Autobiographical Memoir: Joseph Pedlosky

1/11/16 (start)

Preface

In my 78th year there is so much more to look back on than there is to anticipate that it seemed useful to me to spend some time trying to organize my thoughts about the past and to try to describe how my life evolved. It's almost a truism that a life story told from its near ending tends to assume an apparent logical and inevitable sequence when, in fact, going forward, it appears as a jumble of accidents, inadvertent misunderstandings, and unforeseen results of seemingly small choices. The theory of chaos calls that sensitivity to initial conditions. So, while that makes prediction nearly impossible beyond a small time interval, the trajectory isn't random but is strongly conditioned by personal history and seemingly trivial personal choices.

I am writing primarily for my own amusement and wonder, understanding that my personal life may be of little interest to the wider world. I hope it will be of some interest to my children, Dove and Anna, to an extended family and perhaps to a group of close friends and colleagues. A secondary motivation follows from my rare and, I think unique, good luck to have had the opportunity in my scientific life in the area of meteorology and oceanography to have had worked with two of the greatest scientists in the field, Jule Charney and Henry Stommel. I also benefited greatly from mentoring by a number of other first-class scientists. Part of my story is theirs and perhaps that might add some more general interest in my storytelling.

Whenever someone writes an autobiographical memoir the major question that must be asked is whether it is accurate and truthful. To quote Colonel Stingo, a colorful character and a favorite of the writer A.J. Liebling, " Memory grows furtive". So, for the record: I aim to be as accurate as memory and the written record allow. But I will concur with the great Venetian hero, Paolo Sarpi, who claimed to never tell a lie but who never pledged to tell everything. Surely, some events in life whose revelation might hurt others need not be included unless those events are defining features necessary to understand the path of an unfolding life. That is the standard to which I will attempt to hold myself in what follows.

It has been, on the whole, a good life and I have been lucky. It does not mean that I escaped tragedy or failure, but I do count myself fortunate in family, friends and mentors. My scientific life has been rewarding beyond my childhood dreams and I have many people to thank for that as I hope to make obvious in what follows. Now, on with the task!

Chapter 1

Childhood

Memories are important to people. They keep the wolf of insignificance from the door. ♦

I was born April 7, 1938 in the city of Paterson, N.J. The great advantage of Paterson as a birthplace is that in later life you never need suffer from nostalgia. It was, even then, a decaying post-industrial city although it was safer and more attractive than it came to be with the passage of years. Children were safe playing in the streets and serious crime was very limited. It has since gotten much worse.

My father, David Pedlosky, was a gym teacher in the public school system. He was not a sophisticated man, nor an intellectual. His teaching position required only a two -year certificate that he obtained from a small college near Newark called Upsala. He was a nervous man and since teachers in those pre union days earned very little he was much beset by financial worries. The world was also changing in ways he felt was beyond his control and sympathy. He was quick to anger and when I was around 8 years old, he fell seriously ill with diabetes, a malady that was to lead to his death at age 59. He was also a man whose stubbornness could manifest itself as great integrity and loyalty and he expressed his love for his family indirectly through very hard work to supplement his meager teaching salary. As many teachers did in those days, he had a second job, in his case as a salesman in an auto appliance store so he was often away from home working until late at night. Exhaustion was

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♦ From Mr. Sammler's Planet. *Saul Bellow* Viking Press 1970 probably a large contributor, along with his diabetes, to his quick temper and early death.

My mother, Lillian Pedlosky (nee` Levit), born, like my father in 1905, was an inherently elegant woman. Although she ended her education after high school, she was proud of her high school diploma; many of her childhood friends never obtained one. Her childhood started in a small mining town in Pennsylvania named Shenandoah, after the beautiful valley in Virginia. My mother assured me it shared none of the beauty and her family was one of the few Jewish families in town. Her father owned a sequence of businesses, fairly successful, including a movie theater, and a bakery. After her early years working as an office bookkeeper at Prudential Insurance Company in Newark, the city where she met my father, she had to leave her job because married women were not allowed to continue to work at the company. She was then a housewife when I was young but later, when I was about 15, she, too, took on a job as a bookkeeper in a clothing store in Paterson to supplement the family's finances. Both my mother and father were physically stretched to exhaustion. Still, on the occasions of weddings and bar-mitzvah's some inherently joyful aspects of their personalities would peek through and it was a childhood pleasure to see them dance and laugh together in a short vacation from reality.

So, here I came, firstborn, into a family of tense, nervous people trying to make their way. I was deeply loved by both of them and the security of that love and the general insecurity of circumstances together fed contrasting feelings in me about my future. I was, all in all, though, a happy child.

Arguments at home, usually about money, led me to try to find refuge in more orderly places. School was one of those. The calm of school was very attractive even when the classroom could provide me with even more puzzles about life. At the same time, I could find my own secure places by reading, an early passion. Going deeply into a book, whether it was an adventure story or a history was an escape where the outer world disappeared, at least for the moment. We had few books in our house but my parents had bought an inexpensive encyclopedia, so inexpensive that for illustrations it often used classical etchings and paintings for historical events, for example, the marvelous painting by Gerome of Cleopatra unrolled from the rug in front of Caesar remains embedded in my memory as well as a heroic etching of Roland at the Pass by an artist whose name I cannot retrieve (Gustave Dore?). All these heroic adventures in history and literature, so foreign to my humdrum daily life, had the curious effect of convincing me that I was destined for a "big" life even in the absence of any rational reason why that should be true.

The accident of my birthdate meant that I started elementary school at 4 ½ years old and so was always the youngest and least physically adept in my class. That led to more fantasizing and more interior escape and to also encourage the one early academic ability I had, which was an ability to read easily and swiftly.

The readers we had in elementary school, the Dick and Jane series, were another source of perplexity. In those books the father and mother were so confident, kind, and lacking for nothing that they were unrecognizable types for me. The economic situation for Dick and Jane was also foreign. So, I began to wonder if somewhere there was a possible life different from my own, one less tense and one less insecure. We were living at the time I started school during WWII in a small three-room apartment. With my younger sister and just one bedroom, it meant I slept in the living room on a cot wheeled from a closet in my parents' bedroom each evening. My sister, Irene, slept in a crib in the single bedroom with my parents until we moved to our own house when I was 11 and she was 6. I assumed this was normal. It did mean that when my Dick and Jane reader had a chapter about Dick's favorite tree in his backyard, where he delighted to sit in its shade on warm sunny days, it was as fabulous as any historical myth I might see in the encyclopedia. I remember the teacher, in the insensitive way that was normal in PS 13, asking if each of us had a favorite tree. Since there were no trees in the stunted, unkempt yard behind the apartment we lived in, I had to admit I had no such tree and I got the impression that I had given the wrong answer. So, the separation between my reality and what was described, as a normal child's life in school, was a constant perplexity to me.

Indeed, I seem to recall a tremendous fog of confusion. I started school with the single advantage of being a good reader but other aspects of elementary school work often flummoxed me. Simple tasks like writing my name in script seemed Herculean to me. Writing the script capital *J* of my first name was appallingly hard. My greatest challenge seemed to come in my first collision with arithmetic. I had no difficulty in understanding the idea of addition. However, in what must have been an early grade, the teacher used flash cards and the idea was that when you were shown a flash card that had, for example, 4+3 written on it, you were supposed to shout SEVEN as fast as you could. For some reason I can't explain I would say with great confidence, “four plus three is seven” and the teacher, always a woman in those days, would tell me “ No, no, no; just say seven”. Then she would flash another combination and the routine would repeat. I could tell she was close to the edge. One day she invited my father, who was teaching in that school, to watch my stubborn refusal to play it right. She asked a tall Chinese student to get up and demonstrate how it should be done correctly. I saw no difference in our two responses. Had it been stubbornness on my part my father perhaps would have sympathized but it seemed clear I was deficient in some way. So, later that week my father, at home, fashioned our own set of flash cards and perhaps with special pleading on his part finally made me understand what was wanted. The light dawned and the fog temporarily lifted. Still, arithmetic, especially the addition of long columns of large numbers, drove me crazy with boredom and I began to believe that arithmetic was a special problem for me. This would hold true for all math until well into high school.

Not that the teaching was particularly good in PS 13. I remember one day, in the 5th grade, the teacher without warning started filling all the blackboards with text dealing with light. I remember little of the text and there was certainly no explanation of why it was suddenly appearing but one part of the text baffled me and that was the reference to “artificial” light, meaning the light from light bulbs, for example. In my innocence I asked why the light from a light bulb was any different than light from the sun. What made it artificial? I got the look again that said, “Where are you from?”

Another time we were studying South America in our geography books and the text said the people of Chile were “industrious and ambitious”. Those words were new to me and were mouthfuls and were especially emphasized by the teacher. So, again in innocence, I raised my hand to ask "*why* are the people of Chile so industrious and ambitious?" My teacher looked at me with that same quizzical look and swiftly recovered, and searching her own mind for an explanation, quickly remarked, " Well, they probably have more white blood than the other countries". I wish I could say I was appalled by that answer but at the time, when my class and my neighborhood were all white and I knew no black children, I accepted it as a truth given with the indisputable authority of my educator. I am chagrined to remember it as going unchallenged. But it did strike me as odd.

Sometime when I was around 7 or 8 years old my parents told me that I was Jewish. I didn't know what that meant but I inferred from the way they mentioned it that it meant we were different from other Americans. I remember crying about it and resisting the idea but in the end, as the good boy I was, I accepted it. The knowledge came with the understanding that I would shortly start going to Hebrew School each weekday except Friday, after finishing public school, and also on Sunday morning. I was assured I would have a half an hour between those schools during the week to "play". Once I started the Hebrew School, which was located just across the street from our house, I fell right into the rhythm of things. I learned to read and translate Hebrew. I learned to read the Bible in Hebrew and learned the history of the Jewish people* and on Saturdays I attended religious services. As an atheist now it seems strange how fully I accepted all the aspects of the religion as

* One compact version of all Jewish history I've heard is: They tried to kill us, we survived, let's eat.

natural. As I grew older I actually threw myself into Jewish studies and became a standout in the little school. My singing voice in those days was considered sweet and since I knew Hebrew fairly well, I became the boy cantor at the Saturday morning services. On days when other boys celebrated their bar mitzvahs at age 13, I often still sang the service for them and was often mistaken by strangers in attendance for the rabbi's son! In fact, I found the Hebrew school in many ways more intellectually challenging than public school. Biblical interpretation fascinated me. It was the first experience I had with close reading and intensive examination and the sense of the enjoyment of pure learning. When we read the Old Testament stories in Hebrew, they were accompanied by a commentary occupying the lower third of the page by the famous medieval biblical commentator, Rashi. I still remember the introduction to the story of Noah that begins with, " Noah was a good man in his time". "Ah", says Rashi, "it doesn't mean he was a <u>good</u> man, only that he was good <u>compared to everyone else"</u>. Very Jewish, that. I continued long after my own bar mitzvah studying Hebrew Law (Mishnah) privately each Sunday with my rabbi until I left for college since for quite some time I thought Law was going to be my adult occupation. I never had religious doubts until later, but that is something I will get to soon.

On the other hand, public school in its order and safety was a place I felt comfortable and my reading ability and general ability to speak well allowed me to do well in school, math being the exception. Sometime in the seventh grade my parents heard about a small, special public school on the campus of the nearby Montclair State Teachers College (now Montclair University, part of the New Jersey

University system) where there was a demonstration high school, Montclair College High School (CHS). The professors at the Teachers College taught the high school students and the college students observed them teaching. The tuition was negligible but it required a daylong exam to gain entrance. My parents, after being reassured that there was no quota on Jews, encouraged me to apply and take the exam. Seven students from Paterson, 5 of them Jewish, applied and we all got in. It was the first time I had succeeded in something that distinguished me from my background. The school was seventh through twelfth grade and required some travel to get to and from the school daily but with a parents' car pool in the morning and public buses in the afternoon it was easily done. The class size was moderate, about 30 to a class and so only about 180 students in the whole school. I loved it. My life was opening up and the possibility for the big life I had hoped for seemed a bit more possible.

There was a poignant side story to my change of schools. One of the girls in my class, I will call her Marilyn M, and so disguise her last name, was just about the smartest person in the class. She also had an open and pleasant personality and was quite popular. When the cohort from Paterson decided to apply to CHS I encouraged her to join us. She came back the next day crestfallen; her parents had decided not to allow her to apply. She told me the news wistfully. It clearly had nothing to do with money; the tuition at CHS was negligible even for my family. I have often wondered what became of Marilyn and wonder about these small life-changing decisions and what effect it has on future trajectories of personal history.

There was a strange feature of the school, at least to me, which made a lasting impression on me. At that time, it was common for the 8th graders to subject the 7th grade boys to a form of initiation or hazing. Each 7th grader was supposed to walk a narrow concrete culvert that was used to channel rainwater to prevent flooding. We were each told to do the walk when the culvert was dry so there was no danger or discomfort in doing it. At one point one had to pass under a low bridge that led to the school cafeteria, but again there was only the slightest discomfort in doing so. Almost all my classmates, including my Paterson friends, did it immediately. For reasons I can't imagine and don't remember, I resisted doing it. I think, if memory serves me, that I felt it violated some innate sense of personal dignity. The pressure from the 8th grade boys became psychologically intense and I felt very alone and wondered if I could continue in the school, which, as I mentioned, had deep symbolic meaning for me as an entrance to a larger life. Finally, one day when I was standing alone in an empty class room, two of my Paterson friends came to me and implored me to "walk the concrete", as it was called. I succumbed to their entreaties, did it with a sense of relief and also a deep sense of being diminished. In retrospect it was the signal of submission that most bothered me, while what most bothered my friends was that I had resisted while they fell into line immediately.

Had it ended there it would have just remained an embarrassing memory. But there was an important part of the story still to come. One other boy whose name I still remember, Doug Hale, also refused. Doug was neither particularly adept academically, nor was he athletically gifted but he was stubborn. While I had caved in to the pressure he resisted. What followed was illuminating to me. The other 7th grade boys then turned on him viciously; his holding out was humiliating to them, I suppose. They cornered him in a rarely visited part of the campus and showered him with acorns and shouted at him to give in but his strength of character was greater than mine and he did not succumb. Although I did not join in the harassment, I saw it and did nothing to stop it and that made me feel even worse than having succumbed myself. He must have finally told his parents what was happening because soon after, in gym class (boys' gym), the instructor lectured us on our behavior and scolded us for not respecting Doug's wishes. The teacher, a Mr. DioGuardi , and what an appropriate name♦, made us feel small and I felt infinitesimal. I swore to myself that if that situation ever arose again, I would be on the side of the Doug Hales in life. A situation like that did arise much later in life and I did not succumb. This is a story that is told further on in this memoir. I know that at that later time I did not consciously think of this CHS episode but reflecting on it now it must have planted a healthy seed. So, this might have been one of the greatest lessons I learned in High School.

Otherwise, I did well in that school but mathematics still seemed difficult for me. But in the more humanistic subjects I did better. I made good friends in school who came from outside Paterson and felt generally very happy. And, I discovered girls.

In public school I had a fondness for one or two of the girls in my class but nothing very specific, just the sense that I especially enjoyed their company while in school but the idea of seeing a girl outside of school never occurred to me. That changed at CHS. I developed a crush on an attractive girl in my class. She was a cheerleader although given how small our school was that did not have any special

♦ In Italian: "Let God watch."

significance. A substantial fraction of the girls in my class were cheerleaders. More significantly, she was enamored of another boy in the class and although we sometimes dated and went to dances together it never progressed very far in spite of my longings that it might. The principal effect that disappointment had on me was to make me subsequently think it was simply marvelous, even miraculous, whenever any woman expressed enthusiasm for a romantic relationship with me. I became very vulnerable to that response.

The other big effect of that puppy love crush was to produce a religious crisis. Since the girl involved was not Jewish, both my parents and her parents were very upset. I think it was then that it crossed my mind that if there had to be a choice between God and this girl, there was no question where my inclinations were; I could easily get along without the Deity. Of such things are philosophies made and unmade.

In our high school homeroom meetings various issues were discussed that the students were allowed to handle in the general context of a student government. I found that when the issues became complex, I could often suggest a method of solving what had appeared to be difficult obstacles. Often, I was perplexed that other students couldn't see how easy it might be to deal with such problems. I remember one homeroom teacher who one day looked at me in amazement and said, "that's the second good idea you've had today". I was very pleased but in retrospect it must have meant that I had been expected to contribute very little.

I was neither a standout "brain" nor a popular athlete. Eventually, I was elected to the student council but I would have to admit that I was not a class leader although my opinions in classes met with increasing approval. I did sense a developing skill in speaking and I believe that I considered that would be an advantage if I entered the law profession.

Sometime during my 8th grade year I developed a young boy's interest in airplanes, especially combat airplanes. I suppose a psychologist of Freudian persuasion could link it to my nascent interest in sex; flying often has that identification. In my own case it meant a serious collecting of information about many airplane types and their performance. Aircraft companies in those days were happy to send beautiful photos and detailed information about their products and I collected them in organized scrapbooks that have disappeared after the deaths of my parents who had, for years, stored them as evidence (for them) of my "special" intelligence.

A more concrete manifestation of that airplane interest was demonstrated when I volunteered in what was called the "Ground Observer Corps". This was an Air Force sponsored group of civilian citizens who manned observation platforms around the country to identify air traffic since in those days low flying aircraft were literally "under the radar" and unseen. For several hours each week I would man an observation post in Paterson on the roof of the YMCA, as shown in the figure on page 24, and with a special phone connection, call into a central processing center the sighting of any aircraft, its estimated altitude and its direction of flight. It was an activity both exciting and boring. Exciting, because it really was part of the US air defense system and boring because the activity itself rarely was anything but routine. After a certain number of hours spent in observing one received an official medallion pin in the form of wings. I still have mine together with pins denoting membership in several other honor societies, but this was the first.

Another interest that developed about this time was in jazz. I still can recall the first time I was consciously aware of jazz. As mentioned above, the return trip from CHS involved taking public buses. One bus took the crew of Paterson boys from Montclair to downtown Paterson where we went our separate ways. I had to walk around the square containing City Hall to get a bus ride to our home in Fairlawn. As I was passing a record store one day, I was struck by the music coming from the loudspeaker over the store's entrance. The music was like nothing I was aware of having heard before. I was so taken with it that I immediately entered the shop and asked the owner what I was hearing. I was informed that I was listening to Artie Shaw's version of "Softly as in a Morning Sunrise". I was transfixed. Spinning the dial on the radio at home I discovered others, especially Benny Goodman and became enamored of the clarinet. It was to be another 25 years before I was in the position to learn how to play the clarinet but my interest in jazz became deeper and deeper. Alas, we didn't own a phonograph at home and so my ability to hear jazz was limited to what was sometimes available on the local radio stations.

The issue of my future direction in life became more and more pressing as I advanced through High School. My father was dead set against my becoming a lawyer and when I was a senior in High School and developed an interest in Physics, I joined that interest with that pre-existing, youthful interest in airplanes and decided to become an aeronautical engineer. I was pleased by that possibility, my father was relieved, and I felt I had chosen a good path out of childhood. It wasn't clear how this would happen but at least I had the glimmer of a plan.

Also as a senior, something remarkable started to happen with regard to math. We were studying trigonometric relations, i.e. the relationship between various combinations of trigonometric functions. While most of my classmates approached these problems (rather simple and banal, certainly) by searching in a handbook for possibly useful relations between the functions, I began to "see" in the original formulae other "natural" equivalences that were slightly hidden. I began to be the fellow giving help to my mates rather than being the fellow asking for help. When it dawned on me what was happening it made me more confident about my engineering career choice although, to be fair, I had no realistic idea of what it meant to be an engineer, aeronautical or otherwise. In fact, I discovered later that I was really not cut out to be an engineer and that "scientist" would be a more natural career. At that time and in my situation, there was no guidance available to help me think that through and, to be truthful, I didn't even know that one could be a scientist as a career choice. Naturally, I had heard of Einstein, and knew of the physicists who made the Bomb, but I automatically assumed these were very special people whose numbers were so small that it never occurred to me to imagine myself as one of them.

My ideas of my future were somewhat shaken by the results of my SAT scores. I took the required college pre-application exam with no preparation as was common in those days (1954). I was told that I finished in the 98th percentile in the verbal SAT but in only the 75th percentile in the math portion. I remember one high school teacher asking me if were really sure I wanted to go into engineering! At that point, it seemed to me that it was a little late to change plans.

On the basis of a very rudimentary advice I applied to five East Coast schools that each had a Guggenheim School of Aeronautical engineering. The five were MIT, RPI (Rensselaer Polytechnic Institute), NYU (which then had an engineering campus at Washington Heights), Georgia Tech, and the University of Alabama. I wrote to each of the five asking for a catalogue and mentioning my intention to apply. I immediately received a rather smudgy letter from U. of Alabama saying that I had been admitted subject only to my successfully completing high school! I got a lot of good- natured ribbing from my friends about that. Talk about safety schools!

All five schools admitted me. The choice was difficult. I ruled out Georgia Tech immediately for no good reason except it seemed very far away. Note that I did not apply to any West Coast schools, e.g. Cal Tech which had an excellent program in aeronautical engineering. The idea of going to school that far away seemed bizarre to me. It's also true that *I had never heard of Cal Tech* and no guidance counselor mentioned it. I ruled out NYU simply because it was too close to home and I found New York City unattractive and daunting. I was strongly attracted to RPI for two reasons. First, it had a good reputation and was not as scary a prospect as MIT and, second, a good friend in my high school class and an excellent student, was admitted there and was enthusiastic about it and I thought it would be reassuring to start this adventure with a ready made friend. Everyone told me that MIT was really, really hard and that the flunk out rate was high in the first year. I was terrified of being embarrassed at failing. My father, as soon as I was accepted there, began telling friends and neighbors about it and I kept shushing him. I hadn't decided to go there and I was frightened to consider it. I was afraid to be home by Thanksgiving of my freshman year as a failed flunk out.

MIT impressed me in another way. As part of the application process, a personal interview was necessary. It was not as common in those days (1955) to consider distant travel so casually so the interview was generally held in your hometown (or the closest location) by an MIT alumnus who forwarded a report to the Institute. It turned out that my father knew the MIT alumnus in Paterson (he had taught so many kids in gym class that he knew about as many people as the local obstetrician) so he drove me to the interview (I was too young to drive myself) and waited in the next room while I was interviewed. When the really rather pleasant interview ended and the alumnus and I returned to where my father was waiting, my father asked him whether MIT had a quota for Jewish kids. Quotas for Jews were quite common in the Ivy League and even MIT had a quota earlier but WWII and the Holocaust had the positive effect of making anti-Semitism no longer publicly acceptable. There is a good side to hypocrisy. You might dislike Jews but it was no longer acceptable in polite circles to say so or act on those feelings.

My interviewer asked my father to wait for a moment and went back to the study where my interview took place and returned with the form he had to fill out. He showed my father the printed instructions from MIT to the interviewer that stressed that under no circumstance should the interviewer write anything that would identify the racial or ethnic background of the candidate. He was quite proud of this and I began to get the feeling that, inadequate as I might feel, at least my future at MIT would depend only on my capabilities.

I have mentioned the financial tightness of my family's situation. It never occurred to my parents to take that into account in determining where I should go to school. The only consideration was where I could prepare myself best for the future. When I understood, far too late in life, how much my parents sacrificed for me, I am abashed at the intensity of their love for me and whether I sufficiently acknowledged it to them.

Photos :Early Days

A naturally happy child.

An early enthusiasm for baseball.

With my younger and less happy sister Irene and my father on E 26th St. Paterson, NJ.

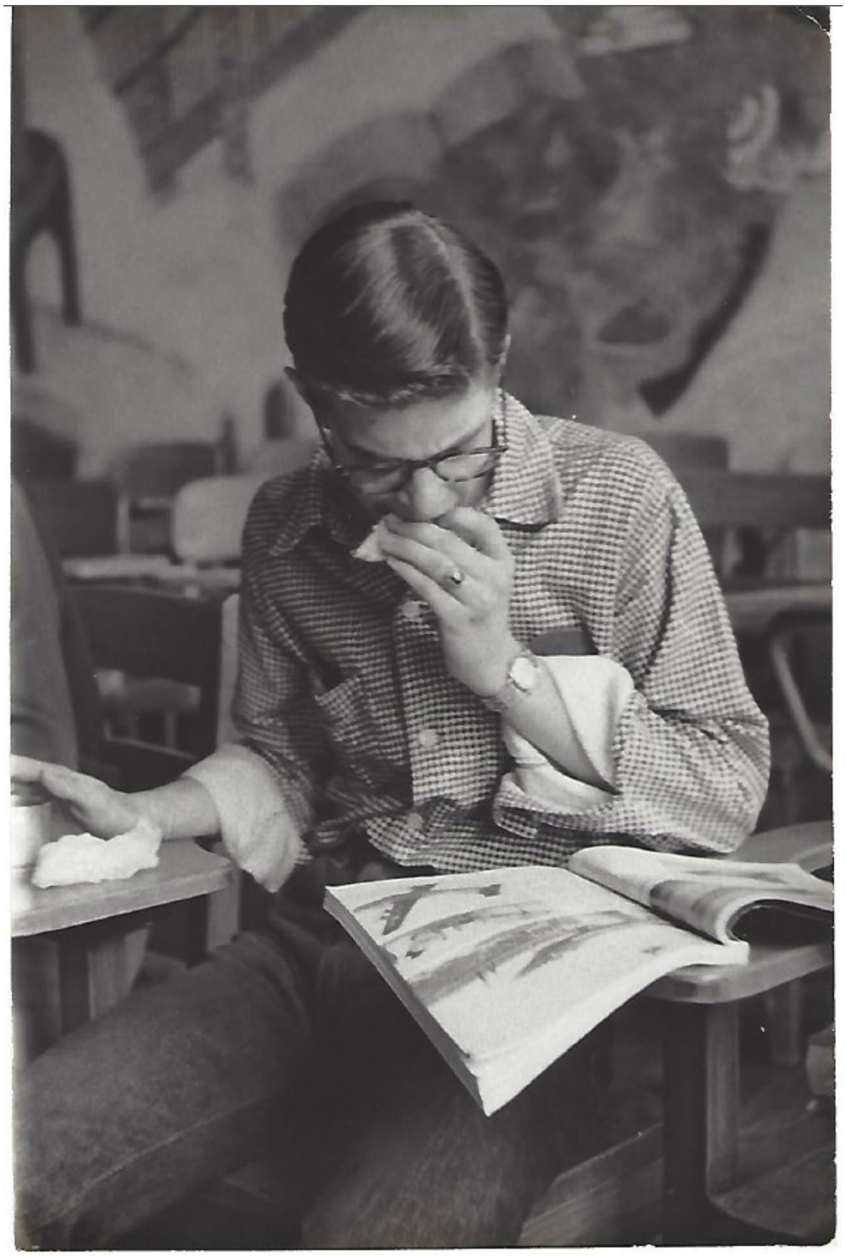
Photos: High School

The GOC “lookout post” in Paterson,NJ.

High School photo: taken in my senior year for our Yearbook. I think it shows a very young kid (barely17 yrs.) not really ready for life’s rigors.

I am third from left. The girl on the far right of the photo was the object of my unrequited affections.

My proud parents and I get ready for High School graduation.



In Senior Home room having lunch while perusing an aviation magazine. I am proudly wearing my College High School class ring and my bar mitzvah watch.

With some of my classmates at our post graduation party in New York. I, and my date are at center. Although my date for the evening was lovely I would have preferred you-know-who.

Chapter 2

An Undergraduate at MIT

In the end, without much deep knowledge of any of the schools, I chose to go to MIT. The principal reason was that I had heard it was the best school and the hardest and that I was frightened to go there. It would not be the last time that I decided to do something because I was frightened of it. I felt that if I avoided MIT because I was scared of it, I would be burdened all my life by the knowledge I had avoided the best possibility out of fear. I was more afraid of being so disappointed in my opinion of myself than I was afraid of what challenge MIT presented.

MIT did turn out to be hard for me even before my first semester began. I had applied for financial aid, a scholarship, and was directed to visit an interview site in nearby Newark. A group of young men (it turned out that in the class of '59 there would be 8 young women and about 980 young men!) waited to be called in one by one for a crucial interview. As I recall it, a committee of older men did the interview and since I said I wanted to become an aeronautical engineer they asked if I had built model airplanes. It had never occurred to me to try to build a model airplane in the restricted space of our apartment, especially one that had a real engine. I knew that a boy somewhat older than I who lived in the more elegant apartment house next to us had been burnt horribly when *his* flying model somehow exploded while he was refueling it and the scarred tissue on his face was all the evidence I needed not to go in that direction. My negative answer clearly elicited their disapproval and so, for that first year, there was no scholarship. I should add that tuition that first year was

$900 /year and even my parents could swing it without asking me to work during the school year although I worked at a series of menial jobs in the summers from age 14 to save money for college. I worked as a dishwasher in a small restaurant in Paterson and then got a more lucrative job delivering lunches that people ordered by phone from downtown offices. The tips were generous and I often made more than $40 a week in tips, a tidy sum in those days. I worked as a stock boy in the same auto-appliance store my father worked in and worked one summer as a playground director in a low cost housing development. On the whole I found these jobs interesting because it put me in contact with people from quite different cultures; often people with little education. What impressed me in my restaurant job was the generosity of spirit of the waitresses. Their futures were already limited and rather than begrudging me the opportunity that college presented me, they cheered me on and were supportive and happy for me when I got into MIT. Humor helped.

In the late afternoon I often was called out of the kitchen to work behind the soda counter when reporters from the nearby newspaper offices of the Morning Call would come in for a coffee or soda. One of the reporters was an extremely attractive woman, slightly older than one of the young reporters who was trying to impress her. He rather ostentatiously directed his attention to me one afternoon and asked what I wanted to be "when I grew up". Without hesitation I replied, "An engineer. What do you want to be when <u>you</u> grow up?" The owner of the restaurant who was at the cash register nearby doubled over in laughter at that and promised me a raise as a reward.

I found the first semester at MIT excruciatingly difficult and disorienting. The required courses that semester were Physics, Chemistry, Calculus, Humanities and an “elective”. As an aeronautical engineering student my “elective” was a required course in projective geometry, a type of a drafting course requiring the visualization of the intersection of cylinders and cubes, etc. I had no experience at the drafting board and my spatial visualization skills are limited. My freshman advisor, a physicist who had great disdain for this kind of thing, talked me into taking this course before taking a drafting course. It turned into a nightmare for that first semester.

I enjoyed the Physics course even though I did not do well that semester. The immediate emphasis, justly so, on quantitative reasoning was strange to me compared to high school Physics. I loathed Chemistry with its memorization and the lack of basic structure (at least as I saw it). I received a merited C in both those fundamental science courses. I really enjoyed calculus, which seemed very intuitive to me and although I expected an A, I received a B for the semester. I got an A in my Humanities course. Those Humanities courses at MIT were spectacular. Perhaps considered retro today, they focused on Western Civilization and we started by reading Greek tragedies, Western Philosophy and epistemology, all with the original texts (some, of course in translation) instead of using a survey textbook. I recall being given the assignment in that first semester of refuting any one of Thomas Aquinas’s propositions and that seemed simple enough, since some of his propositions seemed so bizarre, until I realized that if I accepted his previous argument, his reasoning about the proposition I was to refute was bullet proof. Attempting to disprove his previous argument led me back to the one preceding that until finally I was led back to try to refute his first which was his proof of the existence of God. I thought I did a pretty good job on that one and said so as I wound up the paper that was written as a scholastic debate between old Tom and me. I received an A for the paper with the professor's admonition "A little humility Mr. Pedlosky, please".

The grade I was most concerned about was the damned projective geometry class. I had passed the first exam but failed each succeeding exam and did poorly on the final. I was wretched waiting for the grades to be mailed home since, if I failed the course I would have to take it again. The grades came and I received a D in the course. A poor but passing grade it might be but I felt immense relief. I took a pure drafting course as my second semester elective in the belief that I had better learn to do that work if I was going to continue as an engineer. I had the same professor and although I did B work, he gave me a C as a final second semester grade as probably a justified balancing of his compassionate D of the first semester. My grade point average the first semester was a 3.5 out of 5, an average grade, and only that high because of my A in the Humanities course. Nevertheless, I was hooked on the MIT way of thinking and looked forward to the second semester.

When I called my father the Saturday the grades arrived (in those days the grades were sent home so the parents could see them) he was, as usual, working in the auto appliance store. When I told him my grades, that I considered mediocre at best, he surprised me by saying just how proud he was of me. At key times like that his basic love and generosity punched through the brusque and angry exterior he normally showed. I was, of course, deeply grateful for that generous response.

The second semester was better. I "got it". I was placed in an Honors Calculus course. My grades improved (4.2/5) so that I made second Dean's list (i.e. my GPA was a B+) and I began to feel more and more at home at MIT. By my second year I was doing very well and I truly enjoyed the courses and the professors I was getting to know. I also started some of my aeronautics courses and although there was still a drafting course in the curriculum that semester, I managed to get through it although by then it was clear to me that if I stayed in aeronautics it was going to have to be as some kind of theoretician. I was truly excited by the thermodynamics course I took; the elegance of the subject and its power was inspiring.

It also was the occasion of a spontaneous bull session that struck me as being uniquely MIT-like. A small group of my classmates in my dormitory would spend time together discussing the homework or lecture material in the classes we had in common as well as the material in our Humanities courses. One evening the topic of conversation turned towards the newly introduced idea in thermodynamics of entropy. Entropy is often loosely mentioned as a measure of disorder or a measure of the failure to use all the available energy to do useful work but its thermodynamic definition is mathematical and the connection between the mathematical definition and the qualitative attributes associated with it is not obvious. This for us was the first example of a concept like that. Up to that point any concept with a precise physical/mathematical definition also had an intuitive meaning to us that predated its mathematical appearance. Ideas like momentum, energy, angular momentum all had their precise definitions that resonated with an intuitive notion we already had.

Entropy was different and we spent an interesting if fruitless several hours that evening trying to understand the connection between the precise thermodynamic definition of entropy, and the qualitative ideas we heard it was supposed to represent. In retrospect, it was the seriousness of our attempt, without pretention, and the time we gave to the discussion (especially when we were all very pressed for time), which thrilled me. It gave me a glimpse of a future in which conversation with my peers would be on a level of seriousness that betokened the "big life" I was so eager to have. In a strange way it was similar to the biblical studies I no longer had much interest in. I don't mean to imply that it was a religion substitute; it was rather that it had a feeling of being imbued by an idealistic search for knowledge for its own sake. In many ways MIT taught us how to think, to speak and to write. It was a great shaper of lives for those of us who took it seriously beyond its professional career possibilities.

Sometime at the end of my sophomore year or beginning of my junior year I was first introduced to fluid mechanics. I fell in love with the subject immediately. I felt the phenomena were important and beautiful and the mathematical basis for the physics was elegant and in first seeing the equations of motion I began to have that feeling that I first had as a senior in high school; that I could see "into" the equations something of the physical phenomena. I felt swept along with the advective derivative; I felt the push of the pressure gradient. I was in love.

There were three young professors in the department that I thought were charismatic, effective teachers of fluid dynamics: Leon Trilling, Holt Ashley and Erik Møllo-Christensen. Trilling was the teacher in my first fluid mechanics class and his mastery of the subject and the grace with which he taught the basic theorems like Kelvin's theorem just swept me away. With his slight Polish accent, and European manners he radiated a heightened sophistication for a boy from Paterson. I also felt in the class lectures, where there were about 20 of us, that he was speaking directly to me. In fact, some of my classmates jokingly said they thought so too, i.e. that he was lecturing to me. Ashley was a large, physically imposing man, well over 6 feet tall, full of *joie de vivre* and who taught both fluid mechanics and the allied field of aero-elasticity. He had a significant role to play in my private life that I will get to later. Finally, Møllo-Christensen was perhaps the most exotic of the three and had a talent to make simple but elegant connections between fluid mechanics and common day to day phenomena we would never have otherwise imagined. A creative experimentalist, he became my Master's Thesis advisor and my advisor on many academic issues. He also had an interesting personal history as a young member of the resistance to the German occupation of Norway that landed him in a German prison camp for the remainder of the war. He shared those stories with me and another student, Jim Poor, during the time I was working on an experiment with Møllo-Christensen, to be described below. We adored him. He was brilliant and approachable.

Among the many advantages of going to school at MIT is that you were allowed to take as many courses you could handle without paying any extra fee. So by the time I became a junior, I began to realize that my inclinations were more scientific than engineering and that so-called "overload" policy allowed me to take a full suite of advanced physics and math courses in addition to my aeronautical engineering courses. So, as did my Physics Department roommate, Morton Rubin, I took courses in Quantum Mechanics, Statistical Mechanics, advanced applied math, modern algebra and tensor calculus. The intellectual stimulation was intense. The disadvantage of this gorging on a large number of courses was that it amounted to avoiding going very deeply in any subject. In fact, I had a real disinclination, almost a horror at this stage, of spending very much time on any one thing for fear of missing out on the rest of the buffet laid out before me. This would be a serious problem later but I didn't see any warning signals at this time. I was simply having a wonderful time intellectually.

After the freshman and sophomore years when the Humanities courses were the same for all students, one had to choose an area of concentration and I chose literature and I was introduced for the first time to the tremendous richness of the novel, of drama and poetry. A course in American Literature taught by Carvel Collins, a Faulkner expert was inspiring. So, too, was a course on drama taught by Joe Everingham, a drinking buddy of Dylan Thomas and director of the MIT theatre group.

At the same time a course with Dirk Struik in Tensor Calculus introduced me to General Relativity and the formulation of the geometry of curved spaces and I remember the afternoon, when reading in the text we used, a discussion of the fundamental mathematical definition of parallelism that made it obvious how parallel lines could meet in non-flat spaces. I got so excited I started bouncing up and down on the bed in the dormitory. I found the advanced physics and mathematics I was taking to be entrancing. The ideas and mathematical structure (at least how it was in the late 1950's) of Quantum Mechanics seemed to me so elegant and amazing that at times my head was spinning with astonishment. A superb teacher, Francis Lowe, taught the first Quantum Mechanics course I took. His lectures, given without notes, were spellbinding.

Nevertheless, nothing captured me emotionally as much as fluid mechanics. I took every course I could on various aspects of the subject. Some were taught in the MIT math department, one on viscous flows by C.C. Lin (who was the chair of the Applied Mathematics group at MIT where I would go after my Ph.D; something far in the future). Other courses were on supersonic gas flows, stability theories, etc. It was clear to me that this is what I wanted to concentrate on in the future. I wanted to prepare myself as best I could by taking every course that I thought could help me in that goal. There was, however, a big difficulty.

The undergraduate program in aeronautical engineering in those days had as its capstone, a senior year entirely dedicated to a single course, a design course in which the students designed some vehicle, airplane, missile, helicopter, or whatever the choice was that year, that was to meet a certain predetermined performance standard for speed, range, fuel efficiency, etc. The idea was to use that as a device to allow the student to synthesize all the previous courses to meet that requirement. As an engineering educational plan, it clearly had merit. However, the thought of spending my whole senior year in a drafting room designing an airplane filled me with dread and I decided to see if I could do the unthinkable and get released from that fundamental requirement.

I went and spoke to the Chairman of the department at that time who was courteous but unmoved by my plea that I would prefer to use that time to take more physics and math courses. The fact that I was making this effort got back to some of the younger faculty I mentioned earlier and I think it resonated with ideas of their own and a certain kind of informal negotiation started. First, I was asked if I would agree to take the course if, instead of an airplane, I would design a missile nose cone. From my point of view, that missed the point. Finally, the suggestion was made that I could fulfill the requirement by designing the apparatus for a laboratory experiment and do it under the supervision of Professor Møllo-Christensen. Furthermore, I could do that in the summer between my senior year and the 5th year of the Honors Program in the department. The Honors program invited students in their junior year into a 5-year program that led to a Master's degree without the necessity of writing the usual Bachelor degree thesis and a separate Master's thesis. A single thesis at the end of the fifth year was all that was necessary. That sounded like a winning plan to me and I leaped at it. I would also be paid as a Research Assistant for the summer so it was a very attractive alternative to the regular requirement as it allowed me to take all the extra courses I wanted in my senior year.

I had earlier worked with Møllo-Christensen on a simple project during the Christmas break of my senior year. I stayed at MIT that week and did an experiment on oscillating airfoils in the MIT low speed wind tunnel. The idea sprang from a Scientific American article that wondered if the ability of certain marine mammals to maintain such high speeds for as long as they did might be related to a mechanism of drag reduction due to time variations in their body shape. The idea of oscillating the airfoil was a crude attempt to mimic that. I wasn't able to gain much insight in the short amount of time I spent on the project but I enjoyed working with Eric very much and the summer experiment sounded like another possibility for a very stimulating time.

Together, and here memory fails me on the details of exactly how the idea emerged, we decided to try to study heart murmurs. It turns out that as the heart ages the lips of various valves in the heart, for example the mitral valve regulating the flow from the upper to lower ventricle of the heart, begin to calcify. When the lips calcify, they prevent the valve from closing completely when the blood is pumped from the lower ventricle into the aorta thus producing a backflow through a narrow passage into the upper chamber. Similarly, when the blood is pumped from the upper ventricle to the lower chamber, the mitral valve doesn't open completely. Again, the flow passes through a restriction. In both cases the suspicion was that vortices would be produced as the flow moved through the restricted passage. The vortices would bounce against the heart wall, resonate in the chest cavity and produce the heart murmur. At least, that was the idea we operated under. The idea of the experiment was to use some very sensitive and tiny microphones embedded in the wall of the apparatus to relate the sound to the nature of the observed vortices. It is not surprising now, recounting the ambitious agenda of the experiment, that a summer was nowhere near long enough to accomplish the goal. Nevertheless, I learned a lot, some of it humiliatingly primitive. After reading the appropriate sections in Gray's Anatomy, the standard English language anatomy textbook I had a somewhat clear idea of the basic geometry of the flow. The design of the experiment itself was up to me and I proposed an apparatus with the aid of a sympathetic older carpenter in the Department's woodshop. It involved running water from the tap through a vertically standing box, wide laterally but narrow in the 3rd dimension, and perhaps five feet tall. At the entrance a vertical channel led to a constriction provided by movable metal plates forming a V with a slight gap in the bottom that led to the larger lower box that served as a settling chamber and from which the flow exited. The microphones were set in small holes in the wooden walls of the container and the flow was visualized by inserting red dye in the entering stream. There were a host of problems with this simple design. When the box was first filled with water it sprang leaks from all the joining surfaces and inundated the lab and me. That required finer joints and firmer bonding and was eventually solved. The entering flow at the top of the apparatus went through a curved plastic pipe and there was a tendency for large bubbles to form whose oscillations interfered with any conceivable signal from the vortices. The only way I was able to overcome this difficulty was filling the rather heavy apparatus while it lay on my knee (!) with the curved tube below and then slowly raising it across my knee to the vertical position we required. I am glad there is no video record of this absurd protocol being carried out. But it seemed to work.

It was never possible to completely staunch the leaks completely and that meant, with the use of the red dye, that I was usually soaked with red colored water. This led to one special moment when I was walking rapidly from the Aero Department to the Chemistry supply shop to obtain some needed glassware. I happened to meet a friend of mine, a student in another department, and he asked what I was doing that summer. I said I was conducting experiments about the mitral valve of the human heart and he immediately went pale. I suddenly realized that I must have appeared to be drenched in blood! This was a moment to remember.

Sometime during the middle of this travail, a committee of the sympathetic faculty who had helped me with this substitute for the design course came to visit the lab. The lab was on the ground floor with doors open to the lawn around the building and in fine summer weather they were open. There were a lot of electronic measuring devices with wires running everywhere and I was often standing in puddles of water because of the inevitable leaks. The faculty group came and showed great restraint in not telling me to go home and forget about the whole thing. Holt Ashley merely remarked in a friendly way that one day I would look back on the whole thing and laugh. As usual, as the reader can note, he was right.

I did get through the summer (1959) with Møllo-Christensen's help, wrote a report on the experiment which satisfied the requirement for the course I was excused from and was ready to start my fifth year which would, with a thesis, lead to a Master's degree.

While these academic matters were the paramount issues in my life just then, it was also true that my social life had become enriched since my freshman year. There was a period before the first semester at MIT in which new freshmen were invited to visit the various fraternities at MIT. Those parties were an opportunity for the fraternities to decide whether to offer membership to any of the interested new students. It became clear to me that the atmosphere of the fraternity house was socially beyond me. Whether it was too worldly or too socially conscious I felt ill ease in that atmosphere. Perhaps I was just too young, having just turned 17. In any case I received no bids to join any of them and took up residence in a larger dormitory, Burton House, that was the home to 600 students, roughly 4 times the number in my entire high school! I enjoyed the social atmosphere there and made good friends and felt comfortable about the dormitory's attitudes towards study and social life.

The first year at MIT was spent treading academic water full time in an effort to maintain myself at the Institute (as MIT was affectionately or not so affectionately called). There were precious few opportunities to meet young women easily for, as I mentioned, there were few women enrolled at MIT. There were "acquaintance" dances where groups of young women were invited to MIT dorms or other public spaces for an evening of ballroom dancing. The modus operandi was to try to spot an attractive woman in an entering group and dance long enough with her to ask for a phone number and to gauge whether there seemed to be the possibility of carrying things further. If you had the phone number the next challenge was to make a date with the girl. This sounds easy, but also in those days not only did we not have cell phones, we also did not have ordinary phones in our rooms. To make a call in the evening to a girl in another school, you had to descend to my dorm's lobby where there were 6 public telephone booths for 600 students. After waiting for your turn, you dialed the girl's dormitory telephone number and usually found it busy. It was crucial to pretend you were still talking on the phone to prevent impatient other fellows who were waiting their turns to oust you from the booth. Finally, after making contact you might find that the girl who attracted you would agree to accompany you to a weekend movie or a dance. For the first two or three of my years at MIT the idea of a date on a weekday night was considered academic suicide.

In my second year I met a very nice girl from Simmons College who was studying to become a chemist. She was a serious student and a delightful companion. I became very fond of her and we dated regularly throughout that year. I took a job at Sikorsky Helicopter Company that following summer to be near her since she lived in Bridgeport, CT. I found the work at the company rather depressing and an older couple who worked there and with whom I developed a friendship, strongly advised me to go to graduate school and stay in academia. That reinforced an already existing attitude of mine.

By the end of that summer it seemed like my relationship with that very nice young lady had run its course for reasons I couldn't quite figure out. I was certainly rather immature in my relationships with women and my experience with my high school unrequited crush made me tense and unselfconfident around women. In the next few years I had a sequence of girl friends at MIT and my academic work seemed to be going so smoothly that I even dated sometimes during the week; that made me feel very racy and left my roommates astonished. I remember a somewhat older nurse from Mass General I enjoyed being with in my junior year and a lovely young woman from Boston University during my senior year. There were also others. None of these were lasting although the young women were all very nice. I was just not ready for something deeper.

An issue that might seem strange to younger people today was the question of sex. I did know fellows of my age who had very serious girl friends with whom they had complete sexual relations. I was part of a group of young men who did not. The principal reason was that in the pre-pill era the only available contraceptive measure was the essentially unreliable condom. The young men of my group were mostly first- generation collegians. This was our big chance to rise above the economic and cultural stringencies that formed our youth and each of us was terrified that an inadvertent pregnancy would ruin all our hopes for a wider life. Caution was our watchword. So, our sexual life was limited to what we called heavy petting, i.e. anything possible but short of true sexual intercourse. Naturally, there was a lot of frustration involved on our part and from time to time, on the part of the woman. Once in my senior year the girl I was dating, and whom was my guest in my dormitory room made it clear she wanted to “go all the way”. I declined and that ended our nascent relationship. It was a pity because I really liked her but never learned what happened to her after that.

Another part of my social life as an undergraduate related to a discovery that I did have some athletic ability, something of an epiphany for me. It all started with an MIT Athletic Department open house where various sports were advertising themselves in the one large field house the Institute had in those days for athletic activities. I was attracted to a table that advertised the MIT fencing team and in conversation with the young man at the table I was told that if I were good at Ping-Pong, I might be good at fencing. It had something to do with hand-eye coordination. I was very good at Ping-Pong, which was the only non- academic activity at the Hebrew School where I spent my weekday afternoons. So I decided to give it a try. It meant working out 4 days a week and joining the team. I had to choose among the 3 fencing weapons, foil, sabre or epee. The fencing Maestro, Silvio Vitale, suggested that I choose the epee on the basis of how he saw me as I fenced foil. The epee is the direct descendent of the dueling sword. Each of the 3 weapons has a different allowable target. For epee it is the whole body. Each weapon also has different rules about whether you need to parry an adversary's attack before you counter attack. Epee had no "right of way" rule so it counted on just rapid reaction, no parry necessary. It suited me perfectly and I became pretty good so that by my junior year I was fencing regularly on the team and became the lead epee in my senior year. I remember Vitale, or the Maestro as we called him, once came up to me in either my junior and senior year and remarked, with marvel in his voice, that he had spent some time looking up the academic records of students on the team. He expressed astonishment that I was doing so well because it seemed to him that I was so *casual* about things that he took me for a rather unserious student. I realized that my exterior could hide both the intensity of my devotion to my studies and my nervousness on the fencing mat.

I often would invite a girl friend to watch me at a match and once, as a junior, I did quite well and actually, at least that one time, won the meet for the team. The older team captain complimented me that time and said, in a show of great familiarity and generosity, "here, you will need this tonight" and passed me a condom. I was too embarrassed to say I wasn't going to use it but I certainly had a pleasant evening anyway!

All this was background to the events of my fifth year. It started well. I was taking lots of courses: high-speed aerodynamics, statistical and quantum mechanics, advanced math. In fact, one of the teachers of the advanced math course was Louis Nye Howard. A brilliant applied mathematician and a superb teacher. He would have an important role to play in determining my future career path but that's for a later chapter. Harvey Greenspan who would be my future boss when I worked in the math department at MIT taught another math course, one that I found also interesting but more difficult. He had an aggressive exterior but I quickly caught on that he responded to honest questions with care and concern. Questions asked just to impress received withering disdain. In one lesson he was carrying out a rather involved calculation when a student who was in the pure math group but had been forced to take one applied math course asked "aren't you afraid to take a step like that?" implying that the step did not demonstrate the rigor that the student, as a superior pure mathematician, thought was required. Harvey glared at the student and replied with heavy emphasis, " An applied mathematician is not afraid of anything". He won my heart with that one.

At some point during that year I took the qualifying exams in the Aero Department, the first step in the candidacy process. I remember that exam as being rather easy and was informed, almost in passing by Møllo-Christensen as he bounded down the stairs past Jim Poor and me, that we had both succeeded.

The fifth year required a Master's thesis. I worked with Møllo-Christensen on the stability of supersonic shear layers. It was an attempt to deal with a hard problem, the instability of a supersonic shear layer radiated by sound waves that produced inflexion points in the velocity profile. To me it implied the likelihood of instability induced by the radiation but in those pre-computer days it was impossible for me to make much definite progress. It was acceptable as a Master's thesis but it left me feeling unsatisfied.

One day in the spring semester of that year I was walking through the corridor of the Physics Department, Building 6 in the MIT building numerology, when I chanced upon an advertisement. This turned out to be one of those strokes of good fortune, seemingly insignificant at the time, which eventually altered the direction of my life in several and fundamental ways. It was an advertisement for the Geophysical Fluid Dynamics (GFD) summer program at the Woods Hole Oceanographic Institution (WHOI). Had I not been taking those extra physics and math courses I never would have seen the poster advertising the GFD program and my life might have moved on a quite different trajectory This small deviation in a life trajectory changed my life-path drastically. It was an announcement for the 1960 course, which turned out was only the second summer of the program and the first truly organized program in the form it settled into. As I read the announcement it seemed to say to me " Come to the beach on Cape Cod, study fluid dynamics and we will pay you". It was irresistible on all counts.

Chapter 3

I Meet GFD and Other Attractions

During this fifth year at MIT I had moved out of the MIT dormitory system and was living in a spacious if inelegant apartment near Central Square in Cambridge. That area has since been considerably gentrified but in those days our nickname for Central Square was Squalor Square. One of my three apartment mates was a Geology and Geophysics student at MIT, Kern Kenyon, and we had a fine time that year double dating and enjoying the freedom of our own abode. Kern had coincidentally applied to WHOI for a summer position and had been accepted as a general summer student and was committed to a long cruise to Norway on the research vessel, RV Chain.

In my application to the GFD fellowship summer program I stressed my strong interest and commitment to the study of fluid dynamics, but perhaps in an excess of candor I also admitted that I thought it was unlikely I would ever become an oceanographer. In that second summer of the program, with GFD in its infancy, the program was in an evangelical stage. Hoping to attract students who had not made a prior commitment to oceanography, a number of students with no previous knowledge of any of the geophysical sciences were admitted. I was one of the nine students admitted in that year's course.

It seemed like an exciting and pleasurable prospect and in the spring of 1960, Kern and I drove down in his car to take a look at Woods Hole. The beauty of the village of Woods Hole impressed me as it was then, and especially the view overlooking Little Harbor, at the entrance to the village. It seemed idyllic and to this day that view still provides me, after 57 years, deep pleasure and contentment.

That fifth year at MIT ended with a sense of great anticipation and excitement. One Sunday evening after Kern and I had gone to a Boston restaurant for a good dinner together, and after a year in which we had each been dating young women from Tufts (they were attending the coed part of Tufts, then called Jackson College), he invited me to visit him in the period between the end of the Spring semester and the beginning of our summer in Woods Hole. I was intrigued by the invitation since his mother lived in the San Francisco area but I had to decline the invitation for the simple reason that I certainly could not afford the cost of the plane ticket to California. Later that evening, Kern's mother, who I knew was quite wealthy, called and told me to not worry about the plane ticket and that they were looking forward to seeing me in California and not to mention the question of money again. I was overwhelmed by the generosity and for a very young man (22) who had never been west of Philadelphia it sounded like an adventure not to be missed. So, Kern and I spent a fine week or so in the Bay area, staying in his mother's house in Mill Valley and their cottage in Nicasio and enjoying the pleasures of Northern California and the city of San Francisco. It also gave me the opportunity to deepen my friendship with one of my dormitory friends and roommates, Stein Weissenberger, whose family also lived in the city of San Francisco.

So with this exciting vacation finished, I was ready to see what Woods Hole and GFD had to offer. My parents had scraped enough money together to give me a VW bug (a car with no gas gauge) as a graduation present. They had been delighted when the Lockheed Aircraft Corporation had given me a full scholarship for that fifth year and the VW was in the nature of a reward. I drove the VW down from the apartment in Cambridge to see what this field of GFD was all about.

All of my fellow GFD students were male (how times have changed for the better!) and we were all lodged in a dormitory on the WHOI campus in Woods Hole. The dormitory was nicknamed the Barn and it was rather rough and ready. There were three small apartments on the ground floor for married couples and a series of double rooms on the second floor for us bachelors. I checked in with the housing officer of the Institution to find where I would be staying. It turned out the woman in charge of housing was Harriet Rossby, the widow of the famous meteorologist and oceanographer, Carl Gustav Rossby. I had no idea who Carl Rossby was, or any notion of his importance to the field, but I was immediately taken with Harriet's vivacity and charm. She seemed quite motherly to me and was in her early fifties then.

I found my room, unpacked my few belongings and discovered that my roommate for the summer was a biologist, Walter Eckhart. He was really interested in molecular biology but thought it would be a good idea to spend a summer studying "critters" as a balance to what his future place would be in biology. He later became a lab director at the Salk Institute in La Jolla and we became, that first summer, and have remained, very good friends.

I remember as we were all unpacking in our rooms, other students were striding up and down the second floor hallway introducing themselves. One chap, who turned out to be Bob Blandford, introduced himself and said right off that his plan for the summer was to study the physics of the ocean thermocline. He was especially interested, he said, in meeting and working with Henry Stommel. These words meant absolutely nothing to me. I had not the foggiest idea what the ocean's thermocline was or who Henry Stommel was. That spring Møllo-Christensen was teaching a special topics course in fluid dynamics, which, naturally, I took. After he learned that I had been accepted in the GFD summer program he announced in class that he was going to prepare me for the GFD course by teaching us about the Coriolis force. We had seen it in our engineering courses on aircraft dynamics where the dynamical analysis of an aircraft in motion was done from a frame fixed to the moving and rotating aircraft but its importance and significance escaped me then and did not sink in until quite a bit later. He also recommended that I read Stommel's monograph, " The Gulf Stream" and, again, I gained a sense of the flavor of oceanography as it was at that time without obtaining a useful understanding of the dynamical framework. So it was with a mixture of anticipation and trepidation that I showed up for the first day's lesson.

In 1960 the GFD course had not yet moved to its own turf at Walsh Cottage, a small wooden structure that was the gardener's cottage on the original Woods Hole estate where the Oceanographic has many of its buildings. The course that summer was held instead in the Bigelow building. In those days there were two main buildings of the Oceanographic Institution. The oldest, the Bigelow building, and the newer one, the Smith building, were the two principal buildings of the Institution. In those days they were simply called "the old building", i.e. the Bigelow building and “the new” building, i.e. the Smith building. The lecture room was a small room in the “old building” and that was also where our offices were.

There were nine of my fellow students. The ones I remember best from that year were, besides myself, Bob Blandford, Bill Blumen, Bill Holland, Dick Lindzen, Gus Furomoto and three others who I won’t name. Two of those three decamped before the end of the summer without presenting a final research project report that was, and continues to be, the focus of the summer for the students, while the third unnamed individual attempted a flight, was stopped and gave a thoroughly undistinguished final presentation. The reader who has some background in meteorology and/or oceanography will surely recognize most of those I have named for their contributions to the field. In addition to the admitted students there were other students in residence in a more informal summer student program, like my friend Kern, or students who had made individual arrangements to be at WHOI that summer. Among those was Bruce Taft, a slightly older student who became a well - known observational oceanographer and who became a good friend. Also in attendance in 1960 was Andy Ingersoll who later became an important figure in the dynamics of the atmospheres of the planets of the solar system and was the star of our softball team.

The permanent staff of the GFD program that summer consisted of Melvin Stern, who became my research project advisor, Willem Malkus, one of the founders of the program, Arnold Aarons, at that time a faculty member in the physics department of Amherst who worked with Stommel, and, of course, Stommel himself. George Veronis a co-founder of the GFD program with Malkus, was not present that summer.

Other participants in this stellar cast included Ed Spiegel, a bright and witty astrophysicist from NYU, Harry Wexler, then an important figure in the Weather Bureau (chief of the Scientific Services division, i.e. the Bureau's chief scientist), and Fred Bishopp, then an assistant professor in Brown's mathematics department who had done fine work in thermal convection with Chandrasekhar at the University of Chicago, and Doug Lilly, an expert on violent storms. Joanne Malkus, previously Joanne Starr, subsequently Joanne Simpson, also participated and gave some lectures on the dynamics of clouds, a field to which she had made fundamental contributions.

The central figure for me at least for the first two weeks of the summer course was Lou Howard. Lou was a professor in the MIT mathematics department and he gave 10 beautiful lectures the first two weeks and his lectures were simply brilliant. He gave us the fluid dynamical foundation for rotating flows, with emphasis on wave motions, instability principles and boundary layer methods. It was the first time I saw boundary layers described in a clear mathematical way in terms of inner and outer limits of the governing equations. Prior to that, the only approach to boundary layer dynamics was the rather heuristic approach of Prandtl as described in Schlichting's text on the subject. I had a somewhat stronger background in fluid dynamics than the other students in the course and I found all the material very accessible, very fascinating and the application to the oceans intriguing. I added Lou to my personal Pantheon of heroes and felt as if I learned a tremendous amount from him that summer. I was particularly impressed one day to overhear Harry Wexler, who shared an office with the students, decline to attend a Washington meeting because he didn't want to miss a single lecture by Lou Howard.

Willem Malkus had been working with Veronis on one of the earliest weakly nonlinear theories of convection. I later was able to appreciate the creative character of their work but Willem was never a clear lecturer and we were all lost in his presentation. When he went further, beyond weakly nonlinear theory, to lecture on his ideas of *turbulent* convection the fog grew even thicker. He had introduced the idea that turbulent convection would somehow organize itself to maximize the vertical heat transport by the turbulent motion but his idea only became clear to me when Lou Howard took up the problem a couple of years later. Although Willem's idea turned out to not be right, he gave a mighty impulse to turbulence theory with the idea, which was ingenious. Indeed, in his career he had many very creative ideas that proved to be not quite right but they were so stimulating that his wrong ideas often advanced the field much further that other people's ideas that were correct but routine.

Henry Stommel gave a set of lectures on a variety of oceanographic topics ranging from salt fingers to Sverdrup-style ocean circulations driven by precipitation and evaporation. The latter material was used to introduce the notion of western boundary currents, which Lou had also touched on. Finally, Hank talked about the theory of the thermocline in what we would now call a 1½ layer model and then approached the general continuous problem in a model similar to the one he developed with Alan Robinson. It led to a very difficult partial differential equation that admitted only very special solutions and I think for most of the students in the audience it was pretty impenetrable material.

Melvin Stern gave lectures on baroclinic instability and the atmospheric general circulation. After deriving the quasi-geostrophic version of the instability problem, he considered the problem of a basic flow with uniform vertical shear, and ignoring the Earth's sphericity, i.e. Eady's model, derived the solution.

It is interesting for me to review these subjects of that summer because, by and large, those problems became the focus of my career's research. Baroclinic instability, boundary layer theory, ocean circulation and the theory of the thermocline do a pretty fair job of describing the physical problems I would take up in my own work and there they were, all displayed in that one short summer. It should be clear that it was more than I could possibly digest at the time. In spite of, or perhaps because of, the unclear nature of these subjects to me then, I found the whole subject fascinating and so much more *physical* than the fluid mechanics I had studied in aeronautical engineering.

I remember after I started my summer research problem and was working with Melvin Stern as my advisor, that at one point he started talking about the motion of fluid elements and he held his hand up and gestured with his thumb and index finger as if he were holding that fluid element and could see the forces acting on it. That was a considerably different emotional relationship with fluids that I had been used to in aeronautical engineering. After my junior year at MIT I wangled a summer job at the Naval Supersonic wind tunnel on the MIT campus. One of my tasks that summer was running a sequence of experiments on hypersonic flow past a coneshaped metal body. The temperatures were so high and the speed so great that visualization of the flow was indirect obtained through a Schlieren system that used the small changes of density in the flow to see the shock waves around the cone. The air-flow itself was separated from the viewer by thick walls. The fluid seemed distant and rather abstract and the idea of holding a piece of fluid in your fingers, as Melvin was doing, restored fluid mechanics to the physics of experience to me. I was simply charmed.

The problem I worked on that summer with Melvin was related to the recently discovered phenomenon of salt fingers, or more generally the instability of systems whose density is determined by two fluid attributes. In the case of salt fingers, it was the simultaneous effects of temperature and salt. The presence of salt increases the density while an increase in temperature reduces the density. In certain cases where the two effects are present a fluid that seems to be stably stratified, with light fluid resting above heavy fluid, can become unstable and begin to convect. This can happen, for example when warm salty water rests over cold fresh water if the temperature reduction of the upper fluid’s density exceeds the increase in its density due to the added salt. The process becomes unstable only because of the much weaker ability of the salt to diffuse in the fluid compared to the temperature. The physics of so-called *double diffusion* processes was revealed by some early heuristic work by Stommel and put on a firm fluid mechanical foundation by Melvin Stern’s analytical analysis of the instability. Melvin showed me a simple experiment in which the convection manifests itself in tubes, or fingers of relatively salty water sinking side by side with fingers of relatively fresh water rising. What was of particular interest was the tendency of the fingers to appear to themselves become unstable and produce an apparent buckling mode.

With Melvin's help I formulated a simple model of the instability; probably too simple because we assumed the solution would be periodic in the horizontal plane with the spatial period and its harmonics, of the original fingers themselves. A general Floquet term should have been included. However, using a Fourier expansion in the horizontal and a ruthless truncation of the Fourier series to just its first two terms I was able to obtain an equation for the growth rate of this secondary instability. A result in which I have no confidence now but it did represent something new for me then, i.e. the working through of a physical model to completion for all its faults. In fact, on the basis of this minor success Melvin invited me to accompany a small group of scientists from WHOI (Stern, Stommel, Aarons) to a meeting in Baltimore of the American Physical Society's annual meeting on Fluid Dynamics where, for the first time, a session on GFD would be presented. I was ecstatic. Indeed, I was thrilled by my scientific experience that summer and drawn instinctively to the people I had interacted with scientifically.

I would be remiss though to not describe the social side of that summer because it was wonderful. As I mentioned, I had a good friend in my roommate Walter Eckhart and we both rather quickly found female companionship in the numerous, mostly female, students taking summer courses at the Marine Biological Laboratory (MBL) in Woods Hole. I was quite fond of the girl I had met that summer and we were romantically involved in that idyllic environment until, at the end, we weren't.

One day this very nice young woman took me to visit a family friend of hers, Otto Loewi who had won the Noble prize in physiology for discovering the role of a chemical agent that passed signals across synapses. He was a delightful person. Although quite old (he died the next year at age 87) he was full of marvel about the informal nature of science in the US and especially in Woods Hole. He gleefully told us about the time he was walking along in Woods Hole with a scientific acquaintance when they passed another fellow and Loewi's companion greeted him with a hearty, " Hi, Frank!" When asked the surname of the man they had just passed, Loewi's companion replied, " I don't know him well enough to know his last name". For someone raised in the atmosphere of prewar Austria it was astonishing and he laughed heartily at the memory.

Everyone took meals at the MBL mess hall, which in those days occupied a large, old wooden structure across the street from the aquarium belonging to the US Fisheries, which also had labs in Woods Hole. So, all the meals, and especially dinner were jolly social events, made jollier by the end of the summer when we discovered we could bring a bottle of wine to the table to supplement the cafeteria fare.

From a culinary point view point the summer also had special significance for me. I had discovered in my early teens that I was strongly allergic to eating fish. My experience made me believe I was allergic to all fish. In 1960 scallops were quite inexpensive and they were often on the menu of the meal program we were signed up for at the MBL mess hall. So each time scallops were offered, I would demur and explain my allergy and someone in the cafeteria would make a ham sandwich for me. I soon tired of ham sandwiches.

Since I was brought up in a kosher home, I had never eaten shellfish and I assumed that I was as allergic to shellfish as I was to tuna, salmon, etc. So, one day, since the allergy in those days was very uncomfortable but not as severe as it became later, I hazarded eating one scallop when I could not face another ham sandwich. I discovered two important things. First, scallops are delicious. Second, I was clearly not allergic to them. Like any good scientist I generalized and developed the hypothesis that perhaps I was not allergic to shellfish in general. Gingerly tests of shrimp, lobster and mussels revealed the correctness of the hypothesis. To this day I still feel as if I am getting away with something dangerous whenever I eat a dish, any dish, of shellfish.

I mentioned my sexual inhibitions about complete intercourse and they continued that summer. The very nice girl that I was seeing that summer made it clear one romantic night on the beach by Nobska Lighthouse that she was ready to go “all the way”. I held back and I believe that brought our relationship to an end. In each of these experiences I believe now it was more than prudence that held me back. I think I was not emotionally mature enough or emotionally serious enough about the women involved in order to take that step of complete intimacy.

When the summer ended, I returned to MIT and the aero department and entered the Ph.D. program. Although I had been strongly attracted by my experience with GFD, I felt it would be more prudent and practical to finish with my aero degree before gradually switching over to GFD full time. This was not a fully thought out plan and clearly reflected a lot of mental inertia but it seemed persuasive at the time.

I started the autumn semester at MIT with a full set of courses and my next challenge was going to be the general exams that were required before I could be fully entered as a doctoral candidate. I expected no difficulty on the basis of my easy time with the earlier qualifying exams.

At the same time, I applied, for the adventure of it, for to the Fulbright Foundation for a fellowship for the following year to study in Sweden. I had been strongly attracted by the Swedish political model and especially its generous social welfare structure and thought it would be interesting to observe it first hand. A host professor of aerodynamics was suggested to me and I made my application. The coincidence of what followed still strikes me as remarkable but I will get to that later.

After the semester started, I received an announcement that the first Woods Hole/MIT GFD seminar of the year would be held in September at MIT in the Sloan building. These seminars, started a few years before, were twice a month events and their location alternated between MIT and Woods Hole but occasionally occurred at other places like Harvard. The first that year was at MIT and so was convenient for me, and with my warm feelings about the summer before, I decided to attend.

There was a brief period before the seminar where people from MIT and Woods Hole gathered for coffee in the MIT Faculty Club lounge in the Sloan building where the seminar would be given. I remember quite clearly that I was sitting on a sofa next to Lou Howard and we were discussing some fluid dynamical issue when a completely dream-like event happened.

I saw entering the lounge one of my favorite aero professors, Holt Ashley, accompanied by Harriet Rossby. This seemed like an incongruous combination of two quite disparate parts of my life as usually only occurs in the unreality of dreams. That was not all. Between the two of them was a third person, a young woman of exceptional beauty who became the cynosure of all eyes. I gulped and turned to Lou and asked him if he knew who she was. “Ah”, said Lou, “She’s Carin Rossby. She’s something, ain’t she”?

I then remembered an incident from earlier in the summer. The wonderful secretary for the GFD program was Mary Thayer. She had come out of retirement to handle the secretarial tasks of the program but also provided a motherly figure for the students. Blue haired, chatty, and very spirited, I was very fond of her and she returned the feeling. I became a pet of hers that summer. Later in the summer she invited me to lunch at a private club in Falmouth, the Nimrod Club (which later became the public Nimrod restaurant, now, alas, defunct) along with Harriet Rossby, her close friend. Sometime during the lunch, she mentioned to me that Harriet had a daughter in Sweden who was an actress. I took in that information in the same way any young boy from Paterson, NJ would absorb the information that there were beautiful gardens at Versailles; possibly interesting but entirely personally irrelevant. Well, now here the beautiful garden had crossed the Atlantic.

Then in what seemed like a continuing dream sequence, Ashley left the two Rossby women and came over to where I was sitting and said that he, Harriet and

Carin were going to have dinner at the Faculty Club after the seminar and did I want to join them? You can guess my answer, can't you?

I had a wonderful time at dinner. Carin's intelligence matched her beauty and our conversation quickly became stimulating. I learned that Carin was in the US to look for acting work and would be travelling around the country on a job search. She had recently finished a major role in the Disney film, "Hans Brinker and the Silver Skates" in which she played Hans' sister. Over the years it was always fascinating to hear her describe her acting work in films and on stage. However, here in the states her job search came up empty and when she returned for a visit with her mother, I called her up and we had a date to go to a concert in Falmouth and to a small party afterwards at Woods Hole. I was bewitched and we had a very romantic evening.

I found out later that Carin was engaged to be married when she arrived in the U.S. but I did not notice the ring since Swedish custom puts it on the right and not the left hand. One evening we were dining together in an Italian restaurant in Boston and I was talking about my work and how important it was to me. I mentioned that I got pleasure from the intellectual depth of doing science and said, in all innocence, that I couldn't imagine doing something like being a dentist and spending my life looking into other peoples' mouths. What I did not know was that her fiancé was a dentist and that at that moment, as she told me later, she surreptitiously slipped the engagement ring off her finger and mentally broke her engagement then and there. Our relationship became ever deeper and I experienced complete sexual love for the first time one late evening on Nobska beach in the moonlight. I was swept by passion and prudence was given the boot. The following January we were married.

We had a small apartment in on Brighton Avenue in Allston, just where Brighton Avenue intersects Commonwealth Ave. We had a car dealership across the street as a neighbor and it was not a particularly beautiful location. But I was happy as I continued in my first year of the PhD in the Aeronautics Department. Carin took a job in an insurance company in Boston to help pay the bills and a Ford Foundation Fellowship of $3,000 was my contribution to the family income. We were poor, content and felt secure. We were very much in love. Although money was very tight an occasional congratulatory check from an extended family member would arrive as a delayed wedding present. No matter how low a cash reserve we had we always used the newly arrived check to go out to dinner. Things looked sunny. Then came the shock.

Chapter 4

Shock and Recovery 1961-1962

The general exams were scheduled for the beginning of the second semester and I had little anxiety about them. The form was a take-home exam over a weekend. When I received the exam, the questions threw me for a loop. I don't recall them all but in one case there was the description of an experiment in pipe flow turbulence and I was asked to interpret the results. Another question had to do with underwater cavitation from propellers. To say that my answers were inadequate would be kind. I had the ominous feeling from the start that I was going to fail the exam and that is the only part of the weekend that I got right. I was shaken to the core. I was embarrassed and I believe the faculty was too. Coincident with the official news that I had flunked the exam was the receipt of a letter informing me that I had won a Fulbright Fellowship for the following year. The universe is notorious for its love of irony.

A rather newly arrived faculty member, Martin Landahl, gave me the bad news. Although I did not know him well, I admired him as a teacher and I was embarrassed to have let him down as well as the other faculty who had been so helpful to me over the years. He suggested that since I was going to be away in Sweden the following year, they would make an exception for me and allow me to retake a new general exam in a month so I could "get it out of the way before I left". I had an ominous feeling about that and sure enough, when given the sheet containing the questions for the new exam a month later I immediately realized that there was no question but that I was going to flunk that one too. I was right again.

Now I was really shattered. At first, and for some time afterwards, I tried to rationalize it by telling myself that these failures were really just a sign that engineering was not my cup of tea. But when I was more honest with myself, I realized that these failures were a reflection of the fact that I had resisted going very deeply into any research material in my efforts to develop my physics and math background. Not much of a consolation to realize that and the responsibility was purely mine. The question was what to do next.

I thought in terms of making a fresh start and the attraction that GFD had for me seemed to hold out a possible path out of my misery. The summer at Woods Hole had been so successful and the subject matter seemed so natural to me that my thoughts naturally moved in that direction. What I am about to relate seems unbelievable now in its informality but the academic world seemed a smaller place then and I am fairly sure my memory is correct.

I had met Jule Charney socially, perhaps at Harriet Rossby's apartment in Woods Hole, and I had a vague idea that he would be a good person to work with. So, I went to him and asked whether when I came back to MIT after my year in Sweden, I could switch to the Meteorology Department at MIT and work with him. He just said "yes" and the business was done. He loaded me up with books and articles he thought I should read in my year away and went to the effort of finding a place for me in Stockholm at the International Institute of Meteorology that Rossby had set up in the 50's and which was headed then in 1961, after Rossby's death, by Bert Bolin, later better known for his important work on the ICPP reports.

Charney could not have been more generous and tactful. Looking back on these events it is clear to me that Jule must have inquired about me to the people in the Aero department and been aware of my problem with the exams but he never mentioned it and it apparently did not greatly trouble him. So, with this change in direction and his guidance I felt I could spend the academic year 1961-1962 in Sweden studying meteorology and oceanography, largely on my own, and return to MIT to face what I needed to do for a Ph.D. in that field. To what extent my marriage to Carin Rossby smoothed the way for me is something I will never know. I do know that I never consciously took advantage of it.

I felt I needed to tell people in the aero department that I would not come back to the department after the year away and I went, as seemed most natural, to tell Møllo-Christensen. "Erik", I said, "next year when I come back from Sweden, I am going to enroll in the Meteorology Department and not the Aeronautical Engineering Department". He astonished by saying, "Me too!" He had himself made the plan to switch that year to the Meteorology Department and had a long and productive career doing oceanography. The informality of change in those days is hard to believe now.

Well, now it was up to me to make the transformation work. Carin and I prepared as best we could for the year in Stockholm. Her family still had their apartment in the city on Ehrensvärdsgatan in a lovely neighborhood by the Nor Mälarstrand on the main island of the city. The meteorological institute was on

Lindhagengatan, in those days about a 20-minute walk from the apartment. I attempted to study Swedish before we left but I had a hard time learning to speak the language. On my way to the American Embassy my first day in Stockholm in order to sign the papers for the Fulbright I took a trolley to a predetermined stop and after descending, asked a passerby for directions in a carefully practiced attempt in Swedish. The woman looked a bit quizzically at me for a moment and answered my question in perfect English. My Swedish did improve but I did tend to rely linguistically on Carin a lot and most people in the Institute spoke English perfectly and I was concentrating on learning Meteorology more than Swedish.

Our trip to Sweden was the very first time I had been out of the United States. We booked passage from New York to Le Havre on the French Line vessel the Liberte`. Ocean liners were still then a standard way to cross the Atlantic. It was really our honeymoon and the crossing, in tourist class, of course, was delightful. The food was excellent and abundant. Wine was served with every meal. I learned about Camembert cheese. The waiters took particular delight in serving Carin and would come around with trays of second helpings saying “Good for you, good for me”. The tourist class cocktail lounge was wood paneled and a stringed trio accompanied a pre-prandial sherry each evening.

We spent the better part of a week in Paris in a small hotel near the Louvre (5 rue des Bons Enfants) where each morning we were awakened by a cheery chambermaid bringing coffee and croissants. We visited Versailles and the boy from Paterson did see the gardens after all. We had a splendid time in Paris, ate well and I exercised what little high-school French still remained available. We had a picnic in the Bois de Boulogne but were disconcerted to notice local Parisian men watching us furtively from behind trees. I suppose they were hoping to see an amorous after lunch scene.

From Paris we moved on to Brussels for a couple of days, visited a museum, the beautiful central square and just off the square dined at a wonderful restaurant called L'Epaule di Mouton where a tuxedo-clad chef cooked the main dishes on a small stove in the center of the restaurant and gave a stiff reproof to a show-off American (not me!) who had the temerity to look over the chef's shoulder as he was cooking and announce knowingly to the others at his table; "This is real French cooking". The chef drew himself up to his not very great height and in the dignity of his rotundity snarled, "This is real *Belgian* cooking!" I understood then the origin of many European wars.

From Brussels we moved on to Brugge for a delightful day of art and countryside before pushing on to Germany. We visited Cologne, Bonn and Hamburg. There were many things of interest to see but what was most interesting to me was how much the devastation from the war that ended 16 years before was still evident. From Hamburg we went by ferry and train to Stockholm. By this time, I was exhausted and dealing with sensory overload. We were met by Carin's brother, Tom, and he took us to dinner in Stockholm. Rising to leave the restaurant I was suddenly overtaken by a great sense of fatigue and fainted. This was not an auspicious start to my year in Sweden.

In fact, although I was treated with great courtesy (on the whole) throughout that year I never felt completely comfortable in Stockholm. In 1961, especially in

Stockholm, the relations between people, unless they were truly intimate, were very formal. I recall that when I arrived in the Institute of Meteorology, a person in the administration came to my small office to ask me what my title was. Mail could not be delivered without a proper title for the addressed person. Indeed, in the city's phone book in those days the person was listed alphabetically by last name and *by title* and only then by given name. If you were trying to look up the phone number of someone named Carlson, for example, and you did not know his or her title you were out of luck.

The Fulbright administration needed my address and title (I forget which title they finally settled on as appropriate) to send me a small check each month, and there is a little story involved there. When I received the notice of my Fulbright award, I was informed that I would be getting 800 kronor (crowns) a month. In my naiveté, not knowing the dollar/kronor exchange rate, I assumed it was a considerable sum or at least sufficient for Carin and me to live on. She knew right away it was insufficient but was reluctant to share the bad news with me. What she supposed would happen when I found out is still a mystery. During the summer, while in Woods Hole before our trip I had met Pierre Welander a Swede who acted against the stereotype. He was exuberant, open and full of fun. He was also an excellent scientist but I only appreciated this much later. He also had the habit of asking me how much money I was making every time I changed jobs. So, when he dropped into my cubicle in Stockholm later in the year one of the first things he asked was what the Fulbright stipend was. When I told him, he reacted with astonishment; "You and Carin will starve!" I was nonplussed. He immediately said he would take care of it. He had a contract from the US Office of Naval Research (ONR) and he said he would pay me an additional 800 kronor from that. He then wrote 2 letters. The first to ONR saying he had hired me to do some work on the grant. The second he wrote to the Fulbright Foundation saying that although I was receiving this money it was in the form of a scholarship and that I had no obligations to do anything for him. We survived on the double-dealing and I hate to think what would have happened otherwise.

As I remarked earlier, Sweden was not entirely a comfortable place for me. Many people had very negative feelings about the US and this was, remember, before the US involvement in Vietnam. When we first arrived in Stockholm Carin took me to visit her uncle, Carl Rossby's brother, and his wife Margarita. As we entered their apartment her aunt swept her into her arms and said, "Thank heavens you are back in a country with culture!" That attitude was widespread. There was also great disdain for the problem of racism in the US and I was told in no uncertain terms that nothing like that could ever happen in Sweden. In fact, when one of Carin's girlfriends invited us to dinner, the friend's boyfriend started the after-dinner conversation by asking me whether I had left the US because I was fed up or was I one of the "dirty nationalists". The conversation grew a bit heated. Carin later told me with a chuckle that the same girl asked her if I wasn't a bit on the homosexual side. When Carin, astonished, asked her why she thought so, she replied that she was confused that I had not yet made a pass at her.

I went to Sweden more than willing to admit the many flaws in American society but there was much in America that I admire and am proud of, and while I initially held my tongue, eventually I could not restrain myself. I remember one conversation with a Swede who said he considered the US deeply provincial. I replied that being from an area near New York City, that Stockholm struck me just the same way. He was speechless.

However, my main focus that year was going deeply into the fields of meteorology and oceanography. I started with the simplest texts I could lay my hands on and worked my way into more sophisticated material. A review article by Arnt Eliassen was particularly useful. I read, read and read. I discussed what I read with some of the friendly staff at the Institute. Knowing that I would have to take a rather broad general exam in the Meteorology Department when I returned, I even read a text on cloud physics and another on atmospheric radiation.

Sometime during that year Bolin asked me to look at an article by Alwyn Burger that had been sent to Tellus that was later published elsewhere and it involved Burger's work on Charney's model of baroclinic instability. The model's solution involved the solutions of the Confluent Hypergeometric Equation, something I had studied in my advanced math courses and finally that investment was paying off. Burger had shown that the curve of the shear versus wavelength that Charney had found and interpreted as a boundary between stable and unstable shears was just the boundary between contiguous unstable regions; a very surprising result. Burger's argument was indirect and subtle and it required close reading to follow it. When I told Bolin that I thought Burger's argument was correct he asked me if I would give a little seminar on the subject to the staff. I did, and I remember one of the meteorologists saying, almost in despair, “ Now we have to know about special functions too?!”

I was now doing what I had neglected to do before. That is, really immerse myself in problems in depth. Now it suddenly seemed natural and attractive. I even wrote a small paper on spectral properties of the 2-dimensional vorticity equation that was published in the journal, Tellus. So, this was a remarkable maturation year for me. In about the middle of that year Charney sent me a draft of a paper on the theorem that he and Melvin Stern had done on the what is now called the Charney – Stern criterion; a necessary condition for the instability of zonal flows, in the Quasi-Geostrophic limit. They restricted themselves to the case where the horizontal boundaries were coincident with isentropic surfaces, the so-called internal jet, and derived the strong condition that the quasi-geostrophic form of the gradient of potential vorticity needed to change sign for instability. It was an elegant and important result, even in that restricted case, and I found it quite exciting and familiar from my studies of classical shear flow instability. Part of the paper also contained a more systematic derivation of Quasi-Geostrophic dynamics than I had ever seen before. At Bolin’s suggestion I gave a small seminar at the Institute on that paper and I was beginning to feel as if I were gaining a foothold on at least one important part of the field.

I also developed a deeper interest in the stability problem and began to think of that area as a possible Ph.D. thesis subject but this was still a vague and unshaped thought.

As the year in Stockholm drew to a close, Carin and I decided to make our way back to the US via Italy. The idea of sun, good food and art was irresistible. About a month before I left Stockholm, Charles Keeling arrived at the Meteorological Institute on a Guggenheim Fellowship. Now famous for his long term and prescient measurements of CO_2 from an observatory on Mauna Loa in Hawaii, his work has been key in documenting the increasing amount of greenhouse gas in the atmosphere. At the time what impressed me most about Keeling (for I knew little about the greenhouse gas question) was his request to everyone at the Institute to speak to him only in Swedish as a way of helping him become fluent. The English skills of the staff, as I've mentioned, were so excellent that I spoke most of the time in English with them except when there were parties and they wanted to relax in their own language. Within a month Keeling's command of Swedish was many times better than mine, even with or perhaps because of, my half-Swedish wife. I vowed that if I were ever in that position again, I would follow his language example.

Carin and I travelled by train to Italy. I can't remember what accommodations we had on the lengthy trip but we were comfortable. I do remember that as we entered Italy and we were passing through fields of red poppies (it was June) we one day went to the dining car and were seated at a table with two English nuns. We couldn't have had two more delightful travelling companions. They kept encouraging us, and especially Carin, to whom they were very motherly, to eat lots of pasta. Giggling, they kept saying just how good it was in Italy and how eager they were to get there.

We visited Venice, Florence and briefly Rome. We sailed back from Naples on the Leonardo da Vinci and it was my first experience with Italian dysfunction. We had paid a bit extra to have a cabin with a private bathroom but the porter leading us to the cabin raced in and locked the door to the bathroom. When I protested, he replied with a shrug that another room, too, connected to the bathroom was to be occupied by a couple boarding in Gibraltar and we would have to see which of us had paid more. This obvious ploy for a bribe was so outrageous that it would have been funnier if I had been wealthy. As it was, I had to do some desk pounding at the purser's office to obtain the bathroom opened. No one ascended at Gibraltar.

My plans for that summer of 1962 again revolved around Woods Hole. I had applied again for the GFD program. I saw no reason not to, and without a Ph.D. I seemed still eligible. I can't understand now why I thought that reasonable but in any, case I was admitted. In the 57 years of the program to this point, I am pretty sure I am the only person who has been a student twice. I like to joke that perhaps they hoped that by the second chance I might finally "get it". In any case Carin and I were lodged in what was the former Carriage House (L'Hirondelle) on the Woods Hole campus (where the safety office is now). It was a delightful space just a short walk from Walsh Cottage where the program had its new and permanent residence. My advisor again that summer was Melvin Stern. I was delighted to work with him again and my familiarity with his paper with Charney and my intention to pursue some aspect of the baroclinic instability problem meshed perfectly with his interests.

The course lectures that summer were by Leon Mestel on astrophysical matters. I remember them vaguely as being interesting but I was focused intensely on finding a research topic that could be expanded to a thesis. The Charney-Stern theorem was restricted to a case where the horizontal boundaries of the domain were potential temperature surfaces and Melvin was wondering what would happen if that were relaxed and somehow, and here memory grows furtive, he made the suggestion that I look at the two-layer model with the addition of topography as a way into understanding the restriction on the theorem better. Somehow, he was under the impression that the topography would similarly prevent a strong theorem being proved for the two-layer model as was the case with temperature gradients on the horizontal boundary in the continuous model and that might give insight into the continuous model he and Jule had worked on. I think the rationale was that the boundary term depended on the difference between the slopes of the boundary and the slopes of the isopycnals at the boundary.

I got right to work and tried to see the problem from Melvin's point of view without success. The proof of a theorem in any case at first seemed hard to do. I write that now without understanding what the difficulty was because, one morning as I was shaving before breakfast, I suddenly saw in a flash how to prove the theorem with topography in the most general case. The result was clearly generalizable to an arbitrary number of layers. In fact, I was shortly to show that a similar theorem could be proven in the continuous case and the additional boundary terms could be interpreted as especially thin layers in a layer model, or as Francis Bretherton later posed it, as delta function sheets of potential vorticity. After proving the two-layer result I rushed to show it to Melvin. He quickly checked it against the result of the Phillips model; a case with no lateral shear where the critical shear needed for instability was simply proportional to the beta term. When the criterion from my theorem coincided with the Phillips result Melvin became very pleased and asked me to show him how I had gotten the result. I was terrifically pleased myself and considered that result the very first thing I had done of any real importance. To round out the result I was able to extend Howard's semi-circle theorem that gave bounds on the growth rate and speeds of unstable waves to the baroclinic problem although I made an error, which I corrected later when doing my thesis, when I included the beta effect. I was feeling pretty swell as the summer ended and I gave my report and I was looking forward to the move to Cambridge and MIT and really getting launched on my Ph.D. thesis. For the first time in a long time I felt confident that I was on my way. But not too confident, because I knew there were still general exams to confront at MIT in the Meteorology Department.

Chapter 5

My Ph.D. and Charney

At the end of the summer of 1962 Carin and I moved up to Boston where we had rented a three-room apartment on Newbury Street. Newbury Street is now rather fashionable but in those days, except for the few blocks closest to the Boston Public Garden, it was pretty seedy. A block closer to Massachusetts Avenue than our apartment, for example, was the Glass Hat club, an after hours joint, kept open by contributions to the local police. The residue, left by the clientele on the sidewalk, was evident each morning. It was, though, an easy walk from there, across the misnamed Harvard Bridge, to MIT. Carin's brother, Tom, and his wife took a similar apartment in the building next door. Tom, partially at my urging, had entered the Geophysics Department (in those days separate from the Meteorology Department) although both were in Building 24 at MIT.

The Meteorology Department was a stellar place in those days. Aside from Jule Charney, who was clearly the department superstar, there were Norman Phillips, Ed Lorenz, Victor Starr, Yoshi Ogura and a few other lesser lights. I shared a large office with several other students and two of Charney's technical assistants. My desk abutted, face to face, the desk of Jim Holton who was also Charney's student and we developed a strong friendship as a consequence.

Jule had his office one floor below ours. Across the hall from his office was an office with other grad students. I remember Conway Leovy and Bill Blumen there. Phillips and Ogura also shared that office and later Gene Birchfield joined them. That office also housed Jule's secretary. For those that knew Jule it will come as no surprise when I say that the secretary, Berit Larsen, was the beautiful wife of a Norwegian graduate student in another department. Jule tended to come in late each morning and Berit was sure to have his office neatened up for his arrival.

I started that September with a germ of an idea of what I wanted to do for a thesis. My first meeting, though, with Jule was something of a downer. He thought it would do me good to do some kind of observational project to familiarize myself with meteorological phenomena. My heart sank when he outlined the idea. My personal goal was to get out of graduate school and out of the subservient role of graduate student as soon as possible. So, I listened with dread as he discussed his plan. I went home that night frustrated to the point of tears and vowed that I would refuse to do it and if necessary, try to make my way with just my Master's degree from the Aero Department.

Next morning, I met with Jule and told him I had an idea for a thesis that I wanted to get started on right away. He listened attentively as I started to say that it was a problem on baroclinic instability. His face twisted into a painful grimace. "Not that old problem. There's nothing new there". I think he had in mind the, by then, classical problem he had worked on and the one more recently redone by Burger. I told him it was not that problem but that what I wanted to do is examine the role that horizontal shear of the current would play in the problem and in particular the possibility of simultaneously producing Reynolds stresses (momentum transport by eddies) and eddy heat transport in the two-layer model. As I spoke his face relaxed and he seemed to become more receptive. After I outlined my general plan, I asked him whether that would be sufficient material for a Ph.D. thesis. He then gave the best answer possible to my question. He said simply, “Yes, if the results are fascinating”. I couldn’t ask for a better answer and he became immensely supportive of my idea. The plan for an observational project disappeared and was never heard of again.

Nowadays, we ask students, as part of their general exam to enter candidacy to present and defend a formal thesis proposal. That conversation with Jule was the closest thing to a thesis proposal I submitted but I was on my way and it was my responsibility to make it work, i.e. to make the results fascinating.

That level of engagement was typical of Jule and he was beloved for it. If you had an idea of what you wanted to do, he would treat you like a colleague. He could be skeptical, demanding, questioning, but just as he would with any scientific colleague. I saw him act the same way with Holton.

As I got started, I first extended the work I had done with Melvin in preparation for an attack on the difficult instability problem with both vertical and horizontal shear. In particular I corrected and sharpened the semi-circle theorem I had proven the summer before and developed other bounds on the growth rate of disturbances in terms of the magnitude of the shears and also the product of the zonal flow velocity and the potential vorticity gradient. Indeed, I was able to show that the two-layer version of the Fjörtoft theorem could be obtained directly from the same integral condition that gave the first necessary condition for instability by taking the real part of the complex equation instead of the imaginary part. It also gave an upper bound on the growth rate in terms of that zonal velocity-potential vorticity gradient product. Bounds on the phase speed and general considerations of the possibly singular nature of the governing differential equations were developed. They led me to the intuitive notion that if the horizontal shear of the initial current was limited to the upper layer and was broad enough so that it did not possess an inflection point, a useful simplification would result. It seemed reasonable to me that the absence of a sign change of the potential vorticity *within* any single layer would allow only baroclinic instability even if Reynolds stresses were formed. I also inferred that if the basic zonal flow was limited to the upper layer the real part of the phase speed would need to be less than the minimum value of the current speed of the upper layer and greater than the constant value of the lower layer. In other words the governing equations would not possess a singularity. This allowed the solution to be obtained as a simple Taylor series.

My hope was that in the case of broad shears the horizontal shear would induce up gradient Reynolds stresses. Since there was, in this picture, no barotropic instability in the absence of a change in sign of the potential vorticity gradient within the layer (there is no theorem that proves this but it seemed a reasonable inference) the Reynolds stresses would have to be, on average, moving horizontal momentum *up gradient* sharpening the flow and producing a nascent Jet Stream. This behavior was first analyzed by Starr on the basis of observations obtained during WWII and was at first controversial. Normally, one expects eddies that are the result of instabilities to smooth out gradients, like stirring milk in coffee. In fact, the eddies in the atmosphere do just that on the horizontal temperature gradient as a result of baroclinic instability and they bring warm semi-tropical air northward, smoothing the temperature gradient and thus providing a more temperate mid-latitude climate.

One might expect the same smoothing on the momentum gradient but Starr found just the opposite in his observations. I recall a visit to the Meteorological Institute in Stockholm while I was there by Eric Palmen, a noted Finnish observational meteorologist. He pooh-poohed Starr's ideas saying they were impossible, crazy.

There were several efforts by others to find a mechanism to yield Starr's results. One thing was obvious, the spatial phase relation needed to get up gradient momentum flux could occur in *decaying* barotropic waves. So, there were suggestions, one by Charney, that it was a two-step process. The eddy was first born as a result of baroclinic instability releasing the store of potential energy in the initial state and then, in a second step, the horizontal shear led to eddy decay sending momentum up gradient.

I had the idea that perhaps it could all happen at once in a single mode of instability. That is, the instability of a current with lateral shear could be produced by baroclinic instability but its form, because of the lateral shear would yield, simultaneously, up gradient momentum flux. On the other hand, if the current were too narrow so that it contained a sign change of the potential vorticity gradient *within* a layer, barotropic instability would tend to broaden the jet. Thinking of it this way would suggest that there was a limit to how narrow the jet would become and thus an equilibrium value for the jet width.

The first step was to solve the problem of the broad jet. I chose a velocity profile for the upper layer with a smooth parabolic shape. I wrote the solution as a Taylor series about the channel center and then needed to find the values of complex phase speed that allowed the boundary conditions of no normal flow at the channel walls that I used to confine the flow. This led to a very complicated eigenvalue problem that could not be solved analytically.

It was Jule's style to leave his students to work independently but you were encouraged to come and talk whenever something interesting or difficult appeared in your work. So, one day I went down to his office and described what I had done, my thinking and what I needed to do next. He responded exactly as if we were working on this together, exactly as colleagues. He said it was clear that numerical work was needed and he was sure I didn't want to slow down my work to learn how to program the eigenvalue problem myself. He only asked me, looking at the complexity of the formulae I was trying to deal with, if I were sure of their correctness. I had checked them several times before showing them to Jule and with a, I hope, inaudible swallow, said, "sure". He then gave the numerical problem to one his technical assistants, a woman named Leola Odland who did the necessary programing in *machine language*, i.e. not a higher-level language like Fortran but something now inconceivable, in the language of the IBM 360 itself. My job then was to take the output in which the values of the determinant were printed out on a 2-dimensional grid in the space of real and imaginary parts of the phase speed, c, and find places where zeros of the real and imaginary parts of the two determinants simultaneously vanished yielding the eigenvalue, c. It was painstaking work, done by hand, and exactly the kind of work I had earlier tried to avoid but I was now so excited by the prospect of what the results might yield that I worked feverishly to get to the solutions.

I was thrilled to discover that instability could occur in the model as long as *somewhere* in the field the potential vorticity gradient changed sign from layer to layer and that the Reynolds stresses for the *growing* mode did move momentum up the gradient to sharpen the jet. There were other results in the thesis but that major one was probably the centerpiece along with the theorems.

I also answered a question that had been raised in a lunchtime conversation with Bolin earlier in Stockholm, namely how arbitrary initial conditions could be satisfied in the Eady model that had only two normal modes. The answer was that the initial conditions were satisfied by the addition of a continuous spectrum of stable waves and using Laplace transforms I showed in the last chapter of the thesis how that worked. Not only were things working out well but they were working out swiftly. It looked as if I would be ready to defend my thesis in June of 1963, nine months after entering the department. There was just one small detail. I still had to take the general exam. I found myself in the weird position of having a perfectly defensible thesis but I first had to take the general exam whose stated purpose was to see whether I was ready to do the independent research required for the Ph.D. When I mentioned the absurdity of this to Jule he reassured me that he was certain I would pass easily. When I asked why, if he were so certain, it was necessary for me to take the exam he merely shrugged. It brought up all the fears I had been living with since my exam failures in the Aeronautics Department.

A curious incident occurred before the time of the general exam. Jule's own thesis advisor of record, Jørgen Holmboe, was visiting MIT. He must have been in his early 60's in 1962 and had done excellent work in meteorology with a well-known shear flow instability problem carrying his name. He had left Norway and been an assistant professor at MIT in the 30's before moving on to UCLA. I was unfamiliar with him and his work at that time but I was shortly to have a strange introduction.

As I was working in my student office, one floor up from Jule, I received a phone call from Jule asking me to come down and to describe my work to "a few interested people". Well, there were a few rather important people there. Aside from Jule, I was happy to see both Melvin and Lou and then I saw what I took to be an elderly visitor, i.e. Holmboe. Jule just asked me to informally describe what I was working on to the assembled group. I have to say that since my work had been going so well, and the results so satisfying, I was more than happy to do so in that informal way in which one scientist will explain to another what they are up to. So, I started off by saying I was examining the general stability problem in the two-layer, quasi-geostrophic model and wrote down the generally well-known equations. In those equations there are two coupling constants, one for each layer that measures the degree of coupling between the two layers. These constants involve the magnitude of the Earth's rotation, the density difference between the layers (i.e. how stratified the model is) and the characteristic horizontal scale of the motion and the *mean* depths H_1 and H_2 of the layers. In the presence of different velocities in the two layers the interface between the two layers slightly tilts, this is the physical source of the potential energy for the instability. So, in principle, the layer thicknesses are not exactly constant. The tilt is small, though, of order of the small Rossby number. The equations are derived under the assumption that the Rossby number is asymptotically small and so it is consistent with the quasi-geostrophic approximation that the coupling constants *are* constant. It would, in fact, be inconsistent to include the variation when similarly small terms are ignored elsewhere.

However, as soon as I wrote the equations on the board in Jule's office and explained what each of the terms were, Holmboe just said curtly that the equations were wrong and not to be trusted because of the lack of spatial variation in the coupling constant. I tried several times to explain why the equations were valid but each time I ran up against a stone wall. It was evident that Holmboe considered *any* approximation to the equations as an error, whether the approximation was consistent or not. It struck me as strange given that Jule, himself; working with Holmboe, had done pretty much the same kind of thing in his derivation of the quasi-geostrophic equations, a prerequisite for any kind of progress in this difficult problem. It was not so much Holmboe's opinion that bothered me, erroneous as it was, it was more the unwillingness to engage in a suitably clarifying discussion.

Finally, he allowed me to describe my principal results to date but it was clear that he was holding to his opinion that they were probably based on a fallacious starting point. I was rather flabbergasted and was relieved when afterwards Melvin and Jule were as astonished at that behavior as I was. I gathered then that Jule's relations with Holmboe had not been easy and in Jule's later recollections with George Platzman he talked of Holmboe as a pedant. In fact, a short while later Jule confided in me that he thought of Rossby rather than Holmboe to be his mentor even though Holmboe was his Ph.D. advisor of record.

Pursuing this for a moment before returning to the chronology of my graduate work, it was during a drive down to Woods Hole for a GFD seminar with Jule that he ruminated aloud on his mentors.

It was just the two of us in the car and he started to reminisce, saying that he had always considered Rossby his mentor. In fact, it was Rossby who, in a letter, had recommended Jule to von Neumann to head the Numerical Weather Prediction Project at the Institute for Advanced Study in the '40's at Princeton.

Jule then went on to talk about the chain of mentoring that led to him. He pointed out that Rossby had worked under Vilhelm Bjerknes, who had studied under *his* father, Christian Bjerknes, who in turn had worked with Heinrich Hertz of electro-dynamic wave fame. Jule clearly felt inspired by this chain of mentors leading back to the heroic 19th century days of physics. As you might imagine I felt he was inviting me to join that chain and consider myself equally connected to that glorious past. This was the kind of emotional and intellectual experience I had been dreaming about for graduate school and here it was really happening. It was no wonder Jule Charney was so greatly esteemed by his students. Perhaps beloved, as I mentioned earlier, is the more accurate word.

Nevertheless; the chain of mentoring might have had an abrupt end right then, for while Jule was recounting these splendid ideas as he was driving towards Woods Hole, he made a wrong turn on Rte. 93 and took the off ramp leading to Logan airport. I suppose in those days when he was going on business to Washington so often, that turn off became automatic for him. When he realized his mistake, he took a quick look over his shoulder and backed up the ramp to regain the highway!

Fortunately, there were no other cars making that ramp turn off and we survived that nutty maneuver that might have snapped the mentor chain rather definitely. A close call it was.

My relationship with Jule became very close and he increasingly treated me as a colleague. Carin and I were frequently at his and his wife, Elinor's, house for dinner.

In the middle of that thesis year he had to leave MIT for a weeklong trip during the semester that he was teaching his course on dynamics of the atmosphere. He asked me if I would take the class for him for that week. I was, of course, deeply flattered that he thought me prepared to do that. I sat in on the course the week before he left to see where he had left off. Jule was a wonderful teacher but not a very good lecturer. He seemed in the middle of an argument to lose the thread and wander off in some other direction. If you already knew the material it was fascinating to see how his mind worked. If you were trying to learn it for the first time it must have been deeply frustrating for the students. When I was given the task of going through his personal papers after his premature death, I was astonished to find that his notes for the course were incredibly detailed and well organized. His problem as a lecturer, I think, is that he allowed himself to mull over the material again as he lectured and if a new thought came to him, he would allow himself to muse over the new direction of thought to the despair of the frequently confused student. I found the musing inspiring but the students were often frustrated.

I found that I deeply enjoyed teaching and the students responded enthusiastically. They asked me if I would review some material, especially on the derivation of Quasi-Geostrophy that Jule had lectured on and that seemed opaque to them. I was happy to do so since I had just derived those equations in my own fashion with a straightforward asymptotic approximation.

In fact, when Jule returned I continued to attend his classes because I enjoyed his lectures so much. At one point when he was lecturing on the Charney-Drazin theory of vertical propagation of planetary waves the issue arose about the fate of unstable waves that the theory did not cover. I made a remark in class about the effect of the complex phase speed on the wave behavior at large heights and this led to us working together on a small paper that we published in the Journal of Geophysical Research that year. I was really having a good time.

Sometime towards the middle of May, with my thesis results almost entirely in hand, I took the general exams. Another graduate student, John Young, took them with me at the same time. I don't remember the questions too clearly but I do recall the sense of relief I felt when I realized I was going to have no difficulty with the exam. I recall that one question Jule asked on the written exam was how to work out the spin-up time for an annulus of fluid with a sloping bottom. Greenspan and Howard had recently published their pioneering paper on the same problem with a flat bottom and this was only slightly different since for axially symmetric solutions the variable depth enters only parametrically.

Another part of the exam was take-home and this time I enjoyed it. I remember one question was posed by Lorenz and asked for the origin of the tropical Easterlies. I knew the desired answer in terms of the classical Hadley circulation problem but I couldn't resist referencing my calculation of the instability of the parabolic profile of zonal flow in the horizontal in the two-layer model. One of the consequences of the Reynolds stresses produced was to accelerate the eastward flow in the jet's center and produce westward flow on its edges and I pointed out that this would yield low latitude easterlies. This was a bit mischievous on my part but I enjoyed it and my whole attitude towards the exam could not have been more different than the exam I took just 2 years before in the Aeronautics Department; so much had changed. The take home exam was rendered a bit uncomfortable by the presence of the temporary home of the Berklee School of music on Newbury Street in a Brownstone next to our apartment. As I was doing the take home exam Herb Pomeroy was leading some student band in rehearsal rather late in the evening as I was dealing with Tropical Easterlies and the music was accompanied by Pomeroy's loud and upper register trumpet. Although a jazz fan I could have done without it at that moment. Pomeroy or not, I passed the exam and was ready to finish the draft of my thesis and defend it. The oral exam that was supposed to accompany the written exam was waived.

Nevertheless, it was not so simple. The morning *after* I was told that I had passed, I was showering in our little apartment on Newbury Street when the phone rang. Carin was at work and I managed, dripping, to reach the phone in time to hear the department secretary, the estimable Jane McNabb, say, "Good morning Joe; please come to the department right away. Victor Starr wants to give you an oral exam". I scooted across the Harvard Bridge as rapidly as possible wondering about this unexpected development since I had been told the oral exam had been waived for me because of my performance on the written exam. I entered Jane's office where she pointed to a vacant classroom where I awaited Starr.

Of course, I knew about Starr. He made that terrifically important contribution in analyzing high altitude data and coming up with the result that the turbulent atmosphere, on the scale of synoptic weather systems, was actually transporting momentum up the momentum gradient and strengthening the Jet Stream. This was the non-intuitive result that I was so proud to have explained in my thesis calculations. I also knew that his graduate students adored him and spoke of this shy man protectively.

He entered, sat down, introduced himself and proceeded to ask a question that immediately flummoxed me. I can't remember it in detail but it had something to do with the *shape* of the atmospheric envelope. As I was silently wracking my brains to find an answer, he began to talk in a bemused tone, "Most people believe…. But the real reason is…." and so answered his own question. Looking pleased and satisfied he asked another, to me impossible question, and the pattern repeated. After the third such essay, I realized I had to do nothing but only wait expectantly for his answer to his own question, nod knowingly and wait. After about three quarters of an hour of this routine he rose, declared himself satisfied and said that he had heard I was about to finish, that I had done some interesting work, and he wanted to meet me before I left the department. While relieved, I remember thinking that there were many more pleasant ways he could have asked to meet me, e.g. a morning meeting for coffee or an afternoon cocktail but I suppose his shyness required some academic excuse for a meeting.

As I was writing the draft of the thesis, Harvey Greenspan came nonchalantly into the office, sat down next to my desk and started by saying, " Jule tells me you are about to finish soon; would you like a job in the Math department". It must sound dreamlike to any modern student. Nowadays, the Ph.D. is usually followed by at least one post-doc year, if not more and only after a considerable time is it possible to obtain a full-time academic position. Here was an offer before I had even defended my thesis! I hasten to add that this was, at the time, not that unusual. Several of my fellow students obtained positions as Assistant Professors at excellent universities right out of graduate school. Holton went to the University of Washington's very fine Meteorology Department, for example. In my provincialism, I thought such positions represented a kind of exile from the world center, which I assumed was MIT. Also, now, looking back on that period I also realize with a shock that while preparing the thesis draft, I had made no plan for what would come afterwards. I can't imagine now what I expected to happen but what did happen was wonderful. The idea of staying on at MIT in the Math Department seemed so wonderful to me that I just accepted Harvey's offer on the spot and felt that my life had reached a point of great stability with a foundation for scientific growth that would be unequaled. In 1963, the year I finished my thesis, the Applied Math group at MIT included C.C. Lin (who was its head), Harvey Greenspan, Lou Howard, Dave Benney, Alar Toomre, and Norman Levinson. The interests of this group centered on fluid mechanics and, moreover, there was a serious interest in problems in geophysical and astrophysical areas, just perfect for someone with my background. The group also would include Victor Barcilon who became a collaborator with me on a number of scientific papers as well as becoming a very close friend.

The thesis defense took place in June. It consisted of a brief presentation of the results of my work, a few desultory questions from several of the faculty who had clearly not read the thesis, and a few substantial questions from people like Howard who had but who were clearly positive about the work. So, after all, it was pro forma. I was now Dr. Pedlosky nine months after entering the Meteorology department and without having taken a single formal course in meteorology.

I came home to tell Carin the good news, and with Harriet, we went out to celebrate at a restaurant on Charles Street, followed by a happy stroll through the Public Garden in Boston. After some years of anxiety, I felt happy and relaxed.

The position that I had at MIT that first year (1963-1964) was on the research staff and I can't remember the salary. However, I do remember the following year, when I was advanced to the faculty as an assistant professor, that my salary was $9,000 for nine months. I thought it was quite sufficient. I do remember Harvey telling me a few months later that if I stayed at MIT I could expect my salary as a full professor to grow to as much as $25,000 a year!

When I moved over to the Math department, I started to work on a subject not connected to my thesis work on instability. Instead, following some informal conversations with Charney, with whom I remained on close and friendly terms, I decided to examine the problem of Rossby normal modes in closed basins. I did not realize the Michael Longuet-Higgins in England had already worked out the linear problem but my work did go beyond his. With some help from Lou Howard I dealt with the initial value problem and showed the completeness of the modes and their unusual orthogonality principle that allowed an expansion in these unusual modes that although confined in a basin always had phase lines that propagated westward just like Rossby waves in an unbounded domain. Beyond that I used a perturbation expansion to calculate the nonlinear self-interaction of the modes, or in a more general case, the linear solution with friction forced by fluctuating winds, to obtain steady second order solutions that might add to the steady solutions produced by steady wind forcing.

George Veronis had earlier examined the nonlinear forced problem of the time dependent circulation in a closed basin but, following an approach used by Lorenz for an open annulus, expanded the solution in a series of trigonometric functions, i.e. a sine series, and appeared to obtain something like chaotic behavior for the amplitudes of the sine modes. Since the sine functions are not appropriate basis functions for the linear operator because of the beta term, such an expansion produces apparent transfers of energy between sine modes that appear to be nonlinear transfers but merely reflect that the real, linear normal modes are not sine functions. I knew I should reference Veronis' work but I did not want to be openly critical so I just mentioned that he had taken up the problem earlier, had found "interesting" results and then went ahead with my own analysis. This turned out to produce a problem for me, as I will explain later.

Nevertheless, things were going well at MIT for me. Harvey was doing brilliant work on the dynamics of rotating fluids; work that eventuated in his classic monograph, *The Theory of Rotating Fluids* so our interests overlapped. One of the problems he was interested in, which he needed to examine, was how spin-up for a homogeneous fluid worked in containers for which the container depth measured parallel to the rotation axis did not have closed contours, so called *geostrophic contours*. His existing theory worked perfectly for containers with closed geostrophic contours but not otherwise. A simple, perhaps the simplest example of such a container with no closed geostrophic contours, would be a spinning cylinder whose bottom was sliced at an angle so that the lines of constant depth ran from one side of the cylinder into the other where they were interrupted by the cylinder wall. At that time Raymond Hide had just arrived at MIT and opened his lab. Harvey explained the problem and Ray was immensely helpful in setting up a simple apparatus so we could see what happened to the spin-up problem. The container with a sloping bottom was filled with fluid and set into rotation until the fluid was rotating with the container's rotation rate and then we slightly increased the rate of rotation.

All Hell seemed to break loose. It looked as if strong eddies were separating from the "eastern[1]" boundary of the container although there were no sharp corners. Harvey was sure we were seeing boundary layer separation but I was not so sure and I thought it likely we were seeing Rossby waves. It turned out that the fundamental solution could be represented by a sum of linear Rossby normal modes of the container, of the type I had recently calculated in a rectangular domain, and in which each mode decayed with the scale-independent spin-up time of Howard and

[1] Looking down on the container it is customary, for dynamical reasons, to call the direction of the shallowest depth north. East is thus to right of that direction.

Greenspan while the sum of the modes represented the initial circulation. When we summed the modes, the resulting solution looked just like the lab experiment. Harvey got very enthusiastic about the sliced cylinder and we also used it to examine the generation of a "Gulf Stream" in the cylinder with the sliced bottom that for dynamical reasons mimicked the dynamical effect of the Earth's sphericity so that the shallow part of the cylinder was "north". Bob Beardsley turned the steady Gulf Stream model into an experimental and numerical thesis and used the power of the experiment and computer to go far beyond our analytical solution. This was wonderful start in my work with Harvey.

Again, that experimental work was also accomplished in Raymond Hide's lab. I should also mention that Raymond was also responsible for another life lesson for me. When he arrived at MIT and set up his lab, he was also plunged into a great deal of administrative work that limited his ability to carry forward his own research even though he was indirectly responsible for a great deal of the work in his lab like the work that Beardsley was doing. He found himself asked to give research seminars and was forced to discuss work he had reported before to the disappointment of his audience and, I am sure, to his own discomfort. I made a vow to myself to first of all avoid as much administrative work in the future as I could and second, if I did not have relatively new work to report, to keep my mouth closed.

Harvey, with whom I had become quite close, had a curious habit. Whenever I walked across the hall from my office to his to talk to him about some new idea or new direction in my work, his first response seemed, if not negative, then certainly not encouraging. At first this left me nonplussed. However, what usually happened was that the next day he would come into my office, smiling, and saying, "you know, that stuff you talked about yesterday is really interesting". I grew to learn not to be dismayed by the first reaction and wait for the next day's judgment.

I need to emphasize how generous and supportive Harvey was. When we had written the manuscript of the sliced cylinder phenomena, I assumed that the author page would read Greenspan and Pedlosky. We had contributed equally to the paper and in such circumstances the order of the authors' names should be alphabetical. Harvey insisted that the paper's authors be listed in the opposite order, Pedlosky and Greenspan. When I protested, Harvey merely said with finality, "You need this more than I do".

I also continued work on the baroclinic instability problem by examining a perturbation method to deal with extensions of the Eady problem but nothing truly exciting came out of that. I also started to work on the spin-up problem when the fluid was density stratified but that is something I will take up shortly.

My private life seemed happy and uneventful. Carin and I moved from our cramped and dark student apartment to a more spacious and pleasant apartment on Commonwealth Avenue (#48) just one block from the Public Garden. I remember being a bit concerned about the high rent ($175/month) but figured we could swing it. It was such a pleasant location and the corner apartment had windows on two sides and was bright and airy. Carin felt she could quit her job at the insurance company; the work itself was not interesting but she and I underestimated how important the social aspect of the work was for her. She tried to find theatre work in Boston but without success.

As seemed natural at that time we naturally felt she would soon be fully occupied with a growing family. That turned out to be an unexpected difficulty and several tests seemed to indicate some anatomical problem of Carin's and she became increasingly melancholy as a consequence. However initially it did not seem like a big problem and we were sure she would soon conceive.

There was another unexpected challenge. About a month after I joined the faculty and was teaching two sections of a "service" course, e.g. Advanced Mathematics for Engineers, as it was called those days, I received a letter in the interdepartmental mail asking me to come down to the Registrar's office to sign a Teacher's Loyalty Oath, a copy of which was included. I knew immediately that I would not sign but thought of it as a bureaucratic triviality that could be ignored. I think the basic reason I did not want to sign was that it seemed as an attempt at intimidation, warning teachers not to act in a manner that would make their loyalty questioned. Not signing led to a long battle, and happily, an eventual victory and the overturning of the law, but it took several years and an appeal to the Massachusetts Supreme Court. I am attaching to this memoir an appendix containing a talk I gave in 2004 describing the step-by-step, blow-by-blow, nature of that action. Suffice it to say here that it added a great deal of stress to my life during that period, stress that for other reasons I will describe, was certainly not needed. I will only say that during that period, 1964-1967, many colleagues supported me for which I remain very grateful. One of my proudest moments of that difficult time was when the case was heard in the Massachusetts Supreme Court. The notion that the action in the court that I observed from the back of the courtroom, an unknown, impecunious Assistant

Professor, could have such an influence on the law struck me as a vindication of the American system. Even before the decision came down, I was satisfied I had done the right thing. This 79 year-old man is happy the 26 year-old took the heat for his beliefs. Although, I was frightened of the possible consequences it might entail, again it was one of those things I did largely because I was frightened, i.e. frightened of what I would think of myself it I hadn't done it.

During that period, I continued my research and teaching. As a research topic I delved into the dynamics of rotating, stratified fluids essentially examining what the effect of a stable stratification would have on the many phenomena that had recently been examined by others, such as the spin-up problem studied by Greenspan and Howard and the steady, forced circulation in closed containers studied by Stewartson.

The key feature of all of those studies centered on the metamorphosis of the boundary layers on the vertical boundaries of the domain. In the case of a homogeneous, i.e. unstratified fluid, those boundary layers allowed a vertical mass flux to balance the vertical mass flux in the interior. It was easy to show that the vertical mass flux in the boundary layers was insufficient to close the mass balance for strong enough stratification, that is, when the restraint on the fluid motion by rotation was equal to the restraint on the vertical motion by the stratification. In the first problem I tried, the spin-up of a stratified fluid, I rather impulsively assumed this meant that the whole time -dependent circulation in the vertical plane would be choked off and the fluid would have to adjust its rotation rate by the much slower process of diffusion. This, of course, was a great blunder. The error was pointed out by Gösta Walin and it remains exhibit A in the museum of horrors I can visit at night to humiliate myself. The resolution of the problem was that fluid spins down in the usual spin-up time in regions near the lower boundary whose depth depends on rotation. In the parameter range I had been considering this includes most of the fluid. For larger stratification the problem is more as I had imagined but that was not the case I had considered. I have had to grow accustomed to the fact that when you are the first to do a hard problem you have to expect to be sometimes wrong!

At the same time, I began a long collaboration with Victor Barcilon on the steady dynamics of linear motion of rotating and stratified fluids. We worked out the complete morphology of the boundary layers as a function of the strength of the stratification and applied the results to several interesting problems. One important result was to demonstrate that while the boundary layers on the sidewalls of the containers were passive when the fluid was homogeneous, the effect of stratification was to give those layers an active role in determining the interior flow in the same way the boundary layers on the horizontal boundaries did for a homogeneous fluid. This sidewall effect altered the shear of interior flow such that the swirling velocity near the upper and lower boundaries were driven to the velocity of the boundary eliminating the need for bottom and top boundary layers and choking off the vertical circulation. This was what I erroneously thought would happen for the time dependent spin-up problem but which we could demonstrate really did happen for the steady flow problem. I remember that when I realized the active nature of the side-wall layers on altering the interior flow, something that does not happen for homogenous fluids, that I leaped out of my chair at home where I had been working and danced alone around the living room so struck with the elegance of the connected physics of all the elements of our new theory.

It was during this period that my private life took on a nightmarish quality. Carin, whom I loved dearly, became more and more melancholy until her depression became overwhelming. Partly, it seemed to be connected to our inability to conceive a child but it was also deeper than that. I discovered only later that she had suffered from depression in the period before we met but no one had mentioned that. Her depression grew deeper and more severe. In the Fall of 1966, I attended a conference in Sweden, on the island of Bornö. While I was there Carin visited her mother who had moved to Hawaii and was recovering from breast cancer. When Carin returned and we met in Boston she was much subdued. She told me she had suffered a seizure in Honolulu and it was the beginning of a struggle with epilepsy as well as depression. She began to talk often of suicide and was soon hospitalized. After a few months in the psychiatric ward at Mass General Hospital, during which time I was constantly hoping for an improvement and her return, she was moved to long term care at the Massachusetts Mental Health Center in a rather grim building near Huntington Avenue on the edge of the city. She was there for many months. I would visit each evening hoping to see a change for the better but in that time before there was much in the way of psychopharmacology to help, things did not improve. I felt lonely and sad and could not see what could happen in the future to change the situation. Carin's mental state became so perilous that the doctors in despair recommend electro shock therapy. Carin told me this one evening when she said she had told her doctor at the Center that she was thinking of breaking a glass medicine jar and eating the shards as a way of putting an end to her life. She was very low.

The next day I found her asleep on a couch in the common area where visitors were permitted. I gently woke her and she opened her eyes and gave me a big smile. It seemed like a miracle. She seemed like her old self. We were warned the effect of the shock therapy would wear off but the hope was that in the period of her positive attitude, the talk therapy could reach her and help. Even then it was clear she was fragile.

Part of the stress I felt, in addition to the emotional strain of seeing Carin so ill, were related to the financial consequences. Carin required round the clock nursing just to prevent her from harming herself, and I would come home each evening and find the mailbox stuffed full of bills from the private nurses. This was all contemporaneous with the Loyalty Oath battle so I was feeling very beset and very alone.

I spent a good deal of time visiting Carin in this long-term facility and got to know several of the patients. Most seemed aware of their conditions and on one level aware of their illness. I remember one pianist who was obsessed with working out the static forces on the furniture legs in the room. He was aware that this was a device to keep him from thinking of other things but it didn’t help relieve the obsession. Others seemed completely confused and hard to talk to. It was a very depressing environment.

At the same time, I received an invitation from the Geophysical Science Department at the University of Chicago to come and give a seminar. A few days before I was to fly out to Chicago, Norman Phillips mentioned over lunch at the MIT cafeteria in the Walker Memorial building that Chicago was going to make me an offer of a faculty appointment. My first reaction was that I was not at all interested in leaving MIT, where I was so happily ensconced to move to an unknown (to me) university as well as leaving behind the rather cozy city of Boston for Chicago.

I, in fact, had a wonderful time during my visit to the Geosciences Department at Chicago and enjoyed the meeting with the Faculty there. The Geosciences Department had just been formed as a merger of the Meteorology Department that had been set up by Rossby and the Geophysics Department. Nowadays that kind of unified Earth Science department is rather common but it was a particularly bold step at the time.

The department's Chairman then was Julian Goldsmith and we got along splendidly right away. As part of the entertainment I was offered that evening after my seminar, Julian, with some other faculty took me to the more elegant neighborhood of the North Side of Chicago. Where did he take me to make an impression on me? Not a nightclub or a show but to a second hand bookstore. This was Chicago! In fact I bought there a second hand copy of a translation of a collection of short stories by Giuseppe Lampedusa, noted for his novel, " The Leopard". I still treasure that collection which I have in my personal library twinned with its original in Italian.

In spite of the positive impression Chicago made on me, I was not ready to even contemplate a move. Although, as Norman Phillips had predicted, the University did make me an offer, I had to reply that as I was in the middle of a court case involving the Massachusetts Loyalty Oath Law, I felt that I was honor bound to not entertain a move during that process. So, it was quite possible that the appointment would go to another person but there was nothing I could do about it. I wasn't even sure if I *wanted* to think about moving.

During this difficult time, with Carin in the hospital, with financial worries mounting and my legal situation still unresolved, I leaned heavily on the friendship of my colleague Victor Barcilon. It was a pleasure working with him and it was a relief to have someone with whom I could unburden myself about my fears and sorrows. Harvey was also a great support and he was very clear-eyed and explicit about how he saw my situation. One day when I was talking about the sadness I felt with Carin's illness, I said something like, "I don't know what will happen in the long run". Harvey looked at me a moment and then gently said, " You are in the long run now!" That sympathetic realism made me understand better how things stood.

My friendship with Jule was also important to me during this time. Perhaps it was also to him. At some point that year, when Carin was again in the hospital and he had recently divorced Elinor, he paid me a visit one evening in my Commonwealth Avenue apartment and together we did honor to a bottle of bourbon. He started to talk very candidly about his private life and mentioned that he was deeply in love with a new woman. He was eager to marry her; too eager I thought, and I suggested he wait for a period after his divorce before making that step. What became clear in that conversation was that despite of, or perhaps because of, his rather gallant personal affairs, he had a deep yearning for the stability of an old-fashioned family life and was certain this new woman in his life, Lois Swirnoff, would provide that. It was not to be as he hoped but that is not a subject for me to go into.

During this same period Jule and I discussed several scientific problems. He had started to work on his very fruitful extension of 2-dimensional turbulence theory to include vertical structure with the use of quasi-geostrophy. He was deeply concerned about his inability to include the influence of the dynamical boundary condition at the lower surface. I attempted to reassure him that even with a purely internal model, as in the Charney-Stern theorem, his expansion in the vertical would still be a complete set even if it ignored some effects of the surface constraint. Bill Blumen later took up the complementary problem but Jule remained dissatisfied with what he considered his own partial treatment. He was always candid with me about his self-doubts and given his enormous contributions it says a lot about his intrinsic modesty and integrity.

Then in the spring of 1967 several things happened that seem to give the promise of better times. Carin's mood improved immensely after the electro shock therapy and it was conceivable that she could be released from the hospital soon. At the same time, I received word that I had been awarded a two- year Sloan Foundation Fellowship. At that time, you did not apply for the Fellowship. If you were fortunate you were just awarded it out of the blue. I never discovered who was responsible for my being considered but it was probably some people at MIT who knew me best. The award carried a small cash stipend the first year to allow for some travel and a book allowance and a larger sum for the second year to allow the recipient to take a year off teaching. The idea was that for young academics recently graduated, the year away would allow for the start of new research directions beyond what had been done in graduate school.

I had planned to use the money to go to the Math department at Imperial College in London. The people there were first rate, the language was not a problem and it seemed far enough away from our problems so that, I thought, we might have a fresh start on our married life. Naiveté, certainly; but it seemed just possible.

At the same time, I made the decision to take up the offer from Chicago now that the legal issues of the Loyalty Oath had been resolved. The Department had been willing to wait until the case was decided and, again, I was hoping a fresh start in a new atmosphere would be helpful to Carin. She knew several of the older people in the department like George Platzman from her time living there as a young girl with her father. The offer, over the course of the year since it had first been presented had been improved to include tenure and the salary was substantially greater than the MIT salary I was getting and given the medical bills I had been accumulating the money issue was not a trivial one. So, with some trepidation, mostly about the somewhat daunting and insecure nature of life on Chicago's South Side I accepted. We sold most of our furniture, stored the rest and prepared for the move to Chicago via London.

Another attraction of Chicago for me was the opportunity to take an appointment in a more natural department for me than Mathematics. Although I was happy to use sophisticated mathematics, I viewed myself as a person doing physics of the atmosphere and oceans and a home in a Geosciences Department seemed more fitting to me. I was going to miss my MIT colleagues and friends but the combination of advantages and the hope that my personal life would take a turn for the better made me hope that it was the right decision. Still, in my inner soul I was still very nervous about Carin's health and the robustness of her apparent recovery. There were still ups and downs in her mood but I held my misgivings to myself if I even admitted them consciously to myself. It was a life on eggshells.

We sailed for London on the SS United States, established residence in a comfortable garden apartment in the attractive London suburb of Putney and started to explore what London and England had in store for us. I was just 29 but felt much older.

Chapter 6

The English Interlude (1967-1968)

My host for my visit to London was the Mathematics Department at Imperial College. In 1967 the department on Exhibition Road was next-door, and in fact was physically connected, to the Victoria and Albert Museum in South Kensington. It was a charming location, made more so by the nearby presence of a wonderful pub, The Grove, on Beauchamp Place (pronounced Beecham in the English mode) where a good pub lunch with colleagues was a daily pleasure. Museums like the National Gallery and the National Portrait Gallery were within walking distance (long) or a short tube ride, as well as the many bookshops on Charing Cross Road. In the early days of our stay we frequented the theatre in London a great deal. The price of a theatre ticket was about the same as the price of a haircut and we took advantage of it to see a stellar array of actors on stage live: Lawrence Olivier, Alec Guinness, and Robert Morley are just three that come to mind immediately. The dollar was strong and the pound had just been devalued so my Sloan Fellowship left us financially comfortable. We dined out often. An Italian restaurant in Soho, La Terrazza, became our favorite and it involved only an inexpensive English taxi ride to our place in Putney. It felt, initially, as if life had found a stable and happy place to rest.

The situation at Imperial was equally positive. The colleagues there were superb. J.T. Stuart (Trevor) was the head of the applied math group. His work on the nonlinear theory of shear flow instability was particularly interesting to me and at first I thought it could be applied directly to the baroclinic instability problem. That was not the case. I had tried to do so without success while I was still working at MIT with Harvey but that part of the story has to wait. Another good colleague at Imperial was Derek Moore. I knew Derek from his summer visits to the GFD program in Woods Hole where he came (with his tenor sax) to visit Ed Spiegel a close friend and scientific collaborator. Derek and Ed's early work on chaos theory, roughly contemporaneous with Lorenz' work has never been properly acknowledged. I shared an office with a young, recent graduate, David Creighton, a charming man who later became an important figure in English applied math.

In addition, there were two other American visitors that year. One was Steve Davis from Johns Hopkins, then at Northwestern and Stan Berger from Berkeley. I became very close with Steve both scientifically and as a friend. So, the intellectual and human environments were truly superb.

Just before our move to London I became interested in what I thought of as an overlooked aspect of the theory of the ocean circulation. The principal theory, as it was then, for the mid-ocean involved a simple relation, called the Sverdrup relation, between the total, vertically averaged horizontal mass transport, and what is known as the *curl* of the atmospheric wind stress on the ocean surface. That seemed to hide a somewhat weaker but non- negligible part of the circulation that was driven by the stress itself. This was connected in an indirect way to the work I had been doing with Victor on vertical boundary layers on lateral boundaries of a basin. The circulation so produced did not produce a western intensification and this was interesting in its own right. A small paper I published in Journal of Fluid Mechanics explained that point. At Imperial I began to ask myself whether I could put together a similar picture for a stratified ocean. I knew that, in general, this would be too hard but I thought I might have a chance to do it if I limited myself to a linear model and ignored all nonlinearities. Although not realistic it could be the first such model that included many disparate aspects of the circulation problem that had not been dealt with simultaneously. I gave myself the year at Imperial to work on that model. It was a complicated problem but I enjoyed putting the pieces together.

At the same time, I was invited to many universities in England to talk about my earlier work. I gave seminars at Cambridge, Liverpool, Manchester, Norwich, Newcastle, Bristol, Glasgow and others that escape me know. Carin usually accompanied me and with the help of our hosts and the irreplaceable Good Food Guide we were able to eat well in England: a minor miracle.

Everywhere we went we found the English people friendly and warm to us. I recall a trip to Salisbury where we went to visit Stonehenge. We visited the pub in the Trust House Hotel we were staying in and the young men in the pub insisted that I join them in a game of darts in spite of my warning that I had never played before. It was a very good time. Similarly, one Saturday afternoon we were having lunch in the pub on Putney Green (where cricket was often played) near our apartment when two very young men came up to our table as we were about to leave and asked, “Are you Yanks?” I thought this was a rather menacing start to the conversation especially when, after I replied in the affirmative, he asked us to stay put, please, and not to leave. Anxiety was mixed with curiosity and shortly the seedy looking young men returned with a rather frail older man. They explained that the old fellow stayed in his rooms all week long but on Sundays they usually went to fetch him and give him an outing at the pub. The old fellow had been a joiner (carpenter) in his youth and had travelled to the US for work and the young guys thought it would interest him to meet some Americans again. They went to fetch him even though it wasn't Sunday. So, these young hoodlums turned out to be very sweet and we had a splendid afternoon helping the old carpenter reminisce about his happy, youthful days in America.

The happy times for us in London did not last. One evening Carin and I were dining in a restaurant in London after a play when her face suddenly began to contort and twist and her eyes began to rapidly open and shut. A moan and cry followed and Carin fell violently to the floor in an epileptic seizure. A trip to the ER followed and from that evening on things went downhill very rapidly. There were more grand mal seizures always at unpredictable times so that simple things like dining out became nerve-wracking. Carin had been advised against late nights and alcohol but she refused to follow the advice. The return of the epilepsy was accompanied by a swift return of her depression and worse things followed. Carin began to harm herself by cutting her arms. She told me it made her feel better. I was close to a breakdown myself; life had become so tense. Her depression turned to suicidal thoughts and one day when I came home after a day visiting Cambridge University, I found her alive but unresponsive in bed with just a two word note that said "I'm sorry". Emergency doctors from the hospital revived her and she was hospitalized in a rather grim English mental hospital.

From then on, for our remaining time in England, life returned to the pattern it had established in Boston; suicidal attempts, self- harm, and deep depression. There were times when things seem to improve and Carin was discharged from the hospital but they were temporary. The days were full of tension and danger. I had to keep her medication for the epilepsy in my office at Imperial, bringing home only enough pills for a few days at a time to prevent their abuse. Near the end of our stay we were both visiting therapists together and it seemed to help a bit. I was eager to get to Chicago as soon as possible to get settled in a more stable environment. George Platzman, by coincidence was spending the year in London and his presence was a godsend. He was very helpful.

In August of 1968 we had plane tickets for Chicago and George's permission to stay temporarily in his large house in the Kenwood section of Hyde Park, the University neighborhood. As we were packing our suitcases the BBC reporter in Chicago covering the Democratic convention that was taking place there was sobbing on the radio describing "scenes of unprecedented violence". I was having doubts about the wisdom of the move to Chicago but I was eager to conclude the London sojourn as soon as possible.

Chapter 7

Chicago and a New Path

When we arrived at O'Hare airport in Chicago, I went to a rental car desk to get a car to take us to Hyde Park and to use until I could buy one of our own. My mood was not brightened by the response of the agent when, in response to her question, I told her I would be staying in Hyde Park, the neighborhood near the University. "Oh my God", she said, "That's the home of the Blackstone Rangers". That was all I needed. I now felt I had made another life blunder and was prepared for the worst. In fact, it was the beginning of one of the most interesting periods of my life.

We quickly settled in George and Harriet Platzman's home. Their arrival a few days later seemed to cheer Carin perhaps because she had company throughout the day. We quickly found a rather spacious apartment on Blackstone Avenue (no Rangers in sight). I bought a used Corvair from the Ernie Banks dealership. It might have been unsafe at any speed but it started smoothly in the bitterest of Chicago winter days. We shopped for an apartment's worth of furniture and I moved my scientific books and papers into my office in the small Meteorology Building, at that time right next to the University of Chicago faculty club. I remember being so frightened of being mugged that I parked the car and ran back and forth between the car and my office to unload it as quickly as possible.

The Geosciences Department had just been formed by melding the meteorology and geology departments. There were weekly faculty meetings in another larger building, Rosenwald, while the future home of the department in the

Hinds Laboratory was being constructed. Those days were exciting because the new ideas of global tectonics, mantle convection and seafloor spreading were becoming the subject of widespread interest. As a fluid dynamicist, I found the subject fascinating but at first it seemed rather distant from my own work and interests.

Aside from George Platzman, the other faculty member in the meteorology building was Larry McGoldrick. Larry was a former student of Owen Phillips of Johns Hopkins. He was then doing beautiful experiments on the interaction of capillary waves and analyzing the results through the aid of a technique called the method of multiple time scales, an asymptotic method that allowed the analysis of weakly nonlinear systems. Both his experimental results and the mathematical method were very intriguing and I quickly learned the basics of the method through the published work of Julian Cole. Larry and his wife Clare became good friends and we did a lot together socially.

The University in those days was led by an inspiring figure, Edward Levi. He had a long history at the University of Chicago (UC) starting with his attendance at the Laboratory School, the elementary and high school associated with UC. He impressed me almost immediately. He had a breakfast with the new faculty hired that year in the Quadrangle Club, the university's faculty club, shortly after I arrived. He had clearly done his homework and knew each of us by name and something significant about our backgrounds. He was a master of the deep gesture. One of our number, a new faculty person in the Chemistry department, arrived just a bit late to the breakfast. In a measured tone that made clear the importance of his remark, Levi greeted him by name and then added, " Now that you are here at the University of

Chicago, I hope that you will publish much less!" The message was clear; the important thing was significant scholarship, not resume building.

Levi also guided the University through the academic turbulent times in the 70's. A student protest on the denial of tenure to a professor in the Social Science department severely tested the school. Some hotheads seized the administration building. Levi handled it perfectly. The police were not called. The occupiers were told they would be subject to disciplinary action for interrupting the academic life of the University but no physical action was taken. Levi just kept to the message of what the University was about and what it was not. It was clear that this was a subject he had thought about a lot and had built his views long before the crisis. In the end the students left the administration building on their own, frustrated that they had been unable to provoke a violent response.

A few years later when Levi was chosen by President Ford to restore the Justice Department to health following Watergate, the Faculty had a going away party for Levi. It was a love fest. In Levi's response he expressed gratitude to the Faculty for their support but also left us with an admonition that I have never forgotten. He noted, "Everything excellent is fragile".

So, from the professional side of my life I felt as if things could not be better and I quickly settled in to that stimulating environment.

But the atmosphere quickly darkened. We had not been long in our newly rented apartment with all our new furniture when one evening at dinner Carin said matter-of-factly, " I was walking across the Quadrangle[2] this afternoon and everyone

[2] The Quadrangle was the central courtyard of the University's campus.

was laughing at me”. My heart sank and in fact in just a few days Carin was admitted to the psychiatric ward of Billings Hospital, the University’s hospital. From that time forward she was hospitalized almost continuously with only brief periods spent at home. It was the beginning of a long dark period. I felt very much alone.

I tried my best to hold things together. In one of the relatively brief periods where Carin was out of the hospital, we bought a condominium apartment. I think we paid $35,000 for it. The University had a program that allowed faculty who bought homes in the Hyde Park neighborhood to get a second mortgage so that even with little cash for a down payment we were able to purchase our own home. I had hoped that this token of permanence and stability in our lives would encourage Carin to a more hopeful state of mind. Unfortunately, I spent most of my time in that condo apartment alone.

To keep my sanity, I threw myself into my work and teaching. I enjoyed teaching a very great deal. I was not too happy with the course structure that was available to our students and with the support of the older faculty I was able, with their approval, to outline a sequence of courses and their content that I felt had greater coherence than the exiting program. This became our Core GFD program and I personally taught much of it.

I also began to think more deeply about the nonlinear baroclinic problem. The principal difficulty with Trevor Stuart’s method applied to that problem is quickly explained. For the problems Trevor examined, the threshold for instability was determined by overcoming friction or some other form of dissipation. That meant that at the parameter threshold for instability, where the wave was not growing but just holding its own against friction, there had to be an energy drain from the background flow to balance the dissipation. That energy flow implied an alteration of the basic flow that could be calculated from the non-growing solution at the marginal curve. In turn, that altered mean flow could be used to obtain the effect of nonlinearity on the growing wave near the marginal curve. This led to a first order differential equation for the amplitude of the wave that would start to grow exponentially when infinitesimal and then level off and reach a steady state when the effects of small nonlinearity balanced the slight instability obtained just above the threshold. It is an excellent method for such problems. The difficulty for the equivalent problems in meteorology and oceanography, i.e. the baroclinic instability problem, is that the stability threshold is normally determined by inertial constraints and not dissipation. That meant the change in the background field only occurred after the wave starts to grow. However, if you take as your basic solution the exponentially growing wave of linear theory you have frozen in exponential growth and can't answer the question of possible equilibration. That was the basic conundrum; that is, how to allow for the structural changes in the growing wave that would give rise to alterations in the basic flow without specifying that growth as exponential, since one anticipates that the nonlinearity is eventually going to alter the growth obtained from purely linear theory. Put that way, it seemed to me that the method of multiple time scales would allow the possibility of slow evolution of the wave amplitude, as yet unknown, and relate the alteration of the basic flow to that slow, but not yet determined evolution on the slow time scale associated with slightly unstable flows. Weak growth balanced by weak nonlinearity would allow an asymptotic approach to the dynamics.

After struggling with the fact that I had to posit the existence of the alteration of the mean flow at a lower order than the equation that would specify it, the method worked perfectly. The system was really third order in time rather than first order in time as in the dynamics of the Trevor Stuart models (as well as the convection problem worked out by Malkus and Veronis) and the time rate of change of the mean flow was related to the time rate of change of the square of the wave amplitude. That latter equation could be integrated directly so the final, non-dissipative, system was second order. It had to be second order since in the absence of dissipation the dynamics has to be time reversible. I ended up with a nonlinear oscillation; exponential growth would give way to stabilization but the amplitude would overshoot a possible equilibrium and the wave would decrease and reach such small amplitude that linear dynamics would start the unstable cycle again. I thought the theory was beautiful and furthermore, the nonlinear problem could be solved analytically in terms of elliptic functions. This helped reveal the basic physics of the inviscid equilibration. I was ecstatic. It was moments like this that made me momentarily forget, if only for a brief period, just how gloomy my personal life was at this period.

I later added a small bit of friction to the model so that the system would have a damping term of the same order as the nonlinearity and growth. This meant the equation for the mean flow had to remain first order in time; it could no longer be integrated so the total system was genuinely third order. At first, I thought the system might slowly damp to a steady state but the inherent instability of the system prevented that. Then I showed that limit cycles were possible, i.e. perpetual nonlinear oscillations that lost their dependence on initial conditions. I was able to find such solutions in a quasi-analytical way, again using asymptotic techniques of the type described by Cole in his useful text.

About ten years later, with the help of a graduate student, Chris Frenzen, we were able to demonstrate that the system also contained a parameter region of chaotic behavior and that the threshold could be predicted using the universal formula of Feigenbaum for the period doubling sequence of the limit cycles. The fact that the Feigenbaum ratio, derived for much simpler algebraic systems governed by simple difference equations also worked for the baroclinic instability problem was, in my mind, a spectacular result. Indeed, the kind of chaotic behavior I found was the first time a truly deductive model of such instability showed deterministic chaos. Lorenz's beautiful and pioneering work employed a model system not derived in any deductive way from the physics of a real problem. He used an approximation to the equations for thermal convection that were a poor approximation and gave misleading results for *that* problem in just the region where chaos occurred in his model. Nevertheless, his intuition that the system he used as a model would be characteristic on the truly deductive system was a stroke of genius.

My academic life was also enriched by my interaction with the first two students who worked with me for their Ph.D. degrees, Frank Richter and Arthur Loesch.

Frank came to see me about a year after I arrived in Chicago. He had a degree from the Colorado School of Mines and was interested in the new ideas of sea-floor spreading and mantle convection as was I. As he phrased it, our collaboration seemed like a natural team. He knew geology but not fluid mechanics and I had the complementary background so we started to work together. The literature on the subject seemed to me to be largely polemical involving arguments about what would happen in the complicated earth system. The GFD style of formulating a tractable problem, shorn of unnecessary complexity seemed unknown. There was, for example, a literature on the question of whether the mineral phase change from Olivine to Spinel in the mantle would be a barrier to convection or an energy source for the convection. It seemed natural to pose a simple model with such a phase change and see, but it wasn't for some time before that was done. It seemed as if people were having too good a time arguing.

Without going into details, I will just say that working with Frank was wonderful. We posed some simple models, came to the conclusion that it was the negative buoyancy of the subducted slab, connected to the surface slab or not, that provided the locomotive force for the convection. Suffice it to say, Frank's thesis was superb. The only drawback was the misleading sense he gave me that advising a student's Ph.D. research was easy. Point the student at the problem and see it all work out. I learned later this was a seriously overly optimistic expectation in many cases.

Arthur's thesis work also went along swimmingly. He examined the role of the resonant interactions of a weakly unstable wave, originating in the parameter range of instability, with two *neutral* waves in the stable regime and so sharing the energy it received from the mean flow with waves that could not grow on their own. It was also a first- rate thesis.

So, I was delighted with my work environment. My faculty colleagues were doing interesting work. Dick Lindzen became a good friend. He did beautiful work on the Quasi-Biennial oscillation in the tropical atmosphere that could be seen as a natural follow-on to his work unraveling the mystery of the diurnal and semi-diurnal atmospheric tides. Dave Fultz was concluding his monumental work on the so-called dishpan experiments for which he was elected to the National Academy. George Platzman had just started his work on realistic models of the oceanic tides including topography, reawakening interest in that classical problem. Hsiao-Lan Kuo continued his work on tropical dynamics. McGoldrick was a close friend and helped me with my limit cycle work. He constructed an *analogue* computer to integrate the equations and provided direct output as phase plane figures. Victor Barcilon had started his work on inverse problems; i.e. can you hear the shape of a drum. So, it was a stimulating atmosphere in the department with additional interest provided by the groups in paleontology, mineralogy and petrology. Each Friday someone, on short notice, was asked to stand before the faculty at lunch and explain in lay terms what research they were occupied with at the time. This was a great way to let each of us know what the rest of us were doing and helping us expand our interests out of our specialties.

I took special pleasure in my relations with our department chairman, Julian Goldsmith. It was largely his vision of Geosciences department as a home to all the

Earth Sciences that led to the formation of the department. There are many such departments in the country now but at the time it was unusual. Goldsmith himself was unusual. A brilliant geochemist he was an extraordinarily warm and approachable person and we did many pleasurable things together like trips to Wrigley Field to see the Cubs play during some years like 1969 when they were a splendid team if always fated to lose when it was important. He was also cultured but unpretentious. He was a collector of pre-Columbian art in the days before it became prohibitively expensive and was a genuine patron of the arts. He commissioned a local ceramic artist to design the entrance way to the Hinds Building, our department's new home. The enclosed space of the entrance was completely covered with sculpture consisting of slabs of ceramic whose abstract forms called to mind the textures of the Earth's geology or the atmosphere and oceans. He asked me if I would write a brief paragraph describing the connection between the art and our science. I tried to find the words of a suitable prose paragraph but in the end, I settled on a free verse poem:

> "Earth, Air and Water,
>
> These are their forms and rhythms,
>
> Around us, always, everywhere."

Julian was enthusiastic about this simple poem and had it engraved on a bronze plaque in the hall.

At about this time I was put up for promotion to Full Professor from my position as tenured Associate Professor. I was asked to provide a list of people who could provide useful letters of recommendation. In that list I included George

Veronis. My relations with George had become frosty after the publication of my paper on the rectification of Rossby basin modes and their contribution to the general circulation. Suddenly, one summer in Woods Hole Carin and I noticed that in public social occasions George would conspicuously avoid talking to me. It was embarrassing. Eventually, though, his hostility seemed to diminish and I had no reason to believe his letter would be anything but an objective appraisal of my work.

A few weeks later I got a phone call from Julian asking me to come down to the chairman's office for a chat. This was not unusual for we often discussed departmental matters together. After asking me to be seated he said in a rather melancholy way that he was going to break a fundamental rule and read to me one of the letters of recommendation in my promotion case. It was from Veronis and, fortunately, it was so ad hominem and outrageously personal in its attack on my work and personality that it could not be taken seriously by any academic committee. Julian just said that while the letter would be ignored, he had read it to me so that I knew I had an enemy and could protect myself in the future. I found the whole thing immensely sad. I never let on to Veronis that I knew but I had just entered the list of his enemies that included Carl Wunsch and several other scientists among whom I was proud to be counted. It was good to keep the information to myself. I want to emphasize, since most people believe academia is rife with such personal enmities, that this one seems happily to be one of the few I have come across personally.

The work life at Chicago was intellectually satisfying but it did nothing to deal with my personal black hole of a life. There would be times when Carin was allowed home for a brief period and she would frequently use such occasions to obtain something: matches, razor blades, etc., that she would use on her return to the psychiatric ward to hurt herself. Finally, one day one of the doctors asked me how long I was going to be able to keep it up. The brutal honesty of his question brought me up short. I had been thinking, naturally, that I would stay in this situation until Carin recovered and here was one of her doctors essentially asking me what I would do if it became clear that there was not going to be a recovery. I was in despair.

In that state of despair my relative naïve innocence about adult academic behavior was considerably shaken. Women, some of them wives of colleagues in my department, called me at home as well as at the office and made clear that they were offering to console me. After years of living in a never-land between an officially married state and a de- facto single state, I accepted some of those offers. The physical release I obtained in those consolatory relationships was a deep relief but at the same time I could not help but feel I was participating in some kind of moral self-degradation. Still, the experience of physical tenderness was very much appreciated and, in some cases, as I learned about the unhappiness in the lives of those women, I felt less guilty. I discovered there was a kind of community of sexual adventurers, both husbands and wives in the local academic community. I was invited to parties whose nature I never would have dreamed would be true. In retrospect this was happening to a generation of adults who had just missed the sexual revolution young people were experiencing with the introduction of the Pill into the moral sexual calculus and were eager to make up for lost time.

I felt I had reached bottom when I was invited by one of my woman friends of the time to dinner at which she introduced me to a younger friend of hers who was engaged to a man from another city, was about to be married, and it was made quite clear that the woman was looking forward to a last fling before that marriage. That was too much for me and I decided I had to break this mode of living.

In the spring of 1970, two events happened coincidentally that changed things forever. Carin made another spectacular attempt at suicide by throwing herself from a second floor window in the hospital while she was being taken to another ward for tests. She rather surprisingly survived and recovered. In fact, as was the case after many such attempts, she temporarily felt in better spirits. However, the University hospital reached the decision that its psychiatric ward, which was not meant for long-term care, was inappropriate for Carin. She was moved to a long-term care facility outside Hyde Park. It wasn't as grim as the place in Boston but the meaning of the move was clear. Little hope was possible.

During the period of Carin's illness, I had been reluctant to travel very much out of town so I could be quickly present in any crisis. One morning as I was preparing to leave our apartment for work, I received a phone call from Willem Malkus inviting me to be the principal lecturer for that summer's (1970) GFD course. I thought about it for about two seconds and agreed immediately. I thought of it as a chance to get away and into a serene and familiar environment where I could recover some psychic balance. At the same time, I accepted another invitation for a little later in the same summer to lecture at a conference on Mathematical Problems in the Geophysical Sciences held at RPI in Troy, NY. I thought this little holiday was desperately needed and that I could use the time to reestablish a more orderly life.

I prepared a set of lectures for the Woods Hole GFD program based on the work I had done with Victor on the theory of stratified, rotating fluids supplemented by a discussion of the instability of such flows using the techniques and ideas I had just formed by my recent work on weakly non-linear baroclinic instability.

The picture I had in my mind for the Woods Hole part of the summer was that I would give my daily lecture, interact with colleagues, perhaps go to the beach in the afternoon and have a quiet supper by myself in one of the local restaurants and turn in early. I was looking for a quasi-monastic experience, clear of any emotional involvement or sexual adventure. It was a plan to restore some deeper sense of order to my life.

Again, I wonder with awe about the ability of the Universe to regard my plans with an ironic chuckle.

I was delighted to be again in Woods Hole. I rented a room in a home within walking distance of the Oceanographic Institution and started my lectures dealing with the structure of the steady dynamics of stratified flows. I lectured slowly and carefully and aside from the usual banter and skeptical questioning that is standard in Walsh Cottage, everything went smoothly. I had on one occasion to bring Willem to task with a humorous story. After he had asked far too many questions than his confusion merited, I told about the rural mountain man who, despite the warnings from his friends, married a woman in town notorious for her nagging. After the ceremony, as they were leaving town in his horse and carriage, the horse stumbled, shaking the carriage. “That’s one”, said the man. A short time later the horse stumbled again. “That’s two”, said the fellow. After the third time the groom said, “That’s three” and got down from the carriage and shot the horse dead. His new bride started to scream, “You imbecile. That’s our only horse. How can we do the plowing we need for planting? You are so stupid. Why did I ever marry you”? The country fellow listened calmly and when his new wife paused to take a breath, merely said, “That’s one”. So, I turned to Willem and said, “Willem, that’s one”. The audience erupted in laughter. It kept Willem in relatively good behavior until later in the summer when my good friend Steve Davis (whom I had met, remember, at Imperial College) was lecturing and Willem reverted to type by asking an unnecessarily aggressive and inappropriate question. Steve merely gazed at Willem and said, “Willem, that’s two”. It was a good summer.

After my morning lecture on the second day, as I left Walsh Cottage to go to lunch, I saw Holly Schulman sitting on the lawn in front of the cottage waiting for her husband, Elliot, to exit. Elliot was attending the lectures that summer. The Schulman’s were part of the wine tasting and poker playing group in Cambridge that I had gotten to know during the years I was teaching at MIT. She mentioned that she and Elliot were going to dinner at the Landfall restaurant that evening and asked if I wanted to join them. I was happy to have the invitation and accepted. It was a dinner that changed my life.

The Landfall is a restaurant in Woods Hole with a marvelous water view, quite good food and a charming atmosphere. In those days the dinner tables did not use the white tablecloths they use today, instead, they had paper place settings embellished with a quote from Joseph Conrad talking about a ship making landfall. Holly and I started talking about Conrad, about how much we admired him as an author and how much we enjoyed his books. Elliot seemed to take no interest in the conversation and so in our conversation we seemed to become a couple apart. In fact, by the end of that evening we both knew we were in love, as strange as it might seem. I owe Joseph Conrad a lot. It was that meeting of the minds that was the start of our relationship.

Soon we became lovers and in the following January Holly, with her 2 year old daughter, Anna, moved to Chicago to join me. We both were soon divorced from our current spouses and started a life together that lasted 42 years until cancer sadly ended a very happy marriage with Holly's untimely death.

With my private life restored to health and with the support of a loving family, I was able to concentrate on my work, not as an escape from realty, but as an enrichment of life. For most of the following decade I developed further my ideas of the nature of the nonlinear baroclinic problem. I have already mentioned the work on the chaotic behavior of weakly nonlinear instabilities. I also used a long wave model of instability to discuss, analytically, the criterion for the selection of the observed wavelength when the supercriticality of the system was large enough so that many wavelengths could be unstable. The conventional understanding was that it would be the linearly most-unstable wave that would dominate the finite amplitude state. For a fully developed turbulent state with many wavenumbers there was much evidence to suggest that energy would flow to longer wavelengths. However, I was able to show that even for weakly nonlinear systems that had a large enough supercriticality to allow several waves, that it was not necessarily the most-unstable wave that would dominate the end state. Instead, I was able to show for systems in which wave-mean flow interactions dominated wave-wave interactions that it would be the wave that, by itself, could reach the largest amplitude that would dominate in the competition for the energy available in the basic state. This was generally not the initially fastest growing wave, which might saturate quickly leaving available energy for more slowly growing waves. I also was able to discuss the problem of the spatial growth in the nonlinear regime and models of wave amplitude vacillation.

In the early years of that same decade, 1970-1980, my attention was drawn to the problem of coastal upwelling. In 1973 I was invited to act as theoretician in residence at a coastal upwelling workshop in Corvallis, Oregon at the Oregon State University (OSU). Holly, Anna and I left by train for Corvallis and were given the home of an OSU professor who was away for the summer and the use of a Ford Pinto auto for the summer. The underpowered Pinto was a State car and when we visited campsites in Oregon people gave us looks that made me want to explain that were not public employees abusing the system.

We found the natural surroundings beautiful and idyllic, and Holly, especially, enjoyed the feeling of personal security that, at the time, was sadly lacking in the Hyde Park neighborhood of Chicago where we were living. We typically went to the mountains or the seashore each weekend and enjoyed to the fullest the natural setting of each. I think that summer made us question, as a family, whether remaining in Chicago was something we wanted to imagine lasting indefinitely. We began to think of the possibility of a move.

I found the discussions of the models for coastal upwelling fascinating and was pleased that the boundary layer work I had done in the decade before with Victor Barcilon could be adapted and extended to produce useful models of the upwelling phenomenon. Our models had two deficiencies. Most importantly, they were linear models that unrealistically assumed small deviations of the isotherms from the horizontal and second, most of the work Victor and I had done involved circulations with negligible variations along the boundary. One of the pleasures of that summer was establishing a working relation and friendship with John Allen, who was at Oregon State University. I had met John a year before at a meeting at the University of New Hampshire where we had brief discussions of the dynamics of rotating and stratified fluids. With John's help I was able to derive a useful theory for the general linear problem including long-shore variations and distinguish a limit on the long-shore scale below which the onshore flow would be in geostrophic balance and the vertical velocity much reduced. Larger long-shore scales than this critical would recover the more traditional theory with no variation along shore and consequent strong upwelling.

The summer had another unique professional event for me. It became the one and only time I went to sea for professional oceanographic reasons. When the assembled group in the workshop realized I had *never* gone to sea on an oceanographic cruise they good-naturedly, but relentlessly, teased me into accepting a short cruise of, I think, 3 days duration, whose stated purpose was the recovery of current meter moorings on the Oregon shelf. The sea, fortunately, was like glass so I avoided seasickness. I also discovered if you were not involved in the scientific work of the ship at sea that the time spent on a cramped, smelly (fuel oil) ship can be devastatingly boring. I did take one bucket thermometer reading of the sea surface to establish ground truth for an overflying NCAR plane measuring sea surface temperature with an IR instrument. That cruise and that measurement is my total experience with seagoing, observational, oceanography!

After a year or so living together in Hyde Park, Holly and I decided to move from the condominium apartment I had bought with Carin to a small, 19th century house on Blackstone Avenue. The house was charming and not too practical for the Chicago winters but we enjoyed having our own house and Holly particularly enjoyed the large backyard and garden that came with the property. We took full advantage of the University's policy to provide second mortgages to faculty who bought homes in the Hyde Park neighborhood around the University. There were actually wild strawberries in the garden and a fruit bearing peach tree in the backyard. The house cost $45,000 and with the help of the University we were able to buy it for only $ 2,000 in cash. It was a wise investment.

We enjoyed much of what Chicago had to offer: great restaurants, wonderful art museums and the Lyric Opera to which we had season tickets. What was less attractive was the sense of personal insecurity I referred to earlier because of a serious crime problem in the Hyde Park neighborhood. Holly was especially concerned about our safety and after the summer in Corvallis, during which time our security was not an issue, we began to ruminate about a possible move to a less tense environment.

We had been living together since the winter of 1971 and 3 years later we decided to get married. As neither of us was religious, we hoped to have a Justice of the Peace marry us in our back garden. We discovered Justices of the Peace do not make house calls so we asked a compliant Unitarian minister to do the ceremony and we wrote out the language of the ceremony to be sure nothing religious crept into the ceremony. We invited a host of friends and hired a friend of the McGoldrick's to cater the small reception after the ceremony. Since some of the guests brought their children, we asked the caterer to provide peanut butter and jelly sandwiches for the children while more adult fare was prepared for the others. In later years the myth grew that the reception fare was purely champagne and PB&J sandwiches. A charming myth, but like many memories, entirely wrong!

During this period, we developed the pleasant habit of leaving Chicago in the torrid summers and driving to Woods Hole where I attended the GFD summer school as a visitor. We deeply enjoyed the beauty and serenity of Woods Hole and it seemed a pleasant alternation with the urban life of Chicago, which to be fair, we also enjoyed, security issues aside.

As we mulled over the possibility of a move to someplace outside Chicago where Holly would feel more secure, we were soon presented with an attractive possibility. In 1975 the Scripps Institution of Oceanography made a very tempting offer of a position with 11 months per annum of hard money (i.e. institutional and not grant dependent) support. Our visit to Scripps was rendered even more delightful by the generous offer by Walter Munk to lend us his house during our visit while he and his wife were away in Colorado. So, we set off with our curiosity piqued, to investigate the possibility of new life perpetually away from winter.

We found La Jolla charming and friendly and I found, as expected, that the people I would interact with at Scripps, were I to move there, absolutely first rate. However, two big issues deterred us. First, the nature of the department at Scripps in the area of physical oceanography seemed very disjointed and split into sub groups, some of whom were in different buildings distant one from the other. I worried about the seeming lack of interaction, and could not understand how I could do there, something like what I had done in Chicago with the curriculum, that would require an agreement across the department. The second big deterrent was the real estate market in La Jolla. Prices of houses were so much higher than in Chicago that it appeared to us that we would end up living far from the Institution and probably far from the ocean in a situation requiring a long daily commute. That took some of the potential bliss off the table. In the end we decided to stay put in Chicago for the time being. It was during this time that Holly's interest in artistic photography became an increasingly important part of her life. She took courses at the Art Institute of Chicago. During the week, I would stay home one day from the University so Holly could have the day free to use the darkroom at the Geosciences Department. Our good friend Julian Goldsmith made the necessary arrangements to give her that opportunity. Her photographic work flourished and established a foundation that would enable her to teach the subject. She was a terrific teacher as well as artist and it was exciting that we both had intellectual pursuits that we could share with one another.

I still continued my work on coastal upwelling and one paper that came out of that effort had an indirect influence on some later work.

The major weakness of my upwelling work, at least in my own eyes, was its restriction to linear theory, i.e. the unrealistic idea that the isotherms departed only slightly from horizontal surfaces. I tried to confront that problem by recasting it as the steady state reaction to a mass sink located in the corner region at the intersection of the sea surface and the coastal boundary, envisioning that sink as the manifestation of the net upwelling produced by the offshore Ekman flux. The sink was fed by an equal onshore deeper mass flux, independent of depth. That provided an offshore boundary condition for the problem. The solution of the nonlinear problem was straightforward once the conservation of potential vorticity (pv) was used. For the case of uniform stratification far from the coast this reduced the problem to a standard linear partial differential equation and the solution was nearly immediate. The principal reason I get pleasure recalling this is that just a few years later this same device, using the same conservation law of pv allowed me to solve a much more important problem and it reminds me how much that method was in the forefront of my mind at that time.

I also adapted a method introduced by Francis Bretherton and Brian Hoskins in their study of frontogenesis to discuss the time dependent problem of the *onset* of upwelling in the nonlinear regime, again capitalizing on pv conservation.

On the whole, this period in the late 1970's at Chicago was a very fertile time for my work. It was at the end of that decade and shortly before we left Chicago that

I did the work I mentioned earlier on the chaotic dynamics of weakly nonlinear baroclinic unstable waves. So. I felt that I had hit a period of creative satisfaction.

At home things were going very well also. We started planning a sabbatical year away for the period 1977-1978 and while we had no definite offers to go places other than Chicago it was becoming clear that for both Holly and me that the tension of living in the urban setting that Hyde Park, Chicago presented was unsustainable. We had no definite plans but we felt open to change if one was presented to us.

We discussed together where we would spend that sabbatical year and, having enjoyed many summers at Woods Hole, came to the conclusion that a yearlong sabbatical year there would be good for the whole family. I was successful in obtaining a Guggenheim award to support the sabbatical and we found tenants for our house in Chicago and drove off to Woods Hole in the fall of 1977 in the expectation of a new adventure.

My plan, as outlined in the Guggenheim application, was to attempt to write a book on Geophysical Fluid Dynamics. While there were excellent modern books on fluid mechanics, e.g. Batchelor's book, then recently published, and good books on elementary meteorology and oceanography, there seemed to me to be a lack of a good book dealing with the fundamentals of Geophysical Fluid Dynamics on a graduate level at least in the form that I thought was essential and which the curriculum that I helped introduce at Chicago reflected. So, part of the luggage I brought with me to Woods Hole was a pile of course notes from the various courses I had taught at Chicago and earlier at MIT. I was a bit daunted by the prospect but also excited to see if I could do it.

I made one big decision first and this was based on an experience I had with my good friend Victor. We had made an earlier attempt to write a GFD book but it somehow got bogged down in excessive detail and an over emphasis on rigor and we came to the mutual conclusion to set aside the existing draft of several hundred pages and not pursue it further. We both felt relief at the decision and were especially happy that we had not contracted with any publisher to deliver a book for if we had, we might have found ourselves the authors of a published book we were discontented with. In going ahead with my new book project, I was equally determined to write the book first and then if, and only if, I were happy with the result would I look for a publisher.

We moved into our rental home in Woods Hole, on Ransom Road, not far from Quissett Harbor and in easy walking distance to the Clark Laboratory on the WHOI campus where I had my visitor's office. I already was acquainted with a fair number of the Physical Oceanography department's scientists. They were a stellar group with Hank Stommel clearly the brightest star. Others were also very impressive: Peter Rhines, Bill Schmitz, Bruce Warren, Breck Owens, and many others of the same caliber. I quickly settled in to this stimulating environment and got to work writing.

I found that the process of writing, and I hasten to add that in those days it was *writing*, i.e. all in long hand, was extremely pleasing to me. I had formed a rough outline for the book while I was still in Chicago. My intention was to write a text and not a research monograph. My feeling, which I believe time has validated, was that a research monograph, while of some fleeting value, becomes out of date fairly soon by its very nature. Instead, I wanted to deal with what I considered to be the basics and the essentials of GFD in a coherent way that brought the student to the jumping off point where the student was in possession of a strong enough foundation to begin independent research and where a researcher already in the field could refer to the book to provide a starting point for advanced work on a new research problem. I felt it was that kind of book that was sorely lacking in the field.

So, my plan was to avoid repeating some of the basic fluid mechanics such as the derivation of the Navier Stokes equations but discuss their consequences, e.g., basic theorems, with a heavy emphasis on the physical content and then carefully and systematically derive and discuss things like quasi-geostrophy and its applications. Basic ideas about the ocean circulation and synoptic scale instabilities in the atmosphere and oceans would also find their place in a systematic development of the theories. I was especially anxious to provide a text in which the basics were derived so clearly that many traditional approximations like geostrophy, the Boussinesq approximations or the use of the beta plane could be appreciated in a mathematically rigorous but yet physically meaningful development. A collection of classical examples like the explanation of the oceanic western intensification or baroclinic instability could be interpreted so clearly that a path to further research would have a clear starting point. At least, these were my goals.

So, I wrote. I wrote every weekday. After reviewing what I wrote the day before, making modifications when I thought they were needed, I moved on to write what I had the day before planned to cover today. Then, for about an hour before walking home, I would carefully outline what I would write about the next day. This pattern of composition worked very well for me and as I filled loose-leaf notebook after notebook, I felt a growing sense of satisfaction as I saw the text take shape. The department was extremely generous. I was asked to teach a Joint Program course on the evolving manuscript and I was happy to do so because I was anticipating the feedback from the students would help correct pedagogical deficiencies. In exchange I was able to get the help of a secretary (now a very retro term, such helpers are now called Administrative Professionals –AP's-) to type the evolving manuscript and the graphics department to help in the preparation of the figures. It seems rather quaint to me now that the figures had to be first drafted by hand from calculations done with a simple hand-held calculator and graphed as best I could. But it all went exceedingly well and when I gave a reading course based on the manuscript, the student response was enthusiastic.

I asked one of my Chicago colleagues, Peter Wyllie, who had written several texts on petrology, his advice about choosing a publisher. He strongly recommended a commercial rather than an academic press. As I recall, his main reason was that a commercial publisher would do a better job making the new book known. He also recommended Springer Verlag as a company who did a fine job aesthetically with their books and mentioned that they really liked books, a characteristic he said that was not always the case with commercial publishers. So, in April of 1978, with a large, heavy stack of hundreds of pages packed in a sturdy cardboard box, Holly and I marched into the small Woods Hole post-office and mailed the package, as requested, to Springer in New York and waited for their reaction. When I first contacted them and mentioned the size of the manuscript they seemed taken aback but when I gave them examples of other fluid mechanics texts of the same size they agreed to accept the manuscript for review.

I should mention that as I was writing away and the manuscript grew in size, I began to worry about what would happen if the manuscript would be destroyed by some accident. I took the first 500 pages of the text to an art supply and service store in Falmouth and had two Xerox copies made. I kept one in my WHOI office and one in the closet of our Woods Hole rental. My thought was that if both were destroyed by fire simultaneously that it would be a signal from fate to let the project drop.

After mailing the finished manuscript, Holly and I walked into the Landfall restaurant and at the table where we had fallen in love eight years before we had an alcohol enlivened lunch where we toasted each other and the delights we were experiencing in Woods Hole.

Indeed, even beyond the satisfaction of the composition of the book, we enjoyed visiting many parts of Cape Cod. The bird sanctuaries, the various town, state and national parks on the Cape were all delightful. We especially enjoyed Nantucket, which in those days was reached by ferry leaving from Woods Hole instead of Hyannis as it does now. We found our times together very romantic and, as folk wisdom remarks, a sabbatical year is the most successful time to conceive a child as it had been our intention to do for some time and we finally succeeded that autumn. Dove our younger daughter was born the following spring.

We also enjoyed the surprising bounty of music available in the winter in Woods Hole, both jazz and classical and Holly found it inspiring her photographic work. At the same time, I started, at age 40, to learn to play the clarinet. It had been a childhood dream of mine, largely driven by my enthusiasm for Benny Goodman and Artie Shaw, to play that instrument. Sadly, money was lacking as a child and public schools had no instrumental music programs as they do now. I thought a sabbatical year would be a perfect time to start. I had a "now or never" feeling. A music teacher in the Falmouth Schools, Jan Von Hertzen, recommended a possible instructor, Mike Crocco, and I started lessons with him on a rented clarinet. I loved it immediately and the clarinet, and the ability to play music has enriched my life ever since. I have continued weekly music lessons with a sequence of teachers to my great benefit and pleasure.

While waiting to hear from Springer what the reviewers would say about the book, I began to interact with Hank Stommel on a problem he and an associate, David Behringer, were working on. Hank had the idea that the heat release to the atmosphere by the warmer water of the Gulf Stream would alter the atmospheric flow and feed back onto the wind stress driving of the ocean and so alter the Gulf Stream's strength and path. I tried without success to come up with an analytical model that would be helpful. Nevertheless, I found the interaction with Hank to be an immense amount of fun. He later remarked to me, after we had worked on something else much more substantial, that working together with people was one of his favorite ways of making a new friend. We talked about a number of ideas that year, none of which led immediately to a project together but it did lead, at least on my part, with a desire to get to know this fascinating person better and to grasp a good chance to work together. We connected not only on a scientific level but we had long talks about non-scientific topics and we often found ourselves in agreement. We did disagree about a few important things; he felt, for example that it was an error for the Oceanographic to have gotten involved in an education program, even on the graduate level while I thought the graduate program it ran together with MIT was a brilliant idea and a fascinating educational experiment.

A special Christmas PO seminar was a tradition. Melvin Stern had, one year, given one dressed in a Santa Claus costume. When I was asked to give one that year, 1977, I gave it on my relatively new nonlinear theory on the onset of upwelling. I also ended the talk by a reiteration of the seminar in doggerel verse based on the Night Before Christmas, which I called the Night Before Onset. It started with:

T' was the night before onset
And all through the ocean
Not an eddy was stirring
The large scales of motion.

The boundary layers were hung
By the coastlines with care
In hope that the wind stress
Soon would be there.
(and so forth).

In short, that year was great fun for all of us in our family, a family about to increase in size. Some other of the pleasures were modest but had a special Woods Hole quality. As I walked to work each morning, part of my pathway would skirt the ocean on the Woods Hole to Falmouth bike path (now much extended northward). Each morning I would be met by two golden retrievers and the dogs loped along beside me (especially as I began sharing small portions of the lunch Holly had packed for me) until we passed close to a small salt pond. Two swans swam serenely in the pond and almost every morning the same little drama would take place. One of the dogs would plunge into the pond swimming aggressively towards the swans that, in turn, swam in retreat. As this was happening the other dog quietly reached the point on land to which the swans were being driven. The dogs must have been thinking themselves very clever. At the last moment, when the two dogs had nearly driven the swans into their trap, the swans would take wing leaving the dogs staring dumbstruck at their flight. The look on the dogs' faces was, "How did they do that?" It was merely amusing the first time it happened but when it was often repeated, I began to see it as some kind of deep philosophical lesson although I am not sure I can articulate it clearly. Nevertheless, the year in Woods Hole seemed to be full of personal lessons of that sort.

We left Woods Hole to return to Chicago in the fall of 1978 and realized we were going to miss that environment greatly. That led to an exciting period of decision-making. First of all, WHOI made me an offer of a nine-month hard money position funded by the Doherty Chair for 6 months and 3 months of support from the Education program under the condition that I pledge to be involved deeply in the department's education program. Of course, I loved the idea of being so involved, especially in the Joint Program with MIT that I thought of as one of the most innovative graduate education programs in oceanography in the country.

John Steele, who was the Institution's director at the time, assured me that I could think of the offer of becoming the holder of the Doherty Chair in Oceanography as if I were at a university. Perfect, I thought. To make the issue even more complex and exciting, I almost simultaneously received an offer from the University of Washington of a professorship in their School of Oceanography while becoming the Director of their Joint Institute between NOAA in Seattle and the University. Seattle was very tempting. A beautiful and cosmopolitan city nested in a beautiful natural setting. We also had many friends there, Jim and Emily Baker (Emily was a roommate of Holly's at Radcliffe), Jim Holton who shared an office with me as we were both writing our Ph.D. theses under Jule's supervision and others like Bruce Taft whom I had gotten to know that magic summer of 1960 in Woods Hole. A visit that autumn to the University and Seattle only made it seem more attractive. We were torn.

In the midst of our conundrum, I received the reviews of the book manuscript from Springer and they exceeded my fondest hopes. I was particularly struck by one review that forecast that the book would find a place on bookshelves alongside the classic texts of Lamb and Batchelor. I was exuberant at this response. There was a little tussle with Springer over the book cover. They considered the book, Geophysical Fluid Dynamics (GFD) as part of their geophysics series and so wanted to give it their standard clay colored cover. That seemed bizarre to me for a fluid dynamics text and they finally relented and allowed a cover in deep blue.

That final winter in Chicago was very hard. We experienced record cold and record snowfall. The streets were packed with snow and all streets became de-facto one way, so much snow had been heaped to each side by the plows. We put our house on the market because we knew we were going to move somewhere in the next year even if we weren't sure where. The grip of snow on the city and a moderation in the previous hot housing market produced a dearth of interested buyers. We were tense about the prospect of moving with the house unsold.

To deal with our indecision, Holly and I played a psychological game with ourselves. We decided to choose a destination as final and not tell anyone our choice and live with it for a week and see whether, in our hearts, we were happy with the decision. One event that helped us make up our minds was a gift box of used baby clothes in anticipation of our soon-to-arrive baby, sent to us by our friends, Breck and BL Owens in Woods Hole.

When we opened the box, we were struck with a feeling of great nostalgia by the aroma emanating from the clothing. We both burst into laughter when we realized that our warm feelings had been produced by the unmistakable odor of mildew---classic Woods Hole mildew. That certainly told us where our hearts were and we soon after made the decision to accept the offer from Woods Hole. Except for the annual occurrence of a too-cold New England February, I've never regretted the decision. We were going to Woods Hole.

On March 28th, 1979 our younger daughter Dove Helena was born at the Michael Reese Hospital and Julian Goldsmith's wife, Ethel, said to me, "We will miss you but your daughter is a Chicago girl."

We sold the house and closed the sale two days before we pulled away from the curb with the car loaded with children, luggage, bicycles and a perhaps a hamster cage. Our furniture was simultaneously being loaded into a moving van that was to arrive at our new home shortly after us; at least that was the plan.

Our period in Chicago was ending and yet as I wrote to the wonderful Dean of the School of Science at U Chicago, Albert Crewe, the University had made such a deep impression on me that part of me permanently remained behind. My recollection of the University of Chicago, how it was organized around the Faculty and the integrity of its principal academic leaders remains in my mind as the ideal model of an academic institution. The University navigated the difficult period of student unrest in the 1970's with grace and wisdom. I was also leaving behind many good friends on the Faculty. I had become particularly close to George Platzman. On the surface George was very reserved but he had an antic sense of humor and a passion for collecting original musical manuscripts and also original scientific ones. His proudest possession was an original of Newton's Principia that he acquired in London. Larry McGoldrick, Victor Barcilon and the paleontologist Tom Schopf made leaving U Chicago emotionally painful. Tom was the last person in my life to knock on my house door with a baseball glove and ball in hand to ask me if I could come out and play! We went down to the Midway, a stretch of green along S. 57th St. where we played catch and where he taught me how easy the famous Willie Mays over the shoulder catch really was. The hard part was the quick recovery and the throw to the infield.

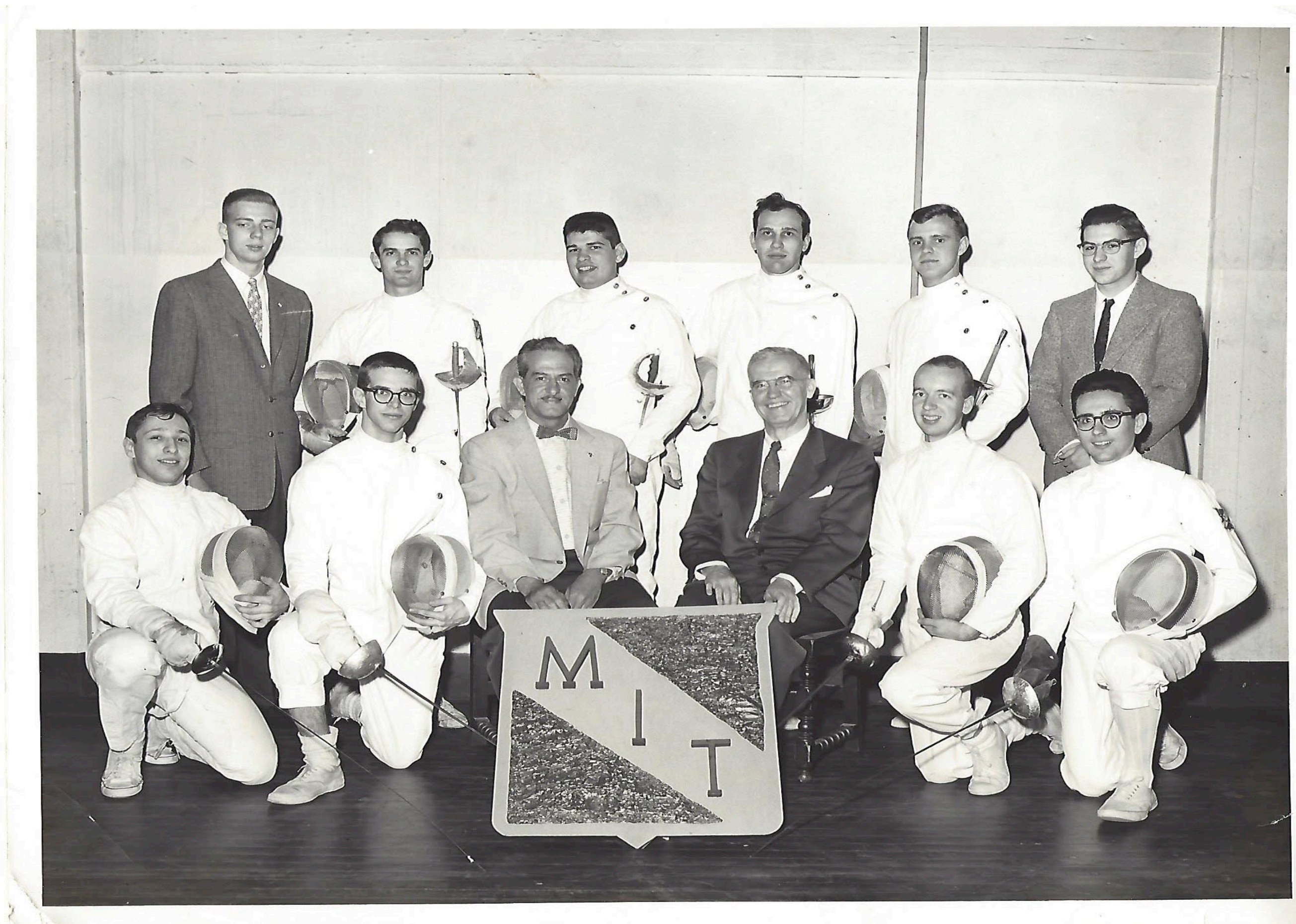
There have been many occasions since when I have used the University as a measuring stick to judge the quality of academic leadership here in Woods Hole, usually in Chicago's favor. I missed Julian Goldsmith as a friend and department chairman and missed the inspirational leadership of Ed Levi for the University as a whole.

It's a mistake, though, to focus on what good things are lost when you make a move like that one. In a telephone discussion I had with Hank Stommel when Holly and I were agonizing over our decision, Hank said something that struck me as very wise. "You can't hold on to everything you have and always add to it. You always have to give up *something* if you try to better your situation". I have found that the remark is deeply true. In moving to Woods Hole there was clearly a tangible improvement in my scientific career but in spite of the cultural riches unexpectedly available in the small Woods Hole environment, the broader, less tangible riches of a great university were traded for them.

Photos:

MIT Senior Year Photo (1959). The lapel pin shows an airfoil and the streamlines around it. It marks my election to Tau Beta Pi the university aeronautical engineering honorary society.

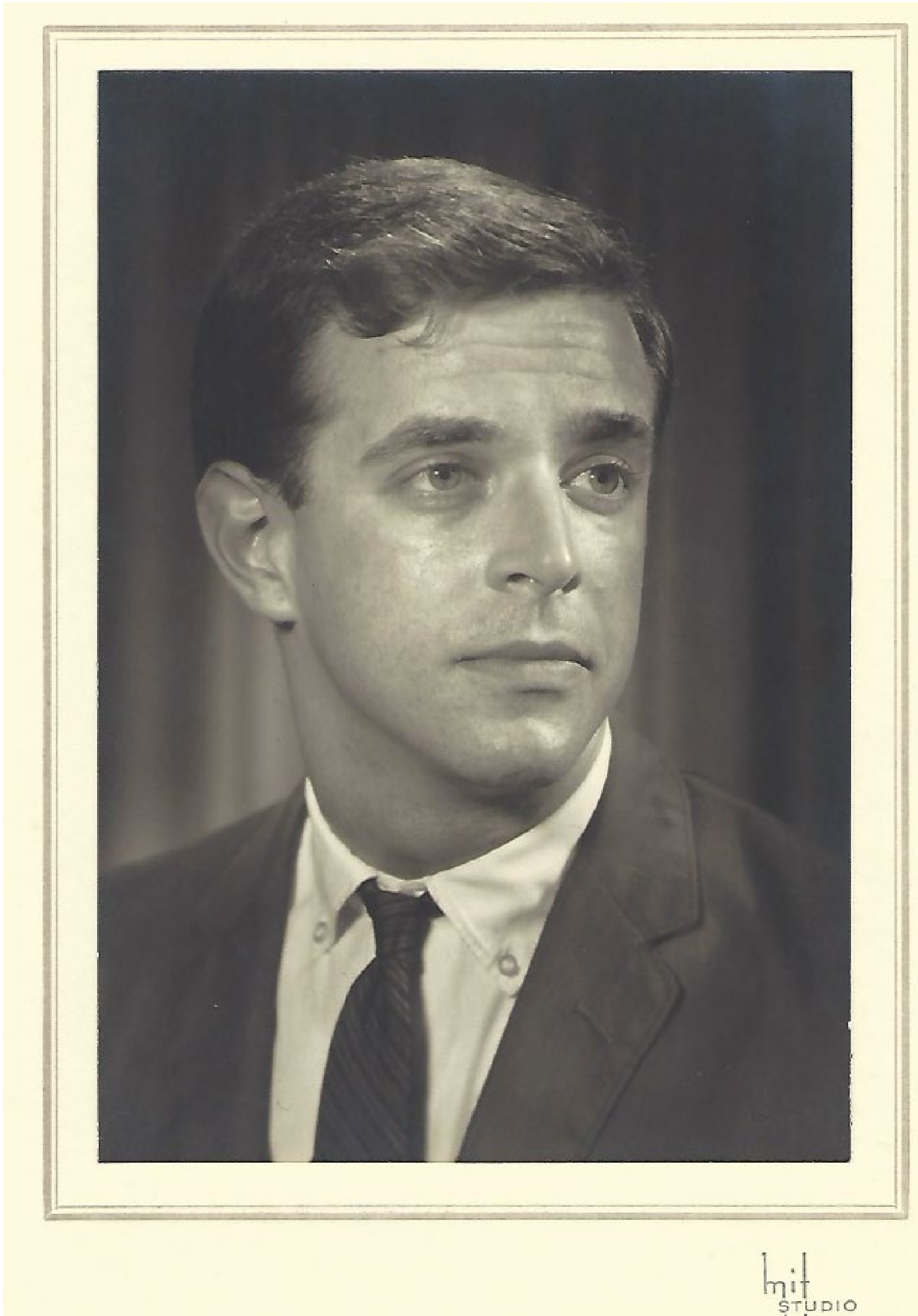
An evening in New York: I am second row far left. The cigarette in my hand is mostly for show. I never inhaled. The girl I am with is again, the same object of my unrequited passion. Some men never learn. Kern Kenyon, one of my college roommates and with whom I travelled to California before my GFD summer of 1960 is first row, 3rd from right.



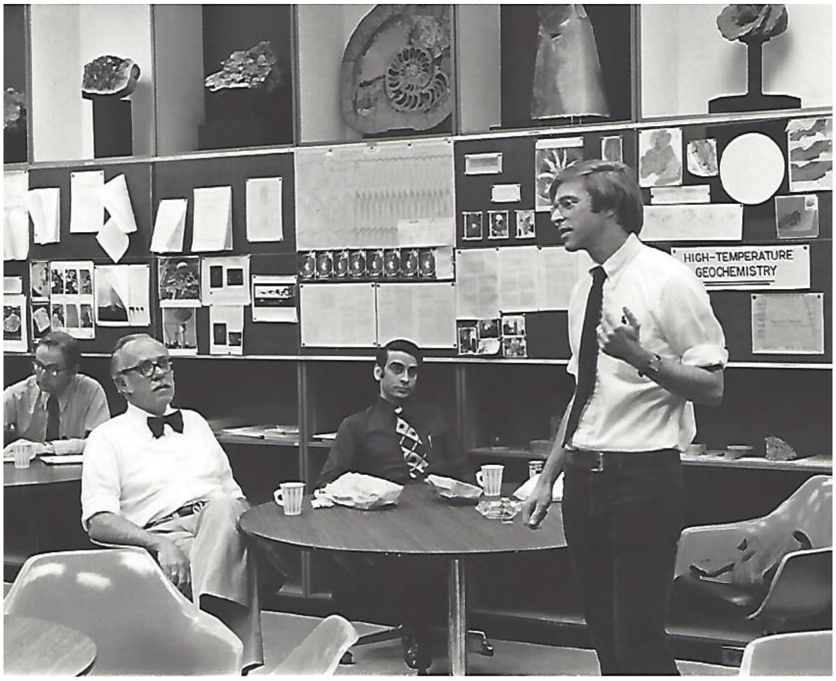
The MIT fencing team (minus one): I am second from left, first row. The fencing Maestro, Silvio Vitale is on my left.

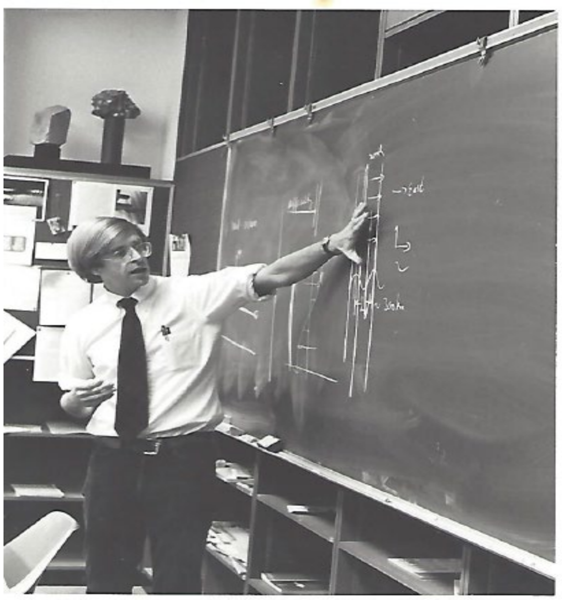
My three favorite Aero professors, Leon Trilling, Holt Ashley and Erik Møllo-Christensen

Our wedding reception: From L-R; my mother, my father, Carin and I. The reception was held in the living room of Challenger house on the campus of the Oceanographic Institution. It is now a business office.



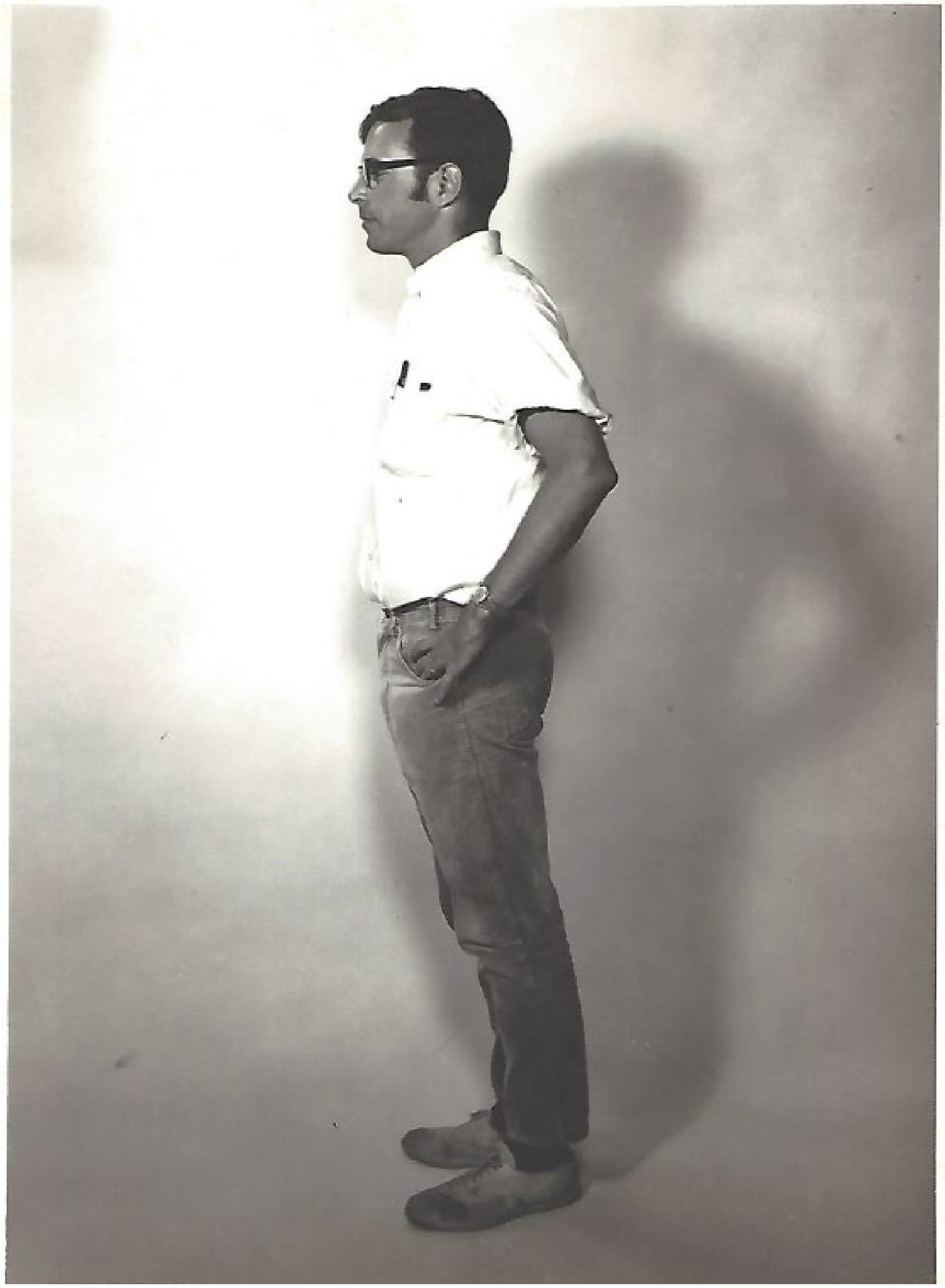
My official MIT Faculty portrait, 1964. My battle with the Massachusetts Loyalty Oath for teachers was about to begin.





At the Geosciences department in Chicago, each Friday noon a faculty member was asked, at short notice to describe recent work. Here, I am describing my ideas of mid-ocean instabilities.

We became great friends with David and Pat Farmer. David was curator of European painting at the Chicago Art Museum (the Art Institute) while Pat was curator of painting for one of the large downtown Chicago banks. Their Halloween parties were legendary and one year I was able to fulfill my fantasy and became a Renaissance prince (for an evening). The plot thickened when a Chicago policeman rang the door-bell to mention an illegally parked car but backed away when I answered the door dressed as shown.



A photo taken by Willem Malkus during the summer of 1970 while I was lecturing at the GFD program. He took the photo at many angles, sliced it horizontally and recomposed the photo as Pedlosky, Rotating and Stratified; the title of my lecture course that summer.

Chapter 8

Woods Hole: A New Life

While Holly remained in Chicago that spring, too pregnant to easily travel, I flew several times to Woods Hole hunting for a house to buy. It wasn't easy. Our first choice would have been a house in the village of Woods Hole but there was nothing available we could afford or if we could afford a place there, it was in such bad shape that the cost of repair would have been excessive. Finally, with the help of the estimable Alice Morse, a real estate agent who had shown possible summer cottages to us when we had entertained (fruitlessly) the possibility of buying a summer place in Woods Hole, I found a house. It was not in Woods Hole but it was located near a salt marsh that Holly was fond of and the house was spacious enough and in good shape. Although it wasn't clear that our house in Chicago would be sold in time for the move because of the brutal winter, I made an offer on the house that was accepted. As it turned out we closed on the house in Chicago in time, and the sale price was still high enough, so that we were in a position to buy the house in Woods Hole without a mortgage. Our very good lawyer, Bob Ament, advised us to take a relatively small mortgage of $20,000 to establish our local credit. Our profit on the house in Chicago was large enough so that no extra cash was needed for the Woods Hole house and so I have always felt that I bought the Woods Hole house for $2,000, the amount of cash I needed to pay to buy the Chicago house.

The first evening we were in the house we had no furniture except a lent card table a couple of borrowed cots but we all immediately walked down the road near our house to the beach and stood watching a starry sky over the water and feeling we had arrived at our real home.

When our goods from Chicago arrived Holly and I worked strenuously to turn the house into a comfortable place for all of us. Holly was delighted to have an area around the house for a garden, to feel physically secure and to enjoy the natural setting and I had to admit, as much as I enjoyed aspects of life in Chicago I, too, felt more relaxed. We had at least two heavy locks on our front door in Chicago, always bolted. Shortly after we arrived in Woods Hole I returned home one day from work to find the entry door we used to be locked and I grumbled, " Who locked the damned door?" We did learn doors needed to be locked but there was no comparison to the background fears for our safety as before.

I had a pleasant but small office in Woods Hole but when Bill Von Arx's office became available it was offered to me and I have been in that corner office with a view of woods and sea for over three decades as I write.

Part of the pleasure of being at the Oceanographic Institution was in the large number of colleagues who shared my interest in oceanography. The Physical Oceanography department in those days of the late 1970's still had the personality of an earlier epoch of oceanography. There were quite a number of established scientists in the department who had never studied the fluid dynamics of the ocean in any systematic way. In fact, there were a substantial number who had no graduate education at all, or if they did, it had nothing to do with science and certainly not fluid mechanics. They were a colorful group. Fritz Fuglister, for example, had been an artist who came to Woods Hole during the Depression to do some post office murals as part of the WPA project. He fell in love with a librarian at the nearby Falmouth Public Library. He stayed and became a draftsman at the lab and started going to sea as a technician and gradually, on the basis of his basic intellect, rose to become a scientist and eventual department chairman. Similarly, Valentine (Val) Worthington had only an undergraduate degree in the classics but on the basis of his observational work became an influential figure in describing the North Atlantic gyre circulation. He, too, became department chairman and was chairman when I arrived. He was a very good and understanding administrator and although he was emphatic in telling me that Newton's Laws of Motion should not constrain his conceptual picture of the ocean circulation, we got along very well. There were several others as well and they made up a small club they called SOSO (Society of Sub-professional Oceanographers). Stommel himself never proceeded further than his undergraduate degree from Yale in Astronomy and was a member of the SOSO community and this didn't prevent him from being the leading figure in Physical Oceanography. He did receive an honorary Ph.D. from the University of Chicago while I was there and I had the pleasure and honor of shepherding him through the ceremonies. The increasing professionalization of oceanography has closed down the club as the members passed away. That professionalization is certainly a sign of some progress but one cannot but feel regret that a certain colorful flavor of those days has been lost.

At first my work was a continuation of my studies of the nonlinear baroclinic instability problem. Although these were still the pre-personal computer days, there was an easy public terminal to link to the central (VAX) computer for the Institution and with online editing now standard I was able to do some problems much more easily. One of the first problems I considered after I arrived was the effect of the planetary vorticity gradient, the so-called β effect on the generation of chaos. I discovered that even a small amount of that effect, i.e. even when its contribution was small compared to the potential vorticity gradient due to the vertical shear of the zonal flow in the two-layer system, eventually expunged the chaotic behavior. This rather dramatic effect was due to the role β played on the evolving phase of the wave so that the amplitude equation was no longer strictly real but had an imaginary part. Looking at the evolution in the phase plane, this introduced a term like an angular momentum effect on a particle orbiting a body while attracted and repulsed by a nonlinear spring. The angular momentum term prevented the orbiting body, i.e. the solution curve, from approaching too closely to the unstable origin, the source of the instability and the chaotic behavior.

I also was able to examine the instability of resonant topographic waves that Charney and DeVore had recently discovered using a heuristic truncated Fourier expansion. I was able to extend and strengthen their analysis using asymptotic methods. Still, my work had not yet taken on a new flavor reflecting my new scientific home. I enjoyed greatly my Woods Hole colleagues and the new focus on oceanography. The weekly Physical Oceanography (PO) seminars were of generally high quality. I was also teaching in the MIT/WHOI Joint Program and I enjoyed the interaction with my MIT colleagues, especially Carl Wunsch, Glenn Flierl and Paola Rizzoli. Although I enjoyed the teaching very much, the course offerings to our students struck me as incoherent. We had no basic fluid mechanics sequence and our students had to take fluids courses in other departments that, of course, emphasized aspects of fluid dynamics special to those disciplines rather than our own. I initiated a series of meetings of the WHOI /MIT faculty and, as at Chicago, we began a discussion of what a core program of courses should look like. I had my own ideas but my principal goal was to get us to agree on the concept of a stable core whose content would not depend on who was teaching the course. In that way people teaching more advanced and specialized courses would know what the students had seen and supposedly knew from their core courses. There was general agreement as to the principle of the idea. I advanced a draft of a sequence of 6 courses and a rough outline of their contents. As anyone who has been in such a position will realize, the person who submits a fairly complete draft generally easily gets agreement on 80% of what is suggested. It was my strategy to accept all modifications that were proposed as long as there was substantial agreement and after a year of meeting, we had reached a consensus as to the core and its content. Naturally, over the more than 3 decades since that time there have been some substantial changes. More recently there has been some, sadly, unilateral changes in a few of the core courses by some of the colleagues at MIT. No human construct lasts forever.

One of the great pleasures of teaching in the Joint Program was the opportunity to interact with such very good students. Over the years there were a number of excellent students each of whom were special in their own way. Perhaps one of the most interesting was the first Phd student to work with me for her thesis in Woods Hole, Lynn Talley. Lynn, an accomplished pianist had to choose between science and music for her career after graduating from Oberlin since she was outstanding in both areas. She chose oceanography and quickly developed and interest in *observational* oceanography. That was what was so fascinating about her choice to ask me to be her thesis advisor. She told me she was quite certain she wanted to do observational work for a career so she wanted the experience first of doing a theoretical research project for her doctor's degree. She did a fine job, wrote and excellent thesis and is now a leading figure in observational oceanography. The list of young men and women who worked with me contains many names and each contributed something rare and positive to my own intellectual development.

The direction of my research work changed dramatically in 1980. At one of the PO seminars, Peter Rhines, recently returned from a sabbatical year in Cambridge England, announced that he was going to give a lecture only reviewing the extant Sverdrup theory for the mid-ocean flow since he did not have a computer available to him in Cambridge for new work for his research. This was a sly and humorously disingenuous introduction. I, and I supposed my other colleagues, settled back to hear what we were sure would be an interesting review of standard material by one of the most innovative people in our field. It started that way but quite soon turned into a presentation of Peter's new work, which he had done with his exceptional student Bill Young, on the problem of the oceanic thermocline. Any notion that the lecture was covering old material vanished and we were presented with some really novel ideas about the ocean circulation.

I think it was Norman Phillips who, a few years earlier, pointed out to me the conundrum in understanding the nature of the oceanic thermocline and the wind driven circulation of a density stratified ocean. On the one hand, what is it that limits the depth of the warm water region to something of the order of 1 kilometer while the ocean has a depth of 4 or 5 kilometers and there has been millions of years for the surface heating in the subtropics to reach the bottom by simple molecular diffusion? The answer to that was rather simple, i.e. that the process of producing a thermocline is dynamic and fluid motions sweep the fluid into the western boundary layer long before the heated fluid could extend to the full depth. The opposite problem was less commented upon. Namely, how did the heating and the wind driven motion of the warm water extend in depth *beyond* the layer directly affected by the wind, a region of the order of only 100 meters. The classical but incomplete thermocline theory envisioned it as a balance between the downward diffusion of heat balanced by a combination of vertical advection of deep, cold water, and the lateral movement and removal of the warm water by horizontal advection. In this picture the thermocline was a type of thermal boundary layer below the sea surface whose thickness depended on the turbulent vertical mixing coefficient of heat and the three-dimensional motion field. This led to a fiendishly difficult nonlinear partial differential equation. There were few useful solutions of the equation and they were typically of the type that required guessing a form of the solution which was structurally the same everywhere and only stretched or compressed in the vertical as a function of latitude and longitude. Worse yet, the solution could only satisfy very unrealistic surface boundary conditions. In particular, these similarity solutions could not deal with the simplest and most obvious case where the surface boundary condition considered a surface applied temperature that was a function of latitude only. These solutions also depended critically and unhealthily on the unknown turbulent mixing coefficient of heat. One approach that avoided a dependence on the vertical mixing coefficient had been suggested by Pierre Welander, one of the most innovative workers in physical oceanography. He abandoned the thermal boundary layer analogy completely and searched for solutions for a completely adiabatic and frictionless thermocline. It still required some guesswork from Pierre because the adiabatic solution preserved potential vorticity along streamlines and that relationship between the streamlines and the potential vorticity should, in principal, be derived from the surface boundary conditions and not specified arbitrarily as Pierre did. Nevertheless, Pierre's solution showed that an adiabatic solution could be found that had many realistic features and did not depend on an artificial and unknown mixing coefficient.

The theory of Rhines and Young took a completely different approach. They considered an ocean of many layers, each of different density, in which the frictional coupling between the layers was so small as to be directly negligible. The wind forcing would put the uppermost layer into motion. That motion would alter the thickness of that layer. Sufficient driving would so distort the isolines of potential vorticity which are lines on which the ratio of the Coriolis parameter, $2\Omega\sin\theta$ (Ω is the earth's rotation rate, θ is the latitude) to the layer thickness $\boldsymbol{h_n}$ of the nth layer. If only the first layer is in motion one can easily calculate how the depth of the first layer and hence how the potential vorticity of the layer beneath it is altered since *its* thickness will thus be altered. If the forcing is weak the isolines are nearly latitude circles and the intersection of the isolines with the eastern boundary disallows motion on those lines that is not directly wind-driven. If, however, those lines are sufficiently distorted and close on themselves even the smallest driving, say by a synoptic eddy field, could give rise to an order one circulation. Once that layer is put into motion, *its* motion will distort the thickness of the third layer and, if distorted enough, can set that into motion. Rhines and Young showed how to do the calculation and, importantly, showed how these domains would become nested one inside the other as one moved lower in the water column. In this ingenious way, they proposed how an adiabatic, nearly frictionless, fluid could have many deeper layers put into motion. In each of the deeper layers Rhines and Young appealed to a theorem of George Batchelor to infer that the potential vorticity within those deeper nested regions needs to be constant and this allowed a specification of the velocity field in those regions. All this was done without the need to specify a mixing coefficient. I remember that the audience in the seminar was deeply impressed by the Rhines and Young suggested solution of the thermocline problem. More to point, the elements of that theory were so elegant that the natural reaction was that we all wished we had done it. As Hank and I rose to leave the seminar room Hank expressed his admiration for their work and I remember agreeing but pointing out to Hank that a key missing element of their theory was the absence of outcropping of their layers since they used a quasi-geostrophic theory that disallows such strong changes in layer depth. I believe that was the beginning of our germ of an idea for the ventilated thermocline.

Still, I believe the truly major contribution that Rhines and Young made was to restore the problem of the thermocline to the domain of fluid mechanics and not just the search for precious and ingenious special solutions of a hard partial differential equation. I am still impressed by the originality of their suggested solution.

I put the problem out of my mind as I was working on a stability problem I was interested in when, a couple of weeks later, Hank came to my office to show me some numerical work he and Jim Luyten, who functioned as a kind of junior colleague to Hank, had been doing. They took the two-layer model that Rhines and Young were using and allowed the second layer's interface with the upper layer to come to the surface within the subtropical gyre. I can no longer remember what numerical routine they used but the results looked interesting and realistic. Hank left the office and I started to think about whether there might be a more direct way of getting an analytic solution. I fiddled with set of simple sketches showing a column of fluid in the second layer being swept beneath the upper layer, subsequently preserving its pv. I can only say that it was natural for me to think in terms of the conservation of potential vorticity and its relation to the streamfunction of the flow. My mentor Jule Charney had developed his theory for the Gulf Stream using just such a technique and I had not long before used the same approach to the nonlinear upwelling problem. The principal challenge was determining just what the relationship was between the lines of constant pressure, that were also the streamlines, and the potential vorticity isolines that coincide with them in the ideal fluid theory. It suddenly occurred to me that if the outcrop line were a latitude circle, precisely the case that had eluded analysis by the similarity/guess solutions, it would be possible to find an extraordinarily simple relation between the potential vorticity of the second layer and its pressure field. It was *so* simple that I was astonished that this fundamental problem of the relation between potential vorticity and the pressure field in the thermocline *had* such a simple solution. This, in turn, allowed an extremely simple solution of the whole thermocline problem for the two-layer model with outcropping. Nothing more than high-school algebra was needed! I quickly wrote up the results and left them on Hank's desk. He was not in his office and I looked forward to his reaction. He called later that afternoon and left a message on my home phone that remarked that the theory must be wrong because it produced an algebraic dependence in latitude via the Coriolis parameter whereas the numerical solution appeared to have an exponential behavior.

I couldn't figure out how the theory, being so simple, could be so drastically wrong, and looked forward to learning more the next day. I didn't have to wait for more than just a few hours because later I got another call from Hank excitedly saying that he and Jim had plotted their numerical results over the predictions of the theory. They appeared to match exactly! Could we talk and could I please explain the how the theory worked? I still remember the thrill of that evening. It was the beginning of one of the most exciting scientific periods of my life. We had really gotten a deep understanding of the inertial thermocline and our theory was so simple that it could form the solid basis for further extensions and elaborations.

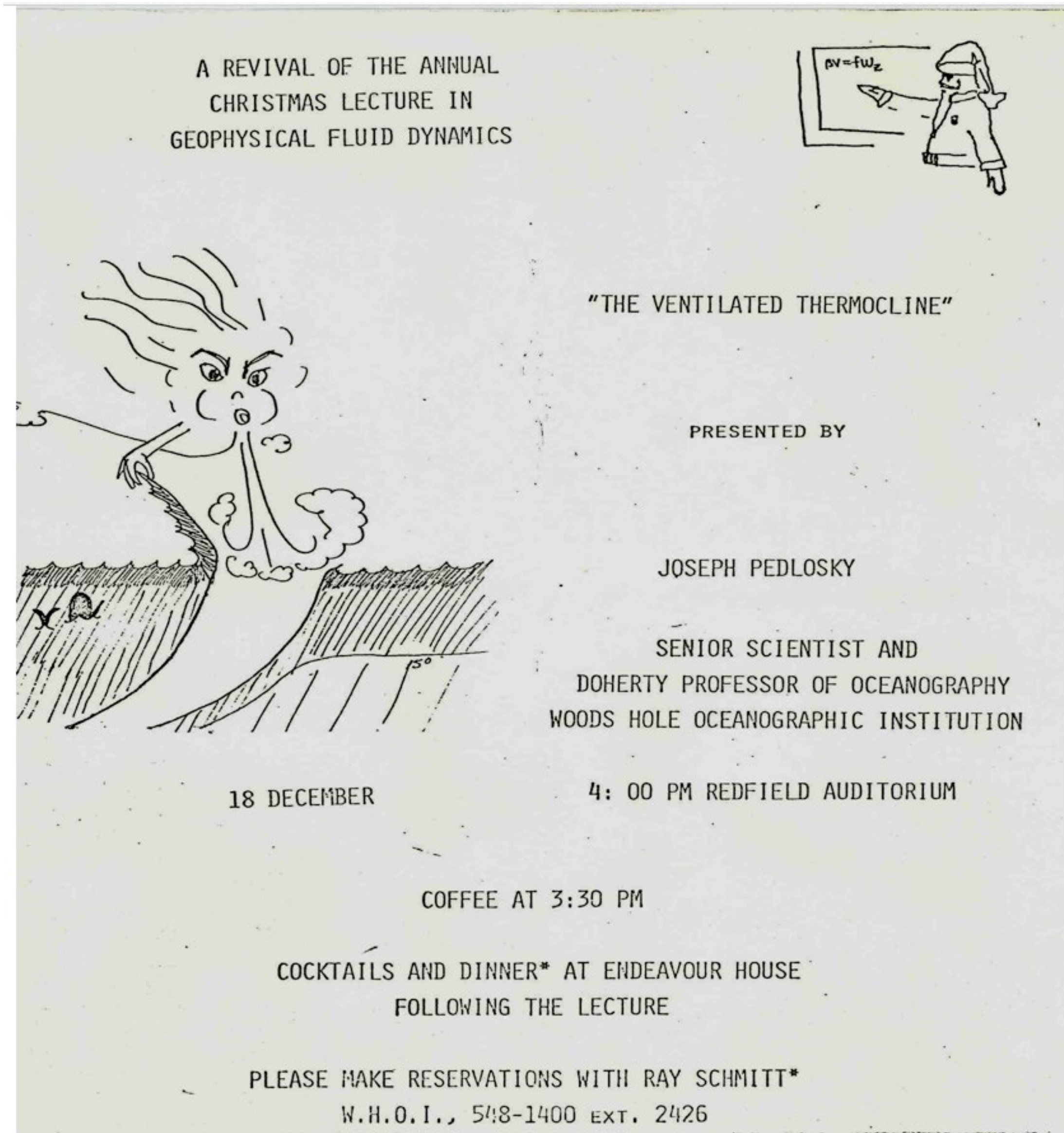
I believe that on one level, Hank was actually disappointed that an analytical model could work so easily. He often felt that doing work that required the computer was a mark of real professionalism and despite the fact that he was clearly recognized as the most productive and creative physical oceanographer of his time, or perhaps of all time, he sometimes felt that lack of a formal Ph.D. credential. So, he kept challenging our analytical solution and this turned out to be very stimulating. First, he asked why we couldn't have a moving layer with a non-zero depth on the eastern boundary. In our initial model the layer interfaces each rise to the surface to satisfy the eastern boundary condition. It turned out not to be difficult and it led to the presence of a special region, the so-called Shadow Zone, in which the lower layer flow was at rest separated from the region of moving subducted flow to the west of the Shadow Zone boundary. I was secretly amused to use the word subduction to describe the process by which the Ekman pumping at the surface drove fluid down into the thermocline. I borrowed the word from the seafloor spreading and mantle convection work I had done with Frank Richter at Chicago. The term was naturally accepted in oceanography without a murmur.

In addition to the Shadow Zone it was clear that one of the streamlines issued from the intersection of the outcrop line and the *western* boundary if the outcrop line was near enough to the zero line of the Ekman pumping and this delineated a region that could not be reached by any fluid subducted from the outcrop. Fluid within that contour seemed to be emanating directly from the western boundary. We therefore proposed on physical grounds that the fluid, recirculating through a western boundary current (which was not part of our solution) would, a la Rhines and Young, evolve to have uniform potential vorticity. One thing that pleased me enormously about our solution was that it was *not* a similarity solution. The structure of the solution was different in each of the three regions of Subducted fluid, the Shadow Zone and the Pool of Potential Vorticity (which had a sort of New Testament ring to my ears) and put a final stake in the heart of the special solutions that had been found of the previous theories that assumed self-similarity in all regions.

Hank's final challenge was his doubt that the analytical solution could go beyond the two-layer model and so I worked out the rather tedious three-layer model. The three- layer model naturally generated more special regions and I need not describe them here but it was clear that there was no limit, in principle, to the number of layers we could use and so approach a continuous model of the thermocline. In fact, our colleague, Rui Xin Huang, shortly afterward developed an ingenious computer version of the analytic layer model that could increasingly refine the vertical resolution with an enormous increase in the number of layers.

At about this time Hank wandered down to my office and said with a ring of great satisfaction in his voice, " No one will ever think about the thermocline the same way again". It was not said boastfully; it was more in the nature of his measure of satisfaction that we had done a very fundamental piece of work that moved the field in a new direction.

We sent our paper off to be published. Hank got considerable pleasure that we listed the authors alphabetically. Jim Luyten was up for promotion to Senior Scientist and being listed as first author on the paper would certainly help his case. Hank had a strong, rather paternal affection for Jim and at that moment we all felt warmly about each other. The Christmas holidays were approaching and we decided to give a public lecture about our work. Nancy Copley designed an announcement with a cloud-like figure pulling away the surface of the ocean while puffing a blast of air into “The Ventilated Thermocline”, the title of our paper.



A local production of Amahl and the Night Visitors allowed Jim, Hank and I to put on crowns I had borrowed from a participant in the opera and, to the musical score of “We Three Kings of Orient Are”, we were able to add an encore to the seminar in verse:

We three kings of geostrophy are
Tracing streamlines that travel far,
The wind-driven gyre, (but not quite entire)
Following yonder isobar

(chorus)

Oh, Oh, isobar of ideal lubricity, (Joe)
Isobar of potential vorticity,   (Jim)
Westward swerving, still conserving (All)
Guide us to beta helicity. (Hank)

The three Kings of Geostrophy

I was uncertain beforehand whether I could persuade Hank to join Jim and me and put on a silly crown and sing a silly song I had written, and I was amazed when he not only agreed, but also agreed enthusiastically. I think that was the proper measure of how happy Hank was with the work we had done.

It was a happy time and the fundamental simplicity of theory met with widespread acceptance.

It is therefor, somewhat amusing to report that the reviews of the paper we submitted were less than enthusiastic. One reviewer criticized it for being a “special and limited solution”. Still, the paper was accepted.

Hank sent preprints of the paper to a number of colleagues in the US and abroad. Sometime after he did that, I got an early Sunday morning telephone call from Hank who asked in an agitated tone of voice if I had seen the most recent issue of Ocean Modeling, a gray literature journal put together by Peter Killworth. I replied that I hadn't since my copy was sent to my office and not my home as Hank's was. I said I had not and he replied I should see it right away and that he would be right over. A few minutes later he was at the kitchen door in a rather agitated state. I was still in my pajamas and invited him in for a coffee and he replied, "No, no, you please take care of this" and thrust his copy of Ocean Modeling into my hands. What had disturbed Hank was the lead article in Ocean Modeling in which Killworth, its editor, attacked our solution with the Shadow Zone and claimed that any solution with a non-zero eastern boundary layer thickness for a moving layer had to be wrong. He then presented an ingenious argument. Expanding the solution in a Taylor Series from the eastern boundary and using the equations of motion he could show that each term in the Taylor Series in that layer had to be zero if the depth on the eastern wall was non-zero. When I first read the article, my heart pounded in the anxiety that we might have made a brutal mistake although I couldn't understand where that might be. Then it struck me that the Taylor Series expansion was only valid up to the first singularity of the function and/or its derivatives. Of course, the Shadow Zone boundary was the line separating resting from moving fluid so at that boundary the layer thicknesses suffered a discontinuity in their first derivatives. So, what Killworth had done was only prove precisely that the Shadow

Zone had to be at rest! This had been already clear from physical considerations. I called Hank with the reassuring news.

Hank's first reaction to the news was relief and then he became angrier than I have ever seen him before or afterwards. The fact that Killworth rushed his criticism into print without first contacting us, especially as his criticism was unfounded, struck him as unethical. He wrote a letter to Killworth that Jim and I co-signed, and his anger turned to outrage when Killworth suggested we reply by writing a letter to his journal as part of the correspondence dealing with the "controversy", as Killworth called it. Hank was so angry he sputtered that he would be damned if he would play into that and wrote to Killworth that there was no controversy only Killworth's mistaken criticism.

The theory has held up well for 35 years to this point but the lesson learned again is that *nothing is easy*.

One result of working so closely with Hank was the pleasure of drawing closer to him. It was natural for him to strike up friendships and deepen them through working together. He became increasingly candid to me about his opinions about other people and some of these opinions astonished me.

When Hank turned 60 in 1980, he was feted by a wonderful party in Falmouth and the publication of 3 volumes of essays and reminiscences by his colleagues. It was a happy joyous moment and many colleagues from around country came to participate. Among them was Jule Charney who stayed over that weekend at our home. Jule was in apparent remission, after surgery, from a serious lung cancer and was still suffering the debilitating effects of the chemotherapy that he had recently undergone. There was so much affection shown for Hank at the party that I wondered subsequently whether Jule was annoyed that his own 60th birthday had passed largely unrecognized three years before.

During this period of his remission Jule was often in his MIT office continuing his research and I recall that once he gestured towards some correspondence on his desk and said with a note of bemused resignation in his voice that after he received his cancer diagnosis he was determined to focus on only important things in life. “Now that I seem to be well again”, he said, “ I find myself going back to doing the same damn routine things that took so much of my time before”. Of course, I was delighted that Jule seemed to be over his bout of the disease, which only illustrates my ignorance of the nature of the disease. I would learn more about that later.

Ted Shepherd was Jule’s last Ph.D. student and during the spring of 1981 his thesis defense was scheduled at MIT. I was also on Ted’s thesis committee and so had travelled up to MIT to participate in the exam. Ted was an exceptionally gifted student and there was going to be no problem defending his work but it was an important rite of passage. Ted and each member of his committee gathered in a classroom near Jule’s office at the appointed time that I believe was in the morning.

That is, all of us except Jule, his advisor. It was not unusual for Jule to be late but as time grew later and later, we developed some concern that something was amiss. I stepped out into the hallway with the intention of telephoning Jule to see if everything was OK. As I entered the hallway, I saw Ed Lorenz in the hallway and he beckoned to me and said that Jule was on the phone and wanted to talk to me. Thinking that there was some minor reason of his delay I affected a humorous tone of voice and asked, " Jule, what's cooking". His chilling reply was, " My fever is cooking".

Jule was hospitalized right away and was in very bad shape. We spoke on the phone but he was adamantly resistant to the idea of a visit. I wish I had visited anyway because he did not hold on much longer. However, during that period after the resurgence of the cancer until his death, Jule was deeply troubled by one thing and we had several lengthy phone conversations about it. In the volume, Evolution of Physical Oceanography honoring Hank on his 60th birthday, there was an essay written by George Veronis that deeply offended Jule. Aside from a gratuitous remark about Jule's forgetfulness that might have possibly put Hank's draft of his book, The Gulf Stream, in peril, he also implied that the basic idea for the inertial theory of the Gulf Stream, one of Charney's most significant oceanographic contributions, was in fact, Hank's idea. Jule in his last days was tortured by the idea that he had been portrayed as a computational assistant to Hank whom, Veronis said, had suggested the problem to Jule and another mathematician George Morgan at Brown. He talked to me heatedly about what he considered to be a slur in print and was deeply troubled.

I have to say I was astonished. It is true that Veronis' remarks were ungenerous (Charney was another scientist on George's enemies list) but given the immense and magnificent nature of Jule's contributions I couldn't understand his focusing on this one paper on his deathbed. I have not yet had to face the experience of approaching death myself so I suppose I am not in a position to make that judgment but I was troubled that Jule was mentally so suffering. Jule's Gulf Stream paper on the subject concluded with a warm and generous acknowledgement of the conversations he had had with Hank about the problem and how it could be said that "the problem solved itself" as a consequence. I was always stirred by that acknowledgment picturing two of my heroes linked in a heroic scientific friendship. "So", I thought to myself, " this is easy". I would go to Hank, describe Jule's agony and ask him to talk to Jule and put his mind to rest on the subject. When I broached the issue with Hank I was astonished by his response. " I won't go", he said, "it won't do any good". I then found myself in the middle between them carrying messages back and forth and I became deeply depressed by the whole business. It became clear that for all these years since Jule's publication of his Gulf Stream paper in 1955, there had been a bitter falling out between the two of them. Hank thought he had contributed so much to the idea that he should have been asked to be listed as a co-author of the paper and Jule was equally adamant that the idea of a frictionless current was his quite independently. Hank had, in fact, tried to construct a frictionless model of the current but was limited to a constant potential vorticity model that was not able to close the mid-ocean flow through the boundary current; a key ingredient in the theory. It was also clear that Jule's mathematical approach was his own and to what extent Hank's previous effort suggested that attempt is something we will never know for sure now. Carrying messages back and forth and attempting a diplomatic rapprochement was more than I could accomplish. I was in the end astounded that my two heroes, indeed quite justifiably considered scientifically heroic figures, had lived all that time with such a feeling of bitterness between them. Since I was fond of and intimate with both of them I could only marvel how complicated the human psyche could be.

Jule passed away shortly thereafter. He was only 64. It was for me like losing a second father. When I recall the effect Jule had on my career and my scientific development, one of his characteristics that impressed me the most was the generosity with which he supported the scientific efforts of his colleagues. When I first became aware of the work of Ed Lorenz, and this was before his work on deterministic chaos, I mentioned to Jule that I was concerned about Lorenz's use of arbitrarily truncated Fourier series to represent the solutions of complex nonlinear problems. Jule responded with warmth that I did not yet understand what an intuitive as well as mathematical prodigy Lorenz was. He emphatically maintained that Lorenz had the soul of an artist and that I should realize the great value of his contributions. Of course, he was correct.

Similarly, in the early sixties Akio Arakawa had developed an ingenious way of representing numerically the advection of vorticity in models of atmospheric flows so that important properties of the motion, such as energy and enstrophy[♦] would be conserved. It previously had been difficult to have the conservation properties of those quantities preserved in the numerical calculations leading to grievous errors that sorely limited their meteorological capabilities. Arakawa's technique heals that fundamental deficiency. After Arakawa had given his seminar outlining his new method, Jule turned to the seminar audience and praised Arakawa unstintingly remarking that he hoped we all understood what an important advance this was. Of course, Arakawa glowed during Jule's encomium.

[♦] Enstrophy is half the square of the vorticity, a positive definite measure of the fluids vorticity (elemental spin), i.e. its relation to vorticity is analogous to the relationship of kinetic energy to momentum.

For the next few years I worked on consequences and extensions of thermocline theory as well as some ideas on stability theory. But genuinely new directions were a few years off. In the meantime, a new family adventure came to pass. As 1983 came to a close I felt the need for a sabbatical year away from Woods Hole and at first thought that another year in London might be enjoyable and scientifically profitable. My year in London in 1967-68 had been darkened by Carin's mental illness and the thought of enjoying the city with a healthy family was attractive. Fate intervened. One of my MIT colleagues, Professor Paola Rizzoli, mentioned that there was a small oceanographic lab in Venice, on the Grand Canal, where I would be a welcome visitor. I had been in Venice once before and had fallen in love with Venice, as who hasn't? It seemed like a provocative idea and Holly was game to try it. My idea was to use the sabbatical to do a second edition of my book, Geophysical Fluid Dynamics including new material (like the Ventilated Thermocline ideas) and to correct typographical errors in the first edition. I had also taken some Italian classes in the '60's in Boston when I was at MIT and the adventure sounded like a dream. A Guggenheim grant made the dream a reality and so in the fall of 1984 Holly, Dove and I flew to Venice; Anna had preceded us by a month.

Photos: Family life and travels.

Holly 1970

Top: Holly at the beach in Woods Hole. Below: Holly, Anna and I in Woods Hole in 1974.

In our new home in Falmouth after moving to Woods Hole. Holly, Anna, Dove and Joe

My mother, Lillian, paying us a visit in the early ‘80’s.

We were as happy as this picture shows. On our Saconesset beach around 1985.

Ca` Papadopoli on the Grand Canal, Venice where my visitor’s office was for many years from 1984

Dove and Joe in 1984 from the Basilica of San Marco overlooking the Piazza

During one of our summer stays at Villa Cipressi in Varenna where Holly taught her photo workshops. The girl next to Holly is Dove's cousin Megan.

As Holly films in Varenna, I try to be helpful and amuse the subject.

When Dove graduated from Boston University (with high honors) we were touched when Alberta Bianchini, Dove's pre-school teacher in Venice, and her husband, Memi, flew to Boston to be present at the ceremony.

One of the pleasures of the clarinet was playing in Falmouth's Town Band. Here I am shown with a more expert clarinetist, Joe Olivera. We were both in the first clarinet section but when the going got rough Joe O would cover my mistakes.

Chapter 9

Italy!

Holly and I had made a short reconnaissance trip to Venice in the spring of the year to find schools for our two daughters and an apartment to rent for the year. During that visit we stayed in a hotel room over a very well-known restaurant, Montin's. We found Venice to be beautifully enchanting and the people we met were warm and friendly. One day while I was visiting the lab where I would have an office for that year, Holly followed up a tip on an apartment in the Cannaregio section of town, a section that was typically Venetian and not yet submerged in tourist traffic. The apartment was large, bright and airy and belonged to the family of Countess Foscari. The apartment had belonged to her father and he was anxious to rent it out after his wife had recently passed away. The Countess told Holly that the apartment would be rented unfurnished and for a rather high sum, both of which were deal breakers. As Holly was about to leave the Countess asked Holly how we decided to come to Venice and when Holly replied that our friend, Paola Rizzoli, had recommended it, the Countess exclaimed, " Lei e` la mia migliore amica" (she is my best friend!). The furniture stayed, the price came down and we had found a home for the year. Indeed, we rented that apartment for that year and almost 10 summers afterwards.

Jule Charney's companion, Patricia Peck, was living at that time in Venice and she was a wonderful guide and advisor for all things Italian and Venetian. She recommended restaurants and concert settings and introduced us to her Venetian friends so that when we arrived in Venice in the autumn of that year, we felt we were, to some degree, returning home.

The two girls went to Venetian schools. Dove, who was 5 at the time, went to a “Scuola Materna”, a Venetian pre-school. The young girls had to wear white smocks, grembulie, that covered their clothes and Dove was very proud on the first day of school to be properly clothed for school, just a short walk from home. She was somewhat dismayed to realize everyone was speaking Italian but she quickly latched on to one of the teachers, Alberta Bianchini, who spoke English. Of course, after a matter of a few months Dove was doing fine in Italian but the transition wasn’t easy and on some evenings we could hear her in her room softly practicing one of the Italian songs she learned at school, “The serpent’s dance”. Alberta and her husband Domenico (Memi) became two of our closest friends in Venice and until their deaths remained important figures in the lives of all four of us.

Anna, who was originally resistant to the idea of a year away from her American high school friends was persuaded to go to Italy a month before the rest of us and stay on the farm of a family who were connected to friends we had made in Falmouth, Rick and Paola Price. They had a daughter Anna’s age and the original idea was that the following year we would host their daughter, Elisa. Although that never came to pass, we did become good friends with the family. We had visited the farm and family in Holly’s and my earlier visit in the spring and we liked each other immediately. I scored a hit with my primitive Italian gained from a few lessons with Paola that Holly and I took. When asked by some member of the family whether I would like to visit the burial place of Mussolini who came from that area, I replied to their evident approval that it was enough for me to know he was safely and permanently buried.

When we were thinking about schools for Anna, who turned 15 the year of our move to Venice, it seemed that a Linguistic High School might be the most comfortable for her. In fact, Anna showed great facility in picking up the language. When we arrived to fetch her from the farm family, she was already fluent in Italian although her fluency was limited to the present tense. Her Italian only got better as time went on and she had a good year in Venice and considered, Amadea, the farmer's wife, her "Italian mother".

Prior to the arriving in Venice I had received a very courteous letter from the Director of the lab asking me if I needed any special equipment or other material. I was touched by his hospitality and replied that since my goal was preparing a second edition of my book, I had need only of a good library, which I was certain the lab possessed. I was unaware that the Director was deeply unpopular among the scientific staff. The reasons were multitudinous and connected with the political situation in Italy, the threat of left and right wing terrorism, and the Director was charged with keeping a lid on possible trouble in the lab. I never noticed anything that might have caused such concern although a few people were politically active on the left.

The office I was assigned by the Director was a small office in one of the mezzanine floors of the palace. It was barren, cold in the winter, with a single small high window. Next-door was the office of a few technicians who appeared to spend their time loudly arguing, singing and occasionally drinking. Nothing bothered me. I felt I was in paradise.

I made good friends with several of the scientific staff. Andrea Bergamasco became a great friend and the natural clarity of his Italian was a great help in my attempt at mastery of the language. He was also the person whose oceanographic interests were closest to mine. Another good friend I made was with Luigi (Gigi) Cavaleri, who specialized in the measurements of wind-driven surface gravity waves. Stefano Zechetto, who commuted from Vicenza daily was a particularly warm personality and we often visited him and his family at their home. The secretary for the lab was an American lady, Jane (Frankenfield) Zanin, married to an Italian and she was a great help to us in settling in.

One of the rules for government laboratories was that they had to provide a cafeteria for mid-day meals. Since many of the labs, including the one in Venice, were in buildings that were unsuitable for a cafeteria, the employees were given tickets called “buoni” because they were good, for a small price (about the equivalent of a dollar in 1984), in local restaurants where they were accepted as payment. That was my introduction to a very Italian activity: “beat the system”. I had asked whether I could join them when they went for lunch at the local restaurant, The Madonna, one of the best in Venice, especially for seafood. The reply was that they would have to check. At first, I thought it would be up to the Director, but no, it was a decision made in Rome but I was told not to worry. The decision when it came was that I could not be given my own ticket book but I was told again that I was not to worry. There were many employees at the lab who also were in the same situation but everyone who wanted to dine at the Madonna could. It turned out that, in those days, the buoni were only good in restaurants; they couldn't be used for other purchases, not even food. So, several of the staff who went back to their homes for lunch had unused buoni and each day you could ask someone who was going home if you could use their buono and their name. With the buono and 1,900 lire (about a dollar in 1984) you could enjoy a four-course meal: first a pasta dish, then a main dish usually of meat, then a salad or other vegetable, a desert and espresso. Abundant wine was served with each meal. It was the task of the most recently hired junior scientist (that year it was Andrea) to keep track of the buoni to make sure they matched the number of diners and that the legitimate owner of each buono was correctly identified. So, we "abusers" (abusivi) would turn our tickets in and respond to the surreal question, "Who are you today?" The meal was worth the daily hunt for a name that was "unused". It was great fun and the food was terrific.

Again, the project of writing was deeply satisfying. There were sections of the book that needed rewriting and/or correcting. There were new sections I wanted to add, for example, a discussion of our new ideas of the thermocline.

I worked each weekday but took advantage of the compactness of the city to do some sightseeing, either on the way to work or after work before heading home. From time to time I would meet Holly for lunch at one of the little restaurants we liked so much. The dollar was so strong that year that we felt comfortable dining out regularly. We spent a good deal of time together and enjoyed each other with a new sense of deeper intimacy.

Holly had found a photography group in the Giudecca island in Venice and gradually developed plans to give photo workshops in Venice that became both a motive and a means for us to be able to come back regularly to the city we both had grown to love.

The city is full of art and its easy accessibility was a delight. I remember one cold, cloudy day in the autumn I took a detour on my way home to our apartment in rio S.Felice in the Cannaregio section and went to the Frari church. The church is noted for its two paintings by Titian, especially its magnificent altarpiece showing the Assumption of the Virgin. Almost by accident I wandered into the Sacristy and discovered what became my favorite painting in Venice, a painting by Giovanni Bellini, one of his Sacra Conversazione. The painting glows and it has been in its current location for about 500 years. Although I am not a religious man, I found the painting inspiring in its beauty and serenity.

There were many such personal excursions. Holly used to tease me that I, a Jew, spent more time in Christian churches than she or anyone she knew. There were the parish churches of Tintoretto, Veronese; the Scuole or clubhouses of the various Confraternities that played such an important role in historical Venice, and, of course, sites like the Doges' palace and the Basilica of St. Marco. The list is nearly endless. The art museum, the Accademia, was an especial delight, especially the rooms with the Carpaccio paintings of the story of Saint Ursala in which her home in Brittany took on the flavor of a dream-like Venice.

And, there was Vini da Gigio. It was a neighborhood *osteria*, a bar and a rudimentary restaurant, but the new owners, Paolo and Laura Lazzari, brother and sister, were determined to improve it and Holly and I found the atmosphere welcoming, the food good and the prices reasonable. Since the restaurant was just down the street from our apartment in Rio S. Felice, it became our favorite. That winter in Venice was an unusually cold one and the heating system in our apartment failed and it was some time before it could be fixed. One afternoon when the girls were at school Holly and I decided to go for lunch at Gigio's and explained our plight to Paolo. His reaction was immediate; "our kitchen warms the restaurant" and then indicated the electric heaters then used to heat the dining room. "Take the heaters and return them when your "riscaldimento" is fixed! It seems in retrospect that similar experiences of friendliness and generosity occurred regularly. The restaurant remains my favorite in Venice.

We made friends with an older couple, Ernesto and Rosa, who ran the small outdoor fruit and vegetable shop near our house. We each developed a fondness for certain coffee shops (and the coffee). It was a splendid year.

My life at the lab was equally magical. Housed in a beautiful palace on the Grand Canal, Ca` Papadopoli, it is now a luxury hotel. In spite of the cramped nature of my office I enjoyed my time there largely because of the friendly staff. Remembering the tactic that Charles Keeling used in Stockholm, I asked the Italian staff to speak to me only in Italian. They then asked me if I wished to be corrected when I made language mistakes and I assured them I did. As a consequence, my Italian rapidly improved and that added to my enjoyment of my time at the CNR lab. I believe the other people in the lab were aware of the undesirability of my office but the fact that I had embraced it without complaint stood me in good stead. Indeed, one day a large group burst into my office and started singing. The reason? It was my Saint's name day and required celebration!

As the year wore on, we began to travel in Italy. I went numerous times to Bologna to visit the meteorology department there. We also went as a family to Pisa, Florence, Siena and, of course Rome. Towards the end of our year I was invited to the oceanographic lab at La Spezia and we took that opportunity to visit Liguria and Cinque Terre. Everywhere we went we were overwhelmed with the beauty of the countryside and the art and architecture of Italy.

During the year Holly and I took very inexpensive courses in Italian for foreigners taught at the Venetian University and that helped our Italian and it was something that was fun to do together especially as we were normally the oldest folks in the class. We discovered that the younger foreigners had a less positive view of Venice and we grew to believe that the fact that we were a family and that our daughters were particularly winsome certainly helped us be accepted as Venetians "in training" rather than visiting strangers.

One day in April of that year (1985) when I entered the apartment after walking home from the lab, Anna met me in an excited state. "Daddy", she said, "Jerry Namias called and said to tell you that you have been elected to the National Academy!" This was a moment that put the cherry on the sundae of the year that we were enjoying. I had watched a few of my colleagues like Peter Rhines and Carl Wunsch attain membership in the National Academy of Sciences and although I easily recognized the merit of their elections, I had to confess to a feeling of envy and disappointment that I had not been. So, when I got the news from Anna and I relayed it to Holly when she came home, I told it to her with the phrase, "Finally! Now I don't have to think about it anymore". I think one is supposed to modestly express surprise at such news but I felt it more as a relief and a validation than anything else.

Then, as the anxious person that I am, I began to wonder if it were someone's idea of a joke but was reassured when a congratulatory telegram from my friend Carl Wunsch arrived. Holly told all our acquaintances in Venice and I was so deeply moved by her happiness and pride in my good fortune. The year seemed filled with golden experiences and we bonded so firmly with Venice and its people that it was the beginning of a lasting relationship with Italy. In subsequent years we would go back to Venice for at least a month, sometimes for the whole summer. Holly began teaching photography workshops for Americans, mostly middle-aged women, through the International Center for Photography in New York. The program helped pay the rent for the summer and gave both of us a structure to our summers spent in Venice.

In 1988 I was invited to give a lecture at a summer school run under the auspices of the Italian Physics Society and its Enrico Fermi program. The course was held in the lovely Villa Monastero in Varenna on the eastern shore of Lake Como. I had been invited for the full two weeks of the school but I was reluctant to leave Venice for such a lengthy period. When Holly implored me to accept for the full period, I questioned why should we give up the marvels of Venice for a vacation spot on the water given that we lived full time in just that environment in Woods Hole? I agreed to go for a shorter period. It was one of the stupidest decisions of the many I have made. When we got to Varenna and saw just how beautiful it was I sheepishly had to apologize for my insistence of a short visit.

Holly discovered an equally beautiful villa next door, also along the lake, named Villa Cipressi for its many Cyprus trees. She quickly learned that the villa belonged to the town and that she could rent rooms in the villa for workshop students and from then on until her death in 2012 we would go to Varenna every other summer from our base in Venice until Varenna, as Venice, became a second home to us. We became friends with the locals and appreciated the local beauty and culture as a supplement to the beauty of Venice.

Of course, other cities of Italy attracted our attention and were visited as well and as our familiarity with Italy grew so did our understanding of the subtleties of Italian life in the public and private spheres. The former was frequently dysfunctional while the latter consisted of many tactics invented to circumvent the obstacles of the former. As an observer it was frequently amusing to experience both, while for inhabitants it was often exasperating but accepted as immutable.

We returned to Woods Hole and Falmouth with a better sense of our place in the world and our daughters returned with a view of the world deeper than most children their ages. For Dove, our younger daughter, the return had temporarily traumatic overtones. When she enrolled in first grade, she realized that the other children her age had learned to read in kindergarten. Confronted with her (temporary) inability she became convinced that she was “stupid”. When I tried to point out to her that she spoke a foreign language while none of her friends could, her response was that it wasn’t important. Of course, with a little extra tutoring at home she quickly caught up and ended her high school career as class valedictorian but that was off in the distant future.

Chapter 10

The Equator and other lessons

When we returned home from Italy we fell more or less into the rhythm of our lives before with some significant differences. The year in Italy had changed the whole family. It gave Holly a stronger sense of desire to deepen her commitment to photography and she worked hard to find ways in which her teaching of artistic photography could help us return frequently to Venice, the city we had grown to love.

Our two daughters found the reintegration into their American school lives more difficult. I already have mentioned Dove's initial struggles with reading but the social aspects of the return were more difficult for Anna. She felt somewhat isolated from her classmates who could not easily understand the significance of her year in Italy and she became saddened by a lack of friendship from the other girls in her class. She put on weight and that added to her social isolation. Family life was still good and she eventually found her way but it took some years and a happy college career to really point her in a good direction.

In my own case I was happy to be involved again in the life of the department. I continued my work on nonlinear instability theory and with a French colleague, Patrice Klein, whose numerical modeling skills were exceptional, we were able to push the theory of baroclinic instability more deeply into the nonlinear regime and demonstrated, among other results, the inadequacy and sometimes misleading behavior of truncated nonlinear systems in which only a few spatial modes are used to describe the fluid when, more naturally, many modes are excited. The inadequacy was especially surprising when the additional modes contained relatively little energy. The higher modes with tiny amounts of energy did change the temporal behavior of the larger scale modes through their interactions in some cases rendering "interesting" chaotic behavior much more regular in the fuller system.

A new direction for my work occurred in an unexpected way. I think it was in 1986, attending a meeting of the American Geophysical Union (AGU) at its annual meeting in San Francisco, that I heard Harry Bryden talk about his recent work with his Joint Program student, Esther Brady. They had carefully analyzed the motion in the Equatorial Undercurrent, a subsurface current in the equatorial zone, i.e. the region within 2 or so degrees of latitude on each side of the equator. The major conclusion they came to was that the rising motion of water in the top 100 meters or so occurred as water flowed along surfaces of constant density that slope upwards from west to east. The traditional picture of this region of equatorial upwelling usually depicted the motion in the latitude/vertical plane so it appeared that the fluid was *crossing* density surfaces. That, in turn would require mixing of density and render the motion non-adiabatic. The power of a careless pictorial representation of reality places a scratch on the mind that is often irremovable and once the brain absorbs the picture of a mixing induced upwelling the governing dynamics is naturally *assumed* to be non-conservative requiring a recipe for the mixing. The Bryden/Brady picture instead suggested the possibility that a thermodynamically conservative dynamical model could be apt with all the simplifications that allows. I immediately thought of how such an equatorial model could be connected to the mid-latitude thermocline theory Stommel, Luyten and I had developed a few years earlier. I remember that my colleague, Nelson Hogg was sitting next to me during Harry's lecture and I could not but whisper to him my basic idea and he immediately asked how many layers I thought the model required and I said, "Two, just two". That lecture and the ideas that flowed from it all were stimulated by Harry's lecture and the irony was that I had to travel cross-country from Woods Hole to San Francisco to get the idea from someone whose office in those days was just across the hall from me in Woods Hole!

When I got back to Woods Hole, I quickly formulated the model and it was easy and natural to consider the problem, as in the mid-latitude thermocline problem, as an exercise in potential vorticity conservation. Once again, the issue was what the relationship between the streamfunction and the potential vorticity would be. The big difference was that the form of the Equatorial potential vorticity was more complex because it now needed to include the relative vorticity and the streamline was connected to the Bernoulli function that now included the kinetic as well as the potential energy. Nevertheless, the relationship between those two functions, the potential vorticity and the Bernoulli function, was established in mid-latitudes for all streamlines emanating from mid-latitudes that reached the equator. The dynamical model gave rise to scales for velocity, depth and width that were more realistic than the extant representations of linear theories of the Equatorial Undercurrent (EUC), the strong subsurface flow that existed in the Pacific, Indian and Atlantic Oceans. I was thrilled to see the basic nonlinear theory emerge so easily. There was some difficulty in coming up with a representation of the layer *above* the EUC but with the encouragement by Carl Wunsch to whom I had mentioned that difficulty, I decided to reduce the dynamics to a simple condition on the interface between the two layers.

The principal idea was more solid. The EUC in this picture was not a purely equatorial phenomenon. It was driven by its connection to the mid-latitude flow and I found it aesthetically pleasing that the continuation of ventilated thermocline theory to the equator explained the nature of this major current. Previous theories considered it a local equatorial phenomenon; and linear, and even previous attempts at nonlinear theories, considered the EUC in isolation from the rest of the ocean and I believed the *connection* of the Equator to the mid-latitude circulation was the most interesting part of the theory. There was some resistance to the new theory for that reason. Indeed, when one of my graduate students took up a post-doc at Princeton, he called me up and, in an embarrassed tone of voice, let me know that the first project he was assigned to do was to use a large numerical model to show this new theory was wrong because of the reliance on the connectivity of the two regions!

The idea that equatorial dynamics was isolated from the rest of the ocean had a natural origin. Theories of large-scale waves in that region, starting with the earlier work of Dennis Moore, Mark Cane and Ed Sarachik, showed that these time-dependent motions were naturally trapped by the Earth's rotation to a narrow zone around the equator and the idea took hold that this was generally true for all dynamics. Indeed, people began talking about Equatorial Oceanography as distinct and separate from the rest of the ocean and for certain phenomena that are time dependent, like El Niño, that is largely true. For essentially steady motions, like the

EUC, that trapping effect no longer holds and the flow of water to the Equator connects the mid-latitude dynamics to the equatorial domain.

The following year, after the theory was essentially complete, I was giving a seminar about it to the Oceanography Department at the University of Washington and as I was developing the ideas for the audience a helpful image suddenly occurred to me. In an improvisation on the spot, I asked the audience to consider the potential vorticity and the Bernoulli function to be two members of a "happy couple". She is the Bernoulli function and he is the potential vorticity. In the rural area of the mid-latitude gyres she is mostly potential energy and he lacks any substantial spin of his own. I then asked the audience to think about the couple as they move to the region of the Equator where constraints are looser. She gains intense kinetic energy; his spin is less statically local, perhaps metro-sexual in character. Nevertheless, the relationship between the two of them is solid and remains what it was before as they superficially change form. The audience loved it.

I did have one difficulty initially in completing the theory and I'm embarrassed to own up to it now. I originally solved the coupled non-linear differential equations for the layer depths and the velocity by starting the calculation in mid-latitudes and proceeding to the equator having given the depth of the thermocline *and* the zonal velocity at the starting points along a set of different longitudes. This was a mistaken formulation of the problem but I didn't realize it immediately.

As a consequence, when I integrated the theory's equations to the Equator I found a EUC that seemed to speed up and slow down, even reversed direction in a bizarre and physically unacceptable way. I was flummoxed.

One day I mentioned to Holly that I was bewildered by this apparent fundamental deficiency of the theory and couldn't figure out what to do. She listened, and suggested one morning that I come home for lunch that day instead of taking lunch at the lab. We often arranged such "dates" and the delectable lunch that she always prepared typically followed an intimate, romantic interlude while our daughters were at school. It was in the immediate aftermath of the interlude that day, while in the usual idyllic and dreamlike post-romantic state, that I turned to Holly and murmured, " It's not an initial value problem; it's a boundary value problem!" In that moment of relaxation, I suddenly realized that I should be specifying the thermocline depth at each longitude far from the Equator and the same constant Bernoulli function on the Equator at each longitude. That resolved the problem.

I believe many of us have that experience where a difficult problem is hard to resolve because we fervently believe our initial approach to a problem is basically correct. When it fails to resolve, we try harder and harder on the original path to find a way to break through to the answer and remain stymied until we allow our internal censor to admit another approach, different than our original idea. It is in periods of relaxation when the censor lowers its guard allowing hitherto blocked lines of thought to arrive that progress can be made. At least, that is what I have experienced several times. It was just a bit more extreme and romantic in this case. Holly didn't bat an eye; she was used to the intrusions of science gobbledygook at all hours.

The Equator was also the site of one of my greatest embarrassments. I can think of two occasions in which I have published a paper than turned out to be very wrong. When I say very wrong, I mean wrong not only as a calculation but wrong conceptually. The first example in this little museum of horrors that I revisit from time to time, usually in disturbed sleep, was my misconception about the nature of the spin-up of a stratified fluid. For a homogeneous fluid the flux of fluid flung out radially in the bottom viscous boundary layer, the so-called Ekman layer, rises in the viscous boundary layers on the sidewalls, the Stewartson layers. In my work on the stratified flow problems that I later continued with Victor Barcilon I was able to show that if the sidewalls were thermally insulated those boundary layers could not carry that mass flux. I jumped to the conclusion that it meant that the spin-up circulation would be choked off and the spin up process would proceed much slower by diffusion. I overlooked the possibility, as demonstrated by Gösta Walin, that the flow from the Ekman layer would circumvent that problem by issuing directly into the interior of the fluid via jets issuing from the corner intersections of the bottom and sidewalls. I was correct in arguing the sidewall layers could not carry the flux but incorrect in understanding the consequences. I was abashed and depressed when I realized my mistake. Of course, I try to give myself solace by reminding myself that when you are the first to try to do something really new it is not surprising that you can flub. More telling was that several people had told me as I was formulating the problem that I must have been making a mistake but my certainty was anchored on the idea that without the sidewall layer the spin-up would be blocked. Ignoring those criticisms in retrospect appeared more and more like intellectual arrogance and I hated to think of myself in those terms.

Similarly, when I took up the problem of the existence of Deep Equatorial Jets, the jet-like flows on the equator beneath the region of the EUC, I had an intuition that forcing from the eastern boundary radiating its effect westward as arrested Rossby waves might be an explanation. I formulated an equatorial wave model recognizing that this was a dynamical regime that was new to me but was delighted when the calculations done in the application MATLAB gave a very good likeness to the deep jets. Later calculations by a group more adept than I am in such kinds of models showed convincingly that I was wrong. Checking and rechecking my calculations showed that the excellent but erroneous result I had obtained was due to a single misplaced parenthesis in my MATLAB program. It was a very dark day indeed. Those two papers out of an oeuvre of about 160 research papers remain seared in my consciousness as examples of my occasional hubris and the need for greater humility. I find it especially painful because as a rule, when I first hear criticism of some work of mine my very first inclination is to believe the criticism is probably true and my more serious problem is not to be swept away by self-doubt. In those two cases I would have done better to listen to that inner skeptic.

That brings me to another personal issue. About this time, I was entering my 50's, and although it looks to me like a relatively young age now, it was pretty clear that youth could no longer define my situation in the world. Indeed, I began to realize that with the scientific successes I had been fortunate enough to attain, really younger people began to interact with me in a different manner. When introduced to a new, young person in my field I could not help but notice a slight startle and change of demeanor. I don't want to exaggerate this but it was something inescapable. This, and the sense that I was no longer one of the kids, started me on an introspective train of thought and a conversation with myself.

It was also initiated by an experience that made a profound impression on me. One summer, about this time, I was attending a seminar given by one of the founders of the GFD program in Woods Hole who was about 15 years older than me. A fairly good scientist in his youth he gave a seminar about some of his recent work that could only be called mediocre at best. It was actually embarrassing to hear him lecture. I almost felt sorry for him but his lack of self -knowledge didn't allow for too much sympathy. I was wondering about his inner thinking when the person sitting next to me leaned over and whispered to me, " His problem is that he thinks of himself as the Grand Old Man of GFD". The correctness of this diagnosis struck me as obviously true and I pledged to myself that as I aged, I would do my best to continue to approach my work with the same attitude I had when I started out in the field. That is, to not ask for any special privilege in judging my work. If I had nothing really interesting to say I would keep quiet until I did, and not ask for special treatment based on past accomplishments. I know it's easiest to fool yourself in such evaluations but I promised myself to keep the example of that day's lecture in mind as a cautionary example of what can go wrong when you set yourself a different standard because of age.

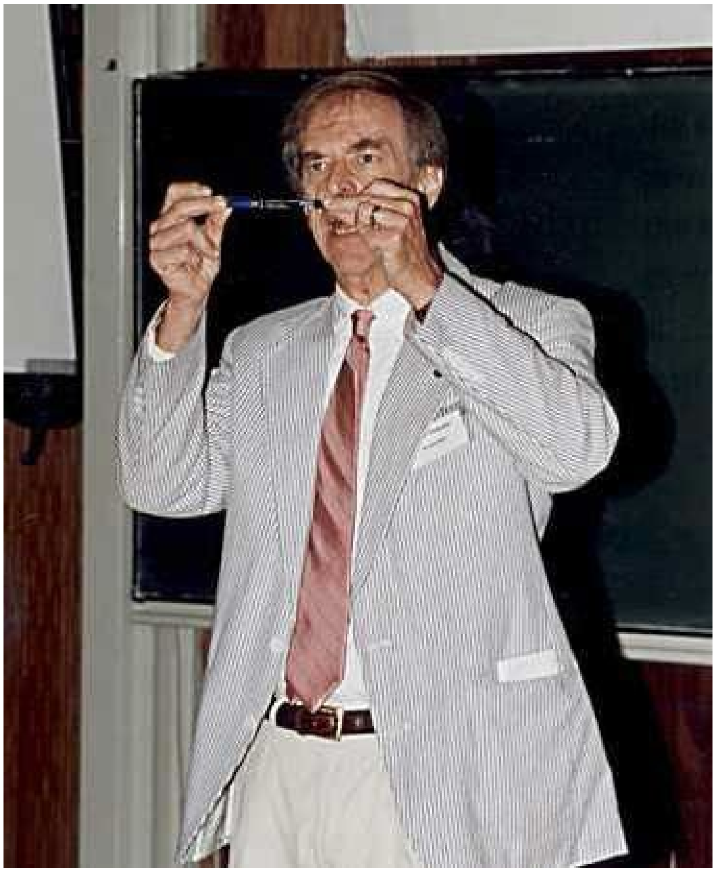
Another discussion I had with myself revolved around an experience that all scientists who have any self-knowledge have to confront sooner or later. How do you respond to a younger scientist who is clearly smarter than you? I have known some scientists in my field who out of envy or personal insecurity play the equivalent of the children's roughhouse game "king of the hill" where you do anything to prevent a competitor from reaching your level. There are many ways it can be done. People sometimes write destructive letters of recommendation about people they fear will compete with them or struggle over authorship precedence.

In the conversation that I had with myself about the future I intended to behave quite differently. I think this was made easier for me by the fact that in all my school years I was *never* the smartest person in my class and in my professional life I never considered myself superior in capability to everyone else. When I thought of the giants in my field, e.g. Charney, Stern, Stommel, Munk and such others, my goal was simply to be able to play in their league. If I could be considered a valued player in that major league, I would be happy with myself. When I recognized younger people who I thought were clearly superior, for example, Bill Young at Scripps or Roger Samelson at OSU and Mike Spall and Karl Helfrich here at WHOI, I was supportive and I feel I've been rewarded by working with each of them and by their generosity in their interactions with me. Life is incredibly more pleasant when you are working to collaborate rather than working to thwart other people. It sounds simple but some people are so eaten by envy that they consider the appearance of new stars as a threat to the security of their self -image. Since I never had the experience of being able to consider myself the smartest person in my solar system or the brightest star in the scientific galaxy it was natural to content myself with feeling good about my successes on my own internal scale of measure.

Photos: Teaching.

Lecturing at the GFD summer course 2007.

The course in the MIT-WHOI Joint Program that I enjoyed teaching the most was the first-year course, which was an introduction to fluid dynamics. My classes were large with students from MIT, Woods Hole and Harvard. Here is the class in 2007. I am in the second row, second from the left.



Lecturing at a course in Limerick, Ireland, 2000. I was fond of limericks and often made them up. One of my favorites referred to a lecture at a different meeting, in which the speaker, from Imperial College was attempting to simply represent the complicated behavior of atmospheric eddies. I thought the attempt was too simple:

There once was a man from Imperial
Whose view of the world was ethereal.
His theory is fine,
It fits a straight line
But his assumptions are many and serial.

Chapter 11

A Shattered Calm; instability and Islands

One of the pleasures of academic life is the opportunity to work with a stream of bright young people whose strengths complement my own. In the period of the 90's I had that experience with several excellent students and post-docs. One of the brightest was Roger Samelson who came to WHOI as a post-doctoral scholar and stayed on as an Assistant Scientist until he was lured away by a faculty position at Oregon State University. I had become interested in the consequences of the variation in the downstream direction of the stability properties of a zonal flow. Roger and I looked at a simple idealized model in which topography, sloping to produce a stabilizing effect like beta, would satisfy the *sufficient* condition for stability everywhere, i.e. for all zonal stations, except for a relatively small region in the downstream direction. In that region the *necessary* conditions for the instability of the flow would be satisfied. It was unclear whether that would be enough to locally destabilize the flow. Our analytic work provided a surprising result. We found that if the necessary conditions were satisfied in a small region of length $2a$, the flow would become unstable even though the length a might be small compared to a deformation radius. It is not the place to go into the details of the analysis but since zonal flows are normally unstable to perturbations *greater* in length than the deformation radius it was natural to expect that small domains of instability would continue to be stable. The fact that it was not true was astonishing. One's intuition is built by the experience of a collection of examples and intuition can be misleading when the configuration of the problem seems familiar but actually has new elements outside previous experience.

Some years later my colleague Bob Pickart, an excellent observational oceanographer, pointed out to me a curious region along the West Greenland Current where the current produces eddies from a limited region of the current flowing up the western coast of Greenland on its continental slope. It seemed surprising because that was also a zone of particularly steep slope, normally more stabilizing. However, the slope was so steep that in that short distance along the flow the current over most of its width was flowing over such a deep bottom that it did not sense the topographic slope. This seemed like a great opportunity to apply the ideas Roger and I had developed and another excellent post-doc, Annalisa Bracco, took up the problem. The new situation was more complex than the one Roger and I had first studied but Annalisa's careful numerical work showed the same behavior, i.e. locally enhanced instability in the domain in which the current does not experience the stabilizing effects of topographic slope. Her work led to an elegant explanation of the localized source of eddy energy in the West Greenland Sea.

However satisfying my work was at that time, real life provided instability of a far more ominous character. After several medical examinations in 1990 it was discovered that Holly had breast cancer in her right breast. We found good doctors and treatment at the Dana Farber Cancer Hospital in Boston but, needless to say, our lives were overwhelmed by this terrible news and the news kept getting worse. At first it appeared that a simple lumpectomy would suffice but the results of that procedure required further surgery, a mastectomy and follow-up chemotherapy. In a seeming instant everything else in life fell away and the fear and anxiety of the cancer diagnosis took over everything. During the follow-up period of chemotherapy Holly continued the teaching of photography she had been doing at the school Dove was attending, Falmouth Academy. The pressures were great and the emotions were very raw. Shortly before the mastectomy Holly was standing in the bathroom one night before coming to bed and burst into tears at the thought of the breast disfigurement to come. When chemotherapy started, her normally abundant hair thinned noticeably but she avoided the need for a wig. Nevertheless, the pain, nausea and fatigue associated with the chemo were often overwhelming for her. She was amazingly stalwart during this period, never complaining to our daughters, keeping apparently serene in public but, occasionally, when there were just the two of us in the house she would burst into tears before recovering her composure.

She joined a breast cancer support group of women who were going through similar trials and, in fact, she was the only woman of that group who survived her cancer. As time went on and periodic examinations showed no sign of the return of the cancer her confidence grew. The reconstructive surgery of the breast was also an important psychological boost and within a year we were traveling to Italy again and life seemed to return to normal. Of course, after cancer life is never normal again. The fear of recurrence hangs ever in the background. Holly was always a physically strong woman in excellent health and if the cancer did nothing else it destroyed the automatic self-assurance in her health. Still, she did survive that bout of cancer and her subsequent cancer 20 years later in 2011 was judged to be an independent and unrelated event. I will get to that sad story later but at this point in the history I am recalling, family life was restored and we had many happy years ahead of us.

During this period of time an unexpected task was thrust on me at the Institution. Two fairly senior scientists, not in my department, were accused by two younger postdoctoral researchers of scientific misconduct. The younger scientists accused their older colleagues of stealing their ideas and publishing a paper on the subject without giving them proper credit. I was asked to chair a small committee of three to look into the matter and present a report to the Director so the appropriate action could be taken. The other two committee members, Stan Hart and Holger Janasch, both outstanding scientists, were eager, as I was, to get to the bottom of the matter. A preliminary investigation had already been performed and a report written and when I read the report my first reaction was to believe the accusation and I found myself in sympathy with the younger scientists. I will not go into detail to protect the identities of all involved but it required that we three investigate the matter in detail and interview everyone involved. The two younger scientists had left the Institution and they were invited to come back and tell their side of the story *in camera* in confidence. The same opportunity was given to the more senior people. Somehow, in the process my original certainty became shaken and I grew to believe someone was not telling the truth and I decided to use a technique I associated with the fictional detective I most admired, Simenon's Inspector Maigret. I encouraged each of those involved to tell their story in greater detail, constantly encouraging them to describe things more deeply and at greater length. As I hoped, when the story telling went beyond the point of the originally prepared story, the people telling the truth had no difficulty in maintaining consistency. The others fell into increasing confusion and contradicted their own initial stories. We three on the committee were able to come to a conclusion that the original allegations had been so embroidered that the case against the senior people could not be maintained and said so in our report. I think we saved some innocent people some serious grief.

Cancer touched not only our family but also one of my close friends and colleagues, Hank Stommel. We had remained close since our work on the ventilated thermocline and like many others of his colleagues I was dismayed when a colon cancer metastasized to his liver leading to his death in 1992. There was a period of remission between the initial cancer and its recurrence and during that time we had many occasions to talk about humanly deeper things than any particular oceanographic phenomenon. He mused aloud to me, during that time, that he felt basically lucky. " Just think", he once remarked to me, " I could have been an apple and never even known I was alive".

He and I had been working on a paper concerning a mechanism, involving stratification dependent mixing, for self-excited inertial waves in the mixed layer. It was not a major piece of work but the physics was intriguing and it was fun for me, and I suppose an escape for Hank, to be working together again. When we had almost a full draft of the paper ready Hank came over to my house one Saturday to discuss the form of the final draft. We ended up standing in the kitchen while I prepared tea for the two of us. Hank was in a melancholy mood. The suggestion had been made to surgically remove the most cancer- affected region of the liver to perhaps give Hank more time. He was reluctant to undergo another surgery. He mentioned that his neighbor, Bill von Arx, had counseled against it saying that the time gained would be lost in the period of recuperation from the surgery. Hank sighed and said that his family was pressing him to have the surgery and he supposed that to satisfy them he would. He lamented that he was "tired of being everyone's role model". He was very depressed which of course was very natural in the situation in which he found himself. Death after all, was imminent. He shortly submitted to the surgical procedure and soon afterwards called me from the hospital to tell me that the surgery was to no avail. The surgery showed the cancer so widespread in the liver that no benefit would be obtained by removing any part of it and he asked me to pass on the news to his friends so he could be spared telling the same grim story over and over again. I did visit him once while he was recovering from the operation. He was in a deeply blue state and called himself a "deeply feeling person" and I think it was a way of expressing his anguish at what he was confronting. He died of a heart attack during the night soon after, no doubt a consequence of surgery. So passed the second of the two greatest scientists I have ever known personally, Charney and Stommel. My world is a much bleaker place without them.

By this time, I had spent more time at Woods Hole and the Oceanographic Institution than I had ever spent in one place in my life before. Holly and I found the atmosphere suited us perfectly. The beauty and serenity of Cape Cod, the annual excursions to Italy, the availability of Boston and New York and an increasing circle of friends and colleagues made both our lives richer and both more relaxed and stimulating. The connection with MIT and the excellent people there only added to my sense of satisfaction about our decision to move to Woods Hole. We considered ourselves immensely lucky.

The decade of the 90’s was also a particularly productive period for me scientifically. I worked with a number of excellent Ph.D. students and colleagues. Patrice Klein, from France, and I worked on fully nonlinear baroclinic instability where the condition of weak nonlinearity could be relaxed by employing direct numerical calculations. Roger Samelson and I showed how the radiation of energy from unstable jets could actually, and surprisingly, *destabilize* the basic flow. Karl Helfrich and I worked on models of atmospheric blocking as solitary waves and carried it further to include strong perturbations beyond the limits imposed by the solitary wave model. With Igor Kamenkovich, I was able to show how currents slightly tilted with respect to latitude circles could easily radiate energy to great distances as an explanation for the occurrence of strong eddy motions in oceanic regions with relatively weak background flows.

Perhaps the most interesting and unexpected results came from work initiated with my colleagues Karl Helfrich, Larry Pratt and Mike Spall. We had become interested in planetary scale oceanic motions in regions containing large islands, e.g. Australia. Such large islands represent “holes” in the oceanic domain and present new mathematical and physical elements for the oceanic circulation. Karl and I had worked out the solutions for steady flow for a basin with a long island oriented in the North-South direction with only small gaps between the island tips and the basin boundaries. To our surprise the smallness of the gaps did not impeded a substantial circulation between the two sub-basins of the domain. That is, although the “island” nearly divided the basin into two nearly separate domains, dynamical conditions produced a full basin circulation. It also, even in linear theory, produced a region of recirculation on the eastern side of the skinny island. Karl's experiments validated the theory in a beautiful way. The video of he made of his experiment was always a crowd pleaser whenever I gave a seminar on the subject.

Then with the addition of Larry and Mike to the team we began an ambitious investigation of the island problem in a more extensive manner. Our paper combined analytical theory, numerical modeling and laboratory experimentation in a way I thought was very fruitful and I enjoyed working with all three of my colleagues to a great degree.

One of the results that puzzled us was that the magnitude of the mass flux from one sub-basin to the next seemed to not depend very strongly on how nonlinear the circulation was. Increasing the forcing led to major changes in the shape of the configuration but the streamfunction value on the barrier did not change much correspondingly. I wondered whether the use of the no-slip condition on the island, which vitiated much of the net relative vorticity transport was responsible for this and I wondered if a baroclinic model with potential vorticity that included thickness fluxes might alter the result qualitatively. Mike began to look at the same configuration in a 2-layer model where baroclinic eddies could transport potential vorticity without suffering the same constraints as the barotropic single layer model. He came into my office a few days after he had gotten started with a rather astonishing result. As expected, strong eddies were generated in the western boundary current on eastern side of the barrier that almost severed the eastern from western basins. Nevertheless, the western basin was also full of eddy activity and in the lower layer where the mean field was too small to mask the nature of the time dependent motion, there appeared to be fluctuations that looked remarkably like Rossby basin modes. How in heaven's name could such large-scale modes sneak though the two small gaps and dominate the western basin?

Classical diffraction theory would seem to imply that an incident disturbance impinging on barrier from the east would be almost totally reflected if its north-south scale was much larger than the width of the two little gaps. I was very puzzled.

Then one day not long after, probably when my interior censor was taking a brief break it occurred to me that applying Kelvin's theorem to the island showed how the region of the barrier between the two gaps evolved from being a barrier to being an antenna that radiated wave energy to the western basin. Using that idea, I first calculated the coupled normal modes of the two sub-basins and Mike was able to verify the calculations using his numerical model. Some years later a summer student Fellow in the GFD program, Alexis Kaminski, carried out a laboratory experiment under Karl's guidance and she verified the very unexpected result. The large- scale modes squeezed through the two gaps like some kind of soft invertebrate animal squeezing through tiny cracks in a fence. A string of our papers followed the original one, each one demonstrating the striking power of Kelvin's theorem to allow this kind of unique wave diffraction result rendering what might be thought of as nearly impenetrable barriers completely transparent.

Our daughters, Dove and Anna were now grown and no longer living at home except for visits and vacations. Anna had left New England for graduate school in

San Francisco and discovered that she was, at heart, a natural Californian. She had some difficulty in finding a position, post-MBA, that was at once emotionally satisfying and financially sufficient. After many years in various management and administrative positions, did she finally find an occupation that was emotionally satisfying, serving as independent contractor and counselor for people moving to the area around Silicon Valley. Dove was in her senior year at Boston University when she was struck by an illness that brought unremitting pain and an interruption to her personal life that lasted for over a decade. Never have I been so proud of her as how, during that time, she soldiered on with her life without becoming bitter about the limitations the disease placed on her. Finally, a doctor in Washington, DC, found a simple topical medication that helped with her pain and although she has needed to pay constant attention to her situation, she has been able to recover a relatively normal professional and private life. Both girls married good men and are happy in their marriages to my own great satisfaction.

Chapter 12

The new millennium and tragedy

As we left the 20th century and entered the 21st, my life seemed established in a profoundly stable manner. My work continued to go well, our daughters were mature and independent, and Holly's and my lives seemed destined to continue in a comfortable and satisfying way in both our work and private lives. I was enjoying teaching in the MIT/WHOI Joint Program and my teaching seemed to please the students and that made me very happy. I was given the Arnold Arons for teaching and mentoring and that gave me the opportunity to publicly ruminate on the fundamental importance of teaching and how much it meant to me. My remarks on receiving the award are in Appendix C.

My research work evolved and there were a series of new and interesting problems that focused my attention.

It is impossible to describe all of the research problems and the collaborations involved in coming to grips with the challenges they presented. Among the more salient problems I examined at this time, was the problem of the instability of time dependent flows. When the basic flow is steady, as is often assumed in geophysical settings, there is usually a well-defined threshold of some parameter, e.g. the temperature gradient, or the degree of friction, that needs to be surpassed to make the flow unstable and generate self-sustaining perturbations. Working with graduate students Jim Thomson on one paper, with Francois Poulin on another and with my MIT colleague Glenn Flierl on yet another, we were able to show that flows of meteorological and oceanic interest could become unstable for values of critical parameters well below the normal thresholds established for steady flows. It was a very exciting time for me and the collaborations were continuously stimulating.

With colleagues Igor Kamenkovich and Pavel Berloff, we suggested a mechanism for the generation of recently observed zonal jets or striations embedded in the large-scale ocean circulation.

Perhaps the most unusual result from this period was a study I made of the development in time *and* space of baroclinic instabilities. I was interested in examining the spatial and temporal development of baroclinic wave moving downstream from an origin where small disturbances were introduced and the disturbances could grow and evolve in naturally unstable current. It turned out that for some values of the parameters, when the growth rate was small but no smaller than the viscous decay time, that the motion along rays in the downstream-time plane, would be chaotic, That meant that on two neighboring characteristics with slightly different initial values, the disturbance at long distances from the origin would have strongly different values as a consequence of the chaotic sensitivity to initial data. That in turn implied very rapid variations in space from one characteristic to another at a given time. I thought the results that led to what I called chaotic shocks represented something of fundamental importance that I had not seen before. However, since it was published in a journal of rather limited circulation (J. Marine Res.) that I chose in order to avoid page charges, it drew little public attention. Perhaps in some way it will be noticed in the future.

Such considerations lost most of their importance to me because of the more pressing developments at home. Starting in the late winter of 2011 Holly started complaining of an upset stomach, usually after we had gone out for dinner. We thought little about it. Usually, Holly would check with me to see if I also was affected by the meal but since we rarely ate the same meal we put it down to something in the food she had eaten. When this began to occur more frequently she consulted our family doctor. Simple tests showed no problem and there was nothing suspicious in the food she was eating or the restaurants we frequented. She recently had her annual physical and everything seemed in perfect order with nothing suspicious in her blood work or the doctor's hands-on examination. Finally, after a few months of inconclusive tests the doctor ordered an abdominal scan.

I still remember the phone call I received from Holly. The radiologist had discovered what he called "masses" in her liver. This was, of course, very shocking. I rushed home and we made an appointment to see a specialist, a Dr. Cobb, right away. He studied the radiologist's report and the weirdest thing was that he kept shaking his head and saying, "Very bad, oh, this is very bad" while all the time smiling as if he were having a fine time. I could have murdered him on the spot. The report said the liver was filled with cancerous tumors, over 50 of them. We were of course very shaken. Then Holly, bless her soul, decided to get in touch with a doctor at the Dana Farber in Boston who had shepherded her through a recent alarm of cervical cancer that turned out to be nothing. She had great confidence in him and she made an appointment to see him and we were able to do it the following day in Boston. I remember it was a Friday.

The doctor read the radiologist's report and seemed annoyed at the diagnosis that assumed the masses were cancerous. He kept saying that they should just describe what they saw without jumping to conclusions about the identification of what the masses might be. Then he said to Holly, “ If these were cancers, you’d be dead by now”. We took that as reassurance since clearly Holly was not dead but in good health. It’s clear we were desperate for reassurance that everything might still be ok. When Holly mentioned to him that a mesh had been inserted in her abdomen as part of the breast reconstruction after her mastectomy 20 years before, the doctor said with authority that it was probably the mesh that was being mistaken for cancerous masses. After further examination he said a hysterectomy might be advised but that it was nearly certain it was not cancer. My heart leaped and I shouted out, “Doctor, I feel like kissing you”. He replied that he’d much rather that Holly kiss him and I conceded the point.

When the doctor told us we could go and that, to double check, he would show the radiology films to his radiologist but that he thought there was nothing to worry about, we left his office in a mood I can only call ecstatic. On the bus ride home, we held hands and kept smiling foolishly to each other expressing our joy over and over again. Walking in Woods Hole from the bus to our car we bumped into a local friend, the jazz pianist, Glenway Fripp, and told him we had just dodged a bullet, that we were happy beyond measure and that it was a wonderful day. We had a splendid weekend together and felt that life had been restored to us.

The following Tuesday I was in my office when Holly called and with a quiet and somber voice told me the doctor had called and said that his radiologist had confirmed that the masses were cancer tumors after all. It was a crushing moment and I rushed home to be with Holly as we started what was to be the journey into the darkness that followed.

Holly's stepmother had died of cancer not long before, but Holly and her father had been favorably impressed by her stepmother's oncologist at the Dana Farber Institute. Holly called and made an appointment to be seen right away and had the films sent up to the Dana Farber for the doctor, Michael Rabin, to see them for himself. Our appointment was for the following Friday, July 3 and it took place at the Faulkner hospital just outside Boston which was easier for us to reach than the Dana Farber itself. We had a somber interview with Rabin. He told us immediately that the cancer had metastasized from an unknown organ to the liver. Although the cell type was a lung cancer type there was no sign of it in the lungs. He told us straight out that the cancer was incurable but treatable. We hung on to the work treatable and hoped it meant that death could be put off indefinitely with proper care and a good deal of luck even though he had also told us that the typical survival time, with treatment, was about a year. We both chose to believe in our naiveté that, of course, we would beat that dark forecast. Such is the hope born of desperation. Rabin then asked Holly when she wished to start the chemo treatments, whether the following week or sometime later and Holly, the fighter that she was, said, "Now!" Although it was the Friday before the Independence Day weekend, the nurses and doctor put aside any early leaving plans they may have had and got to work. Since the treatments were a 3-day affair we stayed overnight in the home of our friends in Milton, Morris and Shirley Carnovsky. Indeed, they were to be amazingly generous over the months of Holly's therapy. We had a key to their house and often stayed there by ourselves.

The treatments were tough on Holly. As opposed to the chemo she had had earlier for breast cancer (and we were told this cancer cell type was not connected to that earlier cancer but was a second independent event) the physical effects were stronger. Her hair fell out almost immediately and completely. She cried to see that happen but gamely went with my wonderful secretary, Kathy Ponti, to a wig store where she let on that she enjoyed choosing a wig.

The treatments became the center of our lives. We would drive up to the Carnovsky house, settle in, and go for the treatment. Holly bore the treatments well and, in fact, began to feel better as the tumors shrank.

The pattern with Rabin was that he would come and talk to us about the evidence of how the treatment was going and then Holly would prepare for the following three-day treatment. There was a Legal Seafood restaurant between the hospital and the Carnovsky home and we would have supper there and, at least initially, it was not an unpleasant time if you could ignore the reason for what was happening. Indeed, before one of our early sessions, Rabin swept into the little examining room where we would anxiously be waiting and said. "You're doing great!" and we were thrilled. We told ourselves we were going to beat the cancer and with proper care our lives would go on much as before. Rabin quickly tried to dash those hopes when in answer to my question he emphasized that there was no correlation between the length of any remission and the initial success of the treatment. Still, we hoped.

Once the protocol of the treatment was established, we were able to transfer the treatments to a clinic in North Falmouth, just a few miles from home and this was a great benefit to both of us. The treatments continued until the end of October 2011 when, on the advice of a local radiologist Holly decided to undergo radiation therapy to the brain under the theory that it could help prevent a spread of the cancer to the brain. I was skeptical because the therapy itself was so fatiguing but Holly, with her fighting spirit, wanted to do anything that might help increase the time of remission. That therapy took another month.

The universe again demonstrated its love of irony. In the middle of this family tragedy that occupied our entire emotional life I received an email from the American Geophysical Union saying that I had just won the Ewing Medal, the society's highest award for oceanography. Were the situation normal it would have filled me with delight. As it was, and as I told our friends, Stan and Pam Hart, I would gladly have traded all such awards and any success in science to have Holly healthy again. But one can't choose.

The chemo treatments were going so well, and Holly was responding so well that with the improvement we decided to go together to San Francisco for the award ceremony. We also planned, with the Harts, a little vacation before the conference. We toured the wine country of Napa Valley. We stayed in a charming little hotel, ate well, and enjoyed visiting the vineyards and tasting their best wines. It was to be our last vacation together.

The award ceremony requires an answering speech to the encomium describing the recipient's accomplishments. I used the speech, reprinted as appendix

B, to publicly thank Holly for her support over the years of our life together and to ruminate on the possible uniqueness of *intelligent* life in the universe and what that means for our responsibility to the cosmos.

When we returned home the doctors suggested a scan to establish a baseline to compare the effects of further treatment. We were not too concerned about the scan since only a few weeks before, the cancer had retreated so strongly.

We were therefore unprepared for the very bad news that the cancer had already sprung back and was, of course, resistant to further treatment by the first chemo treatment. A second and then a third chemical was tried and while the third type seemed to stay the further advance of the disease it was clear that Holly's health was failing. At the end of March, we were advised to move to hospice care, fortunately possible at home. Our daughters rushed home and the late evening of April 5, 2102, Holly passed away as I was administering her pain medication. She had been in a coma for a few days, we were clearly at the end but the actual passing, as those who have experienced the death of a loved one know, makes painfully clear the difference between being alive and dead and is shockingly enormous.

Now, six years later, I continue to work and friends and children have been a great support.

It has been a good and rich life and if I have added something to our understanding of the natural world, and if I have helped younger people make their way in science, and if I have helped my children find their places in the world, it will all have been worthwhile.

Appendix A

The Loyalty Oath

Woods Hole Oceanographic Institution
Department of Physical Oceanography
Clark 363 MS 21
Woods Hole, MA 02543

tel.: (508)-289-2534
fax : (508)-457-
2181

e-mail: jpedlosky@whoi.edu

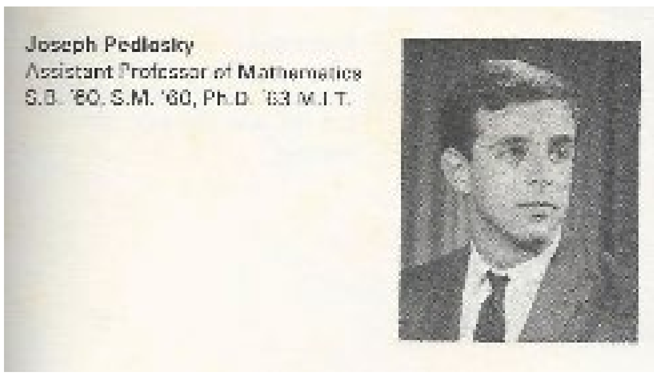
This is a slightly revised version of a talk given in 2004 with the addition of files of documents and newspaper clippings from that period. The original documents are now (12/6/13) in a notebook in my WHOI office.

September 7, 2004

<u>Reminiscences of the loyalty oath case of 1964-67 for the PAOC retreat 2004 re: scientific controversies in Atmospheric, Oceanic and Climate studies.</u>

I want to talk to you about the history of different sort of controversy that involved many scientists and other academics at MIT. It is perhaps not what the organizers of this retreat had in mind but I think you may find it of interest.

Imagine you are a young assistant professor of mathematics in the year 1964 and it is October and you have already started to teach the fall semester with two sections of advanced calculus (18.075 today, M-351 in 1964).



(Photo of JP from the MIT 1964 album of faculty)

You receive in the mail the following letter from the office of the MIT Registrar.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
CAMBRIDGE, MASSACHUSETTS 02139

OFFICE OF THE REGISTRAR

October 7, 1964

Professor Joseph Pedlosky
Room 2-375

Dear Professor Pedlosky:

Since October 1, 1935, signing the Oath of Allegiance in duplicate has been a requirement by Massachusetts law of every citizen of the United States who "... is in service as a professor, instructor, or teacher at any college, university, teacher's college, or public or private school in the Commonwealth..." In our case, all faculty members and all staff members holding the rank of Instructor, Lecturer, Technical Instructor, Teaching Assistant or Graduate Assistant are required to sign. One of the documents is kept on file in the Registrar's Office and the other is delivered by the Registrar's Office to the State Commissioner of Education.

Inasmuch as our records do not show that you have previously signed the Oath of Allegiance as required by Massachusetts law, would you please do so at your earliest convenience by contacting Miss Morrissey in the Information Office, Room 7-111, to have your signature notarized.

If you are not a citizen of the United States, please give that information to Miss Morrissey in the Information Office, Room 7-111, Telephone Extension 795.

W D Wells
W. D. Wells

92 State St
Boston 9, Mass.

Mr Louis Nordlinger
LA-3-5580

Mr Reuben Goodman
HO2-8040

(PRINT) (Last name) (First name) (Initial)

OATH OF ALLEGIANCE

I do solemnly swear (or affirm) that I will support the Constitution of the United States and the Constitution of the Commonwealth of Massachusetts, and that I will faithfully discharge the duties of the position of

(Title of position)

in M. I. T.

(Designation of school, college, university or institution)

according to the best of my ability.

(Signature)

(Address)

Sworn to before me this .... day of .... 196 ......

NOTARY PUBLIC

(Official Position)

(SEE OTHER SIDE)

The oath may be subscribed to before any officer authorized by law to administer oaths or, in the case of a public school teacher, before the superintendent of schools or a member of the school committee of the city or town in whose schools he is appointed to serve, each of whom is hereby authorized to administer oaths or affirmations under this section.

This oath is to be taken in duplicate and one copy forwarded to the State Department of Education, 200 Newbury Street, Boston.

ED-ADM-22

This is an instruction to sign an oath of allegiance and loyalty. Please note that you **all** would probably have to sign, as it, in principle, refers to professors, instructors, teaching assistants, and graduate assistants. The oath itself is rather simple and straightforward.

It is an affirmative oath, not a disclaimer of the I-am-not –now – and –never – have –been –a -----Yankee fan (fill it in yourself).

Instituted by the Massachusetts state legislature in 1935 over the strong protests of the state's universities it had become, after 30 years a largely non-controversial rite of passage. So much so that although its signing was by law a condition of employment, since no one for decades had ever refused to sign it had fallen into obscurity and was routinely enforced *after* employment. After the law was passed there were several attempts to repeal the law but they failed.

The history of the loyalty oath is interesting but too time consuming to describe in detail now in the time I have. But, in brief, beginning in the years right after the First World War and the Russian revolution there was a tremendous anti-"radical" movement in the US with great xenophobia in response to the perceived "Bolshevik" threat. In fact, there were indeed a number of terrorist bombings and at least one case of multiple bombs sent through the mail to elected officials and fear was widespread of additional terrorist plots. The situation should sound familiar to us today. The response of the Attorney general of the time (Attorney General Palmer) was savage. Aliens were rounded up and deported without trial. Some members of the New York assembly were denied their elected seats and there was then started a movement to require oaths of allegiance of teachers to ensure their loyalty. In the 30's, partly as a result of the increased social tensions associated with the Great Depression, partly again due to the menacing character of Stalin's Soviet Union, such loyalty oaths became a common feature of many states. Sixteen states eventually passed similar measures. Massachusetts was neither the first nor last to pass such a law. Its enforcement in Massachusetts was weak and it prevented very few people from employment. In at least one case, though, a very famous literary critic and writer, Edmund Wilson was denied employment at Harvard because he refused to sign the oath.

When this memo arrived on my desk without warning, my first thought was simply that this was a bureaucratic triviality that could be safely ignored. I knew without thinking that I would not sign the oath but my first thought was that I would, at worst, be considered eccentric and the whole thing would be ignored and forgotten. However, I could not be sure of that.

As I said, mine was an immediate, instinctive reaction without forming a formal argument with myself. Why I felt that way is somewhat complicated to explain, even to myself. I always had a rather romantic and elevated view of what it meant to be a teacher and moreover, a college professor. Perhaps this was related to my Jewish heritage in which the Rabbi is both a teacher and the central ethical figure. I had a particularly *simpatico* and intellectually ethical Rabbi. Partly it ran in the family. My father was very much <u>not</u> an intellectual but as a grammar school teacher he had a strong and instinctive sense of the honor of his position. Partly it was a romantic identification with the Socratic story of the ethical role of the teacher in society. Whichever of these emotional strands was dominant it led to my decision not to sign. I was, as I said, only a bit worried about the consequences.

So, as a card-carrying member of the American Civil Liberties Union, my first thought was to get in touch with them and ask for help and advice. It seemed like a natural for them and you can see on the copy of the notice my scrawled notes of the phone number and address to use to make contact. *The ACLU response could not have been more depressing.* And while waiting for the response a second memo from the Registrar, labeled SECOND NOTICE made me realize that MIT was not going to just forget about the whole thing if I didn't sign.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
CAMBRIDGE, MASSACHUSETTS 02139

OFFICE OF THE REGISTRAR

October 30, 1964

S E C O N D N O T I C E

Professor Joseph Pedlosky
Room 2-375

Dear Professor Pedlosky:

Since October 1, 1935, signing the Oath of Allegiance in duplicate has been a requirement by Massachusetts law of every citizen of the United States who "... is in service as a professor, instructor, or teacher at any college, university, teacher's college, or public or private school in the Commonwealth..." In our case, all faculty members and all staff members holding the rank of Instructor, Lecturer, Technical Instructor, Teaching Assistant or Graduate Assistant are required to sign. One of the documents is kept on file in the Registrar's Office and the other is delivered by the Registrar's Office to the State Commissioner of Education.

Inasmuch as our records do not show that you have previously signed the Oath of Allegiance as required by Massachusetts law, would you please do so at your earliest convenience by contacting Miss Morrissey in the Information Office, Room 7-111, to have your signature notarized.

If you are not a citizen of the United States, please give that information to Miss Morrissey in the Information Office, Room 7-111, Telephone Extension 795.

W D Wells

W. D. Wells

This was the first sign of trouble

LOUIS M. NORDLINGER
ATTORNEY AT LAW
92 STATE STREET
BOSTON 9, MASSACHUSETTS
LAFAYETTE 3-5580

October 31, 1964

Mr. Joseph Pedlosky
Assistant Professor
Department of Mathematics
Massachusetts Institute of Technology
Cambridge, Massachusetts 02139

Re: Oath of Allegiance

Dear Professor:

I want to thank you for your letter and for sending me the photocopy of the oath and its reverse side. After you contacted me, I received a similar expression of interest from the gentleman to whom I am sending a carbon copy of this letter. I know that both of you were anxious to hear if the Massachusetts Civil Liberties Union would wish to become involved and if the Union would deem it advisable that in your interests an issue should be made of this particular oath. I am writing you today because last evening the Executive Committee of the Union met and discussed this problem.

Aside from the principle involved, any organization that lends its support to an effort to correct some wrong is compelled by practicalities and a certain sense of strategy, to take those cases that present the most serious instance of the wrong that can be found. With regard to teachers' oaths, the governing body of the Union wishes to take the case of a more serious imposition, and that is the oath and disclaimers that are forced on the City of Boston teachers. These are much more serious than the oath which confronts you and without meaning to play down the objectionable nature of any oath, the Union, for other reasons, feels you should not make an issue in this case and that it would be much better to litigate the question of the incomparably more objectionable affidavits which I have mentioned. Again, I would like to say you should be complimented for thinking about this matter and initiating some protest.

Very truly yours,

Louis M. Nordlinger

Louis M. Nordlinger

cc: Asst. Prof. Wm. B. Watson

The ACLU response left me with the same feeling you might get if you called the Fire Department to report a fire in your house and they responded by saying that they were saving their engines for a *possibly greater fire that might, just might break out elsewhere.* So, sorry. (As it turned out there never was a case brought by a public school teacher; they were far too vulnerable).

I was really rather scared now and did not know what to do. A third notice followed a month later, and then a fourth notice in December with its red heading that led me to call the attorney general's office to check with the civil liberties section to see whether I really had something to worry about.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
CAMBRIDGE, MASSACHUSETTS 02139

OFFICE OF THE REGISTRAR

December 1, 1964

F O U R T H N O T I C E

Professor Joseph Pedlosky
Room 2-375

Dear Professor Pedlosky:

Since October 1, 1935, signing the Oath of Allegiance in duplicate has been a requirement by Massachusetts law of every citizen of the United States who "... is in service as a professor, instructor, or teacher at any college, university, teacher's college, or public or private school in the Commonwealth..." In our case, all faculty members and all staff members holding the rank of Instructor, Lecturer, Technical Instructor, Teaching Assistant or Graduate Assistant are required to sign. One of the documents is kept on file in the Registrar's Office and the other is delivered by the Registrar's Office to the State Commissioner of Education.

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If you are not a citizen of the United States, please give that information to Miss Morrissey in the Information Office, Room 7-111, Telephone Extension 795.

W D Wells
W. D. Wells

Miss Mc Ilvain (CA 7-4200)
Assistant Att General for
Civil Rights + Civil Liberties
does not know of such a statute for
non-public employer but later
agrees such a law is constitutional

Section 30 A
Chap 71
of the General
Laws

At first, the pleasant Assistant Attorney General said she thought the oath did not apply to non-public employees (whew!) but then she found the section of the law that showed that it did and I was plunged back into deep worry.

About this time, and this is the only benefit of the ACLU contact, I became aware that a second Asst. Professor at MIT, Bill Watson a historian in the Humanities department, had also refused to sign but his case was made obscure by the fact that as a teaching assistant earlier at Wellesley he had already signed the oath there and may not have needed to sign again. However, MIT was not willing to reappoint him without his signature when his current term of appointment was to end. Bill became my chief ally and his wider range of contacts in the world of law and civil liberties was crucial in what follows.

Indeed shortly thereafter (although it seemed like a long time to me) a letter from the MIT Vice President M.G.Kispert arrived explicitly asking me to sign as a condition for continued employment, copies going to my dean, Jerry Wiesner and my department Chairman Ted Martin.

In fact, two days later I got a call from one of the MIT lawyers (a Mr. Milne) warning me that the letter from would be the last written statement I would get from MIT officially. They were unwilling to commit anything else to paper because it would reveal that they had hired me in advance of my signing the oath and *they* would be in trouble. Further, if I did not sign in "the next few days" <u>my employment would be terminated</u>. This was a heavy moment for me. I felt very alone. No ACLU, no one to bat for me, and the weight of MIT bearing down on me. Furthermore, most of my colleagues at MIT that I might have expected to be sympathetic were rather reserved in expressions of support. If liberal, they were somewhat embarrassed that they had signed and argued the whole issue was really trivial and if they were more conservative they felt that I ought to sign. And, we were not so far removed from the McCarthy period where there was real worry about appearing less than completely loyal although the Federal Courts were now considerably more liberal. I also got a call from someone (I no longer remember who) on the MIT faculty committee on educational policy asking me what I was doing to deal with the situation and I had to admit to him and to myself that until that point I had done very little except to hope that it would somehow all go away. It was not and I realized I had to do *something* but it wasn't clear to me what.

At this stage I was ready to throw in the towel. I am not by nature a martyr and while romantic, I have a strong attachment to the habit of eating and to sleeping under shelter. However, every time I was about to cave in something would so infuriate me that I would say "the Hell with it" and press on.

For example, after the rather chilling telephone call threatening dismissal, I decided to talk to my Dean, Jerry Wiesner, well known for his liberal views and famous for his period as Kennedy's Science Advisor. I was ready to sign the Oath in exchange for a simple but public statement from MIT that they objected to being placed in a Police Role as the enforcer of the loyalty oath. That's all. I would settle for that.

When I arrived at Wiesner's office for my appointment he was packing a suitcase. Without hesitating in his packing for very long he told me he was about to fly off to a disarmament conference in India and that the issue of disarmament was *inconceivably more important* than my troubles with the loyalty oath in Massachusetts. Well, I couldn't deny that and I didn't have the wit at the moment to remind him and myself that he *was* my Dean and this *was* an academic issue that was occurring in *his* School of Science. But as I left his office I got so angry at his indifference that I just knew I was not going to cave in, at least not then. Indeed, although frightened through much of this period, every time I was tempted to quit someone would do or say something that would make me so mad that I could not bear to face myself had I given up.

Bill Watson and I also got in touch with the AAUP but they were also not interested in following up the case and giving any help and so Bill and I felt really trapped. That moment was the lowest moment and if it is possible to point to a single moment of moral courage it is when Bill said, "I think it's time to throw in the towel" and I said "let's wait just a bit longer before we give up".

Unbeknownst to us, other wheels were moving. The administration was being asked by the faculty CEP (faculty committee on educational policy) why they were in such a hurry to decapitate us, and, urging calm and at this time someone with real moral courage came to our aid and I am forever grateful to him.

Charlie Townes, the inventor of the laser, was the non-charismatic provost of MIT and his decision completely altered the tenor of the proceedings. It was only then, after about 6 months of high anxiety that I could start to breath again. I got a message left on my desk that Townes would give me a call and he did and set up an appointment to talk directly with Bill Watson and me. At the same time Bill, who was much more worldly than I, helped me find a lawyer since it was now clear that to protect ourselves we were going to have to go to court. Now enters the second major hero of the story after Townes.

Our lawyer was Gerry Berlin. Onetime Assistant Attorney General in Massachusetts for civil liberties, Yale law school, WWII naval hero (bronze star for the invasion of southern France), this soft spoken Virginian with his delightful southern accent agreed to defend us, come what may, at no cost. He only asked that we somehow pay him for printing and legal research costs. A short note: An assistant professor of mathematics in 1964 made $9,000 a year and this particular assistant professor was perpetually broke for personal reasons not pertinent for this talk. We eventually had to pay Gerry about $3,000.

We tried to get MIT to take the case as plaintiff to argue against the law but to no avail although the attempt burned up a good deal of time and effort. There were seemingly endless discussions about whether MIT was really a private or a Land-Grant Institution and the lawyers at MIT were unwilling to concede that MIT was truly a private institution.

In our meeting in the spring of 1965 Townes outlined the following course of action MIT would take.

First, since we could not take a moot case to court to fight the legitimacy of the Oath, which is what we were now committed to do. I would be fired at the end of the term (Spring 1965) if I did not sign.

The case would go forward in my name and not jointly with Watson since his prior signing of the law would render the case obscure. However, and this was Townes' idea, MIT would put me on the research staff in a non teaching capacity, if necessary, at the same salary until the case was decided. It would then be up to me, if we lost, to decide what to do subsequently. So here was, at least temporarily, real relief.

As spring turned to fall all decisions were delayed until MIT finally decided to not take the role of plaintiff and that it would be entirely up to us. The MIT lawyers delayed a final decision until just before the start of the 1965 Fall term when it suddenly became clear that I would have to go to court to get an injunction in order to teach that Fall. MIT and my department chairman were adamant that if the injunction was not in place by (I am not kidding here) by precisely 2:30 pm on September 20, I would not be able to teach the Fall semester and there was no elasticity allowed in the time. We were scheduled in court for that day but there was no certainty we could get a judgment by 2:30. My interview with my department chairman was very unpleasant and he suggested an obvious way out of my troubles was to just sign the oath and get it over with. The department chairman, the estimable Ted Martin, conveyed to me the time limit with great severity in the presence of his executive assistant. I only found out recently that he had gotten into some political trouble for his own very liberal views as a young man and was probably reluctant to be involved again.

Berlin sprang into action and we rushed by subway to Superior Court in Cambridge. I signed our papers requesting an injunction to prevent my being fired while on the Green Line and we used fellow passengers to witness our signature. At 1:30 pm we had the injunction so, for at least a while, I could teach. At the same time we filed for an injunction in Superior Court to find the Loyalty Oath Law unconstitutional. As Berlin noted at the time, this would be passed directly for decision to the State Supreme Judicial Court.

We needed to do some fund raising at MIT and I formed a committee of people, one from each department, who agreed, without committing themselves to the correctness of our case to act as funnels for the money that would be held by a Episcopal minister at MIT for Berlin's costs.

MIT COMMITTEE FOR THE LOYALTY OATH FUND

DEPARTMENT	COMMITTEE MEMBER	NUMBERS	ADDRESS	PHONE
ADMINISTRATION	WILLIAM C. GREENE	50	14N-411	4455
AERONAUTICS & ASTRONAUTICS	LEON TRILLING	50	33-410	4529
ARCHITECTURE	HENRY MILLON	50	7-308	4405
ATHLETICS	CHARLES BATTERMAN	20	W32-109	4918
BIOLOGY	SALVADOR LURIA	55	56-421	4707
CENTER-INTERNATIONAL STUDIES	DONALD BLACKMER	20	E53-467	3141
[CHEMICAL ENGINEERING	MAX DEIBERT]	30	12-184	4581
CHEMISTRY	JOHN WAUGH	60	2-121	4550
CITY & REGIONAL PLANNING	AARON FLEISHER	15	7-344	4462
CIVIL ENGINEERING	DONALD HARLEMAN	55	48-213	2726
ECONOMICS	EDWIN KUH	35	E52-254B	3637
ELECTRICAL ENGINEERING	RICHARD ADLER	160	13-3006	4651
GEOLOGY & GEOPHYSICS	RAYMOND HIDE	50	54-520	3396
HUMANITIES	ROY LAMSON	80	14N-413	4447
MANAGEMENT	MICHAEL BROWER	100	E52-556	2655
MATHEMATICS	NORMAN LEVINSON	90	2-365	4387
MECHANICAL ENGINEERING	RONALD PROBSTEIN	90	3-254	2240
MEDICAL	SAMUEL CLARK	30	11-110	4486
METALLURGY	**CYRIL STANLEY SMITH	50	14N-317	3722
METEOROLOGY	ERIK MOLLO-CHRISTENSEN	20	54-1722	6281
MODERN LANGUAGES & LINGUISTICS	NOAM CHOMSKY	30	20C-132	5221
NAVAL ARCHITECTURE	J. HARVEY EVANS	20	5-230	4333
NUCLEAR ENGINEERING	ELIAS GYFTOPOULOS	25	24-109	3804
NUTRITION & FOOD SCIENCE	GEORGE WOLF	50	56-235	6781
PHYSICS	DAVID FRISCH	100	24-036	2396
POLITICAL SCIENCE	FREDERICK FREY	35	E53-563	3645
PSYCHIATRY	JOSEPH BRENNER	18	7-202	2917
PSYCHOLOGY	RICHARD HELD	25	E10-014	5745
RELIGIOUS COUNSELORS	MYRON BLOY	10	317 M.D.	2326
	TOTAL	1580		

ASSISTANCE FROM:

HUMANITIES	DAVID SCHALK
	PETER GURNEY
	BERT DREYFUS
MODERN LANGUAGES	DAVID PERLMUTTER
MANAGEMENT	DON FARRAR
PHYSICS	AL LAZARUS

** METALLURGY: MAIL TO THOMAS KING 50 8-307 3303

Once all these people were involved the word of what was going on got out and became public. First, articles appeared in the Tech

Loyalty oath questioned

By Chuck Kolb

Two MIT professors have obtained an injunction in the Superior Court of Massachusetts seeking to have the state's Teachers' Loyalty Oath declared unconstitutional.

Dr. William Watson, Assistant Professor of History, and Dr. Joseph Pedlosky, Assistant Professor of Mathematics, are conducting what is believed to be the first legal test of the oath, which every public and private school teacher or professor in the state is required to sign, since the law was passed in 1935.

Oath positive

The oath in question requires each signer to swear that he will "support the Constitution of the United States and the Constitution of the Commonwealth of Massachusetts." This type of oath is not generally considered as objectionable as negative oaths which read "I am not now and never have been . . ."

The law applies only to United States citizens and provides for fines up to $1000 for violations of the oath.

Professor Pedlosky has refused to sign the required oath, and the litigation will be conducted in his name. Professor Watson signed the oath while teaching at Wellesley several years ago but has not signed it since coming to MIT.

Committee formed

An MIT Committee for the Loyalty Oath Fund has been formed by faculty members from all departments in order to obtain funds and support for the legal action. Members include William C. Greene, Administration; Leon Trilling, Aeronautics and Astronautics; Henry Millon, Architecture; Charles Ballerman, Athletics; Salvador Luria, Biology; Donald Blackmer, Center for International Studies; John Waugh, Chemistry; Aaron Fleisher, City Planning.

Donald Harleman, Civil Engineering; Edwin Kuh, Economics; Richard Adler, Electrical Engineering; Raymond Hide, Geology; Roy Lamson, Humanities; Michael Brower, Management; Norman Levinson, Mathematics; Ronald Probstein, Mechanical Engineering; Samuel Clark, Medical; Cyril Smith, Metallurgy; Erik Mollo-Christensen, Meteorology; Noam Chomsky, Modern Languages; J. Harvey Evans, Naval Architecture; Elias Gyftopoulos, Nuclear Engineering; George Wolf, Nutrition; David Frisch, Physics; Frederick Frey, Political Science; Richard Held, Psychology; and Myron Bloy, Religious Counselors.

Funds sought

Last week the Committee sent letters to each member of the

(Please turn to Page 2)

The Tech

Vol. 85, No. 19 Cambridge, Mass., Wednesday, Oct. 13, 1965 5c

and the Harvard Crimson (copy of Crimson 10/11/65)

Loyalty Oath Is Attacked At MIT

By ANN PECK

Two M.I.T. professors who wish to have the Massachusetts Teachers Loyalty Oath declared unconstitutional have obtained an injunction from the State Superior Court and are awaiting funds to carry out their plea.

This first test of the oath since the law was passed in 1935 was initiated by MIT when Professors Joseph Pedlosky, in whose name the litigation will be conducted, and William Watson refused to sign the loyalty oath, which is a precondition for employment in Massachusetts public and private schools.

Gerald Berlin, a Boston lawyer, has agreed to take on the case without charge, but money is needed for printing and court costs. The MIT Committee for the Loyalty Oath Fund, consisting of representatives from each department, has been set up for this purpose. The case will be dropped if financial support is inadequate.

The Massachusetts Loyalty Oath is a positive one, affirming adherence to the Massachusetts and United States Constitutions and has not been considered as controversial as those negative oaths which state "I am not and never have been. . . ."

(Continued on page six)

Loyalty Oath

(Continued from page one)

In particular, Watson and Pedlosky question the oath's application to private institutions and the clause which refers to proper discharge of teaching duties. The latter does not appear to come under the police power guaranteed to the states by the constitution, according to Berlin.

They also argue that the state should not discriminate against the teaching profession by questioning its loyalty and not that of other groups.

The fund is currently soliciting formal backing only from M.I.T. professors, but believes donations from other sources will help indicate popular support.

The case probably will pass to the Massachusetts Supreme Judicial Court, according to Berlin. If it is unsuccessful there, it can be appealed to either a three-judge Federal court or to the U.S. Supreme Court.

Berlin believes that the case has "a good chance of success" due to recent opinions handed down by the Supreme Court and to a "change in climate" of public and judicial opinion in the last two or three years.

NOTICE COLUMN

(Continued from page five)

from Vancouver Film Festival will be shown.

HRO—Sectional rehearsal for strings only at 7 p.m. tonight in Paine Hall.

Harvard Dames—Opening meeting at 8:00 p.m. Thursday in Harkness Commons Graduate Center. Guest speaker: Mrs. Nathan Pusey.

Harvard Debaters—Anyone interested in debating the national topic in a GBFA meet should come to the Executive Committee at 11 p.m. on Tuesday in the Quincy House Basement or sign the board in the Debate Room.

Harvard Student Employment Office—The Bartending Course to be offered by this office will start today (with subsequent meetings Wed. and Mon., Oct. 18). If you have not already done so, please come to the Employment Office to pay the $5 fee (time and place of first

and the Boston papers (see below copy of Dalton article from Boston Traveler, a now defunct conservative paper) not all of them sympathetic.

Political Pulse — By Cornelius Dalton

Here We Go Again On Oath Test

DR. Joseph Pedlosky, who is going to court to challenge the constitutionality of the Massachusetts teachers oath law, is an assistant professor of mathematics at MIT, which is pretty good proof that he is awfully bright.

Dr. Pedlosky also is fairly young, being only 27 according to the newspaper reports, and this may possibly explain why he decided to test the constitutionality of the law by refusing to sign the oath.

PEDLOSKY

We haven't got the slightest doubt about Dr. Pedlosky's sincerity or loyalty. But we do have some doubts about his wisdom in reviving the oath controversy, because if he had been around when the law was enacted 30 years ago he might sympathize with a friend of ours who, commenting on the MIT professor's action, inquired wearily: "Do we have to go through all that again?"

(ANOTHER MIT PROFESSOR, William B. Watson, who is 33, teaches history and is backing Dr. Pedlosky, also refused to sign the oath. But since Dr. Watson did sign it when he previously taught at Wellesley College, some authorities feel he doesn't have to sign again, because there doesn't appear to be much difference between supporting the Constitution in Wellesley and supporting it in Cambridge).

The teachers oath battle, which rocked Beacon Hill and most of our colleges and universities for several years in the 1930s, provoked a furious frenzy which seems rather ludicrous today.

Sixteen distinguished college presidents, protesting passage of the law, solemnly warned that it might be "the first step" toward the regimentation of our schools and colleges "as they are now so effectively regimented in Russia."

And some of the law's supporters patriotically proclaimed that it would save the country and the commonwealth from both atheism and Communism, and that it would drive the "Red professors" out of our colleges and universities.

Not a single Communist has ever been exposed by the teachers oath law, as Dr. Pedlosky pointed out in his statement denouncing the statute and announcing his challenge of its constitutionality.

The law, furthermore, has not proved to be the "first step" toward the regimentation of our schools and colleges, nor has it restricted the academic freedom of a single teacher or professor.

The oath which aroused so much furor in the 1930s and which Dr. Pedlosky now wants to abrogate is a rather simple one.

IT SAYS: "I DO solemnly swear (or affirm) that I will support the Constitution of the United States and the Constitution of the Commonwealth of Massachusetts, and that I will faithfully discharge the duties of the position of (title of position) in (name of institution) according to the best of my ability."

Whether Dr. Pedlosky or any other professor faithfully discharges his duties is something the Legislature has no interest in, because it has left this matter entirely in the hands of the administrators of our educational institutions. What's more, the Legislature deliberately exempted the "faithfully discharge" provision of the law from a 1948 amendment imposing a $1,000 fine for violation of the oath.

The most obvious fact about the teachers oath controversy, looking back over the years, is that a tremendous amount of emotional energy was needlessly wasted by both proponents and opponents three decades ago.

Why Dr. Pedlosky wants to start the ruckus all over again is his own business. But we think he might accomplish a lot more if he abandoned his oath fight and took legal action to force the General Court to carry out Section II of Chapter V of the state Constitution, which says that it is the duty of the legislators to encourage "good humour" among the people of the commonwealth, including, presumably, MIT professors.

Even the NY times carried a small story in which my identity was conflated with Bill Watson's so, young as I was, I had already been teaching all this time at Wellesley. It is hard for me to take newspapers stories literally since.

M. I. T. PROFESSOR CHALLENGES OATH

CAMBRIDGE, Mass., Oct. 13 (UPI)—A mathematics teacher at the Massachusetts Institute of Technology has challenged the right of the state to force him to sign a loyalty oath.

Dr. Joseph Pedlosky, 27 years old, maintains the oath "discriminates against teachers as a group."

He said today he had refused to sign the oath and had obtained an injunction against M.I.T. to prevent the school from discharging him.

The oath affirms adherence to both state and Federal constitutions. It does not mention the Communist or Nazi parties or any subversive group.

The oath law was enacted in the mid-1930's as a requirement for employment in private and public schools and colleges. A person who refuses to sign must be discharged, the law says.

Dr. Pedlosky said he was not affiliated with any political organization.

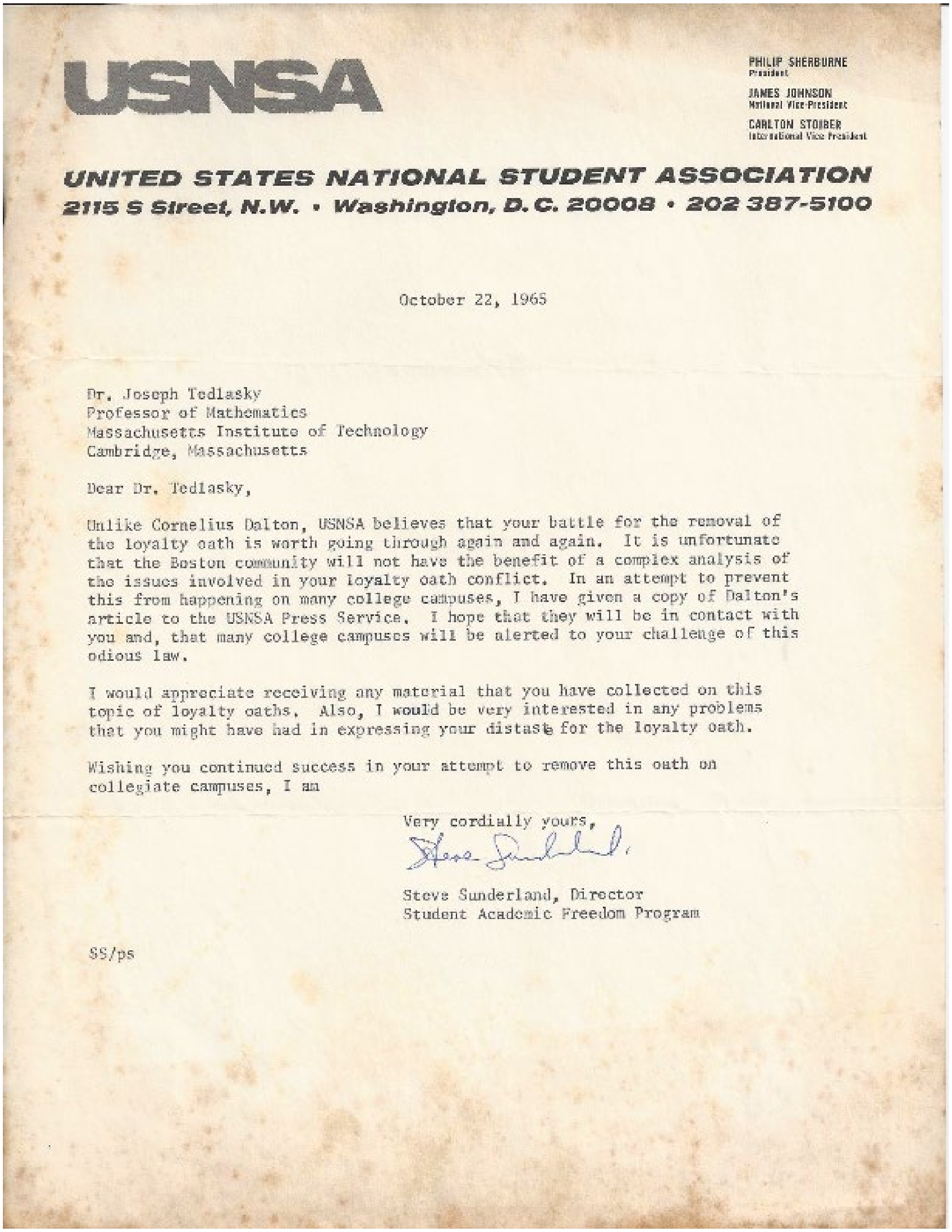
He signed the oath at Wellesley College, but refused to do so at M.I.T.

He said a group had been organized to raise funds to take the case to the United States Supreme Court if necessary.

The New York Times
Published: October 14, 1965

When the story and the committee list appeared in The Tech one of the committee members hastened to write a letter to the paper protesting the portrayal of the list as a list of supporters. He did not wish to be identified as a supporter only as a good citizen facilitating the wishes of his colleagues. You get the picture. The Oath law was both trivial and its possible consequences frightening.

After the story was in the papers I got a lot of mail, almost all of it supportive. Some even contained small checks as contributions for the defense fund.



USNSA

PHILIP SHERBURNE
President
JAMES JOHNSON
National Vice-President
CARLTON STOIBER
International Vice President

UNITED STATES NATIONAL STUDENT ASSOCIATION
2115 S Street, N.W. • Washington, D.C. 20008 • 202 387-5100

October 22, 1965

Dr. Joseph Tedlasky
Professor of Mathematics
Massachusetts Institute of Technology
Cambridge, Massachusetts

Dear Dr. Tedlasky,

Unlike Cornelius Dalton, USNSA believes that your battle for the removal of the loyalty oath is worth going through again and again. It is unfortunate that the Boston community will not have the benefit of a complex analysis of the issues involved in your loyalty oath conflict. In an attempt to prevent this from happening on many college campuses, I have given a copy of Dalton's article to the USNSA Press Service. I hope that they will be in contact with you and, that many college campuses will be alerted to your challenge of this odious law.

I would appreciate receiving any material that you have collected on this topic of loyalty oaths. Also, I would be very interested in any problems that you might have had in expressing your distaste for the loyalty oath.

Wishing you continued success in your attempt to remove this oath on collegiate campuses, I am

Very cordially yours,

Steve Sunderland, Director
Student Academic Freedom Program

SS/ps

SSRS

SOCIETY FOR SOCIAL RESPONSIBILITY IN SCIENCE

President
J. Melvem Benjamin, Jr.
Biomedical Electronics
37 E. Herman St.
Philadelphia 44, Pa.

Vice President
Norman E. Polster
Electrical Research
655 Woods Rd.
Southampton, Pa.

Secretary
Betty Polster
Mathematics
655 Woods Rd.
Southampton, Pa.

Treasurer
Mark D. Shaw
Agricultural Engineering
1317 Houserville Rd.
State College, Pa.

Room 16-734
ext. 4710

October 12, 1965

Dear Professor Joseph Pedlosky:

I have learned of your valued efforts against the state loyalty oaths only today. The MIT Society for Social Resonsibility in Science (whose aims are similar to the international SSRS--pamphlet enclosed) would like very much to have you and/or Professor William Watson address an open meeting on this subject at MIT.

We would normally expect 20 - 40 people for such a meeting, but if we are successful in getting the TECH interested in a series of articles on the controversial issues which are crucial to MIT environment and education, we could expect better attendance. See our July Newsletter articles on MIT student character, security clearances and NASA.

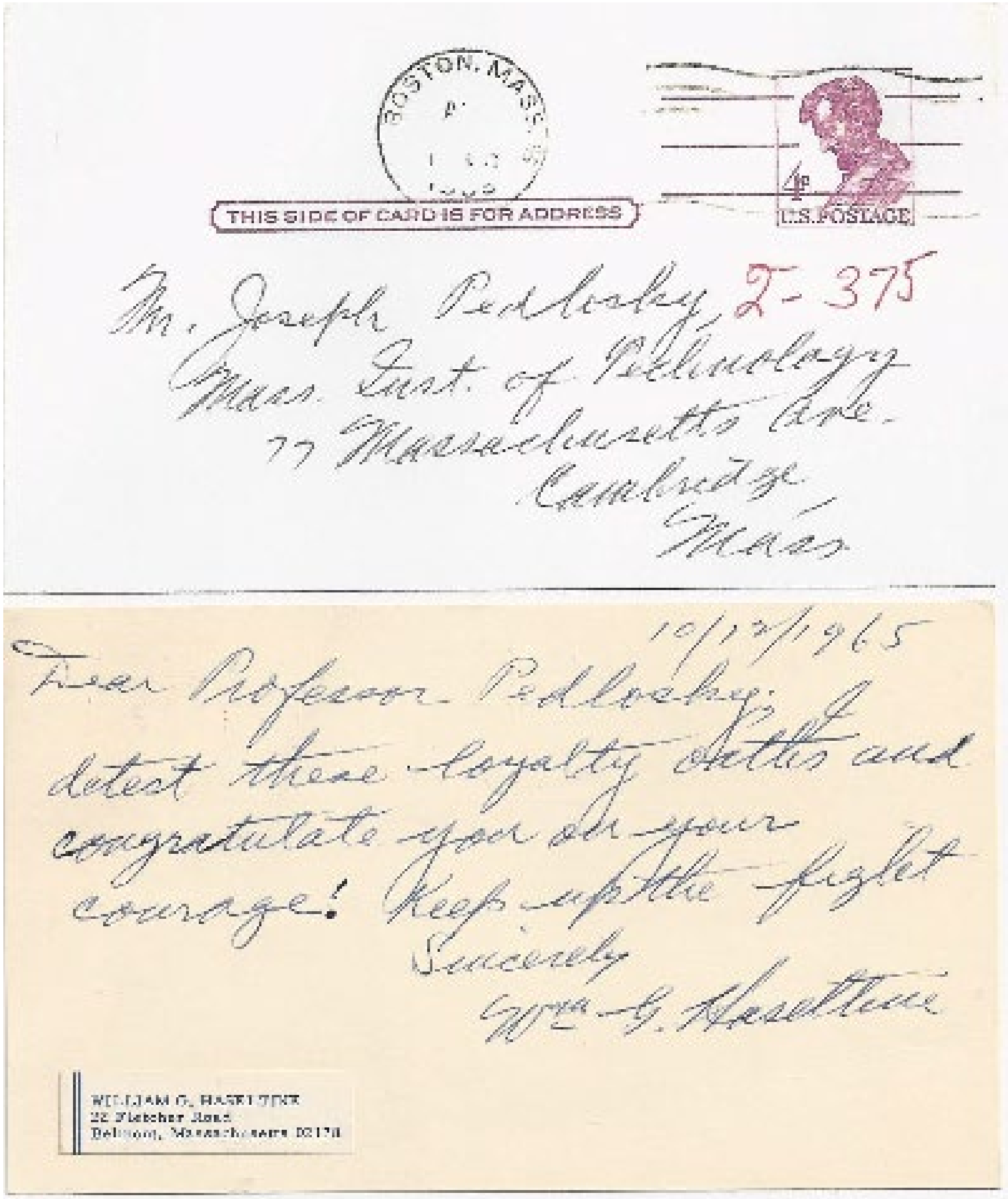
Note also our meeting next week Thursday with Joseph Fanelli, who argued the famous Chasanow case (Naval hydrographic employee whose clearance was withdrawn mainly on guilt by association charges) before the Supreme Court.

Sincerely,

Peter Ralph

Peter Ralph
Program Chairman
MIT-SSRS

cc to Professor Watson
cc to Dr. Herbert Meyer
Advisor, MIT-SSRS


BOSTON, MASS.

THIS SIDE OF CARD IS FOR ADDRESS

4¢ U.S. POSTAGE

Mr. Joseph Pedlosky Z-375
Mass. Inst. of Technology
77 Massachusetts Ave.
Cambridge
Mass.

10/12/1965

Dear Professor Pedlosky, I
detest these loyalty oaths and
congratulate you on your
courage! Keep up the fight
Sincerely
Wm G. Haseltine

WILLIAM G. HASELTINE
22 Fletcher Road
Belmont, Massachusetts 02178

11 March 1966
Woodstock, New York

Dr. Joseph Pedlowsky
Department of Mathematics
Massachusetts Institute of Technology
Boston, Massachusetts

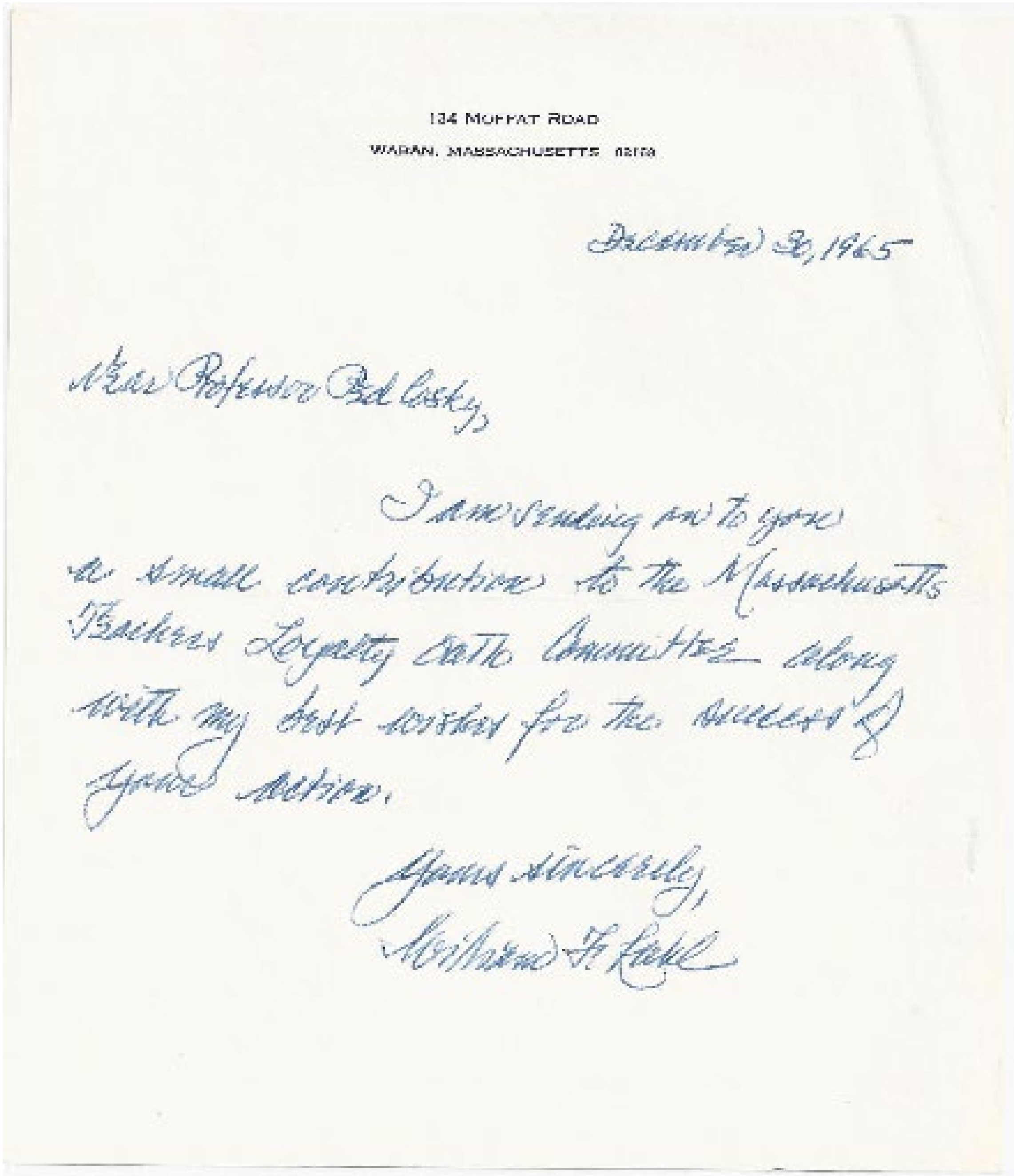
Dear Dr. Pedlowsky:

I saw your name in today's issue of The New York Times in connection with the loyalty oath problem. I'm writing to say that I support and applaud your efforts on behalf of the many of us who are compelled to sign this repugnant requirement. I hope that a conclusive and favorable judgment is the result.

Very truly yours,

D. Loprieno

D. Loprieno



134 Moffat Road
Waban, Massachusetts 02168

December 30, 1965

Dear Professor Pedlosky,

I am sending on to you a small contribution to the Massachusetts Teachers Loyalty Oath Committee along with my best wishes for the success of your action.

Yours sincerely,
[illegible]

I did get one card that I treasure for its clarity in making clear what was at stake. It was unsigned and addressed to Prof. Dr. Joseph Pedlosky and it said (the ink has somewhat faded and it is hard to read)

PHILADELPHIA, PA.
PM
20 OCT
1965

THIS SIDE OF CARD IS FOR ADDRESS

4¢
U.S. POSTAGE

Prof Dr Joseph Pedlosky
Massachusetts Inst. of Technology
Cambridge, Massachusetts

2-375

So you won't sign a loyal oath
It's a shame so many decent
boys died to save you from
Hitler slavery. They must be
turning over in their hallowed
graves. It's a shame we can't
bring Hitler back. They
should deport you and
your kind to your beloved
Russia, you who owe so
much to America and love
it so little you really
are not fit to live in
America

"So you won't sign a loyal (sic) oath. It is a shame so many decent boys died to save you from Hitler slavery. They must be turning over in their hallowed graves. It is a shame we can't deport you and your kind to your beloved Russia, you who owe so much to America and love it so little. You really are not fit to live in America"

It is probably at this point appropriate to mention that politically, I am left of center on domestic issues and probably considered slightly right of center on foreign policy and I really do love America. All during this process it was psychologically *impossible* for me to say that in public because in my own eyes it would have been equivalent to taking the loyalty oath and I wanted to make sure I kept the issue clean. That is, that I should be able teach and be accepted as a teacher without stating my loyalty just as a plumber at MIT need not declare his loyalty.

As time dragged on with one or another delay the issue grew in size. A faculty member at Harvard refused to sign (Sam Bowles, the son of Chester Bowles) and his case was held in reserve in case we lost our own. The Faculty of the Harvard Law School were pessimistic about our chances and it is particularly delicious to see how wrong they turned out to be.

The Bowles Campaign

NO WRITER ATTRIBUTED

March 24, 1966

The Middlesex Superior Court last week ordered the Corporation to reinstate Samuel Bowles until the constitutionality of the Massachusetts Teachers' Loyalty Oath is determined. The University has indicated that it will comply with that order and simply postpone any action against Bowles until a suit brought by Joseph Pedlosky, as M.I.T. professor, is heard this October.

Pedlosky has contested the constitutionally of the statute requiring the oath because it demands a "kind of orthodox nationalism contrary to the principles of free speech and thought." He claims the statute says, in effect, that the only way teachers may express their support of the constitution is to sign this particular oath. Pedlosky has included two other arguments in his suit; that the actual procedures of the law violate the notion of due process and that it discriminates against teachers, singling them out as a group whose loyalties are particularly suspect.

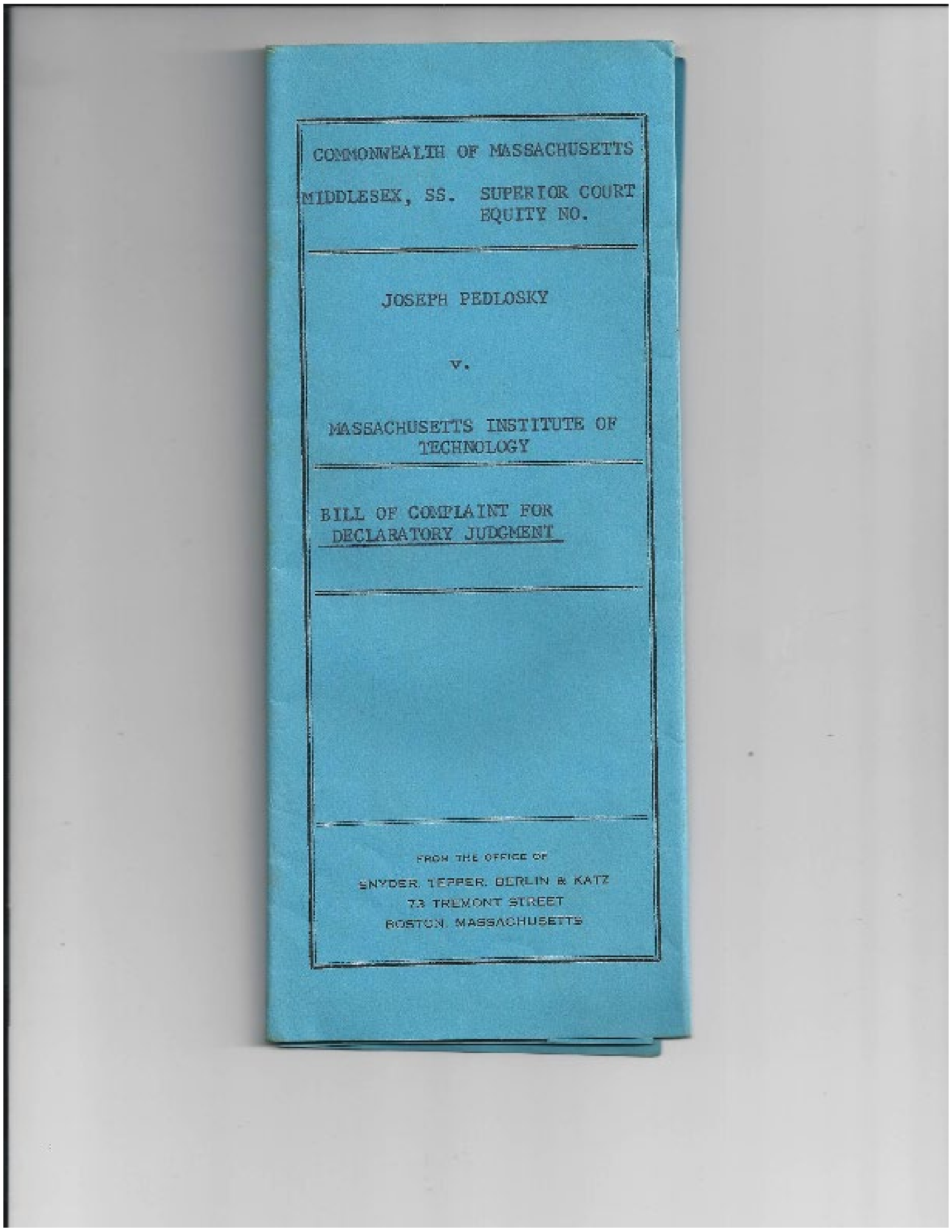
Legal experts are pessimistic about Pedlosky's chances of winning his suit. Several professors of Law have predicted that the oath will stand in the Supreme Judicial Court. The oath is widely regarded as an innocuous one; it has no disclaimer and merely requires signers to affirm loyalty to the constitutions of the Commonwealth and the United States and to promise to "faithfully discharge the duties" of their offices.

Pedlosky plans to appeal the case to the Supreme Court, and a long, perhaps futile, judicial struggle now seems inevitable. While the suit is being adjudicated--and in case it fails--Bowles, Pedlosky, and their supporters should consider the most effective remaining alternative: legislating the repeal of the oath by mobilizing sentiment on campuses throughout Massachusetts. If they sincerely believe that the loyalty pledge represents a threat to academic freedom, they should extend the campaign through petition from Harvard and M.I.T. to other colleges across the Commonwealth.

A Salem representative and the Massachusetts Federation of Teachers previously introduced motions to repeal this oath and a similar one required of public employees; they were unsuccessful primarily because they failed to marshal the support of the academic community. Certainly colleges and universities are not apathetic toward the oath; many members of the

The Massachusetts legislature was not inactive. Just to show we were not wasting our time the Massachusetts House voted 185-36 to **not** repeal the Loyalty Oath law even as our case was pending.

So, after much delay the case was heard in the Massachusetts Supreme court December 8, 1966 over two years after I had declined to sign.


COMMONWEALTH OF MASSACHUSETTS

MIDDLESEX, SS. SUPERIOR COURT
EQUITY NO.

JOSEPH PEDLOSKY

v.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY

BILL OF COMPLAINT FOR DECLARATORY JUDGMENT

FROM THE OFFICE OF
SNYDER, TEPPER, BERLIN & KATZ
73 TREMONT STREET
BOSTON, MASSACHUSETTS

COMMONWEALTH OF MASSACHUSETTS.

SUPREME JUDICIAL COURT

FOR THE COMMONWEALTH.

MIDDLESEX COUNTY. NOVEMBER SITTING, 1966.

IN EQUITY, No. 6827.

JOSEPH PEDLOSKY

v.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY.

BRIEF FOR PLAINTIFF.

Jesus F.Huidobro
Humanities Dept.
Dean Junior College
Franklin,Mass.
02038

Loyalty Oath Fund
Cambridge,Mass.
02038

Franklin,September 22,1966

Dear sirs:

I send you check for $ 1.00 after reading the letter from Mr.A.Orin Leonard to AAUP Chapter Officers For New Faculty Members in Massachusetts Institutions Of Higher Edu-cation.

Of course the amount of the check does not correspond with my feeling of solidarity with Professor Pedlosky.

Sincerily,

Jesús F. Huidobro

J.F.Huidobro.

AMERICAN FRIENDS SERVICE COMMITTEE

Chicago Regional Office for Illinois and Wisconsin

431 S. Dearborn Street, Chicago, Illinois 60605

HArrison 7-2533

Oct. 15, 1965

Joseph Pedlosky
Mathematics Department
Massachusetts Institute of Technology
Cambridge, Mass.

Dear Joseph Pedlosky:

I read about your stand in the New York Times. I wanted to tell you that last year, when I was teaching at Smith College, I agonized long and hard over signing that oath. All my friends as well as a number of people long active in civil liberties matters and the movement counselled me, really, against taking a stand and not signing. And the College was very sweet; they said they sympathized and agreed--only they couldn't pay me anything.

So I signed. I'm sorry I did, now, and feel guilty about it. Which I perhaps shouldn't say to you since that's all easy hindsight now that I'm out of academia.

But I wanted to write just to add a little support and to say my feeling that all the plausible arguments for signing are specious crap.

Yours sincerely,

Paul Lauter

Paul Lauter

HARVARD UNIVERSITY
DEPARTMENT OF SOCIAL RELATIONS

William James Hall
XXXXXXXX
CAMBRIDGE, MASSACHUSETTS 02138
14 October, 1965

Prof. Joseph Pedlosky
Department of Mathematics
Massachusetts Institute of Technology
Cambridge, Mass. 02139

Dear Prof. Pedlosky:

Enclosed is my check to aid in your fight against the Massachusetts Teachers Loyalty Oath. I wish you complete success.

If your committee at M.I.T. has prepared any kind of pamphlet or statement, or if one is prepared in the future, I would appreciate receiving a copy of it.

Thank you.

Sincerely,
Charles A. Thrall

Charles A. Thrall
Teaching Fellow

19 Appleton St.,
Cambridge, Mass.

10/15/65

Dear Professor Pedlosky:

Here's a contribution of $20. toward your court fight re loyalty oaths. (I learned about this fight through the Harvard Crimson.)

Sincerely

William A.Shurcliff
Physicist
at the CEA,Harvard

THE UNIVERSITY OF MICHIGAN
DEPARTMENT OF ENGLISH LANGUAGE AND LITERATURE
ANN ARBOR

15 October 1965

Dr. Joseph Pedlosky
Massachusetts Institute of Technology
Cambridge, Mass.

Dear Mr. Pedlosky;

I see in The New York Times for today that you are challenging the Massachusetts State Teachers' Oath, and I hasten to enclose a check for $20 as an expression of support in this worthwhile enterprise.

My reasons are more than purely abstract. In 1959 I refused to sign the oath as a Teaching Fellow at Harvard, but ultimately capitulated after it appeared that I would not be able to muster any legal support, and after a rather unpleasant interview with McGeorge Bundy. Consider this as conscience money, therefore, or payment on a debt incurred in the past.

I wish you every success, and assure you that you can count on my further support (on a rather limited scale) if the litigation should drag out.

With best wishes,

Walter H. Clark Jr.
Assistant Professor

FRIDAY, DECEMBER 9, 1966 Page 4

I pledge allegiance

Most of the Institute's staff and student body seem to be unable to generate any enthusiasm over the Massachusetts Teachers' Loyalty Oath case which finally got into court yesterday.

One reason for the lack of interest might be traced to the innocuous nature of the 30 year old oath which does little more than call on its signers to 'support the Constitution'. Since its passage in 1935, even the most liberal-minded members of the Massachusetts academic community have tended to regard the law as a mere nuisance, not worthy of the effort that it would take to challenge its validity.

While it is true the state's teachers' oath hasn't really restricted anyone's academic freedom, we feel that Joseph Pedlosky '59 was right to challenge the law when he was appointed to the faculty last fall. We can see no reason why a faculty member of a private university like MIT or Harvard should be forced to sign any statement of intellectual intent by the state of Massachusetts.

Even an innocuous oath like the present teachers' oath can set a dangerous precedent for state control over what can or cannot be expounded by a university professor.

We hope the state's Supreme Judicial Court will give the case a quick and fair decision. Since the present oath contributes no good to the state or the academic community, we feel the wisest decision would be to remove it from the books.

THE TECH

Vol. LXXXVI, No. 51 Dec. 9, 1966

Loyalty oath case faces deliberation

by Dean Roller

The Supreme Judicial Court of Massachusetts is now deliberating the case of Joseph Pedlosky, Prof. of Mathematics, against the Massachusetts Teachers' Loyalty Oath.

Pedlosky challenged the constitutionality of the oath by refusing to sign it last fall. When MIT made it clear that it had no intention of prosecuting Pedlosky, the Attorney General's Office assumed the task of preparing a defense of the oath's constitutionality.

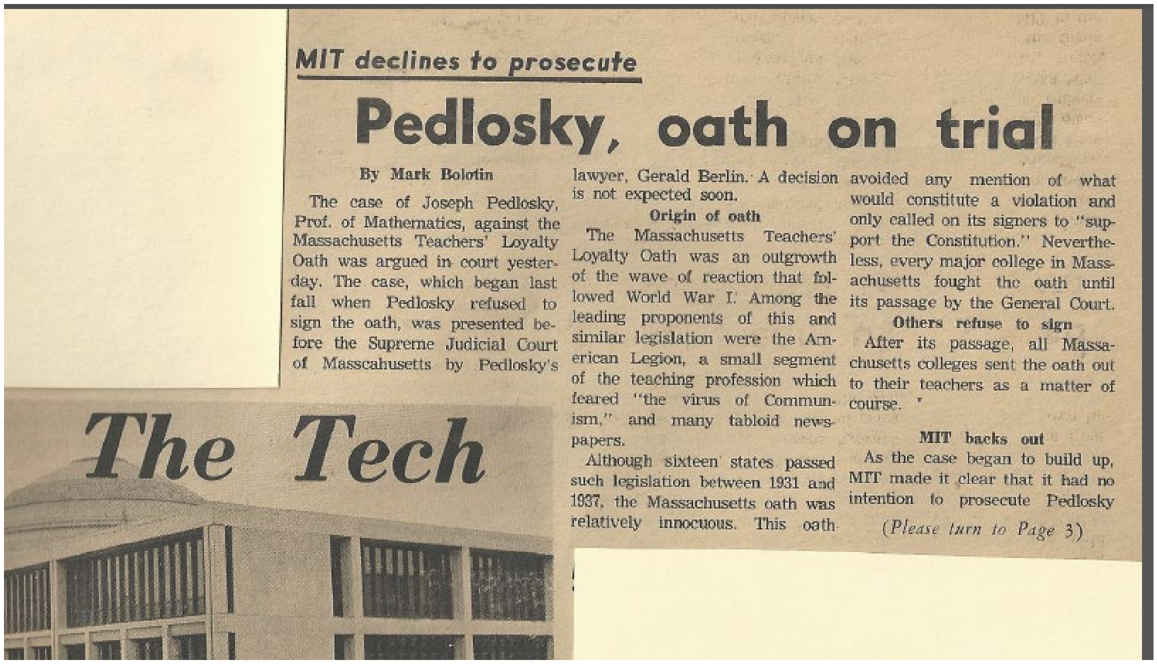
Gerald Berlin, attorney for Pedlosky, argued Thursday against the vagueness of the oath, stressing that it applies only to teachers, including those at private institutions. Berlin challenged that the loyalty oath forbids only "treason or slave-holding."

Edward T. Martin, first deputy attorney general, defending the oath's constitutionality stated that even if the oath was interpreted so narrowly, the Court was still obliged to uphold it as such.

Presiding at the session was Chief Justice Raymond S. Wilk-

(Please turn to Page [illegible])

Pedlosky On Trial!!



MIT declines to prosecute

Pedlosky, oath on trial

By Mark Bolotin

The case of Joseph Pedlosky, Prof. of Mathematics, against the Massachusetts Teachers' Loyalty Oath was argued in court yesterday. The case, which began last fall when Pedlosky refused to sign the oath, was presented before the Supreme Judicial Court of Masscahusetts by Pedlosky's lawyer, Gerald Berlin. A decision is not expected soon.

Origin of oath

The Massachusetts Teachers' Loyalty Oath was an outgrowth of the wave of reaction that followed World War I. Among the leading proponents of this and similar legislation were the American Legion, a small segment of the teaching profession which feared "the virus of Communism," and many tabloid newspapers.

Although sixteen states passed such legislation between 1931 and 1937, the Massachusetts oath was relatively innocuous. This oath avoided any mention of what would constitute a violation and only called on its signers to "support the Constitution." Nevertheless, every major college in Massachusetts fought the oath until its passage by the General Court.

Others refuse to sign

After its passage, all Massachusetts colleges sent the oath out to their teachers as a matter of course.

MIT backs out

As the case began to build up, MIT made it clear that it had no intention to prosecute Pedlosky

(Please turn to Page 3)

The Tech

This, I have to say was one of the moments when I was most proud to be an American. The courtroom was packed and I knew very few people there. I sat way in the back to try to see everything. When the Justices entered and the case began I had a profound thrill. I was an unknown, with no money and no influence and this whole process, the whole argument, unfolding before my eyes was happening because I had said “no” to a law I thought was unjust. Win or lose I felt so good that the machinery really was working the way I had always believed it would. I can never be cynical about the “system” after that.

MIT refused to argue for the law so it was defended by the State Attorney General’s office. The ACLU, two years after turning me down, filed a friend of the court brief and Gerry Berlin argued our case eloquently basing it on our view of its unconstitutionality because of its coercive demand for an oath of loyalty and for its legal weakness for its vagueness. How, for example, could I know if it could be determined that I was teaching to the best of my ability or if in making a statement about a political disagreement that I was not supporting adequately the constitution of the state of Massachusetts (whose provisions I admit never to have read, have you?). The arguments finished, I left the courtroom and waited.

I have to remind you that in this two-year period I was still teaching and carrying out my research. Indeed, my work was a great solace during this time and was the basis of a deep friendship I developed with my colleagues Harvey Greenspan and Victor Barcilon. I recall that Harvey, who was my immediate mentor or boss, and who had a reputation for being a really hard case, asked me at the beginning of the whole deal whether I was really serious about carrying it through to the end. I said *I thought so*. He grunted what I took to be approval and we moved on to our work on Rossby waves and spin-up but he never ceased to support me with generous sensitivity.

Finally, at the beginning of March 1967 I got a phone call from a reporter on the Crimson (they were *really* plugged in) who told me before anyone else could reach me that the Court had decided to throw the law out on the vagueness issue. While being a tad disappointed that they did not address the issues of the coercion of statements of loyalty I was delighted, simply delighted that we had won.

TEACHER OATH LAW IN BAY STATE VOIDED

BOSTON, March 3 (AP) — The Massachusetts Supreme Court, in a unanimous decision ruled today that the state's teacher oath law was invalid.

Chief Justice Raymond S. Wilkins, in a three-page decision, said the teacher oath law "is not a reasonable regulation in the public interest."

The ruling came on a suit brought by Joseph Pedlosky, an assistant professor at Massachusetts Institute of Technology. He contended that the oath law violated both the national and state Constitutions.

Massachusetts's teachers oath, enacted in 1935, requires every teacher in a public or private school to take an oath to support the United States and Massachusetts Constitutions. Violations can be punished by a fine up to $1,000.

Mr. Pedlosky, a mathematics teacher, brought suit to declare the oath unconstitutional. His attorney said at the time that teachers had been denied employment at Harvard University and several state colleges because they refused to sign the oath.

The State Supreme Court decision did not pass on constitutional objections, ruling only that since the oath law required the teacher to swear that he will perform his teaching job "to the best of my ability" in addition to swearing to support the Federal and state Constitutions, the law is "altogether too vague a standard to enforce judicially."

N. Y. Times

The Weather
Partly cloudy, high 40's

vard Crimson

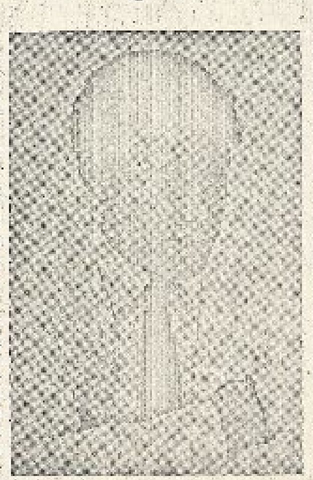
IBRIDGE, MASS., FRIDAY, MARCH 3, 1967 TEN CENTS

Teacher Oath Struck Down By Supreme Judicial Court

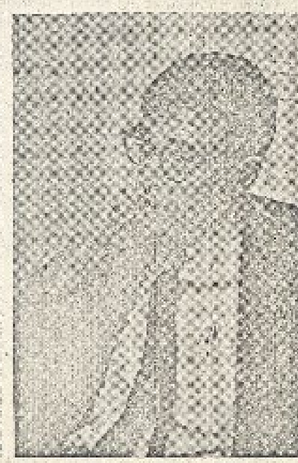
By JAMES M. LARDNER

The Massachusetts Supreme Judicial Court yesterday struck down the Commonwealth's 30-year-old teachers' oath law.

In a short, unanimous opinion, the Court carefully avoided the constitutional questions raised by the oath's opponents, pinning its decision instead on a clause requiring signers of the oath to "faithfully discharge" their duties "according to the best of [their] abilities."



SAMUEL BOWLES
Interested Observer



JOSEPH PEDLOSKY
Winning Litigant

The 500-word decision, written by Chief Justice Raymond S. Wilkins '12, resolved a suit brought by Joseph Pedlosky, the associate professor of Applied Mathematics at M.I.T. who refused to sign the oath more than two years ago. At that time, Pedlosky obtained an injunction barring M.I.T. from firing him until the oath could be tested in court.

Samuel Bowles, assistant professor of Economics here, also refused to sign, and obtained a similar injunction contingent on the outcome of the Pedlosky case.

The limited scope of yesterday's decision produced speculation that the Massachusetts General Court (state legislature) might move to draft another teachers' oath without the illegal clause. "I don't know whether this is the first round or the end of the fight," said Gerald Berlin, attorney for Pedlosky.

Spit in Court's Eye

Mary B. Newman, Republican state

Cambridge Police Begin Square Button Struggle

By ROBERT J. SAMUELSON

Cambridge police have begun a low-key campaign to cut down on the sale

Since last Thursday, he said, police have visited the store four times, bring-

State's High Court Kills Teacher Oath

The controversial 32-year-old teachers oath law is invalid the Massachusetts Supreme Judicial Court ruled today.

The high court, in a decision written by Chief Raymond S. Wilkins, struck down the law in a suit brought by Dr. Joseph Pedlosky of Boston, an associate professor of applied mathematics at Massachusetts Institute of Technology. Dr. Pedlosky refused to take the oath in 1965 and secured temporary injunctions barring M.I.T. from refusing to hire him pending a court decision in his case.

The Supreme Court's decision did not pass on the constitutional objections to the oath raised by Dr. Pedlosky. The high court ruled only that since the oath law required a teacher to swear that he would perform his teaching job "to the best of my ability" in addition to swearing to support the Federal and state constitutions,

★ OATH LAW

Continued from Page 1

the law was "altogether too vague a standard to enforce judicially."

The Supreme Court said the courts should not be put in the position of having to "determine the degree of skill and faithfulness" with which a teacher performed his classroom duties.

The Supreme Court said that the oath "is not confined merely to a declaration of support of the Federal and state constitutions." The oath also "equally concerns an undertaking" by a teacher as to how he will perform his teaching tasks.

The court noted that the $1000 fine penalty provided by the teachers oath law applied only to any violation of the oath requiring support of the constitutions and not to that part of the oath concerning teaching duties.

The court added, "But the fact remains that the courts are exposed to the very real possibility of being asked to determine the degree of skill and faithfulness with which the plaintiff (Dr. Pedlosky) discharges the duties of his private position in teaching mathematics and perhaps to compare that degree with that of the best of his ability."

The court said that the teachers oath law "is not a reasonable regulation in the public interest."

Questions of constitutionality concerning the Federal Constitution, the Massachusetts court said, "are primarily for the Supreme Court of the United States." The state court said the U.S. court had not ruled on oaths like the Massachusetts teachers oath.

The court, in passing over the constitutional questions, said it would not be "constructive" for the Massachusetts court to provide at present the foundation for deciding Federal constitutional questions.

The teachers oath law was originally enacted by the Massachusetts Legislature in 1935 as the result of a campaign led by the then Rep. Thomas Dorgan of Dorchester. The law has been under attack over the years.

In his argument in behalf of Dr. Pedlosky before the Massachusetts high court, Atty. Gerald A. Berlin said, "The oath was the product of a struggle of incredible ferocity between an aggressive legislative and community segment brimming with patriotic fervor, intolerant of dissent and fearful of subversion in the classroom, on one hand, and on the other an embattled education community that felt stigmatized for being singled out and certain that the oath was a device to suppress dissent."

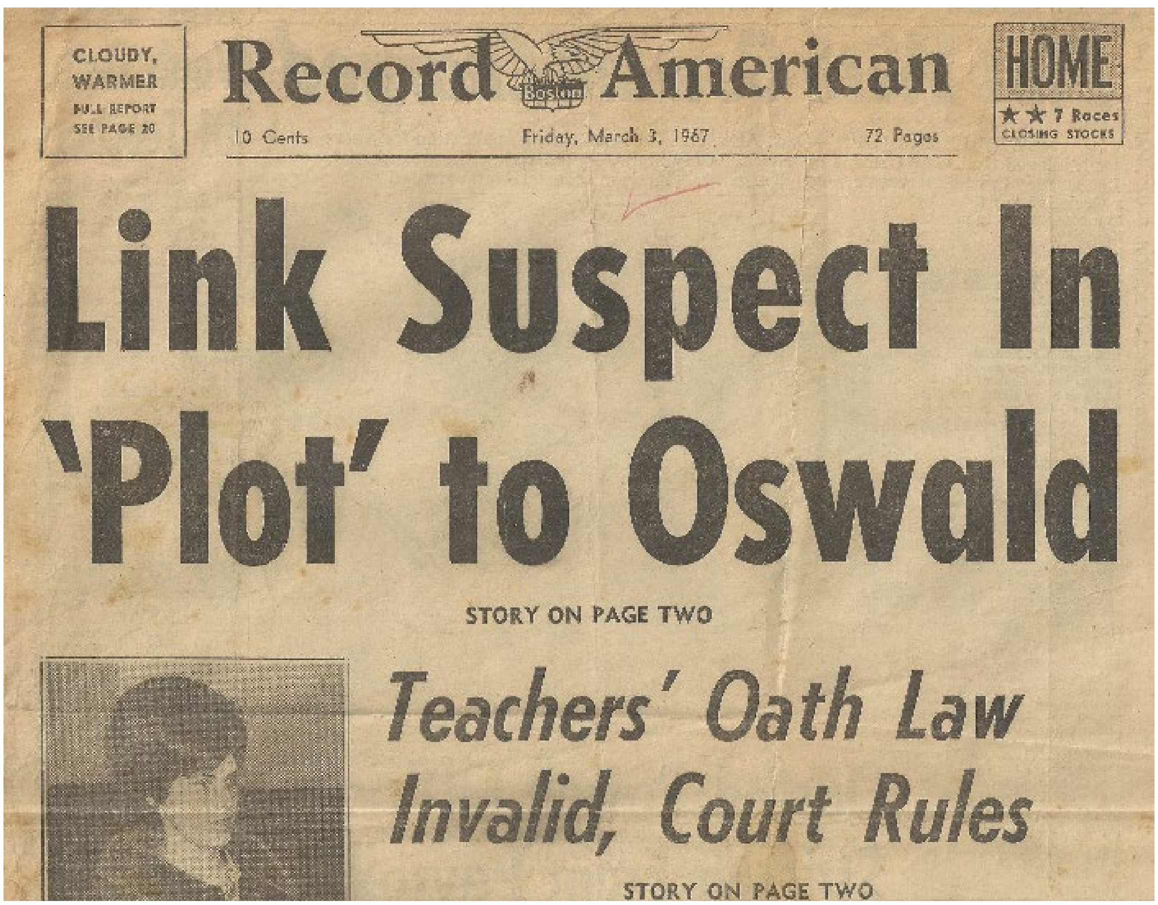
In support of the oath law, Asst. Atty. Gen. Edward T. Martin argued that the law was "a reasonable regulation of the right to teach." If the law was unwise, Martin argued, it was for the Legislature, not the courts, to correct it.

In his suit filed to set aside the law, Dr. Pedlosky claimed the law restricted his freedom of speech. He also claimed the law singled him out "and the professional class with which he is identified for disparagement by requiring assertions of loyalty and professional fidelity not otherwise required of other professional groups or citizens."

Dr. Pedlosky was notified by M.I.T. in July 1965 that he could not begin teaching in September that year until he took the required teachers oath. At that time, Dr. Pedlosky was working on a Summer program on geophysical fluid dynamics at the Woods Hole Oceanographic Institution at Woods Hole, Cape Cod.

In the suit Dr. Pedlosky said he had "always respected his obligations as a citizen of the United States and the commonwealth" and was "deeply committed to his calling as a teacher."

The Supreme Court's decision was unanimous.



CLOUDY, WARMER
FULL REPORT SEE PAGE 20

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Boston

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10 Cents — Friday, March 3, 1967 — 72 Pages

Link Suspect In 'Plot' to Oswald

STORY ON PAGE TWO

Teachers' Oath Law Invalid, Court Rules

STORY ON PAGE TWO

High Court Voids Oaths Of Teachers

The Massachusetts Supreme Court ruled invalid Thursday the state teachers' loyalty oath. In a unanimout decision, written by Chief Justice Raymond Wilkins, the court ruled that the oath, which both public and private teachers must sign, is "altogether too vague a standard to enforce judicially."

"The substance of the oath is not confined merely to a declaration of support of the standard to enforce judicially."

"The substance of the oath is not confined merely to a declaration of support of the federal and state constitutions," Justice Wilkins wrote in the opinion. "It equally concerns an undertaking by the plaintiff that 'I will faithfully discharge the duties of the position . . . according to the best of my ability'."

The case was brought before the State's high court by Joseph Pedlosky, an assistant professor of mathematics at the Massachusetts Institute of Technology, who sought to have the oath declared a violation of the federal and state Constitutions.

"The courts are exposed to the possibility of being asked to determine the degree of skill and faithfulness with which the plaintiff discharges his private position in teaching mathematics, and perhaps to compare that degree with that of the best of his ability," Justice Wilkins wrote. "It is not a reasonable regulation in the public interest," he added.

Pedlosky refused to sign the oath in October of 1965 and later led a group of supporters seeking to have the Legislature repeal the 33-year-old law.

Justice Wilkins noted the U.S. Supreme Court had "made no pronouncement on oaths so limited in scope . . . and that no decision of that court deals with an oath confined to supporting the federal Constitution or that of a state."

Pedlosky said he was "enormously pleased" by the decision.

He was one of two MIT teachers who refused to sign the oath but only Pedlosky brought suit in the case. The other man was William Watson, an assistant professor of history.

ADVERSE EFFECT

"We felt it just violated certain fundamental constitutional guarantees," the 28-year-old Pedlosky said.

"We felt it was not good for teachers, universities and the Commonwealth to make some statement that has really nothing to do with teaching.

"We felt we should fight it because it would have an adverse effect on teachers and their students," he said.

Pedlosky had obtained an injunction in Middlesex Superior

Turn to Page 6, Col. 3

The Boston Globe

6 SATURDAY, MARCH 4, 1967

Great Good Riddance

Attorney Gerald A. Berlin's disappointment in the midst of well deserved victory for his client in the Supreme Judicial Court's unanimous invalidation of the 32-year-old teachers' loyalty oath is understandable.

He had argued mainly that the law is an abridgement of the Constitution and had asked that it be declared illegal on that ground. The court, however, as have the United States Supreme Court and several state courts on similar occasions, went, instead, to fringe issues. It struck down the law because of ambiguous language which made it "altogether too vague a standard to enforce judicially."

Thus, instead of the loaf he asked for his client, Mr. Berlin gets half a loaf; and his client, M.I.T. mathematician Joseph Pedlosky, happy as he too is with his victory, is a mite dissatisfied also.

"Anyone with a spark of idealism," he says, "would like the court to say the oath is a violation of the rights of man."

Such a violation it surely is (or was), and for more reasons than one.

Wholly aside from the manner in which such laws wrench the Constitution out of shape, this one set teachers apart from the rest of society and was downright silly besides.

Patriotism and loyalty are matters of the spirit. They cannot be legislated. A loyalty oath is made to order for the disloyal. There is no handier device for cloaking perfidy. It is a test of nothing except a community's will to live either legally or hysterically. The wonder is that it has taken Massachusetts 32 years and countless victims to get rid of it.

Undoubtedly, the courts will run out of fringe issues one of these days and address themselves directly to the constitutional question itself. That will be a great day for all.

Meanwhile they labor under the pragmatic Holmes-Frankfurter theory of judicial restraint, a sound enough theory especially when the courts themselves are being irresponsibly assailed by some for "usurping the legislative authority."

The theory, simply stated, adjures courts never to attempt to decide any more than they have to decide to meet the ends of justice in any particular case.

It is good theory. And it works.

There were other satisfactions:

The same vice-president who had threatened to fire me apparently had an epiphany (letter from Kispert to faculty 4.6/67),

April 6, 1967

To Members of the Faculty and Teaching Staff

Gentlemen:

The Supreme Judicial Court of Massachusetts recently declared the so-called Teachers Loyalty Oath Law unconstitutional. This law, therefore, no longer has application to the teaching staff of the Institute.

The decision of the highest court in Massachusetts was occasioned by a suit commenced in September, 1965, between Assistant Professor Joseph Pedlosky and the Institute. By agreement between the Institute's counsel and the plaintiff's counsel and at the request of both parties, the case was reported directly to the Supreme Judicial Court.

It will be recalled that when this law was proposed to the Massachusetts Legislature in 1934, the President of the Institute openly opposed it. After the law was enacted, the Institute was obliged to comply with its provisions as long as it was in force. It has been and still is our belief that the law was not a sound law. Accordingly, in compliance with our instructions, our counsel did not argue or submit a brief in defense of the law.

We propose to destroy all copies of the Oath cards in the near future. However, a faculty member who wishes to have his signed oath copy returned, should communicate with the Registrar, Mr. Warren Wells.

Sincerely yours,

M. G. Kispert
Vice President, Academic Administration

Rensselaer Polytechnic Institute

TROY, NEW YORK 12181

Department of Mathematics

3/9/67

Dear Joe,

Congratulations on your loyalty oath victory. Stubborn bastards like you make life pleasanter for milder types like me.

Sincerely,

Lee (Segel)

Colleagues at other institutions offered congratulations (Segal, letter) and I even got a congratulatory letter from an old girl friend (that since I was married, I never answered).

Dear Joe,

You've come to mind many times over the past few years, but I never expected to have you pop up on the front page. So here is one more piece of correspondence to add to the pile of mail you must be getting these days.

I'm sure you're enjoying your work just as much as you did in your student days, and I'm very pleased for you.

Imagine my embarrassment if you're not you, but how many Joe Pehlaskys can there be, just as how many could there be of me?

Congratulations on winning your case,

Best regards,

Carla [illegible]

But, most presciently, I got a letter from Townes reminding me (us?) that such victories are never permanent.

Massachusetts Institute of Technology
Cambridge, Massachusetts, 02139

Room 6-207

April 10, 1967

Professor Joseph Pedlosky
Room 2-375
M. I. T.

Dear Joe:

I want to tell you how delighted I am that you have been instrumental in toppling the teacher's loyalty oath law. While one can regret that the court did not face all of the issues, still you have succeeded in eliminating for some time (and perhaps for a very long time) an odious regulation. Everyone else in the academic world can be thankful to you and Bill Watson for what I know was a time-consuming effort and for your success.

Sincerely,

Charlie T.

Charles H. Townes
Institute Professor

CHT:pd

cc: Professor W. B. Watson

Even then the story was not clearly concluded. Some members of the legislature were eager to start the battle again. The notorious Louise Day Hicks of anti-busing fame supported a new Oath law. The vice chairman of the legislative committee for the American Legion, Jeffery Moulton, (quoting a Tech story), said he “represented 80,000 veterans, ‘Give me a child for two years and I’ll give you a Communist. ‘ Moulton said in an alleged quotation form Marx and Lenin. The public has a right to watch the teachers who build our “citizens of tomorrow”. At that time their efforts died.

Was it worth it? Thinking about Townes’ letter, it *has* been now a very long time since that victory and maybe the lesson has faded. In the present climate of fear and with Federal legislation of the type of the Patriot Act, a Palmer-like Attorney General, and with the imprisonment of American citizens without trial or access to lawyers it is not clear that this particular controversy is ever permanently resolved.

So, I will leave the answer of my question to you. Or better yet, pose the question: what will *you* do next time around?

Appendix B
The Ewing Award Response

10-25-11

Thank you, Liu, for that very kind introduction. I am, of course, deeply honored by the Maurice Ewing award. The vast scope of Maurice Ewing's contributions can only make me feel humble in comparison.

As always, I am grateful to my mentors: Jule Charney, Melvin Stern, Eric Mollø-Christensen and Harvey Greenspan. Some of them are, alas, now beyond my power to thank them directly. My gratitude to my colleagues at MIT, University of Chicago and Woods Hole extends to too many people to be able to name them all at this time but my debt to them is immense. I am delighted to express my gratitude to all my students for the pleasure to have seen them flourish as keen, independent scientists and particularly tonight to Zhengyu Liu and Paola Cessi who jointly nominated me for this award.

Now, though, I want to thank a very special person, my wonderful wife Holly. I have observed in the past that usually thanks are given to a spouse for putting up with an absent partner who spends many late nights in the lab or weeks at sea. As a theoretician, and one with limited mental stamina, I rarely worked in the evenings or on weekends, although brooding over a problem, or lack of one, can be full time. We know the cycle of theoretical work: formulation, painful perplexity, the occasional epiphany and the repeat of the cycle. What is less commented upon is the theoretician's anguish in searching for a new, good problem. The fear that the last good problem is in fact the *last* good problem is oppressive. Reassurance that one has always found a new problem before is not convincing since that speaks to history and not to the future. When Holly becomes aware that I am particularly grumpy, she knows it is because I am in that oneiric, half- awake state, of confusion and anxiety. Somehow, for all these years, she has patiently helped shepherd me to the other side of that abyss where I can emerge into the desired state of constructive perplexity. So, I think she deserves most of the credit this evening.

One final thought. The calculation of the probability of intelligent life in the universe is a difficult one. A nearly infinite number of possible host planets and a near zero probability of intelligent life on any one of them means that the product could indicate a single event. Ours. After all, even here, the lengthy age of the dinosaurs produced no paintings of sunsets, no formulation of the Navier-Stokes equations (we had to wait for Navier and Stokes). If that is so, and we are alone in that regard, our responsibility is immense. It means that the universe is only conscious of itself by our agency. If we were not here the universe would be like a cinema showing Casablanca on an endless loop to an empty theater. It is only through us that the universe can be self-aware and if we were to blow ourselves up or render our planet uninhabitable for anything but the cockroaches the universe might as well be empty.

We are often asked to describe the larger consequences of our work. I know of nothing more noble and important than serving as the self-awareness of the cosmos. Further, it is our *communal* effort, and especially for us as scientists, it is one we need to take as a sacred trust. I am proud to be part of that effort and to place my small contributions into that mosaic of understanding we are constructing together.

Joseph Pedlosky
Scientist Emeritus
Woods Hole Oceanographic Institution
Woods Hole, MA 02543

Appendix C
Arons Award for Teaching and Mentoring

September 15, 2005

Acceptance remarks for the Arons Award September 24 2005

Those of you who knew Arnold Arons, and his fierce commitment to teaching the meaning of things and not just the names of things, will appreciate how proud and delighted I am to receive the Arnold Arons award.

I have many people to thank; many wonderful teachers and mentors have helped me on my way but first, I would like to say that to get any satisfaction from teaching you need to have good students. So, I would like to thank the Joint Program students, past and present for having made this part of my life so rewarding.

Significant mentors and teachers for me go way back and I would like to mention just a few.

Melvin Stern was my first advisor in the Geophysical Fluid Dynamics program here in Woods Hole. He combined brilliance and kindness and helped set me on my present path. My inspiring thesis advisor, Jule Charney, one of the two greatest scientists I have known, showed me how treating *all* his thesis students with dignity as true colleagues was consistent with challenging each to do fascinating work at his level. Harvey Greenspan, a superb mathematician, was my first mentor after my degree and showed how skepticism when combined with warmth and support could be a spur to greater creativity. Then when I moved to the University of Chicago my department chairman, Julian Goldsmith, showed me how it was possible to be a strong, shrewd and realistic academic administrator and remain a *mensch*, i.e. a person of warmth and integrity.

A principal reason I came to Woods Hole, like many before me, was to have the chance to work with Henry Stommel, the second of the two stellar scientists I have known, and from whom I learned so very much. Nevertheless, I would have to say that I found myself really mentored by the *whole* Physical Oceanography department. Each of my colleagues has done his or her best to make me a better oceanographer and I am profoundly grateful to them as I am to all my mentors.

Now, I mentioned Arnold Arons' *fierce* dedication to teaching. Why fierce? After all, many people think of teaching as a secondary, even an inferior activity to our research. Indeed, some people who don't understand how important teaching is to our own research have actually cautioned young scientists not to get involved in teaching too soon (whatever "too soon" means). Those of us involved in teaching *and* research know just how valuable teaching is to our own thinking but a commitment to teaching seems to me even more natural than that pragmatic explanation would suggest.

I was trying to find a way to express that thought when I recalled a book I was lent by a friend and neighbor that consisted of a series of essays presented at a conference that was celebrating the 50th anniversary of Irwin Schrodinger's book "What is life?". You may recall that Schrodinger, the famous physicist, posed as a basic question what the mechanism was for the transmission of hereditary information from generation to generation, that allowed both change and stability. The biologists at the conference were, of course, celebrating the great progress made in our understanding of the mechanism of heredity, since Schrodinger's challenge, due to the Crick/Watson DNA revolution in understanding the genetic basis for heredity.

Two of the speakers, Manfred Eigen and Jared Diamond pointed out that although the DNA mechanism is one, we humans share with all living things, humans have developed another mechanism, unique to our species, for the transmission of accumulated information and that is speech. Diamond has pointed out that the great spurt of progress our species made about 70,000 years ago in development is only weakly correlated with brain size but is more likely connected with the development of our ability to speak complex thoughts. We are unique as a species in that I can tell you about another member of our species who lived hundreds of years ago, on another continent, who wrote music that is sublime and tell you just what it is. We can pass on how to make fire, how to fuse metals, how to plant and irrigate, how to paint portraits with oil and light itself and how we can use potential vorticity to explain the ocean circulation. We can *teach*.

Seen this way, teaching is what makes us fully human. We *are* the teaching species. Teaching is what has allowed our species to survive and thrive in our environment. Looking at the challenges ahead of us we surely must realize that teaching, real teaching, must become even more valued as vital if we, as a species, are going to continue to survive and thrive. In a country like our own, in which more people believe in the folk tales of their bible than in the evidence for evolution or the origins of the universe, we clearly have our work cut out for us. It is not going to be easy.

Arnold Arons was fully and fiercely aware of all of this and this is why I am so deeply honored by this award in his name. I treasure it. Thank you.