Chapter 7

Reflections of an Equatorial Oceanographer

DENNIS WILSON MOORE

My career as an oceanographer began in the summer of 1958, after I finished my first undergraduate year at Harvard. I was a math major, but had managed to line up a job with Brackett Hersey at the Woods Hole Oceanographic Institution (WHOI), working on sound transmission data. I spent the summer playing data tapes on a big Ampex tape recorder, driving an analog output device that drew lines on a strip chart recorder. The strip charts were analyzed by using a planimeter to measure the area under the curve, which was proportional to the amount of sound energy received by a hydrophone used to collect the original data.

At the end of the summer I left for Bangkok, where my father was to spend the year teaching at Chulalongkorn University. I was on leave from Harvard, and spent the year teaching English. This included teaching at the Hydrographic Office of the Royal Thai Navy, where they were preparing for the arrival of oceanographers from Scripps to conduct the NAGA expedition. During the year the Office of Naval Research (ONR) liaison from WHOI came through Bangkok, and told me about a WHOI cruise that was taking place in the Mediterranean Sea the following summer.

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I sent a telegram to WHOI and asked to join the cruise. They said OK, and offered me $250 per month, which was what I had earned the summer before. So I left Bangkok in May 1959 and made my way to Athens to join the R/V *Chain* cruise number seven. I got on the *Chain* in early June and got off in Woods Hole in mid-August. Memorable stops along the way included La Spezia, Monaco, Barcelona, and Bermuda. While we were at sea I stood watch in the top lab 8 hours a day, mostly operating the echo sounder and occasionally helping record thermistor chain data. Earl Hays was on the cruise with a new instrument called a sound velocimeter, built by the National Bureau of Standards. I helped him with the sound velocity profiling work and the analysis of the data, which was all done by hand. The WHOI oceanographers had recently realized that by putting a pinger on the bottom of a lowered instrument, and monitoring the profiling from the echo sounder, they could tell how far off the bottom the instrument was. This technique was developed and refined on this cruise. After the *Chain* returned to Woods Hole I lived aboard for another couple of weeks, until it was time to return to Harvard.

I continued my study of mathematics but I was already thinking seriously about being an oceanographer. I applied for a summer fellowship at WHOI for the following summer, and was fortunate to receive one. I was assigned to work with Claes Rooth, and we both sat in Al Woodcock's lab on the second floor of the Bigelow Laboratory. Andy Ingersoll was there as well. He and Woodcock had been doing laboratory studies on the bursting of air bubbles at the sea surface, and the resulting jet formation that expelled small water droplets into the atmosphere. Claes and I were supposed to find a theory for that process, but made little progress. The main reason was that Woodcock's lab was directly across the hall from the small lecture room where the Geophysical Fluid Dynamics (GFD) course was being held for the second time. There were a couple of empty spaces in the room, and Claes arranged for us to occupy them as informal auditors. Louis Howard was the principal lecturer, and other lectures were given by Wilhelm Malkus, Henry (Hank) Stommel, Melvin Stern, and Joanne Malkus . The students included Dick Lindzen, Joe Pedlosky , Bill Holland, and Bob Blandford, among others. For me it was a challenging summer, because I was still an undergraduate and had not yet studied partial differential equations. I learned a lot in a hurry, but not in a very systematic way. Much of what I was exposed to only made sense some years later. A lot of the course focused on thermal convection, and Henry Stommel produced the Great Seal of the GFD course, featuring the dragon with a fire under its tail and a block of ice on its head.

By the end of the summer I was determined to be a theoretical oceanographer. I returned to Harvard for my senior year as a math major, and applied for graduate school. I applied to Harvard in Applied Mathematics, and Scripps in Oceanography. Henry Stommel was on the Harvard faculty at the time, and when I asked his advice he said, "Why don't you stay at Harvard and learn all the applied math George Carrier and his colleagues can teach you?" So I decided to stay at Harvard.

I started graduate school in the fall of 1961 and originally worked with Henry Stommel. One memorable project was a study that I did while taking his oceanography

course the following spring. He was interested in the dynamics of point vortices in a circular ocean basin on a beta plane, and had arranged with Ed Fredkin to help with the computations. Fredkin had a programming business in Maynard, Massachusetts, in the same warehouse with the Digital Equipment Corporation, and he would occasionally get a block of time on one of their machines. The only problem was, it was always midnight until 8 AM, and we did not find out about it until late the night before. Hank would call me around 10 PM, I would drive to Lexington to meet him, and we would proceed to Maynard for a night of computing. We had a lot of fun with this problem. I was able to show that vortices on a beta plane should accelerate near a western boundary and decelerate near an eastern boundary.

In the summer of 1962 I sat in Henry Stommel's lab at WHOI, along with Tommy Rossby, who was doing thermal convection experiments using mercury as the working fluid. Eric Kraus sat across the hall and we often played chess at lunchtime. Early in the summer I saw a notice about a summer school on air–sea interaction that Henry Charnock was organizing at Imperial College, London. I wrote Charnock but got no reply. I mentioned this to Kraus, who suggested I go talk to Columbus Iselin, the former WHOI Director. Iselin said he thought I should go to the meeting. He picked up the phone and told Henry Behrens, the WHOI comptroller, to arrange it! Richard Goody and Allan Robinson, who ran the program I was enrolled in at Harvard, were surprised to see me in London. They had brought Stan Jacobs and Victor Barcilon to the meeting, and did not expect any other Harvard students. I had a great two weeks in London and learned some basics of air–sea interaction and ocean circulation. During the 1962–1963 school year I completed my graduate course requirements.

In 1963 Henry Stommel decided to move to MIT, and I decided to stay at Harvard. I felt comfortable there, and the Committee on Applied Mathematics only required reading knowledge of one foreign language. I already knew enough German to pass that requirement. So I switched to Allan Robinson as my adviser. I spent a fair amount of time working on the computation of second-order nonlinear effects for barotropic Rossby waves on a midlatitude beta plane. I stayed in Cambridge in the summer of 1964 but Allan Robinson and Francis Bretherton both were at the GFD course at WHOI. Bretherton gave a lecture on time-dependent motions near the equator, and Robinson suggested to me that I investigate the baroclinic waves on an equatorial beta plane, with a view toward studying the nonlinear effects. So starting in the fall of 1964 I began to work on equatorial waves. Rattray (1964), Blandford (1966), Matsuno (1966), and Rattray and Charnell (1966) all published important papers on the topic. Rattray and Charnell tried to find equatorial wave solutions in a closed basin, and I was not satisfied with the approximations they used to obtain their result. The focus of my thesis turned out to be how to construct wave solutions in an equatorial basin bounded by eastern and western boundaries, but open to the north and south. The modes I constructed all had energy propagating equatorward from high latitudes on the western boundary and an equal energy flux going poleward on the eastern boundary. Michael Longuet-Higgins was producing similar modes on a sphere at the same time. For some reason I never looked at reflections off an

eastern or a western boundary separately, but only studied basins with boundaries on both sides.

My fellow graduate students included George Philander and Conrad Lautenbacher (the present NOAA Administrator), among others. For the story of Philander's first meeting with Jule Charney, see the chapter by Stommel and Moore on Charney's contributions to oceanography in *The Atmosphere—A Challenge: The Science of Jule Gregory Charney*, edited by Richard S. Lindzen, and published by the AMS in 1990.

Postdoctoral fellows working with Allan Robinson while I was a graduate student included D. James Baker (who was NOAA Administrator under Bill Clinton) and Pearn P. Niiler. During the 1966–1967 school year Robinson was on sabbatical in India, and Peter Stone acted as my adviser in his absence. By then I had a pretty clear idea of what I was trying to do, and Peter was very supportive. By early 1967 Niiler had left Harvard to join the faculty at the Oceanographic Laboratory of Nova University in Fort Lauderdale, under the direction of William S. Richardson. Niiler called me one day and asked if I would like to visit Nova, with a view toward coming there after finishing my degree. I visited in May 1967. At that time the faculty in oceanography consisted of Bill Richardson, Peter Niiler, and Bill Schmitz. I also visited the University of Washington at the invitation of Maurice Rattray. I eventually accepted an offer to come to Nova. I handed in a draft of my thesis in the fall of 1967, and defended it in March 1968. I went to Scripps for six months for a short postdoctoral stay with Walter Munk, and then I moved to Nova in the fall of 1968. By the time I arrived Dayton Carritt, Charlie Yentsch, and Russ Snyder had also joined the faculty.

Bill Richardson had a unique approach to studying the ocean. At this time oceanographic and atmospheric studies were carried out in three distinct modes. Direct observations of the atmosphere and oceans were essential, theoretical (and eventually numerical) modeling studies were conducted to make sense of the observations, and laboratory scale experiments were conducted to allow the exploration of the parameter space associated with the phenomenon of interest. This was the era of large-scale laboratory experimentation, using elaborate rotating turntables. Richardson had been at WHOI and the University of Miami before he set up the lab at Nova, and he made a conscious decision to concentrate on direct observations of the ocean, along with enough theory to make sense of the observations. There was a specific decision not to get involved in laboratory experiments, but to concentrate on novel ways of measuring how the ocean works. Richardson and Schmitz were concentrating their efforts on measuring the velocity structure and transport of the Florida Current, at seven sections from the Florida Keys up to Cape Fear (see Niiler, 1975). The key to the measurement technique was a navigation system called Decca Hi-Fix, that had accuracy comparable to today's Global Positioning System (GPS). This allowed Richardson to develop a dropsonde instrument that included a free fall STD and vertically integrated transport measurement. Other instruments that drifted on the surface or dropped weights at intermediate depths allowed a current profile to be measured. Further refinements on the basic idea of these instruments led to the White Horse profiler that Schmitz used

off New England to measure the Gulf Stream, the profiler that Luyten and Swallow used in the Indian Ocean in the mid-1970s to first describe equatorial deep jets, and the profiling work that Eric Firing did across the equatorial undercurrent south of Fanning Island during PEQUOD in 1982–1983. The development of the instruments that enable us to learn how the ocean works has been accomplished through a gradual evolution that has depended on significant engineering efforts to bring new technology to bear on the problem of making accurate *in situ* measurements. Without the *in situ* measurement capability everything else that we do would be for naught.

I got to Nova in the fall of 1968 and worked closely with Peter Niiler. He had visited me briefly while I was at Scripps and suggested that we look at the Jule Charney model for an inertial Gulf Stream to see if we could extend it beyond the separation latitude. We started in on this problem when I got to Nova and pursued it at the National Center for Atmospheric Research (NCAR) during the summer of 1969. Elliott Schulman and Jim O'Brien were there, and Elliott had arranged for a number of oceanographers to visit and lecture that summer. Adrian Gill and Allan Robinson were among them, and Henry Stommel was supposed to come, but had to bow out due to a broken rib. We watched the first landing on the moon with a large group in Jim O'Brien's living room. Jim and I sat in the back, playing cards. The game is called Klabberjass, or Klob.

In June 1970 I attended the Congress on Applied Mechanics in Cambridge. Bill Richardson asked me to represent Nova at a meeting that Allan Robinson was convening to discuss a program to study the role of eddies in the deep ocean circulation. I conveyed Richardson's interest in taking part in the program, with a view toward making current measurements from aircraft. I did not think much more about this until the following May, when I got a call at Nova from Henry Stommel and Allan Robinson, who asked if I would consider coming back to Cambridge for 2 years to work at MIT as the Executive Officer for the Mid-Ocean Dynamics Experiment (MODE). This was the program to study the role of eddies in the ocean circulation, and was jointly funded by the National Science Foundation (NSF) and ONR as part of the International Decade of Ocean Exploration (IDOE). Stommel and Robinson were the cochairmen of the MODE program. I visited the program managers in Washington, D.C. (Worth Nowlin at NSF and Denny Kirwan at ONR), and then told Stommel and Robinson I would do it. Bill Richardson was not too happy about this, as he was worried about the impact on my research. He eventually wrote a play on the subject, called "Mid-Ocean Madness, or I'm in the MODE for Love." I never knew for sure why Stommel and Robinson asked me to take on this job. Robinson told me that he and Hank were in his office, and Allan was looking at the collection of Ph.D. theses that he had supervised. Apparently he thought I might be able to do it.

Robinson was due to be on sabbatical in England and India that year, so we agreed to put the MODE office at MIT next to Henry Stommel in the Green Building. Norm Phillips was the department chairman and Jule Charney was just down the hall. Carl Wunsch was one floor down. The MODE program involved 20 principal

investigators from 12 U.S. institutions and the National Institute of Oceanography (NIO) in England. There was an extensive management structure, with two cochairmen and an executive officer (a.k.a. the "troika"), an executive committee, a theoretical panel, and at least six other committees. The 4-month-long field experiment was to take place south of Bermuda during March–June 1973, and in the meantime everyone was busy building instruments, designing arrays, making preliminary measurements, and attending planning meetings. The principal investigators (PIs) constituted a group called the MODE Scientific Council, which met every 2 months beginning in July 1971. There were many new instruments developed for MODE, including the WHOI Vector Averaging Current Meter, the SOFAR float, a bottom-mounted pressure and temperature recorder, the electromagnetic current profiler, and expendable current probes launched from aircraft.

My job was to keep track of everything that was happening without telling the PIs how to run their projects. I talked with everyone about how things were going, planned the Scientific Council meetings, produced the needed documents, and tried to be sure we were on schedule. I had lots of help from Hank, Allan, and the rest of the Executive Committee. Everyone wanted the program to succeed.

A great deal of time was spent figuring out how to design the observing array. During the summer of 1972 the theoreticians all went to NCAR for 10 weeks to work out an experimental design, while the observationalists of the "Array Committee" held a 2-week session at WHOI to put together their version of what should be done. Russ Davis was the only person to attend both, so his ideas were well represented. The MODE Executive Committee met weekly, often by conference phone. The meetings were noteworthy for a number of reasons, including the number and strength of the cigars that were smoked, as well as the loudness of some of the participants. Students in the Green Building maintained that when we met on the 14th floor we could be heard clearly on the 13th and 15th floors as well. Francis Bretherton and I were the loudest! (See Chapter two in this volume.)

In the fall of 1972 there was a meeting on Numerical Models of Ocean Circulation sponsored by the National Academy of Sciences (NAS) and held in Durham, New Hampshire. The meeting organizers felt it was important for the numerical modelers to know something about the measurements that would be available to test their model results against. Nick Fofonoff and I wrote a paper on modern measurement techniques, which I presented (see Fofonoff and Moore, 1975). The Proceedings volume from the meeting was published in 1975. Henry Stommel's remarks in the conclusions section are well worth looking up (see Munk *et al.*, 1975). He was talking about the different styles of doing science that were in vogue at the time. IDOE was in full swing, including MODE, NORPAX and CUE, and GEOSECS, all more or less highly organized and involving lots of players. There were also the individual investigators, like Rattray and Veronis. Stommel went on, "I see the motorcycle gang, too—the Niilers and Gills and Welanders of this world. They are on their own, but I don't know how long they are going to last. . . . Then there are some very big buses, with not very many people on them, and they seem to have UNESCO diplomatic

plates, and everybody's scared they are going to use up all the gas. Then I see one lonely individual, Arthur D. Little, who's been hired by the National Data Buoy Project, who is handing out questionnaires like, 'Do you like your speedometer to be calibrated in miles per hour or kilometers per hour?' " Some present-day concerns are not new.

The MODE experiment was conducted successfully and on schedule, and involved six ships, two aircraft, our own radio station and data center on Bermuda, a private phone system linking the Bermuda data center to Harvard, MIT, Yale, WHOI, and Johns Hopkins, and a network of Xerox telecopiers that could transmit a page in either 4 or 6 minutes. These were used to share data in near real time. There was plenty of excitement. Eli Katz aboard the *Chain* forgot about the watch circle around each subsurface mooring location, and steamed directly over one of Bob Heinmiller's moorings with his towed CTD instrument. He snagged the mooring with the towfish, and could not get loose. So he called me at the command post in Bermuda and asked for the release codes for Heinmiller's acoustic release, so he could pop the mooring and recover his towfish. Needless to say, Bob was not amused.

The main messages I came away from MODE with were that big science could be a lot of fun, that it was important to invest resources in the development of new instruments and techniques, and that while there was a significant role for theoreticians in planning these experiments and interpreting the results, the people who actually made the measurements needed to play the primary role. The other thing that I got from the 2 years at MIT were a set of lasting friendships and collegial relationships that substantially shaped the rest of my career. Ed Sarachik came to MIT to work with Jule Charney, and sat in the office next to mine. Mark Cane, Antonio Moura, and Inez Fung were all graduate students there at the time. Charney was working with George Philander and others to try to design the GARP Atlantic Tropical Experiment (GATE), to be carried out in the summer of 1974. Charney asked Sarachik to build a theoretical model of the tropical ocean, including the mixed layer and the undercurrent, to help guide the design of GATE. Sarachik got hold of Lighthill's 1969 paper on "Dynamic Response of the Indian Ocean to the Onset of the Southwest Monsoon," and as he read it he started asking me questions about equatorial waves. He forced me to consider the effect of a boundary on one side of the ocean basin or the other. This led me to realize the fundamental difference between reflection of baroclinic equatorial waves at western boundaries and at eastern boundaries. At a western boundary an incoming wave of a given meridional mode number n can always be reflected by a finite set of outgoing waves at mode number n or lower, plus a Kelvin or a mixed Rossby–gravity (a.k.a. Yanai) wave. But at an eastern boundary the same is not true, because equatorial Kelvin and Yanai waves only carry energy eastward. Therefore, reflections at an eastern boundary involve higher meridional mode numbers, and eventually include modes decaying to the west which result in energy propagating poleward along the boundary at high latitudes.

The Lighthill paper had left out the Kelvin wave completely and as a result had not gotten the western boundary response correct. The model that Sarachik started

to develop was eventually handed off to Mark Cane to complete. He and Sarachik wrote a comprehensive series of papers on barotropic and baroclinic disturbances, with special attention to the equatorial wave guide.

Before I left MIT Henry Stommel asked me if I could assemble a group of equatorial theoreticians to think about possible experiments in the Indian Ocean. I got Dick Lindzen, George Philander, and Ed Sarachik to agree to serve on the INDEX Theoretical Panel.

I returned from MIT to Nova in the fall of 1973. George Philander contacted me and asked if I could come to Paris for a meeting at IOC to help plan GATE oceanography. I stopped in Boston on the way and went to Woods Hole to see Henry Stommel. Michelle Fieux was there visiting as well, and one evening she and Stommel used a sextant to measure the angular separation between two stellar objects while I was given the ephemeris and nautical almanac from which to calculate the theoretical answer. The results agreed to within the error of the sextant observation, much to our pleasure. I also met with Mark Cane to get his ideas for GATE, because he could not go to Paris. The airport at Boston was full of troops when I left for Paris, as a war had just broken out between Israel and the Arabs. The meeting in Paris was great fun. The participants were an interesting, enthusiatic group. They included John Knauss and a couple of his students, Walter Duing from Miami, Philip Rowlands, and a significant contingent of both East and West Germans, as well as a few Russians. One morning I opened the *Herald-Tribune* to see that U.S. Vice President Spiro Agnew had resigned. I told Knauss there was a job for him in Washington, but he was not interested. He later became the NOAA Administrator under George H.W. Bush.

In terms of planning for some oceanography at the equator, we decided GATE should concentrate on the undercurrent, which was the only significant feature we knew anything about. The meteorologists primarily wanted the oceanographic vessels as observational platforms for the atmosphere, and had no strong ideas about what oceanography we should do. We laid out an observational plan that involved a zigzag pattern as the ships steamed along the equator, to allow monitoring the position of the undercurrent and lowering current profilers through it. By the time the experiment was actually run, the transects were done in the opposite direction from what we had suggested, and the mean core of the undercurrent was observed to be some distance off the equator. There were also significant fluctuations of a biweekly time scale (see Duing *et al*., 1975).

The year I came back from MIT to Nova, Niiler and I wrote up our Gulf Stream separation model and submitted it to George Veronis for the *Journal of Marine Research*. He accepted it for publication the day we submitted it. The work involved a complicated sequence of nested boundary layers. I was rather proud of it, but Carl Wunsch later cited it as an example of "analytic models . . . which . . . seemed untestable and intuitively implausible outside the laboratory." I still like to work on problems of this sort (see Moore and Niiler, 1974, and Wunsch, 1981).

In the spring of 1974 Henry Stommel came to Miami to meet with Walter Duing, Ants Leetmaa, and me, to plan an Indian Ocean program (INDEX) to propose

to NSF and ONR. We all spent an afternoon on Walter Duing's sailboat in Biscayne Bay, laying out a possible program. We got our colleagues to write the necessary proposals, and we wrote a brief cover document for the program. The Global Atmospheric Research Program (GARP) had already announced that the First GARP Global Experiment (FGGE) would take place in 1977 and 1978. Our idea was to do pilot studies to allow us to plan a successful oceanographic component for FGGE in the Indian Ocean.

Later that spring, Stommel and I attended a meeting that Joe Smagorinsky hosted at GFDL to start discussing a monsoon program for the FGGE. We laid out our plans for INDEX. Joe Smagorinsky asked how well we needed to know the wind field that was driving the system. I said that in the tropics we needed daily averages on 1-degree squares, unless someone could demonstrate that the winds were coherent on longer scales. Smagorinsky said it would be a long time before we got that sort of resolution, but that some engineers at NASA thought it could eventually be done from satellites. Only now has this become possible.

In the summer of 1974 I went to WHOI to take part in the GFD course. I stopped at GFDL in Princeton on my way to Woods Hole, to start work with George Philander on our chapter for volume 6 of *The Sea* (see Moore and Philander, 1977). Early in July, Henry Stommel and I made a day trip from Woods Hole to NSF in Washington, D.C. and delivered the first set of INDEX proposals to Gene Bierly. These were eventually funded, and we set up an INDEX program office at Nova, with Jan Witte as the coordinator.

At the end of the summer as I was returning to Fort Lauderdale I met Bill Richardson in Washington, D.C. He was on his way to Maine with the Nova R/V *Gulfstream*. His plan was to spend the fall at the Bigelow Laboratory in Boothbay Harbor, Maine, testing a satellite-tracked drifting buoy he was developing for the NOAA National Data Buoy Center, for use in the southern oceans during FGGE. His approach was to launch buoys on day cruises out of Boothbay, and subsequently track their progress via research aircraft. Peter Niiler was about to move from Nova to Oregon State University, and Richardson asked me to watch out for the laboratory while he was away. By this time Charlie Yentsch had left Nova to take on the Directorship of the Bigelow Laboratory, Dayton Carritt had moved to the University of Massachusetts, and Mark Wimbush had joined the Nova faculty.

In early January 1975, Bill Richardson and four others sailed from Boothbay Harbor aboard the R/V *Gulfstream* and never returned. There was a severe storm that started the next day and the Coast Guard investigation concluded that the ship foundered during the storm. The body of one crew member was eventually found. Ironically, Richardson had just become the chairman of the University–National Oceanographic Laboratory System (UNOLS) Advisory Committee that month. His loss prompted UNOLS to promulgate a set of safety standards.

The loss of Richardson and four others put the lab at Nova in a terrible situation. Through the hard work of those left at the lab at Nova, and the generous support of the University and the community, the laboratory survived. I was made Acting Director,

and eventually Director. Within a few months things began to settle down, and we got back to our research.

In May 1975 the INDEX Theoretical Panel held its first meeting. There were a dozen participants. There were not many people doing equatorial theory in those days, so the panel served as a training ground for young theoreticians. By the next year, there were plans underway for equatorial oceanography during FGGE in all three oceans, under the Scientific Committee on Oceanic Research (SCOR) Working Group (WG) 47, chaired by Stommel. He set up a panel for each ocean, chaired by Taft for the Pacific, Hisard for the Atlantic, and Swallow for the Indian Ocean. I had already entrained all the theoreticians to work on the Indian Ocean, so Taft and Hisard wrote and asked me to expand the scope of the panel to include the other two oceans. Thus, the INDEX Theoretical Panel became the Equatorial Theoretical Panel.

In December 1975 I attended a SCOR WG 47 Pacific Panel Meeting in Hawaii, at the Kuilima Resort on the north shore of Oahu. This was my first visit to Hawaii and I was fascinated with the place. Bruce Taft chaired the meeting. Klaus Wyrtki and Colin Ramage took care of the local arrangements. They put us in an out-of-the-way place with few distractions. I met a student from Scripps named Julian (Jay) McCreary, who was working on a thesis with Myrl Henderschott. It was a linear equatorial wave model for El Niño, and he was having trouble convincing his committee that he had solved the problem correctly. I went back to Florida after this meeting and wrote a letter to Russ Davis and Myrl, trying to convince them that Jay was doing good work.

In the spring of 1976 there was a UNESCO meeting in Nairobi on Cooperative Investigations of the North and Central Western Indian Ocean (CINCWIO). John Swallow, Henry Stommel, Walter Duing, Fritz Schott, and I were all there, along with many others. One of the participants was Mr. J. Findlater, who had discovered and described the low-level atmospheric jet trapped against the east African highlands during the Southwest monsoon. This is basically a western boundary current in the atmosphere, supported by a mountain range. In subsequent documents about INDEX we emphasized that it was timely as a contribution to CINCWIO. Later that spring Luyten and Swallow were at sea in the Indian Ocean and discovered the equatorial deep jets, which they referred to as "Equatorial Undercurrents." Carl Wunsch was with them, and produced a theory that turned out to be unsatisfactory. It is interesting to note that it is nearly 30 years later and there is still no satisfactory theory for these deep jets (see Luyten and Swallow, 1976, and Wunsch, 1977).

In the summer of 1977 we held the FGGE, INDEX, NORPAX, Equatorial (FINE) Workshop at Scripps, to complete preparations for the equatorial oceanography to be carried out in FGGE. This workshop began with 100 participants for the first week, with lectures by myself, Carl Wunsch, John Boyd, George Philander, Walter Duing, David Anderson, Julian McCreary, John Allen, and Ed Sarachik. The second week continued with about 24 participants, and a series of lectures by John Boyd. The afternoons were reserved for general scientific discussion. For the next 4 weeks a dozen participants stayed on and worked together on a variety of problems. This included myself and Jan Witte from Nova, Jim O'Brien and David Adamec from

FSU, Jay McCreary and Mike McPhaden from Scripps, and Phillipe Hisard, Jacques Merle, Joel Picaut, and Jean Marc Verstraete from France. At the end of the summer Jay McCreary defended his thesis, and I hired him to join the faculty at Nova. By the time he got there, the Office of Naval Research had already agreed to fund his work.

In February 1978 there was a NORPAX meeting at Mt. Hood. Jay and I attended from Nova. Klaus Wyrtki was there from Hawaii, and was the chairman of NORPAX by this time. One evening Klaus and I were sitting in the heated outdoor pool, and he asked me why I had not applied for the job that they had been advertising in Hawaii. This was the Joint Institute for Marine and Atmospheric Research (JIMAR) Director position. I did apply, paid a visit to the University of Hawaii (UH), and was eventually offered the job. I agreed to start in March 1979, with frequent visits in the interim.

Some of my most pleasant memories are associated with various educational undertakings. One of these was lecturing at a series of "Expert Lectures on Ocean Circulation Modelling" that Jim O'Brien organized at the Ecole Polytechnique in September 1978. I attended for a week, and was training for a marathon at the time. One morning I ran from the Ecole Militaire, next to my hotel, all the way to the Ecole Polytechnique, about 30 kilometers. It took a bit longer than I expected, and I was late to class.

Back at Nova, Jay McCreary had constructed a linear model for the equatorial undercurrent, and I encouraged him to talk about it at the fall AGU meeting in San Francisco. We got Jack Frost, the machinist and jack-of-all-trades at Nova, to make a plexiglass model showing the equatorial section and five perpendicular sections, as a three-dimensional visual aid to understanding the model. Jay's AGU talk was well received. Jay still has the plexiglass model. The paper (McCreary, 1981) was published in the *Philosophical Transactions of the Royal Society*.

I went directly from San Francisco to Hawaii, where I was scheduled to run a marathon. I spent an hour on the phone with Bob Harvey on Saturday morning, talking about plans for JIMAR. We broke off the conversation when he had to leave for a weekend cruise on the *Holo Holo*.

I ran my marathon on Sunday, slower than I had hoped. I went in to UH on Monday, expecting Harvey to appear later that day or early Tuesday. He was supposed to get off on Monday morning and return to Honolulu. The *Holo Holo* never made it in, and was not heard from or seen again. Ten people were lost at sea on this cruise, including the owner of the vessel, three UH scientists, two NOAA scientists from the Pacific Marine Environmental Laboratory (PMEL), and four others. The Coast Guard searched for more than a week. Because of my earlier experience with the loss of the *Gulfstream*, I was in the center of the UH effort to help with the search. Eventually one instrument box washed up on a beach on the big island. Nothing else was ever found. This tragic loss, occurring just 4 years after the *Gulfstream* disappearance, should never have happened. The Coast Guard concluded that the vessel was not sea-worthy. The University of Hawaii was (and still is) a member of UNOLS, but the people responsible for chartering the *Holo Holo* knew nothing about the safety standards

that UNOLS adopted after the loss of the *Gulfstream*. Subsequently UNOLS adopted a separate set of standards and procedures for charter vessels. Unfortunately, all of this illustrates that oceanographers are no better than anyone else at learning from their mistakes. It often takes more than one.

In March 1979 I moved to Hawaii and became the JIMAR Director. The FGGE fieldwork was already underway, and we were busy planning what to do next. A group who wanted to have a detailed look at the equatorial waveguide proposed an experiment called PEQUOD, which stood for Pacific EQUatorial Ocean Dynamics. Jim Baker, then at the University of Washington (UW), and I were the cochairmen for this program, and in early June we put together a cover document to accompany the proposals. Shortly thereafter Jay McCreary arrived in Hawaii for a summer visit at JIMAR, and he and I immediately set about reworking the document. We had Jan Witte come out from Nova to help us. The cover documents were FEDEXed to NSF in time to be there by the July 1 deadline, only FEDEX lost the shipment. So we had to produce another set of copies the following week, and this time we shipped them to Baker in Seattle, to hand carry to Washington. The program was eventually funded, but Baker's project was not, so he dropped out. Jim Luyten at WHOI became the other cochairman, and Bob Heinmiller at MIT took on the coordination responsibilities. Bob soon realized that the time difference between Honolulu and Boston was going to make communication difficult, and he contracted with GTE to set up a network using GTE TELEMAIL. We all got terminals that the phone could be connected to, and we had the equivalent of email a decade before the Internet became available. Heinmiller eventually took the system private and set up OMNET.

In December 1979 Jay and I attended the IUGG meeting in Canberra. Afterward we traveled with Rana Fine through New Zealand, Samoa, and Fiji. Jay and I concentrated on planning for the meeting of SCOR WG 47, to review what had been learned during FGGE. He also explained the ideas about the delayed oscillator theory for ENSO that he and David Anderson had been working on (McCreary and Anderson, 1984). The SCOR WG 47 meeting was held in Venice in April 1981. Jay was convener, and wore a jacket and tie! It was a memorable meeting. Some of the equatorial theoreticians had been having a disagreement, and the key words were "gullible" versus "skeptical". The argument had to do with sea level variability in the Gulf of Guinea and whether the cause was more likely to be a linear response to zonal wind variations in the western equatorial Atlantic or a nonlinear response to meridional winds in the eastern equatorial Atlantic. At some point during the week in Venice the theoreticians sat down at an outdoor café to work out a truce, but when Fritz Schott asked if he could join the discussion he was refused! The SCOR WG47 meeting results were summarized in *Recent Progress in Equatorial Oceanography*, edited by McCreary, Moore, and Witte, which appeared in December 1981.

The PEQUOD program took place starting in March 1982 and extended into 1983. It turned out to coincide with a major El Niño whose timing did not follow the accepted norm. So at a meeting at Princeton in the fall of 1982 we had a temperature record from Fanning Island showing the highest SST ever recorded there, and yet no

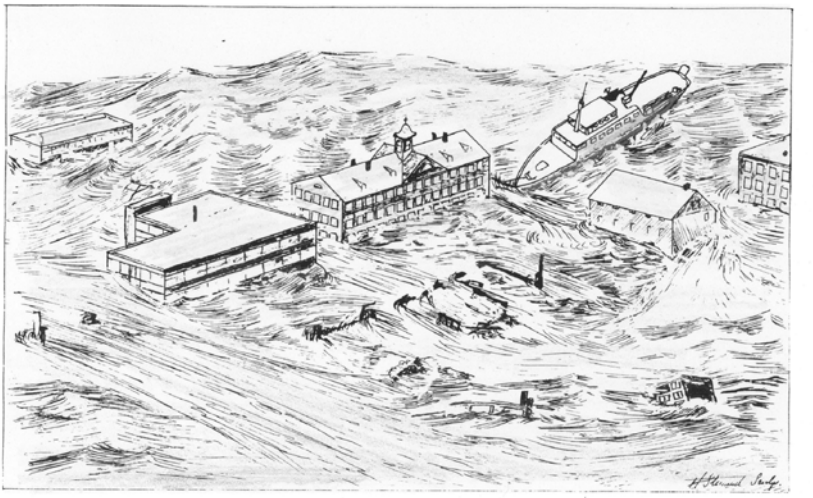
one would admit that an El Niño was underway. (See the Chapter six in the present volume.) In the next half decade many equatorial programs were carried out.

A TOGA workshop in Honolulu in August 1986 resulted in another volume called *Further Progress in Equatorial Oceanography* (January 1987), edited by Katz and Witte. It includes articles on INDEX, NORPAX/FGGE, EPOCS, TOGA, SEQUAL, PEQUOD, Tropic Heat, WEPOCS, and the Equatorial Theoretical Panel. By this time there was no shortage of equatorial oceanographers.

During the early 1980s the FGGE project office under Rex Fleming and Mike Hall was transformed into the Office of Global Programs under Mike Hall and Ken Mooney. Klaus Wyrtki had a variety of sea level projects ongoing at Hawaii at the time, and Rex Fleming and Mike Hall encouraged him to establish a single activity called the Hawaii Sea Level Center, which they agreed to fund. This has been an ongoing activity for more than 20 years now, first under Wyrtki, then Gary Mitchum, and now Mark Merrifield. The sea level data are used both for climate studies and for tsunami detection, with most of the gauges capable of rapid sampling and real-time data transmission. The network also provides ground truth for satellite altimetry.

In March 1987 I called Henry Stommel to ask him about long-term sea level rise, but he immediately changed the subject. He sent me two papers on a laboratory demonstration of the Coriolis force which had been published by Arthur Holly Compton. The first was a 1913 paper in *Science* (Vol. 37, pp. 803–806), written when Compton was still an undergraduate. I read the Compton papers and tried to answer Hank's questions about them. The first one had an error in the theory, with a missing factor of 2, but Stommel could not believe that Compton got it wrong, and wanted me to explain the discrepancy. He then sent me a sketch of Woods Hole and WHOI partially submerged and the Eel Pond overflowing its banks(Figure 7.1). This was his contribution to the discussion of sea level rise! He also sent me a copy of the fourth draft of a book he had been working on called *An Introduction to the Coriolis Force*, and he asked me if I would be willing to be a coauthor and help him finish the book. He had originally tried to do the whole thing without using vectors, because he wanted the material to be easily accessible, even to high school students. I agreed to help, but only if we in fact used vectors to help tell the story. Working with Hank Stommel on this book was a wonderful experience for me. It was very intense, and involved lots of writing in the early hours of the morning. I would typically go to bed early, thinking about some problem, and wake up about 3 AM with an inspiration about what to write. I got to think through the details of many elementary mechanical problems we used to illustrate various concepts. Things I first learned about in the GFD course in 1960 finally made sense to me. The book appeared in 1989, and it was not a great success. But helping to write it was fun (Stommel and Moore, 1989).

By 1990 the biogeochemists were becoming interested in the equatorial oceans, and planning equatorial cruises in the Pacific. The only problem was that while there were lots of equatorial physical oceanographers, there were not many biogeochemists who knew anything about equatorial dynamics. So in June 1991 Lew Rothstein ran



April 1, 1987

Dear Dennis

Thank you for the pleasant phone call last night. It was very nice to speak with you again. The above picture is based upon a general rise of sea-level accompanying global warming. As you can see George Woodwell will have difficulty getting out of the MBL.

Good luck with Compston (Science 1973, vol 37 pp 805 ff).

Henry

Figure 7.1

a month-long program on Equatorial Dynamics at the University of Rhode Island (URI), specifically to help train the biogeochemists. Jay McCreary, Jim O'Brien, Lew Rothstein, and I formed the core faculty for the workshop. Mike McPhaden was there the first and last week, and a number of other lecturers came for a few days at a time. There were about 20 students. We all lived together in three or four rental houses, and shared cooking and housekeeping responsibilities. Jim O'Brien and Lew Rothstein set up a computer visualization laboratory for the workshop, using a number of Silicon Graphics workstations. So the students were not only learning about equatorial theory, they were also running time-dependent computer models to visualize what they were studying. At the end of the workshop we had a 2-day meeting of the Equatorial Theoretical Panel at the URI Whispering Pines Conference Center on the W. Alton Jones campus. Henry Stommel came to the meeting and I clearly remember an early morning discussion with him the second day. He brought me a goose quill he had picked up on the grounds, and suggested I could use it to write with. I think that was the last time I saw him.

Sometime late in 1991 I got a call from Chris Mooers asking if I would be interested in being an editor of the *Journal of Physical Oceanography* (JPO). I spoke with Peter Gent and Eli Katz about it, and said I would be willing to give it a try. I quickly joined the American Meteorological Society (AMS) and sent my CV to the publications committee. I became an editor in early 1992 and continued until late 1995. Editing an AMS journal provided a nice balance to my UH responsibilities as JIMAR Director.

In 1994 Bruce Taft announced that he would retire the following year, and when his job as Leader of the Ocean Climate Research Division at NOAA's Pacific Marine Environmental Laboratory was advertised, I applied. I was offered the job and moved to Seattle in 1995. It has been exciting to work closely with a diverse group of scientists who make *in situ* observations relating to the role of the ocean in climate. I have now been here 10 years and have no plan to retire. I enjoy my job. Lots of things have changed in NOAA over the decade. Because of my 17 years directing JIMAR in Hawaii, I still pay close attention to the operation of the NOAA joint institutes, especially JIMAR and the Joint Institute for the Study of the Atmosphere and Oceans (JISAO). My impression is that the joint institutes have been an excellent mechanism to promote cooperation between NOAA and the universities, and have been the locus of lots of outstanding research over the last 40 years. But I don't think directing a joint institute is as much fun as it used to be.

What lessons can we learn from all of this? The first one is that *in situ* measurements are essential for understanding how the system works. Remote sensing, theory, and numerical modeling are all important, but *in situ* observations using the best available instruments are the sine qua non of understanding the ocean. This requires continual investment in new sensor and instrument development, incorporating state-of-the-art technology, to enable scientists to continue to advance their knowledge. This need to keep investing in new measurement techniques was considered obvious in the early 1970s, and was built in to IDOE programs like MODE. It is no longer as

easy to get the necessary resources to maintain the engineering development that in turn enables scientists to advance their knowledge. And a comparable investment in information technology and data handling infrastructure is also required, especially for the global climate problem.

Theory and modeling are very important for putting the pieces together and understanding how it all works. Only in rare cases does the theory precede the *in situ* observations. In the late 1960s someone said to me, "You don't really think all those equatorial waves exist in the ocean, do you?" During the following decade Wunsch and Gill (1976) found the equatorially trapped inertia-gravity waves in the sea level data, and then Knox and Halpern (1982) found the Kelvin waves in sea level and current meter data. Now these waves are recognized as the fundamental building blocks for understanding the role of the ocean in the climate problem.

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