Informal Memoir. A Working Draft

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1 Introductory

This sketch memoir[1] was originally stimulated by a podcast interview of me by Michael White (16 May 2018), geoscience editor of Nature. He asked some questions about my life and career that I hadn't thought would interest anyone else. But I then thought to record them in writing before my memory goes altogether. If anyone feels obligated to write a memorial of my work, maybe it will be of some assistance. As an overview, my life is almost a cliche of the story of Jewish immigrants to the US circa 1900, and their striving for success via their children and grandchildren in the professions, academic, and otherwise. An earlier, condensed, version of this memoir was published by Annual Review of Marine Science (2021). At best, much of what follows will likely be of interest only to my immediate family. To some extent, my life has been boringly simple: I've had one wife, one job, two rewarding children, and apart from short intervals such as sabbaticals, have lived for over 60 years in one city.

Parents

My parents were both the children of recent immigrants. My mother, Helen Gellis, was born in 1910 Dover NJ 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,

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[1] The MIT webpage (as of this writing) ocean/~cwunsch has various links to things like my cv , discussion of the The Great Global Warming Swindle, etc.

where my grandfather became a successful business man (among other activities, running a seltzer business, and building some of the first gasoline stations). My mother had two younger siblings, both born in Claremont. My mother attended Cornell University (Class of 1932), the first in the family to attend college, and her siblings went to Radcliffe, and to Harvard, reflecting the Jewish obsession with education as a way out of the ghetto. Her brother, Sydney Gellis, became a prominent pediatrician in the Boston area, eventually serving as Dean of the Boston University Medical School and as head of the Boston Floating Hospital (Tufts Medical). Her sister, Edith, married an important local obstetrician, David Kopans, who had grown up in the then heavily Jewish Dorchester section of Boston.

My paternal grandparents came from a village called Zborov in what at the moment is in Ukraine, but at that time was part of the Austro-Hungarian empire (Galicia), and which has changed countries many times over the years. They were called Gallizianers (as opposed to the northern European Jews who were Litvaks, with a certain amount of tribal rivalry amongst the Ashkenazi Jewish population). All of my father's four older siblings were born in Europe, some in Vienna, but my father (born 1910) and a younger brother were both born on the lower East Side of Manhattan.

I never knew my maternal grandmother Rose Haber Wunsch, who died when my father was 11 years old. My paternal grandfather was a failed taylor, 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 povery by my uncle, 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 (also known as Wunsch Engineering) 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. (Joe Wunsch later became a considerable philanthropist, including donating considerable funds to Israeli institutions such as the Technion, and to Brooklyn Polytech.) 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 traumatic death of my father's younger brother in an automobile accident delayed their marriage until 1938.

My mother's family had a branch that made it to Palestine before the European Holocaust. Her cousin, Hannah Rawitz (a Bernstein married to Kurt Rawitz), told in a letter of escaping by sleigh across the fields ahead of the Germans. Many of this family branch were Bernsteins, and at least one of them. in a later generation, Nathan Bernstein came to the US and became a New York dealer in old masters (and married into the Bauer fortune). On my father's side

Figure 1: My uncle Joseph Wolf Wunsch who became a serious philanthropist, including particularly the Weizmann Institute in Israel. Here he is (middle) with physicists Albert Einstein and James Franck. {einstein_joe.

all he seemed to know what was that numerous family had died in the death camps. Some parts of the family he thought had converted to Catholocism. (Note that one branch of the family spells their name as Wonsch—an error made in the papers showing discharge from the Austro-Hungarian Army.The papers were so important that correcting the error was deemed not worthwhile.) Decades later (about 2015) we were located by a branch of the family living in Toronto—we learned that I have a second cousin (Pepka "Marysha" Mandel) who had been a young girl at the time of the German invasion of Galicia, and who had managed to pass as a German, after her entire family had been wiped out. She is a grand-daughter of Abraham Wunsch and Chane Feige Katz of Zborow—per email from Zack Gutfreund, her granson). There was an emotional meeting with our daughter Hannah—the first Wunsch family member she had encountered since 1942. (My brother Jerry knows quite a bit of family genealogical lore on the Wunsch side.)

At the end of World War II some of my mother's family were still alive in the Soviet Union and in letter communication—I can recall food packages being sent to them. They vanished in the Stalin era of the late 1940s to early 1950s. My mother did have endless cousins, many growing up in North Dakota. Hannah constructed a family tree. With at least two families having had 10 children in two different generations, it gets hard to grasp.

Very Early Years

I was born in Brooklyn, New York on 5 May 1941 and where my family lived, until I was 12 years old, at 1316 E. 24th Street near Avenue M in what is called the Midwood section of Flatbush. Michael White asked what it was like then: it was essentially a Jewish-Irish/Italian

Catholic ghetto, very roughly dividing across Bedford Avenue.

He asked what I recalled of World War II given that it ended when I was 4 years old: My father was in a protected profession. He was the Vice-President and chief engineer of the family-owned business near Fort Hamilton in Brooklyn. They made heavy lifting machinery such as fork-lifts, cranes, naval capstans and the like and presumably most of that business was for the military and related industries, hence the protection of my father from the draft. He did serve as a civilian air-raid warden, and I recall his going out at night with an armband, and a flashlight, checking that the local blackout was being observed. (East coast cities were blacked out because there had been major shipping losses to German submarines of near-coastal shipping back-lit by the city lights.) On V-J Day in August 1945, I and a lot other neighborhood children marched down the street banging on pots and pans and generally making noise (in Belle Harbor, where we had a summer house, 144 Beach 101st St., in Rockaway Queens). I also recall my mother fussing with her ration stamps and books, and collecting bacon fat and cans etc., for various war drives. I have a very vague (and conceivably unreal) memory of being woken late one night by what I was told was a destroyer that blew up in New York Harbor. Shortly after the War (I learned to read early), I recall the "Gold Star Mother" signs in neighborhood windows (a Gold Star Mother being one who had lost a son in the War).

I went to Public School PS193 a couple of blocks from home. The teachers were either Jewish or Irish Catholic, many of whom had entered the profession for security during the Depression years. At this distance it isn't easy to evaluate their quality, but I do remember that some were excellent (I particularly recall one I had twice, Beatrice Schwartz, I think in both 4th and 6th grades. She treated me well, but she herself was very musical and I think I frustrated her by my inability to carry a tune or display any real musical talent.) Following my older brother, I belonged to the Cub Scouts. As children we were permitted to freely play games on the local streets, to ride our bicycles in a wide area around Brooklyn, and to take the subway with a friend to visit museums etc. in Manhattan all unaccompanied by any adult. It seemed extremely safe. With my older brother, we built model airplanes and flew them at Marine Park. He also built ham radios, and I did a bit too, but never found it particularly interesting. My older brother was a Yankee fan (in Dodger-mad Brooklyn) and so I followed suit, although with our father we would occasionally visit Ebbets Field.

The family was secularized Jewish. My father would go to the Synagogue on the High Holidays. My older brother and I both had bar mitzvahs (my brother at a Brooklyn orthodox synagogue. Myself and my younger siblings later in Connecticut at a Reform synagogue.) My maternal grandmother, estranged from her husband, lived in her own apartment nearby on

Figure 2: House at 1316 E. 24 St., Brooklyn in the 1940s. For New York City archive, see email from David Wunsch 16aug2020. {e24stbrooklyn

Avenue L and was at our house most days. She kept kosher, but my mother did not, and my grandmother would happily cook bacon for her grandchildren. We were prosperous enough to always have household help—in the form of black (Negro as then described) maids, some of whom lived in with us.

My mother, who had a degree in Library Science from Simmons College (following Cornell graduation) did not work after marriage and stayed home with the children. Before that she had worked for the Widener Library at Harvard as (I believe) a cataloger—probably of German books (her major at Cornell had been German). My father, who could be quite over-bearing at times, was a skillful engineer, and very talented with his hands. The wood carvings around our house are all his. He also proved to be an excellent painter and became quite expert in the repair of mechanical clocks of all sorts. I inherited none of these skills—some of them skipped a generation or two.

My parents had five children, a number more usual at that time. My older brother, David, is an emeritus electrical engineering professor at what is now U. Mass Lowell. My next-younger brother James, is retired as professor of urban history at Empire State College, SUNY, and brother Jeremiah (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 ACLU Massachusetts. At a recent Thanksgiving dinner, I counted seven Professors Wunsch in my own and in the next generation—a scientist, mathematician, electrical engineer, urban historian, urban archaeologist, art historian (with even more by marriage).

Move to the Suburbs

Post-World War II, a major part of the US population moved to the suburbs, with cars being ever-more available. New York City was undergoing a downslide (that perhaps culimated later in the near-bankruptcy notably marked by the famous NY Daily News headline "Ford to City: 'Drop Dead'", Ford being the then-President of the US. My father, who had grown up in the grossly overpopulated, poor areas of the Lower East Side of Manhattan, and in a similarly situated area of Brownsville in Brooklyn, had developed a very strong wish for his own land (the house in Brooklyn was a detached wooden one, on a small plot of land, quite close to the neighbors). Thus in 1953 they joined the "flight to the suburbs" moving all of us to Westport Connecticut which was then farther out than most suburbs and was known as an "exurb".

The town with, I think at that time with about 12,000 inhabitants, was made famous, or infamous, by the Sloan Wilson novel *The Man in the Gray Flannel Suit*, and others of similar ilk. I loathed it there, but my parents were quite oblivious to my feelings (how could anyone not find it a happy place to live?) As a 12-year old and a very shy young teen-ager, I was completely dependent upon others for transportation. Kids didn't ride bicycles around; it was the Eisenhower conformity era, and my peers were all a year older than I was (I had skipped the 7th grade on entering Junior High School —a social mistake—but one of those things my parents thought was in my best interests.) There were cars, drinking, ballroom dancing lessons, Congregational Church cultures, etc. all of which was entirely alien to me. My general shyness became a big handicap.

Previous to the move to the suburbs, my father resigned from the Silent Hoist and Crane Co., and took some time (a year?) to explore his interests. They included courses in various subjects at The New School, including writing. After the move he worked for some time in Danbury, Connecticut for a company that made things like HVAC ducting and its outlets (Connor Engineering).. After a falling out with the owner, he decided to buy a company of his own, a small manufacturer in Stamford Ct. called Fonda Gage Co. They made the high precision "gauge blocks" that were used in manufacturing where tolerances had to be very precise and accurate. The company had about 15 employees, many originally Puerto Rican, most of whom worked in the machine shop taking metal blocks of special materials and reducing them, by a series of cutting, grinding and polishing, with ever-finer abrasives until they passed a test of consistency with National Bureau of Standards directly calibrated blocks. I did work there one summer while in high school, for some weeks running a wet-grinder—coming home at the end of the day covered in water-soluble lubricant—before I was moved into office work. The company was ultimately sold (after I had moved away) when it became clear to my father that none of his children had any interest in taking over the company.

The Westport household was held together by a remarkable, originally southern, black woman, Fanny Drain (was married to Marcellus Drain). Although she did not live with us, she commuted from Stamford most days, when we were all young, to Westport, essentially running the household. Fanny was a member of the family, without whom my mother could never have coped. She was a ball of energy, happy, and served to a large-extent as a supportive surrogate mother particularly to my younger siblings.

High School

Staples High School, then located on Riverside Avenue in Westport, had a focus on humanities: English, foreign languages, etc. as befitted a town with many advertising people, the Famous Artists School, etc. Science/math seemed ok, until I got to 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 between my junior and senior years of high school run by the Dorr-Oliver Foundation at the Loomis School near Hartford. It was there that I first encountered computers. We were first taught to program an IBM605[?], using a wired plug-board, then spent much of the summer learning how to use the FORTRAN language to program the several vacuum tube, punched card input, IBM 704s at United Aircraft. Roy Nutt and others there had been the creaters of the original FORTRAN language.

The summer after my senior year I worked as a computer programmer for Perkin-Elmer, in Stamford. My parents knew one of their scientists and that's probably how I got the job. It was programming things like fourier transforms for the measurements of their instruments. Input to the computers (Burroughs?) was through paper tape—now a very antiquated method. [I think I have the chronology correctly.]

Along the way, in Junior and Senior high schools, I had luckily encountered some exciting teachers. One who sticks in my mind 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. Raymond Tata, a high school teacher, was also a dynamic individual with a real love of mathematics.

College

As a high school senior, I wanted to go to Harvard, and applied there, and also to MIT, Swarthmore, Cornell. Cornell was a "safety school" as both my parents and older brother had gone there. I was rejected by Swarthmore, wait-listed, and then rejected by Harvard. My parents were convinced that I was a victim of the Jewish quota at Harvard.[2] Probably I would have majored in history had I attended Harvard. I opted—not wanting to go to Cornell—for MIT. A lucky break.

I had thought to become a pure mathematician. At MIT the first year seemed overwhelming, as it still operated as a mainly engineering school, with a lock-step curriculum, a rotating series of 3-hour labs (Chemistry, Physics), and weekly exams every Friday morning. Required ROTC had, fortunately, just been eliminated. I struggled with physics in particular, and was unhappy enough to consider transferring elsewhere. But by the end of the first year, I was reasonably content, having friends, figuring out how to cope more or less (even with mediocre grades).

By the second year or so, I came to realize 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 it became an issue of what I would do after college. I won an award in history writing (Boit Prize). I became editorial Editor of the weekly newspaper, The Tech (grades suffered even more), and I got to know all kinds of people throughout the MIT administration (it was a smaller, more intimate

[2] My wife, Marjory, was greatly amused that Harvard did accept to what would have been my class, the man later infamous as the Unibomber!

place then) including the Chairman of the Corporation (Killian), the President (Stratton) and many others.

As a practical matter, I drifted into applied mathematics: the Department had a distinguished applied faculty led by C. C. Lin with David Benny, Alar Toomre, Harvey Greenspan, and others. I gradually realized that geophysics had many very interesting mathematical aspects applicable to the Earth that were equally or even more interesting than proving mathematical theorems. These Earth phenomena included e.g., free oscillations, bulk heat fluxes, etc., with much of my understanding coming from the books of Harold Jeffreys. A close college friend, Phillip Nelson, who was a geology/geophysics major encouraged me to go down that road.

I had a summer job at Sikorsky Aircraft in Stratford Ct, living at home in Westport. It was really a non-job, working in their weights and flight readiness division—I was resented because they had just gone through a series of layoffs and there was almost nothing to do. I spent a fair bit of time, working my way through the second part of Thomas's classic textbook on calculus. The summer was mainly of interest for the experience of being with lower middle class working engineers. I was happy when it ended.

I spent one summer at the Harvard Summer School in Cambridge where I studied Russian and took up sculling on the River. The MIT Math Department required that all of its majors had to pass exams demonstrating reading ability of *two* foreign languages. I could cope with French, but having taken a Russian course (during the Cold War Russian was the language of choice and, of course, it was a major mathematical language in any case), I found it very difficult and didn't think I could pass the exam without more course work. I did later pass the Russian exam, but realized subsequently that German would have been a much more useful and accessible language.

One summer (the chronologies of summers is fuzzy in my memory), I applied for and was accepted by IAESTE, an international organization directed at making it possible for international exchanges of students. I was accepted, and was sent to Newark-on-Trent, in the UK, an almost classical Midlands town where I worked for the Ransome and Marles Ball Bearing Company. It was actually fun—I lived with the Apprentices in a house with a housekeeper/cook and her caretaker husband, and had a very nice supervisor (Roy Gaul) who ran their computer system which I helped program. The town had numerous pubs and not much else for recreation. On weekends I wandered around the UK and then took days in parts of western Europe. I went to and fro Europe, as one did in those days, by ship from New York—the French line vessels Flandre going over, and the Ile de France returning. All were full of students and others, and provided the kind of social experience that is no longer generally available.

After Graduation

In the summer after graduation from MIT, I worked as a general reporter for the Providence Journal-Bulletin. That came about from my role as Editor of the MIT student newspaper, when I got to know Jeff Wylie quite well. Jeff was the head of the MIT News Office, but previously had had a long career with Time Magazine and had been their Boston bureau chief. He suggested that I might consider becoming a journalist and put me in touch with The Wall Street Journal Foundation. That Foundation encouraged students to become reporters by subsidizing their summer salaries. At that time, pre-Watergate, journalism was a poorly paid profession. I applied, was accepted, and assigned to the Providence newspaper. The paper maintained local offices scattered around the state of Rhode Island. I lived in Providence (Thayer Street) in a room rented in the house of two elderly sisters and commuted to Warren, RI (toward the end of the summer I was re-assigned to West Warwick, but that was a much shorter stint).

The experience was quite fascinating as one got to write about everything that happened in the town, from obituaries to covering the Town Council meetings, to the candidates in the on-going gubernatorial campaign. A "hard-boiled" bureau chief (Sid Jagolinzer) presided over a small group of experienced reporters plus me. Just sitting and listening to them was an education in itself. As the junior person, I normally had the 4PM to midnight shift, and on weekends I was often all alone in the office. Copy was sent to Providence either as typewritten manuscripts sent by bus (the Short Line), or if near a deadline, on a teletype connected to the main editorial offices. One could not backspace a teletype machine, so I got a lot of practice in trying to be a fast, and accurate, typist/writer. We were asked to generate weekend "feature" articles that were not hard news, but that had some human interest component. That helped in learning to think of and "tell a story."

The bar across the street was the reporters' hangout. I learned to drink boilermakers and talk to the local politicians there. It was a very different kind of education. I must have made a reasonable impression on the Journal as I was asked to stay on—I think with the idea that ultimately I could become a science reporter. I found that an interesting possbility, but I said I'd first like to try graduate school. The Journal editor I talked with said to come back at Christmas to "keep my hand in", but I never went back to it. It was the "path not taken."

Graduate School and Shift to Oceanography

Following my growing interest in geophysics I applied to graduate school at MIT in what was then the Geology and Geophysics Department, and to the Lamont Observatory of Columbia University. I had interviewed at Harvard which had the distinguished geophysicist Professor Francis Birch whose undergraduate course I had taken, but they wanted me to first learn geology and mineralogy before doing any geophysics, and I had no interest in that. When I visited Lamont (to which I was accepted), I talked with some very good people like the young Lynn

Figure 3: Stephen M. Simpson later on as Anne L. Simpson. {simpson_steph

Sykes and some others of the faculty (John Nafe) They explained that it could take up to 9 years to get a PhD, as grad. students were expected to first spend 6 months at sea, then more months learning how to read seismograms, etc. (at least that's what I recall). Often the students became so useful, that their supervisors were reluctant to let them finish up and go elsewhere. That left MIT Geology and Geophysics. I had thought to leave MIT (the President, Julius Stratton, had suggested I should go to Stanford), but the MIT Department seemed pleasingly free-wheeling, friendly, somewhat disorganized, and totally unlike the MIT Mathematics Department.

I was assigned as a first year research assistant to Stephen Simpson[3], an assistant professor of geophysics, who was running an MIT group that was part of a government-funded effort called Project Vela Uniform. Vela Uniform was directed at the seismic problem of distinguishing underground nuclear tests from earthquakes. (Other Vela efforts were directed at detecting tests in space and the atmosphere.) The effort at MIT was a legacy of efforts at MIT in Geology and in Mathematics to exploit the mathematical prediction tools developed by Norbert Wiener during World War II. I learned a lot of seismology and time series analysis, and took courses in classical mechanics, geomagnetism, applied math. etc. The Vela Uniform group was housed then in the famous World War II Building 20 about which much has been written. My fellow students included Jon Claerbout and Ralph(e) Wiggins who went on to become prominent practitioners of applied geophysics. I wrote one forgettable paper about the structure of multiple time series (Wunsch, 1965) with the urging and help of Prof. Enders A. Robinson, a frequent visitor and a well-known mathematical statistician as well as friend of Simpson's and the other faculty.

After my second graduate year (in 1964) I was part of an ongoing summer program run by Geophysical Services Inc (GSI) which was part of Texas Instruments in Dallas. I can remember

[3]Steve Simpson later became Anne Simpson in a trans-gender change.

Figure 4: Raymond Hide at the time he was an MIT Professor. {ray_hide_phot

the date because it was the year following the Kennedy assasination, and I had some distaste about going to Dallas. I had a shared office in the Exchange Bank Building downtown. Cecil Green, who had founded GSI, and was a co-founder of Texas Instruments, had an office down the hall and would wander by occasionally to see what we were doing. We each had a project, and there were talks by visiting prominent geophysicists. It was where I first encountered Frank Press who was a visitor. John Burg, who apparently invented maximum entropy spectral estimation, was also there. Milo Backus, Bill Schneider, and others at GSI looked after us. I have no memory of what I actually worked on there, but it was part of my geophysical education particularly in time series analysis.

For some reason, an MIT faculty member, the distinguished laboratory fluid dynamicist Raymond Hide, took an interest in me. I believe it was he who secured for me a NASA Traineeship which freed me from then on from the support of any individual faculty member. I think Ray realized I would be of no use in the laboratory, and one day (the spring of 1963) he suggested that in the Fall I should go and see a new faculty member who would be arriving then from Harvard. The new faculty member was Henry Stommel, of whom I had never heard. Ray said that Stommel was a “genius” and that a move from Harvard to MIT was almost unheard of because Harvard paid so much better.[4]

I’ve written elsewhere at length about Henry Stommel (Wunsch, 1997) and will not say very much more here. My encounter with him was in many ways electrifying. He was a fascinating character, charming, accessible, inspirational, and full of interesting ideas and stories. It was my

[4] I think the greatest compliment I ever received was reported to me second hand: That when asked what his greatest contributions to oceanography had been, Ray Hide said "introducing Carl Wunsch to Henry Stommel.” Maybe true.

Figure 5: Henry Stommel, circa 1962, from Royal Society biographical memoir. {stommel_1962.

first encounter with a truly charismatic scientist. 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 by far the most exciting period in the entire history of solid-earth geophysics—the dawn of plate tectonics and a torrent of discoveries and insights that followed. Nonetheless, I never regretted leaving that field!

My experience with Hank Stommel has 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. Several excellent students chose specifically to work with someone other than me—best for all of us.

The first problem that Stommel suggested I work on was the extension of ordinary Ekman layers to the steady viscous motion in a rotating fluid at vertical walls—this period was one of the enthusiastic application of the then-new singular perturbation theory to everything in oceanography. I did not have the mathematical skills to solve this problem and neither did Hank—after some weeks of struggle, he suggested I consult with Allan Robinson, a Harvard faculty member. That was my first encounter with Robinson and it was memorable because he assured me that he had solved the problem and that "we" were just about to publish the results.

Figure 6: Walter Munk. Date? {munk_younger.

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, the problem was eventually solved by the British applied mathematician, Keith Stewartson, in what is now a classic paper—describing “Stewartson layers.”

But I had done enough to get through the PhD preliminary general exam. Hank then suggested that as a PhD thesis topic that I look at the long-period tides. This subject was very unfashionable, but had the great advantage that ignorance of the ocean circulation would not be a serious handicap. The big names in the field were from long ago: Laplace, Kelvin, Rayleigh, G. Darwin, Hough, et al. Stommel had the instinct that long-period tides might be connected to Rossby waves—something which at that time had never been detected in the oceans.

I needed long tide gauge records—the Pacific Islands appeared to have the most promising data. Hank knew that Walter Munk at SIO had a strong interest in tides and had been compiling and digitizing the analogue (pen-recorder) records. Taking advantage of Walter’s attendance at the Symposium for the opening of the new 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 went on to invite me to SIO to work on the records. With Hank’s agreement and financial support beyond the Traineeship, I drove out to La Jolla the following Fall for a several month stay. With hindsight, I was being tested by Walter to see if I was a competent person. It was the start of a life-long, if intermittent, close collaboration and friendship between Walter Munk and myself.

Fellow Stommel oceanography students with whom I overlapped included David Halpern, Ants Leetmaa, Robert Knox and others. From a group of students in the “other” Department

Figure 7: Frank Press with President Jimmy Carter. Press had moved from MIT to being Carter's Science Adviser. (Copyright MIT) {press&carter.

(Meteorology) many working with Jule Charney and Norman Phillips on the same floor of the Green Building from which came a number of distinguished atmospheric scientists (e.g., Dickinson, Wallace, Kalnay, Holton, Fung, ...and others).

It was only in graduate school that I began to realize that even though I tended to be "slow," that I could actually do something useful and interesting in science. MIT was full of very bright undergraduates, ones who appeared to do homework problems in minutes—problems that would take me hours—and who would do the weekly hour exams in 20 minutes. A lot of them seemed to spend much of their time playing bridge. The graduate school transition—from being able to do problems that someone else has set, and knows there is an achievable answer, to ones where one must formulate the questions—is one that many previously successful students never surmount. Experience suggests that if a student has gotten all "A-s" during their school careers, it is not a useful predictor of future research capabilities.

Going to Sea

Both Stommel and I thought that if I were serious about working on the ocean that I should get some experience in sea-going work. He put me in touch with Art Voorhees, a sensible, easy-going physicist working at WHOI who at that point was particularly interested in oceanic surface fronts. In what must have been February 1965, we sailed on the R/V Chain from Woods Hole into the North Atlantic where a blizzard was sitting off-shore. I was violently seasick, but after two or three days I felt far better, and we had crossed the Gulf Stream into the much warmer conditions of the Sargasso Sea. It was a wonderful experience unlike anything I had encountered before. Our instruments were the old fashioned bathythermograph (BT—which was 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 occasionally mix ordinary land-based science with such intriguing and unusual forms of observation made physical oceanography seem an even more attractive field to be in.

One of the more interesting sea-going expeditions I was involved with was the leg of the International Indian Ocean Expedition (1965) with Henry Stommel as chief scientist on the R/V Atlantis II. We sailed from Perth Australia to Kobe, Japan, with an extended stop in Manila. Stommel had been interested in double-diffusive processes beginning with the Stommel-Arons-Faller "perpetual salt fountain" paper. He thought that in the Banda Sea, within the Indonesian archipelago, the warm salty water coming from the Pacific Ocean would overlie the colder, fresher water of the Indian Ocean, giving rise to the so-called salt-fingering regime which he hoped to detect with the then new STD profiling instruments built by Neil Brown.

In this period, Indonesia was run by the dictator Sukarno, with whom US relations were tense. We sailed from Perth with only a promise from the US State Department that clearance for working in Indonesian waters would be forthcoming before we arrived there. In the event, permission was refused, and we were told we had to go through Indonesian waters under "innocent passage." The Captain (widely known as "Charlie Tuna, Chicken of the Sea") interpreted that to mean that no scientific measurements, including operation of the echo sounder, could be made. Days of boredom, the then still-novel equator-crossing celebration, and cheap duty-free whisky sold by Captain Tully, led to a drunken, raucous, all-day party. (The Captain then stopped all sales of duty free alcohol. Days later, he realized that under the system in place, he personally owned the alchohol stores, and that he had to get rid of them before rotating off the ship in Manila. He asked Stommel to find a face-saving excuse to re-start sales. I recall Hank wandering into the wet lab, calendar in hand, anxiously looking for some holiday that would provide a rationale. That time of year there wasn't much, and all he could come up with was Robert Fulton's birthday—who at least had a nautical connection—and that proved adequate to the need.)

But some real science did get done. Stommel, improvising as is necessary on any oceanographic cruise, decided to measure what was anticipated to be boring temperature and salinity profiles in international waters of the Indian Ocean south of Indonesia. These, to widespread surprise, were filled with small scale structures dubbed "microstructure" (later re-defined when higher resolution instruments became available, as "fine structure." The well-known Russian scientist, Konstanin Federov, was on board the ship at Stommel's invitation, and he became so fascinated by the phenomenon that he insisted that he had to make all of the measurements. Stommel and Federov (1967) wrote up the results, essentially founding the field of oceanic fine and microstructure science.

STDs (later CTDs) were controversial devices amongst what were called "water catchers." Val Worthington, coming aboard in Kobe as successor chief scientist to Stommel, took one look at the array of instruments sitting on deck and ordered the bosun to "Put those down in the hold. I don't want to see them again." (Years later he changed his mind.)

Subsequent Career

My PhD on long-period tides was finished in Autumn 1966 and I became a post-doc with Henry Stommel with ONR support. A few months later I was made a lecturer in the Department of Geology and Geophysics as the newly-arrived Frank Press set out to build up the oceanography program in the Department. The next year I was appointed an assistant professor and started on the trajectory through the MIT faculty. Walter Munk had suggested that instead I should come out to work with him at SIO. By then I knew Walter well enough to recognize him as the "Jupiter" of geophysics and physical oceanography: anyone who got into his orbit would likely never escape again. So for several years, I kept the continent between us (although Walter came as a Visiting Professor at MIT, sitting in July Charney's office for a semester. Probably 1968 or 1969. One contribution I made to science in that period was introducing Walter to Dennis Moore, an Allan Robinson student at Harvard. They wrote a paper together on equatorial waves—one which proved wrong—but in an interesting way.) Munk later arranged an SIO faculty offer, but my wife, Marjory who I married in 1970, did not want to live in southern California, and that was an ample excuse for staying put.

Sometime around 1973 or 1974 I was offered the Robert Burden Chair in Earth Sciences at Harvard University. Having by then been at MIT for15 years, it seemed sensible to seriously consider moving, and indeed we were living within a five minute walk of the Harvard Department.

In response, Frank Press went to the MIT benefactor Cecil Green (for whom our Building was named) and convinced him to endow a chair that could be used to keep me at MIT. In the end, I decided to stay at MIT: the deciding factor was Marjory telling me, sensibly, that if I had Allan Robinson for a colleague I would get an ulcer. Thus I became the Cecil and Ida Green Professor of Physical Oceanography. Frank was an excellent faculty mentor in general, and we did spend a fair bit of time together sharing sailboats (see below) and later on, playing racquet ball[5]. I do recall one specific comment he made to me when I was appointed assistant professor: "Remember—now you have to give away your best ideas." Although through the years I had various offers (or offers of offers) to move to some good places, I had by then concluded that my colleagues at MIT and Woods Hole made MIT by far the most desirable place to be. At WHOI, I especially remember interactions with Nick Fofonoff, Bill Schmitz, Bruce Warren, Ferris Webster, Joe Pedlosky, Stewart Turner, Jack Whitehead, Bob Frazell, Gordon Volkmann, Charlie Parker, and others, as well as the Port Captain and Port Engineer. Many interesting

[5] In the interval when he returned to MIT between being Science Adviser, and becoming President of the National Academy of Sciences.

and productive physical oceanographers also spent time at MIT as faculty, including P. Rhines, R. Beardsley, W. R. Young, Willem Malkus, and others. The Mathematics Department faculty included such fluid dynamicists as Lou Howard, Harvey Greenspan, Alar Toomre, CC. Lin, David Benney, et al. And everybody seemed to visit.

Two possibilities did tempt me—sometime around 2000, I was short-listed to become Director of the Lamont-Doherty Geophysical Observatory (as it had by then become known). That interested me because it was flexibly part of a major, private, New York City university (Columbia), had some excellent faculty, and in many ways seemed scientifically less than the sum of its parts. The upper west side of Manhattan was one of the few places that Marjory felt comfortable with as a place to live. My goal would have been to shake the place up (among other possibilties to build a center on the main campus so that undergraduates could be better involved with Lamont).[6] In the end, I was not offered the job, which was both a relief and a disappointment. It would have meant largely stopping doing my own science, which was the most enjoyable part of my job as a professor. The other possibility not long afterwards, came about because I was encouraged to apply for the new G. I. Taylor Chair in the Department of Applied Mathematics and Theoretical Physics in Cambridge UK, a place where the family had spent two year-long sabbaticals. It had been a very important oceanographic outpost, particularly in producing a stream of outstanding physical oceanography scientists (Garrett, Thorpe, Rhines, et al.) with the presence, particularly, of Adrian Gill, Michael Longuet-Higgins, Herbert Huppert, and others. With the departure of Adrian to Oxford, Cambridge fell on oceanographically hard times, and as a source of excellent students it dried up; that hurt the field. In the end, the position went to the Russian scientist Grisha Barenblatt, an excellent, but more conventionally-oriented, fluid dynamicist.

In early 1977, the newly-elected President Jimmy Carter chose Frank Press to be his Science Adviser. By that time, Frank had been the Department Head for 12 years and was clearly looking for new challenges. To my complete surprise, he insisted that I was the only possibility as his successor. At that time, I was 35 years old, in the midst of building my own scientific career, was going to sea, 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

[6] This was the period just following an instance when the then Governor of New York had stated in a public speech that what the State needed was a "Woods Hole on the Hudson." That caused wide-spread embarassment both to the Governor and to Columbia. I probably gave the University the impression of being a trouble-maker. Among other discussions, I did tease the Dean of undergraduate admissions by asking him if Columbia would today admit a Richard Feynman as a freshman—notoriously, Feynman, a New Yorker, had wanted to go there. The Dean's answer was still "no"–Feynman was not a "well-rounded" person. I told the Dean that Harvard would accept him. Probably he was not in favor of having me around!

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 said I had "taste" in scientific problems—which in fact is a very important element in the job. (We have had Heads who clearly had no such taste.) MIT calls its Department Chairmen "Heads," as they are quite powerful positions, when compared to places where the chairmanship rotates among senior faculty every three or so years. At MIT, one is appointed, not elected, and some people had successfully held the position for more than two decades. The other principle I picked up from Frank was that one's role as a supervisor was to find out what people were interested in and good at doing—and to help them succeed. I did try.

We ran a search for Department Head, focussed 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 5 years. In hindsight, it was an interesting and very useful experience in working with people and in organizing things, but not then really my cup of tea. Meetings of the Science Council (the Department Heads plus the Dean of Science) were actually quite interesting—as some of the Heads included John Deutch, Herman Feshbach, Jim Kinsey, Boris Magasanik, Ken Hoffman, Danny Kleitman and others, I escaped to a Cambridge UK sabbatical as soon as the 5 years were up. Bill Brace agreed to become Head. I would like to acknowledge that I had two exceptionally helpful Department Administrative Officers, first Lynn Hodges Dickey, and then Douglas Pfieiffer. They were both highly organized, excellent in keeping an ear to the ground in what was a very diverse Department, and as sources of good advice.

After watching department chairmen, institutional directors, provosts etc. at MIT and institutions all over the world, I concluded that for most people 7 years was the maximum time that anyone should hold one of these positions. One's intellectual capital is depleted, old problems come around again and are far-less interesting, and one should move on. Obviously, exceptions exist to the 7 year rule, but they are rare. To go back to actually doing serious science after more than about 5 years in administration is rarely accomplished.

Joint Program with Woods Hole Oceanographic Institution (WHOI)

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. During this period I was either a graduate student or a very junior faculty member and so had little insight into the process. Discussions began to gel when Frank Press arrived in 1965 as the new Head of the G&G Department. A formal agreement was signed in 1968 leading to the start of the program the next year. 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 fund raising, and the Director, Paul Fye, believed (probably correctly) that raising private funds for an education

Figure 8: John Edmond, photo from Royal Society biographical memoir. {john_edmond_r

program would be much easier than for a collection of special scientific problems.

On the MIT side, certainly in the mind of Frank Press, the need for his faculty to have access to the sea on research vessels was paramount. His intention was clearly to maintain the strength of G&G on the observational side. The first three appointments of assistant professors he made in this area were John Sclater (marine geophysics), John Edmond (marine chemistry), both Scots, and myself in physical oceanography. All three of us were intense users of the ships and all ultimately were elected Fellows of the Royal Society—Frank had good taste. In the days prior to the invention of UNOLS, one could only obtain shiptime by meeting with the local operators—in our case, Woods Hole, and thus Press's priority.

Apparently, a number of people at WHOI were unhappy at the prospect of a PhD program—having specifically gone to WHOI because it was *not* a university with its separate faculties, obligations, and hierarchies. Widely forgotten was that WHOI, its leaders not wanting to be dominated by MIT, sought a parallel program with Harvard University. That proposal failed when voted down by the Harvard faculty, as being a program that could not be properly supervised (personal communication from R. Hide, ~1968).

A former Dean at WHOI, John Farrington, working with others, has written a history of the program and so I will skip over much of it. From my own point of view the JP was essential—almost all my PhD and master's students came in through the JP. Press asked me to chair the first joint oversight committee—in those years, the very disparate elements of oceanography (physical, chemical, biological, engineering, geology, geophysics) were managed by that one committee. We tried to produce a coherent curriculum in all these areas, to quality control instructors, to manage the housing, inter-institutional transportation, etc., much of which was challenging in a non-university setting.

A major administrative mistake was made relative to the JP when Charles Hollister became the WHOI Dean. He insisted that his MIT counterpart had to be of equal status—thus, also a Dean. He was completely unable to understand that the responsibilities of an MIT Dean were totally different than his own. His counterpart until that time had been the Charman of EPS/EAPS for whom the JP was a major attention-requiring matter because a large fraction (half?) of the Department's graduate students belonged to the JP. The Dean of Science had nominal oversight (signed off on the budget) because of the presence of a biology component, and an engineering component in another MIT School. In practice, the MIT Chairman ran the program.

When Charlie Hollister started complaining (I was away on sabbatical and only learned about it later), the responsibility was taken away from EPS, and put into the Provost's Office. For the Provost, the program represented about 100 grad. students out of about 4500 overall, was of little or no interest, and he responded by appointing a (baby) "Dean", John Sclater. Much of the intellectual vigor of the program then vanished (historically, serious MIT programs run at the Department level). A concomitant issue, that further weakened the JP, was WHOI's refusal to appoint a proper faculty. Deans cannot function without a working faculty—which exist almost universally around the world in all educational institutions—for good reasons. It is an attempt to operate a serious PhD program as though all research people, largely self-selected, were the equivalent of properly chosen persons devoted to the teaching programs. The problem became even worse when Joe Pedlosky retired at WHOI as the Doherty Professor, and with its funds being broken into pieces, the last individual properly titled and compensated as "professor" disappeared from the scene.[7] With the intellectual and practical changes that have taken place in physical and related oceanography over the past decades, a focus for planning and understanding has gone away, and I find it difficult to be optimistic about the intellectual future of a program increasingly becoming an arm of WHOI—which is not an educational institution. None of the numerous WHOI Directors has ever taken a serious interest in the operation and health of the JP. But I will stop.

Later Career.

"Old age is a shipwreck." Charles DeGaulle

After I spent a year as the Eastman Professor in Oxford England (2011-2012) I had turned

[7] Prior to Joe's arrival at WHOI, Gabriel Csanady, a deeply experienced faculty member and Department Chairman previously at the U. Waterloo(?) had served as a voice of reason and knowledge. There were a few other experienced previous university faculty in the ranks.

70 years old, and while still deeply interested in science, it did seem to me that I was much less energetic than I used to be. It seemed only fair to not pretend that I could do as much as I had been doing as a faculty member, and I started to consider partial retirement. (I wanted to retire before people started to say behind my back that I should.) I asked the then-Department head about moving to half-time (at MIT it would have been 49% as the benefits packages would then not apply). To my surprise he said "no"—he wanted me to retire completely so that he would obtain control of the Cecil and Ida Green Chair that I held. Instead, he offered me a 5-year 20% academic appointment.

Simultaneously, two of my outstanding former students, Peter Huybers and Eli Tziperman, had become Harvard faculty members, and suggested that I should move to Harvard as a visiting professor. That was extremely attractive—not only would they both be my colleagues, but the Harvard Earth and Planetary Science Department was in walking distance of our Cambridge house, and I could do whatever I wanted: teaching, writing proposals, etc. That appointment, from my point of view, has been extremely favorable and has now been renewed over many years. The irony of Harvard becoming a kind of refuge from MIT, when MIT had been a refuge for Henry Stommel from Harvard—leading to my own career, has not been lost on me. All I will say here is that Harvard is a very different institution than it was in Hank's day (the people and their personalities are entirely different). I did also accept the partial appointment at MIT where I still retain an office (2021), and I have left my external funding there. As of about 2018, I have taken no more post-docs or students. Residual external funding will end on 1 December 2021.

2 Family

In 1970 I married Marjory Markel, who I had known since we had met years before in Westport Connecticut, where her parents had also moved from Brooklyn and who became friendly with mine. We have two children, Jared, born 1971 and Hannah, born 1975. Before marriage, I had given no thought to what having children would mean. What I learned was that they were going to be the most important element of all in my life, and I finally understood the old phrase "to give hostages to fortune." As of this writing (2020) Jared is Professor of Mathematics at Northwestern University and is married to Jennifer Mattson. They have two daughters, Nora, 14, and Harriet, 9. Hannah is Professor of Aneesthesiology and 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. Jared became the pure mathematician for which I had concluded I lacked the talent. Hannah and I have enjoyed discussing the parallel difficulties of medical and ocean/climate research, particularly for long time-scale diagnoses. She is also intensely interested in the history of medicine and she thus carries out another career path I did not follow.

Marjory is an author/illustrator of several children's books, and has done magazine and other illustrations. She has a bachelor's degree in English from Cornell University, a master of architecture, and of education, both from Harvard; she also studied at the School of the Museum of Fine Arts. In recent years, she has focussed on oil painting of portraits and landscapes.

My only real hobbies have been science, my family, reading, and sailing. Much of our family life revolved around summers in Woods Hole where, particularly through the MBL, many activities for children were available, plus an extremely safe beach and an intense social scene. When I was a graduate student, before marriage, I had bought a 24-foot teak sailboat, (a Frisco Flyer, called Feather) moored in Little Harbor, with access through a dock belonging to a house MIT rented for many years for use of Joint Program faculty and students. In later years, MIT subsidized its Joint Program faculty to spend summers in Falmouth and we took full advantage of that.

Marjory was an unenthusiastic sailor, and after we had small children, I sold the boat. But then Frank Press approached me and asked if we were interested in buying a half-share in the fiberglass boat he and his wife Billie owned and kept in Falmouth harbor. (They owned a summer house on Beccles Road in Falmouth.) That first boat (called Frilla, for Frank, Billie, children Bill, Paula) was one they had owned while living in Pasadena when Frank was at Cal Tech and they had had it shipped across the country. As a good southern California boat, it was most suitable to light airs. We split the costs and sailing times, often sailing together as families. When one of Frank's neighbors offered to sell him his 28' sloop, a much heavier Pearson, we didn't hesitate, and shared that boat (we renamed it Pangea) for many years. We finally sold it years later when Frank realized he was getting in very little sailing time, and my own primary crew (my son Jared) had grown up, moved away, gotten married, and started his own family. Jared and I, a few times, happily leased a boat on the coast of Maine for a week at a time.

As a family with young children we spent two very pleasant and interesting sabbaticals in Cambridge UK. The second time when Jared was 10 and Hannah 6 years old, they both went to the private ("public" in UK-speak) Kings College School. Jared discovered mathematics there and Hannah, despite a rocky start (she was the youngest in a school that began in the 2nd grade and was just coming out of Kindergarten), became a life-long anglophile and didn't want to leave. Later, after the children had left home, Marjory and I had leaves/sabbaticals in Princeton Un., Cal. Tech, Un. College London and the Southampton Oceanographic Centre, Toulouse (CNRS/CNES), and Oxford Un.

3 Science

Internal Waves/Time Series Analysis

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 had suggested that inasmuch as the nascent Buoy Group in Woods Hole was struggling to obtain time series data from moorings, that I might usefully try to make temperature measurements using the island of Bermuda as a platform. The suggestion had a history:

(1) In the early 1950s, Stommel had the idea of building an "observatory" on Bermuda—which was within easy flying distance of Boston or Cape Cod (at that time WHOI operated a four-engine aircraft, DC4, of its own out of Otis Air Force Base). Apart from some globally scattered tide gauges, almost no oceanic time series data existed. Using a small vessel, one could reach deep water in a few hours, rather than the days required when sailing from a US east coast port. In addition, the Bermuda Biological Station provided both a base of operations (they operated the R/V Panulirus) and a place to stay. Stommel ran a cable across the beach along the sea floor into deep water and to which two temperature sensors were attached; started a monthly series of hydrographic stations (the "Panulirus station"); reinforced the operations of the local tide gauge; and launched drifters tracked by radar from the island. More information can be found in Stommel's collected papers. The cable data represented two time series, and Stommel turned to Walter Munk and Bernhard Haurwitz for analysis help (Haurwitz, Stommel and Munk, 1959)[8]. Stommel had great difficulty both in maintaining the operations, and with interpreting the data, and he abandoned the observatory. (The Panulirus station continued, at shortened intervals of two weeks, and continues today as "Station S" named for Stommel.).

(2) Stommel wrote a number of exhortative papers urging that "a few good engineers" should get involved with oceanography as he regarded the observational effort as wholly inadequate. He had connected with a group of former Apollo Program engineers at the MIT Instrumentation Laboratory (then part of the MIT Department of Aeronautics; during the Vietnam War upehavals, it split off from MIT as the C. S. 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 apparently draconian engineering rules of working with NASA, for the laissez-faire of the Navy's ONR. Stommel introduced me to the group, helped me obtain my first ONR contract (funding methods were more informal then), and we were in business.

At the time, the WHOI Buoy Group, under the new direction of Nicholas Fofonoff and Ferris Webster, was having difficulties in obtaining data from surface-moored buoys. Conversations,

[8] A bit of this story is covered in Wunsch (2021).

involving particularly John Dahlen and Henry Stommel, led us to propose running a cable *horizontally* out from Bermuda. If that could be done, two advantages would be obtained relative to Stommel's previous cable: escape from the complexities of the bottom boundary layer, and having to analyze the relationship between two thermistors separated both vertically and horizontally.

In a considerable engineering feat, using the rather primitive vessels available at Bermuda (including a barge chartered from Alpine Geophysical), the installation was a success and some interesting data came out of it (Wunsch and Dahlen, 1974).

At the time too, the WHOI current meters did not measure either temperature or pressure (the latter being essential on moorings with flotation below the surface, as they were prone to lying-over in high currents.) The Draper-MIT temperature/pressure (T/P) recorder was at one time in wide use (Wunsch and 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 Klaus Hasselmann/-WHOI Internal Wave Experiment (IWEX; see Briscoe, 1975).

At Sea Work—Bermuda and Elsewhere

More generally, I became interested in the potential that Bermuda acted as a region of stirring and mixing of the ocean. Some biological speculation about the effects did exist. I had obtained two-weeks of shiptime on the then comparatively new R/V Atlantis II. That a new post-doc, a month or two after PhD, could become Chief Scientist on a major oceanographic vessel with a crew of about 30 and a scientific party of similar size, is essentially unheard of today. With hindsight, it was probably the scariest of all my undertakings, involving getting a science party together (there were no ship's technicians), borrowing lab tables et al., STDs, and inviting enough others on board so that we could use the ship 24 hours/day. As we were getting ready to leave Woods Hole for Bermuda, Hank Stommel came aboard and said to me, "Just remember you can't come back early.", a reminder that shiptime 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 "finestructure") in moving towards the island. I returned to Bermuda to do a better job on the marginal, but cheap, WHOI vessel, the R/V Gosnold,[9] but as the instrumentation improved, others have taken up the study of mixing near islands, seamounts, boundaries more generally

[9] The Gosnold was a converted *Army* freighter, designed to operate on inland waterways. It had a top speed of about 7 knots, and its Captain, Harry Seibert, lived in mortal fear of hurricanes.

With my students we carried out a number of internal wave-related and mixing field programs, including observations near Muir Seamount, in Hudson Canyon, on the US continental slope, the flow past an equatorial island (Jarvis I.), Lake Kivu, and too many others to list here.

Boundary Mixing and Internal Waves

In the meantime, I had become more generally interested in and worried about the influence of the huge Bermuda platform on the phenomena we were studying. That led me to concern about how internal waves were influenced by the strongly sloping bottom topography. In my first attempt (Wunsch, 1968) I supposed the waves would reflect from the slope before reaching the apex. When, with the help of the fluid laboratory technician in Woods Hole (Bob Frazell), we tried to reproduce the waves in a wave 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 me to a second paper (1969) which emphasized the role of the "critical" slope.

At that time, only a handful of oceanographers were interested in internal waves and topography. They included Peter Baines, who came to MIT as a post-doc, and who provided the first serious calculation of how topography would generate an internal tide; Hal Sandstrom, originally a student of Chip Cox at SIO; Fritz Schott then in Kiel; and two Australian theoreticians (R. M. Robinson and D. G.. Hurley). When Seelye Martin came to MIT to work as a post-doc with Erik Mollo-Christensen, we used the bottom wedge as an absorber to prevent internal wave reflections as we were studying non-linear resonant internal wave interactions in the large current meter towing tank in Woods Hole (Martin et al., 1972). Forty+ years later, internal waves on slopes have emerged as a small industry and I find it is now almost impossible to keep up with the field.

About 1969, for a few months I visited Stewart Turner, whom I had known at Woods Hole, and who had moved to DAMTP U. of Cambridge. There I started thinking about the influence of a sloping boundary on a time-mean stratified fluid and worked out the mathematics of the upwelling boundary layer along the slope. Stewart immediately recongized the similarity to a solution that his Australian compatriot Owen Phillips (then at Johns Hopkins Un.) had produced. Owen and I exchanged papers and they were printed in successive issues of Deep-Sea Research (even though Phillips's original motivation was primarily in low Prandtl number metallic flows in the solid Earth).

Easily the most influential internal wave paper of the 20th Century was that of Garrett and Munk (1972) and its successors. They succeeded in transforming a literature of "wiggily lines" (e.g., that had defeated Ekman) into a highly sophisticated, quantitative wave science. They showed that at least to a crude approximation, the internal wave field, lying between the buoyancy frequency N, and the local inertial frequency f, had a number of surprisingly nearuniversal characteristics (energy levels, isotropy, steadiness,...). I made some effort to test the limits of the universality assumptions (which cannot be rigorously true as long as the inevritable sources and sinks exist) using a variety of observed time series of the 1970s. That the GM (as it is known) spectra (more than one has been proposed) have a very useful basic descriptive accuracy is not in doubt. Most of the subsequent discussion of GM physics involved nonlinear wave interactions in wavenumber space—leading to some very difficult continuum theories almost impossible to test with field data using available instruments. I mainly drifted out of the field, returning occasionally, into eddies, the global scale circulation, and related problems.

Inverse Methods/Levels-of-no-motion

In the 1970s it became widely known that L. Valentine (Val) Worthington, who had been trying for many years to piece together a synthesis of the North Atlantic Ocean time-average circulation from hydrographic data had finally concluded that the circulation had to violate geostrophic balance. The presumption then (as still common today) was that the long ship-based hydrographic sections did represent some sort of vaguely defined time-mean circulation. To publish his synthesis, Worthington had written a book, about to appear in the Johns Hopkins Un. Press oceanographic series. Just before the book was published, it was announced that Worthington would summarize his estimate of the circulation at the weekly Tuesday afternoon seminar in Woods Hole. 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 etc., he attracted a good crowd to the "case of whisky problem."

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—violating Newton's Laws—struck me as impossible and I thought it would be interesting to tackle the problem, without really having a clear idea about 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 pair 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 assumed no horizontal flow (a "level-of-no-[horizontal]-motion"). To a great extent, the practice was so deeply embedded in the science, it was commonly forgotten that it was nothing but a convenient assumption. The older textbooks (Sverdrup et al., Defant, 1961, etc.) did have sections pointing at the difficulty with the assumption, without offering a solution.

Figure 9: From Worthington's case of miniatures as the reward for solving the Bermuda triangle problem. {whisky_bottle

All I did was write down algebraically the requirement of mass and temperature conservation in the triangle, letting the reference level velocities (the putative level-of-no-motion) be algebraic unknowns in each station pair Despite dividing the sections into numerous layers with conservation requirements in each layer, I had more unknowns than knowns. This situation 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 claimed to be able to deal with such situations in the presence of noise. I then started to read the various papers by Backus and Gilbert, written together and separately. Backus, being a good applied mathematician, had begun proving theorems in functional analysis and I was struggling. So one day I telephoned my remarkable MIT geophysics colleague Theodore (Ted) Madden, briefly explained what I was trying to do, and he quickly said, "Oh don't read that. Read Ralph Wiggins's paper in Reviews of Geophysics." What Ralph had done (he had been my co-graduate student on Vela Uniform) was to rewrite the whole business in discrete space and point at the very readable textbook by Cornelius Lanczos. Between Lanczos and Wiggins, the mathematics became far more straightforward (linear algebra) and I proceeded. It turned the Worthington conclusion on its head: It was not that there were no solutions, there were an infinite number, represented in the"nullspace" 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 (years later I could have made it much simpler) and it seemed well-received. At the end, Val stood up, said he hadn't understood anything I had said but that "his friends had told me I had solved the problem" and he presented me with a case of whisky miniatures! (Some of the wider reception was quite hostile, as linear algebra was not within the toolbox of most of the seagoing community.)

Coincidentally, Hank Stommel, who was on sabbatical in Germany working with Fritz Schott, had produced the so-called beta-spiral method for finding the reference level velocity at a point. So at that time, we had two very different appearing methods for solving the classical problem. But very quickly Russ Davis (SIO) showed that they were identical in using the same underlying equations, but with different weighting schemes. Although the level-of-no-motion still has not disappeared from oceanography, 40 years later probably most oceanographers would recognize that it doesn't exist. Looking back,I recognized that a form of the method I had introduced had been used by the eminent Japanese oceanographer Koji Hidaka (1940). But Defant had shown that Hidaka's equations were ill-conditioned (he wrote them as just N-equations in N-unknowns), killing off that direction (also WWII inhibited scientific communication). In any case, the method was impractical until digital computers were available and the mathematical machnery of ill-posed problems was understood.

More Box Inversions

Although he had been my adviser, I only ever wrote one paper, years later, with Henry Stommel. But we talked all the time, and I think inspired each other. He and two colleagues (Ants Leetmaa and Pearn (Peter) Niiler 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 average Atlantic at about 1000[?]m. In the context of the box inversions, that struck me as exceedingly unlikely; it also implied a very small North Atlantic meridional heat flux, something in which Stommel had also become interested. Dean Roemmich and I (Wunsch and Roemmich, 1985), using our two recent hydrographic lines (see below), showed that the demonstration was closer to being an assumption than a demonstration and that Sverdrup balance did not apply to any single hydrographic sections (whether it would apply in a true average was a separate question—intractable at the time). 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 my first sabbatical at Cambridge University, visiting Herbert Huppert and Adrian Gill, 1973-4, I was trying to understand non-tidal sea level variations—as those were the only extant long (years) oceanic records. I was troubled by a finding of the late oceanographer Gordon Groves (1955, who had been a Munk student years before), of a seemingly bizarre spectral peak in sea level at Canton Island in the Pacific Ocean. It was so strong, that it seemed quixotic to proceed without being able to understand it. In discussions I had with Adrian, we gradually came to believe 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 (Yanai, Dennis Moore, Mark Cane, James Lighthill, and others). But the idea that they would be visible in sea level had apparently never occurred to anyone. With the help of Huppert's computer programmer, Joyce Wheeler, we analyzed the records I had been collecting, and constructed a theory showing that the sea level signature was large enough to be measured by a tide gauge (Wunsch and Gill, 1976).

That linear mode theory appeared to actually describe much of the variability of the equatorial ocean got me interested in better understanding what was going on. Bruce Taft (NOAA) organized the Aries Expedition out of SIO on a transect from Tahiti to Hawaii (Taft et al., 1974). My own particular focus on that cruise, as an extension of the Bermuda work, was to understand the influence of an equatorial island on the essentially non-rotating flow past it—both in the time mean and its variability. We did a CTD survey of Jarvis Island, and which led Ross Hendry, then a student, to a nice theoretical discussion of non-rotating baroclinic shear flow past an island (Hendry and Wunsch, 1973) relying heavily on a theory by Phil Drazin. The current meters we had deployed there, and subsequently recovered, did not prove to have particularly interesting records.

My focus then shifted to the Indian Ocean, where we deployed a number of pressure gauges in the Seychelles, using a small motor yacht chartered out of Mahi, the capital. But more significant, in a cruise on the R/V Atlantis out of Mombasa, in which I participated, Jim Luyten and John Swallow using Bill Schmitz's profiler (the "White Horse") had discovered the deep equatorial jets. I had brought with me on board the ship for light reading a monograph by Jim Holton (1975) describing the theory and observations of atmospheric tropical motions. I was inspired, to try and explain the motions as the downward propagation of monsoonally (annual) forced Rossby waves in an infinitely deep ocean of infinite breadth (Wunsch, 1977b). Although the mechanism does seem relevant, Jay McCreary subsequently pointed out that the energy-containing rays in annual Rossby waves propagated at a sufficiently shallow angle in the vertical that they would encounter the sidewalls long before penetrating to abyssal depths and my solution was not obviously applicable. That was the only time I tried to do serious scientific calculations while on shipboard—like a lot of people, my brain never seemed to be working at 100% capacity at sea.

At that time, because of the growing interest in El Niño, the rise of the TOGA program etc., the field was getting crowded and it seemed sensible to focus on something else. I left equatorial problems to my student, Charles Eriksen.

End of the Classical Period: Mid-Ocean Dynamics Experiment (MODE) 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

Figure 10: The MODE Executive Committee. Clockwise from left: Henry Stommel, Nick Fofonoff, CW, Francis Bretherton, Allen Robinson (back turned). From The Turbulent Ocean (MIT copyright). {mode_steering

to unravel. In his 15 years as an MIT Professor, Hank Stommel was interested in harnessing the ingenuity of the hundreds of engineers that he was surrounded by there. He proposed (see Wunsch and Ferrari, 2018 for more details) that there should be a collective effort to exploit the new time series measuring technologies that had finally come to fruition. The WHOI Buoy Group was, for example, finally able to obtain time-series from both surface and sub-surface moorings extending over several months. W. Munk, D. J. Baker, and others had been perfecting ocean bottom pressure gauges; CTDs were almost commonplace, J. Swallow, T. Rossby and others had developed neutrally buoyant float technologies, etc. and the modeling community was advancing with new computer power. The intellectual context was 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 UK (Crease, 1962) and as interpreted by Norman Phillips (also MIT).

The upshot (see The MODE Group, 1978; Robinson, 1983) was an Anglo-American 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 MODE and its troubled US-Soviet POLYMODE successor (see Collins and Heinmiller, 1989), was the powerful indication that the ocean was filled with an eddy-field (misnamed "mesoscale" eddies, but a label difficult to suppress) that likely dominated the kinetic energy of the ocean. That inference suddenly began to undermine 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: ones including Sverdrup balance, the Stommel-Arons abyssal flows, abyssal recipes, steady Ekman layers, et al. 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 each location (the mooring and moored instrument technology having again advanced) to understand what was "typical," if anything, in the global field. It was clear, however, that given the resources available, that decades would be required to obtain such records representative globally and even then the question existed as to whether one or two years at a location would be adequate? Float tracking had to be done by ship, or by expensive moored acoustic transponders, again producing very short temporal records. (For a longer account see Wunsch, 1981, 2006).[10]

What to do?

Altimeters

(Some of this section was originally written for a CNES volume on the history of TOPEX/POSEIDON and other Earth-looking satellites. To my knowledge, it has never been published except as an online supplement to my Ann Rev of Mar Science memoir.)

I first encountered the notion of altimetry when reluctantly serving as the physical oceanographer on the Committee on Earth Sciences of the Space Science Board of the US National Research Council sometime around 1975. Most of the oceanographic focus with NASA at that time was centered on SEASAT-A—planned for launch a few years down the road—and carrying prototypes of nearly every instrument that could be conceived of as measuring an oceanic property. The sketchy documentation the Committee was given said very little about how the data would be used, or how accurate and precise it would need to be so as to be scientifically useful, or what the space-time sampling problems would be. (The scientific background had been discussed in conferences, notably one at WHOI and the 1969 Williamstown meeting; see Kaula, 1970; Stan Wilson unpublished, 2018.) As everyone knew, measurable properties from space (temperature, color, waves) were effectively confined to the surface ocean—the most complex of all marine regimes. But the altimeter stood out as exceptional—we knew enough about ocean physics to realize that the ocean surface elevation, on sufficiently large spatial scales (much larger than ordinary surface waves) was determined by motions within the interior, and in some cases, motions all the way to the sea bottom. An analogy existed with the meteorological use

[10] One of the more sobering outcomes of MODE and its preliminary work was the inescapable conclusion (John Gould and others) that current meters mounted on taut surface moorings responded to the vertical vibration-like motion, greatly increasing the spectral level of velocity at all frequencies. That led to a decision to use only sub-surface moorings (and there was a subsequent, in POLYMODE, heated debate with the Russians who could *only* use surface moorings, not having working acoustic releases). Along the way, a well-known WHOI biologist took the occasion to tell the world (including NSF) that physical oceanographers were just incompetent. It does carry a serious lesson for scientists to be sure they understand their instruments sufficiently well to draw serious conclusions.

of atmospheric surface pressure: a skilled meteorologist could look at surface pressure maps and make numerous inferences about what had to be happening farther aloft and how it might evolve through time—which forecasts could be checked. Could we have an analogous tool and could we interpret the results? If so, we would suddenly have global coverage of a true dynamical variable (surface elevation pressure) every few days.

So what could be done with the SEASAT altimeter data? While awaiting launch (1978), I was approached by Dr. Michael Gaposchkin at the Harvard-Smithsonian Astrophysical Observatory—a well-known geodesist who had heard that I had acquired an interest in satellite altimetry. Mike understood that even comparatively crude altimetric measurements could produce an estimate of the marine geoid that was far better than anything the science community had. He also recognized that the ocean circulation was, from his point of view, a potentially serious noise contaminant, but for me it was a hypothetical signal of major importance. The SEASAT measurements lasted only three months, but that was sufficiently long for us to write a review article (Wunsch and Gaposchkin, 1980) using real data, and describing both geoid improvement and the tantalizing presence of oceanographic signals.

In early 1980, Charlie Yamarone of JPL found me in the basement of Pierce Hall, Harvard, where I was hiding from the Chairman's duties at MIT. Charlie was the engineering head of what had originally been SEASAT-B (the follow-on), but was now to be focussed on altimetric observations. He was looking for an oceanographer at least willing to talk to him, and who might, with any luck, be willing to help put together a Science Working Group for the Project. By that time, I had developed some enthusiasm for an altimeter—but one whose accuracies and precisions would exceed that of SEASAT. Bob Stewart[11] was the Project Scientist, and he, Charlie and I, along with input from many people, put together a Science 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 electron content and atmospheric water vapor corrections, calibration, etc. as well as all of the engineering expertise required not only to create a working satellite, but one which also could survive both launch and several years in orbit). As one might expect, the earliest discussions of the TOPEX Science Working Group had a large component of mutual teaching, translation of terminology, and understanding of everyone else's issues.

[11] There were two Robert (Bob) Stewarts active in physical oceanography at that time. The Bob Stewart of altimetry was at SIO and JPL. The other Bob Stewart, primarily a theoretician, was an important oceanographic figure, and active in Canadian science. The SIO Bob Stewart, being much the younger one, was almost universally known as "Stewart the less" and he even referred to himself that way.

Figure 11: A meeting in Washington DC, 1980, of the original TOPEX Working Group. From left to right: Fritz Schott, CW, Jim Marsh, George Born, and a bit of Joe Reid. Photo by Robert Stewart. {bobstewartpho

The Science Working Group issued a report via JPL in 1981 that concluded a high accuracy US altimetric mission was both a feasible engineering goal, and a potentially revolutionary scientific instrument. "Selling" the project to the wider US scientific oceanographic community was, however, painful. Boiling it down, one well-known WHOI scientist told me "I'd much rather have another ship," and another "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 possible flight of TOPEX/POSEIDON to help justify the field programs of WOCE—an unusual opportunity to supplement the in situ measurements with a true global data set; we also used WOCE to justify the flight of TOPEX/POSEIDON—an unusual opportunity for a NASA/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 (formerly merchant marine) Michel Lefebvre, who beginning with his involvement in the French altimeter, POSEIDON, became a shrewd, enthusiastic proponent of the global supporting oceanographic program. In parallel, we were also working with ESA, which was planning to fly ERS-1, with a somewhat less accurate altimeter. We wanted the ERS-altimeter data to be as useful as possible. With encouragement from WOCE and TOPEX/POSEIDON organizers, we did urge ESA to put the spacecraft into a "geodetic orbit" (having a dense, but slow, coverage of the entire ocean) for 6 months. Although the ERS-1 altimeter system was less capable than that of TOPEX/POSEIDON, the geodetic measurements were good enough to finally lay to rest any idea that the shape of the Earth could be a national military secret as the US had been insisting.

The extended period from the sketch design described in the TOPEX report, to actual launch and distribution of the first data sets was a considerable saga ranging from the decision to collaborate with the French POSEIDON project (well-described by Stan Wilson in the French volume); long debates about the orbit; and including threats from what was then called the Defense Mapping Agency to classify all of the data (there had been a classified, but low quality, military altimetric mission GEOSAT); the need for NASA/CNES to determine an unclassified geoid; and the connection to WOCE. Numerous crises occurred prior to launch including a NASA demand that the spacecraft should be recoverable; an announcement that the batteries would fail within a few months; etc. 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.

As is almost always the case when a new instrument opens up measurements on previously unobserved time and space scales, there were some "surprises" that emerged from the TOPEX/-POSEIDON data. Amongst them I would list (1) the discovery of the ubiquity and intensity of the internal tides; (2) recognition of the strong barotropic fluctuations existing at high latitudes; (3) that the data were sufficiently precise to discuss global sea level changes.

Acoustic Tomography

The other major technology I was involved in was also an accident of serendipity. As a member of Jason, I went (1975?) to La Jolla for a three-week summer study directed at *non-acoustic* anti-submarine warfare. Walter Munk, who could be very persuasive, convinced me that there really 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. What happened, as we hadn't seen each other for an extended period of several years (both of us were, however, involved in MODE), was that we were sharing an office in a room at the Bishop's School in La Jolla, and we were simply catching up on what each of had been doing 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 mentioned above.

As we gossiped, it dawned on us that if we put together what Walter knew about acoustics (I knew almost nothing), with what I knew about inverse methods generally (he knew almost nothing) that we could make an interesting observing system, particularly at long ranges. We proceeded to work out the details of range, processing, etc. After a few days, the Jason Director, Richard (Dick) Garwin, wandered in to ask what we were doing? After we explained, he said "You've just reinvented [medical] tomography" and the first written account is by Garwin, Munk, Wunsch in a Jason (unclassified) technical report. Walter and I went on to try and make it practical based upon collaboration with Worcester, and a whole series colleagues from acoustics and engineering and oceanography. The book, Munk et al. (1995), attempted a summary as of that date.

When Munk and I shared an office in DAMTP on a joint sabbatical in Cambridge UK, 1981-82, I had been asked to participate in a Royal Society conference on observing the oceans in 1990s. We took the opportunity to write a paper with that title emphasizing the new technologies that had or would become available by the 1990s (Munk and Wunsch, 1982). We did emphasize tomography relative to altimetry etc., because altimetry required getting a recalcitrant space agency to approve a multi-$100million expenditure, while tomography appeared to be much more in our own hands— a small group of working, sea-going, scientists. Although tomography has greatly progressed (see Howe et al., 2019) it has not come into the widespread use we had anticipated. I will say a bit more about that later on.

World Ocean Circulation Experiment (WOCE)

I have described the origins of WOCE at some length elsewhere (Wunsch, 2006). In summary, it was a response to what appeared to be an intellectual crisis in the field following discovery of the intense, rapidly time-evolving, balanced eddy field that dominated oceanic kinetic energy. Given the intense eddy field, was it possible to understand the behavior of the global system?

In 1981, my former student, Dean Roemmich, and I, inspired by the power of the box inverse method, had carried out the first trans-North Atlantic, top-to-bottom hydrographic sections that had been done since the International Geophysical Year (IGY, 1957-1958). In repeating two of the IGY sections, our intent had been to space the stations for the first-time so as to have "eddy resolution". I was chief scientist on the R/V Knorr for the first crossing 36°N , Woods Hole to Cadiz, and Dean was chief scientist on the return leg further south (24°N). I returned from that first leg in a state of frustration: I knew that the ocean on the western side had changed by the time we arrived near Spain a month later; and we had spent endless hours dealing with a recalcitrant winch and conducting cable—in a 19th century technology. Dean and I wrote up the differences from what had been observed in the IGY, but we entitled our paper "Apparent Differences...", as the calibration offsets between the old Nansen bottle sections and the new ones were obscure to us. It all just confirmed my sense that as physical oceanographers we needed a new approach both in technology and sampling strategies. [Charney report]

The first proposal for what eventually became known as WOCE was made, I think, by me in 1979 at a meeting in Miami of an international panel 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 in 1992, continuing nominally for about 5 years at sea. Altimetric measurements have continued since then; the community finally obtained wind-scatterometer measurements from space; gravity missions (GRACE, GOCE); and the Argo program that arose out of the WOCE ALACE float experiment (Davis et al., 2001). A further discussion of this major international effort is left to the references above.

Classical and Modern Eras

MODE, the altimeters, and other quasi-global nearly synoptic data sets such as those from Argo led to a qualitative change in theoretical and observational understanding of the ocean: what I have elsewhere (Wunsch,2021, unpublished) called the “classical period” encompassed the old textbook picture of an essentially laminar, very slowly changing ocean—it was a picture driven by the observational capability. The very long time-scales of oceanic integration of scalars led to the beautiful atlas pictures of temperature, salinity, and quantities such as the oxygen distribution. These pictures were rationalized by the equally beautiful laminar theories that emerged following the Second World War in the Sverdrup-Stommel-Munk analytic theories and the decades of work that followed.

With the emergence of the observational picture of a turbulent ocean, one with space and time-variability on all measurable scales from ocean basins to millimeters and time scales now of several decades, we have entered the “modern period.” Now theory and observation must account for the rapid variability in space and time, and which might be summarized by saying that oceanographers are faced with a fascinating turbulence closure problem, in which the large-scale properties mapped in the classical period are the result of complicated and interesting turbulence interactions. The extent to which the classical laminar theories will emerge as important when very long-term space-time averages become available is, at this time, unknown.

ECCO

By about 1992, it was evident that some form of WOCE would actually occur—we would have a variety of near-global data sets. They were to be of radically different types and sampling attributes. How could we use them? The best 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 (DA)” of observational atmospheric data into general circulation models at intervals normally of 6 hours—for the purpose of producing useful forecasts out to several days. But the oceanic problem was not (at least then) one of prediction, but of understanding. We were interested in annual and longer time-scales, and wanted to understand how the ocean behaved.

What was clear was that prediction and understanding were distinct goals. I knew from my time-series background that Norbert Wiener and others had distinguished “filtering” (estimate what is happening now) from “prediction” (what a best estimate of the future would be), and "smoothing" (what happened over some finite interval in the past). It seemed obvious that what we needed to know was to adjust the ever-improving oceanic numerical models so that they were consistent with the WOCE observations of all kinds. But that too proved a "hard-sell". At least one well-known DA 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 simply ignored for over a year and then returned unreviewed (from an incompetent program manager who I will not name).

In the early 1990s, Jochem Marotzke arrived as a post-doc from Kiel. We set out to demonstrate that we could adjust full-blown oceanic general circulation model (GCM) to be consistent with various data types. Carlisle Thacker (NOAA) had shown that a model adjoint could be used efficiently in fitting a complex numerical model to observations. Marotzke and I set out to try that in determining the steady-state of the GFDL Princeton GCM. We also experimented (Menemenlis and Wunsch, 1997) with model linearization using numerical Green functions.

In the mid-1990s, at the suggestion of Marotzke, I had taken on another German post-doc, Ralf Giering, who had been a student of Klaus Hasselmann's at Hamburg. His PhD thesis had involved producing a computer code that would take another computer code—that of an oceanic general circulation model (GCM)—and "automatically" produce a third code representing the adjoint of the GCM. The use of adjoints for optimization problems was well-known, but because complicated GCM codes were perpetually being modified and updated, maintaining manually a corresponding adjoint code by hand was a forbidding undertaking.

Coincidentally, John Marshall had arrived as a new faculty member from Imperial College, London, and he was deeply into constructing a new oceanic GCM. With advice and help from Giering, we managed to have the MITgcm (as it became known) always be adjointable—at least semi-automatically. 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 (from Hamburg) taking over the local MIT effort when Stammer left for a faculty position at SIO. As of this writing, ECCO continues outside MIT with the major US centers being at the Jet Propulsion Laboratory (Ichiro Fukumori, et al.), AER (Rui Ponte), and Austin TX (Patrick Heimbach).

Paleoclimate

With the expected successes of WOCE, my own attention had turned toward the global ocean and which was going to give us a quasi-snapshot picture. But understanding what the ocean circulation and its expected changes did to the climate system from global sampling would require a wait of many decades and I did not have sufficient patience for that! Or 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 time- and space-scales. Even the most basic theory (and a few scattered observations such as radiocarbon concentrations) suggested that the ocean circulation changed on time scales ranging from seconds out 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) effects for hundreds to thousands of years.

In the 1990s, apart from extremely sparse, very 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 interacted with climate overall either requires very long, global-scale records, or numerical models with demonstrated skill on long time scales—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 Program, and ice coring. Thus the notion that the paleoclimate record was a 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 story-tellers and stories ("geo-novels," after Harry Hess and his "geo-poetry"). Good novels illuminate human nature, but one doesn't confuse them with history. A separate essay 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, 2006; Huybers and 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 had finally solved the multi-century problem of determining the open ocean (barotropic) tides. When T/P was being planned, I had thought to do that myself, as the tides were going to be by far the largest time-dependent signal. Much of the discussion of the

T/P orbit was driven by the need to make sure the multiple tidal frequencies did not alias into such important low frequencies as 0, the annual and semi-annual etc. With my strong support however, the T/P Science Team had decided to make the data completely public at the same time the Science Team received it. Long before I would have gotten to it, a number of tidal specialists (Lyard, Ray, et al.) had made the first true global tide estimates for several tidal frequencies. That proved to everyone's advantage, as they did an excellent job, and rendered much easier the analysis of the residual—which was my own central interest.

But one of the surprises of the T/P data were the conspicuous presence globally of the internal tides. With hindsight, and the experience of the Wunsch/Gill paper and a review paper of the subject I had published in 1976, I should have anticipated that. In practice, the work of Richard Ray, Gary Egbert, and others 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 collaborating on testing the accuracy of tidal models used to correct altimetry data, and a paper I wrote with Dail Haidvogel and Iskandarani about modeling the long-period tides, I 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, and we managed to be together for several days including a theatre visit with Judith Munk (to the scientifically interesting play *Copenhagen*) and various lunches and dinners. We got to talking about the internal tide, and I recall being struck by the apparent resemblance between the horizontal tidal energy flux, and that of the estimated ocean circulation heat flux. That thought did not lead anywhere in particular, but we started to discuss the tidal dissipation problem which had a history going back to Laplace, Kant, and others and which at that time was widely thought—from the work of G. I. Taylor, H. S. Jeffreys, and W. A. Heiskanen, circa 1918-20—to occur primarily on the shallow continental shelves.

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.[12] We began what became extended discussions of the implication of a powerful internal tide, possibly dissipating as much as 50% of the lunar tidal energy loss. That led us in turn to examine the oceanic mixing implied. We wrote this up (Munk and Wunsch, 1998) a bit tongue in cheek, because (as one colleague said to me) "everyone knows the tides have nothing to do

[12] The modern rate of lunar recession if it has been constant through time implies the moon would have been at the Roche limit (so close to the Earth it would have been torn apart by Earth gravity), one billion years ago. Walter called it the "Gerstenkorn Event." It was known not to have happened.

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.)

Acoustics Again

The ocean is opaque to useful wavelengths of electromagnetic radiation, but it is transparent to sound on many scales, reaching the global at sufficiently low frequencies. One might ask why acoustical methods have not come to dominate the field? This subject is one that I wrote about in a Science "Perspective" in 2020, and I will leave it at that: One could imagine a time when many fluid ocean observations occur by listening to it at a vast variety of frequencies, scales, and places, depicting an extremely wide range of phenomena. Hydrophones are comparatively simple, passive, cheap instruments and can be deployed in small arrays to produce directional beams and wavenumbers.

Walter Munk and I didn't see enough of each other in the years following our joint sabbatical. He pushed hard for the Heard Island experiment, which I stayed completely away from as it didn't seem to have mainly scientific goals, and that (as it proved) would be misleading to the wider community which identified tomography with the resulting global scales. As suspected (see the paper by Dushaw and Menemenlis 2014) on the global scales, the acoustic pathways cover a very great variety of disparate physics, so disparate that understanding changes over such distances is nearly impossible. One wants pathways extending over quasi-homogeneous major fractions of ocean basins, not global ones. It also gave the seriously wrong impression that extremely loud artificial sound sources were required, and which led into the nightmare thicket of permitting hearings and arguments with the community dedicated to protecting marine mammals. (And it jargonized the subject by referring to "acoustic thermometry" which Walter once admitted to me was the result of a misunderstanding on his part.)[13]

More recently, Wu et al. (2020) have shown that under some circumstances, repeating earthquakes can be used to determine oceanic temperature changes. That holds out all sorts of interesting possibilities for determinations not only in the future, but also in the past from historical seismic records, from other natural sources such as whales, unnatural sources such as ships, and the placing of large-numbers cheap hydrophones on Argo-like floats (see the Wunsch, 2020 "perspective").

Much more recently, subject to declining algebraic and other skills, I have been trying to

[13] Naomi Oreskes in her 2021 book *Science on a Mission,* U. Chicago Press, muddles the tomographic history.

answer the question of whether one could hear the internal wave and enormous range of scales in oceanic turbulence. There's issues of mathematical complexity with turbulence, their statistics, and the very strong background noise in some of the frequency bands which extend from microHz to many kilohertz. No conclusions yet. If it works at all, hydrophones are comparatively cheap, can be deployed as beamforming wavenumber arrays etc. Might even work to determine the flow under an ice-covered ocean on an outer solar-system moon.

Tracers

In a turbulent fluid, the gross distribution and movement of properties such as temperature, salt, carbon, silicates etc., becomes of prime interest. Fundamentally, distributions and motions are Lagrangian descriptions of the fluid and any attempt at their direct computation is wholly forbidding—both in terms of specification of initial conditions on the turbulence scales, and then of course, numerical integration on an adequately resolving spatial and temporal grid carried out to the longest time scales that are present.

The quasi-steady-state of the globally mapped scalars, along with the estimates of boundary conditions such as air-sea heat transfers become very difficult to interpret in terms of the dominant physics of advection, diffusion across all of the time and space-scales and physical mechanisms that combine to produce the apparent observed "final" or terminal state. The atmospheric nuclear bomb tests of the World War II years produced some nasty radioactive products (tritium–^{3}H, carbon-14, etc.) which unlike the conventional oceanographic tracers, were heavily transient (carbon-14 does occur naturally as well). The sparse observational capabilities, in the hands of people like Bob Key, Bill Jenkins and others, showed the principle of ocean physics inferences directly from the transient state. One of the major goals of WOCE was to produce appropriate global scale data sets such that the ongoing transients (tritium, with a 12-year half-life, was however, fading rapidly) in carbon, fluorocarbons, ^{3}H/^{3}He, etc. Some of the more energetic WOCE debates surrounded the possibility of measurements of tracers requiring vast volumes for individual samples (see the WOCE volumes for outcomes).

A couple of my students X. Li, and C. Siberlin employed the machinery of inverse methods to make generalizable inferences and I myself wrote a few generic papers on such applications. Perhaps most memorably, Patrick Heimbach and I took one of the later global ECCO solutions, and calculated the gross time-scales required for the ocean to reach full equilibrium under forcing at the surface by a passive tracer. The results (Wunsch and Heimbach, 2008) ranged from a few decades in the northern North Atlantic to 10,000+ years in the mid-depth North Pacific Ocean. The very large range, and large numbers present very special challenges to the interpretation of finite duration instrumental and paleorecords.

Geophysics

I did retain a general interest in solid Earth geophysics. In the early 1970s, part of the geophysical community had become very interested in the problem of how mantle deformation would influence the fluid core, leading in turn to the question of whether the core was stably stratified. An Israeli applied mathematician, I. M. Longman, had produced what became known as "Longman's Paradox." In attempting to find a solution with a stably stratified core, he could not find one without mathematical radial displacement discontinuities at the core-mantle boundary. He thus concluded that the core could not be stably stratified(!). We were living in Cambridge UK at the time, and an international meeting on geophysics was taking place across the street from DAMTP on Silver Street. I went along to the session on the core-mantle coupling problem and listened to all kinds of talks about the stratification problem. It struck me immediately that Longman's Paradox was nothing more than one of the well-known and ancient fluid dynamics paradoxes arising from the discussion of perfect fluids (e.g., D'Alembert's paradox). I reformulated the problem to include a viscous/diffusive boundary layer and showed that continuous solutions were readily available. Later, I added a strong magnetic field, the only time I had used what I learned about magnetohydrodynamics as a graduate student. Tony Dahlen (Princeton) then simplified the problem even further by using Rayleigh friction.

Decades later, stimulated by a discussion by Eli Tziperman and colleagues, I looked at the problem of tides on an ice-covered ocean (the Neo-Proterezoic "snowball Earth"). That led me back to the equations of elasticity as applied to kilometer thick ice sheets. I did have to do a lot of reviewing of books such as Love's classical volume on elasticity, etc. (Wunsch, 2018).

Books,..Teaching.

Over the years I taught the usual suite of physical oceanography courses to graduate students. A few times, I took on undergraduate seminars. I did have a number of undergraduate students working within the UROP (Undergraduate Research Opportunities Program). Notable among them were Spahr Webb and Doug Luther, who went on to have important careers in oceanography (Spahr more on the marine seismic side). Finding projects both accessible and interesting to undergraduates has never been easy, and I suspect it has gotten more difficult with the proliferation of blackbox analysis tools for large data sets.

One important book was the one edited by Bruce Warren and myself (Evolution of Physical Oceanography, 1981) put together as a tribute to Henry Stommel on his 60th birthday and including both personal memoirs and serious serveys of many of Stommel's broad interests.. Bruce and I spent a great deal of time both choosing authors, editing the individual chapters, and making sure that cross-references to relevant chapters were made. Bruce double-checked the accuracy of all the references, and I tried to make a uniformly informative index. Lots of work, but much helped by the neighboring presence of the publisher (MIT Press, with Larry Cohen as our editor). The book did seem to become a mainstay of PO for many years. Some chapters (Warren, Munk, Hendershott, and a few others) have had a persistence lifetime much greater than the average.

I did write a book on The Ocean Circulation Inverse Problem (Cambridge) which went into a second edition for a more general audience as Discrete Inverse and State Estimation Problems. I had been offering an MIT course on inverse methods intended for oceanography grad. students, but I found that much of the class came from other parts of earth sciences and for whom the oceaographic examples I was using were more obstacle to understanding than help. Hence, the second book, but which probably falls between the two stools of being a methods book and one about the application of the methods to the ocean.

I was not very pleased by my interactions with CUP (which underwent very disruptive changes of separating out, and then re-combining the UK and US elements). Thus when approached by Princeton Un. Press for another book, I did go that route and tried to explicate the understanding of the turbulent ocean (Modern Observational Physical Oceanography). I included more than was common about the problems of measurement and sampling. By the time it was completed, I was too exhausted to teach out of it—which should have improved the exposition of many parts. So it is, what it is. (A second edition would be highly desirable, but barring some medical breakthrough, I can't even contemplate starting such a thing.)

Outcomes of careers

Looking back at all of the students, post-docs, young colleagues I have known, I am struck by how difficult it was to predict who would ultimately emerge as an important voice in our field. Some people, who appeared to be potential "stars" at the time of their theses or post-doctoral work, essentially disappeared from the field—presumably doing other useful things of more interest to them. In contrast, were students (mainly) who appeared to be dilettantes, just getting by, with little or no indication of imagination or staying power: and then I would realize 30 years later, that they had proven interesting, extremely productive, colleagues in a way I'd never have expected. I stopped trying to predict e.g., at the time of graduate school admissions, who looked the most promising—it seemed impossible to have any skill at that—being a combination of raw talent (which most had), personality, ambition, and probably, luck. Similar considerations applied to junior faculty—although by then, sometimes one could make a pretty good guess! Many (most) productive scientists do seem to lose "steam" at some point in their careers. I've been lucky enough to have found doing science still interesting as I turn 80 years old, although productivity is markedly reduced.

4 Miscellaneous: About Some People

I think I disappointed Walter Munk on at least two occasions. He got me elected to the Cosmos Club in Washington—a club of movers and shakers of science and politics. It was a nice place to stay on repeated trips to Washington, and I met all kinds of people there. But they didn't admit women—and a few years later, an initial vote to change that rule failed. With little or no prospect for change, I resigned. Later the rule did change and I was offered the opportunity to re-join. But I was hoping to spend less time in Washington, and I was never particularly clubbable, so I declined.

At Walter's behest I did join Jason and it led to our invention of ocean acoustic tomography. This period, the middle 1970s+ was when some of the physics community was "discovering" climate and it looked like just the sort of thing that a "good physicist" could do on his summer vacation. People involved included Bill Nierenberg, Will Happer, and others with a superficial understanding of the problems. Much of it of course, was oceanographic, and I decided I couldn't stomach much of what was going on and resigned from Jason. A book exists (Anne Finkbeiner, 2006, The Jasons : The Secret History of Science's Postwar Elite, Viking) which reflects my views, and I will say no more here. Walter never said anything, but to my knowledge he simply stayed away from the Jason attempts to deal with climate change.

Over the years there were some scientific characters who made me wish I had a novelist's skills. There was Allan Robinson who I always thought would have greatly benefited had he *not* been told from an early age how great he was. He's the only person who ever said to me with a straight face, at one meeting, "You know, I have a lot of integrity." I didn't know how to respond to that. Stommel, when he was at Harvard, could have squelched him, and Allan was one of the people Hank couldn't stand. Nonetheless,Stommel walked away from Harvard, but, strangely, felt guilty about how he had treated Robinson. He was impossible to parse, except that maybe he had a sense of desertion(?). Hank wanted Joe Pedlosky to be his theoretical partner in what became the Mid-Ocean Dynamics Experiment—but Joe, who was then still at the Un. of Chicago, declined and Hank turned to Allan. It was sometimes painful to watch their interactions.

Then there was the geochemist Wally Broecker. I have a correspondence file on Wally: he was a wonderful story teller, but a juvenile personality. He came out of the religious community of Wheaton College, Illinois. He couldn't stand any form of contradiction and publicly drove at least one junior chemical oceanographer (Chen) out of the country, to seek refuge in Taiwan. Endless stories of that kind exist.

Broecker had written a paper using radiocarbon (and he was a true pioneer of oceanic radiocarbon) inferring that there was an enormous upwelling of fluid into the near-surface equatorial-band Atlantic Ocean. I had been doing inverse box calculations in the Atlantic and I thought the values much too large (among other problems, Broecker didn't account for a salinity maximum that the upwelling would have had to somehow cross without disruption). So I set up a box model of the tropical Atlantic, included the radiocarbon data (which was extremely sparse–I think two vertical profiles in the entire equatorial band) and calculated the resulting vertical and horizontal flows. The vertical upwelling value was far smaller than Wally had published. I wrote it up and, as one does, gave the draft to Wally for comment, both as a courtesy, and being aware that perhaps I'd made a mistake. We were both staying at a genteel hotel in Haslemere near Wormley UK at one of the International WOCE meetings at the old IOS. I gave him the ms. one night in the bar, and told him I'd appreciate comments. The next morning I was sitting having breakfast with some colleagues along with numerous gray-haired couples at tables around us. Wally stalked into the dining room, flung the manuscript at me, and shouted so the whole room could hear: "I hope you publish that just as it is, so I can write a devastating reply." He then stomped out despite my attempt to ask what was wrong, left for London, deserting the meeting.

I hadn't yet known him very well at that point, and I was worried that something was really wrong. So back in the US, I asked knowledgeable chemical oceanography colleagues, Bill Jenkins at WHOI and John Edmond at MIT, to please have a look and see if I had blundered. They both said they couldn't see anything wrong. The paper was duly published in JGR, and I kept waiting for the Broecker devastating response—which never came. A few years later, I received a manuscript with one of Wally's former students as lead author, and Wally as a co-author. I was pleased to see they had cited my own radiocarbon paper. But when the paper appeared in print, the citation had simply vanished!

Then there was the time during a US WOCE meeting at the National Academy building in Woods Hole, when he said to the whole Steering Committee (of which I was the Co-Chairman), that I was "not competent" to question his scientific plans. That might well have been true, but I thought that when an established scientist starts claiming that he is beyond questioning that something was really wrong. I knew that Broecker had been ill and had had brain surgery. So after the session broke up, I went to find Bill Jenkins at WHOI, explained what had happened, and asked if Wally was again sick? Bill just laughed and said that far from being sick, Wally was becoming his normal self that I hadn't seen before! Such behavior was a real problem for the field, and particularly for young, not-yet-established scientists. He had acolytes and sycophants; encountering a paper in a reputable journal titled "*Wally was right*" was both indicative and painful. There was much more, but I will stop here.

This period was one when the geochemical community was notorious for its combativeness and remarkably strong public language. I remember one time a colleague asked if I wanted to come along and watch the geochemists fight with each other (was probably a GEOSECS planning meeting). They would say really horrible things to each other (e.g., “nothing that ever came of your lab made any sense.”) Then they’d all go drinking together as great pals. Although he could hold his own in that world, Karl Turekian (Yale) always seemed to be a sensible person and I once asked him where this hostile style came from? He said that it arose from a lack of adequate data so that the loudest most aggressive person carried the day and that he thought it would vanish when the data acquisition problem was solved. And he was right—most geochemical discussions are now much more normal. On the other hand, I think people like Broecker never understood that if he told someone they were an idiot, that it could be taken as a serious criticism—words didn’t have the same meaning in that community that they did in the wider world.

The Turekian story had a supporting sequel. After Karl had died, I was at a National Academy meeting where people were making conventional memorial remarks. One of the people present was a Cal Tech faculty member, call him X, where Turekian had recently been a visitor. X said he and colleagues could hear Turekian yelling at one of their colleagues, Y, down the hall, shouting that everything coming out of the lab was crap (words to that effect) and that Y had never done anything useful. X said that later that day he encountered Turekian and asked him why he kept visiting Y since he obviously didn’t approve of him. Turekian’s response was “What are you talking about? He’s one of my closest friends!”

Then there was Rory Thompson, at one time an MIT student, a sad and ultimately tragic figure. Among many other, and much more dire actions, he disliked that his papers were cited as R. Thompson. So he made up three middle names so that citations became R.O.R.Y. Thompson, and some called him Rory-ory. (The complete Rory Thompson story includes a murder, trials, escapes, capture, suicide documented in Tasmanian newspapers.)

Pierre Welander was a larger-than-life Swede who could light up a room with his presence and wit. One of his many divergent interests was winning at roulette about which he had written an entire book. He had researched it by playing in casinos in Europe and the US. Once I went with him to a casino in the unexpected city of Newcastle, UK. He left me at the roulette wheel to go play something else, came back awhile later and said "Let’s go. They’re cheating." I have no idea what they were doing. I don’t think the book was ever published. Pierre died too young.

5 Change

When I entered the field of physical oceanography in the early 1960s, it was still a small, almost club-like group, one dominated by the at-sea observers such as the Woods Hole people L. Valentine (Val) Worthington, Frederick (Fritz) Fuglister and the prominent SIO people like Joseph (Joe) Reid, along with a number of others in Seattle, Hawaii, and in the UK, France, and Germany. In many ways it was a quiet "academic" pursuit, albeit most the work was done in non-university settings such as Woods Hole and SIO. The technology was still almost completely mechanical, being based on the original bathythermograph and the reversing thermometers used with Nansen bottles. Numerous attempts had been to develop electrical or electronically based instruments, but the physical setting of a sometimes violently moving ship, high pressures, corrosion, the available vacuum tubes, and primitive tape recorders meant that almost no such devices were used in any meaningful way. Navigation was still classical, relying primarily on stars, sun, and moon, sextant measurements and elaborate tables. In a few places (North Atlantic primarily) the World War II development of radar had led to electronic systems such as Loran, but coverage was mostly inadequate.

Much of the attraction of the field lay in the sense that one could know almost everyone working in it, that there were so many obvious problems, both theoretical and observational, that one did not need to feel in competition with anyone else, and the wider public had no particular interest in what we thought the fluid ocean was doing. Indeed the textbook picture—used e.g., to tell biologists and geologists what the fluid ocean meant to their fields, were a weak, steady, laminar, almost unchanging flow. Scientific life was much simpler than it became later. This world began its slow change beginning in the 1960s, when the transistor had appeared, integrated circuits were on the way, and massive computing power slowly became available. (The Stommel-Munk correspondence, 1947-1953 (Wunsch, 2021) gives some flavor of the science of the time.)

Apart from the revolution in pure computer power, the remarkable ability to store previously unimaginable amounts of information and data (petabytes+) has taken getting used to. When PCs first appeared, many of us were in the habit of erasing pdf files at the end of every day to have some space left on our disks—now an irrelevant exercise that has gone the way of slide rules!

Another change, of a very different character, has been the emergence of women in all oceanographic fields. When I started out Elizabeth (Betty) Bunce at Woods Hole seemed to be the only sea-going woman in the field. (Someone once told me that "Betty is the toughest man at WHOI." To do what she did required an energy and determination that ought to be celebrated.)

As one gets older, many fall into the trap of looking back to the time when we were young and with it seeming like a "golden age." I do know that Hank Stommel, as he got older, became deeply nostalgic for oceanography as it was done circa 1950—to the point of even suggesting that WHOI should abandon its educational program with MIT in favor of an apprentice-like system as had operated decades before, and deprecating attempts at global observations. He found funding for small boat operations in the Indian Ocean involving a handful of people some of which (as in Somalia) verged on the dangerous both for political reasons and from reliance on primitive vessels. Much as he enjoyed the work himself, Hank did admit that his observations on the equator based on work out of the Seychelles had missed the major discovery—that of the equatorial jets. These had been found by Luyten and Swallow (19xx) using a modern WHOI ship and all of its personnel and technology.

Fields do change with the times. They go in and out of fashion, mostly dependent upon the appearance of new technologies or new ideas. In the case of physical oceanography, it is difficult to separate the appearance of new ideas from the appearance of new technologies. The culture of physical oceanography has changed beyond recognition. In 1971, when the Journal of Physical Oceanography began, it published only quarterly for several years. The paper authorship in the June 1980 issue has one paper with three authors, three papers with two authors, and the remaining, 14 were sole authored. In the June 2019 issue, none is sole-authored, five have two authors, and the remaining 11 have three or more (two have eight authors). These numbers tell a story of a maturing, now highly collaborative, science that would have been unrecognizable to the ocean scientists of preceding decades.[14]

In my view, physical oceanography has always been almost totally dependent upon the observational capability available in any given era. Theories, to the extent they existed, have tended to follow new observations. Thus in the era of hydrographic exploration, elegant theories of the steady-state (Ekman layers, Sverdrup balance, abyssal recipes, etc.) were generated. Post-MODE, innumerable eddy-turbulence studies have been produced, in the shift from what I have called the classical to the modern eras.

A small number of predictions that apparently preceded observations does exist, but the list of explanations that rapidly followed observation is far longer.[15] Along with many sciences, we

[14] But authorship no longer has any particular meaning. One now sees "honorary" authors, and papers with well over 100-names. How many of these people would take the conventional responsibility for what appears over their names? I've told people who've asked to put my name on a long list of authors that I would insist on reading the paper word-by-word, and make sure I understood all its elements, at least well-enough to explain to an outsider what was meant. That stiuplation has often ended the conversation.

[15] The most frequently cited example is probably the Stommel and Arons prediction of deep western boundary

have entered a period of large collective efforts in which it is not easy for individuals to emerge as intellectual leaders. Many people spend their careers appearing as the 8th or 15th author in ever-lengthening lists, and outsiders cannot determine their contribution. With the growth and diversification, committees have taken to simply counting publications, citations, etc. and even (in my own experience) refusing to consult outside scientists who might actually be familiar with a candidate.[16] The growth of model capability has led to a large fraction of the scientific population focussed on the models per se, comparing them with each other, and rarely if ever asking if any are actually skillful in terms of the observations or providing insights (e.g., the CMIP efforts).

The rise of climate science, and with its essentially instantaneous connection to the public has had an often corrosive effect on quality. Journals such as Nature, Science, and increasingly PNAS, and others, live and die by their ability to obtain media coverage. And of course, many scientists revel in being public figures, sometimes leading both authors and journals to greatly exaggerate the importance or implications of their results. I've sometimes compared these manifestations to those occurring in medicine—with newspapers and tv announcing "breakthroughs" and disasters. Medicine has also been known forever for its quacks and snake-oil salesmen. Parts of "climate science" (whatever that really means) are going down that path.

The maturing and change in physical oceanography generates major problems for academics generally and the Joint Program particularly. A major issue lies with the intrinsic long time scales in the ocean circulation. Although there exists a myriad of high frequency fluid dynamical processes (surface waves, turbulence, all kinds of phenomena related to a rotating stratified system with a vast range of Reynolds numbers), the time scales for understanding the general circulation are extremely long. The appearance of large teams required to obtain useful field data exacerbate the problem. The general circulation has been the centerpiece of the JP, personified by Henry Stommel. But in a world in which it takes several years to develop a field program, and then it may require years or decades of data accumulation to define the phenomenon, how does one establish a reputation and obtain tenure in 5-7 years? The situation in academia at least, is not helped by the rise of modelers, who can produce somewhat interesting outcomes in weeks or months. Perhaps a majority have little or no understanding of the importance and difficulties of the observational base. To some (I report first-hand), data are a trivial use of

currents. That example is justified, albeit anyone looking at Wüst's charts (see Wüst and Defant, 1936) of the property distributions in the mid-depth South Atlantic in the 1920s could readily have inferred the existence of an intense boundary flow—one requiring explanation.

[16] One today sees an increasing number of papers with more than 100 authors. That raises the question of what authorship implies? In the past, it seemed to be understood that an author could vouch for everything that was published, right down to the detailed wording. That responsibiity has vanished.

instruments which somehow appear, and are a "truth", reflecting complete ignorance of noise or bias.

When I started in physical oceanography, "applied mathematics" largely meant fluid dynamics. MIT had such people as C.C. Lin, Alar Toomre, David Benney etc. Harvard had George Carrier, Max Krook, and others. These connections with Mathematics Departments have withered, as applied mathematics has come to mean computer science or deep-learning, or biological problems generally.

One of the side-effects of studying the Earth and its place in the universe is that one comes to live in dual time-streams: that of everyday life in which the human time-span controls events and their importance; and then geological and cosmological time in which everything in the first time stream fades into insignificance. What is the meaning of a great work of science, or literature, or art 10,000 years from now? 1 million years? 100 million years? Sanity requires blocking out the implications of "deep time".

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6 Some Humorous Bits

Over the years I collected a number of quotations, some amusing. Here are a few of the better ones. (A complete list is in quotations.tex on my computer. Many there are interesting or acerbic like this example: “This book will hopelessly confuse a beginning graduate student.and provide nothing to the experienced researcher. Cambridge University Press should withdraw this book as a serious embarrassment. Certainly, no library or individual should buy it.” SIAM Rev., p.369-371, 45, 2003.)

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From Space Studies Bulletin (NRC), Vol. 14, Issue 4, 2004:

"My parole officer assures me that none of the aspects of my background will reflect negatively on the stature of this commitee."

"I am not programmed to respond in that area."

"Person A: ‘Lewis and Clark were not Albert Einstein."

Person B: ‘It’s true. I’m not sure Einstein could ride a horse.’"

"Person A: ‘I’m sorry, I have caused this interruption at a moment when we were confused, but please go ahead and confuse us further.’

Person B: ‘No problem. I have eight more slides."

"He was here last week. We traded lies."

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Groucho Marx said: "Time flies like an arrow; fruit flies like a banana" (website attribution)

The will of John Fuller and wealthy member of Parliament "in the habit of lounging at Faraday's lectures in his old-fashioned blue coat and brass buttons, made a generous donation to the Royal Institution,... [...under the terms of Fuller's will...] They include the following account:....'the feebleness of whose constitution denied him at all other times and places the rest necessary for health could always find repose and even quiet slumber amid the murmuring lectures of the Royal Institution and that in gratitude for the peaceful hours thus snatched from an otherwise restless life he bequeathed to the Royal Institution the magnificent sum of £10,000!'" (from J. M. Thomas, 2007, Faraday and Franklin, in *Proc. Am. Phil. Soc.,* 150(4), 523-541).

"In 1845, Faraday persuaded his nervous, loyal and adulating friend, Charles Wheatstone (professor of natural philosophy at King's College, London) to give a Discourse. The custom was (and remained until very recently) for the speaker at a Discourse and his or her guest to be dined along with the director's guests, in the director's flat befhorehand. The director (Faraday) then took the speaker (on this occasion Wheatstone) to a quiet room where the speaker was able silently to contemplate his talk and performance beforehand. A few minutes before the appointed time of the Discourse, the speaker was 'collected' by the director and taken to the theatre. On the night of Wheatstone's scheduled Discourse, when Faraday went to collect him, he had already fled to his home in the Strand, so that Faraday had to extemporise in his place."

The author (Thomas) adds,

"The custom for a considerable time after the famous Wheatstone incident was for Discourse speakers to be locked in the lecturer's room until the appointed hour (giving a new meaning to Faraday cage!)—until it was forbidden by the Health and Safety at Work Act, 1972." (from J. M. Thomas, 2007, Faraday and Franklin, in *Proc. Am. Phil. Soc.,* 150(4), 523-541).

"Friend: What time is it, Yogi?

Yogi Berra: You mean now?" P. J. Davis, *SIAM News*, 4 April 2008 P. 4

"There's nothing remarkable about it. All one has to do is hit the right keys at the right time and the instrument plays itself." J. S. Bach (from a Cambridge Trust Co. ad. 2008)

"There's nothing wrong with southern California that rise in the ocean level wouldn't cure."

Ross Macdonald. *The Guiness Book of Poisonous Quotes*, p. 320

"Another cackle. Will there ever be an egg?" James Chadwick, as quoted by Robert Oppenheimer (note 41, Ch. 15, of Graham Farmelo, *The Strangest Man).*

"Theories should be treated like mistresses. One should never fall in love with them and they should be discarded when the pleasure they provide is over." From E. C. Friedberg, 2010. *Sydney Brenner, A Biography.* Quoted in book review, **Science** , 331, P. 32.

“Edward Selig [a former Rhodes Scholar] was asked by the Library of Congress whether he was really responsible for writing a book on the seventeenth-century love poet Thomas Carew and a later one on the economic incentives for pollution control. He admitted he was but pointed out the ‘underlying continuity, since both books were essentially concerned with nocturnal emissions.”’ P. 332. P. Ziegler, Legacy. *Cecil Rhodes, The Rhodes Trust and Rhodes Scholarships.*

“...Albert Einstein...attracted capacity audiences to Rhodes House. His subject was relativity, and since he spoke in German and there could not have been more than a dozen people in Oxford capable of following his reasoning even in English, there was a certain lack of rappport between lecturer and listeners. Had it been a good audience, Wylie asked anxiously. ‘Ils ont bien dormi,’ Einstein replied, adding charitably: ‘Ils avaient le droit.’ The Trustees hoped to publish the text of the lectures; Einstein politely refused on the grounds that he had subsequently concluded that all of his theories were wrong.” P. 115, Ziegler.

“Isis contended that ‘the pushful Yanks’ were already renowned for their despoliation of Britain and ‘it seemed folly to invite still more’. Ziegler P. 50

■flocci■nauci■nihili■pilifi■cation, n.

Etymology: < Latin floccī , naucī , nihilī , pilī words signifying ‘at a small price’ or ‘at nothing’ enumerated in a well-known rule of the Eton Latin Grammar + -fication suffix. humorous The action or habit of estimating as worthless. (*OED*)

Three engineering students were gathered together discussing the possible designers of the human body.

One said, “It was a mechanical engineer. Just look at all the joints.”

Another said, “No, it was an electrical engineer. The nervous system has many thousands of electrical connections.”

The last said, “Actually it was a civil engineer. Who else would run a toxic waste pipeline through a recreational area?”

Old joke.

“Lev Landau: Cosmologists are often wrong but never in doubt

Quoted by Narlikar and Burbidge in Facts and Speculations in Cosmology, CUP.

"The author thanks all three anomalous reviewers for their valuable comments." T. Lian, Uncertainty in detecting trend: a new criterion and its applications to global SST *Climate Dynamics*, 2017.

from Chris Garrett:

UVic is planning its response to Covid-19. The president (a lawyer) just sent an email including the following sentence:

"We are also considering how we can contribute further to the significant societal disruptions that are taking place."

Misprints:

Conservation of potential voracity in regions of overflow entertainment.

Infernal gravity waves

Diagnosis of an eddy-resolving Atlantic Ocean model simultation in the vicinity of the Gulf Stream. Part I: Potential voracity

In: Nakamura, M.; Chao, Y. URI: http://hdl.handle.net/2014/14180 Date: 2001

Citation: *Journal of Physical Oceanography,* vol. 31 no. 2 2001 pp. 353-378

Abstract:

Output of an eddy-resolving model of the North Atlantic is diagnosed in the vicinity of the Gulf Stream (GA), using quasigeostrophic potential voracity (QGPV), Ertel's potential voracity (PV), and particle trajectories. Time series of QGPV show strong input of QGPV by the GS in the top 100 m of the model ocean.

"signal underwear" for "signal underwater" in an acoustics manuscript