the National Research Council in the summer of 1979 to examine the question. Three oceanographers (H. Stommel, D. J. Baker, and myself) were on the Committee, whose report (National Research Council, 1979) made a best-guess at the range into which global mean temperatures would be expected to rise. But a general theme of the brief report was the inability to be very definite about anything, particularly about inferences concerning the oceanic response, its uptake of carbon, and its thermal memory.[2]

Yet another relevant circumstance was the end of the so-called First GARP Global Experiment (FGGE), renamed, for the public, as the Global Weather Experiment. This program had been put together by the international meteorological community [Global Atmospheric Research Program (GARP)] to address the first of two overall goals—to improve weather forecasts. The organization of FGGE had left some

[2] Remarkably, the Charney Committee's estimate of the probable range for the expected increase in global mean temperature has hardly changed in the intervening decades. Much more is now known about the climate system than was true in 1979, and the continued agreement is largely fortuitous. Unhappily, some critics have interpreted this coincidence as implying that the ongoing scientific efforts to better understand climate change have been a waste of government money. This criticism is addressed by the Committee on Metrics for Global Change Research (2005).

of the oceanographic community feeling bruised, as the meteorological community wanted oceanographic ships as meteorological observing platforms, but cared nothing for the possible oceanography that might be done. With their much greater numbers, and national and international organizations, the weather forecasters essentially commandeered significant seagoing resources, leaving the oceanographers primarily as onlookers and passengers. But with the end of FGGE, GARP turned to their second goal—which was the understanding of climate change. When it came to climate, it was much harder to make a convincing argument that the ocean was largely irrelevant (although some meteorologists very seriously tried to do so, both then and today) and, internationally, efforts were begun to open a dialogue with the oceanographic community.

Thus, the situation in 1979 was that some oceanographers had a sense that the ocean was a far more dynamic place than historically believed; that it probably varied on all time scales—not just those of the newly discovered eddies; that we were being confronted with important societal questions about the ocean that were far beyond our ability to address, either theoretically or observationally. The question was what, if anything, could be done? If nothing could be done, it was clear that physical oceanography would become a marginal science of interest only to a few fluid-dynamics-oriented academics with the much larger meteorological community simply assuming that the ocean was basically passive ("swamp models" of the ocean are only now beginning to disappear). That NSF and ONR budgets for oceanography were shrinking was interpreted by some as demonstrating a field in decline, with no new ideas.

In 1979, I was invited to attend a meeting in Miami of a group called the Committee for Climate Change and the Ocean (CCCO) that had been formed by the IOC (Intergovernmental Oceanographic Commission) and SCOR (Scientific Committee for Ocean Research) and GARP to study the question of how one might address the problem of better understanding how the ocean influenced climate change. Thompson *et al.* (2001) describe the discussions that led to the calling of this meeting. I went, torn between the sense that we, as an oceanographic community, had to do something and that we probably could, and the realization that I was taking a tiger by the tail. If I was to be successful, I was condemning myself and others to years of organization and meetings.

To the extent that I can recall the thinking of the time, it was that our problem was primarily an observational one, and that sufficiently promising new technologies were being developed that, with some collective effort, might go a long way toward solving the fundamental problem. The observational problem was to (1) observe the ocean globally; (2) observe it spatially and temporally at sufficiently short intervals that one could define the dominant modes of variability everywhere. At a time when the main observational tool was still the ship, floats with tracking ranges of hundreds of kilometers, and expensive current meter moorings capable of operating for about a year, the question would immediately arise as to why anyone would think the global ocean could be adequately observed?

The technologies that I was aware of were several. CTDs were gradually becoming easier to use and more widespread. Autoanalyzers were available for nutrient measurements. Titration salinities had been replaced by conductivity methods. Transient tracers, tritium, helium-3, and chlorofluorocarbons were measurable. Bottom pressure gauges had become stable enough to yield months-long records. The neutrally buoyant float methods were rapidly advancing beyond the SOFAR method used in MODE-1 to RAFOS (Rossby *et al.*, 1986) and what eventually became the ALACE floats (Davis *et al.*, 1992). In the summer of 1977, Walter Munk and I (Munk and Wunsch, 1979) had stumbled on the idea of ocean acoustic tomography, which promised to provide large area integrals over the ocean. Perhaps most important, however, was the prospect of certain satellite measurements of the ocean, in particular scatterometry for winds, altimetry for circulation, and gravity for determining the absolute circulation.

Altimetry and tomography were my own particular foci, and as W. Munk describes the evolution of the acoustical capability elsewhere in this volume, perhaps I can be permitted some words about altimetry.[3]

I cannot do justice here even to the history of altimetry, much less all of the other technologies that were emerging at that time. I would argue, however, that altimetry has played a unique role as, to this day, it remains the only true global ocean measuring system (scatterometers and other devices measure parts of the forcing, not the ocean itself).

Altimetric Measurements

Like most physical oceanographers, I had no experience with remote sensing from space, when in 1974 I had a telephone call from Dr. Peter Bender, a space geodesist working for NOAA in Boulder. Peter explained that he was chairman of the Committee on Earth Sciences of the Space Science Board of the National Research Council, and that they were trying to write a report discussing, in part, what NASA should be doing to better understand the ocean. My response, which was a flat refusal, clearly startled Bender. I told him that NASA's contribution to oceanography seemed all hype—based upon a few not-very-accurate infrared measurements of sea surface temperature from space. Sea surface temperature was of much more interest to meteorologists than to oceanographers in any case, and I thought that NASA's public relations machinery was far outstripping the importance of its contribution. After a stunned silence on the other end of the telephone line, Bender said that if things were really so bad it was even more important that I should serve on the Committee, so that the Report would reflect the reality. In a weak moment, I then agreed.

At that time, NASA's oceanographic interests were focussed on the so-called SEASAT-A spacecraft which was to fly circa 1977. It is hard now to credit an era in

[3] In the end, tomography played only a small role in WOCE as the acoustic technology did not develop as rapidly as hoped. It may now be on the verge of large-scale use.

which NASA was looking for things to do. A committee of enthusiasts had been put together by NASA that proposed an ocean satellite to measure virtually everything from space that seemed technically possible, in some cases without much justification for what the measurement would say about the ocean. As part of the Bender Committee, I undertook to read the documentation justifying the decision that had already been made to fly SEASAT-A. (One question we were faced with was how a successor satellite—SEASAT-B—should be configured; it was taken for granted that there would be a follow-on of some sort.) The level of technical detail and justification for SEASAT-A in the reports would be regarded as extremely thin, bordering on the laughable, by today's standards. As I read through the documents, however, I finally came to the discussion of the altimeter that would be on the satellite. Although the report said little about how the measurements would be used, it became clear to me that if the instrument system could live up to the engineering specifications, it represented a very exciting possibility—the measurement of surface dynamic height from space at a useful level of accuracy. From the earliest days of the so-called dynamic method, about 1900, the direct determination of sea surface slopes relative to a reference surface (called the geoid) had been recognized as an important concept, but whose measurement was regarded as essentially impossible. Here was NASA explaining, in primarily engineering terminology, that perhaps it could be done. I got interested.

SEASAT (the "A" was dropped on launch) finally flew in 1978, but instead of running for several years, it failed after three months. (Rumors immediately circulated that it had been deliberately killed by the U.S. Air Force, who were supposed to have aimed a laser at it. In the aftermath of the Vietnam War, many scientists were deeply suspicious of the military, and there indeed had been great tension over whether the SEASAT measurements would be classified. The SEASAT saga remains to be written.) As it turned out, the failure after so short a time was something of a blessing. Cost overruns on the hardware and launch had eaten up the science analysis budget. With the failure, some money from the operations budget was made available to the science community to analyze what data there were. These proved adequate to show that the altimeter actually worked at the levels of accuracy and precision predicted by the engineers. For example, one could clearly see the Gulf Stream and associated rings (Wunsch and Gaposchkin, 1980; Cheney, 1982). The concept had been proven (see Figure 12.1).

A separate (long) paper would be required to describe the events that ultimately led to the launch of what is now known as TOPEX/POSEIDON, a U.S.–French mission that became the centerpiece of WOCE. Anyone who becomes involved with the formulation of a new mission will have their own stories of near-failure, bureaucratic and political craziness, heroic and not-so-heroic individuals, and plain luck. That TOPEX/POSEIDON was actually launched, and has performed far beyond its specifications for, as I write, almost 13 years (the agreed lifetime was 3–5 years) is in the nature of an engineering/scientific/political miracle that deserves its own history.

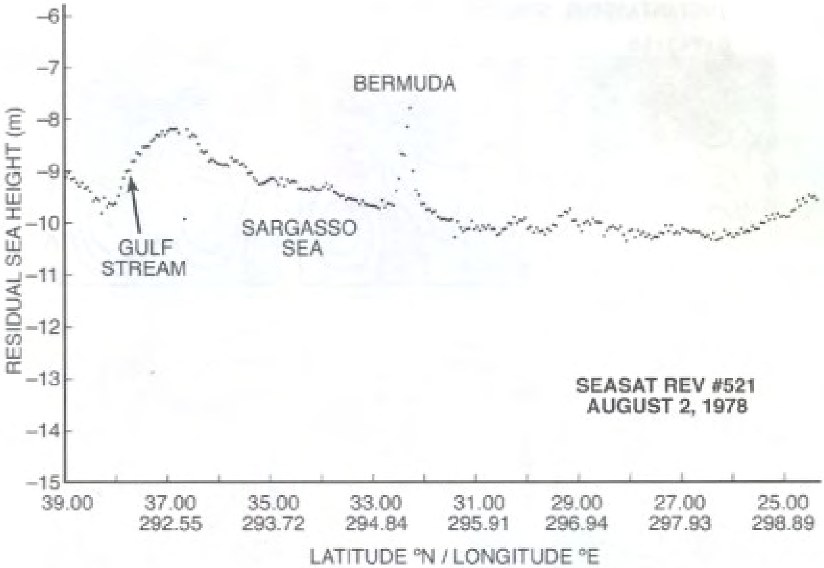


Figure 12.1. An early measurement (Cheney, 1982) from SEASAT showing the presence of the Gulf Stream in altimetric data. The presence of a Bermuda signal is evidence of the large geoid (gravity field) errors present in the data.

Modeling and Theory

By 1979, there were global coarse resolution numerical models, and small-scale, idealized geometry, eddy-resolving models. (See Figure 12.2, from Holland and Lin, 1975.) Moore's Law (Moore, 1965) was already widely known, and extrapolation of work already underway suggested that by about 1990 one would have the beginnings of global-scale eddy-resolving models.[4]

Anyone who understood models realized that the more sophisticated the model, the more demanding the requirements on the observations. It was obvious that numerical models of the ocean were about to outstrip any observational capability for testing them. There was a grave danger that the field would produce sophisticated, interesting models, without any ability to calibrate them. (This situation now exists in paleoclimate studies, where seemingly sophisticated models are compared to sparse, poorly understood observations.)

With a few rare exceptions, the coast-to-coast hydrographic surveys, epitomized by the Meteor surveys of the 1920s and the International Geophysical Year (IGY) surveys of the 1950s, had fallen from favor. They appeared to be of mainly qualitative

[4] The computer story involves much more than the number of circuits on a chip. Moore's Law is a metaphor for cheap storage, parallelization, input–output devices, and new software, that were required for the construction and use of models of a size and complexity far beyond what was possible in 1980.

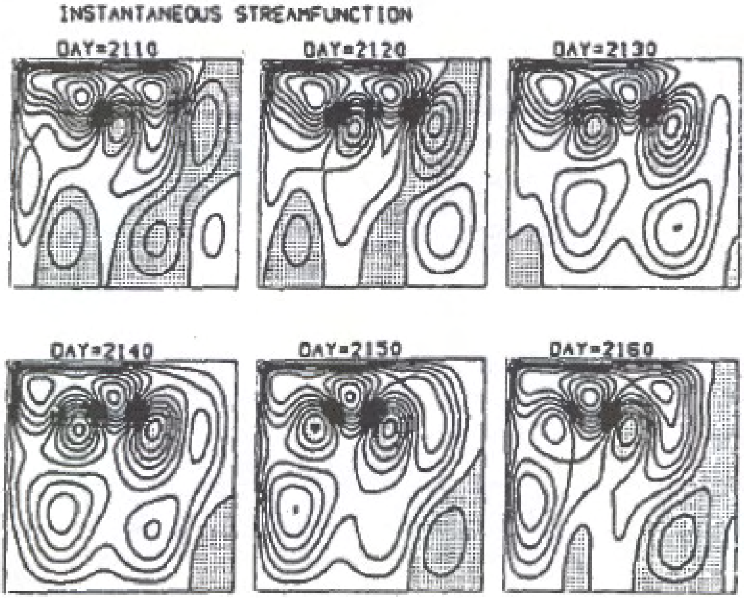


Figure 12.2. From Holland and Lin (1975) showing an early ocean model producing eddylike features. The model had one layer and was nominally 1000 km on a side.

use—and many, perhaps most, physical oceanographers had turned instead to the more scientific-seeming process studies of the era of the International Decade of Ocean Exploration (IDOE). These included MODE, focussing on the mesoscale variability, but also upwelling studies in various places, internal wave studies; the monsoon regime of the Indian Ocean; and so on. In contrast, observations of long hydrographic sections resulted primarily in atlas plates, quite beautiful, but more art than science, with the accompanying scientific papers being primarily descriptions of water masses, or unconvincing attempts to guess the absolute flow directions. By the middle 1970s, the notorious so-called level-of-no-motion problem, which had plagued oceanography from the earliest days of hydrographic surveys, was finally understood, and solved by inverse methods—in several guises (Wunsch, 1996). The advent of these methods meant that coast-to-coast hydrographic lines could be used quantitatively; it was also recognized that altimetry combined with an adequate gravity mission was an alternative method for determining the absolute flow field (Wunsch and Gaposchkin, 1980). With the new ability to calculate flow fields and transports without arbitrarily chosen levels-of-no-motion, it made sense to contemplate a proper "long-line" survey of the ocean.[5]

[5] Dean Roemmich and I (Roemmich and Wunsch, 1985) made the first trans-Atlantic hydrographic sections since the IGY (1958–59) during the summer of 1981. We had the use of inverse calculations specifically in mind, as well as the opportunity to see if the North Atlantic Ocean had changed measurably in the intervening years (it had, in a number of ways).

PROPOSING IT AND SELLING IT

In any event, with the sense that we could develop adequate technologies in a reasonably brief time period, that models would probably improve independent of any field program, and that we knew generally what needed to be done, I proposed at the Miami meeting that there should be an attempt to measure the ocean circulation and its variability, globally, as the oceanographic contribution to understanding the climate state. R. Stewart (Canada) made another specific suggestion: that it would be useful to attempt to formulate a complete, closed heat budget of the North Atlantic Ocean sector, including both atmosphere and ocean as a trial experiment for a possible later global one. Some combination of *in situ* observations of ocean and atmosphere, along with coupled models would be used to understand how heat was transported by both fluids, and how it was transferred between them. At some point, Stewart's proposal was labelled the "CAGE" experiment, as it would basically involve building a cage around the North Atlantic basin in both atmosphere and ocean. In response to the two proposals made at the Miami meeting, the CCCO appointed two Committees: one was chaired by Fred Dobson (Bedford Institute) to examine the prospects for CAGE; the other was chaired by Francis Bretherton (then Director of NCAR) to examine the prospects of a global experiment. The report of the CAGE committee (Dobson *et al.*, 1982) was very impressive and came to a startling conclusion—that CAGE was impractical, not because of the problems of observing the ocean, but because atmospheric measurements were inadequate to close the atmospheric side of the heat budget![6] This wholly unexpected conclusion effectively left the global ocean experiment alone as a serious proposal ("... the concept of a North Atlantic CAGE experiment lies battered and torn, ..." from a letter of F. Dobson to the Committee on Climate Change and the Ocean, January 10, 1983).

Another, completely separate, program ultimately called TOGA (Tropical Ocean, Global Atmosphere) was being formulated and organized. TOGA has been described at length elsewhere (see Halpern, 1996, for a discussion of its origins). Suffice it to say that its flavor was very different, involving as it did a very large meteorological component, a goal of forecasting, and a hard insistence that only the upper few hundred meters of the near-equatorial ocean had to be understood in order to achieve its goals. The latter point of view, in particular, ultimately caused difficulties for what became WOCE.

The Bretherton committee, studying the option of a global ocean circulation program, eventually concluded that it might be feasible, and recommended that serious planning and study should begin.

That, of course, was when our real troubles started. The job was to organize something both nationally (the U.S. contribution was clearly going to be the dominant one) and internationally, on a scale never before tried by oceanographers,

[6] Much of the difficulty lay with the problem of calibrating radiosondes, whose offsets prevented the possibility of closing the atmospheric budget.

and without the managerial infrastructure available to the meteorologists who had organized FGGE—with their national meteorological agencies as a base. Oceanographers had nothing remotely resembling such governmental organizations.

PLANNING IT

Shortly after the CCCO discussion, and the appointment of the Bretherton Committee, I spent a year in Cambridge, England, with the help of a Guggenheim Fellowship. In addition, Walter Munk came for six months, and we split a Fulbright Award (inevitably then known to our wives as a half-bright award). During this period, when we were focussed on trying to turn ocean acoustic tomography into a practical observational method, I attended a Royal Society discussion meeting on oceanography in the 1990s for which Munk and I wrote a speculative paper (Munk and Wunsch, 1982) that laid out a rough vision of how the emerging technologies might be deployed to give a much more realistic understanding of the time-dependent ocean. [A less formal account appears in the Munk Festschrift (Garrett and Wunsch, 1984).]

How does one obtain legitimacy for a proposed national and international program? In the United States, recognition appeared to come through the National Research Council (National Academy of Sciences), through what is now called the Ocean Studies Board (OSB; the name has changed several times over the years. It was then called the Board on Ocean Science and Policy). A small self-appointed steering group (including Baker, Nowlin, Broecker, Wunsch) agreed to try to put together a U.S. national program. I went with some of the steering committee to a meeting of the Board in Washington where I presented the idea of a global ocean circulation program. That both Baker and I were members of the Board appeared to make the request particularly simple. To my very great surprise, the request was flatly refused. The Chairman of the Board (J. Steele, then Director of WHOI) announced that there would indeed be a national oceanographic program, but that it was to include biology, and he would be the chairman.

I returned from the OSB meeting convinced we had failed to even get out of the starting gate. About a week later, however, Steele telephoned me to say that, of course, we could have a workshop, and that the Board would endorse and help organize it. Someone had gotten to him in the interim. Steele was evidently fearful that the physical and chemical oceanographers would have a major program and that the biologists would be left out. Steele's efforts to construct a parallel biologically oriented program eventually became GLOBEC, but that is someone else's story.

A small steering committee (D. J. Baker, F. Bretherton, W. Broecker, J. McWilliams, W. Nowlin, F. Webster, and C. Wunsch) was appointed through the Ocean Climate Research Committee of the Board to organize a Workshop, which took place in August 1983 at the National Academy of Sciences building in Woods Hole, Massachusetts. About 70 people were officially present, including agency representatives and many from abroad. The resulting report (Ocean Climate Research Committee, 1984) was based upon various white papers plus discussion. Its publication was interpreted as endorsement of a U.S. program by the Academy, and by the U.S. government agencies which would have to fund it.

Internationally, the World Climate Research Program (WCRP, with headquarters in Geneva) through its own steering committee, was induced to appoint an international planning committee. The original committee membership was F. Bretherton, W. S. Broecker, J. Crease, K. F. Hasselmann, M. P. Lefebvre, A. Sarkysian, J. Woods, R. Kimura, and myself, as chairman. Because many of the results of WOCE bear directly on physical oceanographic problems, it is not widely recalled that WOCE was a climate experiment—and was accepted as such by the WCRP. Many oceanographic issues had to be resolved, but the goal was, and remained, to quantify the contribution of the ocean to control of the climate system, to provide a baseline against which future climate change could be measured, to understand the extent to which its variability existed, and what its consequences were.

There then proceeded to be several years of seemingly endless numbers of meetings (well over a hundred) devoted to determining (1) what we were trying to do and (2) how we would do it. Discussion meetings were focussed, variously, by technology, by ocean basin, and by scientific goal. A framework with two overall goals was produced (directed at producing data sets adequate to test the models expected circa 1990, and determining what kind of observation program would be adequate for indefinite monitoring of oceanic climate states, respectively).

A few events stand out. The initial WOCE planning envisaged including measurements and understanding of the ocean carbon uptake and redistribution problem, as the fate of fossil-fuel CO_2 was one of the driving uncertainties. It quickly became clear, both in U.S. national and international meetings, that the CO_2 problem could not be dealt with as an appendage to a program primarily in the hands of physical oceanographers. A major problem was that serious technical disagreements existed among the small community of people who measured oceanic CO_2 (e.g., C. D. Keeling, P. M. Brewer, and others) as to how it could be, or should be, done. Expertise necessary to distinguish between the competing arguments was not adequately represented on the steering committees. Furthermore, at least one member of the international committee (WMB) repeatedly insisted that WOCE should be a tracer measuring program alone, with discussion of altimetric satellites, conventional hydrography, and the like being a "dead end."[7]

[7] Letter from Broecker, 30 August 1988 to C. Wunsch. It is perhaps worth quoting from this letter as it demonstrates the divisions in the community over what needed to be done:

> "…the program is too much driven by satellite topography, rapid hydrographic sections and inverse modeling. In my view the approach is basically a dead end. The great hope of the future is atmospheric driven models.
>
> I agree that atmospheric driven ocean models must fit the temperature and salinity field (and that to some extent they currently fail this test.) However, one does not need a WOCE program to generate an observed temperature and salinity distribution. We have a perfectly adequate one for this purpose."

The letter was copied to 31 colleagues around the country, and was representative of several others in this vein, although more restrained than some.

It was finally concluded that a separate program, which became JGOFS, should be spun-off into the hands of the requisite experts, with a commitment (which was honored) for WOCE to provide shiptime and to generally collaborate. With hindsight, this decision was the right one, with WMB focussing his unhappiness primarily on the JGOFS organizers, not WOCE (but see Kerr, 1991).

Organizing national and international programs is a huge time sink. We took as the principle that coordination would be attempted only if was really required—because temporal simultaneity was essential. For example, important as modelling would be to WOCE, it did not require the same degree of international organization that the observational programs did. To a large degree, the modelling community was advancing with the growing computer power—a development that was out of the hands of oceanographers. They were already reasonably well-organized internationally, having periodic meetings that brought the main players together. A policy of "benign neglect" seemed to be appropriate, and seems to have worked reasonably well, although inevitably, some of that community chose to infer that WOCE was antimodelling. The most conspicuous WOCE modelling program was the community effort led by C. Böning, W. Holland, and others (the WOCE modeling effort was reviewed by Böning and Semtner, 2001).

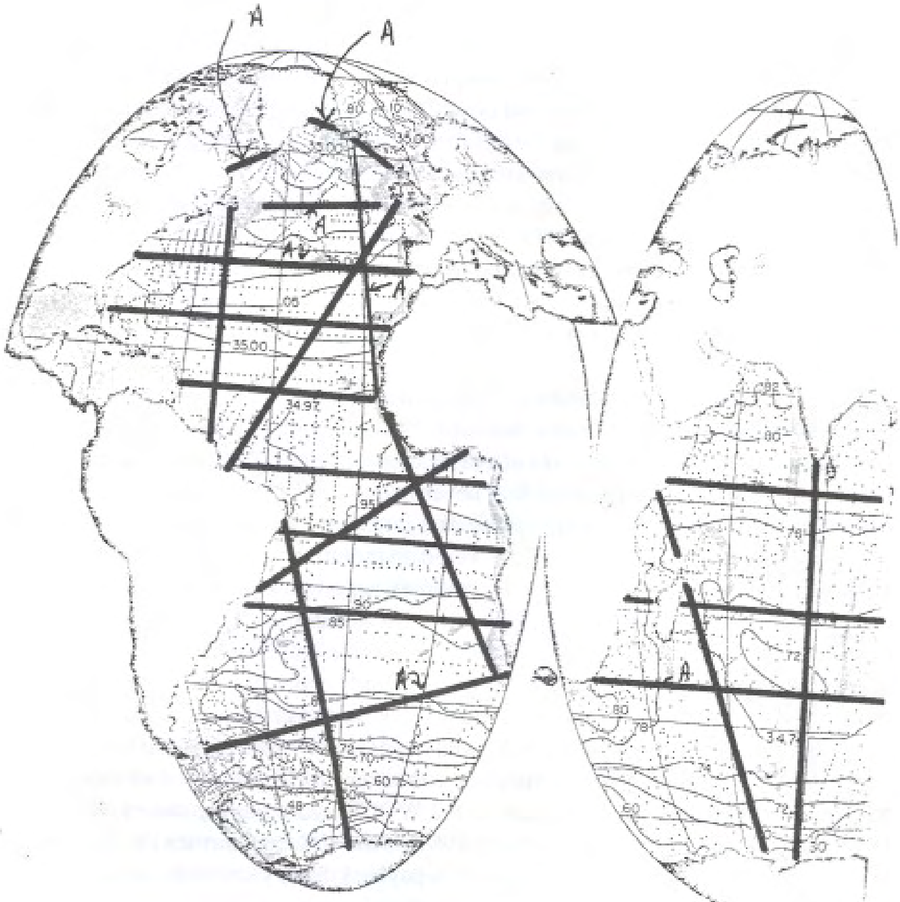
A few of the major strategic debates stand out. One was the conflict between those who believed that the major issues of physical oceanography and climate lay with the inability to parametrize processes in the models, and those advocating a global quantitative description of the circulation. Thus, a strong community wished to deploy the majority of WOCE observational resources into a single ocean basin (there were advocates for the North Atlantic and the North Pacific). WOCE did endorse and carry out a number of regional process-oriented experiments, most notably the so-called Subduction Experiment in the eastern North Atlantic, and the Brazil Basin Experiment in the South Atlantic, but some of the fiercer advocates of what was sometimes called "model testing" declined further participation in the program. Another complication was the organizational separation of the Subduction Experiment from WOCE because the U.S. Office of Naval Research was interested in funding it, but did not want to be attached in any way to a program that was publicly directed at understanding climate.

Overall, the WOCE organizers generally succeeded in maintaining the global-scale, deep-water, measurement focus which had underlain the initial proposals for the program. Other specific regions had powerful proponents (high-latitude marginal seas, the Mediterranean, and so on) who simply could not be accommodated with the resources (human and observational) that were likely to be available. With hindsight, it is clear that the global ocean is so complex, with so many different dynamical regimes, and time and space scales, that few individuals are comfortable with discussions of the system as a whole. Most scientists focus their attention on particular processes, or ocean basins, and the global scale tends to be an orphan. That WOCE did not break up into a series of regional programs was one of the great accomplishments of the various steering committees. (Some of the ongoing travails of the successor CLIVAR can be understood in this context.)

Getting people to think about the global problem was not so easy, if only because the costs seemed prohibitive. Figures 12.3 and 12.4 were drawn by me in early 1982 with a ruler and marking pen, simply to permit a rough calculation of what a global hydrographic program would cost. The reaction that "we could never afford that" was addressed by dividing the number of sections by about 5 years, and by the number of institutions around the world capable of doing high-quality hydrographic work. Although not cheap or easy, it was eventually agreed that such a program was indeed manageable. The final WOCE hydrographic coverage is qualitatively somewhat like what was sketched. (At least one hydrographer had difficulty distinguishing a scale analysis for cost purposes from a detailed plan and was so affronted by it, he assured me that he was going to make certain that *none* of these lines would be measured!)

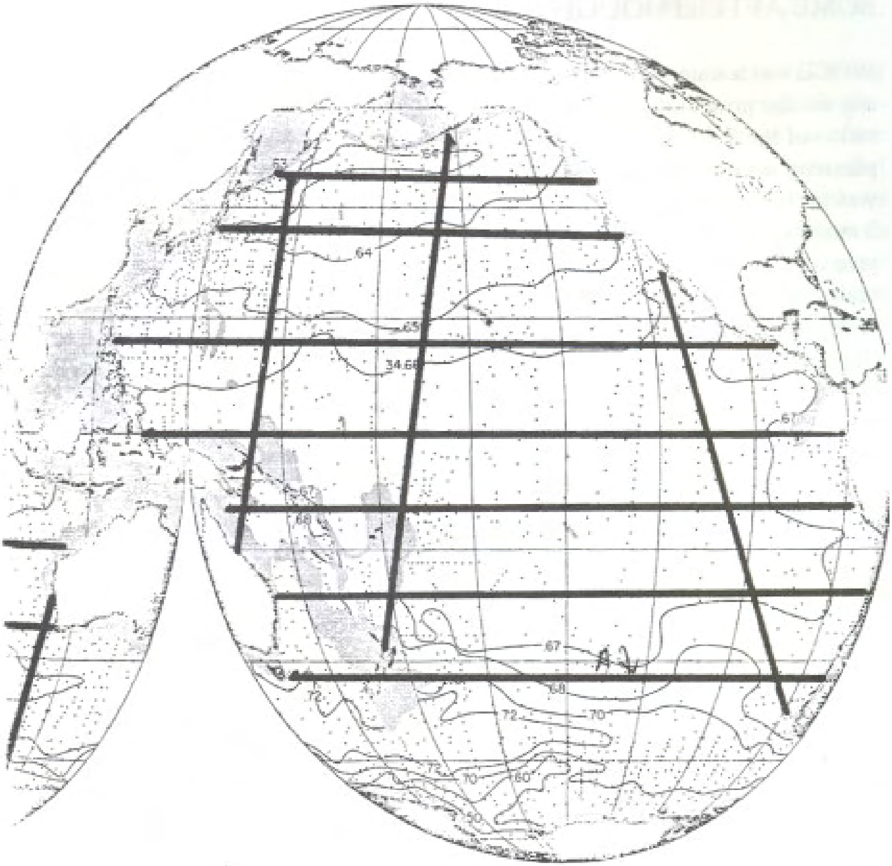
The balancing of costs against scientific benefit, absent any quantitative tools for determining the latter, was a major difficulty. Was it important, and worth the financial costs and human effort, to deploy current meter moorings in the central South Pacific Ocean where such measurements had never been made? Even today, with far more capable models and ability to determine data impact on various estimated quantities, such questions are rarely posed and answered quantitatively. Inevitably, WOCE *in situ* observations were determined through complex negotiations in national and international meetings that gave great weight to the presence of people who had particular observational capabilities, who wished to participate, and were capable of bringing national resources with them to the program. (Funds under the control of the international WOCE steering groups were limited to less than what was necessary to maintain a coordinating office in Wormley, U.K., and travel for the steering group members.) A prime example of the debates taking place concerned the high costs of adding a major transient and "exotic" tracer program to the WOCE hydrographic survey. B. Warren (WHOI) had written a letter, 9 February 1987, to the U.S. cochairmen (W. Nowlin, C. Wunsch) questioning whether the scientific payback from such measurements could justify the very considerable expense, and whose most immediate impact would be to reduce the spatial coverage of the program. Fierce debate ensued between proponents and skeptics of such measurements. Although some of the more burdensome of the proposed measurements were dropped (argon-39 measurements, notably, would have required huge sample volumes—several tons each—and the water could only be analyzed in Bern, Switzerland), a largely political decision was made that without tracer community participation and enthusiasm, the hydrographic program was unlikely to be fundable. A major tracer program thus was carried out. (It would now be possible to answer the question of whether the scientific return from the tracer measurements was worth the cost and overall spatial and temporal coverage reduction, but to my knowledge, no such study has been done. Sleeping dogs are probably best left alone.)

Getting satellites flown (the WOCE planners sought not only what became TOPEX/POSEIDON; but also the ERS-1 satellite; a scatterometer to measure the windfield; as well as a gravity mission to provide for absolute altimetry) proved



Figures 12.3 and 12.4. Sketches (Wunsch, 1984) of a global hydrographic program. Lines drawn with a straight edge on top of Reid's (1981) salinity field. The figure was intended only for estimation of costs, but was taken by some to represent the actual plan—which took many years to define in detail.

to be a very complicated story in its own right. National space agencies, such as the U.S. NASA and the French CNES, have their own politics, dynamics, and a multiplicity of constituencies. International space agencies (ESA) are immensely complicated organizations attempting to respond to diverse national pressures and priorities. Advocates of WOCE and the various satellite missions undertook a long negotiation process to attempt to provide a simultaneous global *in situ* field program along with concurrent flight of the requisite spacecraft. Although I will not attempt to describe the details of this process here, much of the strategy consisted of telling oceanographic funding agencies that WOCE had to be done within a finite time



interval so as to take advantage of the independently funded satellite missions, and simultaneously telling the space agencies that the satellites had to be flown in a finite time window to take advantage of the independently funded *in situ*, WOCE program. The strategy worked for altimetry; only marginally for scatterometery; and failed for gravity missions which are only now becoming reality. (Cost estimates for WOCE vary greatly depending upon whether one includes the satellite expenditures. During the planning process, some oceanographers never did seem to understand that if an oceanographic satellite such as TOPEX/POSEIDON were cancelled, the resulting funds would *not* be available for *in situ* observations. Considerable acrimony existed over this point. The effort to fly a high-precision altimetric satellite was extremely unpopular with much of the physical oceanographic community, many of whom regarded it as a colossal waste of money. This widespread skepticism was artfully concealed, in particular, from NASA management.)

SOME AFTERTHOUGHTS

WOCE was a watershed in the history of oceanography, and it is difficult to envision any similar program being carried out ever again: with WOCE, the era of pure exploration of the fluid ocean largely ended. One could no longer point (as we did in our planning documents) to large regions of frequency/wave number space where there was no information at all (e.g., "how much does the ocean vary on time scales of 3 months on spatial scales of 2000 km?" was an unaddressable question. Now we can give very precise answers for much of the system.). We are now in an era where spatial scales ranging from millimeters to 10,000 km, and global-scale temporal variations of days to decades, have been measured. Not all such scales have been measured in all geographical regions, but there is no longer a "mare incognita" of the same extent. Figure 12.5 shows the completely schematic frequency wave number diagram, used by the TOPEX Science Working Group (1981) to discuss the problems of sampling the ocean. Units were carefully omitted from the contours because it was not possible to make a quantitative estimate of the spectrum at that time. The report argued that apart from limited knowledge of the mesoscale in the North Atlantic, and some

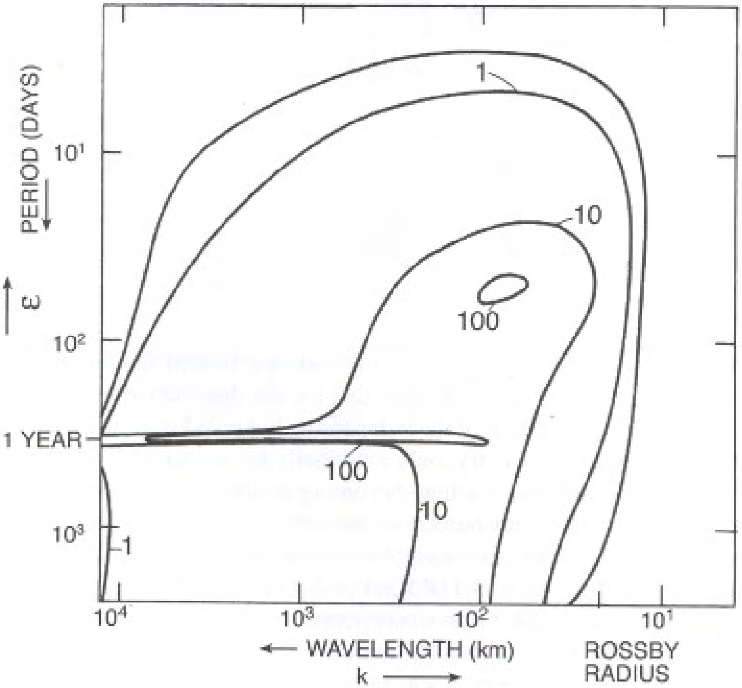


Figure 12.5. Schematic frequency wave number diagram, without units, constructed by the TOPEX Science Working Group (1981).

knowledge of the annual cycle of sea level from tide gauges (Patullo *et al.*, 1955), almost none of the spectrum had ever been measured.

The very success of WOCE has led to present difficulties in further pursuing classical physical oceanography. Major issues now lie with determining how to maintain global-scale measurements for indefinite periods—largely taking them out of the realm of possibility for academic oceanographers working on three- to-five-year grant and six-year tenure cycles. Although many processes are still poorly understood, we now have models on both regional and global scales that when constrained to our WOCE-generated data sets clearly have skill, and are useful in a way that was not true 20 years ago (e.g., Stammer *et al.*, 2002). The increasing regional focus of much of the literature is a paradoxical outcome of the success of the global experiment—much interest now lies with specific regional variations in physical processes (e.g., tidal mixing variations) relative to the presumptive global averages.[8]

Before WOCE, one could, for example, obtain funding to study the monsoon regime of the western Indian Ocean for a year or two. What is now known of that region, from WOCE and parallel efforts, leads to the conclusions that many years, and probably many decades, of observation will be required to make a qualitative improvement in existing understanding—because of the very strong interannual variability that must be accounted for.

Much of what we now take for granted (e.g., global altimetric maps of variability every few days) was science fiction 20 years ago. Students entering the field since about 1995 can, and should, take for granted the existence of a global data base, ongoing efforts to estimate the time-evolving ocean with realistic-seeming models, and a wide variety of remarkable instruments that emerged from WOCE (or during the period in which WOCE evolved). But thousands of people from dozens of countries made it all possible, and sometimes it is worth looking back to appreciate that we do make some forward progress.

What of CAGE? It was a good idea, and to a great extent, WOCE subsumed it.[9] As a token of how far we have progressed, Figure 12.6 shows the global transport

---

[8] I am aware that these are sweeping generalizations to which there are many caveats and exceptions, but it is also true that there has been a qualitative change in the way we do large-scale physical oceanography.

[9] Bob Stewart was not particularly unhappy that his CAGE proposal was not *per se*, carried out. He was a powerful supporter of WOCE, and efforts such as his were extremely important in gaining acceptance for the program. What I did not realize at the time was that Stewart and other prominent physical oceanographers were pleased with the WOCE proposal because it allowed them to shove aside a persistent Soviet Union "Sections" proposal. A very senior Russian meteorologist, G. Marchuk, had for years been advocating at international meetings a program for committing all oceanographic ships to repeated hydrographic sections in regions that Marchuk claimed to have identified as controlling weather. Stewart, H. Stommel, and others were fearful that the plan was going to gain acceptance and absorb much of the world's oceanographic efforts—all based upon one powerful man's insistence. "Sections" was again proposed at the same meeting where CAGE and WOCE were originally discussed, but was brushed aside. Whatever misgivings Stewart *et al.* may have had about WOCE (and Stommel surely did), there was some hope that something useful would come of it. Henry Stommel, who had been my thesis adviser and remained a good friend, privately strongly deprecated the idea of WOCE, resorting on more than one

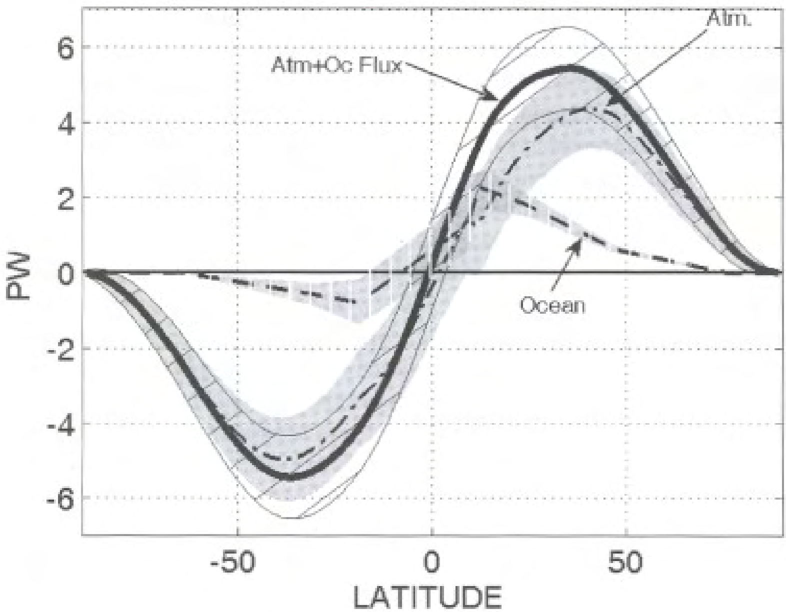


Figure 12.6. Solid curve is estimated transport of the combined ocean and atmosphere as calculated from the net outgoing radiation as measured by the ERBE satellites. Dashed line is the estimate from WOCE hydrography (primarily Ganachaud and Wunsch, 2002, but supplemented by other estimates) of the meridional flux of heat by the ocean. Dash-dot line is the inferred atmospheric transport as a residual of the total and ocean. One standard deviation error bars are shown. (From Wunsch, 2005.) Without WOCE, such calculations would not have been possible for many years, perhaps never.

of heat by the ocean and atmosphere. The ocean component was computed from the WOCE hydrographic long lines; the atmospheric component was estimated as the residual left when the oceanic component is subtracted from the net outgoing earth radiation. Twenty years ago, computing the atmosphere as a residual of measurements of the ocean would have been a laughable goal—indeed, the best oceanographic estimates were done the opposite way—with the ocean calculated as a residual of the atmosphere. Whatever the errors remaining in Figure 12.6 (and they are significant), WOCE made physical oceanography and climate a mature, quantitative subject quite unlike what it was in 1980. The challenge now is to sustain the observations and

---

occasion to asking my wife why I was trying to destroy my career? Sadly, he died just as the program got underway. I like to think that in the end he would have been pleased by how much we have learned about the ocean. Toward the end of his life, he did offer a kind of apology—saying that he thought WOCE was inevitable—in the same way that MODE had been an inevitable program. This comment can be interpreted in several ways!

Bob Stewart was for many years deeply worried about the Soviet initiative, to the point that he published (Stewart and Braarud, 1969) an essay explaining why the effort did not make sense. The Soviet push continued, however (letter from R. W. Stewart to C. Wunsch, 4 April 1983).

model/data synthesis efforts so that our successors will not be as blind as we were in 1980 to the time-evolving ocean.

A number of elements of WOCE failed to come to pass. As already noted, the scatterometer-wind satellite did not fly until the program was almost over (and then failed prematurely), and no gravity mission appeared until the launch of GRACE in March 2002. Efforts to define a full-water column equatorial ocean observation component came to little with the focus of the tropical oceanographic community on the upper ocean alone (to this day, there are no instruments on the TOGA-TAO array—its observational legacy—below 500 m). Some proposed elements, e.g., the open ocean current meter moorings, were never deployed. Few oceanographers have retained an interest in studying the ocean as a whole—rather there has been a reversion toward regional programs and processes (cf. CLIVAR). Whether the wider community will find a way to sustain the global observation network (now primarily satellites, the ARGO float program, the diminishing XBT coverage, and intermittent revisits of WOCE hydrographic lines) is one of the major challenges for the future. Recognition that it needs to be done may perhaps be the ultimate legacy of WOCE. There is little doubt, however, that without WOCE, oceanography would be a very different subject than it is today.

It is worth remembering that WOCE was an extremely controversial program, although the disputes have largely faded from memory. Anyone motivated to organize a future observational experiment of equivalent scope may perhaps be comforted to realize that ultimate success means that the inevitable, if painful, dissent will be forgotten. In the context of the present, more complex situation, in which much more is now known about the ocean and new international bureaucracies exist, an important lesson is that WOCE was a "bottom-up" program—a critical mass of individual working scientists sought to create the program because they believed it scientifically necessary, and because they personally wished to work with the resulting observations.[10] (A few forward-looking scientists recognized that the time span of the program would exceed the span of their own professional careers—they nonetheless worked for its creation because they recognized its scientific importance.) WOCE was born at a time when it was scientifically ripe. Later, "top-down" initiatives arising from the national and international committee structures are often comparatively sterile in outcome because the underlying scientific motivation is secondary to programmatic structures.

ACKNOWLEDGEMENTS

WOCE was the result of efforts by thousands of people in dozens of countries around the world, including program managers, principal investigators, engineers,

[10] That WOCE arose out of the initiative of a few individual scientists eventually became difficult to perceive. Scientists coming to the program after the start of the planning process encountered a WOCE managerial bureaucracy that had been created to implement it, not create it.

technicians, secretaries, ships crews, and many others. In the spirit of a purely personal essay, I would like to particularly acknowledge the work in the earliest days of Professor Worth Nowlin (Texas A&M) without whom the U.S. contribution to WOCE would clearly have come to nought, Professor John Woods (now Imperial College) whose organizational skills were critical in the early years, and Michel Lefebvre for bringing his infectious enthusiasm and the French POSEIDON project to WOCE. Many other far-sighted individuals deserve thanks for their sometimes heroic efforts, but because I am sure to forgot someone, it seems best to simply acknowledge that the community owes a large debt to many people—who at least know who they are. Preparation of this essay was supported in part by the National Ocean Partnership (NOPP) ECCO Consortium funding, an extension of WOCE. I had helpful comments from M. Jochum, D. J. Baker, and W. Munk.

REFERENCES

Böning, C. W., and A. J. Semtner, 2001. High resolution modeling of the thermohaline and wind-driven circulation. In: *Ocean Circulation and Climate: Observing and Modeling the Global Ocean*, G. Siedler, J. Church, and J. Gould, eds., Academic Press, San Diego, 715 pp.

Cheney, R. E., 1982. Comparison data for SEASAT altimetry in the western North Atlantic. *J. Geophys. Res.* **87**, 3247–3253.

Collins, C. A., and R. H. Heinmiller, 1989. The Polymode Program. *Ocean Dev. Int. Law* **20**, 391–408.

Committee on Metrics for Global Change Research, 2005. Thinking Strategically: The Appropriate Use of Metrics for the Climate Change Science Program. Climate Research Committee, Board on Atmospheric Sciences and Climate, Division on Earth and Life Sciences, National Research Council, National Academies Press, Washington.

Davis, R. E., D. C. Webb, L. A. Regier, and J. Dufour, 1992. The autonomous Lagrangian circulation explorer (ALACE). *J. Atmos. Oceanic Technol.* **9**, 264–285.

Dobson, F. W., F. P. Bretherton, D. M. Burridge, J. Crease, and E. B. Krauss, 1982. Report of the JSC/CCCO CAGE Experiment. A Feasibility Study. World Climate Programme Report, WCP-22, Geneva, 95 pp.

Fuglister, F. C., 1960. Atlantic Ocean Atlas of Temperature and Salinity Profiles and Data from the International Geophysical Year of 1957–1958. Woods Hole Oceanographic Institution Atlas Series: I, 209 pp.

Ganachaud, A., and C. Wunsch, 2002. Large-scale ocean heat and freshwater transports during the World Ocean Circulation Experiment. *J. Climate*, **16**, 696–705.

Garrett, C., and C. Wunsch, 1984. A Celebration in Geophysics and Oceanography—1982. In Honor of Walter Munk on His 65th Birthday October 19, 1982. Scripps Institution of Oceanography Reference Series 84–5, March 1984, La Jolla.

Halpern, D., 1996. Visiting TOGA's past. *Bull. Am. Meteorol. Soc.* **77**, 233–242.

Holland, W. R., and L. B. Lin, 1975. On the generation of mesoscale eddies and their contribution to the general circulation. I. A preliminary numerical experiment. *J. Phys. Oceanogr.* **5**, 642–657.

Jeffreys, H., 1933. The function of cyclones in the general circulation. Proces Verbaux de l'Assoc. de Meteorologic. UGGI, Lisbon, Part 2, 219–233, also in Collected Papers, V, 257–269.

Kerr, R. A., 1991. Greenhouse bandwagon rolls on. *Science* 23 August, 845. See Letters to the Editor, by C. Wunsch and by J. McCarthy. *Science* 18 October 1991, 357.

Lorenz, E. N., 1967. The Nature and Theory of the General Circulation of the Atmosphere. World Meteorological. Organization, Geneva, WMO No. 218, T. P. 115, 161 pp.

MODE Group, The, 1978. The Mid-Ocean Dynamics Experiment. *Deep-Sea Res.* **25**, 859–910.

Monin, A. S., V. M. Kamenkovich, and V. G. Kort, 1977. *Variability of the Oceans*. Wiley, New York, 241 pp.

Moore, G. E., 1965. Moore's law. *Electronics* **38**, 114.

Munk, W., and C. Wunsch, 1979. Ocean acoustic tomography: A scheme for large scale monitoring. *Deep-Sea Res.* **26A**, 439–464.

Munk, W., and C. Wunsch, 1982. Observing the ocean in the 1990s. *Philos. Trans. R. Soc. London Ser. A* **307**, 439–464.

Patullo, J. G., W. H. Munk, R. Revelle, and E. Strong, 1955. The seasonal oscillation in sea level. *J. Mar. Res.* **14**, 88–155.

Reid, J. L., 1981. On the mid-depth circulation of the world ocean. In: *Evolution of Physical Oceanography. Scientific Surveys in Honor of Henry Stommel*, B. A. Warren and C. Wunsch, eds., MIT Press, Cambridge, MA, 70–111.

Roemmich, D., and C. Wunsch, 1985. Two transatlantic sections: Meridional circulation and heat flux in the subtropical North Atlantic Ocean. *Deep-Sea Res.* **32**, 619–664.

Rossby, T., D. Dorson, and J. Fontaine, 1986. The RAFOS system. *J. Atmos. Oceanic Technol.* **3**, 672–689.

Stammer, D., C. Wunsch, R. Giering, C. Eckert, P. Heimbach, J. Marotzke, A. Adcroft, C. Hill, and J. Marshall, 2002. The global ocean state during 1992–1997, estimated from ocean observations and a general circulation model. Part I. Methodology and estimated state. *J. Geophys. Res.*, DOI: 10.1029/2001JC000888.

Stewart, R. W., and T. Braarud, 1969. Editorial introduction. *Prog. Oceanogr.* **5**, vii–xi.

Thompson, B. J., J. Crease, and J. Gould, 2001. The origins, development and conduct of WOCE. In *Ocean Circulation and Climate: Observing and Modeling the Global Ocean*, G. Siedler, J. Church, and J. Gould, eds., Academic Press, San Diego, 31–43.

TOPEX Science Working Group, 1981. Satellite Altimetric Measurements of the Ocean. National Aeronautics and Space Administration, Jet Propulsion Laboratory, California Institute of Technology, Pasadena, 78 pp.

Wunsch, C., 1984. A water mass conversion experiment for WOCE. Unpublished study document, 9 February 1984, 10 pp.

Wunsch, C., 1996. *The Ocean Circulation Inverse Problem*. Cambridge University Press, Cambridge, 437 pp.

Wunsch, C., 2005. The total meridional heat flux and its oceanic and atmospheric partition. *J. Climate*, **18**, 4374–4380.

Wunsch, C., and E. M. Gaposchkin, 1980. On using satellite altimetry to determine the general circulation of the oceans with application to geoid improvement. *Rev. Geophys. Space Phys.* **18**, 725–745.