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Right Place, Right Time:  
An Informal Memoir

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**Abstract**

My career spanned the revolution in understanding of the large-scale fluid ocean, as modern electronics produced vast new capabilities. I started in the days of almost purely mechanical instruments operated by seagoing scientists, ones not so different from those used more than a century earlier. Elegant theories existed of hypothetical steady-state oceans. Today, we understand that the ocean is a highly turbulent fluid, interacting over global scales, and it is now studied by large teams using spacecraft and diverse sets of self-contained in situ instrumentation. Mine was an accidental career: I was lucky to be in the right place at the right time.

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**MIT:** Massachusetts  
Institute of  
Technology

**WHOI:** Woods Hole  
Oceanographic  
Institution

**SIO:**  
Scripps Institution of  
Oceanography

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## INTRODUCTION

I have spent most of my career working as a physical oceanographer in the Massachusetts Institute of Technology (MIT)–Woods Hole Oceanographic Institution (WHOI) universe, with forays into such areas as solid-earth geophysics and paleoclimate. Sixty years is a long time in the evolution of any active science, and the field has changed almost beyond recognition both in its culture and in what we think we understand.

When I entered physical oceanography in the early 1960s, it was a small, almost club-like group, one dominated by seagoing observers such as L. Valentine (Val) Worthington and Frederick (Fritz) Fuglister at WHOI and Joe Reid at the Scripps Institution of Oceanography (SIO), along with a number of others in places such as New York, Seattle, Hawaii, the United Kingdom, France, Scandinavia, and Germany. Active participants were largely in the naturalist tradition, using little mathematics, exploring and describing what they saw at sea. A few were academic people (e.g., Ray Montgomery and Henry Stommel) who were not afraid of mathematics. One could come close to knowing everyone in the field everywhere in the world (Soviet Union excluded). It was a quiet pursuit out of the public eye, and most of the work was done in nonuniversity settings, such as WHOI and SIO. The technology was still almost completely mechanical, based on the original bathythermograph and the reversing thermometers used with Nansen bottles. Numerous attempts had been made to develop electrical or electronically based instruments, but the physical setting of a sometimes violently moving ship, high pressures, corrosion, and the available vacuum tubes and primitive tape recorders meant that few such devices were used in any meaningful way. Navigation relied on the stars, sun, and moon; sextant measurements; elaborate tables; and the very real skill of ships' officers. In a few places, the World War II development of radar had led to electronic systems such as loran, but coverage was mostly inadequate. The available English textbooks were those by Sverdrup et al. (1942), which covered *all* of oceanography, and Defant (1961), but the only dynamics described in them concerned waves.

A great attraction of the field to me was that one could be a generalist working on extremely diverse aspects of the physical problems—and have an impact. Today, we know too much; the literature is diverse and overwhelming, and being a generalist is nearly impossible.

In what follows, I have emphasized the old days because that time is less familiar to younger scientists and because it represented much of the transition to the way we do science today, with large, internationally organized field programs; space agencies; and multimillion-line numerical models, all held together, sketchily, with an ever-more-sophisticated theoretical structure. I have abbreviated the description of my personal life; a podcast interview of me by Michael White (2018) covers more. Describing an 80-year lifetime and a nearly 60-year career within *Annual Review of Marine Science* word limits is a considerable challenge!

## Parents

My life is a clichéd story of Jewish immigrants who came to the United States circa 1900 and strived for success via their children and grandchildren in the professions, academic and otherwise. Both of my parents were the children of immigrants. My mother, Helen (Gellis) Wunsch, was born in Dover, New Jersey, the daughter of Morris Gellis and Minnie Bernstein Gellis, who had emigrated from near Vilna, Lithuania, and near Minsk in White Russia, respectively, around 1908. My grandmother was a milliner. My mother grew up in Claremont, New Hampshire, where my grandfather became a successful businessman (running a seltzer business and building some of the first gasoline stations).

My paternal grandparents came to the United States in 1905 from a village called Zborov in what is now Ukraine but at that time was in the Austro-Hungarian Empire (Galicia). All of my

father's older siblings were born in Europe, but both my father and a younger brother were born on the Lower East Side of Manhattan.

My maternal grandmother, Rose Haber Wunsch, died when my father was 11 years old. My paternal grandfather was a failed tailor, who then also failed as a candy-store keeper in Brownsville (as told by my father, he gave too much credit during the Great Depression). The family was rescued from poverty by my father's oldest brother, Joseph Wolf Wunsch, who managed to talk himself into Brooklyn Polytechnic Institute and get an engineering degree. He started the family business, the Silent Hoist and Crane Company, in the Bay Ridge section of Brooklyn, which became sufficiently successful that he moved the family out of Brownsville to Borough Park in Brooklyn and paid for his siblings to go to college. My father went to the College of Engineering at Cornell University, where he met my mother, graduating in 1930 into the depths of the Depression. The financial situation and the death of my father's younger brother in an automobile accident delayed their marriage until 1938.

### Very Early Years

I was born in Brooklyn, New York, on May 5, 1941. My family lived in the Midwood section of Flatbush until I was 12 years old. The family was comfortably middle class and secularized Jewish. My father was a mechanical engineer, first at Silent Hoist and then, when I was a teenager, at a manufacturing plant in Danbury, Connecticut; later, he ran his own gauge block manufacturing company in Stamford, Connecticut. During World War II, he was responsible at Silent Hoist for building heavy lifting equipment, cranes, capstans, and so on for the US Navy.

My parents had five children, of which I was the second. My older brother, David, became an electrical engineering professor at the University of Massachusetts Lowell. My next-younger brother, James, is retired as a professor of urban history at the State University of New York's Empire State College, and my brother Jeremiah (who changed his name to Gerald) is a retired pension lawyer living in Chicago. The youngest, my sister, Sarah, is a well-known civil rights lawyer, now retired from the American Civil Liberties Union of Massachusetts. (At a recent Thanksgiving dinner, I counted seven "Professors Wunsch" over two generations of the immediate family.)

### Move to the Suburbs

In the early 1950s, my parents joined the move of many middle-class people to the suburbs, specifically to Westport, Connecticut. That town had notoriety as the setting of the novel *The Man in the Gray Flannel Suit* and other descriptions of its exurban/suburban milieu. I disliked it and couldn't wait to leave.

Staples High School in Westport had a focus on the humanities—English, foreign languages, and so on—as befitted a town that was home to many advertising people and the Famous Artists School. The science and mathematics at the high school seemed adequate until I arrived at MIT and realized that many public high school students in my freshman class were far better prepared than I was. I did benefit from a summer 1957 program run by the Dorr-Oliver Foundation at the Loomis School near Hartford. It was there that I first encountered computers, and I spent much of the summer learning how to program the several vacuum-tube, punched-card-input IBM 704s at United Aircraft. Roy Nutt and others there had been the creators of the original FORTRAN language, which I learned.

Along the way, in junior and senior high school, I had luckily encountered some exciting teachers. One was William Scheld, who had an enthusiasm for science that was rare in Westport. It was he who encouraged me to apply for the Dorr-Oliver program.

## College

As a high school senior, I wanted to go to Harvard to study history but was rejected. I opted for MIT for various reasons. This choice was also a fortunate one.

I then thought to become a pure mathematician. MIT in 1958 seemed overwhelming. But by the end of the first year, I was reasonably content, having made friends and figured out how to cope, more or less. By the second year, I realized that I lacked the talent to be a pure mathematician—students who did have that talent seemed to operate in a different mental universe, and my direction was unclear. I won an award in history writing; became editorial editor of the weekly newspaper, *The Tech*; and got to know all kinds of people throughout the MIT administration (it was a more intimate place then), including the chairman of the MIT Corporation (Jim Killian), the president (Jay Stratton), and many others.

As a practical matter, I drifted into applied mathematics and gradually realized that geophysics had many fascinating mathematical aspects. These phenomena included, e.g., free oscillations, geophysical heat fluxes, and geomagnetism, and much of my understanding came from the books of Harold Jeffreys. An MIT friend, Phillip Nelson, who was a geology and geophysics major, encouraged me to go down that road.

## After Graduation

In the summer after graduation from MIT, I worked as a general reporter for the *Providence Journal-Bulletin*. At the time, the paper maintained local offices scattered around Rhode Island. The experience was quite fascinating because I was able to handle everything in the town I was assigned to—from obituaries, to town council meetings, to the candidates in the ongoing gubernatorial campaign. A hard-boiled bureau chief presided over a small group of experienced reporters plus me. Sitting and listening to them was an education in itself. As the junior person, weeknights I normally had the 4-p.m.-to-midnight shift, and on weekends I was often all alone in the office. We were asked to generate weekend feature articles that were not hard news but had some human interest component. One learned to tell a story.

The bar across the street was the reporters' hangout. I drank boilermakers and talked to the local politicians there—a very different kind of education. When the paper asked me to stay on, I thought that was an interesting possibility but said that I would like to try graduate school first. The editor I talked with said to come back at Christmas to keep my hand in, but I never went back—the path not taken.

## A LIFE OF LUCK

In looking back at my scientific career, I am struck by how lucky I was—being in the right place at the right time and meeting the right people. I became a physical oceanographer just before the electronics revolution of the 1970s and 1980s, at a time in which the ideas and observational methods would have seemed familiar to the oceanographers of preceding generations, even back to the sailing-ship explorers. I experienced and had found extremely attractive the old-fashioned seagoing work with Nansen bottles, bathythermographs, dead-reckoning navigation, von Arx current meters, and so on. I encountered and sometimes had long sustained collaborations with the much more senior people, including Raymond Hide, Henry Stommel, Frank Press, and Walter Munk. They and other important names will appear below.



**Figure 1**

Professor Raymond Hide, then at MIT. Photo reproduced with permission from MIT.

### Graduate School and Shift to Oceanography

Following my growing interest in geophysics, I applied to graduate school. Compared with other universities I considered, the MIT Department of Geology and Geophysics seemed pleasingly free-wheeling, friendly, and disorganized—another bit of luck.

I was assigned as a first-year research assistant to Steve Simpson, an assistant professor. He was running an MIT group that was part of a government-funded effort called Project Vela Uniform, which was directed at the seismic problem of distinguishing underground nuclear tests from earthquakes. I learned a lot of seismology and time series analysis and took courses in classical mechanics, geomagnetism, applied mathematics, and so on. I thus obtained a background unfamiliar to most physical oceanography students of the time.

For some reason, the distinguished laboratory fluid dynamicist at MIT, Raymond Hide (**Figure 1**), took an interest in me. It was he who secured for me a National Aeronautics and Space Administration (NASA) traineeship, which freed me from the support of any individual faculty member. One day in the spring of 1963, he suggested that in the fall (my second year), I should go and see a new faculty member who would be arriving from Harvard. That was Henry Stommel, of whom I had never heard.

I have written at length elsewhere about Henry Stommel (Wunsch 1997) and will not say much more here. My encounter with him was in many ways electrifying and set the cornerstone of my scientific interests. For the first time, I had encountered a truly charismatic scientist. He was a fascinating character: charming, accessible, inspirational, and full of interesting ideas and stories. I decided I didn't care what he did—I wanted to work with him. I told him I knew nothing about the ocean, and his comment was, "It's better that way." With hindsight, 1963 was the start of by far the most exciting period in the entire history of solid-earth geophysics—the dawn of plate tectonics and the torrent of discoveries and insights that followed. Nonetheless, I never regretted leaving that field! More luck.

The first problem that Stommel suggested I work on was the boundary layers at sidewalls in the steady viscous motion in a rotating fluid. At the time, singular perturbation theory had emerged as the fashionable mathematics of theoretical oceanography. I did not have the mathematical skills to solve this problem, and neither did Hank. After some months of struggle, he suggested

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**NASA:** National Aeronautics and Space Administration

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I consult with Allan Robinson, a Harvard faculty member. My first encounter with Robinson was memorable because he assured me that they had already solved the problem and were just about to publish the results. Returning to MIT, I must have looked a bit glum, because Hide asked me what was wrong. When I told him, he simply laughed and said something like, “Robinson always says that.” As it turned out, this nontrivial problem was eventually solved by the British mathematician Keith Stewartson and subsequent authors, producing what are now known as Stewartson layers.

But I had done enough to get through the PhD preliminary general exam. Hank then suggested long-period tides as a PhD thesis topic. This subject was very unfashionable but had the great advantage that ignorance of the ocean generally would not be a serious handicap. The important names in the field were from long ago: Laplace, Kelvin, Rayleigh, Hough, G. Darwin, et al. An advantage in determining the ocean response was a perfect knowledge of the forcing function. My experience in time series and solid-earth tides made this direction a practical one.

I needed long tide-gauge records, and Hank knew that Walter Munk at SIO had a strong interest in tides and had been compiling and digitizing the pen-recorder records. Taking advantage of Walter’s attendance at the symposium for the opening of the Green Building at MIT in the fall of 1964, Hank introduced us one evening at a party associated with the gathering. He then walked off, leaving me to explain to Walter what I wanted the records for. To my surprise, Walter said, flatly, “No.” But he invited me to SIO to work with the records. With Hank’s agreement and financial support, I drove out to La Jolla the following fall for a several-month stay. Walter was testing me to see whether I was competent. It was the start of a lifelong, if intermittent, close collaboration and friendship between Walter Munk and myself. More luck.

## Going to Sea

Both Stommel and I thought that if I were serious about working on the ocean, I should get some experience in seagoing work. He put me in touch with Art Voorhis, a sensible, easygoing physicist at WHOI who then was particularly interested in oceanic surface fronts. In February 1964, we sailed on the research vessel (R/V) *Chain* from Woods Hole into the North Atlantic, where a blizzard was sitting offshore. I was very seasick, but after two or three days I felt far better, and it was a wonderful experience, unlike anything I had encountered before. Our instruments were the old bathythermograph (a near-lethal device), hydrographic Nansen casts with reversing thermometers, and a towed thermistor chain. Almost everything was mechanical, and I could understand how everything worked. That one could mix ordinary land-based science with such interesting and unusual forms of observation made physical oceanography seem an even more attractive field to be in.

Going to sea had strongly reinforced my inference that observational data were the key to understanding the ocean—no matter how sophisticated the theory. That inference, or obsession, was the major theme of almost everything I did thereafter.

## Family

In 1970, I married Marjory Markel, also from Brooklyn, whom I had known since we had met years before in Westport, Connecticut. We have two children: Jared, born in 1971, and Hannah, born in 1975. As of this writing (2020), Jared is a professor of mathematics at Northwestern University and is married to Jennifer Mattson. They have two daughters: Nora, who is 13, and Harriet, who is 8. Hannah is a professor of critical care at the University of Toronto Medical School and is widowed. Marjory and I feel extremely lucky in our children and grandchildren and very grateful to them. I was again lucky in having a devoted, helpful wife for more than 50 years.



Marjory is an author and illustrator of several children's books and has done magazine and other illustrations. She has a bachelor's degree in English from Cornell University and master's degrees in architecture and education from Harvard. In recent years, she has focused on oil painting.

## Early Career

My PhD on long-period tides was finished in the fall of 1966, and I became a postdoc with Henry Stommel with support from the Office of Naval Research (ONR). In this post-Sputnik era, ONR was actively looking for people and projects to support. A few months later, I was made a lecturer in the MIT Department of Geology and Geophysics, where the newly arrived Frank Press was setting out to build up the oceanography program in the department, which at the time was distinct from the Department of Meteorology but shared the same building. The next year I was appointed an assistant professor and started on the trajectory through the MIT faculty, frequently getting strong advice and support from Press.

Walter Munk became a close friend and colleague, and I have also written extensively about him (Wunsch 2019, Garrett & Wunsch 2020). Like Walter, I had started out in solid-earth geophysics and became interested in time series analysis. By then, I knew Walter well enough to recognize him as the Jupiter of physical oceanography and geophysics: Anyone with similar interests (as I had) who got into his orbit would likely never escape again. I arranged to collaborate and interact over the next decades mainly by keeping a continent between us.

Through the years, I had various offers (or offers of offers) to move to some excellent and attractive places, but I ultimately concluded that my colleagues at MIT and WHOI made MIT by far the most desirable place to be. Press made it easy to stay by convincing the long-time benefactors of the department to endow the Cecil and Ida Green Professorship of Physical Oceanography, which I then held from 1975 to 2012.

In early 1977, newly elected president Jimmy Carter chose Frank Press to be his science adviser (**Figure 2**). By that time, Frank had been the department head for 12 years and was looking for new challenges. To my complete surprise, he insisted that I was his only possible successor. At that time, I was 35 years old and in the midst of building my own scientific career, and we had two young children at home. I didn't want to do it, but Frank was a very persuasive person, and at the end of the day I agreed to become acting department head until someone else could be found to take it on. All I really remember from my long conversations with Frank was that he insisted I had "taste" in scientific problems—a very important element in the job. At MIT, department heads are quite powerful positions, compared with places where the chairmanship rotates.



**Figure 2**

Frank Press with President Jimmy Carter. In 1977, Press had left MIT to become Carter's science adviser. Photo copyright MIT and reproduced with permission.



We ran a search focused on outsiders. After about two years, the search had come up empty-handed, and I bowed to the inevitable and agreed to be the head for a total of five years. In hindsight, it was an interesting, and very useful, experience in working with people and in organizing things, but not at that time my cup of tea. I escaped to a sabbatical in Cambridge, UK, as soon as the five years were up. I would like to acknowledge that I had two exceptionally helpful department administrative officers: first Lynn Hodges Dickey, and then Douglas Pfeiffer. They were both highly organized, excellent in keeping an ear to the ground in a very diverse department, and sources of good advice.

## Joint Program with the Woods Hole Oceanographic Institution

For some years, beginning in the 1960s, ongoing discussions had taken place between MIT and WHOI concerning the possibility of developing a joint PhD program. Discussions began to gel when Frank Press arrived in 1965 as the new head of the MIT Department of Geology and Geophysics. A formal agreement was signed in 1968, leading to the start of the program the next year. During this period, I was either a graduate student or junior faculty and so had little insight into the process. My impression, pieced together from conversations with Hide, Press, Stommel, and others, was that a strong motivation on the WHOI side was the need for fundraising and the perception that an educational program would be a good vehicle for that.

At MIT, Frank Press was determined to strengthen the department in seagoing observations. The first three assistant professor appointments he made in this area were John Sclater (in marine geophysics), John Edmond (in marine chemistry), and myself (in physical oceanography). All three of us were intense users of the ships (and all three of us were ultimately elected fellows of the Royal Society). In the days before the development of the University-National Oceanographic Laboratory System, one could obtain ship time only by meeting with and convincing the local operators to let you have it—in our case, the operators were at WHOI, thus Press's priority.

A former dean at WHOI, John Farrington, is with others writing a history of the joint program (J.W. Farrington, personal communication), and so I will skip over most of it. From my own point of view, the program was essential—almost all my PhD and master's degree students came in through it. Press asked me to chair the first joint oversight committee; in those years, the very disparate elements of oceanography (physical oceanography, chemical oceanography, biological oceanography, engineering, geology, and geophysics) were managed by that one committee. We tried to produce a coherent curriculum in all these areas, quality-control the instructors, manage the housing and interinstitutional transportation, and so on, much of which was challenging in a nonuniversity setting. Over the years, I taught a variety of classes on ocean circulation, time series analysis, ocean waves, inverse methods, and other subjects, partly as the need arose and partly as a reflection of my own interests. My own group was essential to professional and teaching life: I have had 29 successful PhD students, 22 master's degree students, 43 postdocs, and a variety of undergraduate thesis students (a list is included in the **Supplemental Material**). Telling their stories is far beyond my space limits! My experience with Hank Stommel led me over the years to advise students looking for PhD problems to first decide who they wanted to work with. One can change fields and focuses later—but a good adviser is the most important element of all.

The joint program has populated much of the oceanographic community around the world and has to be considered a great success. However, worries do exist—not pursued here—about its future in the changing culture of oceanography.

## SCIENCE

### Internal Waves

At the time I finished my PhD, I probably had as much experience with time series analysis as any oceanography student at that time. Stommel suggested that, inasmuch as the nascent Buoy Group at WHOI was struggling to obtain time series data from moorings, I might usefully try to make temperature measurements using the island of Bermuda as a platform. This suggestion had a history. First, in the early 1950s, Stommel had the idea of building an observatory on Bermuda, which was within easy flying distance of Boston and Cape Cod and had local vessels available. Other than measurements from some globally scattered tide gauges, almost no oceanic time series data existed. After building the observatory, he had great difficulty both in maintaining the several different instrument operations and in interpreting the unexpectedly complicated data, and he abandoned it. Second, Stommel wrote several exhortative papers urging that “a few good engineers” should get involved with oceanography. He had connected with a group of former Apollo program engineers at the MIT Instrumentation Laboratory (which later became the independent Charles Stark Draper Laboratory). Phillip Bowditch was the group head, and John Dahlen was one of the leaders within the group. They were happy to escape the draconian engineering rules of working with NASA for the *laissez-faire* of ONR. Stommel introduced me to the group and helped me obtain ONR support, and we were in business.

At the time, the nascent WHOI Buoy Group, under the new direction of Nicholas Fofonoff and Ferris Webster, was having difficulties in obtaining data records from surface-moored buoys. We proposed running a cable horizontally at depth out from Bermuda. If that could be done, it would provide two benefits relative to Stommel’s previous cable: We would escape from the complexities of the bottom boundary layer, and we could take advantage of the relationship between two thermistors separated only horizontally. In a considerable engineering feat, using the primitive vessels available at Bermuda, the installation was a success, and some interesting data came out of it (Wunsch & Dahlen 1970).

At the time, WHOI current meters did not measure either temperature or pressure (the latter of which is essential on subsurface moorings, as they were prone to lying over in high currents). The Draper–MIT temperature/pressure recorder was at one time in wide use (Wunsch & Dahlen 1974) but gradually became obsolete as the current-meter developers added the capability. The cable-handling skills developed for what the Draper engineers liked to call “the ocean telescope” were usefully applied in the WHOI Internal Wave Experiment (see Briscoe 1975).

I had become worried about the influence of the huge Bermuda topographic platform on the phenomena we were studying. In my first attempt at understanding the influence of sloping topography (Wunsch 1968), I supposed that the waves would reflect from the slope before reaching the apex. When, with the help of the lab technician at WHOI (Bob Frazell), we tried to reproduce the waves in a tank, it became clear that they did not reflect at the frequencies we were using, but instead propagated into the corner, where they broke down. That led to a second paper (Wunsch 1969), which emphasized the role of the critical slope.

When Seelye Martin came to MIT as a postdoc, we used the bottom wedge as an absorber to prevent internal-wave reflections when we were studying nonlinear resonant internal-wave interactions in the large current-meter towing tank at WHOI (Martin et al. 1972). Fifty years later, internal waves on slopes are an industry.

In 1968, I visited Stewart Turner at the University of Cambridge Department of Applied Mathematics and Theoretical Physics for a few months. There, again stimulated by the Bermuda data, I worked out the mathematics of the upwelling boundary layer along the slope. Stewart immediately recognized the similarity to a solution that his Australian compatriot Owen Phillips had

produced at Johns Hopkins University. Owen and I exchanged papers, and they were printed in successive issues of *Deep-Sea Research* (Wunsch 1970, Phillips 1970).

Some biological speculation existed about the ocean mixing and stirring effects of Bermuda. As a recent postdoc, I had obtained two weeks of ship time on the new R/V *Atlantis II*. That a postdoc, a month or two after obtaining his PhD, could become chief scientist on a major oceanographic vessel with a crew of about 30 and a scientific party of similar size is today probably a fantasy. In hindsight, it was probably the scariest of all my undertakings, involving getting a science party together (there were no ship's technicians); borrowing STD (salinity–temperature–depth) systems, lab tables, and so on; and inviting enough others on board so that we could use the ship 24 hours a day. As we were getting ready to leave Woods Hole for Bermuda, Hank Stommel came aboard and said to me, “Remember you can't come back early”—a reminder that ship time could not be (visibly) wasted. We deployed some surface moorings and did a series of hydrographic spokes around the island, leading to a primarily descriptive paper (Wunsch 1972) that seemed to show a distinct increase in what was then called microstructure (now referred to as fine structure) in moving closer to the island. I returned to Bermuda to do a better job on the somewhat marginal but cheaper WHOI vessel, the R/V *Gosnold*, but as the instrumentation improved, others have taken up the study of mixing near islands, seamounts, and boundaries more generally.

With my students and postdocs, we carried out several internal-wave-related and mixing-field programs, including observations near Muir Seamount, in Hudson Canyon, on the US continental slope; in Lake Kivu in Africa; near an equatorial island (Jarvis Island); and too many other places to list here. The major landmark in the study of internal waves was the Garrett & Munk (1972) spectrum—which unified a puzzling field of “wiggly lines.” That in turn led to a highly sophisticated set of theories of statistical interactions in wavenumber space, theories whose demonstration in oceanic observations was extremely difficult (my main interest). It was time to move on.

### Inverse Methods and Levels-of-No-Motion

In the 1970s, it became widely known that Val Worthington, who had been trying for many years to piece together a synthesis of the North Atlantic circulation from hydrographic data, had finally concluded that the flow had to violate geostrophic balance. The presumption then (as is still common today) was that the long ship-based hydrographic sections did represent some sort of vaguely defined time-mean circulation. His synthesis was a book (Worthington 1976), and just before publication, he announced that he would summarize his best estimate of the circulation at the weekly Tuesday afternoon seminar at WHOI. As Worthington had let it be known that he would present a case of whisky to anyone who could find a geostrophic solution for the circulation that would balance mass, temperature, oxygen, and so on, he attracted a good crowd.

I had never worked on the oceanic general circulation, but it all sounded interesting, and I attended (I routinely went to WHOI on Tuesdays on the joint program bus). I listened to Worthington, and the idea that a massive flow (the Gulf Stream return circulation) existed so as to violate geostrophic balance struck me as impossible. I thought it would be interesting to tackle the problem, without really having a clear idea of how I would set about it. I asked Val if he could let me have a data set that I could use to understand the problem, and he promptly gave me a set of hydrographic sections forming a (Bermuda) triangle, running from the US coast near Cape Hatteras to Bermuda and then back to the US coast at Florida. He said that if I could balance that triangle geostrophically, he would be convinced.

Geostrophic flow fields at that time were inevitably computed relative to a depth of an assumed level-of-no-(horizontal)-motion. To a great extent, the practice was so deeply embedded in the

science that it was often forgotten that it was a convenient fiction. Textbooks had sections pointing to this difficulty but did not offer a solution.

All I did was write down algebraically the requirement of mass and temperature conservation in the triangle in different thermal layers, letting the reference-level velocities (the putative levels-of-no-motion) be unknowns. I had more unknowns than knowns. This rang a bell in my fading memory of geophysical problems, and I knew that geophysical inverse theory, as worked out by George Backus and Freeman Gilbert at SIO, claimed to be able to deal with such situations in the presence of noise. I then read the various papers by Backus and Gilbert. Backus, being an applied mathematician, had begun proving theorems in function spaces, and I was struggling. So one day I telephoned my geophysics colleague Theodore (Ted) Madden and briefly explained what I was trying to do, and he quickly said, "Oh, don't read that—read Ralph Wiggins's paper" (Wiggins 1972). Ralph had rewritten the whole business in discrete space and pointed to the textbook by Lanczos (1961). The mathematics became far more straightforward (linear algebra). Worthington's conclusion was turned on its head: An infinite number existed, represented in the null space of the equations.

I wrote it up (Wunsch 1977a) and offered to describe what I had done in the same WHOI seminar series. I explained the solution, and it seemed well received (later I could have made it much simpler). At the end, Val stood up, said that he hadn't understood anything I had said but his friends had told him I had solved the problem, and presented me with a case of whisky miniatures! (Some of the wider reception was quite hostile, as linear algebra was not within the experience of the seagoing hydrographers who dominated the field.) With hindsight, the eminent Japanese oceanographer Koji Hidaka (1940) had tried something similar. But Defant (1961, p. 377) had shown that Hidaka's equations were ill conditioned (he wrote them as  $N$  equations in  $N$  unknowns), effectively killing that direction. In any case, a computer was needed!

Coincidentally, Hank Stommel, who was on sabbatical in Germany working with Fritz Schott, had produced the beta-spiral method for finding the reference-level velocity. We had two very different-looking methods for solving the classical problem, and Russ Davis (1978) quickly showed that they were formally identical, albeit different in practice.

## More Box Inversions

Stommel and I talked all the time, and I think inspired each other. He and two colleagues had written a provocative paper claiming to show that the North Atlantic was in true Sverdrup balance (Leetmaa et al. 1977). Their fundamental assumption, however, was that a level-of-no-motion existed in the time-averaged Atlantic at approximately 1,500 m. In the context of the box inversions, that struck me as unlikely; it also implied a small North Atlantic meridional heat flux, something in which Stommel had also become interested. Using our two recent hydrographic lines (see below), Dean Roemmich and I showed that the result was an assumption rather than a demonstration (Wunsch & Roemmich 1985). I used the same sections to estimate the heat transport, which was indeed a good deal higher than would be consistent with Sverdrup balance (Wunsch 1980).

## Equatorial Physics

During a sabbatical at the University of Cambridge in 1973–1974, I was trying to understand nontidal sea level variations, as those were the only existing multiyear oceanic records. Gordon Groves (1955) had shown the existence of a bizarre four-day spectral peak in sea level at Canton Island in the Pacific. This peak was so strong that it seemed quixotic to proceed without being

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**MODE:** Mid-Ocean Dynamics Experiment (comprising two phases, MODE-0 and MODE-1)

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able to understand it. In discussions I had with Adrian Gill, we concluded that we were seeing a baroclinic equatorial mode but in sea level variations. At that time, equatorial modes were a favorite topic of a number of atmospheric and oceanic theoreticians, but the idea that they would be visible in sea level had apparently never occurred to anyone. We analyzed the records and constructed a theory showing that the sea level signature was indeed large enough to be measured by a tide gauge (Wunsch & Gill 1976).

My interests then shifted to fieldwork in and on the equator in the Indian and Pacific Oceans (e.g., Hendry & Wunsch 1973, Wunsch 1977b), but because of the growing interest in El Niño and the rise of the Tropical Ocean Global Atmosphere program, the field was getting crowded. I left it to my student Charles Eriksen, and it again seemed sensible to focus on something else.

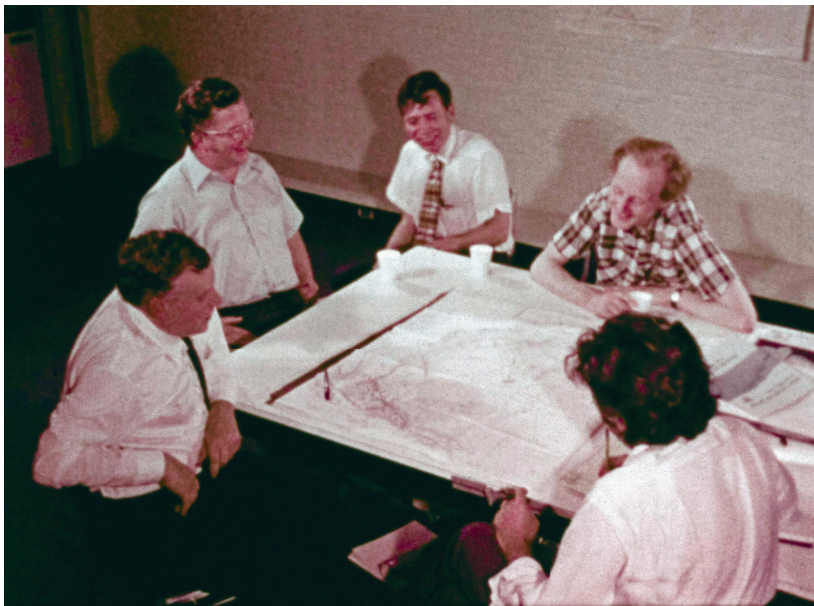
### The Mid-Ocean Dynamics Experiment and Eddies

Ironically, just as the classical problem of a steady oceanic flow in the guise of the reference-level velocity appeared to have been solved, the whole notion of a steady-state ocean began to unravel. In his 15 years as an MIT professor, Hank Stommel had been interested in harnessing the ingenuity of the hundreds of engineers that he was surrounded by there. He proposed that there should be a collective effort to exploit the new time series–measuring technologies that had finally come to fruition (for details, see Wunsch & Ferrari 2018). The WHOI Buoy Group was, for example, finally able to obtain time series from both surface and subsurface moorings extending over several months. Walter Munk, D. James Baker, and others had been perfecting ocean bottom pressure gauges; Neil Brown’s CTD (conductivity–temperature–depth) systems were almost commonplace; John Swallow, Thomas Rossby, and others had developed neutrally buoyant floats and other technologies; and the modeling community was advancing with new computer power. The intellectual context included the known powerful role of the eddy field in the atmospheric general circulation (much of it the work of our MIT colleague Victor Starr and his collaborators) and the somewhat mysterious, unexpectedly energetic, float observations of John Swallow and Jim Crease from the United Kingdom (Crease 1962), as interpreted by Norman Phillips (also of MIT). The **Supplemental Material** includes the first apparent written mention, in a memorandum by Hank Stommel in 1969, of an appropriate field experiment.

The upshot (see MODE Group 1978, Robinson 1983) was a 1973 US–UK collaboration for four months in a region south of Bermuda. Without repeating the great detail that can be found in the published literature, the outcome of the Mid-Ocean Dynamics Experiment (MODE) (**Figure 3**) and its troubled US–Soviet POLYMODE successor was the powerful indication that the ocean was filled with a geostrophically balanced eddy field (misnamed the “mesoscale”) that dominated the kinetic energy of the ocean. That inference suddenly undermined the sense that there was any real description or understanding of the ocean circulation for which a beautiful set of theories had arisen in explanation, including Sverdrup balance, the Stommel–Arons abyssal flows, abyssal recipes, steady Ekman layers, and so on. Global exploration seemed called for, and a number of scientists (notably William Schmitz of WHOI) set out to place moorings around the world for a year or two at different locations (the mooring and moored instrument technology having again advanced) to understand what was typical, if anything, in the global field. It was clear, however, given the resources available, that decades would be required to obtain such records globally, and even then it was unclear whether one or two years at a location would be adequate. Float tracking had to be done by ship or by expensive moored acoustic hydrophones, again producing very short temporal records.

What to do?





**Figure 3**

The Mid-Ocean Dynamics Experiment (MODE) Executive Committee. Clockwise from left: Henry Stommel, Nick Fofonoff, Carl Wunsch, Francis Bretherton, and Allen Robinson (back turned). Image reproduced with permission from MIT from the film *The Turbulent Ocean* and the efforts of Alex Griswold.

## A GLOBAL TURBULENT OCEAN

By the time we had finished MODE-1, it was clear that either physical oceanographers were going to become irrelevant academic theoretical scientists or we were going to have to solve the problem of obtaining usable global measurements. I went looking for solutions.

### Altimeters

My original involvement with remote sensing was reluctant, as I regarded most of what NASA had done (very approximate sea surface temperature measurements) as being of greatly exaggerated importance. Succumbing to some arm-twisting, I became involved with a National Research Council committee that was advising NASA circa 1973. (A lengthier history will appear in an upcoming book edited by Jean-Louis Fellous with the tentative title *A History of Meteorology, Atmosphere and Ocean Sciences from Space in France and in Europe by Its Actors*, to be published in French by the Institut Français d'Histoire de l'Éspace; an English version is provided in the **Supplemental Material**). Sea surface altimetry, if sufficiently accurate and precise, was the one ocean surface measurement we could connect to the whole water column physics. Seasat was to be flown in 1978, carrying a crude (by today's standards) altimeter.

The Seasat measurements lasted only three months, but that was sufficient for Michael Gaposchkin (a geodynamicist at the Harvard-Smithsonian Observatory) and me to write a review article (Wunsch & Gaposchkin 1980) using real data and describing both geoid improvement and the tantalizing presence of oceanographic signals. Scientists and engineers at the NASA Jet Propulsion Laboratory consequently developed enthusiasm for a much more accurate altimetric spacecraft. Bob Stewart was the project scientist, and he, Charlie Yamarone (the project

**Supplemental Material** >



**Figure 4**

A meeting of the original Topography Experiment (TOPEX) working group in Washington, DC, in 1980. From left to right: Fritz Schott, Carl Wunsch, Jim Marsh, George Born, and a bit of Joe Reid. Photo by Robert Stewart and reproduced with permission.

engineer), and I, with input from many other people, put together a science working group that seemed to cover the major and surprisingly diverse elements involved in doing useful altimetric measurements—including geodesy, orbit determination, tides, tracking systems, backscatter from a moving complex conducting surface, atmospheric loads, data reduction and use, ionospheric and atmospheric water vapor corrections, and calibration, as well as all of the engineering expertise required to create a satellite that not only worked but also could survive both launch and several years in orbit (see **Figure 4**).

Selling the project to the wider US scientific oceanographic community was painful. Boiling it down, one well-known WHOI scientist told me, “I’d much rather have another ship,” and another said, “What we really need is a lot more floats near the Gulf Stream.” At one of our new-start briefings, I was pointedly asked by the NASA administrator, James Beggs, why there were so few people from WHOI and SIO involved.

We used the flight of Topography Experiment (TOPEX)/Poseidon to help justify the field programs of the World Ocean Circulation Experiment (WOCE, described below)—an opportunity to supplement in situ measurements with a true global data set. We also used WOCE to justify the flight of TOPEX/Poseidon—an opportunity for a NASA/Centre National d’Études Spatiales (CNES) mission to have an independently funded supporting field program. Collaboration with WOCE was helped immensely by the active participation of the French geodetic scientist Michel Lefebvre—who, beginning with his involvement in the French altimeter project, Poseidon, became a shrewd and enthusiastic proponent of the global supporting oceanographic program. In parallel, we were also working with the European Space Agency, which was planning to fly European Remote-Sensing Satellite 1 (ERS-1), which had a somewhat less accurate altimeter.

The extended period from the sketch design described in the 1980 TOPEX report to the actual TOPEX/Poseidon launch in 1992 and the distribution of the first data sets was a saga ranging

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**TOPEX:** Topography Experiment

**WOCE:** World Ocean Circulation Experiment

**CNES:** Centre National d’Études Spatiales

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from the decision to collaborate with the French, to long debates about the orbit, to threats from what was then called the Defense Mapping Agency to classify all of the data (a classified but low-quality military altimetric mission, GEOSAT, existed), to the need for NASA/CNES to determine an unclassified geoid, to the connection to WOCE. Numerous crises occurred prior to launch, including a NASA demand that the spacecraft be recoverable, an announcement that the batteries would fail within months, and so on—a story in itself. In the end, the now huge altimetric literature and the operational continuation of altimetry are the best testimony that the effort was worthwhile. At one time I was an altimetric expert—now I'm an onlooker.

## Acoustic Tomography

The other major technology I was involved in was also serendipitous. In the summer of 1977, as a member of the Jason governmental advisory group, I went to La Jolla for a three-week summer study directed at nonacoustic antisubmarine warfare. Walter Munk, who could be very persuasive, convinced me that there was a crisis in the US capability and that it was my patriotic duty to come work with him on the problem. Walter's memory of what happened differed a bit from my own, but the bottom line was that we never did anything about submarines. We hadn't seen each other for an extended period (although both of us were involved in *MODE*), were sharing an office in La Jolla, and were simply catching up on what each of us had been doing for the past several years. Walter described the 25-km reciprocal acoustic transmission experiments of his student Peter Worcester. I had been working on inverse methods in the context of the level-of-no-motion problem.

As we gossiped, it dawned on us that if we put together what Walter knew about acoustics with what I knew about inverse methods, we could make an interesting observing system, particularly at long ranges. We proceeded to work out the details of range, coverage, processing, and so on. After a few days, the Jason director, Dick Garwin, wandered in to ask what we were doing; when we told him, he said, "You've just reinvented [medical] tomography." The first written account of the technology was by the three of us in an unclassified Jason technical report. Walter and I went on to make it practical, based upon collaboration with Worcester and numerous colleagues from acoustics, engineering, and oceanography. In a later book (Munk et al. 1995), we attempted a summary of this work.

When Walter and I again shared an office on a joint sabbatical in Cambridge, UK, we took the opportunity to write a paper emphasizing the new technologies that had or would become available by the 1990s (Munk & Wunsch 1982). We did emphasize tomography relative to altimetry and so on, because altimetry required getting a recalcitrant space agency to approve a multi-hundred-million-dollar expenditure, while tomography appeared to be much more in our own hands—a small group of seagoing scientists. Although tomography has greatly progressed (see Howe et al. 2019), it has not (yet) come into the widespread use we had anticipated.

## The World Ocean Circulation Experiment

I have described the origins of WOCE at some length elsewhere (Wunsch 2006b). Was it possible to understand the behavior of a variability-dominated global system?

In 1981, Dean Roemmich and I, inspired by the power of inverse methods, had carried out the first trans-North Atlantic, top-to-bottom hydrographic sections that had been done since the International Geophysical Year (IGY). Our intention had been to space the stations for the first time, so as to have eddy resolution in repeating two of the IGY sections. I was chief scientist on the *R/V Knorr* for the first crossing (36°N, Woods Hole to Cádiz), and Dean was chief scientist

on the return leg farther south (24°N). I returned from that trip in a state of frustration: I knew that the ocean on the western side had changed by the time we arrived at Spain a month later, and we had spent endless hours dealing with a recalcitrant winch and conducting cable—a nineteenth-century technology. Dean and I wrote up the differences from what had been observed in the IGY (Roemmich & Wunsch 1984), but we started the title of our paper with “Apparent Change in . . .,” as the calibration offsets between the old Nansen bottle sections and the new ones were obscure. It all just confirmed my sense that, as physical oceanographers, we needed a new approach in both technology and sampling strategies.

The first proposal for what became known as WOCE was made, I think, by me in 1979 at a meeting in Miami of an international panel that was trying to formulate a successor to the Global Weather Experiment, one that would encompass climate. What followed was years of discussion, planning meetings, and finally the launch of TOPEX/Poseidon and of the major field programs from 1992 to about 1997. Altimetric measurements have continued since then; the community finally obtained wind-scatterometer<sup>1</sup> and gravity missions [the Gravity Recovery and Climate Experiment (GRACE) and Gravity Field and Steady-State Ocean Circulation Explorer (GOCE) satellites] from space and hydrographic profiles from the Argo program that arose out of the WOCE Autonomous Lagrangian Circulation Explorer (ALACE) float experiment (Davis et al. 2001). A further discussion of this major international effort is left to the two scientific summary volumes (Siedler et al. 2001, 2013).

### Estimating the Circulation and Climate of the Ocean

By about 1992, it was evident that some form of WOCE would actually occur, and we would have a variety of global or near-global data sets. They were to be of radically different types and sampling attributes. How could we use them? The only example we knew of the application of global data to a fluid was in numerical weather prediction. What was being done was the data assimilation of observational atmospheric data into general circulation models (GCMs) at intervals of 6 hours, for the purpose of producing useful forecasts out to several days—basically an engineering problem. The oceanic problem was, however, not prediction but rather understanding, for annual and longer timescales.

Prediction and understanding are distinct goals. Norbert Wiener and others had distinguished filtering (an estimate of what is happening now) from prediction (a best estimate of the future) and smoothing (what happened over some finite interval in the past). What we needed to do was to adjust the ever-improving oceanic numerical models so that they were consistent with the WOCE observations of all kinds—smoothing. But that proved a hard sell. One well-known data assimilation expert told me that what I wanted to do was impossible. It took me some months to understand that what he really meant was that he didn’t know how to do it! An initial proposal to NASA was ignored for over a year and then returned unreviewed.

In the early 1990s, Jochem Marotzke arrived as a postdoc from Kiel. We set out to demonstrate that we could adjust a full oceanic GCM to be consistent with various data types. Carlisle Thacker had shown that a model adjoint could be used efficiently in fitting a complex numerical model to observations over extended time intervals. Marotzke and I determined to try that with the Princeton Geophysical Fluid Dynamics Laboratory GCM.

In the mid-1990s, Ralf Giering, who had been a student of Klaus Hasselmann at Hamburg, came as a postdoc. His PhD thesis had involved producing a computer code that would take

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<sup>1</sup>A considerable dispute existed at one time as to whether measuring the wind field over the ocean (the prime circulation driver) was a more important goal than measuring the ocean itself by altimetry. Unsurprisingly, scatterometry was forcefully favored by part of the modeling community.

another computer code—that of an ocean GCM—and automatically produce a third code representing the adjoint GCM. The use of adjoints for optimization problems was well known, but because very large GCM codes were perpetually being modified and updated, manually maintaining a corresponding adjoint code was a forbidding undertaking.

Coincidentally, John Marshall had arrived as a faculty member from Imperial College London, and he was constructing a new oceanic GCM. With advice and help from Giering, we managed to have the MIT gcm (as it became known) always be adjointable at least semiautomatically. With the arrival of Detlef Stammer, also from Kiel, we put together a program and finally a successful proposal and outcome that was named Estimating the Circulation and Climate of the Ocean (ECCO). That program continues to this day, with Patrick Heimbach taking over the local MIT effort when Stammer left for a faculty position at SIO. ECCO now continues outside MIT, with the major centers being at the Jet Propulsion Laboratory (Ichiro Fukumori and others), Atmospheric and Environmental Research (AER) (Rui Ponte), and the University of Texas at Austin (Patrick Heimbach) (see, e.g., Fukumori et al. 2018).

## Paleoclimate

With the expected successes of WOCE, my own attention turned toward the global ocean—it was difficult to understand what the ocean circulation and its expected changes did to the climate system if global sampling would require a wait of many decades. And what if the complex regional behavior could not be generalized from one place to another? It was clear that the ocean really did change everywhere, all the time, on numerous spatial and temporal scales. Even the most basic theory (and a few scattered observations such as radiocarbon concentrations) suggested that the ocean circulation changed on timescales ranging from seconds to thousands of years. And because the ocean is an integrator of disturbances, the ocean should “remember” effects of atmospheric and other changes (ice cover) for hundreds to thousands of years.

In the 1990s, apart from extremely sparse and scattered temperature and salinity measurements of sometimes doubtful accuracy, the only comparatively long records came from tide gauges, spanning in a few instances 100 years, commonly with serious calibration issues. Satellite and in situ measurements, even where quasi-global, were increasing in duration by one year per year at best. To obtain a 100-year record was going to take 100 years, and then only if the systems were sustained. Any serious approach to understanding how the ocean interacts with climate overall requires either very long, global-scale records or numerical models with demonstrated skill on long timescales—but how is that skill demonstrated without long records?

I was aware of the revolution that had occurred in paleoclimate, including paleoceanography. Much of this exciting activity was based on the isotopic fractionation chemistry and measurements that had emerged out of World War II, the Deep Sea Drilling Project, and ice coring. Thus, the notion that the paleoclimate record would substitute for some of the missing data on past behavior was extremely attractive.

I set out to familiarize myself with the capabilities of paleoceanography and paleoclimate, starting with basic textbooks. A robust conclusion is that the climate system and the ocean must have had radically different states in the past. But much of the paleoclimate field is filled with impressive storytellers and stories (what I call “geonovels”); a separate essay on this subject would be required, but some of my somewhat jaundiced view of the need to distinguish between what *could* have occurred in the past and why, and what *did* demonstrably happen and why, can be detected in a few publications (e.g., Wunsch 2006a, Huybers & Wunsch 2010). Perhaps paleoceanography will follow the pattern of physical oceanography—with a (hypothetical) revolution in data density followed by great progress in understanding of the physics.

## Tides, Again

Altimeters solved the multicentury problem of determining open ocean (barotropic) tides. One of the surprises of the TOPEX/Poseidon data was the conspicuous global presence of the internal tides. From the experience of my paper with Adrian Gill (Wunsch & Gill 1976), I should have anticipated that. In practice, the work of Richard Ray and his collaborators opened up a whole industry of understanding the implications of its presence, an industry that continues in full force today. My inclination has always been to seek oceanographic problems where I didn't have to worry about serious competition from numerous other groups. So, apart from a paper I wrote with Haidvogel and Iskandarani (Wunsch et al. 1997) about modeling the long-period tides, I mainly stayed out of this aspect of altimetry.

In 1996, however, when David Cartwright turned 70, a symposium was held in his honor at the Royal Society in London. Walter Munk and I both spoke. We started to think about the tidal dissipation problem, which had a history going back to Laplace, Kant, and others and which at that time was widely thought to occur primarily on the shallow continental shelves, based on work of Taylor, Jeffreys, and Heiskanen, circa 1918–1920.

Walter was deeply interested in the subject and had written several papers about tidal dissipation, in part because there existed a major puzzle about the history of the lunar orbit.<sup>2</sup> We began what became extended discussions of the implication of a powerful internal tide, possibly accounting for 50% of the lunar tidal energy loss. That led us in turn to examine the oceanic mixing implied. We wrote this up (Munk & Wunsch 1998) a bit tongue in cheek, because, as one colleague said to me, “Everyone knows the tides have nothing to do with the ocean circulation.” But it struck a chord and became a stimulus for studies of internal tide generation, propagation, and decay. (Some readers missed the major point—that an energy source had to exist for the small-scale, turbulence-like structure invoked by theoreticians and modelers to mix the ocean. The tide is evidently an important, but not the only, element involved.)

## CHANGE

When I started out in physical oceanography, much of the attraction of the field lay in the ability to know almost everyone; the fact that there were so many obvious problems, both theoretical and observational; and the fact that one did not need to compete with anyone else. The wider public had no particular interest in what we thought the fluid ocean was doing. Indeed, the textbook picture—used to tell meteorologists, biologists, and geologists what the fluid ocean meant for their fields—was of a weak, unchanging flow. A beautiful set of explanatory theories existed (Sverdrup balance, westward intensification, thermocline structure, and much more). This world began its slow change in the 1960s, when the transistor appeared, integrated circuits were on the way, and massive computing power became available. Powerful theoreticians invented and brought the subject of geophysical fluid dynamics to fruition, and real collaboration became possible between theoreticians and seagoing observers. Useful textbooks addressing dynamics appeared [e.g., by Pedlosky (1979) and Gill (1982), both of whom had meteorological backgrounds]. But few seagoing observers were comfortable, e.g., with the details of baroclinic instability in a western boundary or equatorial current, and few theoreticians were comfortable when a hydrographer would explain why a temperature measurement is noisy and not “truth.”

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<sup>2</sup>The modern rate of lunar recession, if it has been constant through time, implies that the moon would have been at the Roche limit (so close to Earth that it would have been torn apart by Earth's gravity) one billion years ago. This event was known not to have happened.

Much of the theoretical leadership in the years following World War II was directed at bringing the ideas of dynamical meteorology to bear on the sparse oceanic observations. These ideas included, e.g., theories of baroclinic and mixed instabilities, Rossby wave properties and interactions, and turbulence in Ekman-like layers. Shifts in these emphases began with MODE-1 in 1973 and related field programs, in which it became clear that the ocean was a turbulent system. (In his original memo—see the **Supplemental Material**—Stommel notably called for analyses of future ocean data by meteorologists rather than oceanographers, as the latter had no experience with an eddy field. It didn't work out that way!) Both the analytic and observational challenges that emerged were forbidding, and it took several decades before observational and computational capabilities began to approach what was needed to cope.

The culture of physical oceanography has changed beyond recognition. In the June 1980 issue of the *Journal of Physical Oceanography*, one paper had three authors, three had two authors, and the remaining 14 had one author. In the June 2019 issue, no papers have one author, five have two authors, and the remaining 11 have three or more (two have eight authors). These numbers tell a story of a maturing, and now highly collaborative, science that would have been unrecognizable to the ocean scientists of preceding decades.

Fields necessarily change with the times and go in and out of fashion, dependent upon the appearance of new technologies or new ideas. For physical oceanography, separating the appearance of new ideas from the appearance of new technologies is impossible. Theories have, with rare exceptions, followed new observations. Post-MODE, innumerable eddy-turbulence studies have been produced.

Many people spend their careers appearing as the eighth or fifteenth author in ever-lengthening lists, and their real contributions are unknown to outsiders. With the growth and diversification, some lazy appointment committees have taken to simply counting publications, citations, and so on, not bothering to understand originality and promise. The growth of model capability has led many in the scientific population to focus on the models per se, comparing them with each other, and rarely if ever asking whether any are actually skillful in terms of the observations. The popularization of climate science, the requisite simplifications, and its essentially instantaneous connection to the public have had an often corrosive effect. Some journals now live or die by their ability to obtain media coverage.

Some simple extrapolations, at least from the standpoint of university science, are possible. The day in which generalists could thrive is likely over. The field is now mature: An enormous amount of technical detail is known; instruments are understood only by specialists; and, as in geology, we recognize the vital importance of specific regional knowledge. On the other hand, a large number of new and interesting problems of the fluid ocean, on all spatial and temporal scales, remain as dragons to be slain over the coming decades.

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## Errata

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