REMINISCENCES OF AN OCEANOGRAPHER

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PREFACE

A good many of my younger friends have expressed curiosity about what oceanography was like in the days when I just entered the field and about some of the grand old men whom I knew in those days and who have since departed on that long cruise. Bob Fournier, in particular, has often urged me to jot down some of my reminiscences, and he more or less convinced me I should do so.

Of course, my elders in the field were few in number in those days, and in most cases I didn't know them well enough to do more than describe my limited associations with them and my personal impressions. As time passed, there were obituaries by their peers, which were more informative than I could be. So this is mainly an account of my own career and my close associates, who initially were of my own generation, more or less.

To be sure, some of them have become grand old men in their own right and some have departed. As time passed, there were younger associates coming in, and stories about the beginning of their careers may not be well known and may be of interest.

An added impetus to this dredging up of old memories was the volume of selected reprints which Joe Wroblewski organized and edited together with essays by other friends who discussed my work and their associations with me. [Selected Works of Gordon A. Riley. Wroblewski, J.S. (ed.) With introductory essays by G.E. Hutchinson, E.L. Mills, R.O. Fournier, P.J. Wangersky, and closing remarks by E.S. Deevey, Jr., Dalhousie University Printing Centre, Halifax. 1982. 489 p.] I was complimented and deeply gratified by their efforts, and I felt that if other people were interested enough to welcome that kind of volume, perhaps I should add an informal supplement. Only about a third of my total scientific output had been included. A little about the purposes and results of the other papers would be in order, as well as the "human side" of the work -- the personal experiences along the way that are never included in a scientific publication but often are important in shaping the development of a particular paper and the general course of one's career. There was a little of that sort of thing in the essays that my friends wrote but much more could be added.

Also the impression one gets from the essays is occasionally quite different from the way things seemed at the time. This is not a disagreement as to facts but rather a matter of perspective and personal reactions. For example, Eric Mills bracketed the period from 1938 to 1946 as a time when I made my best contributions to ecological theory. This is probably so, but heavens to Betsy, it was no triumphal march. I was doing what I wanted to do and was deeply interested in the results I was getting, but few other people were.

Evelyn Hutchinson and a few other relatively young scientists were strongly supportive, but virtually all of the older and well established men in both limnology and oceanography had no respect for the kinds of things I was doing and some were quite outspoken in their disapproval.

I was frequently discouraged and resentful. I really did not feel I was building a secure reputation in the field until I was fortyish and a little over the hill and not doing such revolutionary things any more.

I have never intended to write an autobiography, and this is not one. There is a lot of scientific discussion that will not be very interesting to anyone except fellow scientists and only a limited amount about family and other non-professional matters. To this small audience, I have been more frank in expressing my personal opinions -- perhaps prejudices -- than I would be if I were writing for the general public.

The account is hampered a little by the fact that I have never kept a diary or journal or even a voluminous vitae. There are some events that I cannot date precisely, and probably there are some mistakes in chronology. Also, in some cases, I have deliberately violated strict chronology to give continuity to a series of related events that occurred over a period of several years.

Where does one chop off something like this? Originally, I intended to terminate it at the time when I left Yale and came to Dalhousie. There was no reason to write about people and event here that everyone knows about.

That is a job for some junior staff member somewhere way down the road. However, when I got to that point there were some things that seemed worth saying -- bits and pieces of history about the development of the department and some personal items that probably are not common knowledge.

It is a bit like the proverbial snake that doesn't stop wiggling until sundown. I let it wiggle.

Finally, I want to say that I have written this pretty much as I would write a letter to a friend, just sitting down and letting he thoughts flow, without much attempt to be either well organized or literary, but just informative. As I looked it over afterward, I thought of other reminiscences that might have been included, and I did add a few more here and there. They are like those bubbles that keep rising to the surface of an old swamp. So be it. This is enough to give a flavour of what it was like during those years. End preface, on with the rest of it.

For a beginning, I suppose I should say something about the steps along the way toward an oceanographic career. Or should I call it a random walk?

I really didn't intend to become an oceanographer until I became one. In fact, most of my career decisions have been opportunistic, choosing what seemed at the moment to be the best option and neither looking back nor fully committing myself to the chosen option on a long-term basis.

To be sure, a young man preparing for a career during the depression of the Thirties had to be somewhat opportunistic. Options were limited.

Growing up as I did in the hill country of southern Missouri, I knew nothing about the ocean except what I read in books, and if I read anything about oceanography, I don't recall it. Sea stories --yes. That unknown world was intriguing, but I doubt that I ever thought it might be a significant part of my life.

School was boring as hell and seriously interfered with my education. The elementary schools I went to were stiff and uncompromising. Each year there was a small pile of texts to be mastered-- no less and no more. Other books were forbidden. If time hung heavy on our hands-- well, my poorest grades were in something that was then known as deportment. Outside of school, I read voraciously-- literature, good and bad, and science, mainly. Natural history was a favourite subject when I was in elementary school but gave way to broader studies as I got older.

In high school, my interests were about equally divided between literature and science. I was doing a lot of writing and toyed with the idea of a writing career. However, aside from the financial insecurity of that kind of life, I had developed enough critical faculties to know that my best efforts were poor by comparison with really good literature. The alternative option that I chose was to major in biology in college, although at that time, I was looking no higher than a bachelor's degree and a teaching job in high school.

So on to Drury College, a small liberal arts college in my home town of Springfield, Missouri. James Cribbs was <u>the</u> biology professor. Roland Neil was <u>the</u> chemistry professor, each carrying the whole bloody department course load. I don't know how they did it. Of course I realized later that they didn't do it the way it is done in universities. It was pretty much standard stuff out of the textbooks. They couldn't keep up with the recent literature and bring things up to date every year. Still, I think they provided a pretty good general background. However, in that kind of situation, one poor teacher could wreck a whole department. I have no compliments for the dodo who was supposed to be teaching physics at that time.

My parents were intelligent and liberal minded people but by no means affluent. They made some sacrifices to help provide a college education for my sister and me, luxuries they had been unable to afford for themselves. My sister Mildred, five years older than I, was out of college and teaching Latin in the local high school before I graduated.

She, too, was generous. So with summer jobs and some help from the family, I was able to pay tuition and other school expenses during the first two years, and of course I was saving money by living at home. The last two years, I was able to get teaching assistantships that took care of tuition. That was fortunate, for by that time, we were deeply into the depression, and I was unable to get any summer jobs.

The beginning of the third year was another moment of decision. If I was going to be a high school teacher, I would have to take some education courses, which were reputed to be a complete waste of time, and this would be at the expense of some science course that I wanted.

The alternative was a gamble-- skip the education courses, take the science, and later apply for a scholarship or fellowship to go on to graduate school. I took the gamble.

All the other biology majors were premedical students, and they tried to tempt me to go that way. There were no scholarships available for medical students in those days, and there was no way I could find that kind of money. I knew one student who had worked his way through med school with a part time hospital job, but I doubted I had the endurance to do that. Then during the summer after my third year, and again jobless, I did volunteer work at a local hospital as a lab technician and orderly, and that finished any lingering desire that I might have to become a doctor. The patients' pain was too painful to me for me to want to spend my life that way.

So I went back to college and continued to stuff in all the science I could get-- every biology course in the curriculum, most of the chemistry, general geology and invertebrate paleontology, and one course each in math and physics. What little smattering of liberal education that I've had was in high school and off-the-job reading later.

The time came in my senior year to apply for graduate work. I had decided to specialize in embryology and applied for admission and financial assistance at several universities where there were professors who could supervise that kind of work. Washington University came through with a scholarship for tuition and twenty dollars a month.

Mind you, dollars went a lot further in those days. A generous sister gave me another ten dollars a month. That saw me through, and I ended the year only ten pounds lighter than when I started.

Caswell Grave, head of the Biology Department at Washington University, was an elderly gentleman near retirement age who had done classical work on descriptive embryology of a number of invertebrates. He gave me some paraffin-mounted material of a series of stages of a particular Ascidian larva and asked me to work out the development of the sense organs-- light sensitive filaments and statocysts-- which appeared to be quite different from those of related species. I did. It was fun. There was only one hitch in an otherwise successful year. I flunked both of my mid-term tests the first semester. Big deal. Small town boy from itsy-bitsy cow college washes out as soon as he hits the big time. In a way, it was the fault of those Drury profs. Overworked as they were, they gave high marks for well organized answers that stated general principles in brief terms. University profs wanted that plus as many details as could be crammed into an hour of mad scribbling. As soon as I got that straight, I began to pass exams.

Dr. Grave felt that there was no future in descriptive embryo- logy any more and that I ought to get into experimental work. He helped me get a teaching assistantship at Yale to work with Ross Harrison, one of the top men in experimental embryology at that time. The assistantship, incidentally, paid tuition and sixty dollars a month. I could live on that without losing weight and even save enough money to take a trip home for a visit once or twice a year--on the bus, of course.

Dr. Harrison was a busy man, head of the department and supervising six or eight graduate students who saw him only by appointment. In my preliminary interview on arrival, I thought he seemed cool and aloof and rather forbidding. He wasn't really that way, but only after several years had passed did I realize that he was basically kindly and friendly but was very shy. Anyway, he told me to read the literature and see if I could come up with a thesis proposal.

So I read the literature and came up with no bright ideas at all. In fact, I got the impression that other people had run out of bright ideas, too. Earlier, experiments with limb or neural crest transplants in amphibian larvae had yielded interesting results, but now it seemed that they were trying all sorts of things, not with any particular problem in mind, but just to see what would happen.

The season for experimental operations on amphibian eggs and larvae was of course a short one, only a few weeks in spring, and I realized that if I came up with a suitable problem, I probably wouldn't accomplish much unless I learned something about operating techniques. By way of practice, I went out and collected some hydra and planarians and tried to repeat some of the experiments that had been done on these animals to examine problems of regeneration and reversal of metabolic axes by grafting. I quickly discovered that my operating technique was lousy. I didn't enjoy the work and was unable to complete any grafting experiments successfully. The only enjoyable part of it was going collecting. I was floundering.

Otherwise, I was having rather an exciting experience. This was my first time out of the midwest, and I was in a new and very different environment. I saw the ocean for the first time--well, Long Island Sound, anyway. Those huge masses of igneous rock were very different from the limestone bluffs that I was used to. I saw trees and birds that I had only read about. People talked differently. Accustomed to the slowly articulated drawl of southern Missouri, I found some of the speech, particularly the foreign accents, difficult to understand.

The first semester ended and the second began, and I still had nothing remotely resembling a thesis proposal. A lot of the courses were rather dull, too, things like vertebrate and invertebrate morphology, which simply were rehashes in more detail of things I had studied earlier. However, one course was a real departure. It was a one-semester course in limnology, taught by a young assistant professor named Evelyn Hutchinson. One lecture was enough to make me begin to sit up straight and bright-eyed and to struggle to assimilate every word of his thick British accent. He was dynamic and obviously very bright, full of new ideas, and was dissecting the literature with keen and frequently witty comments. Within a week, I knew that limnology was where I wanted to be. Unsolved problems stuck up all over the place. One didn't have to search for some little unfilled chink. And being a naturally outdoorsy person, I welcomed the idea of a career in which field work would be a significant part of the effort.

I didn't go to talk with him about it immediately. I was concerned about what Harrison's reaction might be if I changed supervisors, but after a month I could postpone it no longer.

In that nice introductory essay to the volume of my selected works, Evelyn described his excitement about that meeting. It could hardly have equalled mine. In the course of a half hour, my future was channelled into a very different direction, and the floundering of the previous few months was ended.

During the discussion, he asked about my chemical background. When he learned that I had an undergraduate minor in chemistry and was currently taking the medical school course in biochemistry, which of course included the use of colorimetric techniques, he immediately started promoting a thesis topic on the copper cycle in some Connecticut lakes.

I really would have preferred a biological problem, but there was an appeal in doing something that no one had tried before. Evelyn undertook the job of making my apology to Harrison and also making sure that an oddball chemical thesis would be acceptable in a Zoology Department. That proved to be no problem.

There were several methods in the literature for measuring copper. During the next month, I tested them all and chose the one that I thought was most suitable.

Evelyn's essay tells about our preliminary exploration of Linsley Pond, but it doesn't tell all. We went out in an inflatable rubber boat that he had used a few years earlier in an expedition to some high-altitude lakes in northern India. We went out as soon as the lake was ice-free. The boat leaked. We managed to get a few samples and a rough idea of the morphometry of the lake and get back before the boat collapsed completely, but two very cold, wet tails emerged from the boat.

Before we started our regular collecting program, we got a pretty little yacht tender, second hand and quite cheaply. We kept that at Linsley Pond. At the other two lakes, we were able to rent boats, only I well remember one time at Lake Quassapaug when all the boats were in use, and the only craft available was a canoe. I assured Evelyn that I knew how to paddle a canoe and that it was stable enough for our purpose. Little did I know. There came a moment when he leaned far out to one side, pointing into the water and saying, "I wonder what that is." I didn't know, I couldn't see it. I was desperately leaning out the other side to keep us from joining the unknown object in the water.

I remember one other occasion that was really perilous. It was in early winter. The ice was new and thin and rubbery, too thick to use the boat and dangerous to walk on. It clearly wasn't strong enough to support two people. Evelyn insisted on going out, and I was too chicken to volunteer doing it myself. But I suffer- ed, hoping he would not go through and wondering if I could somehow help him if he did.

Generally speaking, the field work was pleasant, although standing around a hole in the ice in midwinter sometimes was bitterly cold. The best part of course was that Evelyn was collecting samples, too, so that most of the time we went together, and this was a rare opportunity for me to further my education in leisurely association with a man whom I have always regarded as having the keenest and best informed mind of any scientist I have known. We talked at length about the new papers that were coming out and about limnological problems in general. His course, fine though it was, was little more than an introduction to a new field that I had entered in complete ignorance. Ed Deevey and I, his first students, had the best of it in the old days when he could afford to be generous with his time. This was not true later, as departmental duties and the number of students multiplied. He became almost as hard to see as Harrison had been.

Ed occasionally went with us, although his program was not directly connected with ours. He was studying pollen distribution in lake and bog cores. He had an able assistant on a good many of his field trips, a lively young lady named Georgiana Baxter, who started graduate school the same year we did. He apparently liked her. She later became Georgiana Deevey.

Another member of our little group at that time was W. Thomas Edmondson. Tommy's mother had given him a toy microscope for Christmas. He looked at some samples of pond water, was entranced by the rotifers, and somehow found his way to Evelyn's office to get more information. And stayed there. Evelyn gave him a corner of his lab, a pile of literature, and the use of a good microscope. Tommy was publishing papers on rotifer systematics before he graduated from high school and went on from there to a distinguished limnological career. I'm sure he must have been the youngest charter member of the old Limnological Society of America. A few years ago, there was a gathering of the old dodos, as his wife Yvette called us, at one of the ASLO meetings, and Tommy was certainly the youngest looking and liveliest of the lot.

There was one summer when Evelyn went on a visit to England, and Tommy helped me with field collections in the meantime. Evelyn left his decrepit old Buick with me for the trips to the lakes. It ran-- sort of. In fact, it tended not to stop running. There was so much carbon in the cylinders that when we arrived at the lake on a warm summer day, it went on dieseling as long as I would let it do so. I had to nudge it gently against a tree while it was still in gear to stop it.

Evelyn lived in that magnificent, well-ordered mind, which was a good place for him to be, for he was surrounded by chaos. His clothes were shabby, his cars decrepit. Every surface in his office was piled high with books and papers, although he could instantly locate anything he wanted. His performance in the lab was a disaster, frequently accompanied by crashes of glassware and a fervent, "Oh blast!" Broad English--"Oh Blahst!"--his very worst swear word. One day, he came to my lab to borrow some Nessler tubes--those long test tubes with optically ground glass on the bottom that were used in visual colorimetry. He stuck a half dozen in his jacket pocket. They went through a hole in his pocket and smashed on the floor. "Oh Blast!"

Oscar Richards was another young professor in the department to whom I owe a considerable debt of gratitude. Basically, he was a kind person, but superficially, he had a rather abrasive personality and was disliked by a good many students. One of his departmental duties was supervision of graduate teaching assistants in the elementary biology labs, and he rode us pretty hard. He also taught graduate courses in growth and in statistics, which few students took. I took them and liked them. The abrasion vanished, and we became good friends. Some work that I did in the growth course expanded into a joint paper on the growth of amphibian larvae, and I also became interested in trying to apply statistics to limnological analyses.

In the mid-Thirties, oceanography was a livelier and more progressive field than limnology, primitive as it may seem by modern standards. Evelyn and I were particularly interested in the work at the Plymouth Laboratory in England where a bright and innovative group of investigators had developed a variety of quantitative methods and had applied them to good effect in ecological analyses. One of the methods that appealed to me as a possibly useful tool in limnology was a plant pigment analysis developed by H.W. Harvey. It was ultra-simple, consisting merely of filtering the phytoplankton from a known volume of water, extracting the pigments with acetone, and measuring them against an arbitrary color standard. It was quick and easy and seemed likely to provide a better estimate of total phytoplankton than cell counts, though not entirely supplanting the need for the latter.

I hoped to use this method later when I got into biological problems. Unfortunately, though, I found that it was not very satisfactory for freshwater plankton, which contained variable and often large amounts of carotenoids which, in those days of visual colorimetry, could easily mask the chlorophyll. I dug up an old method for chemical separation of carotenoids and chlorophyll and adapted it for phytoplankton purposes. This made the job a little more laborious, but it seemed to be worthwhile, and I submitted a short paper on the method for publication.

In the third and last year of graduate work, I was finishing field work, doing some experiments on copper tolerance, and beginning to write the thesis. Of course, I was looking for job prospects, too, but they weren't plentiful or promising. There was an opening at the Iowa University field station on Lake Okeboje and another for work on Lake Tanganyika. I applied for both and didn't get either. I wasn't concerned about being jobless, for Evelyn wanted me to stay on for a postdoctoral year, but I would have preferred a more permanent job.

In the meantime, Albert Parr invited me to join him for a brief oceanographic cruise in the spring. Parr was an ichthyologist and physical oceanographer, the latter I think largely self-taught. He was Director of the Bingham Oceanographic Laboratory, a small group attached to Yale's Peabody Museum, and he also taught a one-semester course in oceanography. I had taken that, of course. More often than not, he went cruising for several months in the late winter and spring in the Caribbean and the Gulf of Mexico. That year, he was operating on the Woods Hole ship *Atlantis*, and the schedule called for two weeks' work off the mouth of the Mississippi in March. He wanted me to do plant pigments and nutrient chemistry in an attempt to find out whether river outflow had a significant biological effect on Gulf waters. In order to do this, I had to finish my thesis-- the first draft-- a month before the due date and then go cruising while my poor committee picked it to pieces. That is confidential information, not to be used to needle students who drag their heels around thesis time.

So on an evening in early March, I headed south. I recall standing on a street corner waiting for a bus to the railroad station. I was wearing a summer suit and a raincoat, suitable for where I was going, but hardly for New Haven, where a late winter snowstorm was swirling around me and flapping my trouser legs around my ankles. I shivered with the cold and perhaps a little with nervousness. I was excited by this venture into a new world that I knew so little about, but never having been to sea, I couldn't help wondering if I would be able to do a satisfactory job. I knew that Parr's two assistants on this cruise were frequently seasick and hated the whole thing.

A day and a half later, I stepped off the train in Mobile, Alabama, into lovely spring weather. There was a wharf right across the tracks from the station, and there, unmistakably, was the *Atlantis*, a 140-foot ketch with towering masts, beautiful despite the need for a paint job after several months at sea. As I trudged onto the wharf with my bag, I passed a small group of interested onlookers. I heard one of them say, "I reckon she's a rich bastard's yacht. She ain't noways a fisherman." He would have been astonished, as indeed was the scientific world, at some of the fishes that came aboard, for Parr's particular interest was in those marvellous little midget-sized monsters that he dragged out of the deep sea. I walked aboard to a hearty greeting from Albert Parr, who was certainly not rich and probably wasn't a bastard, although I didn't enquire about that.

Albert was a large and handsome Norwegian with that delight- fully liquid and lilting accent that Norwegians have. He had trained at Bergen as an ichthyologist and fisheries biologist with little physical oceanography thrown in. Opportunities for advancement there were limited. He came to America, landing almost penniless, and got a job in New York at the Aquarium on the Battery. This job, according to his account, amounted to little more than sweeping floors and feeding the fishes. However, somehow he happened to meet a wealthy yachtsman and sportsman named Harry Payne Bingham and persuaded Bingham to set up a program and collection of deep-sea fishes on his yacht, the Pawnee. The collection grew. Parr persuaded him to set up a foundation known as the Bingham Oceanographic Foundation, which supported a small lab, an appendage to the Peabody Museum, and publication of the scientific results. I don't know the details about all this, but the essential facts are a matter of record, and admittedly, Parr could be a super-salesman.

A little later, Parr obtained the use of the *Atlantis* for winter cruises. At that time, the Woods Hole Oceanographic Institution was heavily booked for summer cruises by visiting investigators from universities but had few resident scientists and minimal need for the ship in winter. Parr conveniently stepped into the gap, and that brings us to where we were on that March morning in 1937.

The other two members of the scientific party were Yngve Olsen and Martin Burkenroad. Yngve, the son of Swedish immigrants who had settled in New Haven, had an undergraduate degree in biology and was Albert's assistant in the fish work. Although he was a competent man in the lab, he was often seasick and hated the winter cruises. He was greatly relieved when Albert later became Director of the museum and no longer had time for cruising. After all the fish collections had been worked up, Yngve turned to editorial work on the two lab publications-- the Bulletin of the Bingham Oceanographic Collections and the Journal of Marine Research.

Martin was a native of New Orleans, of Spanish-Jewish descent. He shared Yngve's dislike of the sea, but the resemblance ended there. Yngve was an easy-going plodder. Martin was brilliant, erratic, and somewhat paranoid. He was sophisticated and could be a suave and charming companion when he chose to be, which was not by any means all the time. Although he had been kicked out of Tulane, short of getting his bachelor's degree, he had managed, on his own, to become a respected shrimp systematist.

The *Atlantis* had quarters for six scientists, a small complement for a ship of that size by modern standards, but the oceanographic population was small in those days. At a later time, they managed to squeeze in a few extras by installing pipe berths. I shared the starboard cabin with Martin. There were two inboard bunks, one above the other, and there was an outboard one of intermediate height, with drawers beneath for clothes. There was also a tiny closet. Yngve warned me, with a bitter tone in his voice, to be careful how I hung my suit up. He had ruined a suit by hanging it flat against the thwartship wall of the closet. As the suit swung with the roll of the ship, holes had been worn in the back of each shoulder.

Martin had the lower inboard bunk, where he claimed there was minimum of motion. Disregarding that, I took the outboard one. I wanted to be able to look out the porthole and see everything there was to be seen.

The ship was due to sail the next day, and that evening Martin, Albert, and I went ashore to stretch our legs for the last time. There was no tour of the bars. Alabama at that time was bone dry, although I dare say the sailors managed to find some friendly purveyors of "white lightnin" as well as girls. If such were to had, they would find them. We strolled up the main street and had some strawberry shortcake at a hotel restaurant. After that, Martin and I walked on, while Albert returned to the ship. We found an oyster bar-- big, fat Gulf oysters, twenty-five cents a dozen. I stopped after a mere two dozen, quite unaware of what a bargain they were, compared with how much they would cost before I ended my oceanographic career.

We walked on up into the residential area, and I don't remember much about it except that a lot of the houses had that pleasing old New Orleans style-- stone or stucco and balconies trimmed with delicate and ornate ironwork. We finished the evening with a spaghetti supper near the waterfront. Martin said he was making up for all the meals he had missed at sea. I had no such excuse, although I suppose my eating had been a little irregular during the last frantic days of finishing the thesis.

The next day, we shoved off down a long muddy channel between low-lying banks. That lasted for some hours. I watched from the deck for a little while and then went below to finish lashing and clamping down my equipment. I had the lower lab pretty much to myself. It was a long, narrow room stretching from one side of the ship to the other, with chemical benches at each end and tables of a slightly lower height along one side. The fore and aft companionway went through the middle, and there was a stairwell leading to the upper lab, which was at deck level. The upper lab had racks for Nansen bottles and a bench with running seawater supply for washing specimens. That was where the other members of the party were doing most of their work. I was alone down below except for occasional visitors or people coming to use the fathometer. That fathometer was a new and marvellous invention at that time, but it was quickly superseded by further developments, and I'm sure none of my younger contemporaries has seen anything remotely resembling it. The shallow scale was sonic. There was something that sounded like a little man pounding on the keel with a hammer. We could hear him all over the ship when we were in shallow water, and the echo return was recorded by a flash on the dial as on a modern visual fathometer. For deep water, there was an ultrasonic beep which triggered a point of light that circled the deepwater dial. We listened for the echo return through earphones and noted the position of the light on the dial when the echo came in. Only a few years later, of course, during World War II, fathometers became completely ultrasonic, and strip recorders were invented.

I was down below when we reached the open sea and was immediately aware of it. The weather was moderately blowy. The ship began to pitch, and I almost pitched on my face. The seaman feels the deck under his feet and knows what the ship is doing and automatically adjusts his equilibrium. To the landlubber, the motion is chaotic. I scrambled and bumped my way up on deck, and once I could see the horizon, I began to get the feel of it. And I felt fine. I just happen to be a fortunate one who has never served as an unwilling intermediary between terrestrial and marine food webs. It is no virtue. That's just the way it is. With old Henry Bigelow it was a virtue, or so the story goes. I tell the story with full realization that myths abound among seafaring folk. However, they say he was plagued by seasickness during much of that early period when he was studying the oceanography of the Gulf of Maine. One evening he came into the wardroom, ate a good dinner, and lighted a stogie afterward. The skipper said, "Well, Henry, looks as if you're feeling better." "Yes," Henry replied, "I've decided I'm not going to be seasick any more." And he wasn't. Or so they say.

So much for that. We were heading out under power but with the smaller sails set-- jib, mizzen, and a fore staysail, more commonly called the jumbo. The sails helped to steady her, and on station we hove to by backing the jumbo and sailing up with the other two. The mainsail was so big that the whole crew had to be called on deck to handle it, so it was never used except for long runs.

I was getting the feel of the ship's motion. When I went below, I was able to navigate fairly well. I inspected the lower lab. Everything seemed secure. The first hydro station came up. Scientists were treated to a luxury that is seldom seen anymore. The deck officer put the collecting bottles on the wire and took them off, and a seaman manned the hydro winch. All we had to do was hand the bottles out from the deck lab and specify the depth reading where the next bottle went on. All this was a far cry from Linsley Pond-- one bottle on the wire, lowered by a hand winch to successive depths.

I got my samples and went below to work on them. Harold Backus, the chief engineer, came in to shoot the breeze and get acquainted a little. Harold was a nice guy but just a little old-womanish and gossipy. The quizzical way he was looking at me and asking how things were going made me realize that he was sizing me up for seagoing qualities. I had a feeling that if I performed no better than Albert's other boys, the news would be all over the ship. The fact is, though, that I won that round. When I started the chlorophyll analyses, which involved extraction of the carotenoids with ether, he left rather quickly. By that time, I felt sure I could do the job that Albert wanted me to do.

We were working west toward the mouth of the Mississippi. Part of the trawling was on the shelf for collections of commercially important shrimp for Martin, and part of it was deep-sea trawling. The latter catches were small but always exciting. This was of course the first time I had seen the strange deep-sea life except as illustrations or faded preserved specimens. They were marvellous. And at night, when the light was turned on for a hydro station or bringing a trawl aboard, the water boiled with hatchet fish, prawns, squid, and other migrants from the mid-depths which were attracted to the light.

The blue water out there was really blue, far bluer than I would have imagined, almost as if dye had been added. I enjoyed lying in my bunk and looking out when the ship was rolling enough for the porthole to go under. Mostly it was an empty, blue expanse, although occasionally I would catch a glimpse of a copepod or some other small creature. Up above, the under surface of the water was silvery and bubbly and ever changing. At night there were stars in the water, mostly dinoflagellates, I suppose, but occasionally there were larger luminescent flashes. In later years, all this became commonplace, but I still remember the excitement of the early cruises and my keen appreciation of the beauty of the oceanic world.

I was beginning to get acquainted with some of the rest of the ship's company. The captain was Fred MacMurray, a paunchy man in his fifties who wore a scowl more often than not. Formerly a freighter captain, he had been busted for going aground. He claimed he was not at fault, but, fairly or not, that often happens to skippers who go aground. He was embittered by that and what he now considered to be an inferior command. He was continually bothered by dyspepsia. As he left the wardroom after a meal and walked aft through the lower lab and on to his cabin, almost invariably somewhere along the way he emitted a gargantuan belch, muttered "Bloody awful gas works," and walked on. He was certainly a top notch navigator but had not had much experience handling sail and was not highly respected by a crew that knew more than he did about it.

The first mate was Tom Kelly, a brusque, efficient, and highly competent man. He was a bit macho and frequently barked at the seamen. They didn't like him very well, although he got along fine with fellow officers and scientists.

The second mate was a thoroughly likeable Dane, Knut Nielsen. He had been promoted from before the mast. He and about half the crew were Scandinavians who had signed on in Copenhagen where the ship was built and stayed with her. They were excellent sailors with real pride in their mastery of every aspect of seamanship, but they were a rowdy lot in port, although mostly they were just half drunk and gay and disorderly rather than getting into serious trouble. Knut, despite his responsibilities as an officer, was no exception. They tell a story about one time when they came aboard high as a kite, and on a dare, Knut climbed to the top of the mainmast, a hundred-forty feet up, and balanced on his belly on the truck, arms and legs spread-eagled.

I heard another story, which was typical of the way they were. This was in Woods Hole, and a bunch of them wanted to go to a beer joint in Falmouth one evening. However, there was a pea soup fog, and they decided it was much too dangerous to drive four miles on the narrow, winding road that existed at that time. So they went over in the Institution launch, the *Asterias*.

Most of them had a good command of English-- with a few Scandinavian touches:"Yig and yib and yumbo." Gradually, they drifted away to other jobs. Knut joined the crew of one of the cup racers. Another married and settled down as a rigger in a ship- yard. Finally, the only one of the deck crew left was old Ernie, the bosun. I can't remember his last name. He was a bumbly, friendly bear of a man who had little in the way of conversation and didn't seem very bright, but he had some nonverbal talents. I recall a cruise somewhere around the mid-Forties when our bathythermograph wire got a bad kink in an awkward spot in the middle. We could lose the instrument if we made any more lowerings, and there was no replacement wire aboard. Ernie said, "I cut and splice." "Ernie," I told him, "this is prelaid cable. They say it can't be spliced." "I splice," Ernie said. Two hours later, we had a long splice so neat that we couldn't find the spot afterward.

I'm getting ahead of my story. There was Hans Cooke, another Danish import who was second engineer, a milder sort of man than most of that bunch. To complete the ship's complement, there were a radio operator, a cook, and two messboys. I really didn't get acquainted with any of them. However, I particularly remember the Philippino cook, who served curry just about every day during the last half of the cruise. The refrigerator had conked out. As the meat got higher, the curry got hotter. After all, that is why things like curry were invented. So far as I knew, there were no ill effects except that the bloody awful gas works made more noise.

I'm still ahead of my story. Before the curry diet started, one morning the hammer on the keel woke me up, and as I looked out through the porthole, instead of sparkling blue water, there was a brown, muddy mess. We had arrived at one of the Mississippi dis- tributaries and the river was in flood. Later, off the mouth of Southwest Pass, I saw the river water curving in a great arc, with a clear demarkation between muddy water and the clearer water off- shore, and heading westward, a diagramatic example of a coastwise density current. We continued west as far as Galveston, and at least that far, the density current was providing a significant freshening of the surface layer, although the mud had sunk and vanished.

During the westward leg, there was a Texas norther, and we were hove to for a day or so. It wasn't much of a storm, but the *Atlantis* was never able to operate station patterns effectively in winds of more than about 40 knots. She was built with a curved and bulging bow, designed to give the bow a good lift over oncoming seas and to keep working spaces on deck fairly dry. This it did, but it also made her relatively ineffective in making headway against a heavy sea. She plunged into the wave with a fast deceleration like an express elevator coming to a stop, lifted her bow high, and slid on over with a disconcerting corkscrew wiggle of her stern. As the sailors used to say, in more explicit terms than I'm using, she'd be a great little woman to go to bed with, but she's no good to go to sea in.

Then there was the roll. With an eighteen-foot draft and a lot of weight in the keel but not very hard bilges, she did a lot of minor rolling but seldom any severe ones. I don't think I ever saw a roll of more than thirty-five or forty degrees. However, she had one very annoying habit. It was a slow, six-second roll, and there are a lot of six-second swells out there. I've seen the old girl lying in a glassy calm sea with a lazy swell rolling in on the beam, each swell reinforcing the previous one until she was taking water through the scuppers with every roll and sometimes in over the rail. All ships have their foibles. I grew fond of the old girl over the years despite her faults.

One major virtue was the fact that the wardroom table was mounted on gimbels. We rolled in rough weather; the table remained level. We could eat soup and drink coffee without getting them in our laps. To be sure we had to sit up straight and keep our legs well back. And incidentally, the ship's cat knew that the weighted beam under the table was the right place for a catnap.

Bathroom facilities were primitive, and the freshwater supply often was limited. There was a laundry tub in the scientists' bathroom, and we could put a little water into that for a sponge bath. After a cruise, a bath ashore was a real treat. We, of course, pumped salt water to flush the head, but in port it went into the bilges, which were then pumped after we got to sea. After several days in port, we were flushing with water that was neither clean nor odorless. This was just one more insult to Martin Burkenroad's delicate sensibilities, and he dubbed the ship the Cesspoolantis.

We arrived back in Mobile. I had sufficient data for a paper demonstrating phosphate enrichment around the mouth of the Mississippi and an associated increase in chlorophyll. This now seems like a trivial and expected result, but at that time, there had been few surveys of a comparable sort, and there were differences of opinion as to the importance of rivers. Some oceanographers believed that river drainage was primarily responsible for the relatively high fertility of coastal waters, a conclusion that later proved to be only a half-truth. On the other hand, some studies of English rivers had indicated that they added little to nutrient supplies in adjacent oceanic waters. The Mississippi River tipped the balance the other way and suggested that broad generalizations were premature.

We all took a trip to New Orleans, where Martin provided us with an excellent weekend tour of the city, and I picked up some river water for analysis. I returned to the ship and did the analysis. Then I shoved off for a visit of a day or so with my family and went on to a job interview in Iowa. This was a small and rather strict denominational college. I would be expected to teach the zoology half of the Biology Department. That was enough like Drury for me to know exactly what I would be getting into, and I was not enthusiastic. I would be expected to go to church on Sunday and never be seen smoking or drinking. The salary would be at least $1200 a year and perhaps as much as $1800 if the corn crop was good. The postdoctoral fellowship was looking very good at that moment. The stipend was only $1200, but I could do what I wanted to do, including limnological research and spending Sunday morning in my usual fashion, reading the New York Times and having an occasional smoke even if someone saw me. I returned to New Haven and accepted with thanks.

There were a few things to clear up-- the final draft of my thesis, final oral exam, and the Mississippi manuscript, which I completed about the time of the Commencement exercises. Then back to Linsley Pond. Evelyn was continuing to do nutrient chemistry. I was to undertake a plankton study which would include counts, chlorophyll analyses, and determination of photosynthesis and respiration in a series of light and dark bottles suspended in the lake.

When Eric Mills was writing his essay for my book of reprints, he asked whether it was Evelyn's idea or mine to do the light and dark bottle experiments. I really don't remember. He suggested my thesis topic. I think it was my idea to analyze the results by multiple regression techniques, although that probably wouldn't have occurred to me without the stimulus of Oscar Richard's statistics course. And I think it was my idea to prospect the plant pigment method, though I'm not quite sure about that. Let me put it this way: I am deeply indebted to Evelyn for introducing me to his own scientific philosophy about ecology, which was ahead of its time and has permeated all my plankton work. He maintained that populations needed to be studied in terms of dynamic processes --rates of production and consumption and the way these are affected by ecological factors. Observations of populations as they exist in successive moments are necessary but do not tell us very much about how they got that way.

As a beginning limnology student, I knew nothing of all this. I might or might not have arrived at the same conclusions by myself. Evelyn was a guiding spirit who made the path easy. As for the particular case-- light and dark bottles-- we were both aware of the use that marine biologists had made of them, and to apply the method of Linsley Pond was the natural thing to do. Who hap- pened to say the word first is hardly memorable. The total association with him was and is.

The description in his essay of that postdoctoral year just about tells it all. For me, it was a period of growth in understanding of plankton problems as well as realization of how much I didn't know. Light and dark bottle oxygens seemed to be useful despite the fact that, as Evelyn said, the exposure periods were unfortunately too long. However, they did not by any means tell me all I wanted to know about phytoplankton processes. In the next job, I wanted bigger and more comprehensive experiments which would include concomitant measurements of nutrient uptake and phytoplankton growth.

The premature termination of the project, when the lakeside cottagers poisoned Linsley Pond with copper sulfate, was a bitter disappointment. I cannot recall whether it was then or a little earlier that Albert Parr offered me a staff position in the Bingham Lab. Anyway, there were no good openings in limnology at the time, so I made another opportunistic leap from one field to another. In some respects, it was not a major leap. Parr was quite willing to have me do the same kinds of things in the ocean that I had been doing in Linsley Pond, and the ecological principles were similar. However, there was much I did not know. Albert's course had given me only a bare introduction, and my oceanographic reading had been confined to papers in current journals that looked particularly interesting. There was a large literature to be assimilated and a new fauna and flora to cope with. I already knew that I had less talent for systematics and species identification than almost anything else. Yet, there were interesting prospects. I had enjoyed my first seagoing experience and would like to have more. Without being fully committed in my own mind to a long-term future in oceanography, I took the job.

Actually, I was about as well prepared as other people entering the field at that time. There was a new graduate program in oceanography at Scripps Institution which had not yet produced any Ph.D.'s. Otherwise, people came in via the basic sciences, generally having worked under someone who was interested in a particular oceanographic specialty and with some knowledge of that field but with little breadth of training.

Perhaps this is a good time to pause and digress a little, reviewing the status of oceanography at that time. Prior to 1930, oceanography was really an infant science in North America. The U.S. Bureau of Fisheries had sponsored some seagoing oceanography such as Bigelow's work in the Gulf of Maine. In Canada, there were Hachey, a physical oceanographer, and Huntsman, a marine biologist, both of Bigelow's generation, who were working out of the Fisheries Station at St. Andrews. There were, of course, the seaside marine biological labs, where most of the people were doing basic biology, but there were some studies of a marginally oceanographic nature at Woods Hole, Scripps and Friday Harbour. In the latter part of the Twenties, the U.S. National Academy of Sciences organized a committee to consider the future of oceanography and ways in which it might be stimulated. The results of the committee deliberations were published in a book by Bigelow-- I believe it was in 1927-- and it led to a large grant by the Rockefeller Foundation to stimulate growth of the field. Part of it went to Scripps, which had been primarily a seaside marine station. It was reorganized as Scripps Oceanographic Institution. They hired Harald Sverdrup, a Norwegian physical oceanographer of international reputation, as Director. He developed a seagoing program and the graduate training curriculum that was mentioned earlier. A second grant went to the Bermuda Biological Station, which also developed an oceanographic program. The third grant established the Woods Hole Oceanographic Institution. It provided money for the construction of the *Atlantis* and a building, and an endowment fund yielding an annual income of about $100,000 a year. That was sufficient at that time for operation of the *Atlantis* and the shore establishment, salaries for half a dozen regular staff, mostly technicians, maintenance staff, and small stipends for visiting investigators.

Henry Bigelow was the first Director at Woods Hole, and he was able to gather a group of summer scientists who represented most of the major specialties. Communication among them broadened their knowledge of oceanography as a whole. There and at Scripps, where they were able to maintain a small staff of resident scientists, oceanography began to be welded into a discipline.

Most of the summer scientists at Woods Hole had graduate students in their home universities and some of them came along to Woods Hole in the summer. They were my generation. I met them in due time, and some of them became friends and working associates.

In the meantime, I had an opportunity to do some field work in Long Island Sound. The U.S. Bureau of Fisheries had established a small shellfish laboratory at Milford, Connecticut, a half-hour drive west of New Haven. The Director was Victor Loosanoff, who had been in his last year of graduate work at Yale the year I arrived there. This lab did a fair amount of basic research on shellfish and their predators, but one of its main functions was to provide practical information for the shellfish industry. They determined the time of spawning and setting of the oysters, so the oystermen would know when to put down clean shells to catch the young spat, and they monitored the intensity of setting of oysters and starfish, the latter being one of the principal predators.

I started to go out with them almost immediately but had collected only a few samples when I decided to postpone that research because of another opportunity that came along. I think it was Albert who first heard about it and passed the information along to me. Some years before, the Carnegie Institution of Washington had established a small marine station on Loggerhead Key in the Dry Tortugas. This was a collection of coral reefs and small keys about fifty miles west of Key West, Florida. It was a great place for research on the kind of fauna and flora available in such places, and a lot of fine work had been done there. How- ever, it was very isolated, no women allowed, and the place had an almost monastic air. As a result, it was less popular than such places as the Marine Biological Lab in Woods Hole and the Bermuda Biological Station and had reached a point where it was mainly populated by a few older men who came down year after year. The Carnegie people were eager to get in some new blood.

I was still a bachelor at the time, had never seen a coral reef, and welcomed an opportunity to do some work on subtropical plankton. I laid out a proposal for a month of phytoplankton research and was accepted. In due time, I arrived in Key West with my equipment and met the station ship, the *Anton Dohrn*, which came to Key West every week or so to take on or discharge visiting scientists and pick up supplies.

The *Anton Dohrn*, built to work in coral reef country, was as funny a little ship as I have ever seen. About seventy feet long and a double ender with a very shallow draft, she was terrifically heavily timbered. The ribs were so close together that the portholes had to be oval. She had all the sea-keeping qualities of an old fashioned, round bottomed bathtub. She got us there, though.

I was doing mainly plant pigment analyses, phosphate, and light and dark bottle experiments. The usual procedure was for me to load my gear in a skiff in the morning and get towed out in one of the collecting launches to a channel buoy about two miles away where the water was deeper than immediately around the island. They dropped me off there and went on to do other work. I tied up to the buoy, did my collecting job, and suspended the experimental bottles. Occasionally, I was picked up by a returning launch. More often, I rowed back.

The quantity of phytoplankton was very small. I had to leave the bottles out longer than was desirable, not because of scheduling problems as in Linsley Pond, but in order to get significant readings.

On one occasion, I went out in the *Anton Dohrn* for collections in deep water in the Straits of Florida. On the way back, we stopped to do some sampling on the reef for another man who was aboard. During the course of that work, we went aground on a coral head with a shuddering thump. The captain was not in the least concerned. That was why the ship was built the way it was, to withstand these inevitable accidents in working in shallow water of variable depth. They put a life boat over the side with an anchor and rope in it, rowed out a little way and dropped the hook, and quickly kedged the ship off with the anchor winch. No problem.

Most of the scientists at the station were older than I, pleasant enough but rather a dull lot. The majority had been there before, some for a good many summers, and appeared to be merely dotting the i's and crossing the t's on old problems. I suspected some might be coming just to get a vacation away from wives and kiddies.

One building housed the laboratory, consisting of one big room where most of us worked and a few smaller private labs. There wasn't much equipment except what we brought with us. The dormitory was in another building-- rows of cot beds and lockers for our clothes, with adjoining shower and toilet facilities. The kitchen and dining room I believe were in a third building although my memory about that is a little dim.

We ate well, with lots of fresh seafood, worked fairly long hours, and had no amusements except any books we happened to bring with us, and a late afternoon swim. We were required to swim only as a group, for barracudas were common.

Barracudas have enormous saw-like teeth and inferior intellects. They are very effective in chasing prey that is trying to escape, but any untoward circumstances throw them into a state of utter confusion. If one appeared, we stood and slapped the water with our hands, which was sufficient to keep them at bay, staring at us stupidly. On another occasion while standing on the dock, I saw one chase a school of yellow jacks. The school doubled back and started swimming around their predator in a tight circle. For a minute or so, the barracuda made hesitant little darts this way and that. Then suddenly it plunged out through the circle, failing to catch anything, and swam away.

Around mid-August, they closed the lab for the summer, and all of us departed for Key West. The intention was to close up before the hurricane season started, though on that occasion one had al- ready come into the Gulf a few days before, and a heavy storm swell was still running. We had a rough trip back in our little bathtub. The cook had planned to end the season with a big steak dinner on the way back to Key West, but the weather was uncooperative. The few who did come to the table gorged on two or three steaks each.

For me, the trip would have been personally worthwhile if only to observe the beauty of coral reefs and their associated colorful fauna, a must for any zoologist. The work had been difficult, for all analyses had been close to the limits of analytical error. There was some question as to the validity of the overly long experiments. Indeed there was some criticism of them then and later. Nevertheless, I felt that the work was worth publishing.

They invited me to return the next summer, but I declined. I was deeply involved in other things and had no good ideas about how to improve on the previous year's work. I can't remember whether that was the last season the station was in operation. It may have continued a little longer, although the Carnegie people clearly felt that it was no longer fulfilling its original function very well. A few years later, it was severely damaged by a hurricane, which terminated its existence for all time.

I went back to New Haven, finished setting up my lab, and picked up the Long Island Sound project again. At that time, the Bingham Lab had left over-crowded quarters in the museum and had moved a block away to a large brownstone house at the head of Hillhouse Avenue. Earlier, that was one of the most fashionable areas in New Haven, but those big old houses were no longer practical for family living in a era when servants were fewer and more expensive. This one had been built soon after the end of the Civil War. Aside from the fact that it was large and thick-walled, it had numerous features that no modern house would have, such as hand carved mahogany woodwork. Also, there were some features that no modern housewife would want, such as a cast-iron, coal-fired cookstove at least ten feet long. The kitchen was in the basement, as was common in those days. The cooks lived with their cooking odors in the basement and sent the food in a dumb waiter to be served in the dining room on the first floor.

I set up my lab in the kitchen and had an office on the second floor, next door to Martin and across the hall from Yngve. Albert had by that time become Director of the museum and had moved back there. He only occasionally came to visit us but was always avail- able on short notice if we wanted to go there to discuss something with him.

I started going out on the Sound again, on the thirty-five foot Milford Station launch *Venus*. She was no beauty and of course had been named for the clam then known as *Venus mercenaria*. The skipper was a Polish American named Joe Lucash. Six-feet five and well over two hundred pounds, he handled fish traps and dredges as if they were toys. Joe had only a high school education, which was a pity. With proper training, I think he would have been an excellent engineer. Aside from being a good skipper, he was a skilled mechanic and shop man.

He kept the boat and lab facilities running smoothly and was clever and innovative in designing and building some of the research equipment that was used in the lab.

Victor Loosanoff, the Director, was another big guy, shorter but stockier and wider than Joe. He had had an adventurous life-- I think-- the stories got more adventurous as time passed.

The essence of his story was that he had been a very young lieutenant in the Russian army in World War I. During the Bolshevik Revolution, he was on the White Russian side. With their defeat, he escaped via the Trans-Siberian railway, and by devious means which varied in the telling, he got to Japan and then to the west coast of the U.S. There he got a job in a lumber camp. One day a Washington University football scout came to the camp looking for recruits for his good squad. Victor wasn't really a goon, but he certainly looked like one. The net result was that he earned his way through university playing football and graduated with a degree in zoology, or perhaps it was fisheries biology, and then came to Yale for his Ph.D. He was of course considerably older than most of us graduate students.

I had to get a car for the trips to Milford. At an earlier time, I had bought Evelyn's old Buick for almost nothing when he got another car. However, it was dying a lingering death. Aside from the fact that it didn't want to start when it was cold nor stop when it was hot, the canvas top was coming adrift and flapping in the breeze, and the wooden frame was too rotten for me to nail it down securely. So for a little more than I had paid Evelyn, I got a 1930 Chevy roadster that served me well for two or three years.

So, I began going out with Joe again and continued until they discontinued their field program in mid-autumn. The little launch gave us a rather rough ride, but we generally got the job done. To be sure, I remember one occasion when we didn't. I started to Milford one morning in a hard driving rain. It wasn't a nice day to go out at all, but I had agreed to come that day. Joe and I started out. As soon as we got out of the harbour it was really rough. He was nosing into it at half speed and only barely making headway. Even so, green water was coming in over the bow and smacking against the windshield. A little more of it and Joe said, "I don't like this. I'm going home." I didn't object.

That was the morning of the notorious 1938 hurricane, which did more damage to Long Island and New England than any other one in historical times. That afternoon as we sat in the lab back in New Haven, we saw a lot of stately old trees come down. The wind and rain let up in the late afternoon, and I went to dinner in a nearby restaurant in the eerie calm of the eye of the hurricane. I should have had enough sense to go to the rooming house where I was living right after dinner, but there were things I wanted to do at the lab. Of course the wind blew up from the other direction. After awhile the lights went out. I stumbled home in darkness that was hardly relieved by the glow of candles in a few houses, climbing over fallen trees, a little scared by the fact that there were wires down.

So that was my first hurricane. The second one was scary, too, in a different sort of way. That was several years farther down the road, and I'll get to that in due time.

At the end of the fall season, I was taking stock and wondering how to proceed further. The seasonal cycle in standing crop did not look good. It varied irregularly from one time to the next. I thought this was due mainly to patchy distribution, and I realized that a more extensive sampling program would be required to average out random variations and get a representative picture of the seasonal cycle. However, that was of secondary importance. My principal interest at the moment was to get a seasonal picture of the inter-relations of the various growth processes as determined experimentally. If I could sample somewhere along the shore during the winter, that should be adequate, only I wanted open Sound water rather than sampling off the dock in some harbour.

Joe Lucash came to my rescue. He knew about a Mrs. Poli, widow of a former theatre magnate in New Haven, who had a fancy villa in the Italian style in Woodmont, between New Haven and Milford, with a wharf extending some distance from the beach. It was a straight shoreline with no freshwater inlets nearby. I went to see her and she kindly permitted me to sample there and to hand my bottles at the end of the wharf. This served my purpose for experimental study, although the apparent seasonal cycle became even more messy.

Also, as it turned out, I didn't get a complete annual cycle. I let myself get interrupted for another open ocean cruise in the late spring, but I came back to it in the summer, and the paper that I finally wrote on Long Island Sound was able to compare winter and summer conditions.

As to this deepwater cruise, Albert in his new position was no longer able to take long winter and spring cruises, and that year the *Atlantis* was going south with Bill Schroeder as chief scientist. Bill was a fish specialist and a long-time associate of Henry Bigelow. He intended to circumnavigate Cuba, making extensive col- lections all the way, and leave the ship in Miami. The *Atlantis* would return to Woods Hole via Bermuda, where they would pick up people for a couple of other projects on the last leg.

Albert's suggestion was that I ask Bigelow's permission to join the ship in Cuba and make stations all the way back, and I was able to get an appointment with Bigelow to see him in his office in the Museum of Comparative Zoology in Cambridge.

This was my first meeting with him and also with Mary Sears, another of his research associates, who was working on plankton. Henry was a little guy, no bigger than I, if as big, but still a bundle of energy as he approached retirement age. He had been an avid hunter and fisherman and skier in addition to his extensive seagoing work. They say he was still skiing at the age of eighty.

He was genial but outspoken, as he was reputed always to be. He was clearly skeptical of the use of plant pigments to characterize phytoplankton. All those species were too different to lump them into a single number. Of course, I argued that the pigment measurement was used in the same way as total cell counts but was better because of the wide variations in cell size. He more or less accepted that but clearly didn't believe that pigments could be used in any meaningful way to evaluate seasonal and regional variations in plankton by their statistical relations with environ- mental factors. In the end, he granted me the cruise, but his parting shot was, "Anybody who thinks he can predict more than 10% of plankton variations is a damn fool, but good luck."

I have told this sometimes as a semi-humorous anecdote illustrating both his bluntness and his willingness to be open-minded about giving people a chance to do what they wanted to do. How- ever, at the time, I was not amused. I had already suffered one rebuff that year when I submitted my paper on Linsley Pond phytoplankton to Chauncey Juday, the Editor of Ecological Monographs. He was willing to accept the descriptive part for publication, but only if I deleted the statistical analysis and much of the discussion. I could discount Juday to a considerable extent. Evelyn had always said that Birge was the brains of the Birge and Juday team, and Juday was unimaginative and stuffy. However, when a man of Bigelow's stature exhibited the same kind of skepticism, I knew I was in trouble. I wanted to develop quantitative analyses further. I hoped to build a solid reputation along those lines, and in the latter, I hadn't succeeded very well.

Albert had suggested that I write a little paper discussing the principles and aims of correlation analysis and use the Linsley Pond data as an illustration. I did so, and he published it in the Journal of Marine Research. He and Evelyn were very supportive; otherwise I might have hesitated to continue along those lines. Or perhaps not. I could be stubborn sometimes, or at any rate so I had been told by my mother on numerous occasions.

Admittedly, my own attitude at that time had gone to the opposite extreme. My only interest was in dynamic aspects of ecological relations. The descriptive work of the older generation, including Bigelow, seemed dull and trivial. Not until some years later did I achieve sufficient historical perspective to realize that Bigelow and his contemporaries were doing work that was innovative in their time and was a prerequisite for everything that followed.

However, I just wanted to pursue my own course and to avoid controversy if possible. I was finding that Martin, and to some extent Albert, had an over-aggressive attitude that seemed unpleasant and sometimes quite disturbing. Both of them loved to argue, in person and in publications. Sometimes it seemed that they got more pleasure out of picking flaws in other people's work and demolishing others' theories than in their own positive accomplishments. That wasn't for me.

So much for that. I made one other trip to Cambridge that spring, which was much more pleasant than the interview with Bigelow. I wrote to George Clarke, a Harvard professor only a few years my senior, asking permission to borrow a quantitative zooplankton sampler that he and his assistant, Dean Bumpus, had developed and which I wanted to use on my spring cruise. His reply assured me there would be samplers put on board for my use, and he invited me to come to Cambridge and accompany Bump on one of his routine trips to Gloucester, where he was making zooplankton collections that spring. I did so and got some valuable experience in shipboard handling of the sampler. I also met Bostwick (Buck) Ketchum, whom I had known only as a junior author of an excellent paper on the phosphate cycle in the Gulf of Maine (Redfield, Smith, and Ketchum, 1937). At that time, Buck was completing a thesis under Alfred Redfield's supervision on the absorption of phosphate and nitrate by algal cultures, and he had some interesting data to show me. All three of these men, within a few years of my own age, later became close associates. Bump and Buck, in particular, have been life-long friends. There will be more about them later.

Meanwhile, my relations with Martin in the Bingham Lab had their ups and down. I liked him a lot despite a good many qualities I didn't like, and we could have been casual good friends had we not been thrown together so closely. As I remarked earlier, he could be a lively, entertaining companion or a nasty son of a bitch, depending on his mood. His moods also were reflected in his work. Sometimes he worked night and day for considerable periods, but he was also subject to black depressions when he didn't come into the lab for some days. In his personal life, he was a Don Juan, charming and attractive enough to make easy conquests during the generally brief periods when he was intensely interested in a woman, but I never knew him to achieve a stable, emotional relationship with anyone.

In contrast with Martin, my sex life at that time was nil. I knew a few girls in the zoology department and the museum and had dated some of them, but the choice was limited. I wasn't very interested. Martin decided he had a duty to change all that. He arranged a double date for dinner one evening with a gal for me who was, he said, attractive and "liberated." She did look like a sexy piece though not very pretty, and she had a lively line of brittle, pseudo-intellectual chatter that I found rather less appealing than dinner conversation with the zoology girls. I didn't try to find out how liberated she was, and Martin, with a few choice nasty comments, gave me up as hopeless.

However, along about mid-March, I acquired another dinner partner more to my liking through another friend, Hal Turner. Hal had been an undergraduate whom I got acquainted with when I was a graduate assistant in his embryology class. He got into graduate school. I saw him from time to time and had been invited to his house a few times. One day, he came into my office and announced that he had become engaged to be married. I duly congratulated him, invited him and his gal, Mary, out to dinner to celebrate, and suggested that Mary bring a friend to make it a foursome.

Mary and Lucy, the friend she brought to dinner, had graduated with master's degrees from the Yale School of Nursing and were working in the New Haven Hospital. They and a third gal shared an apartment near the hospital. Both were attractive. Lucy, dark haired and shapely and with a warm and friendly smile, appealed to me instantly, and more so as time went on. We saw each other frequently during the next six weeks until I went cruising, and--surprise-- I was a little reluctant to go off to blue water.

I went to Miami by train and took a tourist ship to Havana, my first and last ride on a passenger ship. Then I had a rather hair- raising ride in an ancient and decrepit bus eastward along the shore to Matanzas, where I met the *Atlantis*. After a day or so in port we put to sea, where I found that Bill Schroeder, although a nice guy ashore, was the sort who should never be a chief scientist on a ship. You run into that kind occasionally-- the kind who hogs all the ship's time and seldom gives anyone else a chance. We trawled between Matanzas and Havana. After two or three days there, we worked our way on to Miami, and I got so little work done that I might as well have met the ship in Miami.

To be sure, I had the educational but not very pleasant experience of seeing something of life in Cuba under the repressive Batista dictatorship. Of course there was no visible evidence of the arrest and torture of political dissidents. What I saw was rural peasants living in abject poverty, and poverty and vice in the cities. In Havana, there were armed soldiers on every street corner. Wherever I walked, I collected a train of child beggars and frequently was accosted by pimps. Down near the waterfront where we docked, there were more taverns and brothels than anything else, with girls standing in the doorways accosting passers-by and even coming out into the streets to try to pull us in. The ordinary citizens were friendly and often stopped us on the street to talk, seemingly mainly to practice their English. However, what I heard most was, "Meetah, you gimme one penny?" and "Meetah, you want fuckee two dollah?"

I have never understood why the U.S. supported Batista and reacted as it did against Castro, who obviously had great plans for the betterment of Cuba and would have succeeded far better with U.S. help and with economic benefits for both. The reactions of the U.S. were an open invitation to the Soviets. If Moscow had been in charge of directing U.S. foreign policy, I think they would have ordered exactly what the U.S. did. But enough of that.

The aftermath of the shore leave in Havana was not pleasant. Although I ate little ashore, I contracted that well-known tropical malady variously known as the Green Death and Montezuma's Revenge, and for two or three days, I had only enough strength to crawl back and forth between the head and my bunk.

In contrast, the leg from Miami to Bermuda was a pleasure all the way. The ship was a little behind schedule, and the skipper was reluctant to spend a lot of time on station. We compromised on one station a day, which I thought would be sufficient in those relatively uniform waters. So I made a station after breakfast each morning and proceeded in a leisurely fashion with the analyses, generally finishing them by early to mid-afternoon. Compared with most other cruises I've had, this was gentlemanly leisure. We had sunny skies and light airs all the way. I had plenty of time to watch the lazy blue swells rolling past and the porpoises surfing in the wake or lounge on deck at night in the moonlight. This, no doubt, is what some people think the life of an oceanographer is like, and on rare occasions, it is.

This was my first cruise into the Bermuda Triangle, years before some durn fool postulated that it was a perilous place to be. Of course I have been there many time since, and seldom have the seas been quite that tranquil, but I'd rather be there than in high latitudes in winter, thank you.

The only thing that vanished without trace on that cruise was any lingering desire that I might have had for seagoing bachelorhood. I really needed some time to think about that. I hadn't had a lot of opportunities for feminine companionship, or perhaps I should say that I wasn't quite as voracious as Martin Burkenroad in seizing every possible opportunity. Anyway, I was vulnerable-- a gay and attractive gal, a balmy New England spring-- I really needed to get away and try to think about it objectively. That doesn't come easily to a sexually deprived and romantically inclined guy; nevertheless, I felt we had some important things in common. We came from fairly liberal minded, middle-class families that had experienced considerable financial stress during the depression. We wanted financial security and knew how to trim our wants to fit the budget. Neither of us was very religious, but we were brought up to value the old Judeo-Christian ethical principles. These similarities seemed more important than some obvious differences. She was more athletic than I was and more socially minded. She was less interested in academic things. Her intelligence was the solid, common sense variety. It could work out well. As I write this, forty-five years later, I can report that it did work out well.

Now back to scientific reminiscences, which is supposed to be what this is about rather than non-professional things, although I must say that my relationship with Lucy has played a significant role in shaping my career. Long cruises were no longer appealing. I wanted to develop a compromise between sea-going and family life. As it turned out, there were some fairly long enforced separations during and just after the war years. Afterwards, I chose to develop coastal programs involving frequent short cruises rather than long periods away from home.

We survived the triangle, arriving in Bermuda and tying up in St. George's Harbour-- my first visit there. Bermuda seemed more quaint and English that it does now. There were no automobiles. Instead of taxis, there were horse-drawn surreys. A little diesel-powered train with a few open-sided cars ran most of the length of the archipelago from St. George's to Hamilton. Several of us got on it for a short distance to visit the Bermuda Biological Station, which had taken over a fine old hotel on the outskirts of St. George's. Since then, several new buildings have been added, and some balconies have been removed from the main building, but the general appearance has not changed greatly. It was a quieter place then, though, before the airport was established just across the Reach.

Bump joined the ship for routine zooplankton sampling at three locations that had been visited on a number of previous occasions. There was one in the northwestern Sargasso Sea, the other two in Slope and Coastal water. He had been hired two or three years earlier by the Institution to provide technical assistance for George Clarke in zooplankton collections and transparency measurements. A chemical technician named Dayton Carritt (more commonly known as Dave) had been hired about the same time to provide assistance for Norris Rakestraw, a professor at Brown and a summer research associate at Woods Hole. He had accompanied Bump on some of the Bermuda transects but had left after about a year to work on a Ph.D. with Norris. I didn't meet him until sometime later when he came back to Woods Hole for a little while after getting his Ph.D. His principal job on the Bermuda transect was nutrient chemistry.

Bump had a lot of stories about the fun and games that they had on those cruises, and particularly the stopover in Bermuda. The only one I recall now is a cruise when Dave didn't get all of his job done. One of his reagent bottles was poorly secured. It came adrift and smashed on the deck of the lower lab. It was, of all things, the strychnine reagent that he was using for nitrate analyses, which was made up in concentrated sulfuric acid. The deck paint was flaking a little and the exposed steel plates began to sizzle. Dave grabbed a hammer and broke the carboys of distilled water that he had brought along and mopped up the mess. Bump reported that when he finished, his sneakers were falling to pieces and his toes were very pink. Thereafter, Dave spent most of the cruise sunning himself on deck.

Maurice Ewing also arrived in Bermuda for the remainder of the trip to Woods Hole, together with one or more assistants-- I can't recall who or how many there were. That was the first time I met him. Maurice had a Texas origin, as was evident from his speech, and he was then a junior professor at Lehigh University. Primarily a physicist but with considerable geological background, he had become interested in marine geophysics at a time when it was almost a virgin field. In fact, as far as seismic aspects were concerned, he invented them. A year or so earlier, he had published a paper demonstrating that geological formations underlying the middle Atlantic coastal plain extended out across the continental shelf. That was hardly an astonishing result, but it had not been validated until then.

On this cruise, he was preparing to do seismic refraction studies in the deep western basin of the North Atlantic. That was a first attempt, but I was too ignorant then to realize that I was witnessing a historic event. Later, two-ship seismic refraction profiles became commonplace. The earlier exploratory work with a single ship was more difficult. His hydrophones and recording instruments were attached to a buoy which was a flexible pipe about a foot in diameter and fifteen or twenty feet long, capped at each end and filled with gasoline. An anchor dragged it to the bottom, secured by a block of salt, so that when the salt dissolved, the anchor was released and the buoy floated up to the surface.

Ewing was a very bright, hard-driving man who worked around the clock to make the most of the available sea time. However, at meals, he was a pleasant, leisurely companion who was able to ex- plain the complexities of his science to an ignorant biologist in simple terms. This was the beginning of a friendly association that lasted through the years and was fairly close at times, although our paths diverged later.

Bump was a busy guy, too. The little Clarke-Bumpus samplers were easy to manage, but collections at the three standard stations included long series of hauls with big opening and closing nets that had a diameter of 2 m at the mouth and were brutes to handle. He was a tough guy and was equal to it. In his early years, he had polio and still had a slight limp, but the therapy had included a lot of gymnastics, and although not very big, he was heavily muscled.

Bump was gregarious, good natured, and earthy. He had a loud, barking laugh that was heard frequently. He had a considerable fund of old sailors' sayings and rhymes and an even saltier fund of risque stories. As to serious business, one couldn't ask for a better research assistant. He was careful and conscientious in seagoing collections. His scientific training went as far as a master's degree in biology, and he had picked up enough knowledge of zooplankton taxonomy to do routing counts. However, I suspect that he was not deeply interested in biological oceanography. Later, when he became an independent scientist, he abandoned zooplankton and went into descriptive physical oceanography.

The weather continued to be beautiful all the way except for some squalls as we crossed the Gulf Stream, a common occurrence associated with the rapid change in sea-surface temperature across the stream.

I had an old Brownie box camera which took some remarkably good pictures despite its simplicity, and during one of the squalls, I got a good snapshot of the old girl smashing her spoon bow into an oncoming wave and throwing a sheet of water to one side. It was one of my favourite pictures of a typical *Atlantis* performance. I had an enlargement made, and it still hangs on the wall of my office.

The cruise ended. My first view of Woods Hole and the Institution was from the ocean side as we came into the harbour. The view was quite different from what it is now. The Institution consisted only of the wharf and the original building of modest size. The Marine Biological Laboratory was mainly housed in the old red brick building across the street and a little farther west, although there were a few small wooden lab buildings which have since vanished and have been replaced by massive modern structures. The U.S. Bureau of Fisheries Station was in a wooden building near the west end of Main Street, not quite as far as the present, newer buildings.

So I went back to New Haven to write up my cruise results and pick up the Long Island Sound project again. That was terminated at the end of the summer with enough data to write a longish paper, almost a small monograph, which was submitted in due time to the Bingham Bulletin.

In the meantime, the Woods Hole people were planning a major program on Georges Bank. George Clarke was nominal head of the program, although teaching duties at Harvard severely limited direct participation. Bump would be in charge of zooplankton work during term time.

Bigelow's early work in the Gulf of Maine had taken him to Georges Bank on a number of occasions, but the coverage had been minimal. Such data as were available indicated that the spring flowering began earlier and lasted longer than in the deep basins of the Gulf of Maine, and the late spring and summer populations were also larger, with occasional local flowerings in summer. In general, the area appeared to be highly productive, as might be expected in view of the rich commercial fishery.

The main thrust of the new program was to follow the spring period, the most productive time of the year, although the plans also included cruises in September, 1939, and January and

June, 1940. Unfortunately, seagoing needs of other investigators precluded the possibility of getting a complete seasonal cycle or any mid-summer cruises at all.

George laid out a rather detailed network of stations on the bank, with a few in deeper water around the edge, where zooplankton collections would be made with the help of two or three assistants. He invited me to join the program to do plant pigments and nutrient chemistry, and I accepted. With stations coming up every three or four hours, night and day, I knew I couldn't sample more than about half of the stations, but I had to be satisfied with that. It was an excellent opportunity at a time when there were no other immediate prospects for field work. To be sure, for a guy who was courting a gal there would be a lot of reluctant farewells. How- ever, in my more objective moments, I realized that Lucy should know what it was like if she married a seagoing scientist.

Another important member of the team was Alfred Woodcock, who at that time had a staff position as physical oceanographic technician. Al's academic preparation for the job was a high school diploma and one week in an agricultural college. He had no more than started when his father died suddenly, and he quit school to get a job and help support the family. That was just at the time when the building of the *Atlantis* was completed, and a group of young fellows, which included both Al and George Clarke, went to Copenhagen to round out the crew that was sailing her home.

That was an odd assortment sailing before the mast-- those wild Scandinavians and some American youths who included offspring of wealthy Bostonian families as well as ordinary guys like Al. Not that Al was ordinary in any respect except the financial straits that were common in those days. He was bright, one of the keenest observers I have ever known, and full of curiosity about anything that he didn't quite understand. He was quickly promoted from sailor to technician and eventually became a highly respected senior scientist working on problems of air-sea interaction. The seeds of that interest were planted, I believe, one day when he sat looking at a cup of coffee and saw some things that most people wouldn't even notice. More of that later.

George Clarke was a budding young scientist at the time when he helped bring the *Atlantis* over, a member of the new generation that believed in the importance of quantitative ecological methods. In addition to the plankton sampler, he put meters on the larger nets; he built and used a submarine photometer; he and his assoc- iates made experimental studies of grazing rates of zooplankton. Over the years, he accumulated a lot of useful data. However, he was essentially a follower rather than a leader. He was doing things that the Plymouth people had already done, with only slight modification, and his interpretations tended to be pedestrian.

I liked George as an acquaintance, but we didn't become close friends. There was no real warmth of the man, and in some respects, he wasn't a very nice person. He was lazy, always trying to grab a major share of the credit for things in which others-- Bump, students, research associates-- had done most of the work. He was a negligent thesis supervisor, often leaving students to flounder or seek help elsewhere. He thought it was fun to invite a seminar speaker to dinner before the evening lecture and get him drunk, and he gave secretaries and female technicians a hard time. Bump's private nickname for him was Georgie Porgie. Nevertheless, none of that touched me directly, and I was grateful to him for inviting me to participate in a program that looked promising and proved to be so.

The late summer of 1939 was gloomy with war news from abroad. I had been glued to the radio more often than was pleasant, and I remember feeling something of a sense of relief as I loaded my gear into the aging but still serviceable Chevy and headed for Woods Hole. For awhile, I would be too busy to dwell on such things.

We cast off and headed out with a long run to the first station and a favourable breeze. For the first time, I saw the big mainsail go up. The old girl heeled over and took a bone in her teeth. The chug of the engines ceased and there was just a pleasant swish of water along the hull. We were running at about twelve knots, twice as fast as our speed under power. George took a picture of several of us in the upper lab. He held the camera so that the deck appeared to be level, and these weird characters appeared to be in a gravity-defying stance twenty or twenty-five degrees from the vertical.

The next day we were on station and at work. The cruise went smoothly, with good weather all the way. "Clarke weather," Bump grumbled. George of course wasn't able to go to sea except during university vacations, when the weather was fine most of the time. He apparently was not altogether sympathetic and understanding when Bump occasionally had trouble other times, losing gear or cancel- ling some of the program. Bump was hoping for one good blow, for educational purposes. I wanted one, too, to catch up on sleep while the ship was hove to. We didn't get it. I was working about eighteen hours a day and was well done in by the time the cruise ended.

I was doing the usual things-- oxygen, phosphate and nitrate, plant pigments, and light and dark bottle experiments. The latter were 24-hour exposure of surface water in a tub on deck. There was enough plankton to give me realistic looking results in a day's time except at a few stations on the periphery of the bank.

The January cruise was very different from the September one, as might be expected. We frequently were hove to for a day or so at a time. As we drifted astern when we were hove to, we had to spend some time steaming back to station and used up some of our rare and valuable working weather just getting into position. There were time limits on the length of the cruise, and I don't recall why. Perhaps it was a matter of ship repairs or sailors' leave. Anyway, when our working time was cut down, we had to pare the program. On that cruise, we got less than half as many stations as we could have got in good weather. Bump was supposed to send a radio message to George from time to time, reporting progress. There wasn't much to report. On one occasion, George radioed back, "Try harder. Give it the old Harvard fight." I suppose that was intended to be facetious, but Bump wasn't amused: "God damn Georgie Porgie and his Clarke weather. I wish to hell he was out here."

The data yielded the expected results-- a sparse, mid-winter population and photosynthetic rates that were not very convincingly different from analytical error. The results were so nearly uniform that we didn't need a lot of stations.

Of course by that time, I was well accustomed to the do's and don'ts of shipboard analytical procedures. Anyone who tried to pour something out of a bottle held in the thwartship direction rather than fore and aft quickly learns better. Carrying glass- ware around in both hands at once isn't wise. One free hand is needed to grab something in case of a sudden sharp roll. I learned that on the first cruise to the Gulf. I had flasks of seawater in both hands, and the deck was damp and slippery. When I lost my balance. I managed to sit down, and I slid on my tail the full length of the lower lab, from one side of the ship to the other, holding the flasks on high precariously.

The oxygen burette was mounted on a small table beside the steps leading to the after companionway. I could sit on a step, wrap one leg around a stanchion supporting the table, and come what may, I had two hands free for titration. Colorimetry was the most difficult part. The colorimeter was clamped to a bench, of course. It had to be a low, thwartship bench in order for me to see into the tall colorimeter that I used for phosphate and chlorophyll. I stood with legs braced and well apart, clutching the bench with one hand, tilting with the ship's roll in order to look into the colorimeter while I worked the rack and pinion of the tube holding the standard. I think I was lucky to survive the era of visual colorimetry without poking an eye out.

The March cruise started off badly for me. We had left Woods Hole and were well out into Vineyard Sound when the captain ordered the hoisting of all ordinary sail. As frequently happened, the scientists turned out to help. A ship was passing, a little closer than was comfortable. The captain, never really skillful in handling sail, veered sharply and farther than need be, and there was an unpremeditated jibe. I had been in the stern, helping to handle the mizzen sheet, and I didn't get out of the way fast enough as it swung over hard. It caught me and flung me forward along the deck. I somersaulted and landed on the small of my back on one of the big bollards that were used for securing mooring lines. For two or three days my back was so sore I could hardly get around, and I missed the first few stations. Later, x-rays showed no indication of fractures, but those injuries come back to haunt one later. That is one of the places where arthritic gimps remind me of seagoing days.

We had a fair amount of rough weather during the March and April cruises but not as bad as January, and the results were more interesting. The spring diatom flowering was beginning in March in the shallow water in the center of the bank, and in April it was well developed all over. The analyses were easier as the population increased. Bump's life became more difficult, though, as his nets began to clog. As we went on deck for a station, we could of course tell whether we were in the flowering by the appearance of the water, and on a foggy night we could smell it. Bump would shorten his tows, complaining about my damn green glop as he did so.

During the spring period, we also took water samples which we preserved with formalin and turned over to Mary Sears for species identification and counts. I suspected at the time that perhaps Bigelow, suspicious of my pigment measurements, wanted some data of the sort that he was accustomed to seeing. I didn't enquire about it, and I welcomed the counts as a good opportunity to compare the two methods. Also, she was using the same procedure that had been used in recent surveys of the Gulf of Maine, thus providing the only good standard that was available for comparison of Georges Bank with nearby areas.

As I remarked earlier, a good many of the early Scandinavian crew drifted away, and one of the replacements whom I particularly liked was Mandley, the second mate, a rough-tough old seadog and an interesting companion. He had been a whaling captain in his younger days and a rumrunner during Prohibition. He came out of that with about ten thousand dollars and decided to stay ashore and run a garage. Lack of shoreside knowhow and an unscrupulous business partner quickly wiped out the nest egg, and he was back at sea again at an age when the poor guy should have been retiring.

He was a short, burly man and was reported to have been some- thing of a bully on the New Bedford docks, but he had mellowed, and I certainly enjoyed listening to his tales about early seagoing experiences. He apparently had been something of a ladies' man, too, if you can call it that, but time also mellows those urges. He was married to a woman who managed to spend every penny he earned and was sexually demanding as well. He remarked wistfully one day that he had to give up whoring. His wife kept count on him when he came home and would know if he had been playing around. Mandley has long since departed. I still have a piece of ambergris that he gave to me, but it has dried up and lost its delicate aroma.

In contrast with Mandley's lively stories, MacMurray's accounts were never much more than a list of ports of call, cargoes he carried, and the number of days between ports. I kept away from those statistical recitals as much as I could.

Some of the younger sailors felt they were getting a dirty deal bouncing around on Georges Bank instead of going south in winter and sampling the rum and women in one West Indian port after another. The older men who had wives or approximations thereof in Woods Hole liked it that way. Harold Backus was one of those. Aside from having a home and family in Woods Hole, he had a phobia about the cockroaches that always infested the ship during southern cruises. We would go into the galley at night and turn on the light, and the walls were black. Gradually they would turn white as the fauna scuttled into dark corners. Backus continually complained that the food wasn't fit to eat. Surely the cockroaches had walked all over it. They say that on one cruise, he got to the point where he would eat only baked potatoes. He made do with that monotonous diet until even that failed. Some unsympathetic wag cut a slit in one of his baked potatoes and inserted a cockroach.

Harold had his stock of stories, too. One that he told about Parr amused me for reasons that Harold was unaware of. Albert spent some time working at Woods Hole one summer at a time when there was a flock of tame ducks around the Institution. One day, he walked into his room and found that a couple of ducks had come in through an open window, walked around on his table, and defecated on his papers. He went into the secretary's office and announced furiously in his Norse accent, "There are dooks in my room." Helen Phillips, a mild-mannered lady, was thoroughly intimidated and didn't quite understand what he was shouting about. And Harold, telling about this incident in his broad, north country accent, said, "Yu'd think a mon who has been in this country several years wud know how to say a simple worrud like dukes."

I tend to qualify these hearsay stories, for they can be more or sometimes less than the whole truth. There was the time when Harold complained about Mandley, saying he had harpooned a porpoise two years before. And everybody knows it's bad luck to kill a porpoise, and the *Atlantis* had had bad luck for two years, ever since that happened. No doubt the *Atlantis* had a little bad luck just about every year, but I happened to know that the porpoise incident occurred only one year before, on the cruise up from Miami. Thus do myths grow.

I have little memory of most of Bump's assistants on these cruises. They were friends of his on vacation or young unemployed fellows who thought a bit of seagoing life would be fun. Some- times, they found it so, more often not. One who was more interesting than most was a young artist who had recently completed a mural in the Brockton Public Library. He had been supported by the Works Progress Administration, a depression-time organization pro- viding government support for jobless people to do various sorts of public works. His earlier background included a year in art school after graduation from high school and several years' residence in the Provincetown art colony. He also played the violin and at one time was torn between painting and music as his life work. No one at that time would have predicted that Fritz Fuglister's career would eventually be in oceanography, including a term as head of the Institution's department of physical oceanography.

Fritz was bright, articulate, and full of a good stock of anecdotes about a past life that was interestingly different from ours. His father had migrated from Switzerland and worked as a chef in a hotel in Washington, D.C. Fritz earned his spending money and art school tuition as an elevator operator. To hear him tell it, he was a bit of a hell raiser, roaring around Washington on a motorcycle and getting into occasional scrapes with the police. This may have been so but seems a bit at variance with his serious pursuit of music and art. He had similarly wild tales of a hedonistic life in Provincetown, but by the time I knew him, he had settled down to a quiet and peaceful married life, and he and Cecilia, the gentle Quaker girl whom he married, had an infant son.

Incidentally, Fritz was the only person I have known who really liked to sleep in the one-legged bunk on the *Atlantis*, as it was commonly called. It was the mid-ships bunk in one of the port side cabins just abaft the lower lab. The companionway from upper to lower labs cut across the top and one side of the foot of the bunk, severely restricting the space. By jamming one's legs into it, stability and security were improved during rough weather; however, one had to learn to roll over gently with legs well straightened out or it could be almost as damaging to kneecaps as the wardroom table.

A more common solution, the one Al and I preferred, was to roll up blankets and shove a roll under the edge of the mattress on each side of the bunk, bending it up into a shallow U shape. That kept us fairly secure. There was only once when I actually rolled out and landed on the deck. That was in the chief scientist's cabin, which had an unusually wide bunk. I suppose it was intended as a special kind of luxury, but in practical sea-going terms, it was quite the opposite.

There were three more cruises after the April one. In May, we saw the last vestiges of the spring flowering and the rise of the zooplankton population. I missed one in early June. In the last one, toward the end of June, a summer population was established, and at a few stations there was an indication of one of those local flowerings that I had read about.

This was a happy time in my life. Lucy and I had become engaged during the spring. During one of the cruises, I left my car with her, having taken the train to Woods Hole, and she drove up to meet me at the end of the cruise. We stayed with Bump and Kit, a Falmouth school teacher whom he had married the previous summer. They had a party that night and invited Fritz and Cecilia. That was Lucy's first acquaintance with some of my best friends in Woods Hole, who would become her friends too in time.

Aside from these human relations, I felt that the Georges Bank scientific work had gone well, too. Among the young oceanographic contingent most of us were agreed as to the way oceanography should develop. The skepticism of some of the older people became easier to bear.

I started another summer's work at Milford and moved there to be closer to the lab and to Lucy, who had left the hospital and had taken a job as a public health nurse in Bridgeport, some ten miles to the west beyond Milford. With regular hours rather than hospital shifts, she was more available for dinner dates and often tennis or swimming afterward. We spent a good many weekends at her parents' place near Stamford, Connecticut.

Insofar as possible, I had been keeping abreast of the Georges Bank results as they came in and was far enough along with the analysis to accept an invitation to give one of the weekly evening seminars at Woods Hole. George Clarke invited me to dinner. Having been forewarned, I resisted his efforts to get me completely potted and went into the seminar only a little high. I guess I did all right, and I had an excellent opportunity to meet a number of the usual summer visitors who, until that time, had been only names in the volumes of Woods Hole Collected Reprints.

The previous year, I had come in from the southern cruise before the main crowd arrived, and I can't recall meeting any new people except Dick Fleming, a visitor from Scripps. He wanted to tell me about an analytical model that he had been working on, which was intended to simulate the spring diatom flowering in the English Channel. The Plymouth people believed that it was control- led mainly by an increase in the population of grazing copepods. Fleming had postulated an equation for the rate of change of phytoplankton that was determined by a constant coefficient of increase of phytoplankton and a grazing coefficient that increased linearly with time, so that the phytoplankton population in the integrated form first increased, then leveled off and decreased, forming a bell-shaped symmetrical curve.

I wasn't impressed, and I'm afraid my reaction showed, for I was told later that he was disappointed that I didn't seem more enthusiastic. I didn't mean to give that impression, but I couldn't be very complimentary either. It was quite contrary to my point of view about ecological matters.

Many of the people who disapproved of the over-simplification involved in my use of pigments as an index of total phytoplankton were even more simplistic in their consideration of environmental factors, trying to pinpoint some key factor that controlled a population-- in this case, a grazing factor. In my opinion there was a complex of factors involved in almost every observed change, and the whole purpose of my statistical analyses was to try to evaluate their relative importance. As for the equation itself, it was merely a slight adaptation of the Lotka-Volterra prey equation, al- though possibly Fleming wasn't aware of that literature. He could be excused for that, being primarily a chemical oceanographer. I had been through all that with Evelyn and Oscar Richards, and al- though the theory was concerned only with simple populations growing under constant conditions, there were possible ecological applications.

In retrospect, I suppose I can say that Dick Fleming's paper made me think more seriously about that line of approach than I had previously. It wasn't an inspiration as much as a burr under my saddle blanket, but that is an inspiration of sorts. I felt he had completely missed the boat. The important and difficult problem was that the coefficient of increase in phytoplankton had to be an ecological variable, too. I didn't know how to handle that yet, but it was a problem to tuck away somewhere and let it incubate.

That was a digression and a bit of backtracking. The summer of 1940 passed pleasantly enough. I was sampling a number of stations in Long Island Sound and getting a better picture of generalized patterns of variability, but there wasn't anything that seemed significant enough to stand on its own feet as a paper. I simply filed the data for possible future use.

Lucy and I were married in September; a quiet, informal wedding on the lawn of her parents' place with some relatives and a few friends in attendance, including Mary and Hal Turner and Kit and Bump. Our honeymoon was a leisurely auto tour through most of the New England states. Then we settled down in an apartment in Milford, half way between my job in New Haven and hers in Bridge- port. In the morning, I would drive her to the Milford station to catch a commuter train, drive to New Haven, and meet her again upon her arrival in the evening.

I was finishing a manuscript on the first year's work on Georges Bank and also a general summary of regional aspects of phytoplankton production based on all the work I had done thus far. I was also beginning to plan the work for the spring cruises to Georges Bank. Much of it would be the same routine that I had followed already, but I decided that instead of light and dark bottle oxygen experiments at each station, I would do more extensive experiments at a few stations. In part, this was the same as earlier work in Long Island Sound, but also I was pursuing the problem of seasonal succession and inter-specific competition.

The latter was a real puzzle. Simple theory said that when two species are living together in competition for the same nutrient source, the one with the fastest growth rate will eventually eliminate the other. This certainly was not true of phytoplankton in nature where, during the course of seasonal succession, a good many species would be present at any one time, with perhaps two to half a dozen dominating the population. I had a simple working hypothesis that the dominants at any one time were those in which environmental conditions were nearly optimal for their growth, with little difference in growth rates, and that environmental conditions did not remain constant long enough for the classical sort of competitive exclusion to occur. Then, with seasonal changes in environmental factors, these species would begin to drop out and others would achieve dominance as conditions became more nearly optimal for the latter. And so, I wanted to look at growth rates of individual species in the experimental bottles to see how they compared with net changes in nature. They might have some bearing on these problems.

I crossed the street to expound my hypothesis to Evelyn and get his opinion about the program I was planning. He endorsed it fully. Then he threw a monkey wrench into the machinery. If there was a monkey wrench to be found, he could find it as quickly as he could find a reprint that he wanted among the chaotic piles of paper that surrounded him. "How about tropical plankton?" he asked. I knew what he meant. The tropical phytoplankton was more diverse and with less tendency toward a few dominants than temperate and boreal populations despite relatively uniform environ- mental conditions. "I'll have to think about that one," I said. I thought about it for about fifteen years before I went back to him with even a semi-plausible theory.

When I went back to Woods Hole in the spring, there had been marked changes since the previous year. The Institution was in a state of transition from the earlier period, when a tiny permanent staff and a number of summer research associates were expanding the frontiers of pure research, toward a period of tremendous growth with virtually the total effort directed toward applied wartime research.

Bigelow had retired from the directorship, and his young protegée, Columbus Iselin, had stepped into the post. Columbus, a handsome and personable scion of a wealthy New York family had, in his earlier years, picked up a good general background in physical oceanography, although he had not gone on to an advanced degree. He had fitted out his yacht, the *Chance,* for physical oceanographic measurements and had collected a significant amount of data in the western North Atlantic, particularly in the Labrador Current off Labrador and Newfoundland. With the advent of the *Atlantis,* Columbus became the first captain, directing the operations of the ship with the help of a sailing master. He made stations across the Atlantic on the maiden cruise and then embarked on an intensive study of the Gulf Stream system, repeating three standard lines of stations across the Stream on a number of occasions.

Columbus made some very significant advances in descriptive physical oceanography at a time when information on the subject was sparse. However, he was not highly innovative nor thoroughly grounded in theoretical aspects of the subject. I don't think anyone could claim that he was a great scientist, but more important at the time, he was a great leader of men. He guided the Institution through the difficult wartime period, and the even more difficult aftermath of readjustment to a peacetime basis, with gentle firmness. He managed to maintain a smooth running organization and yet somehow could always find time to spend a few minutes discussing problems, scientific or personal, with any staff member who came to him. Or, more often, he toured the building, visiting this lab and that, discussing any subject that happened to come up. Bigelow is reputed to have done that, too, although I wasn't there enough during his directorship to know much about it. Although many of us during that period were required to do things that were far from our areas of principal interest, I cannot recall anyone who did not like Columbus personally and respect his leadership.

Among other things, he was an officer in the Naval Reserve and undoubtedly discussed navy problems with fellow officers from time to time. I don't know much about the details, but he had heard about problems in the use of echo-ranging gear that was being tested for anti-submarine warfare. Frequently, the range was excellent in the morning and decreased drastically in the afternoon to the point where the gear was virtually useless. Columbus guessed what the answer to that problem was. He participated in some tests at Guantanamo Bay-- in 1937, I think it was-- and demonstrated that the reduction in range was associated with diurnal warming of the surface layer, which had the effect of refracting the sound beam downward.

With the advent of war in Europe, there was a stepping up of military preparations for the possible eventuality-- many thought it was a certainty-- that the U.S. would get into the war. Woods Hole got a navy contract in 1940 to work on this problem of echo-ranging forecasts. That was the sort of thing that Maurice Ewing was well fitted to do. He moved to Woods Hole and went to work with a small team of physicists and shop men. Data were available on the speed of sound in sea water and its variations with temperature, salinity, and depth. Application of Snell's law of refraction resulted in formulas for calculating sound-ray paths in terms of depth and vertical temperature gradients. In most cases, salinity could be ignored.

The other part of the program was the development of an instrument for quick measurement of the vertical temperature structure of the water. A few years earlier, Athelstan Spilhaus, another one of the research associates, had invented a device called a bathythermograph, which contained temperature and pressure elements and recorded a graph of temperature against depth as the instrument was lowered into the water. Originally, it was rather a cumbersome instrument. In the hands of Ewing and his group, it was redesigned-- almost re-invented-- and was housed in a streamlined fish which could be lowered underway from a winch that provided a free fall and then pulled it in. It could be used effectively at speeds up to about six knots, and the little smoked glass slide with the recording on it was then placed in a grid calibrated for easy reading. The name was streamlined to BT.

At the time of our 1941 cruises, BT's went with us for routine observations and testing. Maurice also developed an underwater camera which he brought out on one of the cruises. It worked most of the time, and there were interesting pictures of the bottom. We were impressed by the strength of the bottom currents. There were sand ripples on the bottom in depths of as much as a hundred meters. One prize exhibit, though of no scientific value, was a picture showing a haddock that had got caught on a fisherman's longline. Most of all, I enjoyed the opportunity of going to sea with Maurice again and getting better acquainted with him. As usual, he was on the job night and day with little sleep, yet in leisure moments, he was relaxed and friendly. He was obviously very bright and full of new ideas. The fact that he later achieved an international reputation as one of the world's top geophysicists came as no surprise.

Fritz Fuglister assisted Bump on most-- I think perhaps all-- of the cruises that spring. He provided better assistance to Bump than most of the casual helpers, as well as more interesting companionship. Somewhere around that time or perhaps a little earlier, he became a full-time employee of the Institution, mainly working as a draftsman. This was a bit of a comedown for a creative artist, but that's the sort of thing that happens when one acquires a family.

I had an assistant on two of the cruises, too. One of them was Hal Turner. Hal unfortunately had failed his written comprehensive exam for the Ph.D. and was completing a thesis for a terminal master's degree. Later he got a job at Woods Hole, too.

The other was Tommy Austin, who had come to Yale to get his Ph.D. under Evelyn's supervision. He had become a good friend of mine, and although he intended to become a limnologist, he wanted some seagoing experience. He got it, and it wasn't altogether pleasant. It was the roughest cruise we had that spring, which is saying quite a lot for Georges Bank, and Tommy was seasick much of the time.

We finally finished our stations and started home. I hit the bunk, expecting to be in Woods Hole in the morning. When I woke up, the ship was quiet, but going on deck, I discovered we were anchored in Provincetown Harbour. The passage had been too rough for the captain to want to take the ship all the way in. I was edgy. I was due to attend a conference in New Haven the next morning and give a paper.

Reluctantly, the captain ordered a life boat over the side. Most of the scientific party went ashore. We were able to catch a bus to Hyannis and telephone a friend who picked us up and drove us to Woods Hole. Tommy and I went on from there to New Haven and Milford. What with a late arrival and a loving greeting from Lucy, I got little sleep that night and was half asleep during the Saturday morning conference. I can't remember much of anything except that I did present the paper. It was a controversial thing in which I stuck my neck way out with an estimate of the total productivity of the ocean. Subsequently, my neck almost got chopped off, although that was some years later. Einer Steemann-Nielsen was my sternest critic. His results with the carbon-14 method, as exploratory as my own, indicated that the productivity was an order of magnitude lower than my estimate. Subsequent work of course has revealed numerous problems in the use of carbon-14, and the method as he used it erred on the low side. Forty years after that sleepy Saturday morning, there are still no universally accepted methods for measuring oceanic productivity, but those two rash guys -- Riley and Steemann-Nielsen-- approximately bracketed the likely range.

That spring, Bump was carrying on with his routine Clarke-Bumpus sampling and tows with a big two-meter net for epibenthic fauna. He had rigged this net with thick wooden wheels on one side of the rim so it would roll along the bottom rather than digging in, and the opposite side was buoyed up with three spherical glass balls of the sort that fishermen use as buoys on gillnets. The bottom side also had a bunch of frazzled rope ends that dragged over the bottom to shield the net from chafing. Bump frequently had unpublishable names for his gear. This apparatus reminded him, with some stretch of the imagination, of a bawdy limerick about a young man from Dundee, and the epibenthic net was commonly referred to as the Dundee chariot.

George Clarke had also acquired a Hardy plankton sampler, and horizontal tows between stations were added to Bump's tasks. It was a clever but awkward and heavy gadget and hard to handle in rough weather. Bump had no love for it, and the dignified Sir Alastair Hardy would not have appreciated his name for it. It was roughly fish-shaped in a squat and ugly way, with orifices that had no counterpart in nature. Bump invariably referred to it as the fish with the square asshole.

On the one occasion when I met Sir Alastair, I got the impression that he was a pompous stuffed shirt. Admittedly, I don't trust my first impressions and they were perhaps colored by the fact that I didn't have a high regard for his scientific stature. He was chiefly known for his "theory of animal exclusion," which contained a small element of truth but was largely wrong, and for this sampler which was certainly clever and yielded an abundance of descriptive data which added up to nothing, so far as I could see, in the way of significant scientific conclusions. That of course was symptomatic of my prejudice about an older generation that was content merely to compile descriptive information.

My routine station data during the spring of 1941 added little to what I already knew and was only briefly summarized as part of the paper that I was writing about the experimental work. The latter more or less bore out some of the hypotheses I had been considering, although many questions remained about the nature of seasonal succession. Indeed that problem remains enigmatic to the present time.

To round out the study, I also wanted to examine some of the features of diatom growth in batch culture. The classical growth curve was a period of logarithmic growth, with or without an initial lag, followed by an asymptotic approach to a stable population as one factor or another became limiting. However, some of the early culture work at the Plymouth Laboratory indicated that diatoms did not necessarily follow this pattern. Sometimes they rose to a peak and then declined, the cells becoming deformed in senescence, gradually disappearing or no longer recognizable as members of the original culture, although they sometimes could be revived by nutrient addition.

I found a little money to hire Hal Turner to do some isolation work for me. He got a unialgal culture of *Nitzschia closterium* going, and I started some experiments with that and hoped to study some other diatoms later, although that didn't work out for reasons that will be apparent later.

This was really *Nitzschia closterium*, isolated from Long Island Sound, and was quite different from the Plymouth strain that was used in so many experiments, an oddball diatom on a half shell that was later designated as *Phaeodactylum tricornutum*. It had the type of growth curve that I mentioned earlier, increasing to a peak and then declining. In senescence, the spines on the ends were shorter and thicker than normal, and the cell contents gradually shrank to a tiny dark spot and then disappeared altogether. The empty frustules became difficult to see and probably gradually dissolved; I was unable to find as many frustules as there had been in the original population at its peak.

I didn't have any fancy growth chamber for this work of the sort that labs had later. My growth chamber was the wine cellar of the old Hillhouse mansion, a sub-basement room where the annual temperature range was only about five degrees, and nearly constant conditions prevailed during any one experiment.

The *Nitzschia* experiments, those with natural populations, and the observations on the bank all fitted together into a reasonably consistent picture. I finished the experiments and started to write up the work about the time the U.S. got into the war. I hurried along, knowing that for awhile, at least, the Georges Bank program and my career in biological oceanography were in abeyance. In my haste to complete the work, I didn't analyze the data as thoroughly as I might have, and there were several careless little errors that I didn't catch before the paper went to pass. An obvious one and a cause of some embarrassment later was that I misspelled *Nitzschia* throughout, omitting the z.

One other Georges Bank paper was completed along the way. It was a study of the timing of the beginning of the spring flowering in terms of the combined effect of irradiation, transparency, and depth of vertical mixing. There was nothing particularly original about this, for all of these factors had been considered by earlier investigators. However, as so often happened in those days, they had been discussed separately and largely in qualitative terms. They were all inter-related and needed to be put together in a logical, quantitative formulation.

The Georges Bank program had provided me with a fine opportunity to do a job that I felt was my best oceanographic effort up to that time, and I deeply regretted its termination. We had some good data, but there were still serious gaps in the seasonal picture. I had hoped to learn more about summer and autumn plankton. That was not to be.

I had made good friends along the way and had won the respect of people of my own generation. The fact that some of the older people were still skeptical of the value of the kind of work I was doing no longer seemed so important. I had advanced from a tyro to an experienced seagoing oceanographer and had seen about every kind of weather the North Atlantic had to offer, short of a full-scale hurricane. I had clawed my way along an icy deck, hand over hand on the life lines that were strung around. To be sure, the stout old girl was never in any real trouble any time I was aboard. The only really scary moment in all the cruises was the time when we were almost rammed by a freighter in a dense fog. Fog was a real worry in the days before radar, particularly when we were working along the south side of the bank near the shipping lanes. On this occasion, I woke up one morning to the sounds of horns and shouts up above. As I looked out my favourite starboard port, I could see the bow of this monster dimly through the fog, no more than fifty feet away. It rose up and up, towering above us. By the time I got on deck, which wasn't long, the freighter had managed to come to a stop and was backing down, and we were pulling off to one side.

Various small incidents lightened the strenuous and often dull routine. We sometimes put a bottom dredge over the side when we had a little spare time, mostly out of idle curiosity rather than scientific purpose. The fauna was remarkably variable, and we realized that to study it effectively would require a much more detailed station pattern than ours. Occasionally, we got a good haul of scallops, and most of us were of the opinion that fresh scallops on the half shell were tastier than raw oysters.

On another occasion when the dredge came aboard, Bump yelled, "We caught an octopus." The word went forward and men surged up on deck drawing their sailors' knives to do battle with the monster. Then they stopped and grinned sheepishly as they saw that the octopus was about six inches long.

At the opposite extreme from the octopus, there was a rather thrilling moment when a finback whale surfaced along the side of the ship hardly more than ten feet away. It blew its foggy and fishy smelling breath in a cloud that drifted across the deck and lazily dived again. This was my first and only really close view of one of those great creatures, and I have no mere descriptive words to convey the feeling that one gets of their majesty and power when in such close proximity.

Anyway, I was finishing that chapter of my life. Early in 1942, I had three job offers. One was from Uncle Sam to join the U.S. Army. The second was from Columbus Iselin, to come to Woods Hole and work on a Navy contract. The third was from Albert Parr, to become director of the Bingham Lab, for he was leaving to become director of the American Museum of Natural History in New York. I dismissed that offer immediately, although not without regret. Uncle Sam had a higher level of priority. As for the rest, I am not very big or very brave and found it easy to convince myself that I could do a more useful job as a Navy scientist than as a dogface, although this was not without a twinge of conscience then and later.

So I went to Woods Hole. Parr's second choice for the directorship of the Bingham Lab was Dan Merriman, an ichthyologist who had been a graduate student at the same time I was and a student of Albert's and had become an instructor in the department later. His job included teaching classes for premedics. Production of more doctors was another army priority, so he had a deferment.

When I arrived in Woods Hole in March, I was assigned to a Navy contract that had the task of testing anti-fouling paints. Buck Ketchum was directing it. The Navy Bureau of Ships had developed a number of paints which used copper or mercury or both as anti-fouling agents, embedded in matrices of various sorts. Ideally, the metallic compound needed to be just slightly soluble, releasing the poison at a relatively constant rate that was just sufficient to prevent attachment of fouling organisms and lasting as long as possible. Buck had developed a team of biologists and chemists to work on various aspects of the problem. Testing methods included chemical determinations of the rate of leaching of the metal into seawater over a period of time and submersion of test plates into the ocean to observe their resistance to fouling.

This was an important project, for existing Navy paints were not very effective. Heavy fouling decreased the speed of the ships and virtually doubled fuel consumption. Longer lasting paints would improve their seagoing capability and reduce the time spent in the shipyard. The Navy later reported that Buck's project increased the overall efficiency of their ships 10% during the war years.

In addition to Buck and me, the biological contingent included a young microbiologist named Charley Weiss and two or three young technicians. There was John Ferry, a physical chemist who made some significant advances in the study of paint formulations, which previously had been largely a matter of cut and try. Dave Carritt also returned to Woods Hole and joined the project, having completed his Ph.D. in the meantime. However he did not stay there very long, and he left without telling anyone where he was going or what he was going to do. Some years later, we learned that he had gone to that hush-hush project at Los Alamos.

Then a subproject was established to examine fouling on buoys and anchors and to prescribe paints that would be suitable for buoys and anchor chains. Lou Hutchins, a recent Yale Ph.D., was in charge of that, and other Yalies were hired, including Hal Turner, Ed Deevey, and, at a later date, Tommy Edmondson. Examination of the collections eventually became scientifically valuable in terms of zoogeographic data.

I was assigned the job of testing paints for amphibious aircraft. These had to be thin and light applications of paints that dried with a very smooth surface. Mercury compounds appeared to be best suited for airplane paints. I needed to select compounds that were toxic enough to prevent fouling in tropical waters but not necessarily as long-lasting as ship paints.

Lucy remained in Milford for the time being. I rented a room from Fritz and Cecilia. She was then well along toward the birth of their second child. They were renting a house in Falmouth at the time, but soon after the birth of the baby, Betsy, they bought a biggish old house in Woods Hole, where they are still living forty years later.

I went along with them to their newer place, which had room to spare, and established a small, two-room apartment on the second floor. Lucy and I visited back and forth occasionally on weekends, and early in the summer she quit her job and joined me in Woods Hole. She and Cecilia got along fine, the beginning of an enduring friendship. They shared the housework and alternated weeks in getting meals.

Buck had a knockabout-- an eighteen-foot centerboard sloop -- that he put up for sale in order to buy a larger boat. We bought it and had a pleasant time sailing that summer, despite the fact that the currents in the area were strong and tricky. Those who had boats often went sailing in the early evening, but wise people got back to their moorings before sundown, when the sea breeze frequently died. We missed it a time or two and had a laborious row home against the two-knot tide in Vineyard Sound.

The current is much stronger than that in a channel, commonly known as the Hole, which lies between the mainland and Nonnamesset, an offshore island, and connects Vineyard Sound and the harbour with Buzzards Bay. One day, soon after we acquired the boat, we ate lunch on the Institution dock, as many people did, and then went for a brief sail. The breeze was light. We headed out at a lazy pace and then hit a swift eddy where the current was flowing out of the Hole into the harbour. In full view of half the Institution staff on the dock, the eddy spun us around three times before we caught enough breeze to steady up and work our way out of it. In due time, other newcomers of course had similarly embarrassing sailing experiences. They were always good for a few laughs around the Institution.

There were few laughs that summer. The war was going badly. The German submarine menace was at its worst along the coast at that time. On one occasion, we saw some of the results in Woods Hole when a ship docked at the Fisheries pier with a load of survivors, some seriously injured.

Bump was out on patrol boats that summer. He came in from time to time and reported that things were bad. He was never explicit, and I didn't know whether he had got involved in serious action or was just being a little dramatic. Al Woodcock was more matter of fact. He was on a big supply ship, the *Capella*, and he reported that it was a ship, but it wasn't like a ship. Officers had water glasses just sitting on their desks, and they never slid off.

I was still having occasional guilty twinges. I was making good progress in a job that the Navy wanted someone to do, but I was having a pleasant and peaceful summer while fellow countrymen were risking their lives, and some weren't coming back.

I had no real dedication to the job I was doing except to get it done. Yet, I was not really bored. I realized then, as in other jobs during the next few years, that I could get interested in almost any scientific project. The personal factors of curiosity and puzzle solving were key elements in holding my interest, and I wasn't seriously frustrated by the necessity of putting biological oceanography aside for the time being, although the feeling of frustration gradually increased as time went on.

The projects and personnel at the Institution multiplied. As implied above, a number of people in Ewing's project were out on the ships gathering BT data and teaching naval personnel how to use the information. Another group came in, known to one and all as the bang boys. They were very secretive about their purposes and results, and no doubt the latter were important secrets. However, everyone in Woods Hole must have known what they were doing and why. They had large wooden frames with measuring instruments around the edge and small explosive charges in the center. They lowered the frames into the water and set off the charges. Sometimes dead fish floated belly up. Generally, there were gulls soaring in the updraft over the Institution building, and they swooped down noisily to pick up the catch. They were among the few who truly appreciated the bang boys, who were a cliquish, non-oceanographic bunch, and who didn't quite seem like members of the family.

Then there was a subproject to adapt the BT for use on submarines. Alfred Redfield had moved to Woods Hole as Associate Director and was in charge of that project as well as providing some supervision of Buck's group. Redfield was a bright and versatile man and a handsome one. He had been a Harvard professor, who first got involved in marine work as a physiologist working on respiratory pigments. He began to get interested in oceanography as the work at the Institution got underway. He had been senior author of that paper on phosphate in the Gulf of Maine that I mentioned earlier and had also done some interesting work on zooplankton. He followed the developmental stages of pteropods and chaetognaths drifting into the Gulf of Maine via the Nova Scotia coastal current and was thereby able to deduce the surface drift of water around the Gulf.

When I met him in 1942, he was a mature and highly respected scientist in his mid-fifties. However, much as I respected him, I did not find him altogether appealing personally. He seemed just a little arrogant and too impressed by his own importance. There was a time in late spring when he invited me to go on a weekend cruise with him and some of the other younger fellows. He had a beautiful forty-foot Friendship sloop, and their weekend excursions were a common occurrence. Unfortunately, I had to decline, for Lucy was coming up that weekend. When Bump heard about it he said, "Oh-oh, a weekend on the Professor's yacht is a command performance. You won't be invited again." He was right. I wasn't. Nor did I feel much regret. Aside from the fact that I liked Lucy better than Alfred Redfield, I tend to react the wrong way to command performances.

That year, too, he published his paper on oxygen and phosphate in the Atlantic Ocean. He was obviously very proud of it, and it was a good paper, but I wasn't highly impressed. I was careful to sound more enthusiastic than I had in the case of Fleming's paper, but this one, too, fell very far short of what I really wanted to know, namely rates of change rather than qualitative relationships. Although I had no idea about how to do it any better, my respect for him was diminished just a little by the fact that he seemed so satisfied with what he had accomplished.

In these free-wheeling reminiscences, my chronology sometimes goes awry, but I really should mention an event during the late spring, before Lucy came to Woods Hole, that was far more disturbing than my fall from Redfield's good graces when I declined his invitation. Albert Parr had acquired a Navy contract and wanted me to come back and work with him. I told him I felt committed to go on with the job I had started at Woods Hole, but he was very insistent, and I reluctantly agreed to come and discuss the matter with him.

He hadn't yet completed his move to New York, and one Sunday afternoon, Lucy and I went to call on the Parrs at their farm near New Haven. Albert and I discussed his project. What the Navy needed was a predictive capability of waves and swell on beaches under a variety of wind conditions. The purpose wasn't entirely clear at that time but became obvious later. They were thinking about amphibious landings of troops on Pacific islands.

I knew nothing about that kind of problem. Albert thought it could be solved by statistical analysis of data. I had no idea what kind of data would be available or whether the problem could be solved that way. Even aside from my reluctance to leave an unfinished job in Woods Hole, I just couldn't let myself get involved in a job in which there was so little likelihood of a successful outcome. Albert was disappointed and angry, and I felt terrible. I knew that our friendship was likely never to be quite the same again. Albert had a reputation in some quarters of using people for his own purposes and discarding them ruthlessly when they no longer served his needs. In this case, I felt he was trying to use me unfairly, but I was discarding him. I couldn't be loyal to both him and to my Woods Hole commitment, and the latter was the only sensible course.

As a postscript, he hired Tommy Edmondson for the job. The contract folded after awhile, and Tommy came to Woods Hole. At Scripps, Sverdrup had a similar project, and it was tackled on a theoretical rather than statistical approach with reasonably satisfactory results. Most of the work was done by a young graduate student named Walter Munk, working under Sverdrup's supervision. When I became well acquainted with Walter a few years later, I learned that he had used the results for his Ph.D. thesis, and he was rather proud of the fact that he had submitted an acceptable five-page thesis, probably the shortest one on record.

That summer, Martin Burkenroad came to Woods Hole for a little while, and we had him to dinner one evening. Our relationship, which sometimes had been quite close and other times light years apart, ended that evening, for I never saw him again. I learned later from Bingham Lab people that he had a gay time during the war years in pursuit of the wives of overseas servicemen. When the men came home, things got too hot for Martin, and he left town. I heard at one time that he was working in some fisheries lab in Central America, and later I heard that he was in a mental institution in New Orleans. I was unable to verify these stories, but from what I knew of him they were both credible.

Those early years in the Bingham Lab had been pleasant and fruitful, but I was left with bittersweet memories. Whatever Martin's fate was, his brilliant mind certainly never achieved its intellectual potentialities, and Albert's career faltered and faded, too, in later years.

After a delightful summer in Woods Hole, Lucy and I were once more commuting for weekends together. Nurses were in short supply, and she felt she should go to work again. There were no local jobs available, but she got a position in the Visiting Nurse Association in Boston. Most of the time, she came to Woods Hole for the weekend, but occasionally, I went there.

With more spare time on my hands, I continued my self-education in oceanography. By that time, I had picked up a pretty fair knowledge of biological and chemical oceanography from reading the original literature, but my grasp of physical oceanography was meagre. The recent publication of *The Oceans* by Sverdrup et al. provided exactly what I needed. Sverdrup's chapters on physical oceanography compiled the existing literature succinctly and synthesized the material in a way that I never could have done for myself. Without that background, I probably would not have had the courage to stick my inquisitive nose into descriptive physical oceanography, which I occasionally did later.

In the spring of 1943, I completed the testing of airplane paints and needed to move on to something else. I suppose I could have shifted into some other aspect of fouling research, but it was well staffed at a time when the BT program was ramifying along various lines and was taking on new people, most of whom had little prior knowledge of the subject. I applied for a transfer to Ewing's group. I was given a copy of his sound transmission manual and a circular slide rule for computing refraction patterns— Fritz had done the drafting for the slide rule-- and spent a couple of months' apprenticeship helping to work up the records that came in. Around the various submarine bases, practice runs on subs were part of the training for surface ships and subs alike. Some of the Woods Hole technicians were recording echo returns on these runs by hooking a power-level recorder into the sound stack. These records came in, accompanied by BT slides, and they were analyzed to deter- mine the effectiveness of the BT for predicting measured sound transmission.

The office staff was supervised by George Woollard, one of Maurice's geophysical friends who, in pre-war days, had specialized in measuring magnetic anomalies in the earth's crust. He had a battered old van full of measuring equipment, and sometimes after a hard week in the office, he would go off for a harder weekend in the field. A tall and friendly man, George's unflappable geniality almost concealed the fact that he was about as hard working as Maurice. He had lost one eye, and sometimes toward the end of a long day, when his eye was tired and the socket was getting uncomfortable, he would take out his glass eye and lay it on the desk-- a tired, bloodshot eye looking down and a big white eye staring back up.

Maurice was busy devising other projects, some of which I got involved in later in rather minor roles. A major subproject, of course, was Redfield's adaptation of the BT for submarine use. The BT elements were mounted on the outside of the hull, and temperature and depth were recorded inside the conning tower on a smoked 3 x 5 card as the sub made its dive. The primary purpose was for ballasting, although it could be used for sound transmission purposes as well. Accurate knowledge of the temperature structure was helpful in making a quick and easy dive into the thermocline after an attack where, with proper ballasting, the sub could float on the temperature gradient with engines shut down and become less vulnerable to detection by listening gear.

Bump and several other members of Ewing's crew were transferred to the submarine group. In the early summer, I joined another project that was headed by Ewing himself. Numerous ships had been sunk along the coast, some in water that was shallow enough so that the wrecks were possible hazards to navigation. In most cases, there had been radio communication before the ship sank, so that the approximate location was known. Our job was to search the areas with our sound gear until we found them, pinpoint the exact position, photograph them in an attempt to verify the identification, and determine the depth of water over the wrecks.

The program was already underway when I joined it. They had been assigned a 180-foot Coast Guard buoy tender, the *Gentian*, and I joined them at the Staten Island Coast Guard Base. Most of the time, we were a three-man scientific party, the third member being George "Rusty" Tirey, a lanky, red-haired Texas boy who had little scientific training but did an excellent job of taking and developing pictures and keeping the cameras in order.

The atmosphere aboard the ship was not altogether cordial. The old hands didn't like the assignment. They would have prefer- red to continue their usual work of buoy tending, mostly in sheltered inshore waters. They felt that when we were lying dead in the water taking photographs, we were sitting ducks for any German sub that came along. To be sure, the subs were neither as bold nor as numerous as they had been the previous summer. They were still around occasionally, but the war against the U-boats had almost been won. Probably no sub commander would waste a torpedo on our little ship, but we were out-gunned, too, and occasionally, a small ship was sunk, generally a hit and run affair at night. On a few occasions when a sub had been sighted in our vicinity, we were called off the job to join a search party, never, so far as I knew, with any success.

A good many of the crew were green hands, just out of boot camp, and two of the officers were brand new ensigns-- ninety-day wonders, they were called. Their inexperience didn't help the captain's peace of mind. The watch on deck was required to inform the bridge of any sighting on the sea or in the air, and I recall hearing one tyro calling out, "Aircraft bearing-- uh, bearing-- uh -- passed overhead."

My principal job was to take BT's and process the data and keep a record of echo returns when we found the wreck and then help with the camera. The usual procedure was to go to the location given by the sinking ship and then run a search pattern around it of gradually increasing size until we found the wreck. A marker buoy was laid down, and we made a number of passes across with the fathometer to get its profile. Then photographs were shot. There was usually enough information available about the ship for identification purposes. I don't recall the names of any of those ships except the *Coimbra*. It made a particular impression because a lucky camera shot caught the name of the ship on its bow.

We worked gradually south, operating next out of the Norfolk Navy yard and than a Coast Guard section base at Morehead City, North Carolina. While working off the latter area, we located an uncharted wreck and put a buoy over to mark the spot. This was late in the day, and we couldn't do the rest of the operating under blackout conditions, so we drew off to ride it out until morning. When we returned the next morning, the wreck had vanished. Obviously, it had been a sub, which had gone down to sit on the bottom in the hope that we wouldn't recognize it for what it was. And we didn't. So all of us were at least close to the enemy that summer, with ignominious results. After that, when we buoyed a wreck, we dropped a depth charge on the first run across it.

On another occasion-- I happened to be on leave at the time-- they located the wreck of a German submarine that had been listed the previous year as a "probable." The photographs included one rather gruesome one of a partly open conning tower hatch with a sailor's leg sticking out of it.

All of us had short leaves at Woods Hole from time to time for a bit of vacation, reports to the home office, and frequently to return with gear that was needed for replacements. I recall one hot and sweaty August trip back to the ship, loaded down with a forty-pound level recorder and a thirty-pound BT as well as my own bag. The trip was by train to Rocky Mount, North Carolina, and thence by buses to Atlantic, the easternmost tip of the mainland. There I caught a mailboat out to Ocracoke, one of the sea islands where there was a little Coast Guard section base and where the natives spoke a patois so strange that I hadn't the foggiest notion what they were saying. From there, they took me out in a launch the next day to the *Gentian*, which was working off shore.

The trip provided abundant evidence of the rapid acceleration of the war effort. On the train, I was surrounded and jostled by crowds of young service men being transferred from one command to another. On the mailboat, we were unwilling targets of dive bombers from the nearby Marine base at Cherry Point. The bombs were only little bags of flour that burst when they hit the water and sent out a puff of white to indicate where they had landed. Even so, we were glad they hadn't yet achieved pinpoint accuracy. Next morning, I sat at breakfast with some young officers whose daily job was sweeping the tortuous channel out through the protective mine field to make sure no mines had come adrift and worked into the channel. Their's was a potentially dangerous job, and they looked nervous.

My feelings about all this were ambivalent and depressing. Like most other Americans, I felt that the war and the defeat of Hitler were necessary, but I hated war and all it implied. I just wanted it to be over and done with.

A few days later, we met some of Ewing's people who were working on the *Anton Dohrn*. We had seen that tubby little ship come into Woods Hole some months earlier. She had been assigned to the Institution after the Tortugas lab folded and had been used for various inshore jobs. I thought I was done with her and her oval portholes forever, but as it turned out, I worked on her several times before the end of the war. I can't remember what the project was at the time we saw her off Ocracoke. My principal memory of that occasion was that instead of following the secretly charted channels through the minefields, she was sailing blithely wherever she wanted to go. The mines were tethered well below her keel, and her bathtub shape was serving a purpose for which it was never intended.

Our job on the *Gentian* was finished around the end of August. One of the biggest dividends of that summer was the opportunity to get to know Maurice better. My respect and liking for him had grown, despite the fact that I saw foibles and flaws that I had not previously been quite so aware of. He had some of the attitudes as well as the speech of a Texas boy-- racist and chauvinistic attitudes that I found a little uncomfortable, or sometimes just amusing. He was not only a workaholic but liked to be seen as one. I remember one time he came to an evening seminar at Woods Hole dressed in shabby and dirty work clothes, apologizing as he got up to speak, saying he had been working in the shop until the last minute and didn't have time to change. He then proceeded to give a polished lecture, which probably was completely off the cuff but was the sort of thing that most people would have to work out carefully. Another time, some years later, he gave a Sigma Xi lecture at Yale and apologized for a slide that wasn't very good, saying he had been busy and rushed the slides through at 3 a.m. that morning.

I never quite knew what his attitude about women was. There were a lot of them in his life-- three wives, one rather long affair in Woos Hole, and occasional casual pickups-- that much I know about. I suspected he used women mainly for sexual purposes and did not form deep attachments, but I don't know that.

Like a good many other brilliant and egotistic people-- and some who aren't so brilliant-- he had to be the boss. There were some very good people who chose to stay with him and do his bid- ding, but some of the best and most independent ones drifted away. As far as his professional life is concerned, that was probably his most serious flaw. And having said the worst things that I can about Maurice, I'll say again that there are few people in the world whom I liked and admired more.

My next assignment was in Miami and Key West. There was a sub-chaser training base in Miami and a training school for sound men and a submarine base in Key West. One of my jobs was to give lectures in the principles of sound transmission and shipboard demonstrations of the use of the BT. Job number two was preparation of monthly charts of average echo-ranging conditions in the Florida Straits. I was also supposed to participate in some of the practice runs on subs at Key West, fodder for the Woods Hole data bank, plus a few little testing jobs that came along from time to time. For this, I had the grandiose title of Oceanographer of the Gulf Sea Frontier.

For the person with a title like that, the trip to Miami was substandard. The train was crowded with servicemen as usual. They had first priority in the dining car, and when they got through, there was nothing left. I had thirty-six hours or thereabouts on a day coach and not a bite to eat.

I was surrounded by a bunch of young ensigns, and on the last morning, I was aroused by a sudden flurry among them. One of them cried out, "There it is !" They all crowded against the opposite windows looking out. I took a quick look, too. They had seen the ocean. These ninety-day wonders were fresh out of the Great Lakes Naval Training Station, and apparently this was the first time any of them had seen the deep blue sea. As I settled back and prepared to doze again, it occurred to me that perhaps my overblown title was not totally absurd. At least

I had seen an ocean.

They gave me an office at Naval Headquarters in Miami and the part time use of a ship, such as it was. I also had two assist- ants, such as they were, and a secretary. I had to wear a Navy uniform with the insignia of U.S. Technician. I wished I was in Woods Hole. Old jeans and no title. Back there if Columbus wore a tie to the lab, people would say, "Gotta go to Washington again, Columbus? That's tough."

One of the assistants in Miami was a young fellow named Dick Cole. He came from a fairly wealthy family. He had been classified 4F-- excused from the draft because of physical disability. The nature of the disability was never quite clear. It certainly did not interfere with his night life or his sex life, if we could believe the tales he told.

Bill Wood was a rather tall, cadaverous hypochondriac. He complained of indigestion, constipation, headaches, and about every other minor ailment one can think of. Neither of these guys was any use at sea, though in fairness, I'll say that the office staff did a fairly competent and conscientious job of working up the data.

I have a clear and not kindly memory of the ship that was assigned to me. The *Boone*, CG335, was one of the few surviving members of the splinter fleet, those narrow-beamed, 110-foot wooden subchasers of World War I vintage. And anyone who thinks of cruising in the Florida Straits as a leisurely sail through placid blue waters has not tried the *Boone* during the months of October to March. Those winter nor'westers come booming down through the Straits, bucking the current and kicking up a devil of a short, sharp swell.

Some of the cruises were a real trial. We made a series of profiles across the Straits for periods of a day and a half or two days, and the only sleep I got was a nap of perhaps an hour between each profile. I needed both hands to reel in the BT, and in rough weather, I had to ride on top of the winch, gripping it with my knees, in order to bring the instrument in and grab it as it came aboard. The cooks couldn't cook, and we broke a lot of crockery on some of those cruises. The sailors complained-- "Horse cock sandwiches again." Nobody loved me. Once the weather got to be just too much, and as we headed home, I got a bunk to sleep in. It was damp, and occasionally I felt a drip on my face. By morning light, I could see cracks between the deck planks, opening and closing as the ship rolled. I had a distinct impression that the *Boone* was about ready for retirement and was not at all sorry when we were later allowed to use a 125-foot minesweeper, a beamier and more stable ship, for our work.

There were some changes in personnel as time passed. Dick Cole was assigned to another project. No longer under my critical eye, he acquired an expensive and handsome tailored uniform and a floppy cap of the sort that was common among air force officers and some admirals and generals. No doubt this was helpful in pursuing dames, but some of his peers tended to speak of him as the Herman Goering of the U.S. Technicians' force.

His replacement was a young fellow named Valentine Worthington who also was rather fond of the dames but was much more capable in every aspect of the job, including seagoing work. Val was one of a number of people who had been hired by the Institution with no particular qualifications except energy and intelligence. later he came close to being drafted, possibly because his background, a bachelor's degree from Princeton with a major in Greek, didn't convince the draftboard that his scientific capability was essential to the war effort. However, he enlisted in the Navy instead, and after going through boot camp, he was assigned to the Institution again, presumably with a little wangling on their part. So he was sent down to Miami to work with us. Eventually, he worked his way up to a permanent staff position and, like Fritz, served a term as head of the department of physical oceanography. I suppose one might say he had a better background than Fritz had, for he already knew all those little Greek squiggles that physical oceanographers use.

Of particular importance to me personally was the arrival of Lucy in December. Poor woman, I had been moving all around, and she was reluctantly giving up jobs to follow me when there seemed to be some likelihood that I would be in a particular spot for some time.

We settled in together in a rather makeshift apartment on Belle Isle, one of the artificial islands on the Venetian causeway between Miami and Miami Beach. Some time before, F.G. Walton Smith, a professor at the University of Miami, had acquired space on Belle Isle to start a marine laboratory. It was the tiny forerunner of what would eventually become a big oceanographic institution on Virginia Key. Buck Ketchum had rented space for testing panels of anti-fouling paints under virtually tropical conditions, and one of his assistants, Charlie Weiss, was running that operation. There was a carriage house associated with the lab-- it was all part of a big old estate-- and I was able to rent an apartment on the second floor of the carriage house.

Part of the island was adjacent to the open waters of Biscayne Bay, and that led into one of the Miami Beach canals. There was abundant sea life in the water and a grove of coconut palms on the island. It was a beautiful setting, although the apartment itself wasn't much. Many years later, Lucy and I revisited it and were appalled to see that all that had vanished and the whole island had been converted into a massive resort hotel, just another one of the hundreds of such buildings that cover most of Miami Beach.

This was Lucy's first trip south, and there were lots of novel sights for her to see. Christmas was novel for both of us. Christmas lights on palm trees seemed rather strange.

For awhile, we just enjoyed being together again. When I needed to go to Key West for a few days, she went with me. We stayed in La Concha Hotel, the town's main hostelry, and sampled turtle and conch steaks and other exotic specialities served up in the mainly Cubano restaurants. During my first visit to Key West a few years earlier, it had been a sleepy little fishing town. Now there were crowds of noisy sailors on the streets, and the bars were doing a booming business, but there was still much of the old flavor imprinted by the Cuban immigrants.

We had been married a little over three years and were beginning to want to start a family. My job seemed secure, a turning point had been reached in the war, and an Allied victory was almost certain, although it was likely to be some time in coming. There was nothing to prevent us from following our private plans except that, after a couple of months, Lucy still wasn't pregnant. Once more, she went to work, this time in the Visiting Nurse Association in Miami. Then she did get pregnant immediately but continued to work until midsummer.

Sometime during this period-- I can't remember just when-- I acquired a new assistant who was also an old friend. Tommy Austin, who was an expert ham radio operator, had quit graduate school soon after the beginning of the war and had been teaching budding radio operators at the Great Lakes Naval Training Station. Then he applied for a job at Woods Hole, and after a bit of indoctrination, was sent down to work with me. The job was expanding, particular- ly at Key West, and there was a need for someone else with more scientific background than my other helpers. He moved down with his wife, Jane, and young son, Herbert.

Tommy was a good helper out at sea as well as in the office. He was overcoming his earlier tendency toward seasickness. We had one or two cruises on the *Boone* that gave him a hard time, but on the minesweeper and practice sub runs in Key West, he was a real asset.

I had another brief but pleasant association with Maurice Ewing that spring. He turned up in Miami to test one of his new ideas, a method for long-range underwater signaling. This system, which later became known as SOFAR, involved the explosion of a small bomb in the deep sound channel in the sea, which could be heard-- he believed-- at a tremendous distance by a hydrophone placed at the same level.

That requires a little explanation. In the main thermocline, a sound wave travelling horizontally will be refracted downward by the temperature gradient. In deeper water where the temperature gradient is slight, the pressure effect causes upward refraction. Thus, at a depth of a thousand meters or a little more, there is a so-called sound channel where the sound waves travel almost horizontally, with just slight upward and downward perturbations. Ordinarily, a bomb blast would produce a pressure wave spreading radially in all directions. In the sound channel, the portion that starts out horizontally is confined to a narrow vertical plane, and the only spread is horizontal, the net effect being that the decay in intensity is proportional to distance, whereas in uniform dispersion in all directions, it would be proportional to the square of the distance.

For this test, Maurice had arranged to have bombs dropped off Bermuda, built to explode at the depth of the sound channel. He would be in a ship off the north side of the Great Bahama Bank, with a hydrophone at a proper depth to record the transmission.

During the war years, the Coast Guard had taken over a number of private yachts for minor duty, and Maurice managed to get a rather nice sailing yacht for this trip. He invited me to go along to do the subsidiary oceanographic work. The cruise lasted about a week. It was a pleasant sail and a rather exciting experience to witness the beginning of an important new venture.

The test was a complete success. There was radio communication with the Bermuda ship so that we knew when the bomb was exploded and when we might expect to receive the signal. Maurice and his gang were ready with a recording oscillograph to photograph the transmission, and there was an audio attachment. The sound began with some low rumbles like distant thunder, building up in intensity in a few seconds to an abruptly ending crescendo. The latter of course was the transmission through the sound channel. The earlier rumbles were sounds that had travelled part of the way through the bottom, where the speed of sound was greater, but so was the attenuation.

In later developments of the SOFAR system, permanent listening stations were established off Bermuda and other locations. By determining the difference in timing at the various locations and triangulating, an accurate determination of the position of the explosion could be obtained, thus allowing a ship to signal its position without being informative to anyone who wasn't supposed to know. That was what the Navy was paying for. In one *tour de force* to establish long range effectiveness, they later picked the longest possible great circle course from the Bermuda station, an area south of Australia (look at a globe if you're skeptical), and a successful transmission was obtained over a distance of 14,000 miles.

There was fun on the cruise as well as science. The sailing was good and so was the fishing as we returned along the northern edge of the Bahama reef. I remember one evening when we had a tasting party with fifteen varieties of fish on the table.

I'm not an ardent fisherman myself. As a youngster, I fished the Ozark streams that were mostly fished out, and people rarely caught anything but crayfish (known locally as crawdads, of course). We used them for bait or tossed them back. Not until some years later did I learn that they are much tastier than the fish I didn't catch.

On this occasion, I put a line over the rail just once. We were cruising along on light airs at 3 or 4 knots. I hooked a big one and had a hard job hauling it in. When I got it aboard, I found I had caught a good big grouper. On the way up, some big guy, probably a barracuda, had sliced it off just behind the gills as neatly as one could do it with a carving knife. The fish head that I hauled aboard had widely gaping jaws. Without the fish that normally went with it, it still made a good sea anchor. I quit in disgust.

In the early summer of 1944, submarines were being built at a rapid rate and were going through Key West in increasing numbers for a few training exercises on their way to the Pacific war theatre. There was enough doing to warrant having a man there full time. Lucy's pregnancy had reached a point where she was ready to quit her job. We packed our meagre belongings in our car and headed for Key West, leaving Tommy in charge of the Miami office.

Parenthetically, I had left the car in Woods Hole when I went to Miami. There was no way I could get gas ration coupons for that kind of travel. However, a little later, a navy lieutenant, whom we knew and who was being transferred to Miami, drove it down with his family and delivered it to us. He had a priority for coupons. The tires were getting bald, and tires were rationed, too, but there were a few more miles left in them-- we hoped.

We found an apartment in Key West about three blocks from the north beach and settled in. I was working on surface ships and subs, and I confess I didn't like the latter. To be sure, a submerged sub never feels rough weather the way a surface ship does, but in cramped quarters and never quite getting a feeling of where we were going, I was in a state of near claustrophobia much of the time. It was like going to sea in a dark closet, much worse than Georges Bank in a pea soup fog.

Key West in summer was hot and humid. I had a three-month chronic case of heat rash. Leather shoes stowed in the closet for any length of time sprouted green fur. Lucy, in an advanced state of pregnancy, suffered particularly from the heat. Evening swims in the milk-warm water at the beach didn't help much. Sometimes we would get up in the middle of the night and take a shower. That didn't help either. The perspiration was starting as the bath water was rubbed off.

The summer passed. Around mid-October, we began to pack up for the return to Miami. Lucy was due to deliver in another three weeks. Lucy's doctor was there, and hospital reservations, and baby clothes. We were all set to go on the morning of the 18th. On the evening of the 17th, a heavy rain set in with gusty squalls. A hurricane had been reported well to the southwest. Hurricane warnings were not very good in those days. Obviously, it was speeding up.

About midnight, Lucy went into labor. Nowadays, women who are near term go into hospitals at the first hurricane warning, but that phenomenon was not well known then. I drove out to the Navy hospital in the rapidly intensifying storm but was unable to get her admitted there. Returning to town, I got on the pay phone at La Concha and was able to locate a doctor who directed me to a small, nearby hospital. It really was nothing more than a little infirmary of the most primitive sort with a single old nurse on duty who flapped around in carpet slippers and looked none too clean.

Fortunately, the doctor was young and capable. Our daughter, Louise, was born around five o'clock that morning, and all was well. Later, I went out into the howling storm. The car, parked nearby, had two flat tires. My wild dashes the previous night had been too much for it. I fought my way across town to our apartment dodging flying branches and other debris. Breakfast that morning was a banana and a coke, the only remaining contents of the refrigerator, which had been well cleaned out preparatory to our departure.

I was able to pick up a few things for Lucy and take them back to the hospital, but the shops were all boarded up, and I had to wait until the next day to buy some baby supplies. In the mean- time, I checked into La Concha to wait out the storm.

When Lucy was released from the hospital, we went back to Miami. The Austins took us in, and we stayed with them for a month or more while I wound up my affairs and turned the management of the office over to Tommy. Our much mended tires had breathed their last. We sold the car and returned north by train.

In the early days of 1945, the Allies were clearly beginning to win the war in Europe, and we all were wondering what would happen to the Institution and its swollen population when the war ended. At the moment, it was still well supported by a variety of Navy projects. My first job when I returned was a quiet one compared with the work at Miami and Key West. The Naval Academy wanted a small text on physical oceanography written in fairly nontechnical language. Columbus assigned that job to Fritz and me. Possibly our chief qualification was that we didn't know enough about it to be technical.

To be sure, Fritz was in the process of becoming a physical oceanographer. He had been put in charge of all the data files, BT card files and hydro station data, and had a small staff that was organizing and compiling the information in ways that would be useful later in preparing distribution charts and such. In his spare time, he was reading a lot of the literature and studying mathematics and making good progress. I think eventually he got a pretty good grasp of the whole field but his primary interest was in the real ocean and its descriptive details. He was always a little skeptical of the simplifications in the models developed by the theoreticians. I recall one day when he was reading one of those and tossed it aside and said grumpily, "Pooey. Someday, I'm going to write one of those. It starts out like this: Assume a topless, bottomless, sideless dry ocean." I think that's as far as that paper got.

I was thoroughly enjoying a quiet life, writing that manuscript and settling into the life at Woods Hole where we had so many friends. The housing situation was tight, and the best we had been able to do was a tiny cottage on the shore of the Eel Pond in the rear of one of the large houses that faced on School Street. It was heated by a coal stove, and baths were in a tin tub. For the time being, though, it was comfortable enough.

However, I was temporarily interrupted before the book was finished to provide assistance on another job. This was a project to study sound transmission off the mouth of the Mississippi during the spring period when a sharp salinity gradient interfered seriously with echo ranging. I arrived there in late March when the project was already underway. The ship was another one of those yachts that the Coast Guard had acquired. The scientist in charge of the job, a fairly new employee whom I hadn't met before, was a young fellow named Henry Stommel.

Hank had gone as far as a master's degree at Yale, with a good background in mathematics and physics, which put him well above the class of most of the recruits whom I've mentioned. He was also interested in the ocean, and while still a student, he had written a small layman's book on whales. In later years, he expressed a bit of embarrassment about that early publication, but I had a look at it once and found it well written and informative.

The crew was a lazy, sullen bunch. They didn't like the sea duty, which admittedly was a bit rugged on the little yacht, and they didn't like being stationed in an isolated Coast Guard section base on the lower delta that offered little in the way of entertainment ashore. Their attitude toward Hank was hostile. I suppose this was mainly because he was the boss, dragging them into a job they didn't want to do, but they showed obvious irritation with about everything he said or did. Hank was full of energy and good humour. He generally came down the companionway with a rush, landing on the deck of the main cabin with a thump. His laugh was boisterous and explosive. They glowered.

I took an instant liking to Hank. He was obviously very bright and was brimming with curiosity about all sorts of things. This was a time when I was hoping that someday soon I could get back to biological oceanography. Just as a start, there should be more to be got out of that Georges Bank data set than I had achieved thus far. The results of the multiple correlations had been disappointing-- empirically good but providing little insight into basic processes. Having been the first to use that technique in ecology, I was now prepared to be the first to abandon it as a dead end.

I was still interested in trying to apply the prey-predator equations in a more detailed way than Fleming's simplistic model. They provided a logical framework for a linear food chain, and although that was simplistic, too, it would have to suffice. I toyed with the idea of an analytical solution and give it up as hopeless. It was probably impossible and was certainly beyond my limited mathematical ability. I needed to settle for a numerical solution.

I discussed a lot of these things with Hank. He knew little about biological oceanography at the time, but his natural curiosity and quick grasp of problems made discussion easy. I don't think he made any really helpful suggestions, but thinking out loud with a good listener was helpful to me, and our talk led to profitable collaboration at a later time.

We were at that little section base when the news came through of Franklin Roosevelt's death. It was a sad moment for me. I had regarded him as one of our greatest presidents and something more than that. Coming in as he did in the worst days of the depress- ion, his courage and confidence-- "There is nothing to fear but fear itself"-- had put faith and hope into millions of hopeless people and had started us on the road to recovery. With equal courage, he led us to face the reality of war that no one wanted but was becoming inevitable. I remember remarking to Hank that I felt as if I had lost a favourite uncle. But that damn crew-- they were glad the old bastard was dead. They had a near-perfect record for being stupid and wrong-headed about everything.

We finished our job and went back to Woods Hole. I started working on the Georges Bank data again. It was an extra-curricular activity of course. On the job, I was finishing my part of the physical oceanographic text and occasionally working on other projects. One of these was a series of tests to determine whether the SOFAR system could be used effectively for coastal waters. That involved a series of short cruises of a day or so. We found that successive bounces of the signal between surface and bottom attenuated it so rapidly that the system was impractical. I wasn't sorry when that project folded quickly. Tommy Edmondson and I had been assigned to the shooting ship, and neither of us was as nonchalant about those little quarter-pound blocks of TNT as the geophysical boys were.

On another occasion, I went to the Portsmouth Navy Base with a group of electronics experts who wanted to get some very detailed records of echo returns from a submarine at all angles. The sub hovered at periscope depth while we circled it a number of times, pinging all the way. I had been sent along to do the BT work. However, it turned out that none of these electronics people, who knew infinitely more about sound gear than I did, had ever manned a sound stack. After seeing them mess it up for a few minutes, I took over and finished the job.

I have only fragmentary memories of what I was doing at the Institution at that time. In off hours, Bump and I put some of our Georges Bank data together and produced a paper on phytoplankton- zooplankton relations. I went on from there to an analysis of the seasonal cycle of phytoplankton. The first section was a straight statistical analysis, demonstrating that correlation techniques provided a good empirical fit for the data, indicating that the important ecological variables had been identified, but it did not provide much insight into the way the variables operated. The second was a numerical model based on the prey equation. Insofar as possible, I was using both seagoing and lab experiments to derive the necessary physiological coefficients, but the data were so fragmentary that I had to introduce a lot of arbitrary assumptions. My modern standards, the results look primitive, but in the context of what was known at that time, it required more hard cogitation than anything else I have done.

The end of the war that summer was a blessed relief to all. Lucy and I, after our numerous enforced separations, had finally settled down into family life together, sharing the delight of watching Louise grow and develop. We even had a little garden that summer. In the autumn, we were able to acquire an apartment that was more spacious and comfortable than the cottage on the Eel Pond. Then a few months later, a house came on the market that seemed within our means, and for the first time, we became home owners. It was a well-built, two-bedroom bungalow with an attic suitable for later expansion, located on what was then called Pleasant Street, parallel to Buzzards Bay Avenue and one block farther up the hill. Our good friends, Buck and Brookie Ketchum and Hal and Mary Turner, lived nearby. We settled in for what we thought would be a nice, pleasant winter.

One morning in January, Columbus walked into my lab and said, "I've got bad news for you. You're going to sea again, for quite awhile." He explained further. This was a command performance. The Department of Defense had decided to conduct two atomic bomb tests at Bikini, a Pacific atoll that I had never heard of, in the Marshall Islands group. They wanted a preliminary survey of currents in and around the atoll in order to estimate how quickly the radioactive products would be flushed out after the blasts, and there would also be studies of fauna and flora and reef structure. He wanted me to head up the physical oceanographic team, and no amount of pleading of my complete ignorance of all things physical swayed him in the least. I was the senior member of the group that he had picked, which would include some good young physical oceanographers. I knew my way around through the red tape of requisitioning things in naval bases-- we would need to pick up various things in San Francisco and Hawaii-- and he felt I was the one to coordinate the program and write the final summary report. I was stuck with it.

The Woods Hole group included Hal Turner, Tommy Austin, and two others whom I haven't mentioned before-- Bill Ford and Bill Von Arx. Bill Ford, Canadian born, with a Ph.D. in physical chemistry, had been working in Woods Hole on the development of a conductivity meter to be included in the recording BT packages for subs so that data on vertical salinity gradients could be included in submarine ballasting procedures. He would be responsible for computing ocean currents in deep water around the atoll and setting up a lab for salinity measurements. Bill Von Arx was one who, like Hank Stommel, hadn't got quite to the Ph.D. level but had a good physical background and had developed a good little current meter that would be useful for studying water exchange through reef passages. In addition to these, Walter Munk would join us in San Francisco.

During the next few weeks, we gathered and shipped oceanographic equipment. Early in February, we shoved off for the west coast by train. The night before we left, a bunch of friends staged a farewell party which, unfortunately, was the nadir of all parties. That day, we had taken the last of a series of immunity shots which included, in their entirety, smallpox, tetanus, typhoid, typhus, cholera, and bubonic plague. I don't think we had quite all of those that day, but they were enough. By nine o'clock, we were in a state of torpor, and our poor wives bundled the bodies home.

This was the first time I had been all the way to the west coast although Lucy and I had got as far as Denver and the Rocky Mountains a year after our marriage, when we visited my family and went on to Denver to see some of her relatives. On that occasion, they had taken us into the mountains and had driven to the top of Mt. Evans, a grand and beautiful view on a warm, sunny day. In midwinter, the sight was very different, beautiful but in an awesome and foreboding way-- snow swirling in the biting wind and the rugged, rocky terrain black by contrast. That evening, I went out to the rear platform for a better view. In ten minutes, I was shivering in the bitter cold and was glad to step back into warmth -- and safety. I couldn't help thinking what it might be like for a poor traveller who got caught in the mountains by an early winter storm.

The third morning, we were passing through the rolling hills east of San Francisco, velvety green from winter rains. Several months later when I next saw them, they would look brown and dead. Before the end of the day, we arrived at the base and the ship that would be our home for some time. It was the *Bowditch*, a ten-thousand ton converted freighter that had served as supply ship and mother ship for minesweepers and other smaller Coast Guard ships. They and the other ships I worked with later had only recently returned from the war in the Pacific to-- they supposed-- local duty and lots of time in port. Naturally, they resented going right out there again. Although they were by no means as sullen and unpleasant as that Mississippi crew, the discontent was frequently evident.

Walter Munk arrived the same day. I had heard a little about him before, but this was our first meeting. He was a soft-voiced, friendly fellow with a faintly foreign accent. Our acquaintances there ripened into a long-term friendship although, with the width of a continent between us, we have not often seen each other.

There were two officers serving as liaison between the scientific party and the ship's officers. One was Commander Clifford Barnes, a tough seadog who had worked for some years on the North Atlantic Ice Patrol. They did their work on a 125-foot Coast Guard patrol boat, which can give one a much tougher beating than we ever got on the *Atlantis*. I had met him before, but only barely. Later he became a professor in the Oceanography Department at the University of Washington. The other was Marston Sargent, a lieutenant commander in the naval reserve and marine biologist on leave from Scripps. He had worked on phytoplankton and attached algae. Although a good scientist, he was not highly motivated toward research and published little. Probably he has been forgotten now except by fellow scientists and students who profited from the association.

Among the other members of the scientific party was K.O. Emery, a young sedimentary geologist whom I had met in Key West. Most of the others were strangers, although some of the names were familiar. There was Martin Johnson, one of the authors of *The Oceans*, a descriptive zooplankton man of the old school. His chapters in the book were mundane, and he was a rather dull person. However, he did an adequate job at Bikini, demonstrating that the species composition in the lagoon differed significantly from that of the surrounding waters, indicating that the flushing rate was slow enough to permit the maintenance of a unique population. Two other older members of the party were W.R. Taylor, an algologist, and Leonard Shultz, an ichthyologist, distinguished in their specialties, but hardly popular with the rest of us because of their continual nitpicking criticisms about all the minor discomforts of shipboard life.

Enough. I won't go through the whole list, which included geologists, biologists specializing in various aspects of reef populations, and three professional tuna fishermen. We were quartered in rooms originally intended for petty officers, one notch better than crews' quarters. They were long, narrow rooms with tiers of bunks on one side, three bunks high, and clothes lockers on the other side.

In a day or so the *Bowditch* slipped her moorings and headed out into the stormy winter seas of the North Pacific. Two outgoing ocean tugs could be seen nearby, rolling and pitching wildly. Our ship plugged along at her usual speed of ten knots with a barely perceptible roll. I remembered Al Woodcock's story about water glasses on the desks of the *Capella*. I was on *that* kind of ship. Many times thereafter, I wished the *Bowditch* was doing the open ocean surveying around Bikini, but that job would be relegated to lively little girls while the big one sat at anchor in the lagoon playing mother to the work parties that came and went in smaller craft.

We went to Hawaii and tied up in Pearl Harbour for a few days while we scrounged dozens of items from the Naval Base: material for current poles, dye packets and photographic equipment to measure current flow over the reefs, batteries, lights, radar targets, buoys, anything they had that we had been unable to gather together in the earlier scramble to get the show on the road. There was only one evening when we felt we could spare the time to go into Honolulu to sample the island food. Then we were off again, over a blue sea that looked the same day after day, until finally, a month after leaving San Francisco, we steamed through the South Channel of Bikini atoll and anchored in the lee of the main island.

Anyone who expects a Pacific atoll to be a lush tropical paradise is due for an unpleasant surprise. There was a sparse and rather dry looking flora of coconut palms, pandanus, and other tropical vegetation on Bikini island. The palm fronds rattled in the breeze. Sudden afternoon showers were common, but the water quickly sank into the soft, calcareous sand. Even the reefs were a disappointment at first glance, although further examination revealed some delightful views. On the windward side, the surf crashed in over the pink algal ridge and flowed down over a luxuriant growth of algae and corals. However, on much of the reef, flat coral heads grew up only to about the mean low water mark and were dead on top. Between the coral heads were lovely little grottos with living coral and associated fauna, but the distant view gave the impression of an abandoned parking lot, with the concrete pavement shattered into irregularly shaped fragments.

The poor Melanesian natives had been removed from the atoll a short time before we arrived, and only their empty houses revealed their former presence-- small houses consisting of frames of poles covered with beautiful and intricately woven matting, set on stilts a few feet above the ground.

All this I saw only on a few Sunday afternoon "picnics" on the island. Much as I would have liked to get a better look at the biology of the reef, my job was on the water. And I didn't avail myself of all the opportunities for picnics because, among that disgruntled bunch of officers and crew, Sunday afternoon generally turned into an unpleasantly noisy and drunken beer bust. The officers didn't try to set a good example for the men. I once saw the captain come aboard so drunk that he had to crawl up the long companionway on hands and knees in full view of all the crew who had assembled to see the Sunday evening movie.

During the trip from Hawaii to Bikini, we had plenty of time to assemble our gear, and the program started immediately after arrival. We used current poles to measure surface currents in the lagoon and passages into the ocean and Bill Von Arx's current meters for deep currents. We set up a series of profiles of routine oceanographic stations in surrounding waters for generalized patterns of ocean currents. This was done on a minesweeper converted for hydrographic purposes, similar to the one I had used at Miami. The work in the lagoon was easy and pleasant, with calm water in the lee of the eastern islands and building up to no more than a three-foot chop at the western end. During these lagoon cruises, we also took numerous water samples for determining the salt balance in the lagoon. Incidentally, they also gave us a glimpse of the loo'ard reefs, quite different from the windward one, with larger and more isolated and luxuriant coral heads, a really beautiful sight.

The cruises outside the reef were a trial. There was a fresh trade wind at that season, usually 15-25 knots but occasionally stronger, and our ship gave us a bumpy ride.

We had a winch powered by a cranky gasoline motor, a trial under the best conditions and almost impossible in wet weather. I remember one blustery, rainy night when we spent all night trying to make one station. The winch would quit and take an endless amount of time to get it going again. We would have been happy to quit, too, but we couldn't. The times when it chose not to run were when we had some hundreds of meters of wire out and a number of Nansen bottles dangling.

Then there was another time when one of the ship's generators quit, hardly an unusual or memorable event except for the fact that it inspired a literary gen-- of a sort. All through the war, I had heard Navy men inserting a certain four letter word into practically every sentence, generally in an entirely meaningless and tire- some way, but on that afternoon, it was used succinctly. The chief motor mac crawled out of the generator room, covered with grease and dripping sweat, sat down wearily on a hatch cover, and said, "The fuckin' fucker is fucked."

Despite the hard time that the little ship, *Blish*, gave us, we liked the atmosphere aboard. It was a much pleasanter place to be than the *Bowditch*. The captain was an old pony, a term for a man who works his way up from the ranks. He understood his men and was informal and no stricter than need be. We gathered on the fantail in the evening for a social hour and a beer, strictly verboten on navy ships but to my way of thinking a much nicer way of dispensing the stuff than saving it for a weekly bash on the beach.

All of our group operated quite effectively at sea, including Walter Munk, who probably had little prior seagoing experience but who caught on quickly. However, he goofed on one expedition that he organized and got roundly teased for it. Our data had shown that there was a wind driven surface current from east to west in the lagoon, as might be expected, and a return flow in the deeper water. What happened to the deep-water flow as it approached the reef wasn't clear. We were willing to assume that there was some kind of upwelling and entrainment and let it go at that, but Walter, who knew much more about these things than we did, wanted to examine the process in some detail and try to find out something about the relative importance of advection and diffusion. He thought we could do this if we made some closely spaced transects of temperature, salinity, and oxygen. He offered to organize the expedition.

In order to get close inshore on the lee side of the reef, we commandeered a DUKW, one of those cumbersome amphibious craft that is uncomfortable on both land and sea. Walter gathered together the equipment-- more cases of bottles than we could ever use, hand winch, BT, etc. We set forth on a trip that was wet and bumpy even though the waves were not very big. We went there and prepared to sample. No Nansen bottles. We had to go back to the ship and get them and start over. The afternoon dragged on, and on the way back, we were drenched by a late afternoon shower. There were pointed comments to the effect that theorists should do their work with a pencil and piece of paper. Of course, only nice, good- natured guys get teased that way, and Walter certainly was and is that.

All of us except Bill Von Arx were involved in these various seagoing operations. He had a subproject of his own and was in the air much of the time in a lumbering old PBM. His job was a study of flow of water across the reefs at all stages of the tide. It involved tossing out packets of dye marker and then flying over the area repeatedly and photographing it to determine the rate of flow. This of course was supplemented by measurements of the depth of water over the reefs to calculate mass transport.

In April, the job was virtually finished. A few odds and ends of field work remained to be completed and from there it was simply a matter of analyzing the data and writing the report. We were then told that we could split into two groups. One would remain to finish the report and then would be allowed to go home. The other group would get a leave of absence and then would return for the post-bomb tests.

Hal Turner and I chose to stay. I really had no option, as I had been given responsibility for preparation of the report, but I preferred it that way anyhow. I wanted to finish it and be done with it as soon as possible.

I had been feeling depressed during much of this time, although I was too busy to spend much time brooding over it. I resented being forced to leave my little family so long to do a job that was of little interest. And I hated the implications. I had never really questioned the wisdom of Truman's decision to bomb Hiroshima and Nagasaki, terrible though the results were. The alternative to that horrible and dramatic termination of the war could have been the deaths of millions of people if Japan had been invaded in the conventional way. However, continued tests of the bomb indicated that the U.S. might be willing to use it again. Other nations could build a bomb. The scientific principles were common knowledge. Only the technology needed to be solved. I foresaw at least a small part of the nuclear threat that later developed, and I hated it. I felt none of the desire of some of my friends to come back and see these "historic" events. I wanted to dissociate myself from the whole thing as much as I could at this late date. Later, when some of the work was published in the scientific literature, I refused to be a co-author. My friends knew how I felt about it. I preferred for the world at large not to know I had been involved. Some might think I approved.

The thing had been code-named Operation Crossroads. I never learned exactly why that name was chosen, but to me it meant that the U.S. was taking a turn onto a wrong road. Aside from the moral principles that were involved, I was wondering as I wrote the report whether our oceanographic program would serve any practical purpose. We oceanographers knew, although perhaps the top brass didn't, that there is a seasonal north-south shift in the wind systems and associated currents. Bikini was in a borderline position. The doldrum belt could intrude on it in summer, changing all our predictions. This caveat went into our report, of course, and it did happen during one of the post-bomb periods.

During the period while Cliff, Hal, and I were working up our report, more and more ships were coming into the lagoon, many of them captured Japanese warships, and were being anchored in various spots in the lagoon for later observations of bomb effects. We heard that animals were to be placed aboard to observe biological effects, another typical top-brass touch. I thought Hiroshima and Nagasaki had provided quite a lot of information on biological effects.

Sometime during this period, Roger Revelle arrived on board the *Bowditch* to help administrate the scientific preparations. He was a Commander in the Naval Reserve, assigned to the Bureau of Ships, and had been involved in a good many of the research con- tracts with Scripps and Woods Hole. Prior to that, he had been one of the first graduates, along with Dick Fleming, of the Ph.D. program developed by Sverdrup.

I had met Roger previously in Woods Hole and had seen him once in Florida. He came to Miami with Iselin, and the three of us went to Key West together. I don't recall the purpose of the visit, and what I particularly remember is that Roger wangled a return trip from Key West to Miami on a Navy blimp. In the early evening, we drifted along over the Keys at an altitude of about 500 feet, and our view of islands and reefs was truly beautiful.

Roger was a tall man--I'd guess somewhere around six-three to six-five, dark-haired, and with a remarkably deep voice. He had some of the qualities of leadership that Columbus had, and as a policy maker in BuShips, he was quite effective, but in routine administration, he was a trial to anyone who worked with him. Marston Sargent had been his assistant in Washington, and Marston told me that any letter or memo that didn't arouse Roger's immediate interest got stuffed into a drawer and forgotten. When Roger went off on a trip somewhere, they emptied his drawers and tidied things up. That habit persisted after he went back to Scripps and became director.

Roger had feet that were remarkably big even for a tall man and was rather clumsy or perhaps just careless. He demonstrated this by falling off a ladder shortly after his arrival on the *Bowditch*. I think he broke a toe. Anyway, he was limping around most of that period.

I think Marston was genuinely fond of Roger, or perhaps it was a love-hate relation; anyway he had no compunction about telling stories about his boss-- the clumsiness and the feet was well as the administrative mess. There was a time when they were at sea, and Roger was manning a hydro winch. He was wearing a tie-- an article that most of us oceanographers wear only under duress-- and the end of it dangled precariously. It got caught under the winch wire, and they stopped the winch barely in time. He was wound up tight, with his head hard against the spool.

Then there was an occasion when they were on a trip to some southern city, and Roger somehow damaged his only pair of shoes and needed some new ones He went out to one shoe shop after another, failing to find any that were big enough. He gradually worked his way into an area where faces were darker, and foot sizes tended to run larger. He finally came back to the hotel with a pair of horrible bright yellow things of the sort that young blacks then regarded as the height of fashion.

My own feelings about Roger were somewhat mixed. We were too different in many respects ever to be close friends. Then and in later years, our relationship was smooth and at least outwardly friendly, but I had no feeling of warmth toward him as I did with Columbus, nor any high degree of admiration either. In intellect and general knowledge, I suppose he was superior to Columbus and possibly had a stimulating effect on a good many people. However, in terms of personal scientific achievement as well as routine administration, he was a lazy under-achiever.

The Woods Hole contingent arrived back on the ship in early June. There was also an increasing number of VIP's around. The Navy had made a big thing of inviting a number of senior scientists out to watch the big bang. For them it was a fine holiday. Hal and I, the working stiffs, were eager to have our holiday in Woods Hole.

The launch took us over to the tired old PBM. Heavily loaded with people and things, it taxied up the lagoon and barely made it over the palms. There was an anxious moment as it clipped a few palm fronds on the way. We made the short hop to Quajalein, where the main Marshall Islands air base was located. There I had my first glimpse of Melanesian natives of the sort that we had displaced. They were short and stocky, with fairly dark skin, and by no means as attractive to our eyes as the Polynesians we had seen in Hawaii. That evening, we caught a flight out on a DC4 headed for Hawaii. It was mainly a cargo plane with just a few bucket seats along the sides and a few sailors aboard. We stretch out to sleep that night. The heating unit conked out, and there were no blankets available. The sudden change from tropical to near freezing temperature was a rude shock.

One engine conked out, too. We limped along and landed at Johnston Island for breakfast and examination of the engine. This was an isolated atoll, manned by a small and dispirited air crew. Breakfast was unidentifiable by either visual or taste test and might or might not have been dehydrated eggs. The process of dehydration was primitive at that time, and the products often could be identified only by the label on the container. I already knew about that, for we sometimes ran out fresh food at Bikini. The steward on the *Blish* would come into the tiny wardroom and put down a bowl of pasty greyish something. "Joe," the captain would say sternly, "what's that?"

And Joe, a cheerful and competent but not very literate young black, would say, "Sorry, boss. Hydrographic potatoes again".

The men on the airstrip decided our plane could make it to Hawaii safely. So we refueled and went on. Despite our slow flight, by virtue of having crossed the dateline we had left Quaj at 8 p.m. and arrived in Hawaii at 11 a.m. the same day, rather better than the two-week trip on the *Bowditch*.

At Hickman field, we sat on one-hour alert for twenty-four hours waiting for the engine to be overhauled and then went on to San Francisco. There we got on another DC4 for the trip east. There were cushioned seats and heat on the plane. Made us feel as if we had been put on an admiral's plane by mistake.

Waking up in the morning and looking out of the window, I saw hilly country below clothed in bright, emerald green. After four months in that yellow-green "tropical paradise," I had forgotten how green and lovely the eastern U.S. could be in the early summer. Of course I shouldn't be so disparaging of the tropics. Certainly the high islands can be lovely, and even the atolls are interesting, but four months were more than enough. To wrap it all up, we landed in Washington soon after I woke up that morning, and we boarded a train for Woods Hole and a happy reunion at last.

By this time, the war had been over for nearly a year, and the Institution had survived without any great reduction in staff or the number of Navy contracts that provided the major part of our support. The wartime jobs that scientists had done convinced the Department of Defense that support of continuing research was warranted.

I went back into Fritz Fuglister's group, which at that time was growing and expanding its activities. In addition to tabulating and analyzing the BT data that came in from Navy ships, there was money available to set up surveys of areas that had received little attention, so in addition to Fritz's girls, seagoing technicians were being added to his staff.

Surprising as it may seem, the coastal area around Woods Hole had received little attention. One of the early series of survey cruises was in the area westward from Woods Hole to about the middle of Long Island Sound, and I was assigned to that. As I recall, I went on only one or two of the cruises but had the job of analyzing the data and writing up the final report. Eventually, it was prepared for publication in the Bingham Bulletin and is one of the papers included in the volume of collected reprints. I was not altogether pleased by its inclusion, for it was a preliminary account based on very incomplete data, and it contained some serious errors that had to be corrected some years later when I made a more thorough study of Long Island Sound.

In explaining that, I'll broaden the discussion a little. The Navy had become interested in the problem of exchange rates and flushing rates in estuaries and inshore waters. General pollution was not nearly as serious a problem then as it is now, but with the usual cold logic and moral idiocy of the top brass, they were thinking about the possibility of nuclear warfare on a widespread basis and wanted to be able to estimate the rate of dispersion of fission materials from coastal waters. Buck Ketchum had temporarily launched into physical problems, too, and had worked out a neat little algebraic scheme for determining flushing rates in estuaries. His analysis showed that an increase of freshwater drainage into the estuary increased the flushing rate.

My analysis of Long Island Sound revealed estuarine characteristics, with a net outflow of freshened surface water and an inflow of deeper and more saline water along the bottom. As nearly as I could tell from the data available then, the exchange should be enhanced by increased drainage as in the estuaries that Buck had studied. However, the Sound is not a typical estuary in that the largest amount of freshwater drainage enters near the eastern passes, and the later survey showed that this significantly affects the exchange pattern. When the Connecticut River is in flood, the outflow quickly leaves the Sound as a thin, freshened surface layer, like a river flowing over the more saline underlying water, and the exchange rate in deeper waters is actually reduced rather than increased. I had to eat crow in the later paper. People who stick their necks out as much as I have during much of my professional career win a few and lose a few.

After two delightful months at home after Bikini, I was off to sea again, this time a one-month cruise on the *Atlantis*. We had a contingent from Maurice Ewing's group aboard for some SOFAR work in an area about five hundred miles east of Bermuda, and Fritz, Al Woodcock, and I were going to do intensive BT work on surface and near-surface phenomena--diurnal warming, convection features, and internal waves in the seasonal thermocline.

Al Woodcock had been interested in problems of surface convection for some time. I think it started one morning on one of those Georges Bank cruises, when Al lingered over breakfast, staring at his coffee cup until the coffee got cold. The cream was starting to turn and there were little flecks on the surface. He watched them collect into little patches with reticular clear spaces between. He remarked on the fact that Sargasso weed tended to collect in similar patches on calm, cool mornings. We both had seen that, of course. He had read something about columnar convection cells, which was clearly what he had in his coffee cup-- a cooled surface film converging into sinking centers. Subsequently, he worked on various aspects of this problem and hoped on this cruise to make a more thorough examination of the water in and around the patches of Sargasso weed.

Back in the BT office, Fritz had occasionally seen slides in which there was a slight negative temperature gradient in the surface layer with a bit of a reversal at the base, the net effect being a toothy little point between the surface layer and the thermocline. The temperature in the reversal was slightly less than that of the immediate surface. A likely explanation seemed to be that the combination of night cooling and evaporation increased the density enough for the water to sink to the top of the thermo- cline.

On that cruise, we put these two disparate observations together. Those odd little hooks in the temperature curve were found only in calm weather and generally in the early morning. In the middle of the patches, the water was virtually isothermal. It seemed to be sinking there and spreading out along the top of the thermocline in a layer considerably less than half a meter thick. We couldn't think of any good way to get a quantitative estimate of the rate of turnover and didn't feel we had enough information to warrant publication. Perhaps we should have. The phenomenon has possible biological implications, as I mentioned briefly in another paper many years later, and has never been thoroughly studied.

Later, while we were sitting on station for SOFAR work, Al did a few experiments on the problem of Langmuir circulation, which was then commonly regarded as convection cells of a different sort. He carefully ballasted some small salinity bottles so that they floated barely awash, and then went out in a whale boat and dropped them off in a row across the wind and the weed lines. He found that they did in fact separate in the middle area and migrate into the weed lines as would be expected with surface flow into convergences and rising water between.

Some years earlier, Albert Parr had been interested in the distribution and abundance of

Sargassum and had made numerous collections on his cruises. He demonstrated a large seasonal variation, with much of it disappearing and presumably sinking in variation, with much of it disappearing and presumably sinking in winter. Al had observed that it frequently was carried down a meter or more below the weed lines, particularly the older plants that were weighted down and epizoans. Al thought that the winter decrease might be due to submergence in the large convergences associated with winter storms, particularly if they were carried down far enough for increasing water pressure to flatten the bladders. He had a pressure vessel built to examine this question. I can't remember whether he had it on that cruise or not. I got some data for him on another cruise when he wasn't along. An increase of only one or two atmospheres--equivalent to 10-20 m-- was enough to collapse many of the bladders, so his theory was at least credible.

Later, Al got interested in air-sea interactions--salt nuclei and that sort of thing. There he attained a fine scientific reputation, despite his humble beginning. However, I have often regretted that his interest in convection problems waned. In a subject that is still poorly understood, I'm sure that his keen powers of observation and logical deductions would have had a significant impact.

During that cruise, the weather was calm and hot most of the time, and we weren't moving around enough to generate a breeze for ourselves. In fact, we anchored at the SOFAR stations, using most of the long trawling cable to do it. We got a taste of what it must have been like in the old days, getting becalmed in the horse latitudes. Though free of the trials of thirst and starvation that sometimes were the lot of those old seamen, we were certainly hot and uncomfortable much of the time. The freshwater supply on the *Atlantis* was limited, as it always was on a cruise of any length. A salt water shower that we rigged on deck gave only a little relief.

Sitting quietly in the water and occasionally tossing out garbage, we attracted a good many sharks. Most sailors hate sharks and often are utterly sadistic in the treatment of any that they catch. There was no sport in the fishing. A stout line was attached to the big shark hook and as soon as the shark swallowed the hook, it was winched aboard. Laid out on the deck, the creature died the death of a thousand cuts.

Some of the geophysical boys were equally murderous and only slightly less callous. On one occasion, they put over a dynamite cap embedded in a hunk of meat. When they exploded it, the shark swam off in crazy, agonized circles. That inspired them to try a block of TNT. That time, the shark sort of dissolved, in a bloody, red-brown cloud, and when the water cleared, any remaining remnants of the shark had vanished.

To end the account of the cruise on a lighter note, while on SOFAR station, the captain had hoisted the black anchor ball, which is used as a signal to other ships to stay clear. On one occasion, a passing freighter altered course to pass nearby to find out whether we had found an uncharted sea mount that might be a hazard to navigation, or were just plain nuts.

We finished the job and came back. An arrival of the *Atlantis* was always something of an event. Word would get around that she was due in, and families and staff would come down to the dock to greet us. Lucy and Louise were there, of course. I jumped off the ship wearing a month's growth of beard. Lucy sort of liked it. Louise pulled back and would have nothing to do with that villainous looking stranger. The beard didn't last very long.

As is true of many young oceanographers, my enjoyment of sea- going life was in conflict with my desire to be at home with my family. Lucy had become pregnant again between my arrival from Bikini and departure on the *Atlantis*, and when another project turned up that was entirely shore-based, I accepted it readily, even though it was not a very exciting one. This one was with Charlie Fish. I had heard about Charlie for a long time and had read most of his papers but had only barely met him up to that time.

Charlie had been one of Bigelow's students and had done a thesis on plankton in the Woods Hole area, which was published in the mid-Twenties. He was involved in some expeditions after that and then had done major work on the Passamaquoddy project in the early Thirties. There was a lot of talk at that time, which turned out to be nothing but talk, about developing a tidal power plant in Passamaquoddy Bay. The only positive step that was taken was a survey which attempted to determine whether an installation of that sort would affect local fisheries. Charlie did most of the zooplankton work on that survey, largely descriptive work on life cycles but well done, using pump samples for quantitative assessment of juvenile stages. Then he went to the University of Rhode Island, where he established a small marine lab on Narragansett Bay, a few miles east of the Kingston campus. That unfortunately was wiped out by the 1938 hurricane.

He spent the war years in Washington, another one of those naval reserve officers. He and his wife, Bobby, an ichthyologist, were principally concerned with gathering information on sound- producing animals that interfere with listening devices. After the war, he toured Japan where he acquired by hook or crook a large amount of Japanese literature on oceanography and marine biology. He brought this back and deposited it in the MBL library (which was shared and jointly supported by the Marine Biological Lab and the Oceanographic Institution). Together with the material that they already had, it was undoubtedly one of the finest collections of its kind in the country.

With this hand, Charlie was able to wangle a Navy contract to abstract the Japanese material and report on things of practical Navy value. This was probably a bit of a boondoggle, but a Navy captain, even a reserve officer, can exert a considerable amount of leverage.

For me, the job provided nine-to-five shore duty, some broadening of my education, and with time to spend with the family and to continue working on problems of plankton dynamics.

There was a good deal that was interesting in this Japanese literature. Most of it was a classical, descriptive sort, but pre-war Japanese oceanography had been far more intensive than our own, presumably because their need for marine food had warranted the expenditure for more ships and more surveys. They had studied the Kuroshio Current system thoroughly and described its meanders and eddies long before our own studies of similar features in the Gulf Stream got underway. They were well aware of the fact that plankton was concentrated in convergences, particularly the major ones in the confluence between the Kuroshio and Oyashio, and eluding to concentrations of pelagic fishes. Japanese fishermen put to sea with thermometers to help them find and maintain their position in these favourable fishing areas. And of course, they were miles ahead of us in aquacultural developments.

Charlie and Bobby were friendly, congenial people to work with but were sometimes a bit of a trial because both were compulsive monologists. Between the two of them, it was a tie battle, but almost anybody else got talked under the table. More often than not, I nodded occasionally and thought my own thoughts. I remember one of the people who had worked with him quite a lot remarking, "I like Charlie very much, and the farther away from him I am, the better I like him". I knew exactly what he meant and felt much the same way. Of course one of the problems was that people who talk that much inevitably repeat themselves, and I frequently knew exactly what they were going to say as soon as they started to say it.

Other people on the program included John Ayers, another biological oceanographer about my age, Ruth Von Arx, Bill's wife, and a young Japanese fellow whose name I can't remember. He had little scientific background but was helpful in translation. Of course a good many of the papers were written in English or at least had an abstract in English or Esperanto.

In my spare time, I was continuing to play with Georges Bank data. I developed a model of the seasonal cycle of zooplankton based on the Volterra-Gause predator equation. The model fit the data fairly well but contained one arbitrary element. During the spring flowering, the rate of increase of zooplankton was much less than it should have been if they utilized all the food that was available. In an earlier paper, Harvey and his associates had reported that assimilation was incomplete during that period and that fecal pellets were green with undigested plant material. I got around the problem by assuming a constant, maximum rate of assimilation during that period and arbitrarily discarded the rest of the phytoplankton that was available. That seemed a reasonable solution at the time and still does, despite more recent experiments indicating a reduction in grazing rate in the presence of super-abundant food. The problem of course is that a constant grazing rate was needed to make the phytoplankton seasonal cycle fit, and some of it had to be discarded in order to have a realistic zooplankton model. A mere reduction in grazing rate wasn't what I needed.

Parenthetically, mentioning Harvey just now reminded me that I should have said a little more about him. He was one of the men I admired most when I was entering the field, and I deeply regret that I never met him in person. However, we became rather good friends anyway. We developed a lively correspondence during the period when I was working on Long Island Sound and Georges Bank, discussing differences in methods and differences in the ecology of our respective areas. The association, distant though it was, was pleasant and gratifying. Winning the respect of a man I regarded so highly was important at that stage in my career.

To go on, I did two other models of phytoplankton seasonal cycles. One was based on data obtained by Lois Lillick in the Woods Hole area. The other was from a Japanese paper on phytoplankton in a Korean harbour with a name that is difficult for western tongues and has been variously spelled Pusan, Husan, and Fusan. These papers were really just pot boilers, trying my hand at solving other seasonal cycles with only minor modifications of the Georges Bank equation, but they added to the credibility of the method. In the Korean one, in particular, there were two years' data with considerable differences from one year to the next. The results fit fairly well and demonstrated that differences in zooplankton and nutrient concentrations could explain these results.

During the latter part of this work, Columbus had budgeted some of the limited Institution endowment funds to pay half of my salary and also to support Ruth Von Arx to assist me. That endowment, which had once supported the whole Institution, was now only a drop in the bucket and was dribbled out here and there to keep a bit of pure science going. There were a good many others in my situation, doing contract work to earn their bread and butter and trying to slip in a few things that we preferred to do. Bootlegging ship time was fairly easy for small jobs that could be done concurrently with Navy projects. I had a cruise or two of that sort, but funds were lacking to set up the sort of biological oceanographic programs that had existed before the war.

I suppose a majority of the people at the Institution were quite satisfied with the state of affairs as it existed at that time. There were scientists who liked applied research, and physical oceanographers generally could find projects that paid dividends in pure research as well as satisfying Navy requirements. However, a good many people were looking for greener pastures.

Several of the friends whom I have mentioned departed. Ed Deevey got an appointment in the Zoology Department at Yale. John Ayers went to Michigan University. Bill Ford got a job in the Canadian Defence Research Establishment in Halifax. Charlie Fish and Bobby eventually went back to the University of Rhode Island when his contract work was finished. Tommy Austin was offered a job in the U.S. Hydrographic Office to help develop an oceanographic capability there, a considerable step up for him. Tommy Edmondson went to the University of Washington.

While in Woods Hole, Tommy had done some of his research in collaboration with his wife Yvette. He had met her some years earlier when he was a graduate student at the University of Wisconsin. If memory serves me right, she was taking her Ph.D. in microbiology. However, when they went to the University of Washington, a nepotism rule there prevented her from getting a paid job in the University. She got into editing work when she and Tommy got involved in a major revision and modernization of the old Ward and Whipple's "Freshwater Biology". Later, she achieved national and international recognition for her outstanding work as editor of the journal Limnology and Oceanography.

Poor Tommy Austin. Not long after he went to Washington, he contracted polio. He survived, virtually paralysed below the neck but with the use of his hands and partial recovery in one arm. With brains, hands, indomitable courage, and nothing else, he managed to go back to work and support his family and carve a successful career. He was in the U.S. Fish and Wildlife Service for awhile (a new name for the former Bureau of Fisheries) and later in the National Oceanographic Data Center, where he worked until the usual retirement age.

I felt quite content to stay in Woods Hole for awhile, although I would have to leave eventually unless we were able to develop some good seagoing programs in biological oceanography. From a personal standpoint, Woods Hole was ideal; the only problem was a professional one. We were well established in a comfortable home and had a lot of good friends. Lucy was happy there. In April, 1947, our second daughter, Grace, was born in the Wareham hospital on a sunny spring afternoon, a vast contrast to the birth of our first daughter.

The plankton models that I had been doing convinced me that I could get fairly realistic results and also could evaluate the role and relative importance of individual environmental factors much more effectively than in earlier statistical analyses. However, I was not satisfied to let the matter rest there. I wanted to go further, removing some of the arbitrary elements and over-simplification.

The theoretical principles seemed simple and clearcut. There was a handful of independent physical factors that controlled the rates of most of the biological processes and, ultimately, the level of production in any area: radiation, temperature, advection and diffusion, and bottom depth in shallow water. Then there were the interdependent biological factors-- nutrients, phytoplankton, zooplankton etc.-- which all affected each other and needed to be solved as a package deal rather than being introduced arbitrarily as independent factors. Simple enough, in theory, but a complicated mess to deal with. I had to consider what simplifications would be allowable without vitiating the whole model. The package deal for biological factors had to be a linear food chain rather than a food web in order to apply the prey-predator equations, and I could think of no other way to do it. Granting that, equations could be formulated for phytoplankton, zooplankton, and a limiting nutrient, and they would require a simultaneous solution.

I decided to aim for a regional analysis based on cruise averages obtained earlier, and in that case, I could simplify the problem to a steady state solution with an assumption that horizontal advection and diffusion could be ignored. Temperature and irradiation were no problem in that case. Transparency was more difficult, for it was in part a biological variable, but to include it as a dependent variable would have been very difficult at that stage of model development. I wanted to develop a more realistic assessment of vertical diffusion than the simplistic methods I had used in the past. For that, I calculated vertical eddy conductivity based on known seasonal changes in temperature. That was a cookbook method in common use by physical oceanographers. I knew that was as simplistic as the linear food chain, ignoring the fact that vertical mixing included some complicated convective processes as well as simple, random diffusion, but again, I couldn't think of a better way to handle the vertical mixing problem. Of course, in the thirty-odd years since then, no other modelers have solved those two problems either.

The paper that eventually would become the Riley-Stommel Bumpus monograph on quantitative ecology grew by bits and pieces. I reworked such data as I had no biological processes and quantitative distribution, aided by a few additional experiments. I bootlegged some observations on a cruise or two to Bermuda and sent out a young assistant, Samy Gorgy, on a summer cruise to the Sargasso Sea. I'll mention him again a little later. Hank did an analytical solution of relative vertical distribution of phytoplankton in a hypothetical two-layered system with net production in the upper layer and consumption in the lower layer. The last section was the thing I had been particularly aiming at from the beginning-- independent physical factors and interdependent biological ones-- calculating the vertical distribution of phytoplankton and phosphate with a vertical eddy diffusivity that also varied in a fairly realistic way. This required simultaneous solution of about forty interdependent equations, and I was indebted to Hank for digging up a method for doing this by arithmetic approximation. The method was simple but laborious in pre-computer days. Twenty-five or thirty hours of arithmetic diddling were required to get a satisfactory solution to each problem.

That was that. I was through with plankton models at least for the time being. I had demonstrated that fairly realistic results could be obtained within an ecological framework that was logical as far as it went. I had learned a lot about how to evaluate the interaction between physiological processes of the organisms and physical oceanographic parameters. I didn't know how to escape from the simplistic constraints of the linear food chain, and even allowing that simplification I felt that little further advance could be made until we had a better knowledge of both physiological ecology and mixing processes in the surface layer of the ocean.

However, I really didn't learn as much from these models as I had from the earlier and simpler ones. There the effect of altering one factor or another was readily determinable at least as a first approximation. Moving to the more complex situation of inter- dependent biological factors, sensitivity analyses required recomputation of the whole problem on an extensive scale. In pre- computer days that was so laborious that I didn't do much of it.

Although this paper attracted more attention than the earlier ones and has been cited more often, in my own mind it was a lesser achievement than the first model. That was when I did the hard thinking about how to tackle the job. This one was simply a logical progression with addition of complicating details and culminating in an arithmetic *tour de force*. The attention it received of course was both pro and con. The dissenters felt that the models were much too simple to deal with nature in a realistic way. To be sure, I have never pretended that they are truly realistic. My only defense has been that they help us to think. They help us identify physiological and physical problems that have been neglected and are conceivably important. They frequently yield results that are not intuitively obvious, and they teach us caution about drawing conclusions that seem to violate mathematical logic. Either the conclusions are wrong or we have to rethink the logic. That is the way physics and astronomy have grown. Biological oceanography, messy though it may be, needs the same kind of disciplined thinking.

Also to me, science is fundamentally an artistic endeavour, and the esthetic pleasure of trying to fit the fragmentary facts into a logical picture is the one thing that I like best about it. I suppose this can be likened to the work of a painter who tries to capture the essence of a scene in the composition of his painting. The daubs of paint that he puts on the canvas are what he sees as the most significant elements in the scene. They are never as detailed as a photographic image and do not need to be. I am sure there is some of this feeling in most scientists, conscious or subconscious, but in me it has always been a highly conscious force. Mere fact finding is of no interest, although admittedly any fact is likely to become significant sooner or later.

With the completion of the Riley-Stommel-Bumpus paper, the old pre-war data had been wrung dry. The only post-war observations in biological oceanography that seemed worth publishing were the results of Samy Gorgy's cruise, and I wrote that up as a joint publication.

Samy was a young Egyptian from Alexandria, where a small oceanographic lab had been set up, and an arrangement had been made for him to come to Woods Hole for awhile for instruction in plankton problems. I was given that job. We had a number of tutorial sessions, and I taught him some lab techniques and sent him off to sea. All that went fairly well. He was a bright young fellow. From an extra-curricular point of view, he left something to be desired. I don't know how many young female technicians he succeeded in getting into bed with, but he gave it a good try. He also wanted almost more than anything to have a big American car, and he got one, albeit an ancient and noisy pre-war model. I gave him a few driving lessons. Long before he was really ready, he insisted on going for a driver's license. Reluctantly, I went with him. The examiner went with him as far as half way around the first block, ordered him to stop, and got out. He said he preferred to walk and would advise Samy to do the same. I valued life and limb too much to continue the instruction, but some other friends did, and Samy eventually got his license. Within a few weeks, he wrecked the car but got out of it with no serious injuries. I was not altogether sorry to bid Samy goodbye.

In January, 1948, Dan Merriman invited me to come to Yale to give half a dozen lectures on plankton in a course in biological oceanography that he had organized. This was a revival of the sort of one-semester course that Albert Parr had given in earlier years. I accepted and thoroughly enjoyed the visit. The students were bright and interested, and there were a lot of good discussions. Some really top notch students had flocked to Yale to work with Evelyn, who, by that time, had a firmly established reputation as a leading limnologist, and they took the oceanography course in passing. I can't remember precisely which ones were in that class and who came along a little later, but the list included names such as Larry Slodbodkin, Fred Smith, Jack Valentyne, Lloyd Dickie, Howard Odum, and Ralph Lewin.

The return to Woods Hole was an anti-climax. The biological oceanographers who remained there were restless. If we were to continue in our chosen specialty, our future careers depended on getting some seagoing programs underway. We weren't likely to get ship time for extensive cruises, but we would be reasonably satisfied if we could begin a study of the coastal waters off Woods Hole. Buck was able to arrange a short, initial cruise in March of that year.

The ship was the *Balanus*, a tubby little vessel with seakeeping qualities little better than those of the *Anton Dohrn*. I don't know who christened her with the name of a barnacle, but it was apt. She never should have come unstuck from the pier.

That cruise was a disaster from start to finish and without doubt the most miserable one I have ever been on. The weather really was no worse than average for that time of the year-- a five to ten foot swell running most of the time. The *Balanus* was what did us in-- a quick, hard roll that made deck work miserable and lab manipulations virtually impossible. We took turns on the pulpit during bottle stations, and everyone got dunked to the waist sooner or later. I particularly remember the last station before we finally gave up and headed home. It was Buck's turn on the pulpit. He had only one set of dry clothes left, and he wasn't going to get them soaked. He manned the station wearing only underpants and rubber boots while a spring snow flurry swirled around his bare torso.

The results of that cruise were minimal and virtually worth- less, and for one reason or another, we were unable to arrange any continuation of the program. I remember walking down the street with Alfred Redfield on the way to work one day, and he was saying that finances were tight, and there wouldn't be much opportunity for anything except contract work during the next year. I was beginning to think rather seriously about a discussion that I had earlier with Dan Merriman in which he said he would like to have me come back to the Bingham Lab and would try to arrange a staff position if I was interested. We had left it as something to think about, but I was beginning to think it was time to go.

In contrast with my dissatisfaction and indecision, Bump had transferred happily to physical oceanography, and some of my other friends were making significant progress in things that interested them. Bill Von Arx was developing an instrument with a name about as long as the instrument itself-- geomagnetic electro-kinetograph-- quickly shorted to GEK-- for measuring ocean currents. As Bill remarked, towed instruments were the way to go. He wanted to be able to do his seagoing research lying in his bunk watching the data come in automatically (not that I ever saw him seasick or lying around much).

In a mere three or four years, Hank Stommel acquired a thorough working knowledge of theoretical physical oceanography as it existed at that time, and I think this was largely on his own initiative. His first major achievement was an ingenious little model depicting the westward intensification of ocean currents, providing for the first time a theoretical explanation of currents such as the Gulf Stream. That paper alone was enough to establish his reputation as a creative young scientist with a bright future. A further development of this theory predicted that deep ocean currents also should exhibit westward intensification. This of course was later verified by direct observation, and the older concept that all deep ocean currents were sluggish and diffuse was laid to rest forever.

During this period, Hank was still a bachelor. I can't remember exactly when he got married, but it was somewhere around the early to mid-Fifties. He and several other young bachelors rented a house that had formerly been an Episcopal rectory and was still called the rectory, although I dare say it was gayer and noisier than it had ever been in its earlier days. There were some gay parties at the rectory. I remember a Halloween party that Lucy and I attended where they had gone to considerable effort to create fearful and way-out decor right from the front entrance through the whole downstairs area, and all dimly lighted, providing lurking places for hobgoblins in every corner. Hank and the other young residents of the rectory typically were about as energetic and imaginative in their fun as they were in their science.

As time went on, Hank continued to pursue various outside interests though never at the expense of maintaining a high level of production in his specialty. Some of these were serious intellectual endeavours in other fields, and others were merely amusements. At one time, he bought a small but versatile printing press. He is reputed to have had a lot of fun with it. I had left Woods Hole by that time and am not as well acquainted with his private output as with the things that appeared in the Woods Hole Collected Reprints. However, at one time, he sent me about a dozen sheets of notepaper, each bearing the letterhead of a club or organization which of course existed only in his fertile and whimsical imagination. Regretfully, I have since lost that little collection, and of all these organizations that he founded, the only one I can now recall was his "Home for the Aged and Indignant". Ah well, back to the summer of '48.

There was plenty of Navy money to support physical oceanography and geophysics in a sufficiently broadminded way to permit a lot of good basic research. Only the biologists were poor relations. A little later that situation would improve with the establishment of a biology branch in the Office of Naval Research, and still later, there would be grant money available from NSF and AEC. However, we didn't have a crystal ball.

While still in the midst of indecision about my future, I was invited to go to Johns Hopkins University to discuss a possible job offer there. They were planning to found an oceanographic lab to be jointly funded by contracts from the Navy and the states bordering Chesapeake Bay. During the interview, I realized that much of the interest would be in physical oceanographic research. Biology, of course, was not excluded, or they wouldn't be considering me for the directorship. However, I would have to start from scratch. I would have to recruit expert physical oceanographic help and biologists as well, for no one in the biology department at Johns Hopkins showed much interest in that sort of work. I went back home to think about it and discuss it with people.

Hank said he would at least consider seriously going along if I took the job, but he didn't sound overly enthusiastic, and rightly so. Hank had a mind that in due time would encompass all the world oceans, and Chesapeake Bay didn't deserve a man of his talents.

Columbus thought I ought to take the job. In weighing it against a job at Yale, he said, "If you want to sit in a little lab by yourself and do your own thing, okay, but this is an opportunity to found another fine, big oceanographic lab". Much as I liked Columbus and respected his judgment, that remark had the opposite effect of what he had intended. I didn't want at that stage in my career to go through the political gymnastics of wangling support from several agencies to promote a big lab. As far as my own scientific interests were concerned, I would be more alone there, in the beginning at least, than at Yale where I could not only do my own thing but also contribute toward the training of some fine young ecologists. So, I turned that job down. It went to Don Pritchard, a young physical oceanographer from Scripps, who was far better qualified than I was for the sort of directorship that they wanted.

Leaving Woods Hole was traumatic for Lucy and me both, but particularly for her. She had put down roots in Woods Hole and had many good friends there, and once more I was trying to pull the roots. However, going back to New Haven was better than moving to Baltimore. She had a lot of relatives in the New Haven area. We sold our house to Bill and Ruth Von Arx and moved to New Haven in August, 1948.

Lucy's brother, Henry, lived there. He was Reference Librarian in the Yale library. He had married Helen Hull, the third member of the group of nurses who were sharing an apartment at the time I met Lucy. The four of us bought a two-family house in New Haven and moved in together.

Henry was two years younger than Lucy and had graduated from Yale in the mid-Thirties, when there wasn't much choice in the kind of jobs that were available. The first year out of college, he taught in a boys' military academy, which he later grimly described as a detention home for juvenile delinquents whose parents were affluent enough to keep their sons out of public institutions. Then he managed to get a job in the Yale library.

During the war, Hank was in the Army, where he was assigned to a crash course in Chinese at Harvard and then was sent to China as a member of a liaison unit working with the Chinese Nationalists. Returning to Yale after the war, he was quickly promoted to head of the reference department. He had a keen analytical mind and was a lively conversationalist. Deeply interested in history and current world affairs, he always had interesting things to say about the passing scene.

Lucy's older brother, Clem, was quite a different sort of person. He was an engineer working in New York, practical minded and less communicative than Hank. I liked him but didn't see him nearly as often, and we didn't become really intimate.

Their aunt, Grace Fuller, also worked in the library, in the periodical department, and was another favourite person. And as they were an old New England family, there were aunts and uncles and cousins sprinkled all over the landscape from Boston to New Jersey, plus a few who had drifted farther west. Family reunions were big gatherings. However, aside from family relations, our social life was never quite as good as the small-town, neighbourly relations in Woods Hole. We formed few lasting friendships.

The Bingham Lab had grown a little although it was still a small outfit, an enclave of marine biologists rather than anything approximating an oceanographic lab. With the departure of Albert and Martin, Yngve and I were the only members of the old group, although I was well acquainted with most of the others. Dan Merriman and I had been friends since graduate school days, but we were quite different in a good many respects, and I never felt that he was a really close friend.

Dan was a nephew of Henry Bigelow and had been one of that first crew that brought the *Atlantis* over from Denmark. As an undergraduate at Harvard, Dan roistered a bit too much and was kicked out. There was no doubt a family conclave about that. I heard none of the details, although I suspect Uncle Henry was influential in shipping him out on a fishing trawler to learn the facts of life the hard way. Later, he went to the University of Washington, where he finished undergraduate work and got a master's degree, specializing in ichthyology. He then came to Yale for his Ph.D., working with Albert Parr. His thesis was on the life history of the striped bass.

Dan was married when he came to Yale, which was unusual in those days. No one could support a wife on a lab assistant pay, nor could a wife have much assurance of getting a job. Dan had an apartment and a wife who wasn't even trying to get a job. Mary was occupying herself with Junior League affairs and such. Clearly they didn't hurt for money, but Boston Brahmins never mention money. That set him a little apart from us poor working stiffs, but he was much too nice a guy for any of us to feel resentful. As soon as he finished the degree, he was appointed Instructor in the Zoology Department, and then soon after, as I mentioned earlier, Director of the Bingham Lab. During the war years, he was teaching those cram premedic courses that were in effect at the time, and he also headed up a three-year research program on fish populations in Block Island Sound. There was a local inshore fishery, mainly for flounder, and he arranged to go out on a fishing boat to make his collections. In addition to scientific merit, the program was intended to serve a practical wartime purpose by increasing the availability of fish food. He hoped to develop a market for fishes which ordinarily have no commercial value and are thrown back. These amounted to almost half the total catch. The practical goal had only limited success, for tradition is strong. People are not easily persuaded to eat ugly looking fishes such as ocean pout and skates, even though a considerable fraction of the restaurant-going public can be fooled by pseudo-scallops punched out of skate wings.

One of Dan's associates on this program was Herb Warfel. He left soon after my arrival, and there's little I can say about him. Another was Ernest F. Thompson, more commonly called Bill. He was a tall, lanky New Zealander a few years older than the rest of us. He went to England for graduate work at Cambridge and then was a member of the John Murray Expedition in the Indian Ocean. Subsequently, he joined the staff of the Bermuda Biological Station, which had a small ship for a few years before World War II and conducted some general oceanographic work around the islands. Although primarily a biologist, Bill was in charge of the physical oceanographic program.

With the advent of the war, the Bermuda Station virtually ceased to exist. The building was taken over as a hospital and Bill came to Woods Hole. There he got into trouble with a staff scientist named Seiwell, and Columbus decided they should be separated. No one wanted Seiwell, and Bill was farmed out to the Bingham Lab. That was where I first met him, about

1940 or 1941.

Seiwell was one of the few Germans I have met who fully conformed to the stereotype of an arrogant, bad-tempered Prussian. He was thoroughly disliked by lots of people. He worked on internal waves in the main thermocline and an oxygen distribution in mid-depths and deep water.

I had good reason to doubt the quality of some of his science. He was not one to work unduly hard at sea. He bulldozed the radio operator into doing a lot of the oxygen titrations, threatening to get him fired if he didn't. Now Sparks (that's what he was always called, of course, and I can't remember his name) was no workaholic himself. He was competent enough on his job but preferred to spend his spare time in his bunk reading pornographic novels, dreaming of his own past or future sexual adventures ashore, or examining his outstanding collection of filthy photos. He told me, with no visible sign of embarrassment, that he only titrated about one station out of every four or five, enough to get a general idea of the type of variation, and manufactured the rest of the data. That was useful information at a later time when I was studying oxygen distribution in deep water. Seiwell's data were not included. That was a considerable diversion. To get back to the point, Bill and Seiwell got into a fight. Bill arrived at the Bingham Lab with his hand in a cast, having broken a bone hitting Seiwell. Seiwell survived but lost another confrontation a few years later when he tried to beat a train to a crossing.

Bill never gave any reasons for the fight, but some facts of the case are suggestive. Everyone knew that Seiwell treated his wife, Gladys, abominably, and after she divorced him, she and Bill got married. He left Bingham Lab for awhile, teaching for perhaps two years in a small college in Quebec, and then came back and got into the Block Island Sound fish program.

Bill was broadly educated and had a fine mind. He was an excellent lecturer and could have been a top notch research man, but he destroyed his research career by publishing almost nothing. He showed me the manuscript he had written on the physical oceanography of the Bermuda area. It looked fine to me, yet it sat in his drawer until it was rendered obsolete by later work. Dan gave him the juiciest part of the fisheries program--the life history of the dominant species, the winter flounder *Pseudopleuronectes americanus*. That didn't get published, nor did a later study of vitamin A in fish livers. He puzzled me. He was often hyper- critical of other peoples' work. Perhaps he was even more so about his own research and was too much of a perfectionist to publish something that might contain some errors, or perhaps he was plagued by some inner sense of insecurity. Dan and I tried to encourage him, with no result and no real explanation of why he couldn't get those papers out of the drawer and into print. My own philosophy of course was that most scientific papers are not much more enduring than today's newspaper but serve the same useful purpose of informing people. Wrong hypotheses and conclusions are almost as good as right ones if they encourage other people to think and do further work. There was plenty of historical evidence (Forbes' azoic zone, for instance) to support that kind of philosophy, and without it, I couldn't have been as rash as I was in some of the things I wrote. I didn't convince Bill.

In later years, he taught a popular course in oceanography for non-science majors, but as a thesis advisor, he was hypercritical, often in a sarcastic and destructive way. I ceased asking him to be on the thesis committee of any of my students. Eventually, he became an associate dean and dropped out of science completely.

Another Binghamite was Grace Pickford, an astute little biologist but hardly an oceanographer at all. She had been Evelyn Hutchinson's first wife, amicably divorced at an early date before I went to Yale. She had worked on several things but settled into fish endocrinology as a specialty. She could have had a staff position in zoology except for the fact that she wanted to devote all her efforts to research and didn't want to teach. Although rather on the fringe of things as far as we were concerned, she had done some good work on deep-sea cephalopods from the Danish Deep- Sea Expedition and also made one important practical contribution to marine biology. Her method of inducing spawning by injection of pituitary extract eventually was applied to oriental fish farming, in which some species never spawned successfully in fish ponds, so that until her method was adopted, the young fry had to be netted at sea.

At the time I joined the Bingham Lab, the fish program had been terminated, and most of the data had been published or were in the process of being worked up. There were no active seagoing programs underway. Dan was not likely to start anything. He had accepted an appointment as Master of Davenport, one of the residential colleges for undergraduates, and from then on, he was virtually a full time administrator. He had become interested in the history of oceanography, and what little time he had for research was devoted to that effort.

However, Fred Smith, one of Evelyn's students, wanted to do a thesis on the invertebrate benthic fauna of Block Island Sound, together with analyses of fish stomach contents to find out who was eating what and to try to find to what extent fish populations were affected by competition for food. This would be a good supplement to the earlier fish program. I decided to join him and do some plankton work. This was at least a small beginning toward the kind of coastal oceanography that I had hoped to do at Woods Hole.

As we got underway, we were joined by another recruit. Georgiana Deevey's family was well started, and she was eager to get back into science again. She agreed to do the zooplankton work, and it was a good addition to the program. I had never learned much about identifying zooplankton and had no desire to start at that time.

Each trip was a long, hard day. Fishermen start their day early. Fred and I had to leave New Haven about 4 a.m. in order to meet the boat at Stonington, near the eastern end of Connecticut. They shot their trawl most of the day and gave us time somewhere in the middle for Fred's bottom dredge haul and my samples. Arriving back in late afternoon, they unloaded their catch at a local fish plant, while Fred and I headed back to New Haven with our samples and a tub of fish for stomach analyses, arriving around dinnertime.

We were going out with Ellery Thompson, the same man that Dan had contracted for the fish survey earlier. He was small and rather elderly and half crippled by arthritis, but he was one of the most knowledgeable fishermen in the fleet and was invariably cheerful and pleasant to work with. Intellectually he was a cut above most of them. He had been writing his memoirs, which Dan encouraged him to do and helped him get the book published. Actually, it wasn't a very well written book, but I suppose such things are of some value to future historians.

The fish were sorted aboard and loaded into barrels with enough ice to keep them fresh on the way to market. After landing, they were loaded into trucks and shipped overnight to New York, to be placed in the Fulton Fish Market the next morning. The fishermen worked hard and made, at best, only a modest living. They were non-unionized and were at the mercy of a variable market which they didn't understand. Even Ellery, more intelligent than most, astonished me one day by saying, "It's just damn fishermen's luck. In winter when prices are high, the fish are scarce," (the founders went inshore to spawn in estuaries in winter) "and in summer when prices are down, there are lots of them".

Freddie and I had our good days and bad ones. He was a lively and interesting companion most of the time, but the little boat took quite a beating in rough weather, and he could be reduced practically to a basket case. Then again, there were warm, calm days in summer when generally some lobsters came up in the trawl. With fishermen's usual disregard for good fishery practices, they kept a pot of hot water on the stove to cook the shorts. We were as immoral as they were. Never again have I eaten quite so many lobsters, and usually there were a few left to take back home, and of course, the fish that we brought back for analysis eventually got to the tables of various lab members.

Freddie had a lot of lab work to do after each trip and couldn't sample more than about once a month. This wasn't enough to give me a very complete account of seasonal plankton cycles, but it was better in that respect than the earlier work in Long Island Sound, which was spotty in coverage, with much of it in shoal, in- shore waters. It was at least a beginning, in an area that had not been examined before, but I was hoping eventually to develop a more detailed program on a coastal ecosystem.

In contrast with Freddie's work, I polished off my samples in a few days and had plenty of time to think about other problems. The problem that was bugging me most was the enigma of the deep sea. Expeditions over the years had gradually accumulated descriptive knowledge about the fascinating deep-sea populations, but we knew absolutely nothing about food requirements and metabolic rates. To one who had spent his professional career trying to evaluate oceanic processes, the deep ocean presented a challenge that was hard to ignore. The distribution of oxygen, phosphate, and nitrate provided convincing qualitative evidence that oxygen was consumed and nutrients were regenerated. That was frustrating because the magnitude of the processes could not be deduced in any simple way, but was also challenging because it offered some hope that the problem might not be completely insoluble.

I was of course well acquainted with the discussion in *The Oceans* of the distribution of conservative and non-conservative concentrations. The local time rate of change of a conservative concentration in terms of rectangular coordinates could be expressed as advective and diffusive terms on each of the three axes. The equation for a nonconservative concentration was the same plus a biological rate of change R. To a process man and a generalist, R was a very desirable number to have. If we knew something about the total metabolism of the deep sea, it would be very helpful in trying to fit the various pieces of the puzzle together. I started rather tentatively to take some steps along that rocky road.

I suppose Columbus would have said, "There he is, sitting in his little room doing his own little thing just like I said". However, it wasn't quite that way, for although I was doing my own thing, I was by no means isolated. Evelyn had so many students by that time that access to him by students was by no means as easy as it had been in my day. He had me appointed to a number of his thesis committees, and I saw a lot more of these bright, young men than he did. Freddie had his lab in our building, the same old Hillhouse mansion. Several others joined us for lunch. The big back room, formerly a ballroom, now housed our library, and there was a long table at one side where staff members and visitors brought their bag lunches. I particularly remember Larry Slobodkin, who came across the street from the Zoology Department, almost invariably coming into my office about half an hour before lunch time to talk about his work or any one of an amazing number of ecological ideas that he dredged up. His thesis problem was a theoretical population study of *Daphnia*, which he reared in hundreds of baby food bottles collected from child-bearing fellow students, and it was a frequent and interesting topic of discussion.

And that little room was the nicest office I've ever had. It had been a small library in the old mansion, lined with magnificent mahogany bookshelves and with hand carved mahogany woodwork. Within another year or so, I would be getting some students to supervise, but in the meantime, I was finding a great deal of pleasure in my association with Evelyn's young people.

Back to the deep ocean. The problem of advection had to come first. Physical oceanographers generally felt that the method of dynamic computation was not sensitive enough to be useful for studying currents in the deep ocean, yet Sverdrup had used it to good effect on a broad, ocean-wide basis. He chose two *Meteor* profiles across the South Atlantic between Africa and South America and used stations at each end to calculate north-south mass transport of the major water masses. The results looked realistic.

Exploring the problem further, I calculated the total profiles and found, as others had, that the average currents between successive stations were messy--a random assortment of north and south currents, many of them an order of magnitude larger than the averages calculated from Sverdrup's transport data. Perhaps these resulted from observational error or they might have been real-- eddy motions on a large scale. In either case, I felt that a statistical treatment could reduce the deviations and lead to a more acceptable generalized result. The question was simply how detailed the analysis could be. The areas to be averaged needed to be large enough so that I could average a significant number of stations but not so large that real variability in distribution would be smoothed and obscured. I had found a little more than 600 stations which included phosphate and oxygen analyses as well as temperature and salinity. This was not a large number to spread over the whole Atlantic Ocean, so the grid had to be fairly coarse. For better or worse, I divided the ocean into a series of squares of 1000 km each and determined average values for each area. All this was quite contrary to previous oceanographic practice, but I was used to flouting custom.

Somewhere during the early stages of this project, Sverdrup visited the Bingham Lab. He had then resigned from the directorship of Scripps and was on his way back to his native Norway where he would be heading up a new institute for polar research. I had a long talk with him about what I was trying to do. He listed attentively and made a few useful suggestions, but he was clearly skeptical as to whether my effort would be possible or valid. I was disappointed by his reaction but not entirely disappointed.

Ray Montgomery, on the other hand, was extremely helpful and encouraging. I had got fairly well acquainted with Ray a few years earlier when we were both in Woods Hole. He was tall, slender, and very shy and diffident, and our acquaintance grew slowly. His specialty was air-sea interaction. However, he had a firm theoretical base in the whole field, and his thesis under Carl Rossby at MIT had been a study of the equatorial current system in the Atlantic Ocean.

During our early acquaintance, I asked him some questions about diffusion problems, generally couched in rather broad terms, and in his modest and diffident way, he would say, "I'm afraid I don't know anything about that". I had to learn gradually how to ask the right questions, and when they were explicit enough, I got very good answers.

Ray left Woods Hole about the same time I did or perhaps a little earlier and went to the Applied Mathematics Department at Brown. I wrote and asked to come up and talk with him about the job I was doing, and when I did so, I was quite surprised to find him enthusiastic and willing to help. Shy and retiring Ray, of all people, was not one to stick his neck out one inch. Everything he published was founded on a rock and built to endure for the ages, but when he saw a friend sticking his neck out, he did his best to ward off the blows. I drove up to Providence several times with my piles of data and spent long afternoons chewing over the problems. Finally, I published the paper, with estimates of oxygen consumption and phosphate regeneration in the deep ocean and minor notes on the less abundant information on nitrate. I sent a copy to Ray and received a note in reply which said, in its entirety, "I have read your paper with great interest and find it internally
consistent".

Some durn fools might have been let down by that kind of response, but I knew Ray well enough to know that it was high praise. He wasn't sure the paper would endure for all time, but he certified that I had treated the available data logically and consistently, and for me, that was praise enough.

The paper was finished somewhere around mid 1950, as I recall, and I have messed up the chronology a little by reporting Ray's reaction after it was published. Having gone that far, I'll describe a few other incidents that were related to it and then backtrack to 1950 again.

My analysis of Atlantic Ocean circulation had yielded results quite similar to Sverdrup's although it differed from his in some details and in ways that might have been interpreted as criticisms of his methods. A few years later, I met him at a symposium in Woods Hole, and he responded to my greeting quite curtly and avoided me thereafter. He apparently had a grudge against me, either for these inferred criticisms or simply for attempting an analysis that he had discouraged in our earlier meeting.

For my own part, I was a little annoyed with him, too, for in the meantime, he had published a paper on factors involved in the initiation of the spring diatom flowering which was really nothing more than a restatement in slightly different terms of my earlier paper on the subject and which made no reference to mine. This of course was the one mentioned by Eric in his essay in my volume of collected reprints as an example of historic myopia. However, I wasn't going to make an issue of that, and I was puzzled and a little hurt by what seemed like an over-reaction on the part of a usually friendly and mild-mannered man. I was puzzled, too, by the fact that his paper in the symposium was a mundane thing that described the kinds of regional variation in primary productivity that might be expected as a result of variations in vertical mixing and upwelling. There was nothing in it that everyone didn't already know.

A year or so later, we received the news that Sverdrup had died of a brain tumour, and perhaps that explained everything, for loss of judgment and personality changes are common symptoms of that affliction. It was a sad ending of the career of a man who must by all odds be ranked as the most eminent physical oceanographer of the first half of the 20th century. The wisdom of Ray's guarded praise about "internal consistency" was fully justified in the mid-Fifties when George demonstrated that my analysis of circulation was a severe over-simplification of the real ocean. He made a very detailed analysis of the *Meteor* data in the South Atlantic. Hank Stommel's circulation theory had predicted westward intensification of deep ocean currents as well as surface circulation, and provided one of the first verifications of their existence. He found strong transports of Antarctic Intermediate Water, North Atlantic Deep Water, and Antarctic Bottom Water hard against the continental slope off South America, one above the next. My coarse grid had averaged all that out. His calculations indicated that the total transport and rate of renewal of bottom water were two or three times as much as Sverdrup's and mine, and the estimated "age" of the deep water was reduced from 1000 years to perhaps 300 to 500 years.

This could have a significant effect on my estimates of deep-sea biological processes. I had started that discussion by saying that the average decrease in oxygen during its residence in deep water was about 2ml/L, or .002 ml. L^{-1}. $yr^{-1.}$ That was an over-estimate of biological utilization because part of the loss was due to upward diffusion toward the oxygen minimum layer. I went on from there to estimate that rate of loss, and the final R estimate was considerably smaller.

If calculations were correct, the initial over-estimate would be two or three times as large.

The biological rate of change would be similarly increased if there was no significant change in vertical eddy diffusivity, but I didn't know that, and there was just no way I could re-analyze on a small enough grid to evaluate the situation. I had to wait a long time to get an answer to that question or any evaluation of the validity of the analysis as a whole. However, in the last ten years, two analyses by independent methods, one in the Pacific and one in the Atlantic, obtained results fairly similar to mine. The one in the Atlantic showed good agreement at mid-depths but indicated that I had in fact erred on the low side in the deep water.

Soon after I finished that monograph, I was invited to come to Scripps for a few months on a visiting professorship. Lucy and I and the girls packed up and flew west in August, 1950, and stayed until December. We were provided with an apartment house on the hill north of the campus; a house long since demolished to make way for new laboratory buildings. I had a formal commitment for a series of plankton lectures. Other than that, my time was my own to do any sort of research that I chose.

After Sverdrup's departure, Roger Revelle had become Director, and one of his first acts was to establish a large-scale oceanographic program consisting of a series of profiles from the coast well out into deep water. The profiles for routine cruises began well north of Los Angeles and extended down to the northern part of Baja California, with occasional even larger coverage.

This was a time when the formerly highly lucrative pilchard [sardine] fishery was in a state of collapse, and no one knew why. Cannery Row, immortalized in Steinbeck's novels, had become a ghost town. Ostensibly, this project, jointly supported by federal and state funds, was supposed to examine the problem.

None of the permanent staff members had much interest in the survey. They had their own private projects which they could do quite comfortably in their home labs. The field work was being done by technicians and graduate students. The latter were being supported by research assistantships, and the amount of time they put into it was decidedly to the detriment of their progress through graduate school. Only the simplest kind of program could be carried out on such a large scale. It consisted mainly of temperature, salinity, oxygen, and phosphate, with net hauls for zooplankton, including fish eggs and larvae. The observations were accumulating as a series of data reports, and I saw no evidence that anyone was doing much of anything with them.

Soon after I arrived, a pile of data reports nearly a foot high landed on my desk. I was offered the doubtful privilege of doing anything I wanted to do with the data and was asked to write a report containing any suggestions I might have about the program. I could have written the report the first day. It would have said that this was looking more and more like a big boondoggle, and they'd better knock it off until they worked up the data and found out if they really were getting anything out of it. The whole thing was contrary to my own philosophy of field operations. I preferred a more modest data set that I could keep up with as it came in, and to modify it if it appeared not to be answering the questions that I was asking. Toward the end of my stay there, I wrote a report that was a little less damning and made a few suggestions, none of which was taken. I can't remember much about it except that I recommended a sharp reduction in the number of profiles and more intensive work at each station. If they were serious about plankton investigations they should at least measure chlorophyll and nitrate. Phosphate concentrations were large and not highly variable and were unlikely candidates for nutrient limitation. I also suggested experimental work on feeding by larval fish. All these things were done - ten to twenty years later and presumably not as a result of digging out that old report. They were things that would be obvious as soon as knowledgeable people got into the act.

I did some work on one of the data reports, repeating the kind of analysis of deepwater distribution of oxygen and phosphate that I had done in the Atlantic Ocean and with essentially similar results.

I also went out on a cruise with Marston Sargent on a little converted fishing boat, *Paolina T*. He had suggested that we try some light and dark bottle oxygen experiments to find out if there was enough plankton to warrant 24-hour experiments. He was reluctant to do longer experiments, probably with good reason. We did the experiments in triplicate and concluded that the method would not be useful except in areas of local upwelling. We also tried concentrating the phytoplankton by centrifuging, and that didn't work. The cells couldn't survive that harsh treatment well enough to behave normally.

We were out together on the *Paolina T* the evening when the 1950 election returns came in, and Marston was plunged into gloom by the defeat of his favourite senatorial candidate, a nice, liberal-minded lady named Helen Douglas. She has been the victim of a vicious smear campaign in which her opponent tried to make her out as a pinko and communist sympathizer. He had been a Congressional equivalent of McCarthy's Senate gang. His name was Richard Nixon, and he had already smeared several good names. Later, I had an opportunity to vote against him three times before I left the U.S., Marston no doubt a few more than that.

I had a lot of good discussions with Marston and with Walter Munk, my two favourite people at Scripps. One of the discussions with Walter gave rise to a little paper on phytoplankton sinking rates in relation to nutrient utilization. He did the fluid dynamics, of course. I added a biological interpretation, and we published it jointly.

There were several other people at Scripps whom I had known earlier. There was Martin Johnson, of course, and Claude ZoBell and Norris Rakestraw. Norris I have mentioned earlier as a chemistry professor at Brown and a Woods Hole associate. He had developed a number of chemical methods and had done a lot of basic analytical work of the sort that was traditional in those days. He had helped in setting up the routine seagoing chemistry program, but I saw no evidence that he was very interested in the results or was active in any kind of research after he went to Scripps.

Claude was a microbiologist who had done quite a lot of good work and had published what was then the definitive book on marine microbiology. At that time, he was interested in pressure effects. This included growing deep-sea bacteria at their normal pressure and also examining the effects of increasing pressure on surface forms. Some of his results were quite interesting, but he had become a publicity hound. He was the sort who liked to publish his results in the daily newspaper and make them look just a little bigger than life size.

A number of these senior professors at Scripps had succumbed too easily to the comforts of the good life in California. There was a saying among the young Turks that there ought to be one more promotion beyond full professorship-- over the cliffs. One who certainly was still active, although his science perhaps was not very profound, was the ichthyologist Carl Hubbs, whom I met there for the first time. Carl was a rough-tough guy who had come there from the University of Michigan, reportedly having been fired from there for seducing too many young females. About sixty and without a gray hair in his head, he went swimming every morning, winter and summer, with a thermometer in his teeth. He was interested in getting detailed temperature data along the coast and finding out to what extent fish populations and their migrations were affected by temperature changes. This work also involved periodic trips down the coast through the rough desert country of Baja California. He had an old Chevy fitted with special heavy-duty springs, and he had some wild tales about his adventures and misadventures on these trips. Carl loved Mexican food, and he liked it hot. He would order a side dish of red peppers and munch them as he went along. I heard that at a later time, he got an ulcer and was furious when the doctor ordered him to eliminate some of these culinary delights.

One of the liveliest of my new acquaintances was a young red-haired engineer named John Isaacs. He was hired to design instruments, but he had a lot of ideas about applying his engineering skills to a number of oceanic problems that others who were trained along more traditional lines would not be likely to tackle. At the time I was there, he was working on the hydrodynamics of fish and whale propulsion in relation to their form and size and the energy requirements for swimming. In later years, he was diverted into administration but continued research on a wide variety of subjects.

I was lecturing to thirty or more graduate students and didn't get to know them very well in that short time, but one I particularly noted was Ed Goldberg. He was one of the most articulate if not the brightest one of the lot-- I didn't know them well enough to make that sort of judgment. He certainly asked penetrating questions and kept me on my toes.

One whom I got to know fairly well was Warren Wooster, who lived nearby and was largely responsible for administering the big seagoing program. He spent so much time on it that his work as a student dragged, and he took a long time getting his degree. That was one aspect of Scripps' policy that I didn't like at all.

There were also two graduate students in physical oceanography who were quartered in apartments in the house where we were living. One was a young Chinese named Han Lee Mao if I remember correctly, and I don't know what became of him. The other was Dick Vetter. By way of avocation, he roared around on a motorcycle and went scuba diving. On one occasion, he presented us with some fine abalones that he had caught. After completing his master's degree, Dick got a job in Washington in the Office of Naval Research. That was the beginning of a long career as an administrator in Washington, and in later years, I saw him frequently.

We all thoroughly enjoyed our stay at Scripps. The girls had a great time at the beach, and we had a lively social life. I had enjoyed the teaching and meeting a lot of people and learning about their research projects. The time wasn't wasted, even though I didn't feel I had done any significant research except the paper with Walter.

When I first went to Scripps, Roger was at sea on a geological and geophysical cruise. He flew back a few weeks later, and at a luncheon seminar, he reported the work of the expedition. He did a beautiful job and generated a lot of interest and excitement. That was Roger at his best. In that respect, he had leadership qualities. However, I had reservations. In my opinion, his big coastal oceanography program was ill-planned and badly organized. He had completely failed to develop the kind of interdisciplinary team of mature scientists that was necessary to get significant results. I would not have attempted a program of that magnitude without an assurance of enthusiastic staff participation.

Toward the end, Roger was making tentative overtures toward a permanent appointment for me there. I really didn't want it. Aside from my reservations about the effectiveness of his leadership, the ocean was less interesting than our varied oceanic environments on the east coast, and I wasn't as entranced with the California scene and the climate as some people were. There were, of course, distinct distinct advantages to being in a place where there were so many people and such varied specialties. Lucy and I liked a lot of the people, but she wasn't enthusiastic about staying either. We went back to New Haven.

One of the first items on my list when I got back was to finish the partly completed paper with Walter and get it into press. Also I had a lot of data on various eddy co-efficients that I had calculated in connection with the Riley-Stommel-Bumpus paper, the physical oceanographic survey of Long Island Sound, and the deep-sea work in the Atlantic Ocean and off the Pacific coast. I was curious to see if I could pull them together and make any sense of them. I developed a set of oddball equations in which the principal variables were current speed, distance between observations, and vertical stability. The fit was remarkably good, and except for the one for vertical diffusion in stable water, they were even dimensionally correct. However, they ran counter to current thinking about turbulence theory. I showed the thing to Hank Stommel. He shook his head and said, "I don't know what to think about it". Anyway, I had the brass to publish this little thing on parameters of turbulence in the sea. It was received with widespread silence. If the physical people noticed it at all, it was probably a bit of a thorn in the side. The accuracy of the fit was hard to dismiss, but they wanted to.

During the post-war years, I was gradually becoming involved in an increasing number of extra-curricular scientific affairs. I think it was about 1946 when I was elected to the Board of Trustees of the Bermuda Station. Columbus was then President of the Board, and there were several other members whom I knew: Ross Harrison, Norman (Jeff) Allen, a Woods Hole business administrator and Treasurer of the Board, and W.R. Taylor, the algologist. I won't try to go through the whole list. The Station had been converted into a hospital for war casualties but was reactivated soon after the hospital people moved out at the end of the war. Dugald Brown, a physiologist from Michigan had been chosen as Director. The immediate postwar years were difficult. A lot of renovation and restoration had to be done to make it once more usable for bio- logical work, and a program had to be started from scratch. They did not receive adequate financial compensation from the government for the restoration, and we had to dig deeply into capital funds. Survival of the Station was touch and go for some years.

As a new member of the Trustees, I wasn't deeply involved in the beginning. I went to meetings and visited the Station a few times when cruises put into Bermuda. Brown resigned after a few years, and Lou Hutchins, who had been working on that buoy project at Woods Hole, became Director. He was able to get a grant or two from the Bermuda Government for work on local fisheries, and somewhere around 1951 or 1952, he organized a conference of Bermuda people and fisheries people from the States to discuss their fisheries problems. I attended that one and for the first time met a young fellow named Bill Sutcliffe, who had been hired to do a life history study of the spiny lobster *Panulirus*, an important element of the commercial fishery there.

Bill had recently got his Ph.D. from Duke with a thesis on zooplankton in North Carolina coastal waters, and he seemed to be taking hold of the Bermuda work very well. He finished the survey in a couple of years, and he did a good job, although I don't think the government paid much attention to the work. For one thing, they had a closed season on lobsters during the spawning season, and although Bill demonstrated that this was unnecessary, they didn't change anything.

Bill found that the lobsters moved offshore temporarily to spawn and then came back, but not one of the young was ever found in the inshore waters. The ones that came in were several months older and must have drifted in from the West Indies in the predominantly northerly drift that occurred at that season. This may seem strange, but evidence had already been found that many benthic invertebrates in tropical waters have pelagic stages that are capable of drifting around for as much as six months. Else how could those isolated tropical islands have acquired such a similar fauna?

During the next year or so, Lou, sad to relate, became seriously alcoholic and had to be removed. He died a few years later. Bill became Director. Not long after that, Columbus resigned from the Presidency of the Board, and I was elected to the post in 1954. I'll take up that chapter later.

When Charlie Fish left Woods Hole and went back to the University of Rhode Island, he began once more to develop a marine lab. A building on the waterfront property that had survived the 1938 hurricane was suitable for installation of a wet lab, and on the main campus at Kingston, he obtained the use of an old building that had once been a county jail. The administration area and the women's quarters were converted into offices. There was no evidence that anyone behaved badly enough to be consigned to the tiny, grim cells in the basement. Charlie initially developed a master's program, which later was expanded to a Ph.D.

program, and as the organization grew and prospered, they gradually acquired more buildings on the Narragansett Bay location.

He invited several people from neighbouring institutions to serve as an advisory committee, and I was one of them. The duties were minimal-- a few guest lectures and conferences with students and talking with Charlie, or rather being talked at, about general policy.

Charlie had a rather small and not very impressive staff at that time. There was only one really bright one in the lot. That was Dave Pratt. He knew the field thoroughly and was an excellent teacher, but his research effort was minimal. He had too many other interests, principally music and painting, to be a dedicated scientist. However, I am mainly indebted to Dave for any intellectual stimulation that those trips provided.

When Charlie's first-year class of students reached the point of working up thesis material, he decided to lighten the burden on his small staff by farming out some of them elsewhere. I accepted two. One was Howard Sanders, who was doing a study of the winter herring population in Block Island Sound, particularly with respect to its food. It wasn't a very exciting project, although he was able to demonstrate a point that was not entirely clear at that time, namely that herring were not indiscriminate plankton feeders but were selecting, probably visually, large food items. Howard arrived in the fall of 1950-- in fact, he and his wife Lil sublet our apartment while we were at Scripps-- completed the thesis during the winter, and applied and was admitted to Yale as a Ph.D. candidate.

The other student was George Moskovits, a microbiologist whose thesis project was an attempt to determine to what extent bacteria were associated with plankton and particulate organic matter. The technique was simple-- perhaps too simple to be credible quantitatively, but URI was setting up thesis problems, not I, and was granting degrees. My only function was to guide things along. He was collecting net samples of plankton, plating an aliquot, and then putting another aliquot through a blender and plating that. The reasoning was that any number of bacteria on an organism would produce only one spot on the agar plate, but if the material was finely divided, the plate count would more nearly represent the total number of bacteria present. He did, in fact, get significantly larger counts in the second series.

George also applied to Yale and was admitted to the Micro- biology Department. However, George was a plodder who couldn't cope with a heavy workload. He just didn't fit into a department that had no marine interests and was heavily oriented toward basic biochemical processes. He failed his comprehensive exam and left. Poor guy-- he finally got his Ph.D. from Texas A and M after a total of ten years as a graduate student.

I believe it was in 1951 that I was invited to be a member of an advisory panel set up to review grant proposals submitted to the recently formed Biology Branch of the Office of Naval Research. I suppose this was an honor. All the others were older and more firmly established in their careers. There was of course a fair amount of work involved in reviewing proposals, but there were dividends. I got my first lessons in that new but all-important craft of grantsmanship, and in due time, I got a grant for research on Long Island Sound.

Most of our meetings were in Washington, but we had one in Honolulu at the University of Hawaii. Our arms were easily twisted by Bob Hiatt, a member of the panel and director of the Hawaii University's marine lab. It didn't cost ONR anything. They could get free rides for us on Navy planes and free housing in bachelor officers' quarters.

We were greeted at Hickam Field in true Hawaiian style by ladies from the marine lab bearing leis, and also Jane Austin, for by that time, she and Tommy were living there. He was employed by POFI, short for Pacific Oceanic Fisheries Investigations of the Fish and Wildlife Service, which had offices and labs on the university campus. In addition to panel meetings and some excellent dining out, I had a trip to the marine lab, a beautiful drive into the mountains with the Austins and an afternoon at POFI. Unfortunately, I didn't get to the Geophysical Institute, where my old friend George Woollard was Director. I'm not sure whether Al Woodcock was there or not. That's where he spent the latter part of his professional life.

The Director of POFI was Oscar Sette, best known earlier for a thorough study of the Atlantic mackerel fishery. The main purpose of POFI was to prospect the possibilities for a high-seas tuna fishery. Being a man of considerable breadth, Sette began by examining upwelling areas-- island effects and the equatorial area, using physical oceanographic methods, phosphate analyses, and zooplankton tows to find the most productive spots, thus limiting the more laborious work of exploratory fishing. In the course of this work, they made some significant advances in our knowledge of the equatorial current systems of the mid-Pacific. I was highly impressed by the program.

Tommy was in good spirits despite his disability. His main job was working up the physical data. I knew he would have loved to be out on the ships, but he was doing what he could do with only a brain and hands and was making the best of it. In later years, Tommy went back to Washington to the National Oceanographic Data Centre, and Sette went to California, where he spent the last few years of his career examining data on large scale temperature anomalies in the Pacific and their effect on fisheries.

Back to the Bingham Lab: We did not have formal appointments in the Biology Department, or perhaps Dan did, I'm not quite sure about that. Anyway, they had no oceanographers in Biology, and there was an informal agreement that we would be permitted to supervise students who wanted to work in that field. Dan had two students. One was Jim Morrow, an ichthyologist working on a life history study of one of the species in the Block Island collections. After graduation, he was appointed to a junior staff position in the Bingham Lab. The other was a not very bright student with presumably not very bright parents-- at any rate, they had bestowed on him the silly name of Forrest Glen Wood-- and I can't remember whether he completed his degree or not, but he got a job in an aquarium when he left us.

My first student was Rudy Haffner, who did his thesis on mid-depth fishes of the genus

Chauliodus. That may sound like an odd thing for me to supervise and there were others just as odd later. Our little lab couldn't afford a lot of specialists. I was willing to assume the position of a jack of all trades, provided the students clearly understood what they were getting into. When students had some particular thing that they wanted to work on, I encouraged them to do so. So when Rudy expressed an interest in mid-depth fishes and Howie Sanders later in bottom fauna, I agreed. I told them they would have to work out the systematics for themselves and take responsibility for sending specimens to specialists when they were in doubt. My functions would be to help them develop an acceptable and manageable thesis plan and to give them advice in general ecology and the mechanics of putting a thesis together. It worked then. I doubt that it would work anymore. The field has grown so much more complex and the literature so profuse that a specialist's help is needed.

I felt that Evelyn in his younger days had been an ideal supervisor, and he was my model. We had worked in the field together on our separate projects. He had helped me when I needed it, but throughout he had treated me as a junior associate rather than an underling, and when the thesis was finished and the manuscript was ready for submission, he declined to put his name on it. I felt that was the only way to produce independent young scientists, walking out into the world on their own two feet and with no handicaps. Too often when a senior man puts his name on a thesis, his name is remembered, and the poor guy who did most of the work gets lost in the shuffle.

Of course, it was never so on the Continent or in some British circles, where the Professor was a little tin god, and in modern America grantsmanship has eroded the old ethical principles. Too often graduate students are technicians assigned thesis subjects to dot the i's and cross the t's of the P.I.'s grant project, and he then rides on their coat tails to swell his bibliography. As Cicero (not one of my early friends) used to say when he was getting to be a cranky old man, too, "O tempora, O mores".

Rudy finished his degree and got a job at Wesleyan University in Connecticut. There were few opportunities for research there, and he wasn't strongly oriented toward research anyway. Eventually, he drifted into scientific editing and publishing.

The year 1952 got off to an uncomfortable start for me but quickly got better. Many months earlier, I had accepted an invitation to go to Ottawa for a meeting of the Canadian Fisheries Society and give a paper on the application of theoretical population principles to ecology. That was fine except that Lucy got pregnant and was due to deliver sometime around the first of the year, about the same time the meeting was to be held. One doesn't like to cancel a commitment for an invited paper, and there was no compelling reason for me to do so, for her brother Hank could take her to the hospital if need be. I was hoping she would deliver before I left and would be comfortably in the hospital when I went off, with Hank and Helen taking care of our girls. However, Christmas week came and went, and no baby. I shoved off on the night train from New York to Montreal and on to Ottawa. The meeting could have been very pleasant if I hadn't been so nervous, running to the registration desk in the Chateau Laurier in every free moment to see if there was a telegram from home. Most of the people were strangers, though very friendly ones, and I knew a few of the people well. Fred Frye from Toronto and Ron Hayes from Dalhousie were there. I had been associated with them in ASLO. Also there was A.G. Huntsman, a fellow member of the Bermuda Trustees. Huntsman belonged to Bigelow's generation and had much the same kind of status in Canada as the elder statesman of marine biology. He was a kindly old gentleman with a prickly manner that didn't fool anyone, who loved to argue at length about almost anything. He of course didn't believe any of this twaddle about population theory and argued with his usual amiable ferocity.

When I got back home, Lucy was still pregnant, although she had gone to the hospital one night with a false alarm. Finally our third daughter, Mildred, was born on January 13. She was nearly five years younger than Grace, an unfortunate spacing, but there had been a miscarriage in the intervening time, and the doctor had advised her not to have another pregnancy for awhile.

I received my ONR grant in early 1952 and started planning and tooling up for a survey of Long Island Sound. I had asked for a fairly modest one. It paid a technician's salary and bought reagents and a little lab equipment and fifty days of ship time a year. The Milford Station had recently acquired a stout little 50-foot ship, the *Shang Wheeler*, which was far more capable at sea than anything they had previously had. There was no problem about making arrangements for its use with Victor Loosanoff.

I worked out a program for routine sampling in the central part of the Sound and occasional longer cruises for more general coverage. There were eight stations in the central area, and I could occupy five in one day's time, so two would be sampled every week and three others on alternate weeks. My part included temperature, salinity, oxygen, phosphate, nitrate, chlorophyll, and water samples for phytoplankton counts. Georgiana Deevey was going to do zooplankton, and Howie Sanders was starting a study of bottom fauna for his Ph.D. thesis. Then another graduate student, Bob Conover, was added to the recruits. He wanted to work on zoo-plankton, too. We need something for him that would complement Georgie's work and be a little more suitable for a thesis than routine descriptive work. I can't remember just when we shaped that up, but it was fairly early in the project. The two dominant copepods in the zooplankton population were *Acartia clausi*, a winter and spring species, and *A. tonsa*, which took over in mid-summer and lasted until early winter. The complete life cycles had not been worked out, and he would do that. In addition, he would do experiments on feeding, food requirements, and temperature tolerance in order to try to find out reasons for the observed seasonal succession.

At that time, Bob was a second-year student. At the end of his first year, he had married Shirley MacMillan, who had just graduated from Oberlin College, his alma mater too, and Shirley was looking for a job in New Haven. Grace Pickford hired her as a technician. That did not work out well. Grace was basically a nice person, but she was meticulous in everything she did and very demanding and often hypercritical with technicians. I don't think there were any that she didn't reduce to tears at one time or another. Shirley was on the point of quitting her job and looking for something else when she heard that I was in the market for a technician. She came to see me. I knew she was bright and hard- working, and I was glad to have her. However, the transition was painful, and it was largely my fault for not being sufficiently communicative and diplomatic with Grace. She was furious. She felt I had stolen her technician, and for some time she wouldn't even speak to me. We patched things up eventually, but it cast a bit of gloom on the beginning of the project.

Somewhere around this time-- I can't remember just when-- another staff member was added to the lab. She was Sally Wheatland, a cousin of Dan's, who had a master's degree in biology and was hired as a research assistant but quickly demonstrated her ability as an independent scientist. She took on the job of identifying and counting fish eggs and larvae in the zooplankton collections. In later years, she went on to various other projects.

The program got underway in March, and although few of the people had any prior seagoing experience, we quickly got shaken down into an efficient operation. Those one-day cruises are difficult for anyone with a tendency toward seasickness. Most of them didn't have any really serious problems of that sort, but Shirley had more difficulty than most, at least in the beginning. It got better as we went along. After the first day, which happened to be a fairly rough one, I offered to relieve her of seagoing duty, but she would have none of it and quickly proved to be a capable assistant at sea as well as ashore. In addition to assistance with routine collection and analysis, she studied phytoplankton systematics with a minimum of help from me and did the routine counts. She used this material for a master's thesis, which was then converted into a paper for publication, and she also wrote a little paper on a red tide event in New Haven Harbour. If Shirley had been a single gal, I certainly would have encouraged her to stay around for a Ph.D. As it turned out, she got it eventually, a number of years and four children later.

Howie Sander's preliminary literature search before he started his bottom survey convinced him that he should use a smaller mesh size than was ordinarily used for sieving bottom samples, even though this involved extra work. He also decided to use an anchor dredge for collection, which obtained a large sample than bottom grabs and seemed to be quite suitable for quantitative work on soft bottoms of the sort he would be working on. This too, made extra work for him, but Howie was one of the most patient and persistent people I have ever known. I suppose that is a necessary characteristic for a bottom fauna man, but it was particularly true in his case. I still have a mental picture of him sitting on the after deck on a cold winter day, hosing and sieving and apparently impervious to the cold, long after Shirley and I had gone into the lab to warm our numb fingers and start processing our samples.

I was well pleased with the progress that Howie was making in research, but some of the zoology professors found him less than impressive in course work. There was some question as to whether he was really Ph.D. material, and I had to argue fairly hard to defend him. I'm glad I succeeded.

In retrospect, I have to say that I have no confidence in our ability to pick the best candidates for admission to graduate school nor, after knowing them some time, to make an accurate prognosis as to their future. They are the late bloomers like Howie, and then there are the brilliant young ones like Larry Slobodkin and Freddie Smith, who hit their peak early and never progress much further. Those two had it too easy. They never had to acquire Howie's kind of patient work habits in order to get along. And although they were inspiring teachers and have some fine publications to their credit, they certainly did not produce the large and solid bodies of brilliant research that they might have. Some years later while attending some meetings at the University of Michigan, I was invited to Larry's home for the evening. And I remember him remarking with his usual candour, "I used to be regarded as a child prodigy, but I had my thirty-fifth birthday the other day, and I really can't be called a child prodigy any more. It's time I settle down and do something" But he didn't. Not a lot, anyway. As for Freddie, I was reminded of him when I saw a recently published paper on benthic fauna in Block Island Sound which cited his work as a Yale thesis. There was good stuff in it on competition problems as well as descriptive information on the fauna, but despite our prodding, Freddie was too lazy to prepare that thesis for publication. Of course, I'm not implying that a brilliant mind is a handicap. There are people like Evelyn and Maurice Ewing who have been prodigiously productive. If there is a valid predictive formula I haven't found it.

Despite his slow start, Howie achieved recognition quite early in his career through two discoveries that might be called lucky breaks but which stemmed directly from wise early planning in his choice of methods plus his usual painstaking work. One was his discovery of a "living fossil" in Long Island Sound, a tiny primitive crustacean with characters that made it appear to be the descendant of an ancestral form that gave rise to several of the modern orders. A picture of *Hutchinsoniella* even made Life Magazine. Then a few years later, the use of an enlarged version of his old anchor dredge in the deep ocean brought up a diverse assemblage of small infaunal invertebrates that was unknown until that time, having escaped the coarse meshes of the dredges that had been used during more than a hundred years of deep-sea exploration, thus giving rise to a new and exciting chapter in deep-sea studies.

I'm getting very far ahead of myself. Bob Conover had no difficulty in convincing the Department that he was a suitable Ph.D. candidate, although there were minor problems along the way. After the first day of written comprehensive exams, a crisis situation arose. No one could read what he had written. Bob's handwriting was virtually illegible to everyone except Shirley, who no doubt had considerable practice reading it during the year after he left Oberlin. The crisis was resolved by having Shirley type it that evening and continuing to type his answers the next day as he finished writing them.

Bob was an ex-football player with good big-muscle coordination but at that time had remarkably little manual dexterity, considering that he later built most of his reputation as an experimentalist. There were occasional crashes in the lab, accompanied by more noise and temper display than Evelyn had ever exhibited. I shouldn't tell naughty stories about a now highly respected senior scientist, but one classic case comes to mind. He was titrating an oxygen sample and shaking the flask so vigorously that he broke the tip off the burette, making it completely use- less. He swore and slammed the flask down on the table, and it shattered. He got up and stamped out of the room, slamming the door behind him. The door had a glass window, and it fell out and crashed on the floor. Sally and I had been watching and trying to be as quiet as two mice, and we managed to hold back the guffaws until he had stamped up the stairs.

Despite occasional crashes, I got along fine with Bob. There was just one thing about him that really annoyed me. He was quite unsympathetic about Shirley's tendency to seasickness and even teased her about it. The gal had a lot of grit sticking it out on the occasional rough days and didn't deserve that treatment. Finally, I got my revenge. We were on one of our longer cruises to the eastern end of the Sound, and I stopped at an anchor station to measure currents in order to make some estimates of transport ex- change. We were well into the job when a dirty sou-easter blew up. I desperately wanted to complete at least one tidal cycle or the whole thing would be wasted. It was hard on poor Shirley and was beginning to get to Bob, too. He made his sacrificial offerings to the sea god, and by the time the skipper and I hauled anchor, we had two limp bodies lying on the deck. Of course, none of this seriously affected our friendship over the years and the high respect that I have for the work that Bob has accomplished.

At the beginning of the survey, Joe Lucash was the skipper, but the Milford lab was growing, and they needed him ashore on a full-time basis as building superintendent and shop man. They hired Mike Glas as skipper of the boat and a general helper for Joe when he wasn't needed at sea.

Mike had been Fire Chief in West Haven. Retirement is fairly early in that kind of job, and I suppose he was about fifty at the time and was healthy and vigorous. He also was good company and a really nice guy, always willing to lend a hand if I needed any help on deck. His was a one-man operation, which was adequate for a boat that size and our kind of program. On the occasional cruises that lasted more than a day, we tied up at night, for we didn't have enough personnel, either scientists or crew, for continuous operation. At the end of a day's run, I had accumulated enough samples to spend most of the evening running off the analyses that needed to be done immediately.

At the end of two years, we terminated the first phase of the program. Bob and Howie had acquired the necessary material for their theses and would no longer be involved in field work. The rest of us wrote up that part of the survey while going on into the next phase, consisting of more extensive coverage of the Sound at less frequent intervals. All of the first set of papers were published in a single large volume of the Bingham Bulletin which came out in 1956 and was generally referred to in later stages of the investigation as the telephone book. Of course, Bingham Bull was the common name for our publication outlet.

In 1950, one of Harvey's last papers gathered together the information accumulated during years of work on the English Channel, attempting to quantify the major groups in the ecosystem as to biomass and productivity. By the time we had completed our two years' work, I had some information that could be used for comparative purposes, although it was by no means as complete as his and was obtained in somewhat different ways. The Sound appeared to be superior in primary production, but much of this production went into a detritus food web, and the Sound was deficient in the production of suitable fish food and commercially useful species. I put this comparison into a general review of the Long Island Sound work, which I submitted to a Festschrift volume honoring Dr. Bigelow, which was published in 1955. I received a polite and complimentary note from him, but there were a few little hints that seemed to say, as I read between the lines, "So, young fella, at last you've learned that you can't put everything into an equation. There are individual species differences, you know". Yes, Henry, I didn't go into that project with any intention of modeling the whole ecosystem, and if I had, I would have changed my mind somewhere along the way.

During that period, I also took on another small research project. Charlie Fish had arranged to have plankton collections made at two of the stations where weather ships were located. One of these was in the central Sargasso Sea, the other off the Labrador coast. He was working on the zooplankton hauls, and he let me have the water samples from the Sargasso Sea for phytoplankton counts. The weather ships also took BT lowerings, and I was able to get the readings from Fritz. Both the plankton populations and the temperature readings were quite variable. The latter were real, according to Fritz. He insisted that the BT's were well calibrated. That part of the subtropics clearly was not as uniform as subtropical and tropical waters are supposed to be. Perhaps it was not typical, for it could be subject to intrusions of cold core eddies from the Gulf Stream and indeed I found temperate and even neritic diatoms in the samples. Certainly in that area much of the diversity was due to admixture of slightly different water masses rather than development of a diverse population in a uniform surface layer. This point of view was strengthened a few years later when I examined some tropical samples, part of which were taken in areas that had been subject to local upwelling events. Each area had its own limited and distinct flora, which then could lead to increased diversity by mixing processes along the line of flow. That is perhaps at least a partial answer to that question that Evelyn flung at me many years earlier--that nowhere are ocean waters uniform enough for that inter-specific competition equation to operate as neatly as it does in a test tube. However, in an increasingly process-oriented field, for which I bear some share of the responsibility, some of those old problems have been shoved under the rug and forgotten while still in an unresolved state.

I continued to be on the list of research associates at Woods Hole for several years and went there from time to time for conferences or informal visits, and of course Lucy always liked to go there to visit old friends. We often had invitations to come and stay for a few days with the Ketchums or the Fuglisters and later with the Conovers or Sanders after they moved there.

When federal funding began to be available for biological work, Buck was able to start recruiting the sort of team that was needed for effective research at sea as well as experimental work in the lab--Ryther, Yentsch, Guillard, Vaccaro, to mention a few of the biologists, and Francis (Dick) Richards, a chemical oceanographer.

One of my favourites in that group was Charlie Yentsch. He had started graduate work at the University of Washington under Dick Fleming's supervision, but that was too confining for his independent spirit. He left without completing his degree and came to Woods Hole. There he worked in close association with John Ryther on several projects but also was able to pursue some innovative ideas of his own. In the late Fifties, he became a Research Associate despite the fact that he lacked that union card. In my opinion, the promotion was well deserved.

The seagoing work of this group during the mid to late Fifties was principally concerned with a study of the New York Bight and the adjacent Moriches and Great South Bays. There were occasional deep-sea cruises, too, principally between Woods Hole and Bermuda. I remember meeting one of those expeditions once when I was visiting the Station; I can't remember exactly when it was.

In the late Fifties, John Ryther began a study of the plankton at a station near Bermuda, using the Station boat, the *Panulirus*. Much of the routine work on this project was done by one of John's assistants, Dave Menzel. Dave had been a resident at the Station for several years. He had got a Ph.D. from the University of Michigan with a thesis on the ecology of Bermuda reef fishes but did not return to that after switching to plankton ecology. When their Bermuda program was finished, he moved to Woods Hole and continued to work with John for some years.

When Howie Sanders went to Woods Hole, he gradually developed an active little group of benthic investigators who were working on adjacent coastal water problems and also the deep-sea infaunal population that Howie had discovered. I don't recall any of these early assistants. The first one that I got well acquainted with was Bob Hessler, who went there about 1960 and was Howie's right hand man for several years until he went to Scripps and organized a group of his own.

Another recruit, whom I had known for some time, was Fred Grassle. Fred got interested in benthic fauna as an undergraduate at Yale. I helped him a little with his honors thesis. As I recall, he then went to Duke for his Ph.D. before settling in for what appears to be a permanent post at Woods Hole.

During these visits to Woods Hole, I was of course seeing physical oceanographic friends as well as biologists. This was educational for me and perhaps for Institution staff as well. With the increase in size of the staff, inter-disciplinary communication had gradually faded. I found I was frequently informing people about things that were going on in the Institution that they were unaware of. This is an inevitable side effect of large size.

Fritz Fuglister was busily engaged in descriptive oceanographic studies, particularly meanders and eddies in the Gulf Stream system. Bump had developed a rather large program for studying LaGrangian movements with drift bottles, sea-bed drifters, and floating radio buoys. Bill Von Arx took a leave of absence to get his Ph.D. at MIT. He was working on a small text on theoretical physical oceanography, only moderately technical and aimed at the advanced undergraduate level but suitable for biological oceanographers with limited mathematical ability. He taught at MIT for some years and probably was an excellent teacher. I heard him give a few seminars. They were polished performances, and he was able to express rather complicated ideas in clear and simple terms. Bill's principal research interest was in the development of oceanographic instrumentation and its application to oceanic problems. Unfortunately, he never quite achieved one of his major goals, which was to develop a level instrument to deduce ocean currents by direct measurement of the slope of the sea surface. He failed by about an order of magnitude in getting sufficient precision for that.

Val Worthington became unusually expert in the technical work of collecting physical oceanographic data. His scientific efforts tended to be iconoclastic and controversial. His first effort along these lines was really wild. He concluded from examining the accumulated data on oxygen concentrations in mid-depth and deep North Atlantic waters that there had been a slight but significant decrease of about 0.2 ml per liter during the preceding twenty or thirty years. Extrapolating backwards, he hypothesized that the whole deepwater mass had been produced catastrophically during a cold period in the early 19th century, with little further formation thereafter. There were several good reasons for not taking that hypothesis very seriously, one of them being that his estimate of temporal change leaned heavily on Seiwell's early data. His later theories about the general circulation pattern in the North Atlantic were also controversial but not quite so easily dismissed.

Hank Stommel, of course, was building a solid reputation for himself in the field of theoretical physical oceanography during this period. During the late Fifties, I was invited to a meeting at Harvard to discuss the question of whether he should be offered a full professorship. This of course was a little atypical in that he had never acquired that well-known union card. My presence in the meeting was atypical, too, the only biologist on the committee, but I guess they wanted a little variety and picked on the only person outside his field who had a published record of collaboration with him. I had little to contribute and don't remember much about the meeting, but I recall one of the MIT professors remarking that Hank's mathematics was crude but powerful. I don't know enough to know whether it is crude or not, but certainly his influence on the field then and thereafter has been powerful.

Unlike a good many other theoretical people, Hank liked to go to sea and do experimental work, too. Frequently, the results were informative but occasionally not. There was one period around 1953-4 when he spent a lot of time, money, and effort in an experiment off Bermuda with floating buoys in an attempt to determine whether wind drift conformed to the principles of the Ekman spiral. He got a mess of results that didn't conform to anything. Years later, I attended a Dalhousie seminar in which the speaker started with a classical Ekman spiral and then used a simulation model to demonstrate the effects of a succession of varying wind directions associated with passing highs and lows. The poor spiral didn't adjust fast enough to keep up with changing conditions and was bashed practically beyond recognition. I realized more clearly than before that Hank had been dealing with an intractable problem, and his results were not necessarily a negative vote on the reality of the spiral.

Incidentally, when Bill Thompson started his undergraduate course in oceanography, he was able to persuade Hank to come and give a series of lectures in it and also a few seminars in our graduate course. This was great for us. We had a spare bedroom and were able to put him up during these visits. His charming young wife, Chickie, came with him on some of the trips. However, the commuting was a time-consuming nuisance, and there was only one year that he was willing to do that.

I continued to see the Rhode Island people from time to time. Somewhere around the mid to late Fifties, Charlie Fish resigned from the directorship of the Narragansett lab. The new appointee was John Knauss, a fairly young physical oceanographer from Scripps who came in with a determination to build the place up into a big establishment and with a tacit agreement on the part of the university administration for co-operation in the development. He began a major recruitment program for young staff members who were, on the whole, considerably more able than the senior members on campus and more diversified as to specialties. The department was upgraded to the status of a School of Oceanography with John as Dean, and he developed some programs that were not strictly oceanographic-- law of the sea, and such. He acquired a ship-- not a very good ship, to be sure, but an ocean-going ship about 180 feet long. The *Trident* was of World War I vintage, built for Army use, and apparently the Army's concept of a ship was something that was flat-bottomed and slab-sided like a river barge but pointed at the front end. Anyway, that's the way she was.

I was not longer going to Rhode Island as often as previously but enough to get on friendly terms with John and a few of his new staff members. One I particularly liked was Ted Smayda, a bright, young phytoplankton specialist with whom I had a number of good discussions. He was doing a good many things in Narragansett Bay similar to my program in Long Island Sound and was doing them somewhat better. He had incorporated new and improved methods. He also had spent some time with Trygve Braarud, a Norwegian planktologist, and was a better systematist than I was. Then there was John Sieburth, a bright but wild young microbiologist, full of ideas, some of them quite good. His principal flaw was that he had too many ideas and was not sufficiently self-disciplined to limit himself. Few of the things he started to work on were carried through to a really satisfactory conclusion. In the early Sixties, he was very helpful to me at a time when I was trying to set up a microbiological program in Long Island Sound. I had hired a technician, Susan Altschuler, who had training in basic techniques but little knowledge of marine aspects of the subject, and I was about equally ignorant. John's advice at that time was invaluable.

Now I've messed up the chronology again and have to backtrack to 1954.

I went into phase two of the Long Island Sound project, which was to be a two-day cruise every two weeks, going alternately to the east and west ends of the Sound. Thus, there would be biweekly coverage of two stations in the central part of the Sound and monthly visits to the remainder. I bade a fond farewell to Howie and the Conovers and had acquired excellent seagoing help from a new recruit named Eugene Harris. Gene was a Newfoundlander who had gone to Memorial College in St. John's, then a two-year college, and then had transferred to Dalhousie, where he completed undergraduate work and a master's degree under Ron Hayes. He had a good background in both biology and chemistry, and he decided to undertake a study of the nitrogen cycle, a difficult thing to do in those days, before the present simple analytical methods were developed. Earlier work by Shirley and me had shown that nitrogen was the most important limiting factor for phytoplankton growth in spring and summer. Nitrate was wiped out. Phosphate was present in excess. The importance of other nitrogen fractions, particularly ammonia, needed to be examined. So he began this study during our series of east-west cruises, but he became ill before the first annual cycle was completed and was unable to finish. An account of his life at Yale and death from cancer was published in an obituary that I wrote for the next issue of the Bingham Bulletin that was devoted to the Long Island Sound project.

After Gene's death, I was left with his data book, a few notes that he had made, and the memory of a good many conversations that we had about the work. Although incomplete seasonally, it was a significant contribution to our project. The nitrogen cycle had been studied in lab experiments but this was the first attempt to deal with the problem in natural marine waters in that degree of detail. It certainly was worth publishing. I wrote the paper, following as closely as memory permitted his own thinking about the results. There were a few gaps that I had to fill, perhaps no more than the suggestions that I might make about any student thesis. When it was completed, I published it under his name, with only a footnote explaining that I had compiled it posthumously. It was my memorial to him, the only way I could ensure an adequate recognition in the literature of a young man who deserved all the credit for the work and had lost the opportunity to do anything further toward a promising career. I was gratified later that his paper was noted and referred to and stimulated further efforts to elucidate the nitrogen cycle, which most certainly was more important than phosphate in most coastal waters and probably most of the open ocean as well.

After Gene became too ill to go out with me, Sally went along. She was a tall, strong gal, as able to tolerate dirty weather as I was, and she was a great help. Subsequently we worked together on several other projects.

The work was completed during a very black year in my life and the lives of the rest of my family. My mother and father both died during that year, and in September, Lucy's brother, Hank, suffered an attack of polio and died within a few days. All this, plus Gene's worsening illness inevitably affected the family, including our young girls, who never before had experienced the sorrow of the loss of a loved one and perhaps had never even thought about death in personal terms.

The work that was done on that phase permitted me to re-evaluate the physical oceanography of the Sound and to correct some errors in the earlier analysis that I mentioned before. Shirley again did the phytoplankton counts-- mostly during infant nap time, she said. After leaving Yale, they had gone to England where Bob had a postdoctoral year at Southampton and then went to the University of Rhode Island for awhile before settling for a longer period at Woods Hole. Their young family was coming along, but Shirley never deviated from her intention to return eventually to a professional career and was eager to do small jobs of the sort of thing I gave her. However, I wrote up the results, integrating them with other aspects of the survey that she wasn't familiar with, and we published a joint paper.

Most of our social life in those days was with Lucy's family, young staff members at the Bingham Lab, and graduate students. I was a bit isolated from the Biology Department, and although Dan and I were intimately associated at the lab, he moved in a very different social circle consisting mainly of college masters and fellows and university administrators, a majority of them conservative Ivy League types. Their values and interests were different from mine, and I didn't quite feel comfortable with them. I suppose I might at least have penetrated into the fringe of that circle, for at one time, Dan invited me to become a fellow of the college. I declined. Afterward, Bill Thompson chided me. Refusing an invitation like that was a no-no. However, I knew what it would be like. I had been invited a time or two to the weekly fellows' dinner and the evening of conversation and drinking that followed. They were a bore.

I disappointed Dan in worse ways than that. He was a good natured guy to put up with me at all. For some years, I had been the only member of the lab heading up a field program, but in 1956, he was planning a major expedition to Peruvian coastal waters, a beautiful opportunity for oceanographic work in that interesting area, and I refused to participate. The circumstances were peculiar.

Part of Dan's job as Master of Davenport was to entertain visiting alumni, and if they were wealthy alumni who might be persuaded to do something for the university, they received red carpet treatment. Dan had acquired a captive millionaire who was an ardent sport fisherman, and they were cooking up a deal between them. His name was Wendell Anderson. He headed up a Detroit factory that produced copper tubing, and he made enough money to warrant hiring an accountant full time to handle tax affairs and find loopholes. Dan brought him into the lab for general discussions. I'll say in all fairness that he was an affable guy though unprepossessing looking-- a fat, red-faced slob who looked like a heart attack about to happen (he did have one a few years later), and it being an election year, he was wearing an Eisenhower-Nixon button about as big as a saucer.

(Dan was also an avid sport fisherman. Maybe he had enough money to find tax loopholes; I never knew anything about that. He certainly was an ardent Republican. He came in one morning asking if I had heard Eisenhower's speech the previous night. "Yeah," I said, "but I can't remember a thing he said. What was important about it?" Dan replied that there wasn't anything very important in it, but it left him with a good feeling. I said I liked to get more than a good feeling out of a political speech. The conversation tapered off.)

The deal they had cooked up was essentially this: Anderson would make a gift to the university (tax free, of course) to set up an expedition fund to do a little oceanography and catch some big fish which would be used for Jim Morrow's ichthyological research and to prepare specimens for museum exhibition. The fishermen, of course, were Anderson and some buddies who were taking their wives along on a free vacation (paid for by the university fund), and I think the accountant had rigged it so Anderson actually made a little money in the deal because the gift put him into a lower income tax bracket. It was legal, of course, but to a guy who pays income taxes and never finds a loophole or gets a free vacation at government expense, it seemed a little smelly.

Dan and Sally and Jim went and a student of mine named Gerald Posner who was rather marginal and got a marginal thesis out of it. I particularly remember that thesis because of the ungodly amount of time I spent helping him to get it into acceptable English. I could have rewritten it myself in half the time, but that of course is verboten. I remember one exasperated moment when I said to him, "Jerry, I bet you can't parse that sentence."

"Parse? What's that?" He was one of the first students of the new wave of progressive education to reach graduate school. Unfortunately, there was more to come.

In contrast, there have been two occasions when students presented me with theses which I returned in one day with compliments and with no suggestions for any changes. Not entirely coincidentally, both of these people are now oceanography professors at Dalhousie. Pete Wangersky was the first. Eric Mills came along a few years later. Pete was Evelyn's student, but like some of the others who preceded him, Pete generally ate lunch with the Bingham gang and often dropped in to talk with me for a little while before lunch.

Pete had majored in chemistry at Brown and had gained some oceanographic experience before coming to Yale through jobs at Scripps and a Fisheries station at Galveston. His decision to switch from chemistry to biology seemed a little unusual, but he proved he could do it by passing the Ph.D. comprehensive at the end of his first year.

Evelyn had grant money to pay for a job which was also to be his thesis research-- an analysis of the distribution of several elements in some deep-sea sediment cores. The subject was somewhat inter-disciplinary, and he had a committee of five drawn from various disciplines. It also proved to be controversial as far as one member of the committee was concerned. Karl Turekian, a geochemist in the Geology Department, was the only one who had done that sort of work, and bad feelings developed when Pete demonstrated that a method Karl had used for calculating his results was statistically invalid. Pete was obviously right, and Karl was overruled by other members of the committee. Privately, Pete was critical of the sloppiness of much of what Karl did, and in cases where I understood the problem, I tended to agree. I remember attending the oral thesis defense of one of his students who was presenting a global balance sheet of some trace element-- I can't remember which one. Some of these early balance sheets were based on minimal data, and in this case, his data on "oceanic concentration" were based on two samples which had been obtained on trips with me in Long Island Sound. Fond as I am of the Sound, I never claimed it to be a typical ocean. Indeed the dirty little puddle was and is subject to unknown quantities of all sorts of pollutants, and any analyses of that sort are suspect. That time, I was the one who was over-ruled, but the rift between Bingham and Turekian widened.

On one occasion, Pete and I drove to Lamont Geological Observatory to pick up some core samples, and while he was doing his job, I spent an hour or so with Maurice Ewing, the first time I had seen him since early post war years.

While still at Woods Hole, Maurice had made increasing demands on Columbus for ship time to the point where Columbus reportedly said enough was enough. Maurice probably wanted to found his own lab anyway; at any rate, he had made an arrangement with Columbia University for joint appointments with some of his staff and had set up his lab in a fine old estate in Palisades, New York, just across the Hudson River from Columbia.

Busy as he was, Maurice could always take time to talk with an old friend, and he showed me around the lab a little, and we got caught up on the news of what each other were doing.

In addition to the core work, Pete also got interested in population dynamics, a subject that would continue to be at least of subsidiary interest during his later career in chemical oceanography. Together with an associate in applied mathematics, he re-worked the old prey-predator equations in a more realistic form, probably the most significant item being the introduction of a time lag between consumption of prey and the reproduction response of the predator. He demonstrated that the classical prey-predator oscillation was not and invariable response. By varying the time lag, he could get a family of curves, approaching an equilibrium state in the extreme case, as I recall.

I also found a little money to help support him at a time when he needed it. He did some analyses of dissolved carbohydrates-- resumption of a job he had done earlier in Gulf of Mexico waters-- and helped me with routine analyses. In that he put me to shame-- the only man I had seen who could get through a set of analyses faster and neater than I could.

And we fed him fish. Sally had a program going on juvenile fish. We got a shrimp try net and made a couple of tows every time we went out. Sally could shoot a trawl-- a small trawl at any rate-- as well as any man. She worked on the little fellows-- food and growth rate and all that-- and the big fellows were simply counted and measured and went into our stomachs.

We had several projects going during the latter part of the Fifties. We constructed a more or less quantitative dredge for sampling the large benthic invertebrates that are not well sampled by the anchor dredge. On each side, there was a wheel with projecting spokes and a counter mounted on the axle to record the distance dragged over the bottom. Sally also had a coarse-meshed, two-meter net to catch large pelagic larvae. We also would have liked to do something about pelagic fish but were unable to do so. They were spotty in distribution and seasonal abundance and required special gear to catch them. There were big runs of menhaden in spring and autumn in those days. I gather that population has since crashed, but they must have been somewhat significant eco- logically at that time. Once we dragged the two-meter net through a big school, with fish jumping all around. Catch zero. The commercial fishermen of course used purse seines.

Then there was Andrew Carey, another student. Drew was a rather shy and quiet and soft-spoken graduate of Princeton. He wanted to work on bottom fauna, and it seemed like a good thing for him. He had the patience of a Sanders; however, Howie had skimmed the cream from that problem. So, I suggested a slightly different tack. He would collect at a couple of stations to get a general picture of the population and pick a few representative and dominant species for a seasonal study of oxygen consumption. From this, he could extrapolate to an estimate of oxygen consumption by the whole infaunal collection. He was also going to take core samples for estimates of rates of total oxygen consumption over the bottom.

So he went to work and seemed to be doing all right. He continued to be rather shy. He always addressed me respectfully as Dr. Riley. Toward the end of the first year, I remarked, "Drew, everybody else in the lab calls me Gordon. You don't need to be so formal." He thanked me, but then for about a year, he didn't call me anything. Eventually, he got to a first name basis. However, he made me realize that the age gap was widening. I wasn't just one of the boys anymore. I was about twice as old as some of them when they first came in, and to them that must seem rather old. I shouldn't force informality on them if it made them feel uncomfortable.

Drew became a little less reserved as time went on, and on one occasion it cracked wide open. We were hauling in his anchor dredge. A shackle parted, and the dredge went down. Drew yelled, "Shit!" Mike Glas looked at me and grinned and whispered, "He's human!"

Drew's measurements of oxygen consumption in core samples varied somewhat depending on the rate of stirring that he applied to the water column over the mud. Earlier, I had made some estimates of bottom oxygen consumption, based on physical oceanographic computations, which fell about in the middle of his range. Surprisingly, the estimates of total oxygen consumption by the infauna that he had studied were hardly 10% of this total. Apparently, the bacteria, nemaodes, forams, and other little infaunal bugs were dominating the population, dynamically speaking, and we never really got around to studying that population although Martin Buzas, a geology student, went out with me for awhile and made a survey of benthic forms.

Along about this time, the starfish population was very large and was clearly the top predator in the system, exceeding fishes in abundance. In 1957, the starfish were crashing. They were extremely numerous but were small and flabby and appeared half starved. There were no big ones left. They had either died or had undergone negative growth, as starfish can. Martin Burkenroad had called the shot. In 1946, he had published a paper in Science in which he combined historical records culled from old newspapers with later scientific data, showing that since the mid-1850's, at least, there had been cyclical variations in abundance with a period of 14 years, more or less. He predicted that the next peak should be about 1957, and there it was. In view of the apparently starved condition of the animals, there was a suggestion of a prey-predator cycle in the classical mode. However, I suppose we will never really know that, having only fragmentary information on the small clams such as *Nucula* and *Modiolus,* which are the principal diet of
the starfish.

I'll briefly mention two other graduate students of the late Fifties and then backtrack to some other matters. One was Bill Pearcy, a bright and personable student, who did a thesis quickly and well on the spawning of the winter flounder in the Mystic River estuary and the larval and juvenile development. He and Drew both went to the Oceanography Department at Oregon State and are still there, at present writing.

Ted Napora was a problem, competent in research but academically poor. He finally passed his comps on the third try, one of the few ever permitted a third opportunity. He did most of his thesis research at Bermuda, a study of oxygen consumption and phosphate excretion of a mid-depth prawn and a copepod at varying temperatures and pressures. His results indicated an increase in rates with pressure, approximately balancing the effect of a temperature decrease at the daytime level. However, subsequent studies along the same lines have yielded varying results, so that problem needs further studies.

One summer when Ted needed a little extra money, I asked him to examine the tintinnid fauna in Long Island Sound. We hadn't done much with microzooplankton, but I suspected they might be important. I still have good reason to believe this, but tintinnids proved not to be abundant enough to provide the answer. There are, of course, huge swarms of invertebrate larvae in summer, and they may be the answer, but they are too spotty in space and time to be easy subjects for quantitative study. Despite years of work in that mud puddle, I just wasn't getting the ecosystem put together the way I wanted. So much for that. Ted eventually finished and went to URI.

Somewhere around the mid-Fifties, I went to a meeting of the International Union of Geodesy and Geophysics in Toronto. I knew few people and felt out of place in that kind of conference. However, Hank Stommel had been asked to organize a symposium on deep-ocean circulation and invited me to present a paper. I don't remember all that I put into it, but one item was a plea to look at all the available data in a broad and interdisciplinary way and make sure that everything fits. I pointed out that coefficients of eddy diffusivity that I calculated in the 1951 paper were considerably smaller than some of the published values which had been obtained by making some simplistic assumptions. The latter were clearly invalid because if they were applied to oxygen distribution in the deep ocean, the apparent oxygen consumption would be larger than surface production. Unfortunately, one of the other papers, by an oceanographer from Miami named Fritz Koczy, had used radio-active elements to calculate vertical diffusion, and he had some of those big numbers, too. He refused to admit there could be anything wrong with his calculations, and there was an uncomfortable impasse.

There was just one nice thing about that meeting. A stranger came up to me and introduced himself as John Strickland and asked if I would spend a little time with him so he could "pick my brains." He had been working as an industrial chemist in England and had recently taken a job in the Fisheries Station in Nanaimo, B.C., where he was assigned to develop a plankton program. I invited him to my dormitory room, and we spent all afternoon together while he fired a barrage of probing questions at me. And I'd like to think I had some small part in helping him toward his meteoric rise toward a position of eminence in the field, although I am sure he would have got there anyhow. He seemed to swallow the whole literature in one big gulp and was doing interesting and original things in a very short time. Some people regarded John as a prickly character, and I gather he was a tyrant in his own bailiwick, but I liked and admired the guy and mourned his untimely death a few years later.

About the same time, I received a letter from C.E. Lucas, Director of the government fisheries lab at Aberdeen, Scotland, whom I had met and knew slightly. He told me he had recently hired a young man with a mathematics A.B. to do statistical work on fisheries problems. This man, John Steele, had seen the Riley-Stommel-Bumpus paper and was interested in doing some models himself. He would like to correspond with me and outline some of his ideas. The outcome of this was his first model, published in 1958, depicting the spring season of plankton growth on the Fladen Ground in the North Sea. He started with a two-layered system, simpler than my more complicated models, but with a similar series of equations for the interaction of nutrients, phytoplankton, and zooplankton. He was able to calculate seasonal cycles iteratively and to examine the effects of variations in vertical mixing rates, etc. Not long after that, he came to the U.S. on a visit , spending some time at Woods Hole and at the Bingham lab. That was, of course, our first meeting face to face, the first of a good many. We had a lot of pleasant discussions and some social life together, and he made progress on a model that he was working on then, depicting a tropical upwelling area and the development of plankton populations in time and space as the water moved away from the upwelling areas.

John was one of a good many visiting scientists whom we invited into our home. Lucy and I liked to invite them home for dinner, an informal meal with all the family. We felt that they were probably pretty ordinary people like us in their home life, who might enjoy getting a glimpse of American family life, and that our children would profit from meeting a few people from far lands. That was certainly true as they got older, although the first time we tried it, I had a feeling that we had perhaps started the custom too early.

This first visitor was Trygve Braarud, the Norwegian planktologist whom I mentioned earlier, and the time must have been about 1950 or 1951. He had been involved in some of the earlier work in the Gulf of Maine as well as in his home waters. There was a persistent rumor, which he denied, that during the war he had been the secret leader of the Norwegian underground for awhile. I never learned the truth about that. I noted then and at later times when I saw him that despite his calm and affable manner, there appeared to be a latent nervousness and persistent tremor of his hands. I didn't know whether that signified anything or not. During this period when I knew him, one of his principal interests was the systematics of the Coccolithophoridae, and he was using a scanning electron microscope to examine those tiny coccoliths that could never be seen adequately in a light microscope. He had some beautiful pictures of those intricate, crown-like bodies and was finding that they were useful diagnostic features requiring extensive revision of the existing classification system.

Trygve was a bachelor, but had a lot of nephews and nieces and was fond of children. He paid quite a lot of attention to our girls. Gracie, then about three or four, took a fancy to him. She was a cozy but squirmy little body, and between dinner and bedtime, she crawled up on his lap and gave him her usual kinds of attentions. He took it in good spirit, but Lucy was a little embarrassed. She said, "I think you'd better get down, Gracie. You're being a little too wiggly for Dr. Braarud." Gracie made it worse. Remembering previous incidents of that sort when neighbours were visiting, she asked, "Why? Is he pregnant?"

Georg Wüst was one whom I particularly enjoyed having to the house because I saw an aspect that I might not have been aware of otherwise. Socially, he was pleasant and lively and good company and did a good job of entertaining our girls, who were older at that time and more interested in the big world. Professionally, he was a bit crusty, nice enough but decidedly the German professor type. What he said was right because he said it. I recall trying to discuss the problem of the Antarctic Circumpolar Current with him. He and Sverdrup had used a reference surface of 3000 m, which gave a transport of about a hundred sverdrups. I had put it at 4000 m and nearly doubled the transport. Presumably, the west wind drift extended deep enough to carry North Atlantic Deep Water with it into the Indian Ocean, and there could be a reversal in Antarctic Bottom Water, but the demarkation was not well defined. I wanted to know what his criteria were. He didn't really explain. That was just the way he decided it was. He was really a nice, old fellow, but I wouldn't have wanted to be his student. However, Maurice Ewing hired him to teach physical oceanography at Lamont for at least one year and perhaps longer.

There were others from time to time-- Anton Bruun, the Danish sea biologist, the elder Zenkevitch, Leslie Cooper, one of Harvey's associates at the Plymouth lab.

Anton Bruun visited Yale twice, and I suppose I met him personally on both occasions, but my only clear memory of the first visit was a seminar in the Biology Department, in which he gave a fascinating talk about his studies of deep-sea animals. He was convinced that there are large animals down there that we haven't succeeded in catching yet. Of course, there is the well known case of the giant squid. I think that I'm correct in saying that none has ever been caught in a trawl, although it is a common dietary item of the deep-diving sperm whale. Bruun had a story, too, about having caught a leptocephalus larva four feet long. This leads us to wonder what mama eel looked like, in view of the fact that the leptocephali of the common eels are small leaf-like creatures. And was mama catadromous like her smaller cousins? Loch Ness monsters, perhaps? Those last wild speculations are mine rather than his.

The second visit was an odd and complicated thing. Bruun and Zenkevitch were travelling together and were visiting Woods Hole, Bingham, and someplace in New York, probably Lamont. In view of the fact that Russians are guilty of all sorts of evil things unless proven otherwise, they had to be escorted by an American, and I was interviewed by the FBI before and after the visit. As I recall, Grace Pickford, who knew Bruun quite well, drove to Woods Hole to get them, and they came to dinner at our place that evening. I also invited Alexander Petrunkevitch, a delightful Professor Emeritus in the Biology Department. He had been one of the young intellectuals of the Russian nobility who got involved in the 1905 revolution and was exiled thereafter; he finished graduate work in zoology in Germany and then came to the U.S. and had been at Yale for many years.

Zenkevitch knew almost no English, so Dr. Pete served as interpreter. Otherwise, Bruun knew a little Russian, and Zenkevitch a little German. They worked that out between them, and Bruun translated it into English. (These Danes put us to shame with their linguistic ability; I will say, though, that Zenkevitch's German was so elementary and halting that even I could understand some of it.)

One further complication was that poor Bruun had broken his leg. It was in a cast, and he was hobbling around on crutches. He probably was rather uncomfortable but was being brave about it.

There were scientific discussions the next day, but that evening was mainly social. Dr. Pete and Zenkevitch found that they had gone to the same preparatory school in Moscow, although Dr. Pete was somewhat earlier. There were reminiscences and discussions of changes in Russia, and I doubt if even an FBI agent could have uncovered anything subversive in that informal and friendly evening conversation.

I mentioned the fact that I became President of the Bermuda Biological Station at about the same time that Bill Sutcliffe was installed as Director, and that continued for about seven years. There really isn't much to say about it, and it didn't take a great deal of my time. There was an annual meeting in New York at the American Museum of Natural History and generally a summer meeting of the Executive Committee in Bermuda. Bill, of course, knew much more about the affairs of the Station than anyone else and had sound common sense in developing policy. Generally, I just discussed things with him and was supportive. We worked together in trying to get gifts and grants and frequently conferred about financial matters with Frank McDowell, the Treasurer of the Station and a Yale accountant. We were gradually building up the endowment fund, as well as getting additional funds, and were no longer running a deficit.

The personal friendship that developed with Bill and the scientific association were of course more meaningful to me than Bermuda business. Bill was not broadly educated but was an original thinker. He got a good idea and pursued it tenaciously, picking up the necessary background information from reading and discussion as the project developed. During the course of his career, he dev- eloped several quite different kinds of research, in some cases only to the point where other people jumped on the bandwagon, and then he turned to something else. Our association, of course, continued long after both of us had resigned from our posts in the Bermuda Station.

During the period from 1958 to 1960, I attended three annual conferences at Princeton that were organized to test the question of whether free and informal discussion among a group of scientists might be a more effective way of disseminating knowledge than sessions of formal papers. I don't think we proved that one way or another, but I think they were more fun and gave us a better chance to get acquainted.

Part of the deal, in the foundation that supported us, was that in order to make the conferences useful to more people than just the participants, the discussions must be made available to the public. So a stenographic record was kept, and a volume was published on each conference. In a weak moment, I accepted the job of editing the first volume, and a worse mess I never got into. A verbatim record of conversation never looks good on paper, and it is particularly bad when a good many of the discussants are foreigners with a limited knowledge of English. Of course, if it is thoroughly edited into a good literary style it doesn't resemble conversation any more. A good novelist ought to be hired to do that kind of job. I fought my way through it, trying to keep to the middle ground. After that, I refused to do any more editing, although I attended two more conferences.

Each conference had a good mix of North Americans, British and Europeans, some old friends and acquaintances and some new faces. One of the latter was Einer Steemann-Nielsen.

I suppose most people who have been in biological oceanography for awhile are aware that when Steemann-Nielsen first published his work on the carbon-14 method, he quite bluntly attacked my estimates based on light and dark bottle oxygen experiments. I was annoyed and replied with an equally blunt letter to the editor. I think a lot of people thought that a rancorous feud had developed, but it wasn't quite like that. There was an unresolved difference of opinion, but it stopped there, with little more being said about it. When we finally met we were able to do so cordially and discuss our differences without getting heated about it. Of course, he realized by that time that there were some problems in his method, too. We made a few concessions on each side, although differences of opinion still remained. We parted on quite friendly terms. (Of course, all this is still far from settled.)

I can't recall now who came to which conferences, and there really isn't a lot to be said about them. It's all there in the books for anyone who wants to look. It was a great opportunity, though, to see some people I had known previously and didn't have a chance to see very often, as well as meeting new people.

Ramon Margalef, of course, was present at some of these meetings. In the oceanographic world, Margalef is commonly regarded as a one-man National Oceanographic Institute of Spain. Pete Wangersky, who has been there, could no doubt tell us if there is anyone else there who is doing anything interesting, but Ramon is the only one we hear about. In his isolated position, he needs to get out from time to time and communicate with others and frequently succeeds in doing so. I have seen him a good many times. I don't know anyone who doesn't like him, but communication is sometimes difficult. He is best known and most highly respected for his theoretical concepts, and his English isn't quite up to expressing himself clearly, either in conversation or on paper. I frequently have to puzzle over his meaning. He has also done a tremendous amount of descriptive and experimental work, mostly in pursuit of original ideas, with some interesting results and some rather trivial. That is to be expected of a man working in isolation. Few of us could do as well. He was certainly a welcome addition to Princeton meetings.

John Strickland was there, and there was considerable interest in work he had been doing analyzing changes in populations and associated seawater chemistry in some large plastic bags that he filled with seawater and suspended in the sea. We asked him if he intended to do more of that sort of work, and he replied, "No, I think I've learned all I can playing around with old bags." He probably would have some scathing remarks to make about the later CEPEX and MEERL experiments. However, times change. I would be inclined to agree that not much more in the way of pure science can come out of such experiments than John got, but I do think they can be useful tools in getting at some of the pressing practical problems of chronic pollution.

John also had no high regard for plankton models, and John Steele and I had some lively discussions with him about that. However, after I published what was probably to be my last phytoplankton model, a simple little thing that appeared in a Redfield Festschrift in 1965, John wrote a note saying he had changed his mind. He learned something from that one.

That was after I moved to Dalhousie and was thinking about staffing problems. I wrote a rather tentative letter asking if he would be interested in joining our staff, and if so, I would go to bat for a name professorship. He declined with thanks, saying he wasn't very well and felt it was best to stay where he was. I didn't know it then, but that was the beginning of the end for John.

Those conferences were also the last times that I saw Trygve Braarud, although he is still alive and recently sent regards via a mutual friend who was visiting him. I don't know how old he is, but he is probably in his eighties.

In 1957, the U.S. National Academy of Sciences formed a committee on oceanography--commonly referred to as NASCO-- to examine the state of the science and make any recommendations that seemed to be needed for further development of the field. In the thirty years since Bigelow's earlier report to the academy, oceanography has grown tremendously and not altogether wisely. There were a few big centers of excellence, which, however, were plagued by financial problems. Many small marine labs had sprung up. Virtually every university on the seaboard had to have one or thought it did. Many were too small and too poorly staffed to be viable. Better training programs as well as financial support were needed if the field was to grow well.

Every expanding scientific program, of course, has growing pains-- the need for money for shore-based facilities and equipment and staff support, as well as recruitment of good people. Oceanography had all of these plus some problems that were unique. Ship operating costs were tremendous, and new ship construction was badly needed. The existing fleet was aging, and some ships were conversions that were not very well suited to oceanographic work.

A second unique problem was the fact that oceanography was so highly interdisciplinary and was having a hard time being accepted by universities as a science in its own right. There had been a time, of course, when "natural philosophers" of intellectual breadth could span the whole field of science, and as recently as a hundred years earlier, Wyville Thomsen, for example, had held chairs in botany, zoology, and geology. In the meantime, however, science has become narrowly compartmented, and American oceanographers typically got their start by specializing in a particular science and then learned oceanography by on-the-job self training, just as I had done. Scripps had led the way in developing an oceanographic curriculum which included basic training in all of the disciplines and specialization in one. A few others followed in post-war years, but most universities, and particularly their departments of basic science, were very resistant to that kind of development. There were places such as Yale, which produced lop-sided oceanographers; they graduated with a reasonably good education in biological oceanography but only a smattering of other aspects of the subject, and on a couple of occasions when we did, they did not find a happy home in other departments of basic science and were unable to accept graduate students in their specialties. At MIT, a student could get good training in geophysical fluid dynamics and its applications to oceanography and meteorology, but those students came out as lopsided oceanographers, too. In a good many cases, people went into oceanographic jobs with a good training in basic science but almost no knowledge of oceanography at all. That kind of person could be valuable as a member of an oceanographic team, but in a small lab and without the guidance of trained oceanographers, he was unlikely to be able to identify and evaluate the important oceanic problems that needed to be studied.

All these problems needed to be addressed by NASCO. The committee was headed by Harrison Brown, a geochemist from Cal Tech. Dick Vetter, my former neighbour at Scripps, was a full time executive assistant. Other members included Columbus, Roger, Maurice, Athelstan (Spilly) Spilhaus, inventor of the BT, Benny Shaeffer, a fisheries biologist. Also there was Sumner Pike, an elderly fish plant operator who had also served on the Atomic Energy Commission and was well acquainted with the Washington scene, Fritz Koczy, who was a nice guy despite the fact that we squabbled at the AUGG meeting, and I. I served on the committee for five years.

We met in Washington and at various oceanographic establishments around the country, often weekend meetings to avoid conflict with our regular jobs. In the Washington meetings, we were frequently joined by bureaucrats working for government agencies that dealt with marine problems in order to get their viewpoints. There were four to six meetings per year, and in addition, most of us at one time or another attended Senate and House committee meetings to present our various cases for support. Generally, they listened politely, and asked a few questions, and dismissed us with thanks. We seldom knew whether we had made any impression or not. I learned, though, that most of these men were brighter than one might think, judging from their public utterances. They had mastered the technique of being able to talk for any length of time without saying hardly anything, which generally was politically wise. However, their questions were penetrating, and in informal discussions, they were knowledgeable and logical.

There were several additions to NASCO during the early Sixties. They included Paul Fye and Don Pritchard. As directors of growing oceanographic institutions and experiencing the associated growing pains, they were both useful to the committee. Don was a thoroughly nice guy although somewhat reserved. We never became close friends but might have if we had seen more of each other. There perhaps wasn't anything very nice about Fye if the opinion of my friends at Woods Hole was correct, although there was nothing about him that I found personally offensive.

I can't say the same for Dixie Lee Ray, who also became a member around 1960. She was a marine biologist from the University of Washington, where her research specialty had been those little wood boring creatures known as gribbles. Many were the times when I wished she would crawl into the woodwork, too, and stay there. She was flamboyant, opinionated, and argumentative, and it just so happened that she and I were at opposite poles in just about every opinion she expressed. On the rare occasions when I happened to agree with her, I carefully re-examined my position to see if I could discover where I went wrong in my reasoning.

By some means that was never obvious to me, she managed to bore her way into some rather important posts. The only obvious component was sheer brass, which hardly seemed sufficient. While serving on NASCO, she was also a special consultant to NSF. Later, she served a term as AEC commissioner. In both of these jobs, she was controversial. In fact, some said they abolished AEC and reorganized it under a different name and mandate to get rid of Dixie.

Then, back home in Washington State, by golly, she managed to get herself elected as governor. However, after one term, the good citizens had acquired the same opinion of her that I had, and she was defeated by a landslide vote. What she is doing now, I neither know nor care.

The result of the committee work was a book dealing with problems and needs for orderly growth of the field during the Sixties. And I think it was worthwhile. We got a fairly large fraction of the things we wanted. When that job was completed, several of us resigned from the committee, including me, and others took our places. It has continued to the present day, now under the name of Ocean Sciences Board. Dick Vetter is still in the executive position, grinding out reports on various subjects from time to time. (However, since the above was written, there has been another change in name and mandate, and Dick no longer is involved.)

There was fun and good companionship as well as hard work on the committee. Columbus and Maurice, of course, were my two oldest and best friends, but I liked the others, too.

During this period, Maurice was as busy as ever, although he suffered physical disabilities as a result of an accident at sea. He was always in too much of a hurry to get things done. There was a saying around Woods Hole that he went to sea with a bag of nuts and bolts and tied things down and built his apparatus on the way to the station. On this occasion, he was caught in a squall off Bermuda with some drums on deck that hadn't been properly lashed down. In the rush to secure them, he was swept overboard. Old Captain MacMurray (Maurice had taken him on after he retired from the *Atlantis*) managed to back down, and they got him back aboard. His neck was broken, fortunately with little damage to the spinal cord, and that mended, but at the time I saw him, he was having trouble with one foot and ankle and had aged prematurely. He later retired from Lamont, presumably a mandatory retirement due to age. He went to Texas and started a new geophysical lab but died only a few years later. I think he died from overwork.

Several other members of the committee have departed, too-- Fritz and Benny and Columbus. Columbus, sad to say, had not made a successful transition from Director to ordinary scientist. After his retirement, the new Director was Admiral Eddy (Iceburg) Smith, formerly head of the Coast Guard Ice Patrol. He didn't last long. His administration was reported to be a disaster, but I don't know the details. Columbus came back briefly as interim Director, and then around 1958, Paul Fye was appointed. He had been one of the bang boys. I suppose he was an efficient director in a bureaucratic way, but he didn't know much about oceanography and made little attempt to establish rapport with the staff. A good many of my friends there thought he was a disaster, too.

Buck Ketchum had succeeded Redfield as Associate Director when Alfred retired. A lot of people, probably including Buck himself, thought the Director's mantle should fall on his shoulders. I was told by friends that Buck was bitterly disappointed, although he didn't mention it to me personally. Later, there was an attempted palace coup against Fye. That put Buck in an awkward situation. He stayed on the sidelines in a gentlemanly way although a good many people felt the Institution would have been better off if he had led the fight and had succeeded in deposing Fye. Anyway, Fye survived and went on for a good many more years.

As for Columbus, he hadn't really got back into physical oceanography again, and his variety of descriptive oceanography was no longer at the forefront of the field. On one occasion, I heard him belittle Hank Stommel in a rather sarcastic way and with a hint of envy, which was very unlike the Columbus I had known. I really don't know what he was doing except that he was somewhat involved in the international oceanographic merry-go-round-- SCOR and SCAR and all the rest of that alphabet soup. And he was drinking too much.

I saw him once more, in 1968, when we were invited to the URI campus and were awarded honorary D.Sc.'s. A little earlier, I had heard that he had been hospitalized for alcoholism. He was thin and had lost a number of teeth and looked like an old man. He spoke pathetically of our good times together. What might have been a happy celebration turned into a very sad occasion. A year or so later, we received news of his death.

An inordinately large proportion of my friends of that era developed drinking problems, and I was no exception. Most of us had it under control most of the time, and I don't think our work was seriously affected. Occasionally, it got out of hand, and I deeply regret that our families, in particular, suffered as a result. That period of rapid expansion, with the anxieties and frustrations of trying to achieve more than was humanly possible, was more than some of us could endure without resorting to that unsatisfactory crutch. In my own case, it was no longer a serious problem after I got out of the administrative rat race, but poor Columbus and a few others were unable to make that adjustment.

That old mansion on Hillhouse Avenue, much as I liked it and especially my pretty little office, was getting over-crowded as the Fifties progressed, and the lab facilities were quite inadequate for modern research. Dan had long cherished the hope of acquiring a new building, and eventually he achieved it. Wendell Anderson was a major contributor toward the cost. Although I have spoken of him somewhat slightingly, he was a generous man. In the late Fifties, we moved in. It was a new wing on the rear of the Peabody Museum. It had constant temperature rooms, a recirculating seawater system, a wet lab for handling new specimens, shops, space for expansion. That much we made sure of in our preliminary designs. A stupid architect put it all together and should have to be sentenced to live with his mistakes.

The exterior was modern glass and metal siding which leaked in wet weather and was beastly hot in summer. That kind of construction should be used only in air conditioned buildings if at all, and we weren't air conditioned. We had the first two floors, and the museum's ornithology unit had the third floor. This included a large bird storage room which had to have controlled temperature and humidity in order to keep the poor dear carcasses happy. The architect put that air conditioning unit right up there with them. He assured us that rubber padding under the unit would eliminate vibration. It didn't. It came right down the concrete columns and lived with us. There was no microscopy when the unit was running, and sometimes even the glassware rattled. Oh well, I didn't have to live with it the rest of my life. The greedy museum people who grabbed the space when the Bingham Lab folded are living with it.

I'll dispose of another gripe or two before I move out of the decade of the Fifties and into the Sixties. A prophet is not without honour save-- I was a nobody at Yale, position wise, as the bureaucrats would say, at a time when I was beginning to get a fair amount of recognition elsewhere. I had come to Yale ten years earlier as an Associate Professor, assimilated rank. That meant that academically I was ranked as an Associate Professor; "assimilated" meant that it was a research post or a service job such as librarian rather than a departmental appointment to a teaching position. Then, in 1952, assimilated ranks were abolished, and I was simply a research associate, which could mean anything or almost nothing. To make it sound a little more impressive, Dan wangled a title of Associate Director for me. That helped, but at a time when I was putting a good deal of effort into formal teaching and supervision of graduate students, I felt I deserved a departmental appointment as a recognition of service and also to make it easier for me to deal with the department in student matters.

The reason I didn't get one was simple. J.S. Nicholas, head of the department, was not sympathetic toward ecology and ecologists, and this was a time when department heads were appointed practically for the duration of their professional careers and could be despots if they chose. He was an experimental embryologist, a former student of Ross Harrison. After Harrison retired, experimental embryology went downhill and ecology went up. During much of the Fifties, Evelyn and I together were supervising nearly half of the students in the department. That didn't make him feel more kindly toward me.

There was a practical side to this which was becoming more important than rank. Departmental salaries had a fixed salary floor. Dan negotiated Bingham salaries directly with the Provost, and Dan was nonchalant about money. He didn't seem to realize that some people need a salary in order to eat. Mine was about 70% of what it would have been with a departmental appointment. I was getting to a point where I was pinching pennies pretty hard to take care of a growing family, but I was too proud to beg for more money.

That era ended in 1959 with a palace revolution that overthrew Nicholas and installed a system of rotating chairmanships. The first chairman, Ed Boell, was also an experimental embryologist but otherwise bore no resemblance to J.S. Within six months, I was a full professor. The official title was Professor of Oceanography, Biology Department. I have always suspected that Nicholas and some others were expressing a last word in the matter, insisting that I had done nothing in basic biology that warranted a simple title of biology professor. However, in a wry sort of way, I liked that. I was the first and I think the only Professor of Oceanography at Yale.

Only I wanted to be a professor in an Oceanography Department. We had been pushing that for some time with little visible effect. The field was becoming too large and complex for us to be a reputable oceanographic lab unless we increased our staff and the number of specialties represented. However, there were all those difficulties that I mentioned earlier in connection with the NASCO report. There was little to attract physical and chemical oceanographers. Those departments had no interest in things marine, and their curricula were unsuitable for students to take their degrees in those departments and specialize in oceanography. Lacking departmental structure, we were destined to continue as we were, producing marine biologists with a minimal amount of oceanographic training superimposed on their curriculum in the Biology Department. It had worked for awhile, and we had turned out some good people. It would become increasingly unsatisfactory with further growth of the science. That was the kind of pitch we were making to the administration. I think they understood the situation, but there was a limited amount of money available for expansion of university programs. They dragged their heels, refusing to make a decision one way or another.

We did in fact succeed in hiring a young physical oceanographer, but it was not a happy home for him, and we were none too well pleased with his performance. He didn't last long.

Then we brought Pete Wangersky back. He had two years at the University of Miami Marine Laboratory and didn't like it very well. He returned with a charming young wife, Ellie, whom he had met there. He was able to get some good research done, but he didn't get a cross appointment in Chemistry or Geology-- Turekian blocked the latter-- and Dan was giving him a substandard salary. He could hardly consider it as a suitable place for a permanent position.

Enough of that for now. I'll write the final chapter of that story later. By 1960, I had completed eight years of phytoplankton observations in the Sound, although they had been minimal during the last few years, generally amounting to approximately monthly collections. I could see a considerable amount of variation from one year to another in the timing and the form of spring and autumn flowerings as well as in species composition and seasonal succession of species. Both seemed to depend to a large extent on year-to-year variations in light and temperature. Some of that information was presented at a symposium on estuarine problems in Savannah, Georgia, and was published in a book entitled "Estuaries," edited by George Lauff, the remainder in a paper in the Bingham Bull.

No one had really cracked that enigmatic problem of what causes seasonal succession of phytoplankton. I thought of it primarily in terms of species variations in temperature and light requirements, and field data seemed to support that point of view to some extent. I got a very different set of hypotheses when I discussed the problem with Luigi Provasoli, a longtime friend whom I had first met in Woods Hole in early post-war years when he was a recent and almost penniless immigrant trying to get a foothold in the New World. Later, he got a job at the Haskins Laboratories in New York where he did some excellent work on organic requirements of various algae. He had also succeeded in culturing the small littoral copepod *Tigriopus* in a bacteria-free condition, feeding it various algae and mixed cultures of several algal species. He had demonstrated that mixed cultures were necessary in order to maintain growth and reproduction for a number of generations. This was little more than a *tour de force*, for it wasn't pursued to the point of determining biochemically what was required for a balanced diet, but it was instructive to people who had tried to grow copepods in the lab and had failed more often than not.

Luigi and I visited back and forth from time to time and saw each other at Princeton conferences and discussed various problems. He thought about seasonal succession in terms of organic effects. He postulated that algae could produce extra cellular metabolites that would permit their dominance by inhibiting other species, or conversely, some metabolites might be usable and needed by other species, allowing them to increase and take over dominance. I don't think he had much evidence to support these hypotheses, but neither did I have very solid evidence to support my ideas about physical control. (Luigi incidentally was a north Italian. Once when I happened to mention the predominantly Italian population in New Haven, he said scornfully, "There isn't *one* Italian in New Haven. They're all *Sicilians*." He spat out the word. Later, there was at least one Italian family in New Haven when Luigi accepted a professorship at Yale.)

I had some NSF money that I could use to support a technician to examine temperature and light effects in cultures. We picked a few key problems to work on with cultures that we were able to establish in the lab. Poor gals. First one technician and then another worked on these problems for a total of two years and got absolutely nothing out of it. Maybe some people can get replicable results in lab cultures, but in ours, the individual variations in duplicates and triplicates were enough to mask the kind of small, inter-species differences that I was looking for. I still have that large folder of statistically useless junk in my files. I thought I might dig into it again some time, but I doubt if it would look any better now. Maybe I was lucky, though, that it was the only major Long Island Sound project that was a complete failure.

There were a few minor jobs that I haven't mentioned. One day, a youngster named Richard Larkin came into my lab for advice. His class was doing science projects, and he had read about drift bottles with notes in them and all that, and he wanted to try a drift bottle experiment in the Sound. I told him how to set it up. He did all the work himself, and because of the semi-enclosed nature of the Sound, he got quite a high rate of returns. I gave him a little more advice in writing his school report. He didn't want to write a manuscript for publication and probably wouldn't have been quite up to it. I did that, and we published it as a joint paper.

Helen Vishniac, a professor in Microbiology, was interested in doing cobalamin and thiamine assays in the Sound. I provided her with collecting facilities and added an ecological section to her paper. The cobalamin was of some interest to me, for it was high in these inshore waters and showed evidence of a seasonal cycle similar to that of phosphate, indicating utilization but not to the extent of being a limiting factor. *Skeletonema costatum*, the dominant diatom in the Sound during much of the year, was known to require cobalamin, and it is largely limited in its general distribution to inshore waters where this vitamin can be presumed to be abundant. It probably is a major factor in controlling the distribution of the species.

Susan Altschuler, who had just completed an undergraduate major in microbiology, applied for a job, and I had NSF money that could support her for a study of that important element of the ecosystem. The analyses of pelagic bacteria worked out well, but neither of us knew enough to feel competent to deal with the difficult and more important problem of benthic bacteria. I was hoping at that time that there would someday be a Department of Oceanography with new blood and new enthusiasm for completing a really intensive ecosystem study, but after ten years of it, I was getting weary of the task.

I also did a little model of nitrogen and phosphorus distribution, which was particularly applicable to Long Island Sound but appeared to be somewhat realistic for coastal waters in general. That essentially completes the list except for a study of nonliving organic matter, which I'll take up a little later.

Most of what I have described was published in the Bingham Bull and was slow in being produced, the last issue not appearing until 1967. As Yngve got older, he became an increasingly picky editor. I could tolerate small changes, but it got to a point where he was rewriting sentences and whole paragraphs, frequently obscuring or changing the meaning, for he really didn't understand the work well enough to make adequate major alterations. Both of us are a bit stubborn at times, and our personal relations deteriorated. I wouldn't accept his alterations, and he wouldn't accept my manuscripts without making the kind of changes he wanted. I think eventually Dan intervened to get the last of my manuscripts published. This is one of the few times in my life when a formerly good friendship has ended in bitterness, and I am embarrassed and regretful now.

Graduate students: Dave McGill, a junior staff member at Woods Hole, came to Yale on leave of absence to get his Ph.D. He came with a pile of data on soluble and total phosphorus collected in transects across the Atlantic Ocean that were conducted by Institution staff during the International Geophysical Year. His thesis topic was a sort of phosphate balance sheet for the Atlantic Ocean, in one sense, and extrapolation of the old Redfield et al. study of the Gulf of Maine and with some of the elements of my earlier work on deep-water phosphate. It was an acceptable thesis, but it really didn't break any new ground.

The other was Betty née Adair, later Betty Wood. She had worked with Helen Vishniac, which is how I happened to know her, and she got her degree in Microbiology. Her thesis was a study of nutrient requirements of *Skeletonema* under chemostatic conditions. This was one of the early attempts to use chemostats for algal growth, and there were problems. The little beast was slow to come to equilibrium, requiring long experiments, and had a bad habit of sticking to surfaces. Her trials did not encourage me to attempt chemostatic work with algae, although later work by other people with small motile forms was more successful.

I was also on thesis committees of two of Evelyn's students who were intellectually far superior to Dave and Betty. One was Ian MacLaren, a Canadian student, and I believe it was 1959 and early 1960 when he was doing his thesis research; at any rate, he had a lab next to mine in the new building, which was later occupied by Pete.

Ian had worked for the Fisheries Research Board of Canada on the life history of the ringed seal, a major item in the Inuit diet. He had also done a study of Ogac Lake, a semi-estuarine and brackish high Arctic lake. That survey, and particularly the zooplankton population, provided the material for a very good thesis. Ian had an unusual combination of talent in modern experimental work and a broad knowledge of natural history. He put that to good use later in projects such as a study of the rare Ipswich sparrow during its breeding season on Sable Island, although most of his work has been experimental.

The other one of Evelyn's students was Eric Mills, who came along a little later. I really didn't see a lot of him. He took only three years to get his degree, as I remember, with a thesis on the systematics and ecology of two sibling species of the amphipod *Ampelisca*, a problem uncovered but not studied in detail in Howie Sanders' earlier survey. Eric came in occasionally to give me a progress report and finally a thesis which, as I said earlier, needed only a comment on its excellence. He returned to his native Canada to a post at Queen's University.

Then there was Satoshi Nishizawa, from the Faculty of Fisheries at Hokkaido University, who got a postdoctoral fellowship to come and work with me for a couple of years, and I got a grant to support his research. His particular interests were light transmission in seawater and nonliving particulate organic matter. We did the usual things to help the Nishizawas get settled in, looking for an apartment and Lucy helping Satoshi's pretty wife to learn about American grocery shopping. Her speaking ability was virtually nil when they arrived, although she could read English.

Satoshi at first treated me with typical Japanese politeness. I was *the* professor, and he was the humble little fellow who nodded and agreed with everything I said. It couldn't be quite like that. He knew more about his specialties than I did, and I was likely to learn more than he did from the association. I gradually succeeded in convincing him that the American custom of frank and open discussion was essential.

He had observed and photographed particles as seen from a diving bell, and some of them were much larger than any I had seen in collections. Out on the boat, he showed me that there were visible particles, as collected in a transparent Van Dorn bottle, that were broken up merely by draining them out through the spigot. Although fragile, they were also biochemically sticky, so that small particles could aggregate into larger ones. The food value of such material was open to question, but there were bacteria and phytoplankton cells stuck to some of it, so that material formed in this way could be at least a food supplement for zooplankton. Satoshi was changing my viewpoint from the traditional one that the organic gunk is being gradually degraded from larger to smaller particles and eventually to the dissolved state, a useless dead end as far as the rest of the ecosystem was concerned until the inorganic nutrients were liberated.

There could be reversals of possible ecological significance.

During a luncheon following a meeting of the Bermuda Trustees, Bill Sutcliffe told me that he and a mutual friend, Edward R. (Ted) Baylor, had done some experiments in which they bubbled air through seawater and found that organic particles were formed by absorption on bubbles. I went home and tried it and confirmed the result. There were similar flakes in samples of Long Island Sound water, suggesting that they were being produced naturally. I had seen them before but had not recognized them as being organic. Then one day, I was filtering a sample of phytoplankton culture for a dry weight determination. Ordinarily, I put through a small quantity of distilled water at the end to wash off the salt, but this time, I added too much, and before it got through the semi-clogged filter, a yellowish color of lysed cell sap began to show in the filtrate. This ruined the material for phytoplankton measurement, but just for the hell of it, I bubbled the filtrate, and there was an immediate snowstorm of flakes. The same kind of thing conceivably could happen in nature to extracelluar feeding, providing relatively fresh food. These various observations coalesced into a seasonal study of nonliving particulate matter in the Sound. It was beginning to look like an interesting dynamic system instead of simple, one-way degradation.

Meanwhile, Satoshi was studying light transmission in the lab, using an extra-long cell that I got for him to use in the Beckman DU. He also constructed an alpha-meter with an attached pressure gauge which he hooked up to an x-y recorder. Lowered through the water column, it produced a graph of absorption versus depth.

These graphs were always rather irregular, and I was aware of the fact that my sampling at discrete depth intervals had given me an over-simplified picture of vertical distribution. The extreme case occurred on a warm morning in late May or early June, a period when seasonal warming generally produced a negative temperature gradient from surface to bottom. We found a high absorption peak centered at a depth of 2.5 meters. I took samples there and at various other depths for later examination. This was a time of the year when there was normally a shift from a predominantly diatom flora to summer dinoflagellate populations. On this occasion, the diatom *Peridinium trochoideum* had come in, and 90% of the total population in the water column was between two and three meters.

If one looks at the old literature-- *The Oceans*, for example-- one gets an impression that this shift from diatoms to dinoflagellates was due to the fact that dinoflagellates could get along on less nutrients than diatoms. However, I had postulated that the changeover was largely due to increasing stability in the water column, which was unfavourable for maintenance of diatoms in the surface layer, while dinoflagellates could seek and maintain a depth that was optimal with respect to light and/or temperature. The present case tended to bear out this hypothesis; nevertheless, I was redfaced. For eight years, I had sampled phytoplankton at 5-meter depth intervals and no doubt had consistently under-rated this dinoflagellate population. You win some and lose some.

Toward the end of his stay, Satoshi had an automobile accident and was hospitalized but made a good recovery. We were sorry to see him go. They entered into our social life and treated us to some delightful Japanese meals. Their daughter had entered school and could speak English better than her mother. We saw a paper she had written and had signed her name-- Ukari. Until then, we had thought it was Ukali-- the old Japanese-English problem. A baby boy was born here. He would have an option to citizenship at age twenty-one. He must be about that age now.

I was continuing pursuit of the problem of nonliving organic matter, and the pursuit led me from the Sound into the open Atlantic. Pete was interested too. He had developed a method for measuring particulate organic carbon by dry combustion. He also hopped a cruise to the Mediterranean and back, collecting material for himself and bringing me a series of samples for microscopic examination. I pulled my assistant, Denise Van Hemert, off that dead end phytoplankton culturing job and put her on the new project. She didn't mind in the least getting a couple of free trips to Bermuda to do some work there. My family didn't mind, either, when I took them to Bermuda for parts of two summers. I was teaching part of a course there and developing some experimental work on particulates.

I was able to get ship time in January, 1963, for a cruise on the URI ship *Trident*. Charlie Fish was aboard and my former student, Ted Napora, working on a zooplankton project. This was my first cruise into the Sargasso Sea for many years, aside from day trips on the little Bermuda station ship. I had been looking forward to it, but it turned out to be an uncomfortable cruise. On the first station, I was out on the pulpit putting 30-liter Niskin bottles on the wire, and a technician was helping me wrestle with those heavy guys. The ship was rolling, and he lurched against me. I fetched up hard against a pulpit stanchion. Later, x-rays showed I had cracked two ribs. The rest of the cruise was painful, particularly on station and lugging five-gallon carboys of sea water around. The weather was bumpy most of the time, too, and the *Trident's* flat bottom slammed into swells with a great whack that sent a shiver aft through the ship. It sometimes felt as if the old girl was about to come apart. She didn't and I didn't. These various bits and pieces added up to two papers on open ocean particulate matter.

Charlie's health was beginning to deteriorate at that time. He had occasional spells of what he called vertigo; I don't know how a doctor would have diagnosed it. Anyway, one night on station, he collapsed on deck, and in a half-conscious state, he grabbed the towing cable and hung on hard. They stopped the winch barely in time to keep his hand from being pulled into the sheave.

After our return-- so I was told later-- John Knauss heard the story and urged Charlie not to go to sea anymore. Charlie's reply was that I hadn't done very well, either-- true enough, but hardly to the point. I don't know whether that was in fact his last cruise.

During this period, I had resigned from the presidency of the Bermuda Station and from NASCO. The latter had been the most onerous of my extracurricular activities, involving long weekend trips, but the opportunity to observe the operations of a lot of other oceanographic labs had been necessary in writing the report and was also useful to me personally. It served to crystallize my thinking about how an oceanography department should be organized and operated. I hoped to put that information to good use at Yale. As it turned out, I used it later and elsewhere. A few other requests came along for extra-curricular services, and I couldn't refuse all of them. I served terms on the NSF panel on environ- mental biology and also a review panel for use of the ship *Eastward*, which was operated and administered by the Duke Marine Lab at Beaufort but was assigned to them by NSF for use by scientists from various institutions in that region.

In the early Sixties, there was a change in the top Yale administration. President Griswold retired. The Provost, a considerably younger man named Kingman Brewster, moved up to the Presidency. We were, of course, still pressing for formation of a department. Eventually, he made one positive move. He invited a committee of Yale and outside people to come in and review the work of the Bingham Lab and advise him on the subject.

I never really heard what they reported, but nothing happened. There was a newly formed Department of Molecular Biology that was getting a healthy slice of the available money, but we didn't get any. We tended to be a little cynical about it. Brewster knew very little about science, and we thought maybe Brewster was reading Time Magazine to see which science was getting the most space and boosting that one.

He may not have been quite that crass. We were, after all, a rather small nucleus of active oceanographers to build a department around. Dan had done no wet-handed research for fifteen years. Aside from administering the Bingham Lab and Davenport College, his principal effort for some years had been to gather historical data for a biography of Sir Wyville Thomsen, leader of the Challenger Expedition. Bill Thompson had published nothing and had become an associate dean. Grace was certainly active but could hardly be called an oceanographer. That left only me and the junior members, Pete, Jim, and Sally (now Sally Richards). Although a good many marine labs had started with no more and had built up rapidly, a lot of money would be required for adequate staffing. We didn't get it.

Pete and I were getting restless. Rumors about our efforts to start a department and failure were flying around. I began to get job offers and tentative nibbles. In the summer of 1964, Brewster called me into his office. For the first time, I heard a definite decision. There would be no department. He hoped I would continue there anyway. I told him I had received other offers and was considering them. I would let him know my decision later. That was that.

I felt quite sure there was nowhere for the Bingham Lab to go except down. Pete was certain to leave as soon as he got a good offer. I would remain virtually a lone researcher except for technicians and perhaps a graduate student or two. I didn't think I could maintain a viable program that way. I had been a jack of all trades too long. The important projects of the future would require the team effort of a group of specialists. It was either that or an administrative job.

URI had made me an offer, which I was considering. They could provide me with most of what I wanted. I had looked at and rejected an offer of the directorship of the lab at the University of Alaska. They had their main lab on the university campus at Fairbanks and a seaside lab at Juneau, with only air travel between. I could see Lucy climbing the walls in a place where the winter temperatures dropped to -60 degrees Farenheit, and a winter day was two or three hours of sunrise-sunset. There were three other possibilities-- just nibbles so far-- and none very attractive.

I was of course very much concerned about uprooting the family again. The children were growing up. Louise was in college, Gracie in high school, and Milly in junior high. Lucy had returned to her earlier profession of public health nurse. She had never liked New Haven as well as Woods Hole, but we were well settled there. She was reluctant to leave, and any place else we went certainly would have to offer a pleasant environment and something interesting to occupy her time. A year after Henry's death, Helen and her son, Dan, had moved to Boston, and Lucy's mother and father had moved to Helen's apartment. Her father had died later and her mother, well up in her eighties and somewhat dependent, would have to be considered. She would need to go with us.

In the late summer or early autumn, Pete was invited to Dalhousie for a seminar and interviews for a possible job offer in the Institute of Oceanography. He came back with a report that they were also interested in me as a possible Director of the Institute. Ron Hayes, who had been the founding Director, had left Dalhousie for a post as Chairman of the Fisheries Research Board of Canada. At the moment, Ewart Blanchard, a geophysicist in the Physics Department, was serving in an interim capacity.

I went to Halifax in October for my seminar and interviews. I liked President Henry Hicks instantly. He was affable and witty and above all open and frank. He was a delight. I was used to guarded and political top-brass types. I never knew what they really were thinking or intended to do.

After a few minutes of informal discussion, we settled down to business. He summarized the structure of the Institute and its financial aspects. Although it was a small organization, he said he wanted it built up into a center of excellence and would do everything he could to support it. That could be a lot. Institutes reported directly to the President. Unlike Departments, I could bypass deans if they got sticky. He spoke convincingly, and I believed him. And I must say that in all my subsequent dealing with him, I never knew him to be less than frank and honest, even when the truth was painful. Probably that is why he had been unsuccessful earlier as a politician. it certainly accounts for the fact that he had both staunch friends and bitter enemies among the faculty.

Dean Trost of the Faculty of Graduate Studies was equally supportive, but he was a promoter type. I suspected I might have to fend off some of his wild ideas.

I had interviews with Dean Cooke of the Faculty of Arts and Sciences and with the staff members, gave my seminar and went back home. Soon I received a letter inviting me to apply for the job, that being the way they did it rather than a formal job offer. The salary would be about the same as I was getting at Yale. Henry said that ordinarily they didn't grant tenure on a first appointment, but for a senior post like mine, they might make an exception. He didn't know how little tenure meant to me. I had it only six years in an almost thirty-year career and was about to throw it away. I applied for the job, saying tenure was optional. I wouldn't want to stay if I didn't feel people were satisfied with the job I was doing.

There were some very positive aspects to the job that I thought warranted taking the job and some negative ones that I would want to correct. Foremost was the assurance of backing by the President. The Institute staff has sole responsibility for admission of students, formulation of the curriculum and recommendations for degrees. This was an odd arrangement for an organization that wasn't really a department, but it was an important one. By original agreement at the time the Institute was established, we would be permitted free ship time on federal ships, and we would receive a block grant from the National Research Council for general operating expenses.

The negative aspects were that professional appointments were in associated departments of basic science. Appointments, promotions, and grating of tenure were at the discretion of these departments, and the staff were also physically housed in these departments. Only the Director's salary was paid out of university funds. Other staff salaries came out of the block grant and their research funds, too, for people paid out of NRC grant funds were not eligible for NRC operating grants.

It was a very strange setup. I had never heard of anything quite like it before. The situation obviously had suited Ron. He was head of Biology as well as the Institute, and most of the students were in biological oceanography. He was pretty firmly in the saddle. I wouldn't be head of Biology and didn't have his long-term entrenchment in the university. I knew enough about university politics to be quite sure there would be troubles with one or more of the departments. The positive aspects were enough for a start, or I would need to change all those negative aspects-- full department status, a home of our own, a university budget that paid salaries, and of course, a more diversified staff. I fully expected to have a rough time of it for awhile, but this kind of administration presented a challenge. The kind of dull paper pushing that accompanied it would be small, at least in the beginning, for staff and student body were small.

All the degrees at that time had been M.Sc. I realized the M.Sc. was valued more highly than at Yale where, with rare exceptions, it was a consolation prize for a failed Ph.D. Canada, with few Ph.D. programs before the university population explosion of the Fifties and Sixties, imported Ph.D.'s from abroad or sent Canadians abroad for study, and under those circumstances, M.Sc.'s helped to fill the gaps in research organizations. M.Sc. programs were better than in most American universities, requiring a good many courses and competence in research, although not necessarily original research.

Anyway I think I'm correct in saying that Glen Geen was the first one to be awarded a Ph.D. through the Institute, and I attended his oral thesis defense in January, 1965. That was after I accepted the directorship, but the job didn't start until July. I had been invited to Halifax to attend some meeting. I think it was some sort of meeting of an advisory board, but I can't remember much about it. Usually, such meetings aren't memorable. I also went to Ottawa before returning home. While there, I saw Bill Ford for the first time in many years. He was then in the Defense Research Board but was leaving in the spring to assume directorship of BIO. Although Bill and I had not been really close friends, I found out rather comforting to know that he would be in that position, for we needed to have friendly relations with our big sister institute across the harbour. I also saw Bill Cameron, a physical oceanographer whom I had met before but didn't know very well. He was in the Department of Energy, Mines, and Resources and would be one of Bill's bosses in his new job.

The main purpose of the trip to Ottawa was to talk to NRC officials about Institute financing, and I got a mixture of good and bad news. Our block grant would be renewed for another three years, but they wanted to phase it out gradually, and they strongly advised me to increase the level of university salary support. I'd been playing the grant game long enough to regret the loss of even one buck, but as far as salary support was concerned, they were giving me leverage to apply to the University to do what I wanted to do anyway.

Then there was a discussion of a large capital grant that Carl Boyd had applied for about two years earlier. This was a request for a million dollars to construct a running seawater system. Hicks had also applied for a matching grant from the Treasury Board for additional lab and office space. This would go far toward providing the kind of building we wanted. They told me they had not given serious consideration to the proposal for the Aquatron (the catchy but etymologically atrocious title that had been bestowed on this facility) on the grounds that our little group was unlikely to be able to use it fully and effectively; however, the plans for expansion that I was promoting put a different light on the matter. They probably would activate the grant soon. "Soon" turned out to be several years later. By that time, I had learned that for the NRC several years was soon, but on the other hand, policy changes could be as sudden and unpredictable as Canadian weather. (The fact that Bill Schneider, the Director of NRC, was one of the old Woods Hole bang boys didn't increase my confidence; I shouldn't have been so prejudiced about that gang, but I was). They did terminate the block grant later, but by that time, there was something else with a slightly different name that served the same purpose. I didn't care what they called it so long as it was the same kind of money.

I came up to Halifax twice more before the move in July. Lucy came with me one of those times for house hunting purposes. That was not successful, but Kraft von Maltzahn, head of Biology, and his wife were helpful in continuing the search, and we were profoundly grateful when they found one that suited us very well. So, in July, we moved into a big, comfortable house off Vernon Street, a short walk from the University. (Imagine being able to walk to work! I hadn't done that since we left Woods Hole.)

As I change the scene from Yale to Dalhousie, this account changes a little, too. I no longer need to say much about contemporaries whom everyone knows, and most of this will be personal reminiscences and a brief historical summary, including some items that may not be common knowledge.

Although Lucy was not too eager to move again, she liked Halifax, and I did, too. The friendliness of the people and the leisurely pace were a welcome change from the bustle of a New England industrial city, and the environment was clean by comparison.

As soon as we got settled, she began to look for a job. She quickly found that she was "over-qualified" for an ordinary job as a public health nurse. They didn't want to pay as much as they should for a person with a M.N. degree and some years of experience. However, within a few months, she got a job of a slightly different sort at the Halifax Child Guidance Centre and continued in that until she reached retirement age. The Centre was then located on University Avenue approximately across from the Forrest Building where I had my office. I not only could walk to work. I could do it with my wife.

The Forrest Building was the first one on the campus when Dalhousie moved from its downtown location, and at that time, it housed the whole university. In 1965, the new medical school building had not yet been completed, and Biology shared the building with a noisy horde of medical students. The biological oceanography contingent had quarters in the basement best described as a rabbit warren. The medical students had lockers in the basement, and we saw little of them except at the end of the afternoon when they came charging down the stairs like a bunch of high school kids. There was one biologist who also had an office and a lab in the basement, but he left the university soon, and we quickly expanded into the vacant space. There was also a small snack bar. Otherwise our only neighbours in the basement were very quiet people-- the cadavers in the medical school morgue.

They remodeled a former basement games room as offices for Barbara Hendry, the secretary, and me, plus an adjoining long, narrow lab. Barbara had the larger front office. She needed more room for office files and supplies. I had a ten by ten cubby hole. The lab space was only barely adequate by the time I also installed two graduate students there.

Barbara had been around the university off and on since early post-war days and had been Ron Hayes' secretary. She knew everybody and knew all the procedures. My job of learning these things would have been infinitely more difficult without her help.

In later days, with the growth of the organization and increasing complexity of administrative tasks, there were a good many things that a good administrative assistant does that she did not do effectively. My successors were dissatisfied with her performance. I suppose I could be blamed for getting her promoted beyond the level of her competence. However, when I went there, secretarial salaries were right at the poverty line, and I had to get her reclassified upward. Eventually, she was replaced and went o the English Department where life was simpler.

The scientific staff consisted of Mike Keen, geophysicist; Carl Boyd, zooplankton; Dan Stanley and Don Swift, sedimentary geologists; and "Tony" Anthony (I can't even remember his real first name. He was not very impressive; a microbiologist and a bit older than the rest, he had got to the level of Associate Professor. He presented an ultimatum to Biology that he would leave unless he got a further promotion to full Professor. They didn't see it that way. He was there only one year after I arrived.) So with the addition of Pete and me, there were initially seven of us and about fifteen graduate students, mostly in biological oceanography. The major disciplines were covered, more or less, except physical oceanography. We got some help from BIO staff for lectures in physical oceanography. There were no graduate students specializing in that subject during the first year or so.

I had expected problems with other departments, and they started soon. Friedlander, a crusty old Swiss and Head of Geology, was the worst. He ran his department like a despot and wouldn't share an inch of his territory with anyone else. He had fought with Ron Hayes over the formation of the Institute, and now I had that albatross hanging around my neck. He blew his top at the merest mention of the possibility of creating a Department of Oceanography, which would presumably would have a geological contingent over whom he would have no control. The problem was exacerbated by the fact that Dan Stanley was something of a young Turk. He was a nuisance to me at times, too. I tried to defend him, but despite my best efforts, he was not reappointed to another term, and Don Swift chose to depart with him.

Friedlander was close to retirement age, and I had little choice except to ignore him and wait it out, making no more attempts toward appointments in geology until he vanished from the scene. Of course he was a thorn in everyone's side. Dean Cooke, also of the Geology Department, and Henry Hicks wanted him to quit as Head of Geology. He was ousted and took early retirement, and that took care of that.

During the next year or so, I had informal discussions with other department heads and a number of meetings with all of them, arguing the question of departmental status for oceanography. Walter Chute, in Chemistry, was a mild mannered and pleasant man who offered no opposition at all. Ernie Guptill, in Physics, clearly was not supporting me, but I didn't quite know why. There were no problems with him about staff, provided our appointments didn't interfere with getting people he wanted. I think he was just being conservative. Possibly he thought, as a good many people did at that time, that oceanography was a conglomeration rather than a real discipline and didn't deserve departmental status.

Kraft perhaps had more to lose than anyone, or thought he did, if we removed his staff, but there was a lot of deadwood, too, and our people added strength to Biology. As long as we were physically located together, there was good inter-communication among both staff and students. That inevitably would diminish if we had separate quarters. He presented more opposition than anyone else except Friedlander.

On one occasion when Ron Hayes came back to Dalhousie for a visit, I discussed these things with him and was surprised to find that he didn't favour departmental status either. He apparently was quite content to see things continue as they had been when he was in charge, primarily as a biological oceanographic institute, and felt that maintenance of a close alliance with basic biology was desirable. I was annoyed afterward to find that he had also expressed that opinion to Hicks. My counter-argument with Hicks was that there were more openings for physical and chemical oceanographers than for biologists. Our original mandate to produce oceanographers for the federal service required us to build up those disciplines. We couldn't be just a biological oceanographic institute.

I won that round, but when Ron came back to Dal after retirement from the job in Ottawa, we were getting closer to departmental status, and I was really concerned that he might throw a monkey wrench into the works. He went back into Biology, and I was probably being hyper-sensitive and not very courteous in not inviting him to be an associate of the Institute as well. When the showdown came of votes on departmental status in Faculty and Senate, he voted for us.

All these arguments dragged on interminably, and the one thing that did more to resolve them than anything else was the development of a building program. Of course that didn't happen immediately, either. NRC and the Treasury Board were passing the buck back and forth. Treasury Board would give us a matching grant if NRC okayed theirs and vice versa. Somehow, however, it finally happened, I suspect as a result of Henry twisting the arms of some of his Liberal buddies in the Federal Cabinet. It happened at a time when the economy was fairly prosperous, university populations were growing, and everything was conducive toward major building programs. A year or two later, we would never have gotten it. Henry was able to use this federal grant to raise additional money from the Province. The idea was that a university could go to the Province with a small pot of money that it had accumulated one way or another, and using it as evidence of solvency and good faith could then get a considerably larger pot on what was known as a long term, no-interest loan. As far as I can make out, it was, in fact, an outright gift. I've never heard of any university paying back any of the money. So Henry went scrounging and came back with a handsome pot. The total building fund was nearly eighteen million dollars.

That was about half of his long-term goal of developing big, new facilities for all of the sciences. The building fund that he had in hand would produce what he chose to call the Life Sciences Centre-- oceanography, biology, psychology-- and eventually there would be a Physical Sciences Centre and probably a Science Library. He appointed me as chairman of a building committee for the Life Science Centre, the other members being the two department heads and Jim Sykes, the university architect. Another committee was appointed to work on preliminary plans for the Physical Sciences Centre, but the financial climate worsened, and it never got off the ground. The ground that it didn't get off of became the parking lot between the Dunn Building and Howe Hall.

Our own hopes for oceanography fitted neatly into his larger scheme. Our money had started the ball rolling. We could get our Aquatron and some associated lab space out of that and could hardly be denied an additional allotment for common facilities for our whole group. Biology couldn't object very strongly to that in view of the fact that they were getting a much needed new facility without having to struggle for it.

In the meantime, I was gradually getting more staff members on the university budget, about one a year, and Henry was saying he didn't think there would be much difficulty getting us departmental status when we moved into the new building. Just be patient. Dal moves with glacial slowness. I reminded him that I had sat for quite a while on a Yale glacier that didn't move at all.

There was only one promotional scheme that I presented during those years that completely fell on its face. It could have involved a university-wide battle, and he didn't choose to fight it. I thought then and still think that he should have done so.

The Faculty of Graduate Studies was a relatively new addition to the family of faculties and was definitely a poor relation. It was little more than a clearing house for graduate student affairs.

This was not a very satisfactory situation for the increasing number of departments that had both undergraduate and graduate student, and oceanography was in a particularly anomalous position. Student affairs went through Graduate Studies. Faculty affairs were handled by the undergraduate dean despite the fact that our teaching was almost entirely at the graduate level. In budgetary matters, the Institute had special status in that the President handled that, but that would change when we became a department and our situation could worsen.

My pitch to Henry of course was to chop off all those anachronistic historical precedents-- to establish a senior officer as most other universities did-- Provost or Academic Vice President or whatever one wanted to call him-- who handled all faculty affairs and departmental budgets, and to reduce Arts and Science to the same status as Graduate Studies. I think he agreed with me that this was sensible for all departments as well as oceanography, but then he asked what should be done about the deans in the professional schools. I told him I thought that ideally they should be pulled into the same system, although that wasn't quite so important. He said he would think about it, and the conversation ended there.

I broached it another time or two, but nothing happened. Of course, any moves along these lines could have set off a disruptive power struggle among the deans. The deans of professional schools, and Dean Stewart of the Medical School in particular, were powerful men in the university who would fight bitterly against any proposal that would reduce their degree of autonomy, and I suppose any Dean of Arts and Sciences would be opposed to giving up his power, especially if it did not apply equally to all the deans. Damn university politics. I never knew whether I really failed to convince the President, or if he simply lacked the courage on that one occasion to meddle with the status quo. Anyway, the system stayed as it was, except that it became even more ridiculous later when they created a post of Academic Vice President without giving him these important duties that he was supposed to have. Arts and Sciences remained firmly in the saddle.

Our relations with BIO continued to be good. There were increasing demands for ship time as their staff grew, but I think we got a fair share. In those days, there was a ship committee that planned future operating schedules, and we had a representative on the committee. As one of their people remarked, everyone went into the meeting with knives of equal length. They were helpful in teaching and thesis supervision, particularly in specialties that we lacked initially such as physical oceanography. A few of their junior staff members took degrees with us on a part-time basis. There were a few problems involved in such relations, but they were ironed out without serious difficulty.

When Anthony left the university, we hired Walton Watt to take his place. Walton had been a student at Dal earlier and had a reputation of being something of an *enfant terrible*. He had gone to England for his Ph.D. and returned as a postdoc. He seemed less obstreperous than was indicated by some of the early stories, and he was certainly bright and capable. However, all sorts of problems developed, which I won't go into here. It reached the point of a mass meeting of students protesting his reappointment, and the morale of the Institute would have suffered if we had kept him. His replacement was Bob Fournier, who had been with us as a postdoc and then had been at the University of Hawaii for two years before we were able to invite him back.

We also decided we had to have a benthic specialist and were able to lure Eric Mills away from Queens. Another one of Evelyn's top students to come to Dal was Ian MacLaren. Although I had urged his appointment, Biology seemed a better place for him than Oceanography. We had a zooplankton man already, and Ian's interests were broader than that specialty, extending into things non- oceanographic.

Our most crucial need was for a good physical oceanographer. They were not easy to find nor to lure to a small lab where, at least for the time being, there would be no associates in their specialty. The man we finally succeeded in getting was Roy Overstreet, although he came in without having quite completed his Ph.D. at the University of Washington. In the interim, he had to be appointed as a lecturer. Roy proved to be an excellent teacher, but the thesis didn't get finished. There was some kind of emotional block. I understood it no more than I had understood Bill Thompson's similar behaviour earlier. Finally, denied promotion, he went back to the States and took a similar job in the AEC at approximately twice the salary we had been able to pay him.

After more futile attempts, Chris Garrett finally came along. He had been working with Walter Munk, who gave him a high recommendation but warned me he might try to get Chris back in a couple of years. I don't know how hard he tried, but Chris stayed. An extra dividend for me was that Chris and Walter were still collaborating, and Walter visited us a couple of times.

We also developed a slot for a second chemical oceanographer. We invited several applicants to visit us, and none was very satisfactory. We ended by hiring Bob Cooke, one of Pete's students, although in general, we wanted to avoid becoming ingrown. Mike Keen left us, moving to the Chairmanship of Geology, and Roy Hyndman replaced him in the geophysics slot.

That was the way it was when we moved into the new building in 1971 and became a department, except that Roy was still with us then, and Chris came in later. Our student body had nearly tripled during those six years, and although biological oceanographic students were still a majority, other specialties were also represented. We had at least a nucleus of a viable oceanographic establishment.

Prior to that, we had gone through a planning stage for the building that lasted about three years, consisting of internal planning by each departmental group, and their wishes were then transmitted to the overall building committee. Each group presented its projected space requirements, which then had to be chopped down to fit within the limits of the budget, and not without some rancour. The oceanography request was the most modest one; its total budget was about four million. The others were obviously inflated. I gave no ground and got what we asked for. The others were chopped down.

Concomitant with this, there were of course discussions within each department of what we should expect in terms of future growth and how internal spaces should be arranged. How big we should be, assuming of course the unassumable that we could become as big as we wanted to be? Already there were indications that the future financial climate might not be as good as it had been in the past. My point of view, based on visits to labs during the NASCO period, was that some institutions were too big for effective inter-disciplinary communication of the sort that I felt oceanography needed. Woods Hole and Scripps, for example, were split vertically into individual disciplines which didn't know what each other were doing. Other labs were too small to be viable. There must be some intermediate optimum number, although I didn't know exactly what that number was.

Most of the staff more or less agreed with this point of view. We were thinking in terms of about 20-25 staff members, perhaps 75 graduate students, and some postdocs and technicians, and we planned accordingly. We wanted representatives in all specialties, but didn't need to have someone in every small specialty, what with BIO across the harbour to give us a helping hand.

We intended to have wet-handed people-- chemists and biologists-- mainly on the second and third floors with some overflow space in the Aquatron area, and lab facilities were planned accordingly. The drier contingent was to be on the fourth and fifth.

Few of these plans worked out fully. Initially, there was more space than we really needed, and we were saddled with unwanted tenants, the Mathematics Department and an overflow from Geology. When Mathematics moved out, the Trace Metal Analysis group moved in. We were innocent victims of the failure of plans for a Physical Sciences Centre plus bad timing in the sense that we hadn't been able to come even close to our projected future size.

Our internal planning also went somewhat awry. I was disappointed that Eric insisted on going to the fourth floor and taking the graduate students with him, thus dividing the biological group. Also some of the space originally intended for graduate students and postdocs was pre-empted as rooms for special equipment, which we had originally expected to be in the large common labs, thus making the latter less useful than they might have been. I didn't like all this, but I disliked even more the dictatorial attitudes of some department heads I had known. I didn't want that reputation.

There were of course a lot of comments about the general construction, pro and con. The materials and external planning were the architect's choice, and there was some validity in the feeling some people had that the result was gray and dull. I can only say that it was one of the most economical kinds of construction. Anything fancier would have given us less total space.

Thus far, I have been writing mainly about the history of the Institute and the political games that we played in becoming a Department. There were of course a lot of other things doing -- teaching formal courses or parts of courses much of the time, supervising graduate students, trying to keep a little research going, and getting involved in a few extra-curricular activities. Much of the time, I felt like a performer in a three-ring circus.

I had two graduate students almost immediately upon arrival. One was Janet Eaton, a Ph.D. candidate in Biology and wife of Peter, an oceanography student. Janet didn't have any particular problem in mind. Bill Sutcliffe and Ted Baylor had recently published a paper indicating that *Artemia* larvae could be reared on particles formed by bubbling, and I was interested in seeing further work along these lines with marine animals. My first suggestion, that she try small mussels, was poor advice. I didn't realize how much water even small mussels can filter. We couldn't produce particles fast enough for a good test. After wasting a little time on that Janet switched to copepods and did a comparative study of feeding on various algae and on particulate matter. The latter proved not to be an adequate food supply by itself. She finished, produced a family, did various part-time things that weren't science, and now I hear that she is getting back into biology again, teaching at Mount St. Vincent.

The other graduate student was Don Gordon. He had been a student in one of the courses that I had taught at Bermuda, and I was highly impressed by his intelligence and vigor in getting things done. He applied to Yale to work with me there, and I was disappointed when he wasn't admitted. Instead, he went to URI for a master's degree, and then, having heard I had moved to Dalhousie, he applied there for Ph.D. candidacy. He was admitted instantly.

Don went to work on the distribution of nonliving particulate matter in the western Atlantic, doing a lot of cruising and extending earlier observations. His data included particulate carbon analyses and microscopic examination, including the use of histochemical stains for rough characterization of the biochemical nature of the material.

I had little time for cruising, although I occasionally got out on a short trip. The first was on the *Sackville*, an old Navy ship that had been taken over by the Defence Research Board and refitted for scientific work. People asked, "Are you going out on that little thing in midwinter?" It was a 1200-ton ship, actually the largest oceanographic ship I had been on up to that time, not counting the *Bowditch*, which we weren't able to use for oceanographic cruising.

The *Sackville* of course was small compared with the *Hudson*, the queen of the BIO fleet. Actually, I had only one cruise on her, and I felt that she was too large and expensive for the kind of work we were doing. With the large number of scientists that went on that cruise, wire time was a limiting factor, and none of us was working efficiently. Of course, a ship of that size was really needed for high-latitude work, such as a winter cruise that she went on during that period for current meter work to measure the outflow of Arctic water over the sill in Denmark Strait. On a previous trip to that area, the *Atlantis II*, the biggest ship in the Woods Hole fleet, ran into serious difficulties. She was taking green water up to the bridge. Val Worthington, in a cabin immediately below, narrowly missed death when a glass port imploded and crashed across the room and into his bunk.

Most of my research at that time was land-based rather than seagoing. I was studying relations of bacteria with particulate matter. I got carboys of water from the nearby Northwest Arm of Halifax Harbour. The work required only initial processing and occasional sampling thereafter. It could be fitted into odd moments amid other duties. The results were included in a monograph on particulate matter that was published later in the *Advances in Marine Biology* series.

Extra-curricular activities were various, major items being membership on an NRC advisory panel on grant applications in biology for three years beginning in 1966, a few NRC site visits, and membership in the Canadian Committee on Oceanography. The latter had much the same sort of goals as NASCO, involving policy making and stimulation of the development of oceanography in Canada, but it operated in a somewhat different way. Originally, it was a powerful committee, composed largely of Deputy Ministers and other senior officials in agencies dealing with marine matters. It not only advised Cabinet but had considerable ability to implement its policies. CCO had established our Institute and its sister Institute at U.B.C. to fulfil the need for producing professional oceanographers in Canada, with guarantees of financial support from NRC and seagoing facilities on federal ships, and the directors of the institutes were given membership on the committee to report on progress. However, at the time I joined, there were fewer deputy ministers on the committee and the underlings who represented them were less able to make policy and put it into effect. Attendance was a necessary duty, but the rewards were not great.

The other academics at these CCO meetings were Dunbar of the Marine Sciences Centre at McGill, Meisner of the Toronto Great Lakes Institute, and Pickard of the Institute of Oceanography at U.B.C.

I didn't get to know Meisner very well and have little to say about him. I had known Max Dunbar for a long time. He was a graduate student at Yale at the same time I was. My memory about that is dim, but I think he didn't stay there very long and got his degree somewhere else.

The next time I saw him was many years later, when he was chairman of a NASCO subpanel. In the meantime, he had spent a lot of time in the Canadian Arctic, working on a tiny ship, the *Calanus*, and he probably knew more about the biological oceanography of the Arctic Ocean than anyone else. He had also founded the Marine Sciences Centre at McGill, a most unlikely spot from a geographical standpoint, and the fact that he made even a moderate success there speaks well for his enterprise and leadership qualities. A centre indeed it was, with spokes radiating toward the Arctic, the Gulf of St. Lawrence, and a little field station that he established at Barbados.

And so, again for a third time, I began to see Max at CCO meetings. His Centre initially had been almost exclusively biological, and he recognized the need, as I did, for inter-disciplinary broadening and was successful to some degree in making appointments in other specialties. I think the attitude of government officials was that it was not a proper place to promote a major oceanographic establishment, and yet a man of Max's ability had to be supported to some degree.

In contrast with Max, George Pickard was, in my not so humble opinion, a dull clod. I don't know exactly when he went to U.B.C. (from England) or very much about the early history, except that he was doing competent but rather dull work with Bill Cameron and Jack Tully on British Columbia fjords. The latter two went to Ottawa and became hard working, hard drinking civil servants, Bill in Energy, Mines, and Resources, Jack in Fisheries.

George set up the Institute of Oceanography at U.B.C. in an even more awkward way than the Institute at Dal. Students were members of departments of basic science and got their degrees from those departments. This was all right for the physical people. George was well entrenched in the Department of Physics, and he had a free hand in developing the kind of program that he wanted. As I saw the place during a visit in the late Sixties, there was a good physical oceanographic program going. The major emphasis was on air-sea interactions, but there was a good training program in the field as a whole, and they were turning out people who filled useful niches in the federal service. However, biological oceanography, was a poor relation, and chemical oceanography was a neglected orphan. Geology and geophysics were a separate department and operated quite independently of the Institute. Of course, I talked with George about our goals at Dal for departmental status and a broader program. His response was polite but non committal. He obviously was content to have a lopsided oceanographic program and some decidedly unhappy people in other specialties, and he didn't intend to change anything. He wasn't doing much research, either. The intellectual leader of the group was Bob Stewart, who was certainly one of the best.

During that trip to the west coast, I also visited the Fisheries station at Nanaimo. I saw something of the fisheries work but spent most of the time on that one-day visit with Tim Parsons and Ray Sheldon. There I met Ray for the first time. They were doing some work on nonliving particulate organic matter, which of course was one of my principal interests then, and the discussions were useful and informative. However, I had a distinct feeling that when John Strickland left Nanaimo, the innovative spark went with him.

I have never been quite as impressed by Tim as some people are. He certainly has a broad knowledge of biological oceanography and no doubt was an excellent addition to the U.B.C. faculty when he left Nanaimo. However, I heard him give a seminar on the plankton at oceanic weather station "P" which was quite pedestrian, and in my opinion the CEPEX project was ill-planned and poorly executed. Perhaps I am being hypercritical, and admittedly I haven't had sufficiently close association with him to have a well-informed opinion. Nevertheless, his reputation was made principally during his association with John Strickland, and I don't think he has made any major advances in the field since then.

As Lucy and I settled into the Nova Scotia scene, we were impressed that there was so much unsettled and relatively wild country so close to the city. New England was not like that. In summer, our neighbourhood was practically deserted on weekends as people went off to summer cottages. Although the city seemed suburban to us, we began to think we would like to have a summer cottage, too, and in the spring of 1967, we found one that we liked, a small cottage on Cox Lake in Hammonds Plains, about half an hour's drive from home. We were able to buy it and an adjacent plot of over a hundred acres of woodland for a ridiculously low price that today wouldn't even buy one moderate sized lakeside lot.

We were on the edge of an area of some fifty square miles lying between the Hammonds Plains and Timberlea Roads in which the only access was logging roads and trails. This was my first introduction to northern softwood forests and I loved them. I was beginning to put down roots in a way that I had not in any place since leaving my boyhood home in the Ozark hills. During the week I was up to my ears in work and taking a briefcase home every night with unfinished tasks in it. Weekends in the country in summer, or perhaps one day in winter, weather permitting, became very important to me. In a canoe on the lake or tramping in the woods, I found a sense of peace that often was otherwise lacking.

We often had student visitors. A recent arrival in the student body was Gareth Harding, who came from McGill and brought with him a passion for cross country skiing, which quickly spread to other students and junior staff members. They cut trails between a series of lakes extending through the area between Cox Lake and Fraser Lake, where Carl Boyd lived. We happened to have several unusually snowy winters, with three or four feet of snow in the woods, so there was good skiing in winter, and they also had canoe parties in summer. Lucy and I didn't feel energetic enough for long excursions, but we enjoyed their visits at the beginning or end of the trail, as the case might be.

Although relations have been generally friendly in Oceanography, I think there was a rapport in the Forrest Building group that has never quite been equalled since. The relatively small size of the group had something to do with it, but Don's and Gareth's leadership in organizing parties and outings was very important.

Don finished handily in three years. He married Joleen Aldous, an undergraduate honours student and daughter of the head of Pharmacology, and they went off to two years at the

University of Hawaii. Then he came back to a job at the Marine Ecology Lab at BIO, where he has remained until now. I remember an occasion soon after he returned when he and Gareth and their womenfolk came out to our place for a long Sunday afternoon tramp in the woods. Every little while, Don would inhale deeply and say, "Ah, these woods smell good." Of course there are good flower smells in Hawaii, but Don, too, had put down roots.

People seem either to love Nova Scotia or hate it, without much in between. Our daughter, Gracie, married soon after graduation from Acadia and went to Toronto, but a few years later, they came back. Milly, too, returned after going to McGill and working in Ontario. Of course, we are delighted to have two daughters nearby.

But then we were able to get Ed Deevey and Georgiana here, he on a Killam Professorship, and they didn't like it. They left again quite soon. In the meantime, Georgiana was helpful to Gareth, who was doing a thesis on deep-sea copepods.

There were a good many stresses and strains during that period, but one thing that disturbed me more than most was the loss of my friendship with Dan Merriman. When I left Yale, we parted on ostensibly friendly terms. He even gave me a nice parting gift-- a print of a picture of the *Challenger* during her stay in Hamilton Harbour, Bermuda. It hangs on my office wall, along with a much prized half model of the old *Atlantis* that Don Gordon made and gave me at the time of his graduation.

After I left, things fell apart for Dan. The Bingham Lab ceased to exist. The museum people took over the building. Dan was also removed from the Master's post at Davenport College, although he remained a Fellow. He was reduced to a mere teaching job in Biology. So was Grace Pickford. She left and went to Antioch College fairly soon; Dan remained.

Dan became a consultant to a power company which had built a nuclear reactor on one of the rivers, the Connecticut River, I think. They wanted to have him do a survey of the fishes to get data on possible effects of the warm effluent on the populations. Sally had a job for awhile helping him with that.

Sometime later, he asked me to come to New Haven for a conference that he was organizing for discussion with some of the power company people about building a reactor in the shore of Long Island Sound. For old time's sake I went, although I wasn't eager to get involved in that sort of thing. In the conference, there was a discussion about preliminary environmental surveys that they had done, and they asked me a lot of questions about what I thought about various aspects of the job. When it became apparent that I thought there was likely to be little damage except very locally around the outfall, they asked me serve as expert witness in a public hearing.

That I couldn't do. I knew those things were conducted as an adversary situation. There were few questions that could be answered unequivocally. I liked to qualify my answers in the usual way of scientists. Also, I knew that witnesses were likely to be called at the convenience of the presiding judge, and I could ill afford to spend a lot of time on that sort of thing. I gave those reasons. I didn't add that I was really on the side of the environmentalists, even though I thought they were opposing the reactor for the wrong reasons. They may have suspected I felt that way.

The conference broke up. I walked back with Dan to his office. He said, "I hope you'll excuse me. I have some letters to write. If you don't want to stay in New Haven overnight, you can get a ride back to Boston with one of the conference people." It was a cool and polite insult, the way a proper Bostonian dismisses someone who is no longer a friend. It hurt. I haven't seen him since, and I don't know what all was in his mind. Perhaps he blamed me for the demise of the Bingham Lab. In my opinion, it would have happened whether I stayed or not, but his reaction made me think there was more to it than merely my refusal to testify.

I suppose everyone loses a few friends through unfortunate circumstances or misunderstanding, and I'm no exception. However, only once have I deliberately fostered a feud without shame or regret. I'm referring to my confrontation with John Ryther. Few people know the whole story, and I think reminiscences should tell things the way they were. I wouldn't like to be remembered as the sort of person who picks fights without some reason, but the reader should have an opportunity to judge whether the reasons were sufficient.

When John started publishing experimental work in the early Fifties, I regarded him as a very promising young man. I talked with him on various occasions, particularly at ASLO meetings, and we became pretty good friends. The first time I made any critical comments about his work was after he published his 1957 paper with Charlie Yentsch, which presented formulas and graphics for computing photosynthesis in terms of light and chlorophyll concentration. It said nothing about nutrient limitation. I asked him about that. He said he didn't need it. When the nutrient supply was limited, the chlorophyll concentration adjusted downward automatically and the equation took care of everything.

I argued that a change of this sort would occur only if nutrient limitation was reducing the rate of production. That was the way it worked in mathematical models, and I thought it was equally true in nature. When I applied his formula to some of my old Georges Bank data, it worked fairly well during the period when nutrients were limiting, but during the spring flowering the ratio of photosynthesis to chlorophyll was approximately doubled. I pointed out that the constant in his equation was an average of several sets of data with considerable variation. There could be a nutrient factor buried there. I was trying to be constructive and was urging him to rework the equation a little by inclusion of a nutrient factor, which could be done easily. It would make his formulation more acceptable in practical terms. I didn't convince him. I succeeded only in making him obviously angry. He didn't change anything. He subsequently used the same method in the survey of the New York Bight and in some of the IGY cruises.

I said nothing more, and we continued to discuss various problems when we happened to get together. Then in a later conversation, there was another thing that bothered me more. I had mentioned some odd and unexpected results that someone had been getting with carbon-14 experiments. I don't recall exactly what it was, but I was shocked when John said quite casually that he had some of those, too, but had ignored them. They didn't fit with the rest of his data. I thought everyone knew that was a no-no. I was beginning to wonder what kind of guy this was--stubborn certainly, and maybe a little dishonest.

John was a productive worker. He had achieved a position of some distinction. He took charge of the biological program in the International Indian Ocean Expedition. In 1962, when the Woods Hole Oceanographic Institution was reorganized on a departmental basis, John became the first chairman of the Biology Department (Buck had become Associate Director when Alfred Redfield retired and was no longer directly in charge of biological matters.) Ryther was a person people would listen to. If he was publishing things that were not completely honest and objective, he could mess up the field. I decided the course for me was to contest anything he said in the literature that I didn't think was correct.

My first little blast was in a chapter on plankton models that I wrote for Volume 2 of the *Seas, Ideas, and Observations*, in which I criticized the 1957 paper along the lines mentioned earlier. I followed that with the mathematical model of regional variations mentioned earlier that I submitted for the Redfield Festschrift. The inadequacy of the 1957 model was mentioned again, and I proceeded to develop a set of production coefficients which were arbitrary but consistent with observed conditions. An increase of phosphate from zero to a non-limiting concentration doubled the ratio of photosynthesis to chlorophyll but increased the ratio of photosynthesis to phytoplankton carbon by a factor of about ten. This was in accordance with a carbon: chlorophyll ratio of 30:1 when nutrients were unlimited and increasing to more than 100:1 with extreme nutrient limitation, a situation also supported by observational data. It went on from there to a simple two- layered model similar to John Steele's and was a steady state regional analysis in early summer when irradiation was sensibly constant over a wide latitudinal range and in which the principal environmental variables were vertical mixing and the phosphate concentration in the lower layer. The method of calculation was simple enough so that I could make the transparency of the water and depth of the euphotic zone dependent upon the chlorophyll concentration. I had, of course, wanted to do that in earlier models, but they had been too complicated for that to be feasible.

I guess the model was somewhat convincing or honest John Strickland wouldn't have given it his stamp of approval. There were plenty of other times when he had no hesitation about telling me I was nuts.

As for John Ryther, we weren't talking. In addition to scientific controversy, a bitter personal quarrel had arisen between Ryther and Bill Sutcliffe. I won't go into that, but Ted Baylor and I had been supportive of Bill, and there was rancor all around.

The scientific controversy shifted to problems of nonliving particulate matter. In 1966, Dave Menzel published a paper claiming that particulate organic matter could not be produced by bubbling air through seawater. Dave was repudiating experiments that he had participated in earlier as a co-author with Bill and Ted. He said these particles were an artifact due to organic contamination in the Woods Hole air supply, and when special efforts were made to clean the air, there was no difference between filtered and bubbled seawater and unbubbled controls.

I had used a variety of methods for producing particles, some of which I felt were certainly free of that kind of criticism. My current experiments, which included studies of bacterial utilization of artifically produced particles, would be vitiated if there was an artifact. The situation required a re-examination of methods, with enough replication for statistical analysis. I pulled together a small team which included Eddie Batoosingh, a microbiological post doc who had taken his degree under Anthony, and a technician named Barbara Keshwar. The three of us went to work on various aspects of the problem. The results did not support Menzel's conclusions.

Bill Sutcliffe had gone to Woods Hole when he resigned from the directorship of the Bermuda lab and Ted Baylor was there, too. However, the quarrel with Ryther made their situation uncomfortable. Bill resigned and came to the Bedford Institute; Ted went to Stony Brook. Others were getting restive under Ryther's dictatorial administration. Bob Conover came to BIO, too. Charlie Yentsch at one time had been a loyal henchman, but he left, and a letter he sent me was scathing in its denunciation of Ryther, saying that the work of all the people in the plankton division was being directed to the point where they were little more than technicians. Not long after that, he established a good little oceanographic lab at West Boothbay Harbour, Maine. Menzel lasted several years longer, but he broke away finally. Howie Sanders and his benthic group kept their mouths shut and managed to stay clear of the mess (that's the way he described it to me later).

In the meantime, Ryther and Menzel were doing a lot of ocean-going analyses of particulate organic carbon and were consistently publishing values for the deep water that were much lower than ours. Don Gordon also got lower values than the ones Pete and I had previously published, and we suspected that his silver filters were not catching as much of it as the glass fiber that we had used earlier. A later study by Pete confirmed that. However, the Ryther-Menzel numbers were still lower. That puzzled us for awhile, but then a friend of ours who had been out on a cruise with Menzel reported that he was using a somewhat different method. They had concluded that there was a significant adsorption of dissolved matter on the filter. Therefore, they put the water through two filters and then, assuming that adsorption was the same on both, they subtracted the second from the first and called that particulate matter.

I didn't know whether adsorption was significant. This was not easy to determine because another factor was involved. Small particles which escaped the filter could be caught by subsequent filtrations. This was evident from some of my experiments. For example, we were never able to sterilize a water sample by putting it through a 0.45 micrometer filter but sometimes could get the sample bacteria-free with three successive filtrations. I didn't know whether it was better to add or to subtract or just forget the whole thing. After all, the definition of particulate matter is an operational one.

However, they did not define the operation. None of these papers described their methods in sufficient detail for me to know exactly what they were doing. In my opinion, a paper is worse than useless if it does not describe the methods in sufficient detail for other investigators to compare results, and this is doubly true when the results are controversial. This was dishonesty of another sort.

Lack of information on methods left me with nothing to say on the subject, but there were other things that I could and did criticize. Basically an experimentalist, Ryther had acquired surprisingly little knowledge of the facts and principles of biological oceanography. His implication in that early model that nutrients are not limiting was a first example. In the later work on the distribution of particulate organic matter, he concluded that it, and other biological properties as well, were essentially conservative below about 200 m. He even went so far as to postulate that the oxygen minimum layer was formed at shallow depths in tropical upwelling areas and spread from there to deeper levels in the rest of the ocean. In my 1970 review of particulate matter, I pounced on several of his ridiculous statements, including that one, pointing out that the oxygen minimum layer is centered on several sigma-t surfaces in different parts of the Atlantic and could not possibly originate by isentropic spreading from a common center.

Eventually, Ryther got out of the deep ocean and back into experimental work. I don't know whether he was influenced by my attacks, and that isn't important. I just wanted him to stop messing up my ocean. Although the feud is now ancient history, I still cannot feel kindly toward a man who has violated so many of the ethical principles that our profession lives by.

So much for that.

When we moved into our new building, I had accomplished the major goals of getting a home of our own and creating a Department, although staffing problems remained, and finances were becoming more difficult. There was an increasing burden of administrative details of the sort that I didn't like. A lot of them had to do with getting settled into the new building, but there was also increasing administrative bureaucracy at higher levels in the university, which always creates more paperwork for all.

I wrote several papers, mostly potboilers associated with symposium presentations, but there was little time for new research. I went on one cruise in 1973, a fiasco that Bob Fournier mentioned in his essay in the book of collected reprints. We got almost nothing done during that long period of miserable weather. Lloyd Dickie tried to shoot his midwater trawl, and the line parted. We tried a station midway back from Bermuda when the weather seemed to be easing a little. Bob took a beating on the pulpit. We just might not have Bob with us now if there hadn't been a life line on him. He told about my injury and doctor's orders not to go to sea again. That wasn't strictly so. The injury was painful but not that serious. The fact was that I no longer had the endurance to do a fair share of the work for long hours under difficult conditions. I was getting tired and accident prone, and it was my decision as much as the doctor's to close that chapter of my life.

Later that year, I ended another chapter. I suppose the main reason I stayed on as head of the Department was that I thought I could maintain a holding operation until retirement and not put that burden on younger and more active researchers. I had remarked on previous occasions that I wouldn't continue if other people weren't satisfied with the job I was doing, and although there were some criticisms in staff meetings, I wasn't aware that they were reaching a critical point.

However, one day Pete paused at my door and said, "I told you I'd let you know if the time comes to get out. This is it." I wrote a letter of resignation to the President immediately and spared the staff and myself the mutual embarrassment of arguing the matter. My pride was hurt a little, but there was an admixture of relief to be getting out of the rat race. I fully realized for the first time that as an administrator, I was burned out.

For the first time in years, I had an opportunity to read extensively and catch up on literature. I was woefully far behind. There were, to be sure, some other duties. I was teaching a course in biological oceanography for Biology students and was doing some graduate student supervision. There were also a few extra-curricular activities. The Brookhaven National Laboratory on Long Island was trying to develop an oceanographic capability, which had the immediate practical mission of evaluating the possible environ- mental effects of establishing nuclear reactors in offshore coastal waters. They had assembled a team of physical oceanographers headed by Gabe Csanady of Woods Hole to study currents and offshore diffusion effects, and they wanted a coordinated team of biologists to examine possible ecological effects. I was asked to serve on an advisory committee in setting up the program.

Brookhaven straddled the fence between applied and basic research. In addition to fulfilling practical requirements, they wanted to combine physical and biological research into an interdisciplinary undertaking that would improve our general knowledge of coastal oceanography. I did some preliminary spade work for them. The main question in my mind was the kind of physical movements that would be required to maintain the productivity of the coastal waters south of Long Island. I reviewed earlier work by Buck and his associates in a survey of that area and calculated exchange rates that would be required to maintain a nutrient supply essential for observed levels of productivity.

In due time, Brookhaven recruited a team of biological oceanographers headed by John Walsh, a bright and creative man, and I gradually withdrew from the program. My services were no longer needed. The program that he had developed was not quite what I had suggested, although he has done a lot of good things and I think the work I did there provided a little help for Bob Fournier in shaping up similar studies on the Scotian shelf.

I also had a three-year hitch on an NSF panel evaluating projects for the International Decade of Ocean Exploration. IDOE was the epitome of modern team research on a grand scale, and I had mixed feelings about it. In my worst moments, I wondered if I was becoming an old Bigelow, out of tune with the times. In moments that I hoped were more rational, I thought that a lot of it was not good research and was wasting millions of dollars.

Of course team research is necessary in many modern oceanic problems. I have no quarrel with that. However, when projects get into the megabuck range, we should be very sure that they are problem solving ventures, planned with imagination and thoroughness and executed well. I think POLYMODE qualified. The fatal weakness of IDOE was that it had too many megabucks to spend and opened the doors wide to a number of inferior proposals.

SEAGRASS was one of the worst, although it was less expensive than most. It was no more than a descriptive account of a variety of sea-grass systems. The leader of the project-- I can't even remember his name-- reiterated that out of this study would come what he called a conceptual model of sea-grass systems. I don't quite know what that meant. From the results I saw, it was a euphemism for fuzzy, qualitative generalities.

Then there was GEOSECS, a little better scientifically but expensive enough to make a strong bid for the cellar position. It purported to establish a data bank on an almost world-wide basis of almost every kind of seawater analysis they could think of. Some of the data were good but in general have not been used well enough to justify the cost. Some of the analyses were relatively new and had not been tested rigorously enough to be used so extensively. Much of it will not stand the test of time; Pete says a lot of it is already obsolete. It was a huge pile of data looking for problems. It qualified for megabucks but was unacceptable as a Ph.D. thesis proposal. (Or am I being prejudiced again?)

Somewhere in between was the project on coastal upwelling systems. I think the physical part, headed up by Jim O'Brien, probably was pretty good. I have little praise for Dick Dugdale's leadership in the biological part. It was not well planned and little has come of the massive amounts of data collected. John Walsh probably got more out of it than anyone else in terms of general understanding of the effects of upwelling on biological oceanography, yet I was not highly impressed by his models.

Having watched the development of plankton models for nearly forty years and expressing varying opinions about the merits of particular ones, perhaps I should explain my reasons. My criteria for a good model are simple: 1) It should investigate biological problems that are not well understood by other means. 2) Realistic results are necessary but not sufficient; the results should be analyzed in order to evaluate inter-relationships. We never have enough information on all of the coefficients that go into these models, and sensitivity analyses are needed to determine the relative importance of individual factors and to investigate possible errors.

Of course I confess I did not do enough of that in complex models such as the one in the 1949 paper. Each sensitivity analysis was about a one-week job in pre-computer days. I got lazy. And after that job, I did not do much more modeling beacuse I felt that a better knowledge of plankton physiology and the physical oceanography of near-surface waters was necessary in order to make a significant improvement.

By the time John Walsh came along, physiological knowledge was of course much further advanced. His biological treatment was quite detailed, possibly more so than was needed. His physical oceanographic coefficients were open to question. The values could vary by an order of magnitude; I suspected that his assumptions about both eddy diffusivity and upwelling might be too large. The results of his complicated computer models looked fairly realistic in most cases, but they were gee-whiz pictures as difficult to interpret by inspection as nature itself. If he examined them for possible errors, he did not share the information with the rest of the world. I didn't learn much from them.

Joe Wroblewski was a graduate student working with Jim O'Brien at the time of the IDOE project. His thesis was a biological model of an upwelling system, and he continued with some similar work after he came to Dal. His models were simpler than John's and were aimed at particular problems rather than being a generalized picture of distribution. His work has fulfilled my criteria for good models. His present work on warm core rings and the biological effects of their impingement on the shelf promises to be very fruitful.

Of course, top honours go to John Steele, who has produced many models through the years, a majority of them excellent. His treatment of both physiological and physical processes tends to be quite simple, but he is ingenious in picking the key elements required for any particular problem. One of the best of the lot in my opinion was a recent model developed by Steele and Evans on problems of plankton patchiness. The basic concept was that diel migration by zooplankton causes a dislocation of phytoplankton and zooplankton populations, thus leading to differential grazing rates that produce phytoplankton patchiness, and furthermore, the part that was new and particularly important, the model demonstrated a mechanism for the development of zooplankton patchiness, too, which is necessary for producing the grazing effect.

I cannot be quite so complimentary about the models produced by Trevor Platt. His mathematical formulations are sophisticated and meticulously evaluated, but the biological problem sometimes is trivial. His model of phytoplankton patchiness re-invented the wheel that Kierstead and Slobodkin invented in 1953 to deal with red tide patches-- a fancier wheel, to be sure, but there was nothing new in the concept. He produced a very complex model of photosynthesis in which carbon-14 experiments were analyzed in terms of physical factors. Effects of nutrient limitation were not included. It was therefore applicable only to a limited part of the year and was vitiated by the growing realization that phytoplankton circulating in the mixed layer prevailing at that time of year generally has a higher rate of production than in experiments conducted at fixed light intensities. He later modified the analysis with a model showing what might happen with certain assumptions about the character of the mixing. Unfortunately, thought, we do not yet know enough about either the spectrum of vertical mixing or the physiological effects of varying light intensity for the model to be useful.

Examination of IDOE proposals and progress reports was a keen reminder of the vast changes that had taken place during the course of my career. I had been frustrated by the fact that our sampling was so crude and spotty, and our analytical methods were uncomfortably slow and laborious. I would have been delighted to have methods for rapid sampling or continuous profiling and an auto-analyser to produce numbers rapidly. Yet extensive use of these methods in IDOE projects had produced awesome masses of data that were well nigh indigestable. They produced pretty pictures of real distributions so complex that they defied interpretation. The statistical averages no doubt were better than mine but probably not markedly so and probably were no better in deducing scientific generalizations.

Walter Munk once remarked that my principal scientific talent was the ability to look at a set of numbers and make sense out of them. He was quite right, and I think most biological oceanographers operate in a similarly deductive fashion, but there is a limit to the amount of data that we can scrutinize effectively.

I feel quite sure that problem solving programs producing a modest and manageable data set, such as Bob's on the Scotian shelf, for example, are more productive scientifically than the mindless data gathering that characterized much of the IDOE.

During that period, I tried an analysis that stemmed from a very interesting paper by Bill Sutcliffe on fish stocks in the Gulf of St. Lawrence, which indicated that the abundance of a number of fish stocks was related to the amount of freshwater drainage into the Gulf at an earlier time when the young of these species were spawned and were developing. Logically, this could be extrapolated to a conclusion that nutrient enrichment associated with the fresh water had provided an abundance of food for developing larvae. However, there are a lot of steps in the food web between, and I wondered if I could fill some gaps. McGill people had done a lot of surveying in the Gulf which had not been thoroughly worked up because of the premature death of the principal investigator. I spent a lot of time on that but with negligible results. The circulation pattern, with discontinous slugs of nutrient-rich water penetrating into the Gulf from the St. Lawrence River, was too complicated for good analysis.

During that period, I also made a couple of trips to URI as a member of an advisory committee on the MERL mesocosm experiments. My previous visit a few years earlier had been brief, and these meetings gave me a better opportunity to get acquainted with the new young staff members and the work they were doing. I was impressed. They were people I would have enjoyed working with if I had taken the URI option instead of coming to Dal. It would have been a simpler life, of a sort that I was more accustomed to than the administrative job of trying to promote a department here. However, I didn't regret the choice that I made. The Dal challenge helped to assuage the Yale frustrations.

As for the MERL experiments, I felt that they were well worthwhile, although I would not have cared to become deeply involved in them myself. Like John Strickland, I felt that only a limited amount of good basic science could be obtained from such studies, and I was quite willing to leave the applied science to people who knew a lot more than I did about the techniques for measuring trace quantities of pollutants and analyzing their effects on the biota. However, such work is important and should be encouraged. With many of our harbours and coastal waters being subjected to a variety of pollutants-- hydrocarbons, other organics, and trace metals-- field studies can only tell us that the environment is sick, for one reason or another. Mesocosm experiments are potentially useful for sorting out similarities and differences in the effects of the various pollutants on the portion of the ecosystem that can be included, as well as possible synergistic or antagonistic effects of two or more pollutants.

I have to add a sad postscript to the account of the MERL project. During a reception at the time of the second visit, John Knauss told me that Charlie Fish was in a nursing home. His health had failed badly, to the point where Bobbie was no longer able to take care of him. He didn't live long after that.

That book of collected reprints tells much of the story about my own research but nothing about the other function of helping to steer graduate students toward the Ph.D., which in some cases was personally more satisfying than my own research efforts. In addition to formal teaching and membership on many thesis committees, I was direct supervisor of nine Ph.D's at Yale and six more at Dal. I've mentioned three of the latter. Then there was Colin Duerdon, who began his work under Walton Watt, and I took over when Walton left. He went to government service.

Pat Johnson-- poor Pat-- her story is tragic. At Dal, she was a top-notch student and well beloved by everyone. Her long-term struggle with diabetes seemed well under control then, but commonly associated ailments rapidly built up after that. Her failing eyesight wasn't improved by attempted operations. Virtually blind and hospitalized for serious kidney malfunction, she died a few months ago. There are heartaches in this as well as rewards.

The last student was John Marra. We considered several options for a thesis topic. The one he chose was an old unsolved problem that had been lying around waiting for someone to work on it-- the question of the effect of phytoplankton of varying light intensity associated with vertical movements in a mixed layer. He found that productivity was significantly increased. His thesis upset the applecart, adding another problem to the interpretation of productivity experiments. He left just as I was retiring and is now making a good name for himself at Lamont.

In 1974, we built a new cottage on our lake, a little more spacious than the other one and well enough insulated so we could be comfortable there in the winter. The previous winter before the cottage was built, we spent most of our weekends cutting a right-of-way through the woods to the site and had it bulldozed in the spring before construction started. Later I did some of the interior finishing-- kitchen cupboards and shelves and such-- and fitted it out with rustic furniture, while Lucy slapped gallons of polyurethane finish on walls and ceilings.

We sold our other cottage to Ken and Isabella Mann. They and other cottagers on the lake provide some pleasant sociability.

During this period, I had decided to retire at sixty-five rather than staying on a few more years as I might have done. I had been teaching courses in biological oceanography for nearly thirty years and was getting a little weary of it, even though the course content had changed almost beyond recognition during that time. I had been amusing myself with data bashing, but nothing very profitable had come of that. In order to return to profitable experimental work, I would need to master new techniques and new instruments that had come in during my paper-pushing years, and I felt no great enthusiasm for a major retraining program.

Most of the associates of my generation have played it the same way, retiring completely or considerably reducing their professional activities. The drive diminishes, if not the intellectual interest, and we feel only occasional slight twinges of guilt that we are no longer workaholics. There are, of course, exceptions. One remarkable "elder," Alfred Redfield, continued to work off and on for years. Alfred had mellowed, and we became good friends. When I last saw him a few years ago, he was approaching the age of ninety, still keen-minded though quite deaf and somewhat disabled physically, and was about to publish another book. Since then, he too has departed.

Anyway, I went into semi-retirement, and Lucy did the same a year later. I could think of nothing more rewarding than for us to have leisure time together after all those busy years, and thus it has proven to be.

We spend most of the summertime at the cottage and occasional briefer periods in winter. The weather hasn't provided much opportunity for winter sports in recent years, but in summer, Lucy has her vegetable garden, and I have a little project of forest management, which also includes converting dead trees and culls into firewood.

Lucy is a sociable creature who has developed numerous volunteer activities since retirement. She is busy with these much of the time except in summer. I've been partially occupied with peripheral oceanographic activities, but I confess that as time goes on, I don't try as hard as I once did to keep up with progress in oceanography and am more interested in broadening my general education, which got short shrift during the years when my nose was on the grindstone most of the time.

We count ourselves singularly fortunate in that two of our daughters-- Grace and Milly-- live in this area. We see them often. Louise and her family live in Missouri. We have a visit about once a year, here or there, but my, those grandchildren do grow and change between one year and the next. We would like to see them more often, but one can't have everything. We have a lot to be thankful for.