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3 **Autobiographical Memoir: Joseph Pedlosky**

4 **4/23/19**

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Preface

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In my 81st 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.

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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

24 from mentoring by a number of other first class scientists. Part of my story is theirs
25 and perhaps that might add some more general interest in my storytelling.

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

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

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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

♦ From Mr. Sammler's Planet. *Saul Bellow* Viking Press 1970

63 probably a large contributor, along with his diabetes, to his quick temper and early
64 death.

65 My mother, Lillian Pedlosky (nee` Levit), born, like my father in 1905, was an
66 inherently elegant woman. Although she ended her education after high school, she
67 was proud of her high school diploma; many of her childhood friends never obtained
68 one. Her childhood started in a small mining town in Pennsylvania named
69 Shenandoah, after the beautiful valley in Virginia. My mother assured me it shared
70 none of the beauty and her family was one of the few Jewish families in town. Her
71 father owned a sequence of businesses, fairly successful, including a movie theater,
72 and a bakery. After her early years working as an office bookkeeper at Prudential
73 Insurance Company in Newark, the city where she met my father, she had to leave
74 her job because married women were not allowed to continue to work at the
75 company. She was then a housewife when I was young but later, when I was about
76 15, she, too, took on a job as a bookkeeper in a clothing store in Paterson to
77 supplement the family's finances. Both my mother and father were physically
78 stretched to exhaustion. Still, on the occasions of weddings and bar-mitzvah's some
79 inherently joyful aspects of their personalities would peek through and it was a
80 childhood pleasure to see them dance and laugh together in a short vacation from
81 reality.

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

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

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

104 The readers we had in elementary school, the Dick and Jane series, were
105 another source of perplexity. In those books the father and mother were so confident,
106 kind, and lacking for nothing that they were unrecognizable types for me. The
107 economic situation for Dick and Jane was also foreign. So, I began to wonder if
108 somewhere there was a possible life different from my own, one less tense and one

109 less insecure. We were living at the time I started school during WWII in a small
110 three-room apartment. With my younger sister and just one bedroom, it meant I slept
111 in the living room on a cot wheeled from a closet in my parents' bedroom each
112 evening. My sister Irene slept in a crib in the single bedroom with my parents until
113 we moved to our own house when I was 11 and she was 6. I assumed this was
114 normal. It did mean that when my Dick and Jane reader had a chapter about Dick's
115 favorite tree in his backyard, where he delighted to sit in its shade on warm sunny
116 days, it was as fabulous as any historical myth I might see in the encyclopedia. I
117 remember the teacher, in the insensitive way that was normal in PS 13, asking if
118 each of us had a favorite tree. Since there were no trees in the stunted, unkempt yard
119 behind the apartment we lived in, I had to admit I had no such tree and I got the
120 impression that I had given the wrong answer. So the separation between my reality
121 and what was described, as a normal child's life in school, was a constant perplexity
122 to me.

123 Indeed, I seem to recall a tremendous fog of confusion. I started school with
124 the single advantage of being a good reader but other aspects of elementary school
125 work often flummoxed me. Simple tasks like writing my name in script seemed
126 Herculean to me. Writing the script capital *J* of my first name was appallingly hard.
127 My greatest challenge seemed to come in my first collision with arithmetic. I had no
128 difficulty in understanding the idea of addition. However, in what must have been an
129 early grade, the teacher used flash cards and the idea was that when you were
130 shown a flash card that had, for example, 4+3 written on it, you were supposed to
131 shout SEVEN as fast as you could. For some reason I can't explain I would say with

132 great confidence, “four plus three is seven” and the teacher, always a woman in
133 those days, would tell me “ No, no, no; just say seven”. Then she would flash
134 another combination and the routine would repeat. I could tell she was close to the
135 edge. One day she invited my father, who was teaching in that school, to watch my
136 stubborn refusal to play it right. She asked a tall Chinese student to get up and
137 demonstrate how it should be done correctly. I saw no difference in our two
138 responses. Had it been stubbornness on my part my father perhaps would have
139 sympathized but it seemed clear I was deficient in some way. So, later that week my
140 father, at home, fashioned our own set of flash cards and perhaps with special
141 pleading on his part finally made me understand what was wanted. The light dawned
142 and the fog temporarily lifted. Still, arithmetic, especially the addition of long
143 columns of large numbers, drove me crazy with boredom and I began to believe that
144 arithmetic was a special problem for me. This would hold true for all math until well
145 into high school.

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

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

165 Sometime when I was around 7 or 8 years old my parents told me that I was
166 Jewish. I didn’t know what that meant but I inferred from the way they mentioned it
167 that it meant we were different from other Americans. I remember crying about it
168 and resisting the idea but in the end, as the good boy I was, I accepted it. The
169 knowledge came with the understanding that I would shortly start going to Hebrew
170 School each weekday except Friday, after finishing public school, and also on
171 Sunday morning. I was assured I would have a half an hour between those schools
172 during the week to “play”. Once I started the Hebrew School, which was located just
173 across the street from our house, I fell right into the rhythm of things. I learned to
174 read and translate Hebrew. I learned to read the Bible in Hebrew and learned the

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

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

195 On the other hand, public school in its order and safety was a place I felt
196 comfortable and my reading ability and general ability to speak well allowed me to
197 do well in school, math being the exception. Sometime in the seventh grade my
198 parents heard about a small, special public school on the campus of the nearby
199 Montclair State Teachers College (now Montclair University, part of the New Jersey
200 University system) where there was a demonstration high school, Montclair College
201 High School (CHS). The professors at the Teachers College taught the high school
202 students and the college students observed them teaching. The tuition was negligible
203 but it required a daylong exam to gain entrance. My parents, after being reassured
204 that there was no quota on Jews, encouraged me to apply and take the exam. Seven
205 students from Paterson, 5 of them Jewish, applied and we all got in. It was the first
206 time I had succeeded in something that distinguished me from my background. The
207 school was seventh through twelfth grade and required some travel to get to and
208 from the school daily but with a parents' car pool in the morning and public buses in
209 the afternoon it was easily done. The class size was moderate, about 30 to a class and
210 so had only about 180 students in the whole school. I loved it. My life was opening
211 up and the possibility for the big life I had hoped for seemed a bit more possible.

212 There was a poignant side story to my change of schools. One of the girls in
213 my class, I will call her Marilyn M, and so disguise her last name, was just about the
214 smartest person in the class. She also had an open and pleasant personality and was
215 quite popular. When the cohort from Paterson decided to apply to CHS I encouraged
216 her to join us. She came back the next day crestfallen; her parents had decided not to
217 allow her to apply. She told me the news wistfully. It clearly had nothing to do with

218 money; the tuition at CHS was negligible even for my family. I have often wondered
219 what became of Marilyn and wonder about these small life-changing decisions and
220 what effect each has on future trajectories of personal history.

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

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

260 Otherwise, I did well in that school but mathematics still seemed difficult for
261 me. But in the more humanistic subjects I did better. I made good friends in school

♦ In Italian: "Let God watch."

262 who came from outside Paterson and felt generally very happy. And, I discovered
263 girls.

264 In public school I had a fondness for one or two of the girls in my class but
265 nothing very specific, just the sense that I especially enjoyed their company while in
266 school but the idea of seeing a girl outside of school never occurred to me. That
267 changed at CHS. I developed a crush on an attractive girl in my class. She was a
268 cheerleader although given how small our school was that did not have any special
269 significance. A substantial fraction of the girls in my class were cheerleaders. More
270 significantly, she was enamored of another boy in the class and although we
271 sometimes dated and went to dances together it never progressed very far in spite of
272 my longings that it might. The principal effect that disappointment had on me was to
273 make me subsequently think it was simply marvelous, even miraculous, whenever
274 any woman expressed enthusiasm for a romantic relationship with me. I became
275 very vulnerable to that response.

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

282 In our high school homeroom meetings various issues were discussed that the
283 students were allowed to handle in the general context of a student government. I
284 found that when the issues became complex, I could often suggest a method of

285 solving what had appeared to be difficult obstacles. Often I was perplexed that other
286 students couldn't see how easy it might be to deal with such problems. I remember
287 one homeroom teacher who one day looked at me in amazement and said, "that's the
288 second good idea you've had today". I was very pleased but in retrospect it must
289 have meant that I had been expected to contribute very little.

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

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

304 A more concrete manifestation of that airplane interest was demonstrated when
305 I volunteered in what was called the "Ground Observer Corps". This was an Air
306 Force sponsored group of civilian citizens who manned observation platforms
307 around the country to identify air traffic since in those days low flying aircraft were

308 literally “under the radar” and unseen. For several hours each week I would man an
309 observation post in Paterson on the roof of the YMCA, as shown in the figure on
310 page 22, and with a special phone connection, call into a central processing center
311 the sighting of any aircraft, its estimated altitude and its direction of flight. It was an
312 activity both exciting and boring. Exciting, because it really was part of the US air
313 defense system and boring because the activity itself rarely was anything but routine.
314 After a certain number of hours spent in observing one received an official
315 medallion pin in the form of wings. I still have mine together with pins denoting
316 membership in several other honor societies, but this was the first.

317 Another interest that developed about this time was in jazz. I still can recall the
318 first time I was consciously aware of jazz. As mentioned above, the return trip from
319 CHS involved taking public buses. One bus took the crew of Paterson boys from
320 Montclair to downtown Paterson where we went our separate ways. I had to walk
321 around the square containing City Hall to get a bus ride to our home in Fairlawn. As
322 I was passing a record store one day I was struck by the music coming from the
323 loudspeaker over the store’s entrance. The music was like nothing I was aware of
324 having heard before. I was so taken with it that I immediately entered the shop and
325 asked the owner what I was hearing. I was informed that I was listening to Artie
326 Shaw’s version of “ Softly as in a Morning Sunrise”. I was transfixed. Spinning the
327 dial on the radio at home I discovered others, especially Benny Goodman and
328 became enamored of the clarinet. It was to be another 25 years before I was in the
329 position to learn how to play the clarinet but my interest in jazz became deeper and

330 deeper. Alas, we didn't own a phonograph at home and so my ability to hear jazz
331 was limited to what was sometimes available on the local radio stations.

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

339 Also as a senior, something remarkable started to happen with regard to math.
340 We were studying trigonometric relations, i.e. the relationship between various
341 combinations of trigonometric functions. While most of my classmates approached
342 these problems (rather simple and banal, certainly) by searching in a handbook for
343 possibly useful relations between the functions, I began to "see" in the original
344 formulae other "natural" equivalences that were slightly hidden. I began to be the
345 fellow giving help to my mates rather than being the fellow asking for help. When it
346 dawned on me what was happening it made me more confident about my
347 engineering career choice although, to be fair, I had no realistic idea of what it meant
348 to be an engineer, aeronautical or otherwise. In fact, I discovered later that I was
349 really not cut out to be an engineer and that "scientist" would be a more natural
350 career. At that time and in my situation there was no guidance available to help me
351 think that through and, to be truthful, I didn't even know that one could be a scientist
352 as a career choice. Naturally, I had heard of Einstein, and knew of the physicists who

353 made the Bomb, but I automatically assumed these were very special people whose
354 numbers were so small that it never occurred to me to imagine myself as one of
355 them.

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

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

370 All five schools admitted me. The choice was difficult. I ruled out Georgia
371 Tech immediately for no good reason except it seemed very far away. Note that I did
372 not apply to any West Coast schools, e.g. Cal Tech which had an excellent program
373 in aeronautical engineering. The idea of going to school that far away seemed bizarre
374 to me. It's also true that *I had never heard of Cal Tech* and no guidance counselor
375 mentioned it. I ruled out NYU simply because it was too close to home and I found

376 New York City unattractive and daunting. I was strongly attracted to RPI for two
377 reasons. First, it had a good reputation and was not as scary a prospect as MIT and,
378 second, a good friend in my high school class and an excellent student, was admitted
379 there and was enthusiastic about it and I thought it would be reassuring to start this
380 adventure with a ready made friend. Everyone told me that MIT was really, really
381 hard and that the flunk out rate was high in the first year. I was terrified of being
382 embarrassed at failing. My father, as soon as I was accepted there, began telling
383 friends and neighbors about it and I kept shushing him. I hadn't decided to go there
384 and I was frightened to consider it. I was afraid to be home by Thanksgiving of my
385 freshman year as a failed flunk out.

386 MIT impressed me in another way. As part of the application process, a
387 personal interview was necessary. It was not as common in those days (1955) to
388 consider distant travel so casually so the interview was generally held in your
389 hometown (or the closest location) by an MIT alumnus who forwarded a report to
390 the Institute. It turned out that my father knew the MIT alumnus in Paterson (he had
391 taught so many kids in gym class that he knew about as many people as the local
392 obstetrician) so he drove me to the interview (I was too young to drive myself) and
393 waited in the next room while I was interviewed. When the really rather pleasant
394 interview ended and the alumnus and I returned to where my father was waiting, my
395 father asked him whether MIT had a quota for Jewish kids. Quotas for Jews were
396 quite common in the Ivy League and even MIT had a quota earlier but WWII and the
397 Holocaust had the positive effect of making anti-Semitism no longer publically

398 acceptable. There is a good side to hypocrisy. You might dislike Jews but it was no
399 longer acceptable in polite circles to say so or act on those feelings.

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

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

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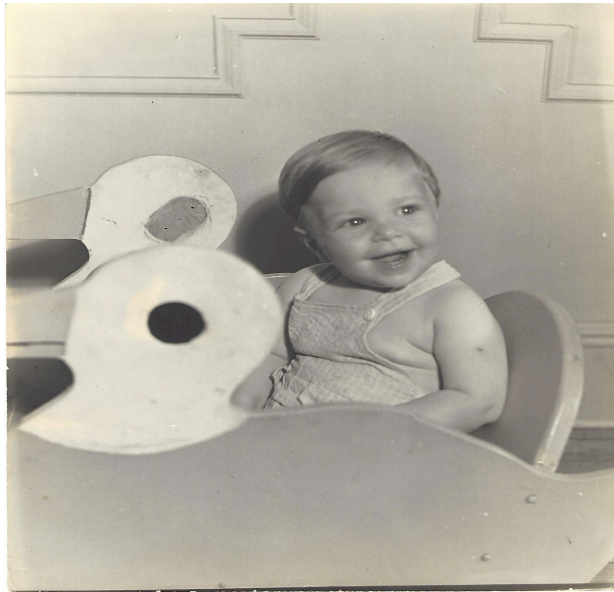
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421 **Photos :Early Days**



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424 **A naturally happy child.**

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428 An early enthusiasm for baseball.

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

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Photos: High School



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The GOC “lookout post” in Paterson, NJ.



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High School photo: taken in my senior year for our Yearbook. I think it shows a very young kid (barely 17 yrs.) not really ready for life's rigors.

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

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

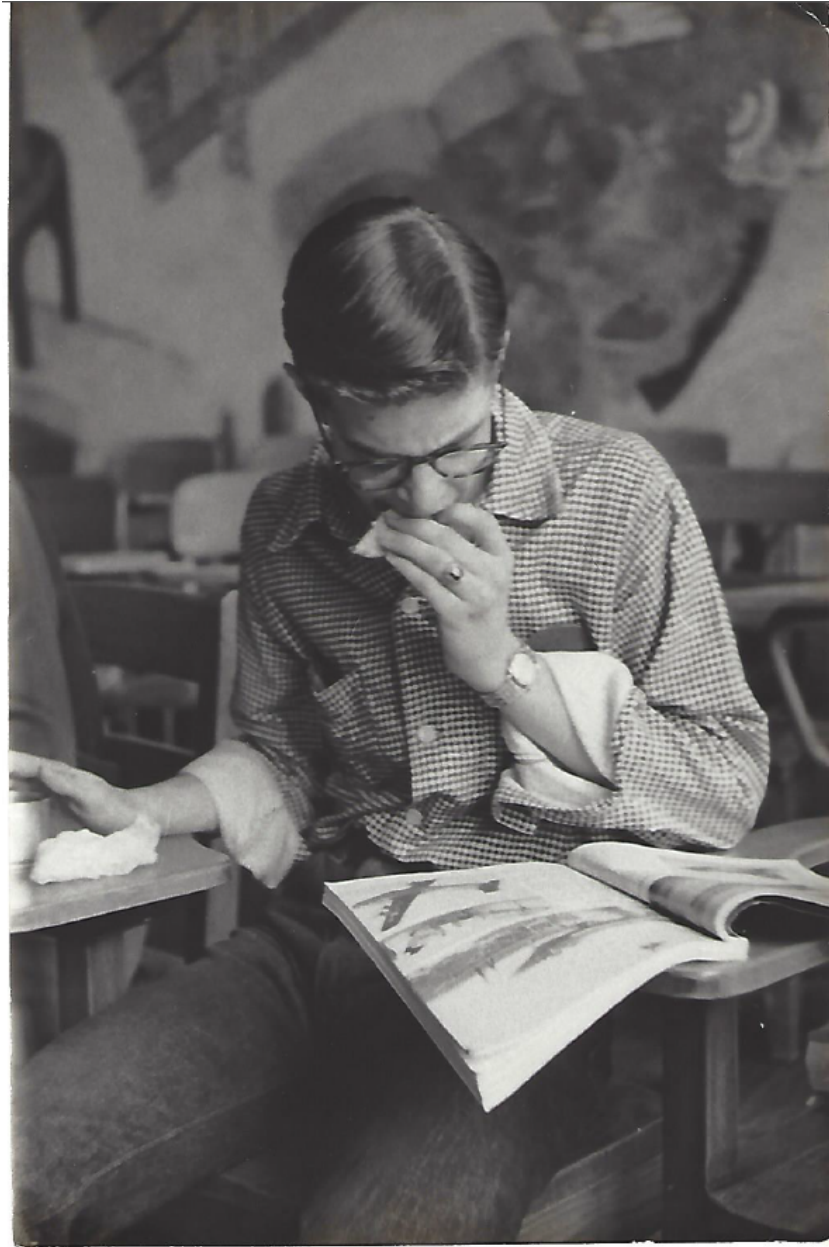
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459 In Senior Home room having lunch while perusing an aviation magazine. I am
460 proudly wearing my College High School class ring and my bar mitzvah watch.

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

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Chapter 2

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An Undergraduate at MIT

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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.

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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

502 \$900 /year and even my parents could swing it without asking me to work during the
503 school year although I worked at a series of menial jobs in the summers from age 14
504 to save money for college. I worked as a dishwasher in a small restaurant in Paterson
505 and then got a more lucrative job delivering lunches that people ordered by phone
506 from downtown offices. The tips were generous and I often made more than \$40 a
507 week in tips, a tidy sum in those days. I worked as a stock boy in the same auto-
508 appliance store my father worked in and worked one summer as a playground
509 director in a low cost housing development. On the whole I found these jobs
510 interesting because it put me in contact with people from quite different cultures;
511 often people with little education. What impressed me in my restaurant job was the
512 generosity of spirit of the waitresses. Their futures were already limited and rather
513 than begrudging me the opportunity that college presented me, they cheered me on
514 and were supportive and happy for me when I got into MIT. Humor helped.

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

523 I found the first semester at MIT excruciatingly difficult and disorienting. The
524 required courses that semester were Physics, Chemistry, Calculus, Humanities and

525 an “elective”. As an aeronautical engineering student my “elective” was a required
526 course in projective geometry, a type of a drafting course requiring the visualization
527 of the intersection of cylinders and cubes, etc. I had no experience at the drafting
528 board and my spatial visualization skills are limited. My freshman advisor, a
529 physicist who had great disdain for this kind of thing, talked me into taking this
530 course before taking a drafting course. It turned into a nightmare for that first
531 semester.

532 I enjoyed the Physics course even though I did not do well that semester. The
533 immediate emphasis, justly so, on quantitative reasoning was strange to me
534 compared to high school Physics. I loathed Chemistry with its memorization and the
535 lack of basic structure (at least as I saw it). I received a merited C in both those
536 fundamental science courses. I really enjoyed calculus, which seemed very intuitive
537 to me and although I expected an A, I received a B for the semester. I got an A in my
538 Humanities course. Those Humanities courses at MIT were spectacular. Perhaps
539 considered retro today, they focused on Western Civilization and we started by
540 reading Greek tragedies, Western Philosophy and epistemology, all with the original
541 texts (some, of course, in translation) instead of using a survey textbook. I recall
542 being given the assignment in that first semester of refuting any one of Thomas
543 Aquinas’s propositions and that seemed simple enough, since some of his
544 propositions seemed so bizarre, until I realized that if I accepted his previous
545 argument his reasoning about the proposition I was to refute was bullet proof.
546 Attempting to disprove his previous argument led me back to the one preceding that
547 until finally I was led back to try to refute his first which was his proof of the

548 existence of God. I thought I did a pretty good job on that one and said so as I wound
549 up the paper that was written as a scholastic debate between old Tom and me. I
550 received an A for the paper with the professor's admonition "A little humility Mr.
551 Pedlosky, please".

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

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

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

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

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

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

613 There were three young professors in the department that I thought were
614 charismatic, effective teachers of fluid dynamics: Leon Trilling, Holt Ashley and
615 Erik Møllo-Christensen. Trilling was the teacher in my first fluid mechanics class

616 and his mastery of the subject and the grace with which he taught the basic theorems
617 like Kelvin's theorem just swept me away. With his slight Polish accent and
618 European manners he radiated a heightened sophistication for a boy from Paterson. I
619 also felt in the class lectures, where there were about 20 of us that he was speaking
620 directly to me. In fact, some of my classmates jokingly said they thought so too, i.e.
621 that he was lecturing to me. Ashley was a large, physically imposing man, well over
622 6 feet tall, full of *joie de vivre* and who taught both fluid mechanics and the allied
623 field of aero-elasticity. He had a significant role to play in my private life that I will
624 get to later. Finally, Møllo-Christensen was perhaps the most exotic of the three and
625 had a talent to make simple but elegant connections between fluid mechanics and
626 common day to day phenomena we would never have otherwise imagined. A
627 creative experimentalist, he became my Master's Thesis advisor and my advisor on
628 many academic issues. He also had an interesting personal history as a young
629 member of the resistance to the German occupation of Norway that landed him in a
630 German prison camp for the remainder of the war. He shared those stories with me
631 and another student, Jim Poor, during the time I was working on an experiment with
632 Møllo-Christensen, to be described below. We adored him. He was brilliant and
633 approachable.

634 Among the many advantages of going to school at MIT is that you were
635 allowed to take as many courses you could handle without paying any extra fee. So
636 by the time I became a junior I began to realize that my inclinations were more
637 scientific than engineering and that so-called "overload" policy allowed me to take a
638 full suite of advanced physics and math courses in addition to my aeronautical

639 engineering courses. So, as did my Physics Department roommate, Morton Rubin, I
640 took courses in Quantum Mechanics, Statistical Mechanics, advanced applied math,
641 modern algebra and tensor calculus. The intellectual stimulation was intense. The
642 disadvantage of this gorging on a large number of courses was that it amounted to
643 avoiding going very deeply in any subject. In fact, I had a real disinclination, almost
644 a horror at this stage, of spending very much time on any one thing for fear of
645 missing out on the rest of the buffet laid out before me. This would be a serious
646 problem later but I didn't see any warning signals at this time. I was simply having a
647 wonderful time intellectually.

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

655 At the same time a course with Dirk Struik in Tensor Calculus introduced me
656 to General Relativity and the formulation of the geometry of curved spaces and I
657 remember the afternoon, when reading in the text we used, a discussion of the
658 fundamental mathematical definition of parallelism that made it obvious how
659 parallel lines could meet in non flat spaces. I got so excited I started bouncing up and
660 down on the bed in the dormitory. I found the advanced physics and mathematics I
661 was taking to be entrancing. The ideas and mathematical structure (at least how it

662 was in the late 1950's) of Quantum Mechanics seemed to me so elegant and amazing
663 that at times my head was spinning with astonishment. A superb teacher, Francis
664 Low, taught the first Quantum Mechanics course I took. His lectures, given without
665 notes, were spellbinding.

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

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

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

701 I had earlier worked with Møllo-Christensen on a simple project during the
702 Christmas break of my senior year. I stayed at MIT that week and did an experiment
703 on oscillating airfoils in the MIT low speed wind tunnel. The idea sprang from a
704 Scientific American article that wondered if the ability of certain marine mammals to
705 maintain such high speeds for as long as they did might be related to a mechanism of
706 drag reduction due to time variations in their body shape. The idea of oscillating the

707 airfoil was a crude attempt to mimic that. I wasn't able to gain much insight in the
708 short amount of time I spent on the project but I enjoyed working with Eric very
709 much and the summer experiment sounded like another possibility for a very
710 stimulating time.

711 Together, and here memory fails me on the details of exactly how the idea
712 emerged, we decided to try to study heart murmurs. It turns out that as the heart ages
713 the lips of various valves in the heart, for example the mitral valve regulating the
714 flow from the upper to lower ventricle of the heart, begin to calcify. When the lips
715 calcify they prevent the valve from closing completely when the blood is pumped
716 from the lower ventricle into the aorta thus producing a backflow through a narrow
717 passage into the upper chamber. Similarly, when the blood is pumped from the upper
718 ventricle to the lower chamber, the mitral valve doesn't open completely. Again, the
719 flow passes through a restriction. In both cases the suspicion was that vortices would
720 be produced as the flow moved through the restricted passage. The vortices would
721 bounce against the heart wall, resonate in the chest cavity and produce the heart
722 murmur. At least, that was the idea we operated under. The idea of the experiment
723 was to use some very sensitive and tiny microphones embedded in the wall of the
724 apparatus to relate the sound to the nature of the observed vortices. It is not
725 surprising now, recounting the ambitious agenda of the experiment, that a summer
726 was nowhere near long enough to accomplish the goal. Nevertheless, I learned a lot,
727 some of it humiliatingly primitive. After reading the appropriate sections in Gray's
728 Anatomy, the standard English language anatomy textbook I had a somewhat clear
729 idea of the basic geometry of the flow. The design of the experiment itself was up to

730 me and I proposed an apparatus with the aid of a sympathetic older carpenter in the
731 Department's woodshop. It involved running water from the tap through a vertically
732 standing box, wide laterally but narrow in the 3rd dimension, and perhaps five feet
733 tall. At the entrance a vertical channel led to a constriction provided by movable
734 metal plates forming a V with a slight gap in the bottom that led to the larger lower
735 box that served as a settling chamber and from which the flow exited. The
736 microphones were set in small holes in the wooden walls of the container and the
737 flow was visualized by inserting red dye in the entering stream. There were a host of
738 problems with this simple design. When the box was first filled with water it sprang
739 leaks from all the joining surfaces and inundated the lab and me. That required finer
740 joints and firmer bonding and was eventually solved. The entering flow at the top of
741 the apparatus went through a curved plastic pipe and there was a tendency for large
742 bubbles to form whose oscillations interfered with any conceivable signal from the
743 vortices. The only way I was able to overcome this difficulty was filling the rather
744 heavy apparatus while it lay on my knee (!) with the curved tube below and then
745 slowly raising it across my knee to the vertical position we required. I am glad there
746 is no video record of this absurd protocol being carried out. But it seemed to work.

747 It was never possible to completely staunch the leaks completely and that
748 meant, with the use of the red dye, that I was usually soaked with red colored water.
749 This led to one special moment when I was walking rapidly from the Aero
750 Department to the Chemistry supply shop to obtain some needed glassware. I
751 happened to meet a friend of mine, a student in another department, and he asked
752 what I was doing that summer. I said I was conducting experiments about the mitral

753 valve of the human heart and he immediately went pale. I suddenly realized that I
754 must have appeared to be drenched in blood! This was a moment to remember.

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

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

768 While these academic matters were the paramount issues in my life just then, it
769 was also true that my social life had become enriched since my freshman year. There
770 was a period before the first semester at MIT in which new freshmen were invited to
771 visit the various fraternities at MIT. Those parties were an opportunity for the
772 fraternities to decide whether to offer membership to any of the interested new
773 students. It became clear to me that the atmosphere of the fraternity house was
774 socially beyond me. Whether it was too worldly or too socially conscious I felt ill at
775 ease in that atmosphere. Perhaps I was just too young, having just turned 17. In any

776 case I received no bids to join any of them and took up residence in a larger
777 dormitory, Burton House, that was the home to 600 students, roughly 4 times the
778 number in my entire high school! I enjoyed the social atmosphere there and made
779 good friends and felt comfortable about the dormitory's attitudes towards study and
780 social life.

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

798 accompany you to a weekend movie or a dance. For the first two or three of my
799 years at MIT the idea of a date on a weekday night was considered academic suicide.

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

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

819 An issue that might seem strange to younger people today was the question of
820 sex. I did know fellows of my age who had very serious girl friends with whom they

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

834 Another part of my social life as an undergraduate related to a discovery that I
835 did have some athletic ability, something of an epiphany for me. It all started with an
836 MIT Athletic Department open house where various sports were advertising
837 themselves in the one large field house the Institute had in those days for athletic
838 activities. I was attracted to a table that advertised the MIT fencing team and in
839 conversation with the young man at the table I was told that if I were good at Ping-
840 Pong, I might be good at fencing. It had something to do with hand-eye
841 coordination. I was very good at Ping-Pong, which was the only non- academic
842 activity at the Hebrew School where I spent my weekday afternoons. So I decided to
843 give it a try. It meant working out 4 days a week and joining the team. I had to

844 choose among the 3 fencing weapons, foil, sabre or epee. The fencing Maestro,
845 Silvio Vitale, suggested that I choose the epee on the basis of how he saw me as I
846 fenced foil. The epee is the direct descendent of the dueling sword. Each of the 3
847 weapons has a different allowable target. For epee it is the whole body. Each weapon
848 also has different rules about whether you need to parry an adversary's attack before
849 you counter attack. Epee had no "right of way" rule so it counted on just rapid
850 reaction, no parry necessary. It suited me perfectly and I became pretty good so that
851 by my junior year I was fencing regularly on the team and became the lead epee in
852 my senior year. I remember Vitale, or the Maestro as we called him, once came up to
853 me in either my junior and senior year and remarked, with marvel in his voice, that
854 he had spent some time looking up the academic records of students on the team. He
855 expressed astonishment that I was doing so well because it seemed to him that I was
856 so *casual* about things that he took me for a rather unserious student. I realized that
857 my exterior could hide both the intensity of my devotion to my studies and my
858 nervousness on the fencing mat.

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

865 All this was background to the events of my fifth year. It started well. I was
866 taking lots of courses: high-speed aerodynamics, statistical and quantum mechanics,

867 advanced math. In fact one of the teachers of the advanced math course was Louis
868 Nye Howard. A brilliant applied mathematician and a superb teacher. He would have
869 an important role to play in determining my future career path but that's for a later
870 chapter. Harvey Greenspan who would be my future boss when I worked in the math
871 department at MIT taught another math course, one that I found also interesting but
872 more difficult. He had an aggressive exterior but I quickly caught on that he
873 responded to honest questions with care and concern. Questions asked just to
874 impress received withering disdain. In one lesson he was carrying out a rather
875 involved calculation when a student who was in the pure math group but had been
876 forced to take one applied math course asked "aren't you afraid to take a step like
877 that?" implying that the step did not demonstrate the rigor that the student, as a
878 superior pure mathematician, thought was required. Harvey glared at the student and
879 replied with heavy emphasis, "An applied mathematician is not afraid of anything".
880 He won my heart with that one.

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

885 The fifth year required a Master's thesis. I worked with Møllo-Christensen on
886 the stability of supersonic shear layers. It was an attempt to deal with a hard
887 problem, the instability of a supersonic shear layer radiated by sound waves that
888 produced inflexion points in the velocity profile. To me it implied the likelihood of
889 instability induced by the radiation but in those pre-computer days it was impossible

890 for me to make much definite progress. It was acceptable as a Master's thesis but it
891 left me feeling unsatisfied.

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

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Chapter 3

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I Meet GFD and Other Attractions

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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.

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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.

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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

930 overlooking Little Harbor, at the entrance to the village. It seemed idyllic and to this
931 day that view still provides me, after 59 years, deep pleasure and contentment.

932 That fifth year at MIT ended with a sense of great anticipation and excitement.

933 One Sunday evening after Kern and I had gone to a Boston restaurant for a good

934 dinner together, and after a year in which we had each been dating young women

935 from Tufts (they were attending the coed part of Tufts, then called Jackson College),

936 he invited me to visit him in the period between the end of the Spring semester and

937 the beginning of our summer in Woods Hole. I was intrigued by the invitation since

938 his mother lived in the San Francisco area but I had to decline the invitation for the

939 simple reason that I certainly could not afford the cost of the plane ticket to

940 California. Later that evening, Kern's mother, who I knew was quite wealthy, called

941 and told me to not worry about the plane ticket and that they were looking forward to

942 seeing me in California and not to mention the question of money again. I was

943 overwhelmed by the generosity and for a very young man (22) who had never been

944 west of Philadelphia it sounded like an adventure not to be missed. So, Kern and I

945 spent a fine week or so in the Bay area, staying in his mother's house in Mill Valley

946 and their cottage in Nicasio and enjoying the pleasures of Northern California and

947 the city of San Francisco. It also gave me the opportunity to deepen my friendship

948 with one of my dormitory friends and roommates, Stein Weissenberger, whose

949 family also lived in the city of San Francisco.

950 So with this exciting vacation finished I was ready to see what Woods Hole

951 and GFD had to offer. My parents had scraped enough money together to give me a

952 VW bug (a car with no gas gauge) as a graduation present. They had been delighted

953 when the Lockheed Aircraft Corporation had given me a full scholarship for that
954 fifth year and the VW was in the nature of a reward. I drove the VW down from the
955 apartment in Cambridge to see what this field of GFD was all about.

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

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

973 I remember as we were all unpacking in our rooms, other students were
974 striding up and down the second floor hallway introducing themselves. One chap,
975 who turned out to be Bob Blandford, introduced himself and said right off that his

976 plan for the summer was to study the physics of the ocean thermocline. He was
977 especially interested, he said, in meeting and working with Henry Stommel. These
978 words meant absolutely nothing to me. I had not the foggiest idea what the ocean's
979 thermocline was or who Henry Stommel was. That spring Møller-Christensen was
980 teaching a special topics course in fluid dynamics, which, naturally, I took. After he
981 learned that I had been accepted in the GFD summer program he announced in class
982 that he was going to prepare me for the GFD course by teaching us about the
983 Coriolis force. We had seen it in our engineering courses on aircraft dynamics where
984 the dynamical analysis of an aircraft in motion was done from a frame fixed to the
985 moving and rotating aircraft but its importance and significance escaped me then and
986 did not sink in until quite a bit later. He also recommended that I read Stommel's
987 monograph, "The Gulf Stream" and, again, I gained a sense of the flavor of
988 oceanography as it was at that time without obtaining a useful understanding of the
989 dynamical framework. So it was with a mixture of anticipation and trepidation that I
990 showed up for the first day's lesson.

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

998 “the new” building, i.e. the Smith building. The lecture room was a small room in
999 the “old building” and that was also where our offices were.

1000 There were nine GFD students. The ones I remember best from that year were,
1001 besides myself, Bob Blandford, Bill Blumen, Bill Holland, Dick Lindzen, Gus
1002 Furomoto and three others who I won’t name. Two of those three decamped before
1003 the end of the summer without presenting a final research project report that was,
1004 and continues to be, the focus of the summer for the students, while the third
1005 unnamed individual attempted a flight, was stopped and gave a thoroughly
1006 undistinguished final presentation. The reader who has some background in
1007 meteorology and/or oceanography will surely recognize most of those I have named
1008 for their contributions to the field. In addition to the admitted students there were
1009 other students in residence in a more informal summer student program, like my
1010 friend Kern, or students who had made individual arrangements to be at WHOI that
1011 summer. Among those was Bruce Taft, a slightly older student who became a well -
1012 known observational oceanographer and who became a good friend. Also in
1013 attendance in 1960 was Andy Ingersoll who later became an important figure in the
1014 dynamics of the atmospheres of the planets of the solar system and was the star of
1015 our softball team.

1016 The permanent staff of the GFD program that summer consisted of Melvin
1017 Stern, who became my research project advisor, Willem Malkus, one of the founders
1018 of the program, Arnold Arons, at that time a faculty member in the physics
1019 department of Amherst who worked with Stommel, and, of course, Stommel

1020 himself. George Veronis a co-founder of the GFD program with Malkus, was not
1021 present that summer.

1022 Other participants in this stellar cast included Ed Spiegel, a bright and witty
1023 astrophysicist from NYU, Harry Wexler, then an important figure in the Weather
1024 Bureau (chief of the Scientific Services division, i.e. the Bureau's chief scientist),
1025 and Fred Bishopp, then an assistant professor in Brown's mathematics department
1026 who had done fine work in thermal convection with Chandrasekhar at the University
1027 of Chicago, and Doug Lilly, an expert on violent storms. Joanne Malkus, previously
1028 Joanne Starr, subsequently Joanne Simpson, also participated and gave some lectures
1029 on the dynamics of clouds a field to which she had made fundamental contributions.

1030 The central figure for me at least for the first two weeks of the summer course
1031 was Lou Howard. Lou was a professor in the MIT mathematics department and he
1032 gave 10 beautiful lectures the first two weeks and his lectures were simply brilliant.
1033 He gave us the fluid dynamical foundation for rotating flows, with emphasis on
1034 wave motions, instability principles and boundary layer methods. It was the first
1035 time I saw boundary layers described in a clear mathematical way in terms of inner
1036 and outer limits of the governing equations. Prior to that, the only approach to
1037 boundary layer dynamics was the rather heuristic approach of Prandtl as described in
1038 Schlichting's text on the subject. I had a somewhat stronger background in fluid
1039 dynamics than the other students in the course and I found all the material very
1040 accessible, very fascinating and the application to the oceans intriguing. I added Lou
1041 to my personal Pantheon of heroes and felt as if I learned a tremendous amount from
1042 him that summer. I was particularly impressed one day to overhear Harry Wexler,

1043 who shared an office with the students, decline to attend a Washington meeting
1044 because he didn't want to miss a single lecture by Lou Howard.

1045 Willem Malkus had been working with Veronis on one of the earliest weakly
1046 nonlinear theories of convection. I later was able to appreciate the creative character
1047 of their work but Willem was never a clear lecturer and we were all lost in his
1048 presentation. When he went further, beyond weakly nonlinear theory, to lecture on
1049 his ideas of *turbulent* convection the fog grew even thicker. He had introduced the
1050 idea that turbulent convection would somehow organize itself to maximize the
1051 vertical heat transport by the turbulent motion but his idea only became clear to me
1052 when Lou Howard took up the problem a couple of years later. Although Willem's
1053 idea turned out to not be right he gave a mighty impulse to turbulence theory with
1054 the idea, which was ingenious. Indeed, in his career he had many very creative ideas
1055 that proved to be not quite right but they were so stimulating that his wrong ideas
1056 often advanced the field much further than other people's ideas that were correct but
1057 routine.

1058 Henry Stommel gave a set of lectures on a variety of oceanographic topics
1059 ranging from salt fingers to Sverdrup-style ocean circulations driven by precipitation
1060 and evaporation. The latter material was used to introduce the notion of western
1061 boundary currents, which Lou had also touched on. Finally, Hank talked about the
1062 theory of the thermocline in what we would now call a 1½ layer model and then
1063 approached the general continuous problem in a model similar to the one he
1064 developed with Alan Robinson. It led to a very difficult partial differential equation

1065 that admitted only very special solutions and I think for most of the students in the
1066 audience it was pretty impenetrable material.

1067 Melvin Stern gave lectures on baroclinic instability and the atmospheric
1068 general circulation. After deriving the quasi-geostrophic version of the instability
1069 problem, he considered the problem of a basic flow with uniform vertical shear, and
1070 ignoring the Earth's sphericity, i.e. Eady's model, derived the solution.

1071 It is interesting for me to review these subjects of that summer because, by and
1072 large, those problems became the focus of my career's research. Baroclinic
1073 instability, boundary layer theory, ocean circulation and the theory of the
1074 thermocline do a pretty fair job of describing the physical problems I would take up
1075 in my own work and there they were, all displayed in that one short summer. It
1076 should be clear that it was more than I could possibly digest at the time. In spite of,
1077 or perhaps because of, the unclear nature of these subjects to me then, I found the
1078 whole subject fascinating and so much more *physical* than the fluid mechanics I had
1079 studied in aeronautical engineering.

1080 I remember after I started my summer research problem and was working with
1081 Melvin Stern as my advisor, that at one point he started talking about the motion of
1082 fluid elements and he held his hand up and gestured with his thumb and index finger
1083 as if he were holding that fluid element and could see the forces acting on it. That
1084 was a considerably different emotional relationship with fluids that I had been used
1085 to in aeronautical engineering. After my junior year at MIT I wangled a summer job
1086 at the Naval Supersonic wind tunnel on the MIT campus. One of my tasks that
1087 summer was running a sequence of experiments on hypersonic flow past a cone-

1088 shaped metal body. The temperatures were so high and the speed so great that
1089 visualization of the flow was indirect obtained through a Schlieren system that used
1090 the small changes of density in the flow to see the shock waves around the cone. The
1091 air-flow itself was separated from the viewer by thick walls. The fluid seemed
1092 distant and rather abstract and the idea of holding a piece of fluid in your fingers, as
1093 Melvin was doing, restored fluid mechanics to the physics of experience to me. I was
1094 simply charmed.

1095 The problem I worked on that summer with Melvin was related to the recently
1096 discovered phenomenon of salt fingers, or more generally the instability of systems
1097 whose density is determined by two fluid attributes. In the case of salt fingers it was
1098 the simultaneous effects of temperature and salt. The presence of salt increases the
1099 density while an increase in temperature reduces the density. In certain cases where
1100 the two effects are present a fluid that seems to be stably stratified, with light fluid
1101 resting above heavy fluid, can become unstable and begin to convect. This can
1102 happen, for example when warm salty water rests over cold fresh water if the
1103 temperature reduction of the upper fluid's density exceeds the increase in its density
1104 due to the added salt. The process becomes unstable only because of the much
1105 weaker ability of the salt to diffuse in the fluid compared to the temperature. The
1106 physics of so-called *double diffusion* processes was revealed by some early heuristic
1107 work by Stommel and put on a firm fluid mechanical foundation by Melvin Stern's
1108 analytical analysis of the instability. Melvin showed me a simple experiment in
1109 which the convection manifests itself in tubes, or fingers of relatively salty water
1110 sinking side by side with fingers of relatively fresh water rising. What was of

1111 particular interest was the tendency of the fingers to appear to themselves become
1112 unstable and produce an apparent buckling mode.

1113 With Melvin's help I formulated a simple model of the instability; probably too
1114 simple because we assumed the solution would be periodic in the horizontal plane
1115 with the spatial period and its harmonics, of the original fingers themselves. A
1116 general Floquet term should have been included. However, using a Fourier
1117 expansion in the horizontal and a ruthless truncation of the Fourier series to just its
1118 first two terms I was able to obtain an equation for the growth rate of this secondary
1119 instability. A result in which I have no confidence now but it did represent
1120 something new for me then, i.e. the working through of a physical model to
1121 completion for all its faults. In fact, on the basis of this minor success Melvin invited
1122 me to accompany a small group of scientists from WHOI (Stern, Stommel, Arons) to
1123 a meeting in Baltimore of the American Physical Society's annual meeting on Fluid
1124 Dynamics where, for the first time, a session on GFD would be presented. I was
1125 ecstatic. Indeed, I was thrilled by my scientific experience that summer and drawn
1126 instinctively to the people I had interacted with scientifically.

1127 I would be remiss though to not describe the social side of that summer
1128 because it was wonderful. As I mentioned, I had a good friend in my roommate
1129 Walter Eckhart and we both rather quickly found female companionship in the
1130 numerous, mostly female, students taking summer courses at the Marine Biological
1131 Laboratory (MBL) in Woods Hole. I was quite fond of the girl I had met that
1132 summer and we were romantically involved in that idyllic environment until, at the
1133 end, we weren't.

1134 One day this very nice young woman took me to visit a family friend of hers,
1135 Otto Loewi who had won the Noble prize in physiology for discovering the role of a
1136 chemical agent that passed signals across synapses. He was a delightful person.
1137 Although quite old (he died the next year at age 87) he was full of marvel about the
1138 informal nature of science in the US and especially in Woods Hole. He gleefully told
1139 us about the time he was walking along in Woods Hole with a scientific
1140 acquaintance when they passed another fellow and Loewi's companion greeted him
1141 with a hearty, "Hi, Frank!" When asked the surname of the man they had just
1142 passed, Loewi's companion replied, "I don't know him well enough to know his
1143 last name". For someone raised in the atmosphere of prewar Austria it was
1144 astonishing and he laughed heartily at the memory.

1145 Everyone took meals at the MBL mess hall, which in those days occupied a
1146 large, old wooden structure across the street from the aquarium belonging to the US
1147 Fisheries, which also had labs in Woods Hole. So all the meals, and especially
1148 dinner were jolly social events, made jollier by the end of the summer when we
1149 discovered we could bring a bottle of wine to the table to supplement the cafeteria
1150 fare.

1151 From a culinary point viewpoint the summer also had special significance for
1152 me. I had discovered in my early teens that I was strongly allergic to eating fish. My
1153 experience made me believe I was allergic to all fish. In 1960 scallops were quite
1154 inexpensive and they were often on the menu of the meal program we were signed
1155 up for at the MBL mess hall. So each time scallops were offered I would demur and

1156 explain my allergy and someone in the cafeteria would make a ham sandwich for
1157 me. I soon tired of ham sandwiches.

1158 Since I was brought up in a kosher home, I had never eaten shellfish and I
1159 assumed that I was as allergic to shellfish as I was to tuna, salmon, etc. So one day,
1160 since the allergy in those days was very uncomfortable but not as severe as it became
1161 later, I hazarded eating one scallop when I could not face another ham sandwich. I
1162 discovered two important things. First, scallops are delicious. Second, I was clearly
1163 not allergic to them. Like any good scientist I generalized and developed the
1164 hypothesis that perhaps I was not allergic to shellfish in general. Gingerly tests of
1165 shrimp, lobster and mussels revealed the correctness of the hypothesis. To this day I
1166 still feel as if I am getting away with something dangerous whenever I eat a dish,
1167 any dish, of shellfish.

1168 I mentioned my sexual inhibitions about complete intercourse and they
1169 continued that summer. The very nice girl that I was seeing that summer made it
1170 clear one romantic night on the beach by Nobska Lighthouse that she was ready to
1171 go “all the way”. I held back and I believe that brought our relationship to an end. In
1172 each of these experiences I believe now it was more than prudence that held me
1173 back. I think I was not emotionally mature enough or emotionally serious enough
1174 about the women involved in order to take that step of complete intimacy.

1175 When the summer ended I returned to MIT and the aero department and
1176 entered the Ph.D. program. Although I had been strongly attracted by my experience
1177 with GFD, I felt it would be more prudent and practical to finish with my aero
1178 degree before gradually switching over to GFD full time. This was not a fully

1179 thought out plan and clearly reflected a lot of mental inertia but it seemed persuasive
1180 at the time.

1181 I started the autumn semester at MIT with a full set of courses and my next
1182 challenge was going to be the general exams that were required before I could be
1183 fully entered as a doctoral candidate. I expected no difficulty on the basis of my easy
1184 time with the earlier qualifying exams.

1185 At the same time, I applied, for the adventure of it, to the Fulbright Foundation
1186 for a fellowship for the following year to study in Sweden. I had been strongly
1187 attracted by the Swedish political model and especially its generous social welfare
1188 structure and thought it would be interesting to observe it first hand. A host professor
1189 of aerodynamics was suggested to me and I made my application. The coincidence
1190 of what followed still strikes me as remarkable but I will get to that later.

1191 After the semester started I received an announcement that the first Woods
1192 Hole/MIT GFD seminar or the year would be held in September at MIT in the Sloan
1193 building. These seminars, started a few years before, were twice a month events and
1194 their location alternated between MIT and Woods Hole but occasionally occurred at
1195 other places like Harvard. The first that year was at MIT and so was convenient for
1196 me, and with my warm feelings about the summer before, I decided to attend.

1197 There was a brief period before the seminar where people from MIT and
1198 Woods Hole gathered for coffee in the MIT Faculty Club lounge in the Sloan
1199 building where the seminar would be given. I remember quite clearly that I was
1200 sitting on a sofa next to Lou Howard and we were discussing some fluid dynamical
1201 issue when a completely dream-like event happened.

1202 I saw entering the lounge one of my favorite aero professors, Holt Ashley,
1203 accompanied by Harriet Rossby. This seemed like an incongruous combination of
1204 two quite disparate parts of my life as usually only occurs in the unreality of dreams.
1205 That was not all. Between the two of them was a third person, a young woman of
1206 exceptional beauty who became the cynosure of all eyes. I gulped and turned to Lou
1207 and asked him if he knew whom she was. “Ah”, said Lou, “She’s Carin Rossby.
1208 She’s something, ain’t she”?

1209 I then remembered an incident from earlier in the summer. The wonderful
1210 secretary for the GFD program was Mary Thayer. She had come out of retirement to
1211 handle the secretarial tasks of the program but also provided a motherly figure for
1212 the students. Blue haired, chatty, and very spirited, I was very fond of her and she
1213 returned the feeling. I became a pet of hers that summer. Later in the summer she
1214 invited me to lunch at a private club in Falmouth, the Nimrod Club (which later
1215 became the public Nimrod restaurant, now, alas, defunct) along with Harriet Rossby,
1216 her close friend. Sometime during the lunch she mentioned to me that Harriet had a
1217 daughter in Sweden who was an actress. I took in that information in the same way
1218 any young boy from Paterson, NJ would absorb the information that there were
1219 beautiful gardens at Versailles; possibly interesting but entirely personally irrelevant.
1220 Well, now here the beautiful garden had crossed the Atlantic.

1221 Then in what seemed like a continuing dream sequence, Ashley left the two
1222 Rossby women and came over to where I was sitting and said that he, Harriet and
1223 Carin were going to have dinner at the Faculty Club after the seminar and did I want
1224 to join them? You can guess my answer, can’t you?

1225 I had a wonderful time at dinner. Carin's intelligence matched her beauty and
1226 our conversation quickly became stimulating. I learned that Carin was in the US to
1227 look for acting work and would be travelling around the country on a job search. She
1228 had recently finished a major role in the Disney film, "Hans Brinker and the Silver
1229 Skates" in which she played Hans' sister. Over the years it was always fascinating to
1230 hear her describe her acting work in films and on stage. However, here in the states
1231 her job search came up empty and when she returned for a visit with her mother I
1232 called her up and we had a date to go to a concert in Falmouth and to a small party
1233 afterwards at Woods Hole. I was bewitched and we had a very romantic evening.

1234 I found out later that Carin had been engaged to be married when she arrived in
1235 the U.S. but I did not notice the ring since Swedish custom puts it on the right and
1236 not the left hand. One evening we were dining together in an Italian restaurant in
1237 Boston and I was talking about my work and how important it was to me. I
1238 mentioned that I got pleasure from the intellectual depth of doing science and said,
1239 in all innocence, that I couldn't imagine doing something like being a dentist and
1240 spending my life looking into other peoples' mouths. What I did not know was that
1241 her fiancé was a dentist and that at that moment, as she told me later, she
1242 surreptitiously slipped the engagement ring off her finger and mentally broke her
1243 engagement then and there. Our relationship became ever deeper and I experienced
1244 complete sexual love for the first time one late evening on Nobska beach in the
1245 moonlight. I was swept by passion and prudence was given the boot. The following
1246 January we were married.

1247 We had a small apartment in on Brighton Avenue in Allston, just where
1248 Brighton Avenue intersects Commonwealth Ave. We had a car dealership across the
1249 street as a neighbor and it was not a particularly beautiful location. But I was happy
1250 as I continued in my first year of the Ph.D. in the Aeronautics Department. Carin
1251 took a job in an insurance company in Boston to help pay the bills and a Ford
1252 Foundation Fellowship of \$3,000 was my contribution to the family income. We
1253 were poor, content and felt secure. We were very much in love. Although money
1254 was very tight an occasional congratulatory check from an extended family member
1255 would arrive as a delayed wedding present. No matter how low a cash reserve we
1256 had we always used the newly arrived check to go out to dinner. Things looked
1257 sunny. Then came the shock.
1258

1259

Chapter 4

1260

Shock and Recovery 1961-1962

1261

1262 The general exams were scheduled for the beginning of the second semester
1263 and I had little anxiety about them. The form was a take-home exam over a
1264 weekend. When I received the exam the questions threw me for a loop. I don't recall
1265 them all but in one case there was the description of an experiment in pipe flow
1266 turbulence and I was asked to interpret the results. Another question had to do with
1267 underwater cavitation from propellers. To say that my answers were inadequate
1268 would be kind. I had the ominous feeling from the start that I was going to fail the
1269 exam and that is the only part of the weekend that I got right. I was shaken to the
1270 core. I was embarrassed and I believe the faculty was too. Coincident with the
1271 official news that I had flunked the exam was the receipt of a letter informing me
1272 that I had won a Fulbright Fellowship for the following year. The universe is
1273 notorious for its love of irony.

1274 A rather newly arrived faculty member, Martin Landahl, gave me the bad
1275 news. Although I did not know him well, I admired him as a teacher and I was
1276 embarrassed to have let him down as well as the other faculty who had been so
1277 helpful to me over the years. He suggested that since I was going to be away in
1278 Sweden the following year, they would make an exception for me and allow me to
1279 retake a new general exam in a month so I could "get it out of the way before I left".
1280 I had an ominous feeling about that and sure enough, when given the sheet

1281 containing the questions for the new exam a month later I immediately realized that
1282 there was no question but that I was going to flunk that one too. I was right again.

1283 Now I was really shattered. At first, and for some time afterwards, I tried to
1284 rationalize it by telling myself that these failures were really just a sign that
1285 engineering was not my cup of tea. But when I was more honest with myself I
1286 realized that these failures were a reflection of the fact that I had resisted going very
1287 deeply into any research material in my efforts to develop my physics and math
1288 background. Not much of a consolation to realize that and the responsibility was
1289 purely mine. The question was what to do next.

1290 I thought in terms of making a fresh start and the attraction that GFD had for
1291 me seemed to hold out a possible path out of my misery. The summer at Woods
1292 Hole had been so successful and the subject matter seemed so natural to me that my
1293 thoughts naturally moved in that direction. What I am about to relate seems
1294 unbelievable now in its informality but the academic world seemed a smaller place
1295 then and I am fairly sure my memory is correct.

1296 I had met Jule Charney socially, perhaps at Harriet Rossby's apartment in
1297 Woods Hole, and I had a vague idea that he would be a good person to work with.
1298 So I went to him and asked whether when I came back to MIT after my year in
1299 Sweden, I could switch to the Meteorology Department at MIT and work with him.
1300 He just said "yes" and the business was done. He loaded me up with books and
1301 articles he thought I should read in my year away and went to the effort of finding a
1302 place for me in Stockholm at the International Institute of Meteorology that Rossby

1303 had set up in the 50's and which was headed then in 1961, after Rossby's death, by
1304 Bert Bolin, later better known for his important work on the IPCC reports.

1305 Charney could not have been more generous and tactful. Looking back on
1306 these events it is clear to me that Jule must have inquired about me to the people in
1307 the Aero department and been aware of my problem with the exams but he never
1308 mentioned it and it apparently did not greatly trouble him. So with this change in
1309 direction and his guidance I felt I could spend the academic year 1961-1962 in
1310 Sweden studying meteorology and oceanography, largely on my own, and return to
1311 MIT to face what I needed to do for a Ph.D. in that field. To what extent my
1312 marriage to Carin Rossby smoothed the way for me is something I will never know.
1313 I do know that I never consciously took advantage of it.

1314 I felt I needed to tell people in the aero department that I would not come back
1315 to the department after the year away and I went, as seemed most natural, to tell
1316 Møllo-Christensen. "Erik", I said, "next year when I come back from Sweden I am
1317 going to enroll in the Meteorology Department and not the Aeronautical Engineering
1318 Department". He astonished by saying, "Me too!" He had himself made the plan to
1319 switch that year to the Meteorology Department and had a long and productive
1320 career doing oceanography. The informality and ease of change in those days is hard
1321 to believe now.

1322 Well, now it was up to me to make the transformation work. Carin and I
1323 prepared as best we could for the year in Stockholm. Her family still had their
1324 apartment in the city on Ehrensvärdsgatan in a lovely neighborhood by the Nor
1325 Mälarstrand on the main island of the city. The meteorological institute was on

1326 Lindhagensgatan in those days about a 20-minute walk from the apartment. I
1327 attempted to study Swedish before we left but I had a hard time learning to speak the
1328 language. On my way to the American Embassy my first day in Stockholm in order
1329 to sign the papers for the Fulbright I took a trolley to a predetermined stop and after
1330 descending, asked a passerby for directions in a carefully practiced attempt in
1331 Swedish. The woman looked a bit quizzically at me for a moment and answered my
1332 question in perfect English. My Swedish did improve but I did tend to rely
1333 linguistically on Carin a lot and most people in the Institute spoke English perfectly
1334 and I was concentrating on learning Meteorology more than Swedish.

1335 Our trip to Sweden was the very first time I had been out of the United States.
1336 We booked passage from New York to Le Havre on the French Line vessel the
1337 Liberte`. Ocean liners were still then a standard way to cross the Atlantic. It was
1338 really our honeymoon and the crossing, in tourist class, of course, was delightful.
1339 The food was excellent and abundant. Wine was served with every meal. I learned
1340 about Camembert cheese. The waiters took particular delight in serving Carin and
1341 would come around with trays of second helpings saying “Good for you, good for
1342 me”. The tourist class cocktail lounge was wood paneled and a stringed trio
1343 accompanied a pre-prandial sherry each evening.

1344 We spent the better part of a week in Paris in a small hotel near the Louvre (5
1345 rue des Bons Enfants) where each morning we were awakened by a cheery
1346 chambermaid bringing coffee and croissants. We visited Versailles and the boy from
1347 Paterson did see the gardens after all. We had a splendid time in Paris, ate well and I
1348 exercised what little high-school French still remained available. We had a picnic in

1349 the Bois de Boulogne but were disconcerted to notice local Parisian men watching us
1350 furtively from behind trees. I suppose they were hoping to see an amorous after
1351 lunch scene.

1352 From Paris we moved on to Brussels for a couple of days, visited a museum,
1353 the beautiful central square and just off the square dined at a wonderful restaurant
1354 called L'Epaule di Mouton where a tuxedo-clad chef cooked the main dishes on a
1355 small stove in the center of the restaurant and gave a stiff reproof to a show-off
1356 American (not me!) who had the temerity to look over the chef's shoulder as he was
1357 cooking and announce knowingly to the others at his table; "This is real French
1358 cooking". The chef drew himself up to his not very great height and in the dignity of
1359 his rotundity snarled, "This is real *Belgian* cooking!" I understood then the origin of
1360 many European wars.

1361 From Brussels we moved on to Brugge for a delightful day of art and
1362 countryside before pushing on to Germany. We visited Cologne, Bonn and
1363 Hamburg. There were many things of interest to see but what was most interesting to
1364 me was how much the devastation from the war that ended 16 years before was still
1365 evident. From Hamburg we went by ferry and train to Stockholm. By this time I was
1366 exhausted and dealing with sensory overload. We were met by Carin's brother, Tom,
1367 and he took us to dinner in Stockholm. Rising to leave the restaurant I was suddenly
1368 overtaken by a great sense of fatigue and fainted. This was not an auspicious start to
1369 my year in Sweden.

1370 In fact, although I was treated with great courtesy (on the whole) throughout
1371 that year I never felt completely comfortable in Stockholm. In 1961, especially in

1372 Stockholm, the relations between people, unless they were truly intimate, were very
1373 formal. I recall that when I arrived in the Institute of Meteorology, a person in the
1374 administration came to my small office to ask me what my title was. Mail could not
1375 be delivered without a proper title for the addressed person. Indeed, in the city's
1376 phone book in those days the person was listed alphabetically by last name and *by*
1377 *title* and only then by given name. If you were trying to look up the phone number of
1378 someone named Carlson, for example, and you did not know his or her title you
1379 were out of luck.

1380 The Fulbright administration needed my address and title (I forget which title
1381 they finally settled on as appropriate) to send me a small check each month, and
1382 there is a little story involved there. When I received the notice of my Fulbright
1383 award I was informed that I would be getting 800 kronor (crowns) a month. In my
1384 naiveté, not knowing the dollar/kronor exchange rate, I assumed it was a
1385 considerable sum or at least sufficient for Carin and me to live on. She knew right
1386 away it was insufficient but was reluctant to share the bad news with me. What she
1387 supposed would happen when I found out is still a mystery. During the summer,
1388 while in Woods Hole before our trip I had met Pierre Welander a Swede who acted
1389 against the stereotype. He was exuberant, open and full of fun. He was also an
1390 excellent scientist but I only appreciated this much later. He also had the habit of
1391 asking me how much money I was making every time I changed jobs. So when he
1392 dropped into my cubicle in Stockholm later in the year one of the first things he
1393 asked was what the Fulbright stipend was. When I told him he reacted with
1394 astonishment; "You and Carin will starve!" I was nonplussed. He immediately said

1395 he would take care of it. He had a contract from the US Office of Naval Research
1396 (ONR) and he said he would pay me an additional 800 kronor from that. He then
1397 wrote 2 letters. The first to ONR saying he had hired me to do some work on the
1398 grant. The second he wrote to the Fulbright Foundation saying that although I was
1399 receiving this money it was in the form of a scholarship and that I had no obligations
1400 to do anything for him. We survived on the double-dealing and I hate to think what
1401 would have happened otherwise.

1402 As I remarked earlier, Sweden was not entirely a comfortable place for me.
1403 Many people had very negative feelings about the US and this was, remember,
1404 before the US involvement in Vietnam. When we first arrived in Stockholm Carin
1405 took me to visit her uncle, Carl Rossby's brother, and his wife Margarita. As we
1406 entered their apartment her aunt swept her into her arms and said, "Thank heavens
1407 you are back in a country with culture!" That attitude was widespread. There was
1408 also great disdain for the problem of racism in the US and I was told in no uncertain
1409 terms that nothing like that could ever happen in Sweden. In fact when one of
1410 Carin's girlfriends invited us to dinner, the friend's boy friend started the after-
1411 dinner conversation by asking me whether I had left the US because I was fed up or
1412 was I one of the "dirty nationalists". The conversation grew a bit heated. Carin later
1413 told me with a chuckle that the same girl asked her if I wasn't a bit on the
1414 homosexual side. When Carin, astonished, asked her why she thought so, she replied
1415 that she was confused that I had not yet made a pass at her.

1416 I went to Sweden more than willing to admit the many flaws in American
1417 society but there was much in America that I admire and am proud of, and while I

1418 initially held my tongue, eventually I could not restrain myself. I remember one
1419 conversation with a Swede who said he considered the US deeply provincial. I
1420 replied that being from an area near New York City, that Stockholm struck me just
1421 the same way. He was speechless.

1422 However, my main focus that year was going deeply into the fields of
1423 meteorology and oceanography. I started with the simplest texts I could lay my
1424 hands on and worked my way into more sophisticated material. A review article by
1425 Arnt Eliassen was particularly useful. I read, read and read. I discussed what I read
1426 with some of the friendly staff at the Institute. Knowing that I would have to take a
1427 rather broad general exam in the Meteorology Department when I returned, I even
1428 read a text on cloud physics and another on atmospheric radiation.

1429 Sometime during that year Bolin asked me to look at an article by Alewyn
1430 Burger that had been sent to Tellus that was later published elsewhere and it
1431 involved Burger's work on Charney's model of baroclinic instability. The model's
1432 solution involved the solutions of the Confluent Hypergeometric Equation,
1433 something I had studied in my advanced math courses and finally that investment
1434 was paying off. Burger had shown that the curve of the shear versus wavelength that
1435 Charney had found and interpreted as a boundary between stable and unstable shears
1436 was just the boundary between contiguous unstable regions; a very surprising result.
1437 Burger's argument was indirect and subtle and it required close reading to follow it.
1438 When I told Bolin that I thought Burger's argument was correct he asked me if I
1439 would give a little seminar on the subject to the staff. I did, and I remember one of

1440 the meteorologists saying, almost in despair, “ Now we have to know about special
1441 functions too?!”

1442 I was now doing what I had neglected to do before. That is, really immerse
1443 myself in problems in depth. Now it suddenly seemed natural and attractive. I even
1444 wrote a small paper on spectral properties of the 2-dimensional vorticity equation
1445 that was published in the journal, Tellus. So, this was a remarkable maturation year
1446 for me. In about the middle of that year Charney sent me a draft of a paper on the
1447 theorem that he and Melvin Stern had done on the what is now called the Charney –
1448 Stern criterion; a necessary condition for the instability of zonal flows, in the Quasi-
1449 Geostrophic limit. They restricted themselves to the case where the horizontal
1450 boundaries were coincident with isentropic surfaces, the so-called internal jet, and
1451 derived the strong condition that the quasi-geostrophic form of the gradient of
1452 potential vorticity needed to change sign for instability. It was an elegant and
1453 important result, even in that restricted case, and I found it quite exciting and
1454 familiar from my studies of classical shear flow instability. Part of the paper also
1455 contained a more systematic derivation of Quasi-Geostrophic dynamics than I had
1456 ever seen before. At Bolin’s suggestion I gave a small seminar at the Institute on that
1457 paper and I was beginning to feel as if I were gaining a foothold on at least one
1458 important part of the field.

1459 I also developed a deeper interest in the stability problem and began to think of
1460 that area as a possible Ph.D. thesis subject but this was still a vague and unshaped
1461 thought.

1462 As the year in Stockholm drew to a close, Carin and I decided to make our way
1463 back to the US via Italy. The idea of sun, good food and art was irresistible. About a
1464 month before I left Stockholm, Charles Keeling arrived at the Meteorological
1465 Institute on a Guggenheim Fellowship. Now famous for his long term and prescient
1466 measurements of CO₂ from an observatory on Mauna Loa in Hawaii, his work has
1467 been key in documenting the increasing amount of greenhouse gas in the
1468 atmosphere. At the time what impressed me most about Keeling (for I knew little
1469 about the greenhouse gas question) was his request to everyone at the Institute to
1470 speak to him only in Swedish as a way of helping him become fluent. The English
1471 skills of the staff, as I've mentioned, were so excellent that I spoke most of the time
1472 in English with them except when there were parties and they wanted to relax in
1473 their own language. Within a month Keeling's command of Swedish was many
1474 times better than mine, even with or perhaps because of, my half-Swedish wife. I
1475 vowed that if I were ever in that position again I would follow his language example.

1476 Carin and I travelled by train to Italy. I can't remember what accommodations
1477 we had on the lengthy trip but we were comfortable. I do remember that as we
1478 entered Italy and we were passing through fields of red poppies (it was June) we one
1479 day went to the dining car and were seated at a table with two English nuns. We
1480 couldn't have had two more delightful travelling companions. They kept
1481 encouraging us, and especially Carin, to whom they were very motherly, to eat lots
1482 of pasta. Giggling, they kept saying just how good it was in Italy and how eager they
1483 were to get there.

1484 We visited Venice, Florence and briefly Rome. We sailed back from Naples
1485 on the Leonardo da Vinci and it was my first experience with Italian dysfunction.
1486 We had paid a bit extra to have a cabin with a private bathroom but the porter
1487 leading us to the cabin raced in and locked the door to the bathroom. When I
1488 protested he replied with a shrug that another room, too, connected to the bathroom
1489 was to be occupied by a couple boarding in Gibraltar and we would have to see
1490 which of us had paid more. This obvious ploy for a bribe was so outrageous that it
1491 would have been funnier if I had been wealthy. As it was I had to do some desk
1492 pounding at the purser's office to obtain the bathroom opened. No one ascended at
1493 Gibraltar.

1494 My plans for that summer of 1962 again revolved around Woods Hole. I had
1495 applied again for the GFD program. I saw no reason not to, and without a Ph.D. I
1496 seemed still eligible. I can't understand now why I thought that reasonable but in any
1497 case I was admitted. In the 60 years of the program to this point, I am pretty sure I
1498 am the only person who has been a student twice. I like to joke that perhaps they
1499 hoped that by the second chance I might finally "get it". In any case Carin and I were
1500 lodged in what was the former Carriage House (L'Hirondelle) on the Woods Hole
1501 campus (where the safety office is now). It was a delightful space just a short walk
1502 from Walsh Cottage where the program had its new and permanent residence. My
1503 advisor again that summer was Melvin Stern. I was delighted to work with him again
1504 and my familiarity with his paper with Charney and my intention to pursue some
1505 aspect of the baroclinic instability problem meshed perfectly with his interests.

1506 The course lectures that summer were by Leon Mestel on astrophysical
1507 matters. I remember them vaguely as being interesting but I was focused intensely
1508 on finding a research topic that could be expanded to a thesis. The Charney-Stern
1509 theorem was restricted to a case where the horizontal boundaries of the domain were
1510 potential temperature surfaces and Melvin was wondering what would happen if that
1511 were relaxed and somehow, and here memory grows furtive, he made the suggestion
1512 that I look at the two-layer model with the addition of topography as a way into
1513 understanding the restriction on the theorem better. Somehow he was under the
1514 impression that the topography would similarly prevent a strong theorem being
1515 proved for the two-layer model as was the case with temperature gradients on the
1516 horizontal boundary in the continuous model and that might give insight into the
1517 continuous model he and Jule had worked on. I think the rationale was that the
1518 boundary term depended on the difference between the slopes of the boundary and
1519 the slopes of the isopycnals at the boundary.

1520 I got right to work and tried to see the problem from Melvin's point of view
1521 without success. The proof of a theorem in any case at first seemed hard to do. I
1522 write that now without understanding what the difficulty was because, one morning
1523 as I was shaving before breakfast I suddenly saw in a flash how to prove the theorem
1524 with topography in the most general case. The result was clearly generalizable to an
1525 arbitrary number of layers. In fact, I was shortly to show that a similar theorem could
1526 be proven in the continuous case and the additional boundary terms could be
1527 interpreted as especially thin layers in a layer model, or as Francis Bretherton later
1528 posed it, as delta function sheets of potential vorticity. After proving the two-layer

1529 result I rushed to show it to Melvin. He quickly checked it against the result of the
1530 Phillips model; a case with no lateral shear where the critical shear needed for
1531 instability was simply proportional to the beta term. When the criterion from my
1532 theorem coincided with the Phillips result Melvin became very pleased and asked me
1533 to show him how I had gotten the result. I was terrifically pleased myself and
1534 considered that result the very first thing I had done of any real importance. To
1535 round out the result I was able to extend Howard's semi-circle theorem that gave
1536 bounds on the growth rate and speeds of unstable waves to the baroclinic problem
1537 although I made an error, which I corrected later when doing my thesis, when I
1538 included the beta effect. I was feeling pretty swell as the summer ended and I gave
1539 my report and I was looking forward to the move to Cambridge and MIT and really
1540 getting launched on my Ph.D. thesis. For the first time in a long time I felt confident
1541 that I was on my way. But not too confident, because I knew there were still general
1542 exams to confront at MIT in the Meteorology Department.
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Chapter 5

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My Ph.D. and Charney

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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.

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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.

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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

1567 as no surprise when I say that the secretary, Berit Larsen, was the beautiful wife of a
1568 Norwegian graduate student in another department. Jule tended to come in late each
1569 morning and Berit was sure to have his office neatened up for his arrival.

1570 I started that September with a germ of an idea of what I wanted to do for a
1571 thesis. My first meeting, though, with Jule was something of a downer. He thought it
1572 would do me good to do some kind of observational project to familiarize myself
1573 with meteorological phenomena. My heart sank when he outlined the idea. My
1574 personal goal was to get out of graduate school and out of the subservient role of
1575 graduate student as soon as possible. So I listened with dread as he discussed his
1576 plan. I went home that night frustrated to the point of tears and vowed that I would
1577 refuse to do it and if necessary try to make my way with just my Master's degree
1578 from the Aero Department.

1579 Next morning I met with Jule and told him I had an idea for a thesis that I
1580 wanted to get started on right away. He listened attentively as I started to say that it
1581 was a problem on baroclinic instability. His face twisted into a painful grimace. "Not
1582 that old problem. There's nothing new there". I think he had in mind the, by then,
1583 classical problem he had worked on and the one more recently redone by Burger. I
1584 told him it was not that problem but that what I wanted to do is examine the role that
1585 horizontal shear of the current would play in the problem and in particular the
1586 possibility of simultaneously producing Reynolds stresses (momentum transport by
1587 eddies) and eddy heat transport in the two-layer model. As I spoke his face relaxed
1588 and he seemed to become more receptive. After I outlined my general plan I asked
1589 him whether that would be sufficient material for a Ph.D. thesis. He then gave the

1590 best answer possible to my question. He said simply, “Yes, if the results are
1591 fascinating”. I couldn’t ask for a better answer and he became immensely supportive
1592 of my idea. The plan for an observational project disappeared and was never heard
1593 of again.

1594 Nowadays, we ask students, as part of their general exam to enter candidacy
1595 to present and defend a formal thesis proposal. That conversation with Jule was the
1596 closest thing to a thesis proposal I submitted but I was on my way and it was my
1597 responsibility to make it work, i.e. to make the results fascinating.

1598 That level of engagement was typical of Jule and he was beloved for it. If
1599 you had an idea of what you wanted to do he would treat you like a colleague. He
1600 could be skeptical, demanding, questioning, but just as he would with any scientific
1601 colleague. I saw him act the same way with Holton.

1602 As I got started, I first extended the work I had done with Melvin in
1603 preparation for an attack on the difficult instability problem with both vertical and
1604 horizontal shear. In particular I corrected and sharpened the semi-circle theorem I
1605 had proven the summer before and developed other bounds on the growth rate of
1606 disturbances in terms of the magnitude of the shears and also the product of the
1607 zonal flow velocity and the potential vorticity gradient. Indeed, I was able to show
1608 that the two-layer version of the Fjørtoft theorem could be obtained directly from the
1609 same integral condition that gave the first necessary condition for instability by
1610 taking the real part of the complex equation instead of the imaginary part. It also
1611 gave an upper bound on the growth rate in terms of that zonal velocity-potential
1612 vorticity gradient product. Bounds on the phase speed and general considerations of

1613 the possibly singular nature of the governing differential equations were developed.
1614 They led me to the intuitive notion that if the horizontal shear of the initial current
1615 was limited to the upper layer and was broad enough so that it did not possess an
1616 inflection point, a useful simplification would result. It seemed reasonable to me that
1617 the absence of a sign change of the potential vorticity *within* any single layer would
1618 allow only baroclinic instability even if Reynolds stresses were formed. I also
1619 inferred that if the basic zonal flow was limited to the upper layer the real part of the
1620 phase speed would need to be less than the minimum value of the current speed of
1621 the upper layer and greater than the constant value of the lower layer. In other words
1622 the governing equations would not possess a singularity. This allowed the solution to
1623 be obtained as a simple Taylor series.

1624 My hope was that in the case of broad shears the horizontal shear would
1625 induce up-gradient Reynolds stresses. Since there was, in this picture, no barotropic
1626 instability in the absence of a change in sign of the potential vorticity gradient within
1627 the layer (there is no theorem that proves this but it seemed a reasonable inference)
1628 the Reynolds stresses would have to be, on average, moving horizontal momentum
1629 *up gradient* sharpening the flow and producing a nascent Jet Stream. This behavior
1630 was first analyzed by Starr on the basis of observations obtained during WWII and
1631 was at first controversial. Normally, one expects eddies that are the result of
1632 instabilities to smooth out gradients, like stirring milk in coffee. In fact, the eddies in
1633 the atmosphere do just that on the horizontal temperature gradient as a result of
1634 baroclinic instability and they bring warm semi-tropical air northward, smoothing
1635 the temperature gradient and thus providing a more temperate mid-latitude climate.

1636 One might expect the same smoothing of the momentum gradient but Starr found
1637 just the opposite in his observations. I recall a visit to the Meteorological Institute in
1638 Stockholm while I was there by Eric Palmen, a noted Finnish observational
1639 meteorologist. He pooh-poohed Starr's ideas saying they were impossible, crazy.

1640 There were several efforts by others to find a mechanism to yield Starr's
1641 results. One thing was obvious, the spatial phase relation needed to get up gradient
1642 momentum flux could occur in *decaying* barotropic waves. So there were
1643 suggestions, one by Charney, that it was a two-step process. The eddy was first born
1644 as a result of baroclinic instability releasing the store of potential energy in the initial
1645 state and then, in a second step, the horizontal shear led to eddy decay sending
1646 momentum up gradient.

1647 I had the idea that perhaps it could all happen at once in a single mode of
1648 instability. That is, the instability of a current with lateral shear could be produced by
1649 baroclinic instability but its form, because of the lateral shear would yield,
1650 simultaneously, up gradient momentum flux. On the other hand if the current were
1651 too narrow so that it contained a sign change of the potential vorticity gradient *within*
1652 a layer, barotropic instability would tend to broaden the jet. Thinking of it this way
1653 would suggest that there was a limit to how narrow the jet would become and thus an
1654 equilibrium value for the jet width.

1655 The first step was to solve the problem of the broad jet. I chose a velocity
1656 profile for the upper layer with a smooth parabolic shape. I wrote the solution as a
1657 Taylor series about the channel center and then needed to find the values of complex
1658 phase speed that allowed the boundary conditions of no normal flow at the channel

1659 walls that I used to confine the flow. This led to a very complicated eigenvalue
1660 problem that could not be solved analytically.

1661 It was Jule's style to leave his students to work independently but you were
1662 encouraged to come and talk whenever something interesting or difficult appeared in
1663 your work. So, one day I went down to his office and described what I had done, my
1664 thinking and what I needed to do next. He responded exactly as if we were working
1665 on this together, exactly as colleagues. He said it was clear that numerical work was
1666 needed and he was sure I didn't want to slow down my work to learn how to
1667 program the eigenvalue problem myself. He only asked me, looking at the
1668 complexity of the formulae I was trying to deal with, if I were sure of their
1669 correctness. I had checked them several times before showing them to Jule and with
1670 a, I hope, inaudible swallow, said, "sure". He then gave the numerical problem to
1671 one his technical assistants, a woman named Leola Odland who did the necessary
1672 programing in *machine language*, i.e. not a higher-level language like Fortran but
1673 something now inconceivable, in the language of the IBM 360 itself. My job then
1674 was to take the output in which the values of the determinant were printed out on a
1675 2-dimensional grid in the space of real and imaginary parts of the phase speed, c , and
1676 find places where zeros of the real and imaginary parts of the two determinants
1677 simultaneously vanished yielding the eigenvalue, c . It was painstaking work, done
1678 by hand, and exactly the kind of work I had earlier tried to avoid but I was now so
1679 excited by the prospect of what the results might yield that I worked feverishly to get
1680 to the solutions.

1681 I was thrilled to discover that instability could occur in the model as long as
1682 *somewhere* in the field the potential vorticity gradient changed sign from layer to
1683 layer and that the Reynolds stresses for the *growing* mode did move momentum up
1684 the gradient to sharpen the jet. There were other results in the thesis but that major
1685 one was probably the centerpiece along with the theorems.

1686 I also answered a question that had been raised in a lunchtime conversation
1687 with Bolin earlier in Stockholm, namely how arbitrary initial conditions could be
1688 satisfied in the Eady model that had only two normal modes. The answer was that
1689 the initial conditions were satisfied by the addition of a continuous spectrum of
1690 stable waves and using Laplace transforms I showed in the last chapter of the thesis
1691 how that worked. Not only were things working out well but they were working out
1692 swiftly. It looked as if I would be ready to defend my thesis in June of 1963, nine
1693 months after entering the department. There was just one small detail. I still had to
1694 take the general exam. I found myself in the weird position of having a perfectly
1695 defensible thesis but I first had to take the general exam whose stated purpose was to
1696 see whether I was ready to do the independent research required for the Ph.D. When
1697 I mentioned the absurdity of this to Jule he reassured me that he was certain I would
1698 pass easily. When I asked why, if he were so certain, it was necessary for me to take
1699 the exam he merely shrugged. It brought up all the fears I had been living with since
1700 my exam failures in the Aeronautics Department.

1701 A curious incident occurred before the time of the general exam. Jule's own
1702 thesis advisor of record, Jørgen Holmboe, was visiting MIT. He must have been in
1703 his early 60's in 1962 and had done excellent work in meteorology with a well-

1704 known shear flow instability problem carrying his name. He had left Norway and
1705 been an assistant professor at MIT in the 30's before moving on to UCLA. I was
1706 unfamiliar with him and his work at that time but I was shortly to have a strange
1707 introduction.

1708 As I was working in my student office, one floor up from Jule, I received a
1709 phone call from Jule asking me to come down and to describe my work to “a few
1710 interested people”. Well, there were a few rather important people there. Aside from
1711 Jule, I was happy to see both Melvin and Lou and then I saw what I took to be an
1712 elderly visitor, i.e. Holmboe. Jule just asked me to informally describe what I was
1713 working on to the assembled group. I have to say that since my work had been going
1714 so well, and the results so satisfying, I was more than happy to do so in that informal
1715 way in which one scientist will explain to another what they are up to. So I started
1716 off by saying I was examining the general stability problem in the two-layer, quasi-
1717 geostrophic model and wrote down the generally well-known equations. In those
1718 equations there are two coupling constants, one for each layer that measures the
1719 degree of coupling between the two layers. These constants involve the magnitude of
1720 the Earth's rotation, the density difference between the layers (i.e. how stratified the
1721 model is) and the characteristic horizontal scale of the motion and the *mean* depths
1722 H_1 and H_2 of the layers. In the presence of different velocities in the two layers the
1723 interface between the two layers slightly tilts, this is the physical source of the
1724 potential energy for the instability. So in principal the layer thicknesses are not
1725 exactly constant. The tilt is small, though, of order of the small Rossby number. The
1726 equations are derived under the assumption that the Rossby number is

1727 asymptotically small and so it is consistent with the quasi-geostrophic approximation
1728 that the coupling constants are constant. It would, in fact, be inconsistent to include
1729 the variation when similarly small terms are ignored elsewhere.

1730 However, as soon as I wrote the equations on the board in Jule's office and
1731 explained what each of the terms were, Holmboe just said curtly that the equations
1732 were wrong and not to be trusted because of the lack of spatial variation in the
1733 coupling constant. I tried several times to explain why the equations were valid but
1734 each time I ran up against a stonewall. It was evident that Holmboe considered *any*
1735 approximation to the equations as an error, whether the approximation was
1736 consistent or not. It struck me as strange given that Jule, himself; working with
1737 Holmboe, had done pretty much the same kind of thing in his derivation of the quasi-
1738 geostrophic equations, a prerequisite for any kind of progress in this difficult
1739 problem. It was not so much Holmboe's opinion that bothered me, erroneous as it
1740 was, it was more the unwillingness to engage in a suitably clarifying discussion.

1741 Finally, he allowed me to describe my principal results to date but it was clear
1742 that he was holding to his opinion that they were probably based on a fallacious
1743 starting point. I was rather flabbergasted and was relieved when afterwards Melvin
1744 and Jule were as astonished at that behavior as I was. I gathered then that Jule's
1745 relations with Holmboe had not been easy and in Jule's later recollections with
1746 George Platzman he talked of Holmboe as a pedant. In fact, a short while later Jule
1747 confided in me that he thought of Rossby rather than Holmboe to be his mentor even
1748 though Holmboe was his Ph.D. advisor of record.

1749 Pursuing this for a moment before returning to the chronology of my graduate
1750 work, it was during a drive down to Woods Hole for a GFD seminar with Jule that
1751 he ruminated aloud on his mentors.

1752 It was just the two of us in the car and he started to reminisce, saying that he
1753 had always considered Rossby his mentor. In fact it was Rossby who, in a letter, had
1754 recommended Jule to von Neumann to head the Numerical Weather Prediction
1755 Project at the Institute for Advanced Study in the '40's at Princeton.

1756 Jule then went on to talk about the chain of mentoring that led to him. He
1757 pointed out that Rossby had worked under Vilhelm Bjerknes, who had studied under
1758 *his* father, Christian Bjerknes, who in turn had worked with Heinrich Hertz of
1759 electro-dynamic wave fame. Jule clearly felt inspired by this chain of mentors
1760 leading back to the heroic 19th century days of physics. As you might imagine I felt
1761 he was inviting me to join that chain and consider myself equally connected to that
1762 glorious past. This was the kind of emotional and intellectual experience I had been
1763 dreaming about for graduate school and here it was really happening. It was no
1764 wonder Jule Charney was so greatly esteemed by his students. Perhaps beloved, as I
1765 mentioned earlier, is the more accurate word.

1766 Nevertheless; the chain of mentoring might have had an abrupt end right then,
1767 for while Jule was recounting these splendid ideas as he was driving towards Woods
1768 Hole he made a wrong turn on Rte. 93 and took the off ramp leading to Logan
1769 airport. I suppose in those days when he was going on business to Washington so
1770 often, that turn off became automatic for him. When he realized his mistake, he took
1771 a quick look over his shoulder and backed up the ramp to regain the highway!

1772 Fortunately, there were no other cars making that ramp turn off and we survived that
1773 nutty maneuver that might have snapped the mentor chain rather definitely. A close
1774 call it was.

1775 My relationship with Jule became very close and he increasingly treated me as
1776 a colleague. Carin and I were frequently at his and his wife, Elinor's, house for
1777 dinner.

1778 In the middle of that thesis year he had to leave MIT for a weeklong trip
1779 during the semester that he was teaching his course on dynamics of the atmosphere.
1780 He asked me if I would take the class for him for that week. I was, of course, deeply
1781 flattered that he thought me prepared to do that. I sat in on the course the week
1782 before he left to see where he had left off. Jule was a wonderful teacher but not a
1783 very good lecturer. He seemed in the middle of an argument to lose the thread and
1784 wander off in some other direction. If you already knew the material it was
1785 fascinating to see how his mind worked. If you were trying to learn it for the first
1786 time it must have been deeply frustrating for the students. When I was given the task
1787 of going through his personal papers after his premature death, I was astonished to
1788 find that his notes for the course were incredibly detailed and well organized. His
1789 problem as a lecturer, I think, is that he allowed himself to mull over the material
1790 again as he lectured and if a new thought came to him he would allow himself to
1791 muse over the new direction of thought to the despair of the frequently confused
1792 student. I found the musing inspiring but the students were often frustrated.

1793 I found that I deeply enjoyed teaching and the students responded
1794 enthusiastically. They asked me if I would review some material, especially on the

1795 derivation of Quasi-Geostrophy that Jule had lectured on and that seemed opaque to
1796 them. I was happy to do so since I had just derived those equations in my own
1797 fashion with a straightforward asymptotic approximation.

1798 In fact, when Jule returned I continued to attend his classes because I enjoyed
1799 his lectures so much. At one point when he was lecturing on the Charney-Drazin
1800 theory of vertical propagation of planetary waves the issue arose about the fate of
1801 unstable waves that the theory did not cover. I made a remark in class about the
1802 effect of the complex phase speed on the wave behavior at large heights and this led
1803 to us working together on a small paper that we published in the Journal of
1804 Geophysical Research that year. I was really having a good time.

1805 Some time towards the middle of May, with my thesis results almost entirely
1806 in hand, I took the general exams. Another graduate student, John Young, took them
1807 with me at the same time. I don't remember the questions too clearly but I do recall
1808 the sense of relief I felt when I realized I was going to have no difficulty with the
1809 exam. I recall that one question Jule asked on the written exam was how to work out
1810 the spin-up time for an annulus of fluid with a sloping bottom. Greenspan and
1811 Howard had recently published their pioneering paper on the same problem with a
1812 flat bottom and this was only slightly different since for axially symmetric solutions
1813 the variable depth enters only parametrically.

1814 Another part of the exam was take-home and this time I enjoyed it. I
1815 remember one question was posed by Lorenz and asked for the origin of the tropical
1816 Easterlies. I knew the desired answer in terms of the classical Hadley circulation
1817 problem but I couldn't resist referencing my calculation of the instability of the

1818 parabolic profile of zonal flow in the horizontal in the two-layer model. One of the
1819 consequences of the Reynolds stresses produced was to accelerate the eastward flow
1820 in the jet's center and produce westward flow on its edges and I pointed out that this
1821 would yield low latitude easterlies. This was a bit mischievous on my part but I
1822 enjoyed it and my whole attitude towards the exam could not have been more
1823 different than the exam I took just 2 years before in the Aeronautics Department; so
1824 much had changed. The take-home exam was rendered a bit uncomfortable by the
1825 presence of the temporary home of the Berklee School of music on Newbury Street
1826 in a brownstone next to our apartment. As I was doing the take-home exam Herb
1827 Pomeroy was leading some student band in rehearsal rather late in the evening as I
1828 was dealing with Tropical Easterlies and the music was accompanied by Pomeroy's
1829 loud and upper register trumpet. Although a jazz fan, I could have done without it at
1830 that moment. Pomeroy or not, I passed the exam and was ready to finish the draft of
1831 my thesis and defend it. The oral exam that was supposed to accompany the written
1832 exam was waived.

1833 Nevertheless, it was not so simple. The morning *after* I was told that I had
1834 passed, I was showering in our little apartment on Newbury Street when the phone
1835 rang. Carin was at work and I managed, dripping, to reach the phone in time to hear
1836 the department secretary, the estimable Jane McNabb, say, "Good morning Joe;
1837 please come to the department right away. Victor Starr wants to give you an oral
1838 exam". I scooted across the Harvard Bridge as rapidly as possible wondering about
1839 this unexpected development since I had been told the oral exam had been waived

1840 for me because of my performance on the written exam. I entered Jane's office
1841 where she pointed to a vacant classroom where I awaited Starr.

1842 Of course I knew about Starr. He made that terrifically important contribution
1843 in analyzing high altitude data and coming up with the result that the turbulent
1844 atmosphere, on the scale of synoptic weather systems, was actually transporting
1845 momentum up the momentum gradient and strengthening the Jet Stream. This was
1846 the non-intuitive result that I was so proud to have explained in my thesis
1847 calculations. I also knew that his graduate students adored him and spoke of this shy
1848 man protectively.

1849 He entered, sat down, introduced himself and proceeded to ask a question that
1850 immediately flummoxed me. I can't remember it in detail but it had something to do
1851 with the *shape* of the atmospheric envelope. As I was silently wracking my brains to
1852 find an answer, he began to talk in a bemused tone, "Most people believe... But the
1853 real reason is...." and so answered his own question. Looking pleased and satisfied
1854 he asked another, to me impossible question, and the pattern repeated. After the third
1855 such essay, I realized I had to do nothing but only wait expectantly for his answer to
1856 his own question, nod knowingly and wait. After about three quarters of an hour of
1857 this routine he rose, declared himself satisfied and said that he had heard I was about
1858 to finish, that I had done some interesting work, and he wanted to meet me before I
1859 left the department. While relieved, I remember thinking that there were many more
1860 pleasant ways he could have asked to meet me, e.g. a morning meeting for coffee or
1861 an afternoon cocktail but I suppose his shyness required some academic excuse for a
1862 meeting.

1863 As I was writing the draft of the thesis, Harvey Greenspan came nonchalantly
1864 into the office, sat down next to my desk and started by saying, “ Jule tells me you
1865 are about to finish soon; would you like a job in the Math department”. It must
1866 sound dreamlike to any modern student. Nowadays, the Ph.D. is usually followed by
1867 at least one post-doc year, if not more and only after a considerable time is it
1868 possible to obtain a full time academic position. Here was an offer before I had even
1869 defended my thesis! I hasten to add that this was, at the time, not that unusual.
1870 Several of my fellow students obtained positions as Assistant Professors at excellent
1871 universities right out of graduate school. Holton went to the University of
1872 Washington’s very fine Meteorology Department, for example. In my provincialism,
1873 I thought such positions represented a kind of exile from the world center, which I
1874 assumed was MIT. Also, now, looking back on that period I also realize with a shock
1875 that while preparing the thesis draft I had made no plan for what would come
1876 afterwards. I can’t imagine now what I expected to happen but what did happen was
1877 wonderful. The idea of staying on at MIT in the Math Department seemed so
1878 wonderfully perfect to me that I just accepted Harvey’s offer on the spot and felt that
1879 my life had reached a point of great stability with a foundation for scientific growth
1880 that would be unequaled. In 1963, the year I finished my thesis, the Applied Math
1881 group at MIT included C.C. Lin (who was its head), Harvey Greenspan, Lou
1882 Howard, Dave Benney, Alar Toomre, and Norman Levinson. The interests of this
1883 group centered on fluid mechanics and, moreover, there was a serious interest in
1884 problems in geophysical and astrophysical areas, just perfect for someone with my
1885 background. The group also soon would include Victor Barcion who became a

1886 collaborator with me on a number of scientific papers as well as becoming a very
1887 close friend.

1888 The thesis defense took place in June. It consisted of a brief presentation of
1889 the results of my work, a few desultory questions from several of the faculty who
1890 had clearly not read the thesis, and a few substantial questions from people like
1891 Howard who had but who were clearly positive about the work. So, after all, it was
1892 pro forma. I was now Dr. Pedlosky nine months after entering the Meteorology
1893 department and without having taken a single formal course in meteorology.

1894 I came home to tell Carin the good news, and with Harriet, we went out to
1895 celebrate at a restaurant on Charles Street, followed by a happy stroll through the
1896 Public Garden in Boston. After some years of anxiety I felt happy and relaxed.

1897 The position that I had at MIT that first year (1963-1964) was on the research
1898 staff and I can't remember the salary. However, I do remember the following year,
1899 when I was advanced to the faculty as an assistant professor, that my salary was
1900 \$9,000 for nine months. I thought it was quite sufficient. I do remember Harvey
1901 telling me a few months later that if I stayed at MIT I could expect my salary as a
1902 full professor to grow to as much as \$25,000 a year!

1903 When I moved over to the Math department I started to work on a subject not
1904 connected to my thesis work on instability. Instead, following some informal
1905 conversations with Charney, with whom I remained on close and friendly terms, I
1906 decided to examine the problem of Rossby normal modes in closed basins. I did not
1907 realize the Michael Longuet-Higgins in England had already worked out the linear
1908 problem but my work did go beyond his. With some help from Lou Howard I dealt

1909 with the initial value problem and showed the completeness of the modes and their
1910 unusual orthogonality principle that allowed an expansion in these unusual modes
1911 that although confined in a basin always had phase lines that propagated westward
1912 just like Rossby waves in an unbounded domain. Beyond that I used a perturbation
1913 expansion to calculate the nonlinear self-interaction of the modes, or in a more
1914 general case, the linear solution with friction forced by fluctuating winds, to obtain
1915 steady second order solutions that might add to the steady solutions produced by
1916 steady wind forcing.

1917 George Veronis had earlier examined the nonlinear forced problem of the time
1918 dependent circulation in a closed basin but, following an approach used by Lorenz
1919 for an open annulus, expanded the solution in a series of trigonometric functions, i.e.
1920 a sine series in the zonal direction, and appeared to obtain something like chaotic
1921 behavior for the amplitudes of the sine modes. Since the sine functions are not
1922 appropriate basis functions for the linear operator because of the beta term, such an
1923 expansion produces apparent transfers of energy between sine modes that appear to
1924 be nonlinear transfers but merely reflect that the real, linear normal modes are not
1925 sine functions. I knew I should reference Veronis' work but I did not want to be
1926 openly critical so I just mentioned that he had taken up the problem earlier, had
1927 found "interesting" results and then went ahead with my own analysis. This turned
1928 out to produce a problem for me, as I will explain later.

1929 Nevertheless, things were going well at MIT for me. Harvey was doing
1930 brilliant work on the dynamics of rotating fluids; work that eventuated in his classic
1931 monograph, *The Theory of Rotating Fluids* so our interests overlapped. One of the

1932 problems he was interested in, which he needed to examine, was how spin-up for a
1933 homogeneous fluid worked in containers for which the container depth measured
1934 parallel to the rotation axis did not have closed contours, so called *geostrophic*
1935 *contours*. His existing theory worked perfectly for containers with closed
1936 geostrophic contours but not otherwise. A simple, perhaps the simplest example of
1937 such a container with no closed geostrophic contours, would be a spinning cylinder
1938 whose bottom was sliced at a angle so that the lines of constant depth ran from one
1939 side of the cylinder into the other where they were interrupted by the cylinder wall.
1940 At that time Raymond Hide had just arrived at MIT and opened his lab. Harvey
1941 explained the problem and Ray was immensely helpful in setting up a simple
1942 apparatus so we could see what happened to the spin-up problem. The container with
1943 a sloping bottom was filled with fluid and set into rotation until the fluid was
1944 rotating with the container's rotation rate and then we slightly increased the rate of
1945 rotation.

1946 All Hell seemed to break loose. It looked as if strong eddies were separating
1947 from the "eastern"¹ boundary of the container although there were no sharp corners.
1948 Harvey was sure we were seeing boundary layer separation but I was not so sure and
1949 I thought it likely we were seeing Rossby waves. It turned out that the fundamental
1950 solution could be represented by a sum of linear Rossby normal modes of the
1951 container, of the type I had recently calculated in a rectangular domain, and in which
1952 each mode decayed with the scale-independent spin-up time of Howard and
1953 Greenspan while the sum of the modes represented the initial circulation. When we

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.

1954 summed the modes the resulting solution looked just like the lab experiment. Harvey
1955 got very enthusiastic about the sliced cylinder and we also used it to examine the
1956 generation of a “Gulf Stream” in the cylinder with the sliced bottom that for
1957 dynamical reasons mimicked the dynamical effect of the Earth’s sphericity so that
1958 the shallow part of the cylinder was “north”. Bob Beardsley turned the steady Gulf
1959 Stream model into an experimental and numerical thesis and used the power of the
1960 experiment and computer to go far beyond our analytical solution. This was
1961 wonderful start in my work with Harvey.

1962 Again, that experimental work was also accomplished in Raymond Hide’s
1963 lab. I should also mention that Raymond was also responsible for another life lesson
1964 for me. When he arrived at MIT and set up his lab he was also plunged into a great
1965 deal of administrative work that limited his ability to carry forward his own research
1966 even though he was indirectly responsible for a great deal of the work in his lab like
1967 the work that Beardsley was doing. He found himself asked to give research
1968 seminars and was forced to discuss work he had reported before to the
1969 disappointment of his audience and, I am sure, to his own discomfort. I made a vow
1970 to myself to first of all avoid as much administrative work in the future as I could
1971 and second, if I did not have relatively new work to report, to keep my mouth closed.

1972 Harvey, with whom I had become quite close, had a curious habit. Whenever I
1973 walked across the hall from my office to his to talk to him about some new idea or
1974 new direction in my work, his first response seemed, if not negative, then certainly
1975 not encouraging. At first this left me nonplussed. However, what usually happened
1976 was that the next day he would come into my office, smiling, and saying, “you

1977 know, that stuff you talked about yesterday is really interesting”. I grew to learn not
1978 to be dismayed by the first reaction and wait for the next day’s judgment.

1979 I need to emphasize how generous and supportive Harvey was. When we had
1980 written the manuscript of the sliced cylinder phenomena, I assumed that the author
1981 page would read Greenspan and Pedlosky. We had contributed equally to the paper
1982 and in such circumstances the order of the authors’ names should be alphabetical.
1983 Harvey insisted that the paper’s authors be listed in the opposite order, Pedlosky and
1984 Greenspan. When I protested, Harvey merely said with finality, “You need this more
1985 than I do”.

1986 I also continued work on the baroclinic instability problem by examining a
1987 perturbation method to deal with extensions of the Eady problem but nothing truly
1988 exciting came out of that. I also started to work on the spin-up problem when the
1989 fluid was density stratified but that is something I will take up shortly.

1990 My private life seemed happy and uneventful. Carin and I moved from our
1991 cramped and dark student apartment to a more spacious and pleasant apartment on
1992 Commonwealth Avenue (#48) just one block from the Public Garden. I remember
1993 being a bit concerned about the high rent (\$175/month) but figured we could swing
1994 it. It was such a pleasant location and the corner apartment had windows on two
1995 sides and was bright and airy. Carin felt she could quit her job at the insurance
1996 company; the work itself was not interesting but she and I underestimated how
1997 important the social aspect of the work was for her. She tried to find theatre work in
1998 Boston but without success.

1999 As seemed natural at that time we naturally felt she would soon be fully
2000 occupied with a growing family. That turned out to be an unexpected difficulty and
2001 several tests seemed to indicate some anatomical problem of Carin's and she became
2002 increasingly melancholy as a consequence. However initially it did not seem like a
2003 big problem and we were sure she would soon conceive.

2004 There was another unexpected challenge. About a month after I joined the
2005 faculty and was teaching two sections of a "service" course, e.g. Advanced Calculus
2006 for Engineers, as it was called those days, I received a letter in the interdepartmental
2007 mail asking me to come down to the Registrar's office to sign a Teacher's Loyalty
2008 Oath, a copy of which was included. I knew immediately that I would not sign but
2009 thought of it as a bureaucratic triviality that could be ignored. I think the basic reason
2010 I did not want to sign was that it seemed as an attempt at intimidation, warning
2011 teachers not to act in a manner that would make their loyalty questioned. Not signing
2012 led to a long battle, and happily, an eventual victory and the overturning of the law,
2013 but it took several years and an appeal to the Massachusetts Supreme Court. I am
2014 attaching to this memoir an appendix containing a talk I gave in 2004 describing the
2015 step-by-step, blow-by-blow, nature of that action. Suffice it to say here that it added
2016 a great deal of stress to my life during that period, stress that for other reasons I will
2017 describe, was certainly not needed. I will only say that during that period, 1964-
2018 1967, many colleagues supported me for which I remain very grateful. One of my
2019 proudest moments of that difficult time was when the case was heard in the
2020 Massachusetts Supreme Court. The notion that the action in the court that I observed
2021 from the back of the courtroom, an unknown, impecunious Assistant Professor,

2022 could have such an influence on the law struck me as a vindication of the American
2023 system. Even before the decision came down I was satisfied I had done the right
2024 thing. This 81 year-old man is happy the 26 year-old took the heat for his beliefs.
2025 Although, I was frightened of the possible consequences it might entail, again it was
2026 one of those things I did largely because I was frightened, i.e. frightened of what I
2027 would think of myself if I hadn't done it.

2028 During that period I continued my research and teaching. As a research topic I
2029 delved into the dynamics of rotating, stratified fluids essentially examining what the
2030 effect of a stable stratification would have on the many phenomena that had recently
2031 been examined by others, such as the spin-up problem studied by Greenspan and
2032 Howard and the steady, forced circulation in closed containers studied by
2033 Stewartson.

2034 The key feature of all of those studies centered on the metamorphosis of the
2035 boundary layers on the vertical boundaries of the domain. In the case of a
2036 homogeneous, i.e. unstratified fluid, those boundary layers allowed a vertical mass
2037 flux to balance the vertical mass flux in the interior. It was easy to show that the
2038 vertical mass flux in the boundary layers was insufficient to close the mass balance
2039 for strong enough stratification, that is, when the restraint on the fluid motion by
2040 rotation was equal to the restraint on the vertical motion by the stratification. In the
2041 first problem I tried, the spin-up of a stratified fluid, I rather impulsively assumed
2042 this meant that the whole time dependent circulation in the vertical plane would be
2043 choked off and the fluid would have to adjust its rotation rate by the much slower
2044 process of diffusion. This, of course, was a great blunder. The error was pointed out

2045 by Gösta Walin and it remains exhibit A in the museum of horrors I can visit at night
2046 to humiliate myself. The resolution of the problem was that fluid spins down in the
2047 usual spin-up time in regions near the lower boundary whose depth depends on
2048 rotation. In the parameter range I had been considering this includes most of the
2049 fluid. For larger stratification the problem is more as I had imagined but that was not
2050 the case I had considered. I have had to grow accustomed to the fact that when you
2051 are the first to do a hard problem you have to expect to be sometimes wrong!

2052 At the same time, I began a long collaboration with Victor Barçilon on the
2053 steady dynamics of linear motion of rotating and stratified fluids. We worked out the
2054 complete morphology of the boundary layers as a function of the strength of the
2055 stratification and applied the results to several interesting problems. One important
2056 result was to demonstrate that while the boundary layers on the sidewalls of the
2057 containers were passive when the fluid was homogeneous, the effect of stratification
2058 was to give those layers an active role in determining the interior flow in the same
2059 way the boundary layers on the horizontal boundaries did for a homogeneous fluid.
2060 This sidewall effect altered the shear of interior flow such that the swirling velocity
2061 near the upper and lower boundaries were driven to the velocity of the boundary
2062 eliminating the need for bottom and top boundary layers and choking off the vertical
2063 circulation. This was what I erroneously thought would happen for the time
2064 dependent spin-up problem but which we could demonstrate really did happen for
2065 the steady flow problem. I remember that when I realized the active nature of the
2066 side-wall layers on altering the interior flow, something that does not happen for
2067 homogenous fluids, that I leaped out of my chair at home where I had been working

2068 and danced alone around the living room so struck with the elegance of the
2069 connected physics of all the elements of our new theory.

2070 It was during this period that my private life took on a nightmarish quality.
2071 Carin, whom I loved dearly, became more and more melancholy until her depression
2072 became overwhelming. Partly, it seemed to be connected to our inability to conceive
2073 a child but it was also deeper than that. I discovered only later that she had suffered
2074 from depression in the period before we met but no one had mentioned that. Her
2075 depression grew deeper and more severe. In the autumn of 1966 I attended a
2076 conference in Sweden, on the island of Bornö. While I was there Carin visited her
2077 mother who had moved to Hawaii and was recovering from breast cancer. When
2078 Carin returned and we met in Boston she was much subdued. She told me she had
2079 suffered a seizure in Honolulu and it was the beginning of a struggle with epilepsy as
2080 well as depression. She began to talk often of suicide and was soon hospitalized.
2081 After a few months in the psychiatric ward at Mass General Hospital, during which
2082 time I was constantly hoping for an improvement and her return, she was moved to
2083 long term care at the Massachusetts Mental Health Center in a rather grim building
2084 near Huntington Avenue on the edge of the city. She was there for many months. I
2085 would visit each evening hoping to see a change for the better but in that time before
2086 there was much in the way of psychopharmacology to help, things did not improve. I
2087 felt lonely and sad and could not see what could happen in the future to change the
2088 situation. Carin's mental state became so perilous that the doctors in despair
2089 recommend electro shock therapy. Carin told me this one evening when she said she

2090 had told her doctor at the Center that she was thinking of breaking a glass medicine
2091 jar and eating the shards as a way of putting an end to her life. She was very low.

2092 The next day I found her asleep on a couch in the common area where visitors
2093 were permitted. I gently woke her and she opened her eyes and gave me a big smile.
2094 It seemed like a miracle. She seemed like her old self. We were warned the effect of
2095 the shock therapy would wear off but the hope was that in the period of her positive
2096 attitude, the talk therapy could reach her and help. Even then it was clear she was
2097 fragile.

2098 Part of the stress I felt, in addition to the emotional strain of seeing Carin so
2099 ill, were related to the financial consequences. Carin required round the clock
2100 nursing just to prevent her from harming herself, and I would come home each
2101 evening and find the mailbox stuffed full of bills from the private nurses. This was
2102 all contemporaneous with the Loyalty Oath battle so I was feeling very beset and
2103 very alone.

2104 I spent a good deal of time visiting Carin in this long-term facility and got to
2105 know several of the patients. Most seemed aware of their conditions and on one level
2106 aware of their illness. I remember one pianist who was obsessed with working out
2107 the static forces on the furniture legs in the room. He was aware that this was a
2108 device to keep him from thinking of other things but it didn't help relieve the
2109 obsession. Others seemed completely confused and hard to talk to. It was a very
2110 depressing environment.

2111 At the same time I received an invitation from the Geophysical Science
2112 Department at the University of Chicago to come and give a seminar. A few days

2113 before I was to fly out to Chicago, Norman Phillips mentioned over lunch at the MIT
2114 cafeteria in the Walker Memorial building that Chicago was going to make me an
2115 offer of a faculty appointment. My first reaction was that I was not at all interested in
2116 leaving MIT, where I was so happily ensconced to move to an unknown (to me)
2117 university as well as leaving behind the rather cozy city of Boston for Chicago.

2118 I, in fact, had a wonderful time during my visit to the Geosciences Department
2119 at Chicago and enjoyed the meeting with the Faculty there. The Geosciences
2120 Department had just been formed as a merger of the Meteorology Department that
2121 had been set up by Rossby and the Geophysics Department. Nowadays that kind of
2122 unified Earth Science department is rather common but it was a particularly bold
2123 step at the time.

2124 The department's Chairman then was Julian Goldsmith and we got along
2125 splendidly right away. As part of the entertainment I was offered that evening after
2126 my seminar, Julian, with some other faculty took me to the more elegant
2127 neighborhood of the North Side of Chicago. Where did he take me to make an
2128 impression on me? Not a nightclub or a show but to a second hand bookstore. This
2129 was Chicago! In fact I bought there a second hand copy of a translation of a
2130 collection of short stories by Giuseppe Lampedusa, noted for his novel, "The
2131 Leopard". I still treasure that collection which I have in my personal library twinned
2132 with its original in Italian.

2133 In spite of the positive impression Chicago made on me, I was not ready to
2134 even contemplate a move. Although, as Norman Phillips had predicted, the
2135 University did make me an offer, I had to reply that as I was in the middle of a court

2136 case involving the Massachusetts Loyalty Oath Law, I felt that I was honor bound to
2137 not entertain a move during that process. So, it was quite possible that the
2138 appointment would go to another person but there was nothing I could do about it. I
2139 wasn't even sure if I *wanted* to think about moving.

2140 During this difficult time, with Carin in the hospital, with financial worries
2141 mounting and my legal situation still unresolved, I leaned heavily on the friendship
2142 of my colleague Victor Barcion. It was a pleasure working with him and it was a
2143 relief to have someone with whom I could unburden myself about my fears and
2144 sorrows. Harvey was also a great support and he was very clear-eyed and explicit
2145 about how he saw my situation. One day when I was talking about the sadness I felt
2146 with Carin's illness, I said something like, "I don't know what will happen in the
2147 long run". Harvey looked at me a moment and then gently said, " You are in the long
2148 run now!" That sympathetic realism made me understand better how things stood.

2149 My friendship with Jule was also important to me during this time. Perhaps it
2150 was also to him. At some point that year, when Carin was again in the hospital and
2151 he had recently divorced Elinor, he paid me a visit one evening in my
2152 Commonwealth Avenue apartment and together we did honor to a bottle of bourbon.
2153 He started to talk very candidly about his private life and mentioned that he was
2154 deeply in love with a new woman. He was eager to marry her; too eager I thought,
2155 and I suggested he wait for a period after his divorce before making that step. What
2156 became clear in that conversation was that despite of, or perhaps because of, his
2157 rather gallant personal affairs, he had a deep yearning for the stability of an old-

2158 fashioned family life and was certain this new woman in his life would provide that.
2159 It was not to be as he hoped but that is not a subject for me to go into.

2160 During this same period Jule and I discussed several scientific problems. He
2161 had started to work on his very fruitful extension of 2-dimensional turbulence theory
2162 to include vertical structure with the use of quasi-geostrophy. He was deeply
2163 concerned about his inability to include the influence of the dynamical boundary
2164 condition at the lower surface. I attempted to reassure him that even with a purely
2165 internal model, as in the Charney-Stern theorem, his expansion in the vertical would
2166 still be a complete set even if it ignored some effects of the surface constraint. Bill
2167 Blumen later took up the complementary problem but Jule remained dissatisfied
2168 with what he considered his own partial treatment. He was always candid with me
2169 about his self-doubts and given his enormous contributions it says a lot about his
2170 intrinsic modesty and integrity.

2171 Then in the spring of 1967 several things happened that seem to give the
2172 promise of better times. Carin's mood improved immensely after the electro shock
2173 therapy and it was conceivable that she could be released from the hospital soon. At
2174 the same time I received word that I had been awarded a two year Sloan Foundation
2175 Fellowship. At that time, you did not apply for the Fellowship. If you were fortunate
2176 you were just awarded it out of the blue. I never discovered who was responsible for
2177 my being considered but it was probably some people at MIT who knew me best.
2178 The award carried a small cash stipend the first year to allow for some travel and a
2179 book allowance and a larger sum for the second year to allow the recipient to take a
2180 year off teaching. The idea was that for young academics recently graduated, the

2181 year away would allow for the start of new research directions beyond what had
2182 been done in graduate school.

2183 I had planned to use the money to go to the Math department at Imperial
2184 College in London. The people there were first rate, the language was not a problem
2185 and it seemed far enough away from our problems so that, I thought, we might have
2186 a fresh start on our married life. Naiveté, certainly, but it seemed just possible.

2187 At the same time, I made the decision to take up the offer from Chicago now
2188 that the legal issues of the Loyalty Oath had been resolved. The Department had
2189 been willing to wait until the case was decided and, again, I was hoping a fresh start
2190 in a new atmosphere would be helpful to Carin. She knew several of the older people
2191 in the department like George Platzman from her time living there as a young girl
2192 with her father. The offer, over the course of the year since it had first been
2193 presented had been improved to include tenure and the salary was substantially
2194 greater than the MIT salary I was getting and given the medical bills I had been
2195 accumulating the money issue was not a trivial one. So, with some trepidation,
2196 mostly about the somewhat daunting and insecure nature of life on Chicago's South
2197 Side I accepted. We sold most of our furniture, stored the rest and prepared for the
2198 move to Chicago via London.

2199 Another attraction of Chicago for me was the opportunity to take an
2200 appointment in a more natural department for me than Mathematics. Although I was
2201 happy to use sophisticated mathematics, I viewed myself as a person doing physics
2202 of the atmosphere and oceans and a home in a Geosciences Department seemed
2203 more fitting to me. I was going to miss my MIT colleagues and friends but the

2204 combination of advantages and the hope that my personal life would take a turn for
2205 the better made me hope that it was the right decision. Still, in my inner soul I was
2206 still very nervous about Carin's health and the robustness of her apparent recovery.
2207 There were still ups and downs in her mood but I held my misgivings to myself if I
2208 even admitted them consciously to myself. It was a life on eggshells.

2209 We sailed for London on the SS United States, established residence in a
2210 comfortable garden apartment in the attractive London suburb of Putney and started
2211 to explore what London and England had in store for us. I was just 29 but felt much
2212 older.
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Chapter 6

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The English Interlude (1967-1968)

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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

2237 was not the case. I had tried to do so without success while I was still working at
2238 MIT with Harvey but that part of the story has to wait. Another good colleague at
2239 Imperial was Derek Moore. I knew Derek from his summer visits to the GFD
2240 program in Woods Hole where he came (with his tenor sax) to visit Ed Spiegel a
2241 close friend and scientific collaborator. Derek and Ed's early work on chaos theory,
2242 roughly contemporaneous with Lorenz' work has never been properly
2243 acknowledged. I shared an office with a young, recent graduate, David Creighton, a
2244 charming man who later became an important figure in English applied math.

2245 In addition, there were two other American visitors that year. One was Steve
2246 Davis from Johns Hopkins, then at Northwestern and Stan Berger from Berkeley. I
2247 became very close with Steve both scientifically and as a friend. So the intellectual
2248 and human environments were truly superb.

2249 Just before our move to London I became interested in what I thought of as an
2250 overlooked aspect of the theory of the ocean circulation. The principal theory, as it
2251 was then, for the mid-ocean involved a simple relation, called the Sverdrup relation,
2252 between the total, vertically averaged horizontal mass transport, and what is known
2253 as the *curl* of the atmospheric wind stress on the ocean surface. That seemed to hide
2254 a somewhat weaker but non- negligible part of the circulation that was driven by the
2255 stress itself. This was connected in an indirect way to the work I had been doing with
2256 Victor on vertical boundary layers on lateral boundaries of a basin. The circulation
2257 so produced did not produce a western intensification and this was interesting in its
2258 own right. A small paper I published in Journal of Fluid Mechanics explained that
2259 point. At Imperial I began to ask myself whether I could put together a similar

2260 picture for a stratified ocean. I knew that, in general, this would be too hard but I
2261 thought I might have a chance to do it if I limited myself to a linear model and
2262 ignored all nonlinearities. Although not realistic it could be the first such model that
2263 included many disparate aspects of the circulation problem that had not been dealt
2264 with simultaneously. I gave myself the year at Imperial to work on that model. It was
2265 a complicated problem but I enjoyed putting the pieces together.

2266 At the same time, I was invited to many universities in England to talk about
2267 my earlier work. I gave seminars at Cambridge, Liverpool, Manchester, Norwich,
2268 Newcastle, Bristol, Glasgow and others that escape me now. Carin usually
2269 accompanied me and with the help of our hosts and the irreplaceable Good Food
2270 Guide we were able to eat well in England: a minor miracle.

2271 Everywhere we went we found the English people friendly and warm to us. I
2272 recall a trip to Salisbury where we went to visit Stonehenge. We visited the pub in
2273 the Trust House Hotel we were staying in and the young men in the pub insisted that
2274 I join them in a game of darts in spite of my warning that I had never played before.
2275 It was a very good time. Similarly, one Saturday afternoon we were having lunch in
2276 the pub on Putney Green (where cricket was often played) near our apartment when
2277 two very young men came up to our table as we were about to leave and asked, “Are
2278 you Yanks?” I thought this was a rather menacing start to the conversation especially
2279 when, after I replied in the affirmative, he asked us to stay put, please, and not to
2280 leave. Anxiety was mixed with curiosity and shortly the seedy looking young men
2281 returned with a rather frail older man. They explained that the old fellow stayed in
2282 his rooms all week long but on Sundays they usually went to fetch him and give him

2283 an outing at the pub. The old fellow had been a joiner (carpenter) in his youth and
2284 had travelled to the US for work and the young guys thought it would interest him to
2285 meet some Americans again. They went to fetch him even though it wasn't Sunday.
2286 So these young hoodlums turned out to be very sweet and we had a splendid
2287 afternoon helping the old carpenter reminisce about his happy, youthful days in
2288 America.

2289 The happy times for us in London did not last. One evening Carin and I were
2290 dining in a restaurant in London after a play when her face suddenly began to contort
2291 and twist and her eyes began to rapidly open and shut. A moan and cry followed and
2292 Carin fell violently to the floor in an epileptic seizure. A trip to the ER followed and
2293 from that evening on things went downhill very rapidly. There were more grand mal
2294 seizures always at unpredictable times so that simple things like dining out became
2295 nerve-wracking. Carin had been advised against late nights and alcohol but she
2296 refused to follow the advice. The return of the epilepsy was accompanied by a swift
2297 return of her depression and worse things followed. Carin began to harm herself by
2298 cutting her arms. She told me it made her feel better. I was close to a breakdown
2299 myself; life had become so tense. Her depression turned to suicidal thoughts and one
2300 day when I came home after a day visiting Cambridge University I found her alive
2301 but unresponsive in bed with just a two word note that said "I'm sorry". Emergency
2302 doctors from the hospital revived her and she was hospitalized in a rather grim
2303 English mental hospital.

2304 From then on, for our remaining time in England, life returned to the pattern it
2305 had established in Boston; suicidal attempts, self harm, and deep depression. There

2306 were times when things seem to improve and Carin was discharged from the hospital
2307 but they were temporary. The days were full of tension and danger. I had to keep her
2308 medication for the epilepsy in my office at Imperial, bringing home only enough
2309 pills for a few days at a time to prevent their abuse. Near the end of our stay we were
2310 both visiting therapists together and it seemed to help a bit. I was eager to get to
2311 Chicago as soon as possible to get settled in a more stable environment. George
2312 Platzman, by coincidence was spending the year in London and his presence was a
2313 godsend. He was very helpful.

2314 In August of 1968 we had plane tickets for Chicago and George's permission
2315 to stay temporarily in his large house in the Kenwood section of Hyde Park, the
2316 University neighborhood. As we were packing our suitcases the BBC reporter in
2317 Chicago covering the Democratic convention that was taking place there was
2318 sobbing on the radio describing "scenes of unprecedented violence". I was having
2319 doubts about the wisdom of the move to Chicago but I was eager to conclude the
2320 London sojourn as soon as possible.

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Chapter 7

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Chicago and a New Path

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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

2345 Hinds Laboratory was being constructed. Those days were exciting because the new
2346 ideas of global tectonics, mantle convection and seafloor spreading were becoming
2347 the subject of widespread interest. As a fluid dynamicist, I found the subject
2348 fascinating but at first it seemed rather distant from my own work and interests.

2349 Aside from George Platzman the other faculty member in the meteorology
2350 building was Larry McGoldrick. Larry was a former student of Owen Phillips of
2351 Johns Hopkins. He was then doing beautiful experiments on the interaction of
2352 capillary waves and analyzing the results through the aid of a technique called the
2353 method of multiple time scales, an asymptotic method that allowed the analysis of
2354 weakly nonlinear systems. Both his experimental results and the mathematical
2355 method were very intriguing and I quickly learned the basics of the method through
2356 the published work of Julian Cole. Larry and his wife Clare became good friends and
2357 we did a lot together socially.

2358 The University in those days was led by an inspiring figure, Edward Levi. He
2359 had a long history at the University of Chicago (UC) starting with his attendance at
2360 the Laboratory School, the elementary and high school associated with UC. He
2361 impressed me almost immediately. He had a breakfast in the Quadrangle Club, the
2362 university's faculty club, with the new faculty hired that year, shortly after I arrived.
2363 He had clearly done his homework and knew each of us by name and something
2364 significant about our backgrounds. He was a master of the deep gesture. One of our
2365 number, a new faculty person in the Chemistry department, arrived just a bit late to
2366 the breakfast. In a measured tone that made clear the importance of his remark, Levi
2367 greeted him by name and then added, " Now that you are here at the University of

2368 Chicago I hope that you will publish much less!” The message was clear; the
2369 important thing was significant scholarship, not resume building.

2370 Levi also guided the University through the academic turbulent times in the
2371 70’s. A student protest on the denial of tenure to a professor in the Social Science
2372 department severely tested the school. Some hotheads seized the administration
2373 building. Levi handled it perfectly. The police were not called. The occupiers were
2374 told they would be subject to disciplinary action for interrupting the academic life of
2375 the University but no physical action was taken. Levi just kept to the message of
2376 what the University was about and what it was not. It was clear that this was a
2377 subject he had thought about a lot and had built his views long before the crisis. In
2378 the end the students left the administration building on their own, frustrated that they
2379 had been unable to provoke a violent response.

2380 A few years later when Levi was chosen by President Ford to restore the
2381 Justice Department to health following Watergate, the Faculty had a going away
2382 party for Levi. It was a love fest. In Levi’s response he expressed gratitude to the
2383 Faculty for their support but also left us with an admonition that I have never
2384 forgotten. He noted, “Everything excellent is fragile”.

2385 So, from the professional side of my life I felt as if things could not be better
2386 and I quickly settled in to that stimulating environment.

2387 But the atmosphere quickly darkened. We had not been long in our newly
2388 rented apartment with all our new furniture when one evening at dinner Carin said
2389 matter-of-factly, “ I was walking across the Quadrangle² this afternoon and everyone

² The Quadrangle was the central courtyard of the University’s campus.

2390 was laughing at me”. My heart sank and in fact in just a few days Carin was
2391 admitted to the psychiatric ward of Billings Hospital, the University’s hospital. From
2392 that time forward she was hospitalized almost continuously with only brief periods
2393 spent at home. It was the beginning of a long dark period. I felt very much alone.

2394 I tried my best to hold things together. In one of the relatively brief periods
2395 where Carin was out of the hospital we bought a condominium apartment. I think we
2396 paid \$35,000 for it. The University had a program that allowed faculty who bought
2397 homes in the Hyde Park neighborhood to get a second mortgage so that even with
2398 little cash for a down payment we were able to purchase our own home. I had hoped
2399 that this token of permanence and stability in our lives would encourage Carin to a
2400 more hopeful state of mind. Unfortunately, I spent most of my time in that condo
2401 apartment alone.

2402 To keep my sanity I threw myself into my work and teaching. I enjoyed
2403 teaching a very great deal. I was not too happy with the course structure that was
2404 available to our students and with the support of the older faculty I was able, with
2405 their approval, to outline a sequence of courses and their content that I felt had
2406 greater coherence than the existing program. This became our Core GFD program
2407 and I personally taught much of it.

2408 I also began to think more deeply about the nonlinear baroclinic problem. The
2409 principal difficulty with Trevor Stuart’s method applied to that problem is quickly
2410 explained. For the problems Trevor examined, the threshold for instability was
2411 determined by overcoming friction or some other form of dissipation. That meant
2412 that at the parameter threshold for instability, where the wave was not growing but

2413 just holding its own against friction, there had to be an energy drain from the
2414 background flow to balance the dissipation. That energy flow implied an alteration
2415 of the basic flow that could be calculated from the non-growing solution at the
2416 marginal curve. In turn, that altered mean flow could be used to obtain the effect of
2417 nonlinearity on the growing wave near the marginal curve. This led to a first order
2418 differential equation for the amplitude of the wave that would start to grow
2419 exponentially when infinitesimal and then level off and reach a steady state when the
2420 effects of small nonlinearity balanced the slight instability obtained just above the
2421 threshold. It is an excellent method for such problems. The difficulty for the
2422 equivalent problems in meteorology and oceanography, i.e. the baroclinic instability
2423 problem, is that the stability threshold is normally determined by inertial constraints
2424 and not dissipation. That meant the change in the background field only occurred
2425 after the wave starts to grow. However, if you take as your basic solution the
2426 exponentially growing wave of linear theory you have frozen in exponential growth
2427 and can't answer the question of possible equilibration. That was the basic
2428 conundrum; that is, how to allow for the structural changes in the growing wave that
2429 would give rise to alterations in the basic flow without specifying that growth as
2430 exponential, since one anticipates that the nonlinearity is eventually going to alter
2431 the growth obtained from purely linear theory. Put that way, it seemed to me that the
2432 method of multiple time scales would allow the possibility of slow evolution of the
2433 wave amplitude, as yet unknown, and relate the alteration of the basic flow to that
2434 slow, but not yet determined evolution on the slow time scale associated with

2435 slightly unstable flows. Weak growth balanced by weak nonlinearity would allow an
2436 asymptotic approach to the dynamics.

2437 After struggling with the fact that I had to posit the existence of the alteration
2438 of the mean flow at a lower order than the equation that would specify it, the method
2439 worked perfectly. The system was really third order in time rather than first order in
2440 time as in the dynamics of the Trevor Stuart models (as well as the convection
2441 problem worked out by Malkus and Veronis) and the time rate of change of the
2442 mean flow was related to the time rate of change of the square of the wave
2443 amplitude. That latter equation could be integrated directly so the final, non-
2444 dissipative, system was second order. It had to be second order since in the absence
2445 of dissipation the dynamics has to be time reversible. I ended up with a nonlinear
2446 oscillation; exponential growth would give way to stabilization but the amplitude
2447 would overshoot a possible equilibrium and the wave would decrease and reach such
2448 small amplitude that linear dynamics would start the unstable cycle again. I thought
2449 the theory was beautiful and furthermore, the nonlinear problem could be solved
2450 analytically in terms of elliptic functions. This helped reveal the basic physics of the
2451 inviscid equilibration. I was ecstatic. It was moments like this that made me
2452 momentarily forget, if only for a brief period, just how gloomy my personal life was
2453 at this period.

2454 I later added a small bit of friction to the model so that the system would have
2455 a damping term of the same order as the nonlinearity and growth. This meant the
2456 equation for the mean flow had to remain first order in time; it could no longer be
2457 integrated so the total system was genuinely third order. At first I thought the system

2458 might slowly damp to a steady state but the inherent instability of the system
2459 prevented that. Then I showed that limit cycles were possible, i.e. perpetual
2460 nonlinear oscillations that lost their dependence on initial conditions. I was able to
2461 find such solutions in a quasi-analytical way, again using asymptotic techniques of
2462 the type described by Cole in his useful text.

2463 About ten years later, with the help of a graduate student, Chris Frenzen, we
2464 were able to demonstrate that the system also contained a parameter region of
2465 chaotic behavior and that the threshold could be predicted using the universal
2466 formula of Feigenbaum for the period doubling sequence of the limit cycles. The fact
2467 that the Feigenbaum ratio, derived for much simpler algebraic systems governed by
2468 simple difference equations also worked for the baroclinic instability problem was,
2469 in my mind, a spectacular result. Indeed, the kind of chaotic behavior I found was
2470 the first time a truly deductive model of such instability showed deterministic chaos.
2471 Lorenz's beautiful and pioneering work employed a model system not derived in any
2472 deductive way from the physics of a real problem. He used an approximation to the
2473 equations for thermal convection that were a poor approximation and gave
2474 misleading results for *that* problem in just the region where chaos occurred in his
2475 model. Nevertheless, his intuition that the system he used as a model would be
2476 characteristic on the truly deductive system was a stroke of genius.

2477 My academic life was also enriched by my interaction with the first two
2478 students who worked with me for their Ph.D. degrees, Frank Richter and Arthur
2479 Loesch.

2480 Frank came to see me about a year after I arrived in Chicago. He had a degree
2481 from the Colorado School of Mines and was interested in the new ideas of sea-floor
2482 spreading and mantle convection as was I. As he phrased it, our collaboration
2483 seemed like a natural team. He knew geology but not fluid mechanics and I had the
2484 complementary background so we started to work together. The literature on the
2485 subject seemed to me to be largely polemical involving arguments about what would
2486 happen in the complicated earth system. The GFD style of formulating a tractable
2487 problem, shorn of unnecessary complexity seemed unknown. There was, for
2488 example, a literature on the question of whether the mineral phase change from
2489 Olivine to Spinel in the mantle would be a barrier to convection or an energy source
2490 for the convection. It seemed natural to pose a simple model with such a phase
2491 change and see, but it wasn't for some time before that was done. It seemed as if
2492 people were having too good a time arguing.

2493 Without going into details, I will just say that working with Frank was
2494 wonderful. We posed some simple models, came to the conclusion that it was the
2495 negative buoyancy of the subducted slab, connected to the surface slab or not, that
2496 provided the locomotive force for the convection. Suffice it to say, Frank's thesis
2497 was superb. The only drawback was the misleading sense he gave me that advising a
2498 student's Ph.D. research was easy. Point the student at the problem and see it all
2499 work out. I learned later this was a seriously overly optimistic expectation in many
2500 cases.

2501 Arthur's thesis work also went along swimmingly. He examined the role of
2502 the resonant interactions of a weakly unstable wave, originating in the parameter

2503 range of instability, with two *neutral* waves in the stable regime and so sharing the
2504 energy it received from the mean flow with waves that could not grow on their own.
2505 It was also a first rate thesis.

2506 So, I was delighted with my work environment. My faculty colleagues were
2507 doing interesting work. Dick Lindzen became a good friend. He did beautiful work
2508 on the Quasi-Biennial oscillation in the tropical atmosphere that could be seen as a
2509 natural follow-on to his work unraveling the mystery of the diurnal and semi-diurnal
2510 atmospheric tides. Dave Fultz was concluding his monumental work on the so-called
2511 dishpan experiments for which he was elected to the National Academy of Sciences.
2512 George Platzman had just started his work on realistic models of the oceanic tides
2513 including topography, reawakening interest in that classical problem. Hsiao-Lan Kuo
2514 continued his work on tropical dynamics. McGoldrick was a close friend and helped
2515 me with my limit cycle work. He constructed an *analogue* computer to integrate the
2516 equations and provided direct output as phase plane figures. Victor Barcilon had
2517 started his work on inverse problems; i.e. can you hear the shape of a drum. So it
2518 was a stimulating atmosphere in the department with additional interest provided by
2519 the groups in paleontology, mineralogy and petrology. Each Friday someone, on
2520 short notice, was asked to stand before the faculty at lunch and explain in lay terms
2521 what research they were occupied with at the time. This was a great way to let each
2522 of us know what the rest of us were doing and helping us expand our interests out of
2523 our specialties.

2524 I took special pleasure in my relations with our department chairman, Julian
2525 Goldsmith. It was largely his vision of Geosciences department as a home to all the

2526 Earth Sciences that led to the formation of the department. There are many such
2527 departments in the country now but at the time it was unusual. Goldsmith himself
2528 was unusual. A brilliant geochemist he was an extraordinarily warm and
2529 approachable person and we did many pleasurable things together like trips to
2530 Wrigley Field to see the Cubs play during some years like 1969 when they were a
2531 splendid team if always fated to lose when it was important. He was also cultured
2532 but unpretentious. He was a collector of pre-Columbian art in the days before it
2533 became prohibitively expensive and was a genuine patron of the arts. He
2534 commissioned a local ceramic artist to design the entrance way to the Hinds
2535 Building, our department's new home. The enclosed space of the entrance was
2536 completely covered with sculpture consisting of slabs of ceramic whose abstract
2537 forms called to mind the textures of the Earth's geology or the atmosphere and
2538 oceans. He asked me if I would write a brief paragraph describing the connection
2539 between the art and our science. I tried to find the words of a suitable prose
2540 paragraph but in the end I settled on a free verse poem:

2541 "Earth, Air and Water,
2542 These are their forms and rhythms,
2543 Around us, always, everywhere."

2544 Julian was enthusiastic about this simple poem and had it engraved on a bronze
2545 plaque in the hall.

2546 At about this time I was put up for promotion to Full Professor from my
2547 position as tenured Associate Professor. I was asked to provide a list of people who
2548 could provide useful letters of recommendation. In that list I included George

2549 Veronis. My relations with George had become frosty after the publication of my
2550 paper on the rectification of Rossby basin modes and their contribution to the
2551 general circulation. Suddenly, one summer in Woods Hole Carin and I noticed that
2552 in public social occasions George would conspicuously avoid talking to me. It was
2553 embarrassing. Eventually, though, his hostility seemed to diminish and I had no
2554 reason to believe his letter would be anything but an objective appraisal of my work.

2555 A few weeks later I got a phone call from Julian asking me to come down to
2556 the chairman's office for a chat. This was not unusual for we often discussed
2557 departmental matters together. After asking me to be seated he said in a rather
2558 melancholy way that he was going to break a fundamental rule and read to me one of
2559 the letters of recommendation in my promotion case. It was from Veronis and,
2560 fortunately, it was so ad hominem and outrageously personal in its attack on my
2561 work and personality that it could not be taken seriously by any academic
2562 committee. Julian just said that while the letter would be ignored, he had read it to
2563 me so that I knew I had an enemy and could protect myself in the future. I found the
2564 whole thing immensely sad. I never let on to Veronis that I knew but I had just
2565 entered the list of his enemies that included Carl Wunsch and several other scientists
2566 among whom I was proud to be counted. It was good to keep the information to
2567 myself. I want to emphasize, since most people believe academia is rife with such
2568 personal enmities, that this one seems happily to be one of the few I have come
2569 across personally.

2570 The work life at Chicago was intellectually satisfying but it did nothing to
2571 deal with my personal black hole of a life. There would be times when Carin was

2572 allowed home for a brief period and she would frequently use such occasions to
2573 obtain something: matches, razor blades, etc., that she would use on her return to the
2574 psychiatric ward to hurt herself. Finally, one day one of the doctors asked me how
2575 long I was going to be able to keep it up. The brutal honesty of his question brought
2576 me up short. I had been thinking, naturally, that I would stay in this situation until
2577 Carin recovered and here was one of her doctors essentially asking me what I would
2578 do if it became clear that there was not going to be a recovery. I was in despair.

2579 In that state of despair my relative naïve innocence about adult academic
2580 behavior was considerably shaken. Women, some of them wives of colleagues in my
2581 department, called me at home as well as at the office and made clear that they were
2582 offering to console me. After years of living in a never-land between an officially
2583 married state and a de- facto single state, I accepted some of those offers. The
2584 physical release I obtained in those consolatory relationships was a deep relief but at
2585 the same time I could not help but feel I was participating in some kind of moral
2586 self-degradation. Still, the experience of physical tenderness was very much
2587 appreciated and in some cases, as I learned about the unhappiness in the lives of
2588 those women, I felt less guilty. I discovered there was a kind of community of sexual
2589 adventurers, both husbands and wives in the local academic community. I was
2590 invited to parties whose nature I never would have dreamed would be true. In
2591 retrospect this was happening to a generation of adults who had just missed the
2592 sexual revolution young people were experiencing with the introduction of the Pill
2593 into the moral sexual calculus and were eager to make up for lost time.

2594 I felt I had reached bottom when I was invited by one of my woman friends of
2595 the time to dinner at which she introduced me to a younger friend of hers who was
2596 engaged to a man from another city, was about to be married, and it was made quite
2597 clear that the woman was looking forward to a last fling before that marriage. That
2598 was too much for me and I decided I had to break this mode of living.

2599 In the spring of 1970, two events happened coincidentally that changed things
2600 forever. Carin made another spectacular attempt at suicide by throwing herself from
2601 a second floor window in the hospital while she was being taken to another ward for
2602 tests. She rather surprisingly survived and recovered. In fact, as was the case after
2603 many such attempts, she temporarily felt in better spirits. However, the University
2604 hospital reached the decision that its psychiatric ward, which was not meant for
2605 long-term care, was inappropriate for Carin. She was moved to a long-term care
2606 facility outside Hyde Park. It wasn't as grim as the place in Boston but the meaning
2607 of the move was clear. Little hope was possible. In the end, Carin and I separated
2608 and she ended her life by suicide not long after.

2609 During the period of Carin's illness I had been reluctant to travel very much
2610 out of town so I could be quickly present in any crisis. One morning as I was
2611 preparing to leave our apartment for work I received a phone call from Willem
2612 Malkus inviting me to be the principal lecturer for that summer's (1970) GFD
2613 course. I thought about it for about two seconds and agreed immediately. I thought
2614 of it as a chance to get away and into a serene and familiar environment where I
2615 could recover some psychic balance. At the same time I accepted another invitation
2616 for a little later in the same summer to lecture at a conference on Mathematical

2617 Problems in the Geophysical Sciences held at RPI in Troy, NY. I thought this little
2618 holiday was desperately needed and that I could use the time to reestablish a more
2619 orderly life.

2620 I prepared a set of lectures for the Woods Hole GFD program based on the
2621 work I had done with Victor on the theory of stratified, rotating fluids supplemented
2622 by a discussion of the instability of such flows using the techniques and ideas I had
2623 just formed by my recent work on weakly non-linear baroclinic instability.

2624 The picture I had in my mind for the Woods Hole part of the summer was that
2625 I would give my daily lecture, interact with colleagues, perhaps go to the beach in
2626 the afternoon and have a quiet supper by myself in one of the local restaurants and
2627 turn in early. I was looking for a quasi-monastic experience, clear of any emotional
2628 involvement or sexual adventure. It was a plan to restore some deeper sense of order
2629 to my life.

2630 Again, I wonder with awe about the ability of the Universe to regard my plans
2631 with an ironic chuckle.

2632 I was delighted to be again in Woods Hole. I rented a room in a home within
2633 walking distance of the Oceanographic Institution and started my lectures dealing
2634 with the structure of the steady dynamics of stratified flows. I lectured slowly and
2635 carefully and aside from the usual banter and skeptical questioning that is standard in
2636 Walsh Cottage, everything went smoothly. I had on one occasion to bring Willem to
2637 task with a humorous story. After he had asked far too many questions than his
2638 confusion merited, I told about the rural mountain man who, despite the warnings
2639 from his friends, married a woman in town notorious for her nagging. After the

2640 ceremony, as they were leaving town in his horse and carriage, the horse stumbled,
2641 shaking the carriage. “That’s one”, said the man. A short time later the horse
2642 stumbled again. “That’s two”, said the fellow. After the third time the groom said,
2643 “That’s three” and got down from the carriage and shot the horse dead. His new
2644 bride started to scream, “You imbecile. That’s our only horse. How can we do the
2645 plowing we need for planting? You are so stupid. Why did I ever marry you”? The
2646 country fellow listened calmly and when his new wife paused to take a breath,
2647 merely said, “That’s one”. So, I turned to Willem and said, “Willem, that’s one”.
2648 The audience erupted in laughter. It kept Willem in relatively good behavior until
2649 later in the summer when my good friend Steve Davis (whom I had met, remember,
2650 at Imperial College) was lecturing and Willem reverted to type by asking an
2651 unnecessarily aggressive and inappropriate question. Steve merely gazed at Willem
2652 and said, “Willem, that’s two”. It was a good summer.

2653 After my morning lecture on the second day, as I left Walsh Cottage to go to
2654 lunch, I saw Holly Schulman sitting on the lawn in front of the cottage waiting for
2655 her husband, Elliot, to exit. Elliot, a newly minted PhD student at Harvard, was
2656 attending the lectures that summer. The Schulman’s were part of the wine tasting
2657 and poker playing group in Cambridge that I had gotten to know during the years I
2658 was teaching at MIT. She mentioned that she and Elliot were going to dinner at the
2659 Landfall restaurant that evening and asked if I wanted to join them. I was happy to
2660 have the invitation and accepted. It was a dinner that changed my life.

2661 The Landfall is a restaurant in Woods Hole with a marvelous water view,
2662 quite good food and a charming atmosphere. In those days the dinner tables did not

2663 use the white tablecloths they use today, instead, they had paper place settings
2664 embellished with a quote from Joseph Conrad talking about a ship making a
2665 landfall. Holly and I started talking about Conrad, about how much we admired him
2666 as an author and how much we enjoyed his books. Elliot seemed to take no interest
2667 in the conversation and so in our conversation we seemed to become a couple apart.
2668 In fact, by the end of that evening we both knew we were in love, as strange as it
2669 might seem. I owe Joseph Conrad a lot. It was that meeting of the minds that was the
2670 start of our relationship.

2671 Soon we became lovers and in the following January Holly, with her 2 year
2672 old daughter, Anna, moved to Chicago to join me. We both were soon divorced from
2673 our current spouses and started a life together that lasted 42 years until cancer sadly
2674 ended a very happy marriage with Holly's untimely death.

2675 With my private life restored to health and with the support of a loving family,
2676 I was able to concentrate on my work, not as an escape from reality, but as an
2677 enrichment of life. For most of the following decade I developed further my ideas of
2678 the nature of the nonlinear baroclinic problem. I have already mentioned the work on
2679 the chaotic behavior of weakly nonlinear instabilities. I also used a long wave model
2680 of instability to discuss, analytically, the criterion for the selection of the observed
2681 wavelength when the supercriticality of the system was large enough so that many
2682 wavelengths could be unstable. The conventional understanding was that it would be
2683 the linearly most-unstable wave that would dominate the finite amplitude state. For a
2684 fully developed turbulent state with many wavenumbers there was much evidence to
2685 suggest that energy would flow to longer wavelengths. However, I was able to show

2686 that even for weakly nonlinear systems that had a large enough supercriticality to
2687 allow several waves, that it was not necessarily the most-unstable wave that would
2688 dominate the end state. Instead, I was able to show for systems in which wave-mean
2689 flow interactions dominated wave-wave interactions that it would be the wave that,
2690 by itself, could reach the largest amplitude that would dominate in the competition
2691 for the energy available in the basic state. This was generally not the initially fastest
2692 growing wave, which might saturate quickly leaving available energy for more
2693 slowly growing waves. I also was able to discuss the problem of the spatial growth
2694 in the nonlinear regime and models of wave amplitude vacillation.

2695 In the early years of that same decade, 1970-1980, my attention was drawn to
2696 the problem of coastal upwelling. In 1973 I was invited to act as theoretician in
2697 residence at a coastal upwelling workshop in Corvallis, Oregon at the Oregon State
2698 University (OSU). Holly, Anna and I left by train for Corvallis and were given the
2699 home of an OSU professor who was away for the summer and the use of a Ford
2700 Pinto auto for the summer. The underpowered Pinto was a State car and when we
2701 visited campsites in Oregon people gave us looks that made me want to explain that
2702 were not public employees abusing the system.

2703 We found the natural surroundings beautiful and idyllic, and Holly, especially,
2704 enjoyed the feeling of personal security that, at the time, was sadly lacking in the
2705 Hyde Park neighborhood of Chicago where we were living. We typically went to the
2706 mountains or the seashore each weekend and enjoyed to the fullest the natural setting
2707 of each. I think that summer made us question, as a family, whether remaining in

2708 Chicago was something we wanted to imagine lasting indefinitely. We began to
2709 think of the possibility of a move.

2710 I found the discussions of the models for coastal upwelling fascinating and
2711 was pleased that the boundary layer work I had done in the decade before with
2712 Victor Barcion could be adapted and extended to produce useful models of the
2713 upwelling phenomenon. Our models had two deficiencies. Most importantly, they
2714 were linear models that unrealistically assumed small deviations of the isotherms
2715 from the horizontal and second, most of the work Victor and I had done involved
2716 circulations with negligible variations along the boundary. One of the pleasures of
2717 that summer was establishing a working relation and friendship with John Allen,
2718 who was at Oregon State University. I had met John a year before at a meeting at the
2719 University of New Hampshire where we had brief discussions of the dynamics of
2720 rotating and stratified fluids. With John's help I was able to derive a useful theory
2721 for the general linear problem including long shore variations and distinguish a limit
2722 on the long-shore scale below which the onshore flow would be in geostrophic
2723 balance and the vertical velocity much reduced. Larger long-shore scales than this
2724 critical would recover the more traditional theory with no variation along shore and
2725 consequent strong upwelling.

2726 The summer had another unique professional event for me. It became the one
2727 and only time I went to sea for professional oceanographic reasons. When the
2728 assembled group in the workshop realized I had *never* gone to sea on an
2729 oceanographic cruise they good-naturedly, but relentlessly, teased me into accepting
2730 a short cruise of, I think, 3 days duration, whose stated purpose was the recovery of

2731 current meter moorings on the Oregon shelf. The sea, fortunately, was like glass so I
2732 avoided seasickness. I also discovered if you were not involved in the scientific work
2733 of the ship at sea that the time spent on a cramped, smelly (fuel oil) ship can be
2734 devastatingly boring. I did take one bucket thermometer reading of the sea surface to
2735 establish ground truth for an overflying NCAR plane measuring sea surface
2736 temperature with an IR instrument. That cruise and that measurement is my total
2737 experience with seagoing, observational, oceanography!

2738 After a year or so living together in Hyde Park, Holly and I decided to move
2739 from the condominium apartment I had bought with Carin to a small, 19th century
2740 house on Blackstone Avenue. The house was charming and not too practical for the
2741 Chicago winters but we enjoyed having our own house and Holly particularly
2742 enjoyed the large backyard and garden that came with the property. We took full
2743 advantage of the University's policy to provide second mortgages to faculty who
2744 bought homes in the Hyde Park neighborhood around the University. There were
2745 actually wild strawberries in the garden and a fruit bearing peach tree in the
2746 backyard. The house cost \$45,000 and with the help of the University we were able
2747 to buy it for only \$ 2,000 in cash. It was a wise investment.

2748 We enjoyed much of what Chicago had to offer: great restaurants, wonderful
2749 art museums and the Lyric Opera to which we had season tickets. What was less
2750 attractive was the sense of personal insecurity I referred to earlier because of a
2751 serious crime problem in the Hyde Park neighborhood. Holly was especially
2752 concerned about our safety and after the summer in Corvallis, during which time our

2753 security was not an issue, we began to ruminate about a possible move to a less tense
2754 environment.

2755 We had been living together since the winter of 1971 and 3 years later we
2756 decided to get married. As neither of us was religious, we hoped to have a Justice of
2757 the Peace marry us in our back garden. We discovered Justices of the Peace do not
2758 make house calls so we asked a compliant Unitarian minister to do the ceremony and
2759 we wrote out the language of the ceremony to be sure nothing religious crept into the
2760 ceremony. We invited a host of friends and hired a friend of the McGoldrick's to
2761 cater the small reception after the ceremony. Since some of the guests brought their
2762 children we asked the caterer to provide peanut butter and jelly sandwiches for the
2763 children while more adult fare was prepared for the others. In later years the myth
2764 grew that the reception fare was purely champagne and PB&J sandwiches. A
2765 charming myth, but like many memories, entirely wrong!

2766 During this period we developed the pleasant habit of leaving Chicago in the
2767 torrid summers and driving to Woods Hole where I attended the GFD summer
2768 school as a visitor. We deeply enjoyed the beauty and serenity of Woods Hole and it
2769 seemed a pleasant alternation with the urban life of Chicago, which to be fair, we
2770 also enjoyed, security issues aside.

2771 As we mulled over the possibility of a move to someplace outside Chicago
2772 where Holly would feel more secure, we were soon presented with an attractive
2773 possibility. In 1975 the Scripps Institution of Oceanography made a very tempting
2774 offer of a position with 11 months per annum of hard money (i.e. institutional and
2775 not grant dependent) support. Our visit to Scripps was rendered even more delightful

2776 by the generous offer by Walter Munk to lend us his house during our visit while he
2777 and his wife were away in Colorado. So, we set off with our curiosity piqued, to
2778 investigate the possibility of new life perpetually away from winter.

2779 We found La Jolla charming and friendly and I found, as expected, that the
2780 people I would interact with at Scripps, were I to move there, absolutely first rate.
2781 However, two big issues deterred us. First, the nature of the department at Scripps in
2782 the area of physical oceanography seemed very disjointed and split into sub groups,
2783 some of whom were in different buildings distant one from the other. I worried about
2784 the seeming lack of interaction, and could not understand how I could do there,
2785 something like what I had done in Chicago with the curriculum, that would require
2786 an agreement across the department. The second big deterrent was the real estate
2787 market in La Jolla. Prices of houses were so much higher than in Chicago that it
2788 appeared to us that we would end up living far from the Institution and probably far
2789 from the ocean in a situation requiring a long daily commute. That took some of the
2790 potential bliss off the table. In the end we decided to stay put in Chicago for the time
2791 being. It was during this time that Holly's interest in artistic photography became an
2792 increasingly important part of her life. She took courses at the Art Institute of
2793 Chicago. During the week, I would stay home one day from the University so Holly
2794 could have the day free to use the darkroom at the Geosciences Department. Our
2795 good friend Julian Goldsmith made the necessary arrangements to give her that
2796 opportunity. Her photographic work flourished and established a foundation that
2797 would enable her to teach the subject. She was a terrific teacher as well as artist and

2798 it was exciting that we both had intellectual pursuits that we could share with one
2799 another.

2800 I still continued my work on coastal upwelling and one paper that came out of
2801 that effort had an indirect influence on some later work.

2802 The major weakness of my upwelling work, at least in my own eyes, was its
2803 restriction to linear theory, i.e. the unrealistic idea that the isotherms departed only
2804 slightly from horizontal surfaces. I tried to confront that problem by recasting it as
2805 the steady state reaction to a mass sink located in the corner region at the intersection
2806 of the sea surface and the coastal boundary, envisioning that sink as the
2807 manifestation of the net upwelling produced by the offshore Ekman flux. The sink
2808 was fed by an equal onshore deeper mass flux, independent of depth. That provided
2809 an offshore boundary condition for the problem. The solution of the nonlinear
2810 problem was straightforward once the conservation of potential vorticity (pv) was
2811 used. For the case of uniform stratification far from the coast this reduced the
2812 problem to a standard linear partial differential equation and the solution was nearly
2813 immediate. The principal reason I get pleasure recalling this is that just a few years
2814 later this same device, using the same conservation law of pv allowed me to solve a
2815 much more important problem and it reminds me how much that method was in the
2816 forefront of my mind at that time.

2817 I also adapted a method introduced by Francis Bretherton and Brian Hoskins
2818 in their study of frontogenesis to discuss the time dependent problem of the *onset* of
2819 upwelling in the nonlinear regime, again capitalizing on pv conservation.

2820 On the whole, this period in the late 1970's at Chicago was a very fertile time
2821 for my work. It was at the end of that decade and shortly before we left Chicago that
2822 I did the work I mentioned earlier on the chaotic dynamics of weakly nonlinear
2823 baroclinic unstable waves. So I felt that I had hit a period of creative satisfaction.

2824 At home things were going very well also. We started planning a sabbatical
2825 year away for the period 1977-1978 and while we had no definite offers to go places
2826 other than Chicago it was becoming clear that for both Holly and me that the tension
2827 of living in the urban setting that Hyde Park, Chicago presented was unsustainable.
2828 We had no definite plans but we felt open to change if one was presented to us.

2829 We discussed together where we would spend that sabbatical year and, having
2830 enjoyed many summers at Woods Hole, came to the conclusion that a yearlong
2831 sabbatical year there would be good for the whole family. I was successful in
2832 obtaining a Guggenheim award to support the sabbatical and we found tenants for
2833 our house in Chicago and drove off to Woods Hole in the fall of 1977 in the
2834 expectation of a new adventure.

2835 My plan, as outlined in the Guggenheim application, was to attempt to write a
2836 book on Geophysical Fluid Dynamics. While there were excellent modern books on
2837 fluid mechanics, e.g. Batchelor's book, then recently published, and good books on
2838 elementary meteorology and oceanography, there seemed to me to be a lack of a
2839 good book dealing with the fundamentals of Geophysical Fluid Dynamics on a
2840 graduate level at least in the form that I thought was essential and which the
2841 curriculum that I helped introduce at Chicago reflected. So part of the luggage I
2842 brought with me to Woods Hole was a pile of course notes from the various courses

2843 I had taught at Chicago and earlier at MIT. I was a bit daunted by the prospect but
2844 also excited to see if I could do it.

2845 I made one big decision first and this was based on an experience I had with
2846 my good friend Victor. We had made an earlier attempt to write a GFD book but it
2847 somehow got bogged down in excessive detail and an over emphasis on rigor and we
2848 came to the mutual conclusion to set aside the existing draft of several hundred
2849 pages and not pursue it further. We both felt relief at the decision and were
2850 especially happy that we had not contracted with any publisher to deliver a book for
2851 if we had, we might have found ourselves the authors of a published book we were
2852 discontented with. In going ahead with my new book project I was equally
2853 determined to write the book first and then if, and only if, I were happy with the
2854 result would I look for a publisher.

2855 We moved into our rental home in Woods Hole, on Ransom Road, not far
2856 from Quissett Harbor and in easy walking distance to the Clark Laboratory on the
2857 WHOI campus where I had my visitor's office. I already was acquainted with a fair
2858 number of the Physical Oceanography department's scientists. They were a stellar
2859 group with Hank Stommel clearly the brightest star. Others were also very
2860 impressive: Peter Rhines, Bill Schmitz, Bruce Warren, Breck Owens, and many
2861 others of the same caliber. I quickly settled in to this stimulating environment and
2862 got to work writing.

2863 I found that the process of writing, and I hasten to add that in those days it was
2864 *writing*, i.e. all in long hand, was extremely pleasing to me. I had formed a rough
2865 outline for the book while I was still in Chicago. My intention was to write a text and

2866 not a research monograph. My feeling, which I believe time has validated, was that a
2867 research monograph, while of some fleeting value, becomes out of date fairly soon
2868 by its very nature. Instead, I wanted to deal with what I considered to be the basics
2869 and the essentials of GFD in a coherent way that brought the student to the jumping
2870 off point where the student was in possession of a strong enough foundation to begin
2871 independent research and where a researcher already in the field could refer to the
2872 book to provide a starting point for advanced work on a new research problem. I felt
2873 it was that kind of book that was sorely lacking in the field.

2874 So my plan was to avoid repeating some of the basic fluid mechanics such as
2875 the derivation of the Navier Stokes equations but discuss their consequences, e.g.
2876 basic theorems, with a heavy emphasis on the physical content and then carefully
2877 and systematically derive and discuss things like quasi-geostrophy and its
2878 applications. Basic ideas about the ocean circulation and synoptic scale instabilities
2879 in the atmosphere and oceans would also find their place in a systematic
2880 development of the theories. I was especially anxious to provide a text in which the
2881 basics were derived so clearly that many traditional approximations like geostrophy,
2882 the Boussinesq approximations or the use of the beta plane could be appreciated in a
2883 mathematically rigorous but yet physically meaningful development. A collection of
2884 classical examples like the explanation of the oceanic western intensification or
2885 baroclinic instability could be interpreted so clearly that a path to further research
2886 would have a clear starting point. At least, these were my goals.

2887 So, I wrote. I wrote every weekday. After reviewing what I wrote the day
2888 before, making modifications when I thought they were needed, I moved on to write

2889 what I had the day before planned to cover today. Then, for about an hour before
2890 walking home, I would carefully outline what I would write about the next day. This
2891 pattern of composition worked very well for me and as I filled loose-leaf notebook
2892 after notebook I felt a growing sense of satisfaction as I saw the text take shape. The
2893 department was extremely generous. I was asked to teach a Joint Program course on
2894 the evolving manuscript and I was happy to do so because I was anticipating the
2895 feedback from the students would help correct pedagogical deficiencies. In exchange
2896 I was able to get the help of a secretary (now a very retro term, such helpers are now
2897 called Administrative Professionals –AP’s-) to type the evolving manuscript and the
2898 graphics department to help in the preparation of the figures. It seems rather quaint
2899 to me now that the figures had to be first drafted by hand from calculations done
2900 with a simple hand-held calculator and graphed as best I could. But it all went
2901 exceedingly well and when I gave a reading course based on the manuscript, the
2902 student response was enthusiastic.

2903 I asked one of my Chicago colleagues, Peter Wyllie, who had written several
2904 texts on petrology, his advice about choosing a publisher. He strongly recommended
2905 a commercial rather than an academic press. As I recall, his main reason was that a
2906 commercial publisher would do a better job making the new book known. He also
2907 recommended Springer Verlag as a company who did a fine job aesthetically with
2908 their books and mentioned that they really liked books, a characteristic he said that
2909 was not always the case with commercial publishers. So in April of 1978, with a
2910 large, heavy stack of hundreds of pages packed in a sturdy cardboard box, Holly and
2911 I marched into the small Woods Hole post-office and mailed the package, as

2912 requested, to Springer in New York and waited for their reaction. When I first
2913 contacted them and mentioned the size of the manuscript they seemed taken aback
2914 but when I gave them examples of other fluid mechanics texts of the same size they
2915 agreed to accept the manuscript for review.

2916 I should mention that as I was writing away and the manuscript grew in size I
2917 began to worry about what would happen if the manuscript would be destroyed by
2918 some accident. I took the first 500 pages of the text to an art supply and service store
2919 in Falmouth and had two Xerox copies made. I kept one in my WHOI office and one
2920 in the closet of our Woods Hole rental. My thought was that if both were destroyed
2921 by fire simultaneously that it would be a signal from fate to let the project drop.

2922 After mailing the finished manuscript, Holly and I walked into the Landfall
2923 restaurant and at the table where we had fallen in love eight years before we had an
2924 alcohol enlivened lunch where we toasted each other and the delights we were
2925 experiencing in Woods Hole.

2926 Indeed, even beyond the satisfaction of the composition of the book, we
2927 enjoyed visiting many parts of Cape Cod. The bird sanctuaries, the various town,
2928 state and national parks on the Cape were all delightful. We especially enjoyed
2929 Nantucket, which in those days was reached by ferry leaving from Woods Hole
2930 instead of Hyannis as it does now. We found our times together very romantic and,
2931 as folk wisdom remarks, a sabbatical year is the most successful time to conceive a
2932 child as it had been our intention to do for some time and we finally succeeded that
2933 autumn. Dove our younger daughter was born the following spring.

2934 We also enjoyed the surprising bounty of music available in the winter in
2935 Woods Hole, both jazz and classical and Holly found it inspiring her photographic
2936 work. At the same time I started, at age 40, to learn to play the clarinet. It had been a
2937 childhood dream of mine, largely driven by my enthusiasm for Benny Goodman and
2938 Artie Shaw, to play that instrument. Sadly, money was lacking as a child and public
2939 schools had no instrumental music programs as they do now. I thought a sabbatical
2940 year would be a perfect time to start. I had a “now or never” feeling. A music teacher
2941 in the Falmouth Schools, Jan Von Herten, recommended a possible instructor, Mike
2942 Crocco, and I started lessons with him on a rented clarinet. I loved it immediately
2943 and the clarinet, and the ability to play music has enriched my life ever since. I have
2944 continued weekly music lessons with a sequence of teachers to my great benefit and
2945 pleasure.

2946 While waiting to hear from Springer what the reviewers would say about the
2947 book I began to interact with Hank Stommel on a problem he and an associate,
2948 David Behringer, were working on. Hank had the idea that the heat release to the
2949 atmosphere by the warmer water of the Gulf Stream would alter the atmospheric
2950 flow and feed back onto the wind stress driving of the ocean and so alter the Gulf
2951 Stream’s strength and path. I tried without success to come up with an analytical
2952 model that would be helpful. Nevertheless, I found the interaction with Hank to be
2953 an immense amount of fun. He later remarked to me, after we had worked on
2954 something else much more substantial, that working together with people was one of
2955 his favorite ways of making a new friend. We talked about a number of ideas that
2956 year, none of which led immediately to a project together but it did lead, at least on

2957 my part, with a desire to get to know this fascinating person better and to grasp a
2958 good chance to work together. We connected not only on a scientific level but we
2959 had long talks about non-scientific topics and we often found ourselves in
2960 agreement. We did disagree about a few important things; he felt, for example that it
2961 was an error for the Oceanographic to have gotten involved in an education program,
2962 even on the graduate level while I thought the graduate program it ran together with
2963 MIT was a brilliant idea and a fascinating educational experiment.

2964 A special Christmas PO seminar was a tradition. Melvin Stern had, one year,
2965 given one dressed in a Santa Claus costume. When I was asked to give one that year,
2966 1977, I gave it on my relatively new nonlinear theory on the onset of upwelling. I
2967 also ended the talk by a reiteration of the seminar in doggerel verse based on the
2968 Night Before Christmas, which I called the Night Before Onset. It started with:

2969 Twas the night before onset
2970 And all through the ocean
2971 Not an eddy was stirring
2972 The large scales of motion.
2973
2974 The boundary layers were hung
2975 By the coastlines with care
2976 In hope that the wind stress
2977 Soon would be there.
2978 (and so forth).
2979

2980 In short, that year was great fun for all of us in our family, a family about to
2981 increase in size. Some other of the pleasures were modest but had a special Woods
2982 Hole quality. As I walked to work each morning, part of my pathway would skirt the
2983 ocean on the Woods Hole to Falmouth bike path (now much extended northward).
2984 Each morning I would be met by two golden retrievers and the dogs loped along
2985 beside me (especially as I began sharing small portions of the lunch Holly had
2986 packed for me) until we passed close to a small salt pond. Two swans swam serenely
2987 in the pond and almost every morning the same little drama would take place. One of
2988 the dogs would plunge into the pond swimming aggressively towards the swans that,
2989 in turn, swam in retreat. As this was happening the other dog quietly reached the
2990 point on land to which the swans were being driven. The dogs must have been
2991 thinking themselves very clever. At the last moment, when the two dogs had nearly
2992 driven the swans into their trap, the swans would take wing leaving the dogs staring
2993 dumbstruck at their flight. The look on the dogs' faces was, "How did they do that?"
2994 It was merely amusing the first time it happened but when it was often repeated I
2995 began to see it as some kind of deep philosophical lesson although I am not sure I
2996 can articulate it clearly. Nevertheless, the year in Woods Hole seemed to be full of
2997 personal lessons of that sort.

2998 We left Woods Hole to return to Chicago in the fall of 1978 and realized we
2999 were going to miss that environment greatly. That led to an exciting period of
3000 decision-making. First of all, WHOI made me an offer of a nine-month hard money
3001 position funded by the Doherty Chair for 6 months and 3 months of support from the
3002 Education program under the condition that I pledge to be involved deeply in the

3003 department's education program. Of course, I loved the idea of being so involved,
3004 especially in the Joint Program with MIT that I thought of as one of the most
3005 innovative graduate education programs in oceanography in the country.

3006 John Steele, who was the Institution's director at the time, assured me that I
3007 could think of the offer of becoming the holder of the Doherty Chair in
3008 Oceanography as if I were at a university. Perfect, I thought. To make the issue even
3009 more complex and exciting, I almost simultaneously received an offer from the
3010 University of Washington of a professorship in their School of Oceanography while
3011 becoming the Director of their Joint Institute between NOAA in Seattle and the
3012 University. Seattle was very tempting. A beautiful and cosmopolitan city nested in a
3013 beautiful natural setting. We also had many friends there, Jim and Emily Baker
3014 (Emily was a roommate of Holly's at Radcliffe), Jim Holton who shared an office
3015 with me as we were both writing our Ph.D. theses under Jule's supervision and
3016 others like Bruce Taft whom I had gotten to know that magic summer of 1960 in
3017 Woods Hole. A visit that autumn to the University and Seattle only made it seem
3018 more attractive. We were torn.

3019 In the midst of our conundrum, I received the reviews of the book manuscript
3020 from Springer and they exceeded my fondest hopes. I was particularly struck by one
3021 review that forecast that the book would find a place on bookshelves alongside the
3022 classic texts of Lamb and Batchelor. I was exuberant at this response. There was a
3023 little tussle with Springer over the book cover. They considered the book,
3024 Geophysical Fluid Dynamics (GFD) as part of their geophysics series and so wanted

3025 to give it their standard clay colored cover. That seemed bizarre to me for a fluid
3026 dynamics text and they finally relented and allowed a cover in deep blue.

3027 That final winter in Chicago was very hard. We experienced record cold and
3028 record snowfall. The streets were packed with snow and all streets became de-facto
3029 one way, so much snow had been heaped to each side by the plows. We put our
3030 house on the market because we knew we were going to move somewhere in the
3031 next year even if we weren't sure where. The grip of snow on the city and a
3032 moderation in the previous hot housing market produced a dearth of interested
3033 buyers. We were tense about the prospect of moving with the house unsold.

3034 To deal with our indecision, Holly and I played a psychological game with
3035 ourselves. We decided to choose a destination as final and not tell anyone our choice
3036 and live with it for a week and see whether, in our hearts, we were happy with the
3037 decision. One event that helped us make up our minds was a gift box of used baby
3038 clothes in anticipation of our soon-to-arrive baby, sent to us by our friends, Breck
3039 and BL Owens in Woods Hole.

3040 When we opened the box we were struck with a feeling of great nostalgia by
3041 the aroma emanating from the clothing. We both burst into laughter when we
3042 realized that our warm feelings had been produced by the unmistakable odor of
3043 mildew---classic Woods Hole mildew. That certainly told us where our hearts were
3044 and we soon after made the decision to accept the offer from Woods Hole. Except
3045 for the annual occurrence of a too-cold New England February, I've never regretted
3046 the decision. We were going to Woods Hole.

3047 On March 28th, 1979 our younger daughter Dove Helena was born at the
3048 Michael Reese Hospital and Julian Goldsmith's wife, Ethel, said to me, "We will
3049 miss you but your daughter is a Chicago girl."

3050 We sold the house and closed the sale two days before we pulled away from
3051 the curb with the car loaded with children, luggage, bicycles and a perhaps a hamster
3052 cage. Our furniture was simultaneously being loaded into a moving van that was to
3053 arrive at our new home shortly after us; at least that was the plan.

3054 Our period in Chicago was ending and yet as I wrote to the wonderful Dean of
3055 the School of Science at U Chicago, Albert Crewe, the University had made such a
3056 deep impression on me that part of me permanently remained behind. My
3057 recollection of the University of Chicago, how it was organized around the Faculty
3058 and the integrity of its principal academic leaders remains in my mind as the ideal
3059 model of an academic institution. The University navigated the difficult period of
3060 student unrest in the 1970's with grace and wisdom. I was also leaving behind many
3061 good friends on the Faculty. I had become particularly close to George Platzman. On
3062 the surface George was very reserved but he had an antic sense of humor and a
3063 passion for collecting original musical manuscripts and also original scientific ones.
3064 His proudest possession was an original of Newton's Principia that he acquired in
3065 London. Larry McGoldrick, Victor Barcion and the paleontologist Tom Schopf
3066 made leaving U Chicago emotionally painful. Tom was the last person in my life to
3067 knock on my house door with a baseball glove and ball in hand to ask me if I could
3068 come out and play! We went down to the Midway, a stretch of green along S. 57th St.
3069 where we played catch and where he taught me how easy the famous Willie Mays

3070 over the shoulder catch really was. The hard part was the quick recovery and the
3071 throw to the infield.

3072 There have been many occasions since when I have used the University as a
3073 measuring stick to judge the quality of academic leadership here in Woods Hole,
3074 usually in Chicago's favor. I missed Julian Goldsmith as a friend and department
3075 chairman and missed the inspirational leadership of Ed Levi for the University as a
3076 whole.

3077 It's a mistake, though, to focus on what good things are lost when you make a
3078 move like that one. In a telephone discussion I had with Hank Stommel when Holly
3079 and I were agonizing over our decision, Hank said something that struck me as very
3080 wise. "You can't hold on to everything you have and always add to it. You always
3081 have to give up *something* if you try to better your situation". I have found that the
3082 remark is deeply true. In moving to Woods Hole there was clearly a tangible
3083 improvement in my scientific career but in spite of the cultural riches unexpectedly
3084 available in the small Woods Hole environment, the broader, less tangible riches of a
3085 great university were traded for them.

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Photos:

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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.

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3094

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3096 An evening in New York: I am second row far left. The cigarette in my hand
3097 is mostly for show. I never inhaled. The girl I am with is again, the same object of
3098 my unrequited passion. Some men never learn. Kern Kenyon, one of my college
3099 roommates and with whom I travelled to California before my GFD summer of 1960
3100 is first row, 3rd from right.

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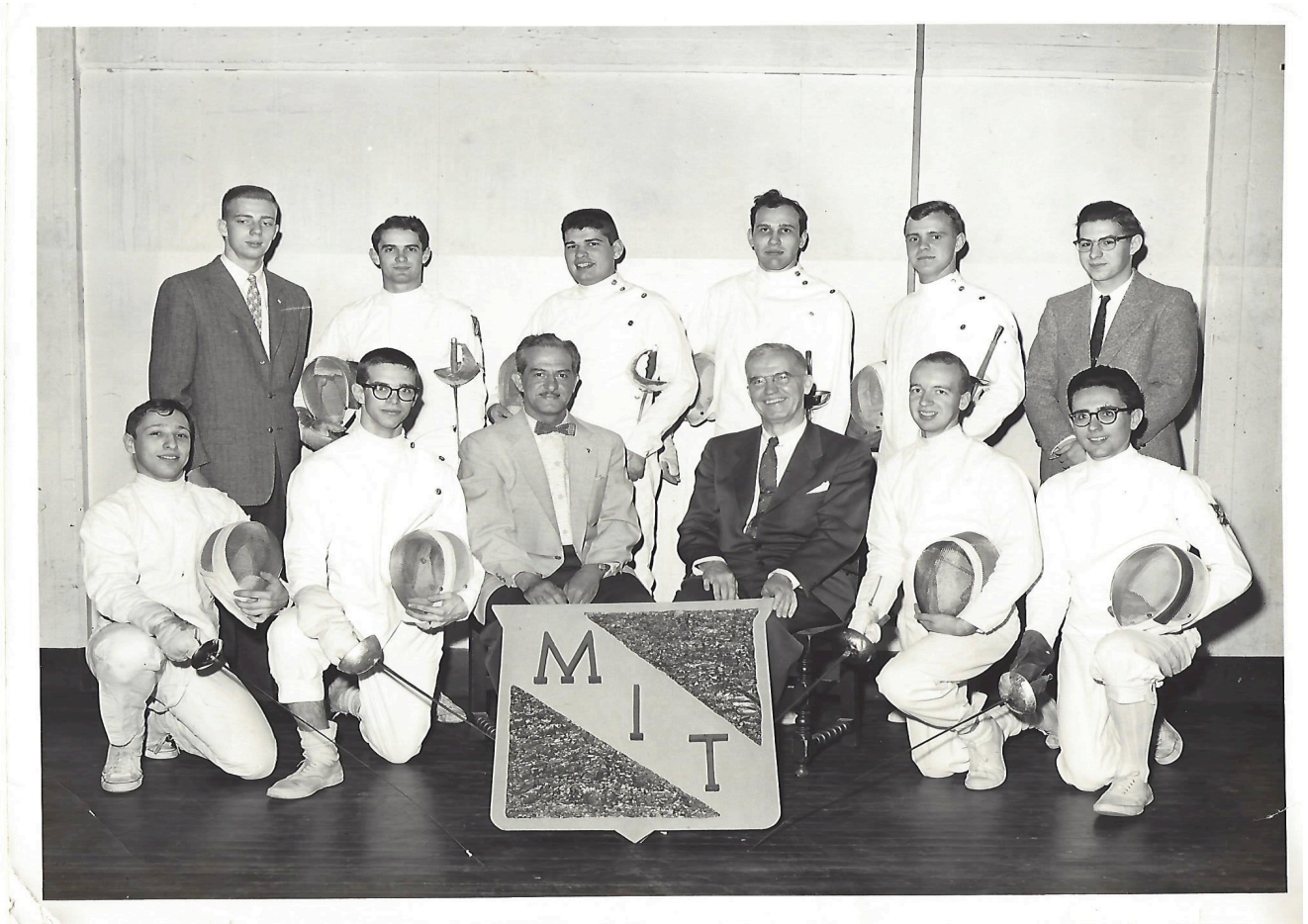
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3111 The MIT fencing team (minus one): I am second from left, first row. The
3112 fencing Maestro, Silvio Vitale is on my left.

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3124 My three favorite Aero professors, Leon Trilling, Holt Ashley and Erik

3125 Møllo-Christensen

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3146 Our wedding reception: From L-R; my mother, my father, Carin and I. The
3147 reception was held in the living room of Challenger house on the campus of the
3148 Oceanographic Institution. It is now a business office.

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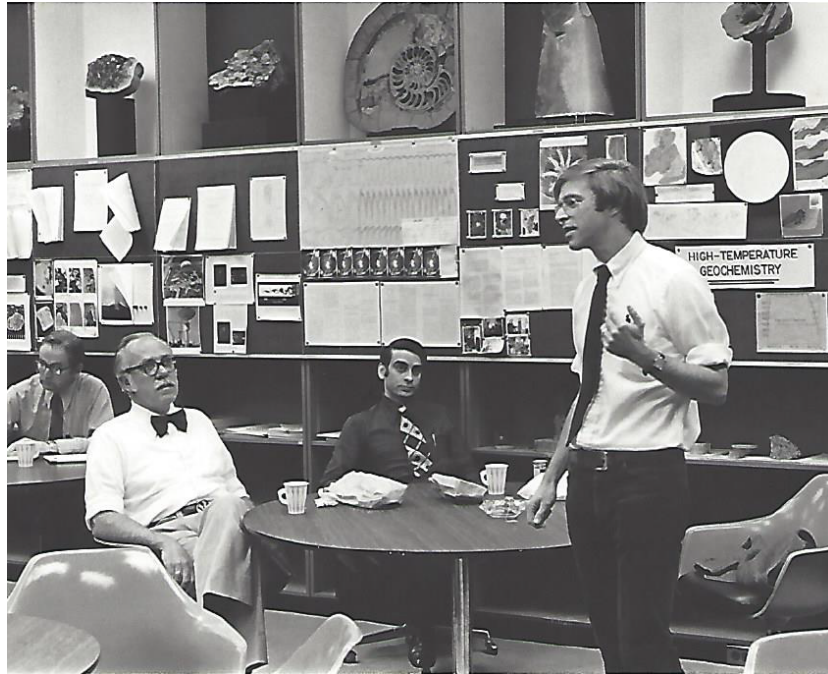
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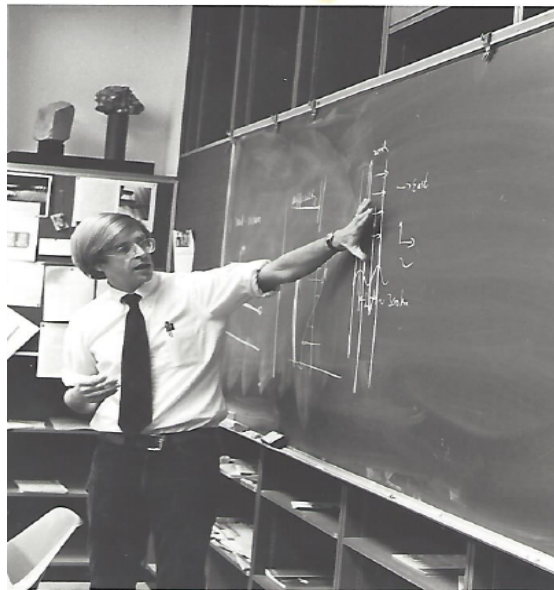
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My official MIT Faculty portrait, 1964. My battle with the Massachusetts
Loyalty Oath for teachers was about to begin.



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3166 At the Geosciences department in Chicago, each Friday noon a faculty
3167 member was asked, at short notice to describe recent work. Here, I am describing my
3168 ideas of mid-ocean instabilities.
3169

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3172 We became great friends with David and Pat Farmer. David was curator of
3173 European painting at the Chicago Art Museum (the Art Institute) while Pat was
3174 curator of painting for one of the large downtown Chicago banks. Their Halloween
3175 parties were legendary and one year I was able to fulfill my fantasy and became a
3176 Renaissance prince (for an evening). The plot thickened when a Chicago policeman
3177 rang the door bell to mention an illegally parked car but backed away when I
3178 answered the door dressed as shown.
3179

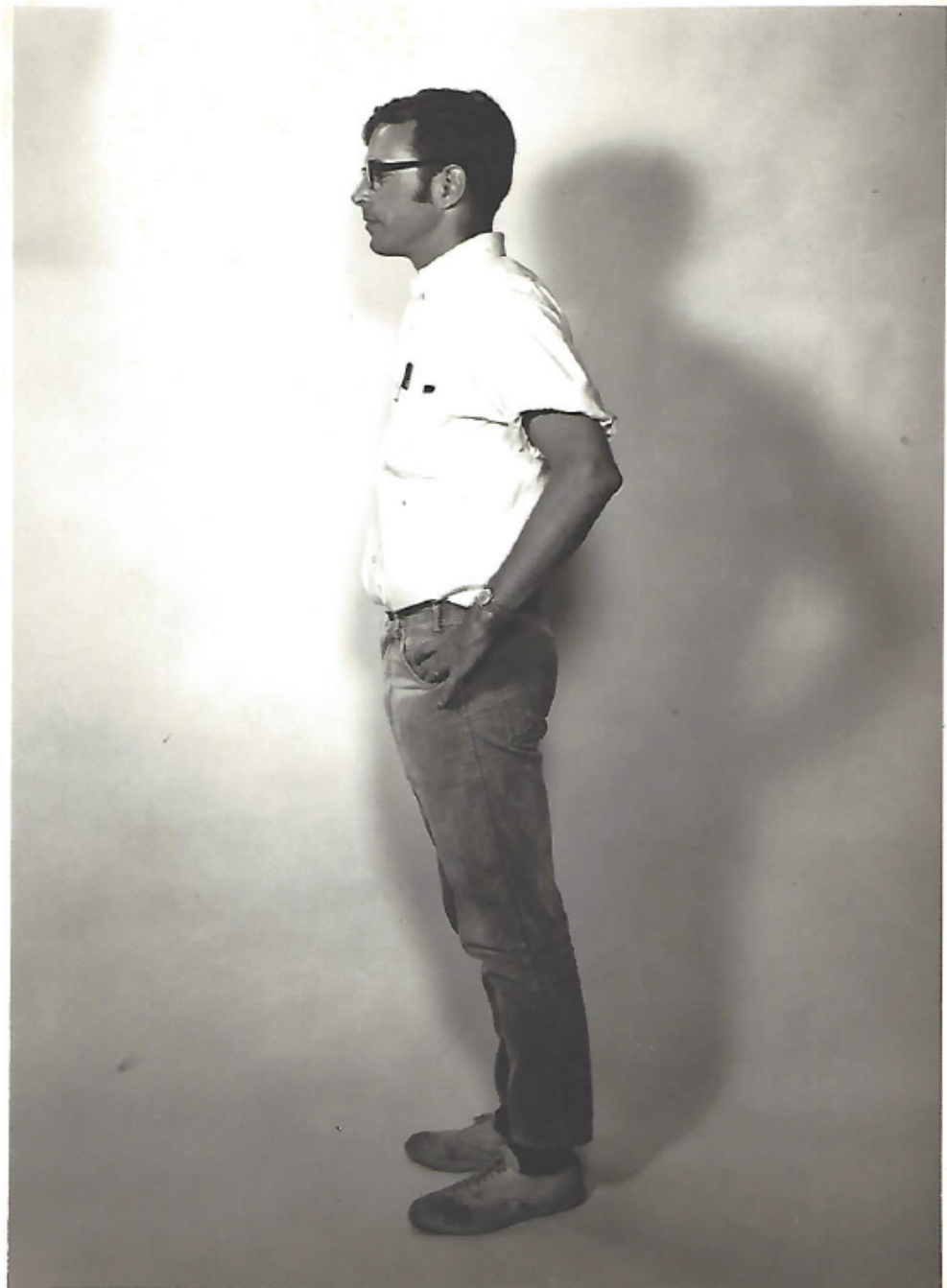
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3186 A photo taken by Willem Malkus during the summer of 1970 while I was
3187 lecturing at the GFD program. He took the photo at many angles, sliced it
3188 horizontally and recomposed the photo as Pedlosky, Rotating and Stratified; the title
3189 of my lecture course that summer.

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Chapter 8

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Woods Hole: A New Life

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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.

3215 The first evening we were in the house we had no furniture except a borrowed
3216 card table and a couple of borrowed cots but we all immediately walked down the
3217 road near our house to the beach and stood watching a starry sky over the water and
3218 feeling we had arrived at our real home.

3219 When our goods from Chicago arrived Holly and I worked strenuously to turn
3220 the house into a comfortable place for all of us. Holly was delighted to have an area
3221 around the house for a garden, to feel physically secure and to enjoy the natural
3222 setting and I had to admit, as much as I enjoyed aspects of life in Chicago I, too, felt
3223 more relaxed. We had at least two heavy locks on our front door in Chicago, always
3224 bolted. Shortly after we arrived in Woods Hole I returned home one day from work
3225 to find the entry door we used to be locked and I grumbled,“ who locked the damned
3226 door?” We did learn doors needed to be locked but there was no comparison to the
3227 background fears for our safety as before.

3228 I had a pleasant but small office in Woods Hole but when Bill Von Arx’s
3229 office became available it was offered to me and I have been in that corner office
3230 with a view of woods and sea for over three decades as I write.

3231 Part of the pleasure of being at the Oceanographic Institution was in the large
3232 number of colleagues who shared my interest in oceanography. The Physical
3233 Oceanography department in those days of the late 1970’s still had the personality of
3234 an earlier epoch of oceanography. There were quite a number of established
3235 scientists in the department who had never studied the fluid dynamics of the ocean in
3236 any systematic way. In fact, there were a substantial number who had no graduate
3237 education at all, or if they did, it had nothing to do with science and certainly not

3238 fluid mechanics. They were a colorful group. Fritz Fuglister, for example, had been
3239 an artist who came to Woods Hole during the Depression to do some post office
3240 murals as part of the WPA project. He fell in love with a librarian at the nearby
3241 Falmouth Public Library. He stayed and became a draftsman at the lab and started
3242 going to sea as a technician and gradually, on the basis of his basic intellect, rose to
3243 become a scientist and eventual department chairman. Similarly, Valentine (Val)
3244 Worthington had only an undergraduate degree in the classics but on the basis of his
3245 observational work became an influential figure in describing the North Atlantic
3246 gyre circulation. He, too, became department chairman and was chairman when I
3247 arrived. He was a very good and understanding administrator and although he was
3248 emphatic in telling me that Newton's Laws of Motion should not constrain his
3249 conceptual picture of the ocean circulation, we got along very well. There were
3250 several others as well and they made up a small club they called SOSO (Society of
3251 Sub-professional Oceanographers). Stommel himself never proceeded further than
3252 his undergraduate degree from Yale in Astronomy and was a member of the SOSO
3253 community and this didn't prevent him from being the leading figure in Physical
3254 Oceanography. He did receive an honorary Ph.D. from the University of Chicago
3255 while I was there and I had the pleasure and honor of shepherding him through the
3256 ceremonies. The increasing professionalization of oceanography has closed down the
3257 club as the members passed away. That professionalization is certainly a sign of
3258 some progress but one cannot but feel regret that a certain colorful flavor of those
3259 days has been lost.

3260 At first my work was a continuation of my studies of the nonlinear baroclinic
3261 instability problem. Although these were still the pre-personal computer days, there
3262 was an easy public terminal to link to the central (VAX) computer for the Institution
3263 and with online editing now standard I was able to do some problems much more
3264 easily. One of the first problems I considered after I arrived was the effect of the
3265 planetary vorticity gradient, the so-called β effect on the generation of chaos. I
3266 discovered that even a small amount of that effect, i.e. even when its contribution
3267 was small compared to the potential vorticity gradient due to the vertical shear of the
3268 zonal flow in the two-layer system, eventually expunged the chaotic behavior. This
3269 rather dramatic effect was due to the role β played on the evolving phase of the wave
3270 so that the amplitude equation was no longer strictly real but had an imaginary part.
3271 Looking at the evolution in the phase plane, this introduced a term like an angular
3272 momentum effect on a particle orbiting a body while attracted and repulsed by a
3273 nonlinear spring. The angular momentum term prevented the orbiting body, i.e. the
3274 solution curve, from approaching too closely to the unstable origin, the source of the
3275 instability and the chaotic behavior.

3276 I also was able to examine the instability of resonant topographic waves that
3277 Charney and DeVore had recently discovered using a heuristic truncated Fourier
3278 expansion. I was able to extend and strengthen their analysis using asymptotic
3279 methods. Still, my work had not yet taken on a new flavor reflecting my new
3280 scientific home. I enjoyed greatly my Woods Hole colleagues and the new focus on
3281 oceanography. The weekly Physical Oceanography (PO) seminars were of generally
3282 high quality. I was also teaching in the MIT/WHOI Joint Program and I enjoyed the

3283 interaction with my MIT colleagues, especially Carl Wunsch, Glenn Flierl and Paola
3284 Rizzoli. Although I enjoyed the teaching very much, the course offerings to our
3285 students struck me as incoherent. We had no basic fluid mechanics sequence and our
3286 students had to chose fluids courses in other departments that, of course, emphasized
3287 aspects of fluid dynamics special to those disciplines rather than our own. I initiated
3288 a series of meetings of the WHOI /MIT faculty and, as at Chicago, we began a
3289 discussion of what a core program of courses should look like. I had my own ideas
3290 but my principal goal was to get us to agree on the concept of a stable core whose
3291 content would not depend on who was teaching the course. In that way people
3292 teaching more advanced and specialized courses would know what the students had
3293 seen and supposedly knew from their core courses. There was general agreement as
3294 to the principle of the idea. I advanced a draft of a sequence of 6 courses and a rough
3295 outline of their contents. As anyone who has been in such a position will realize, the
3296 person who submits a fairly complete draft generally easily gets agreement on 80%
3297 of what is suggested. It was my strategy to accept all modifications that were
3298 proposed as long as there was substantial agreement and after a year of meeting we
3299 had reached a consensus as to the core and its content. Naturally, over the more than
3300 3 decades since that time there have been some substantial changes. More recently
3301 there has been some, sadly, unilateral changes in a few of the core courses by some
3302 of the colleagues at MIT that I felt de-emphasized the systematic development of the
3303 fundamentals of the dynamics in an effort to more quickly bring the student to the
3304 stage of doing research. No human construct lasts forever.

3305 One of the great pleasures of teaching in the Joint Program was the
3306 opportunity to interact with such very good students. Over the years there were a
3307 number of excellent students each of whom were special in their own way. Perhaps
3308 one of the most interesting was the first Ph.D. student to work with me for her thesis
3309 in Woods Hole, Lynne Talley. Lynn, an accomplished pianist had to choose between
3310 science and music for her career after graduating from Oberlin since she was
3311 outstanding in both areas. She chose oceanography and quickly developed and
3312 interest in *observational* oceanography. That was what was so fascinating about her
3313 choice to ask me to be her thesis advisor. She told me she was quite certain she
3314 wanted to do observational work for a career so she wanted the experience first of
3315 doing a theoretical research project for her doctor's degree. She did a fine job, wrote
3316 and excellent thesis and is now a leading figure in observational oceanography. The
3317 list of young men and women who worked with me contains many names and each
3318 contributed something rare and positive to my own intellectual development.

3319 The direction of my research work changed dramatically in 1980. At one of
3320 the PO seminars, Peter Rhines, recently returned from a sabbatical year in
3321 Cambridge England, announced that he was going to give a lecture only reviewing
3322 the extant Sverdrup theory for the mid-ocean flow since he did not have a computer
3323 available to him in Cambridge for new work for his research. This was a sly and
3324 humorously disingenuous introduction. I, and I supposed my other colleagues,
3325 settled back to hear what we were sure would be an interesting review of standard
3326 material by one of the most innovative people in our field. It started that way but
3327 quite soon turned into a presentation of Peter's new work, which he had done with

3328 his exceptional student Bill Young, on the problem of the oceanic thermocline. Any
3329 notion that the lecture was covering old material vanished and we were presented
3330 with some really novel ideas about the ocean circulation.

3331 I think it was Norman Phillips who, a few years earlier, pointed out to me the
3332 conundrum in understanding the nature of the oceanic thermocline and the wind
3333 driven circulation of a density stratified ocean. On the one hand, what is it that limits
3334 the depth of the warm water region to something of the order of 1 kilometer while
3335 the ocean has a depth of 4 or 5 kilometers and there has been millions of years for
3336 the surface heating in the subtropics to reach the bottom by simple molecular
3337 diffusion? The answer to that was rather simple, i.e. that the process of producing a
3338 thermocline is dynamic and fluid motions sweep the fluid into the western boundary
3339 layer long before the heated fluid could extend to the full depth. The opposite
3340 problem was less commented upon. Namely, how did the heating and the wind
3341 driven motion of the warm water extend in depth *beyond* the layer directly affected
3342 by the wind, a region of the order of only 100 meters. The classical but incomplete
3343 thermocline theory envisioned it as a balance between the downward diffusion of
3344 heat balanced by a combination of vertical advection of deep, cold water, and the
3345 lateral movement and removal of the warm water by horizontal advection. In this
3346 picture the thermocline was a type of thermal boundary layer below the sea surface
3347 whose thickness depended on the turbulent vertical mixing coefficient of heat and
3348 the three-dimensional motion field. This led to a fiendishly difficult nonlinear partial
3349 differential equation. There were few useful solutions of the equation and they were
3350 typically of the type that required guessing a form of the solution which was

3351 structurally the same everywhere and only stretched or compressed in the vertical as
3352 a function of latitude and longitude. Worse yet, the solution could only satisfy very
3353 unrealistic surface boundary conditions. In particular, these similarity solutions
3354 could not deal with the simplest and most obvious case where the surface boundary
3355 condition considered a surface applied temperature that was a function of latitude
3356 only. These solutions also depended critically and unhealthily on the unknown
3357 turbulent mixing coefficient of heat. One approach that avoided a dependence on the
3358 vertical mixing coefficient had been suggested by Pierre Welander, one of the most
3359 innovative workers in physical oceanography. He abandoned the thermal boundary
3360 layer analogy completely and searched for solutions for a completely adiabatic and
3361 frictionless thermocline. It still required some guesswork from Pierre because the
3362 adiabatic solution preserved potential vorticity along streamlines and that
3363 relationship between the streamlines and the potential vorticity should, in principal,
3364 be derived from the surface boundary conditions and not specified arbitrarily as
3365 Pierre did. Nevertheless, Pierre's solution showed that an adiabatic solution could be
3366 found that had many realistic features and did not depend on an artificial and
3367 unknown mixing coefficient.

3368 The theory of Rhines and Young took a completely different approach. They
3369 considered an ocean of many layers, each of different density, in which the frictional
3370 coupling between the layers was so small as to be directly negligible. The wind
3371 forcing would put the uppermost layer into motion. That motion would alter the
3372 thickness of that layer. Sufficient driving would so distort the isolines of potential
3373 vorticity which are lines on which the ratio of the Coriolis parameter, $2\Omega \sin \theta$ (Ω is

3374 the earth's rotation rate, θ is the latitude) to the layer thickness h_n of the n^{th} layer. If
3375 only the first layer is in motion one can easily calculate how the depth of the first
3376 layer and hence how the potential vorticity of the layer beneath it is altered since *its*
3377 thickness will thus be altered. If the forcing is weak the isolines are nearly latitude
3378 circles and the intersection of the isolines with the eastern boundary disallows
3379 motion on those lines that is not directly wind-driven. If, however, those lines are
3380 sufficiently distorted and close on themselves even the smallest driving, say by a
3381 synoptic eddy field, could give rise to an order one circulation. Once that layer is put
3382 into motion, *its* motion will distort the thickness of the third layer and, if distorted
3383 enough, can set that into motion. Rhines and Young showed how to do the
3384 calculation and, importantly, showed how these domains would become nested one
3385 inside the other as one moved lower in the water column. In this ingenious way, they
3386 proposed how an adiabatic, nearly frictionless, fluid could have many deeper layers
3387 put into motion. In each of the deeper layers Rhines and Young appealed to a
3388 theorem of George Batchelor to infer that the potential vorticity within those deeper
3389 nested regions needs to be constant and this allowed a specification of the velocity
3390 field in those regions. All this was done without the need to specify a mixing
3391 coefficient. I remember that the audience in the seminar was deeply impressed by the
3392 Rhines and Young suggested solution of the thermocline problem. More to point, the
3393 elements of that theory were so elegant that the natural reaction was that we all
3394 wished we had done it. As Hank and I rose to leave the seminar room Hank
3395 expressed his admiration for their work and I remember agreeing but pointing out to
3396 Hank that a key missing element of their theory was the absence of outcropping of

3397 their layers since they used a quasi-geostrophic theory that disallows such strong
3398 changes in layer depth. I believe that was the beginning of our germ of an idea for
3399 the ventilated thermocline.

3400 Still, I believe the truly major contribution that Rhines and Young made was
3401 to restore the problem of the thermocline to the domain of fluid mechanics and not
3402 just the search for precious and ingenious special solutions of a hard partial
3403 differential equation. I am still impressed by the originality of their suggested
3404 solution.

3405 I put the problem out of my mind as I was working on a stability problem I
3406 was interested in when, a couple of weeks later, Hank came to my office to show me
3407 some numerical work he and Jim Luyten, who functioned as a kind of junior
3408 colleague to Hank, had been doing. They took the two-layer model that Rhines and
3409 Young were using and allowed the second layer's interface with the upper layer to
3410 come to the surface within the subtropical gyre. I can no longer remember what
3411 numerical routine they used but the results looked interesting and realistic. Hank left
3412 the office and I started to think about whether there might be a more direct way of
3413 getting an analytic solution. I fiddled with set of simple sketches showing a column
3414 of fluid in the second layer being swept beneath the upper layer, subsequently
3415 preserving its pv. I can only say that it was natural for me to think in terms of the
3416 conservation of potential vorticity and its relation to the streamfunction of the flow.
3417 My mentor Jule Charney had developed his theory for the Gulf Stream using just
3418 such a technique and I had not long before used the same approach to the nonlinear
3419 upwelling problem. The principal challenge was determining just what the

3420 relationship was between the lines of constant pressure, that were also the
3421 streamlines, and the potential vorticity isolines that coincide with them in the ideal
3422 fluid theory. It suddenly occurred to me that if the outcrop line were a latitude circle,
3423 precisely the case that had eluded analysis by the similarity/guess solutions, it would
3424 be possible to find an extraordinarily simple relation between the potential vorticity
3425 of the second layer and its pressure field. It was *so* simple that I was astonished that
3426 this fundamental problem of the relation between potential vorticity and the pressure
3427 field in the thermocline *had* such a simple solution. This, in turn, allowed an
3428 extremely simple solution of the whole thermocline problem for the two-layer model
3429 with outcropping. Nothing more than high-school algebra was needed! I quickly
3430 wrote up the results and left them on Hank's desk. He was not in his office and I
3431 looked forward to his reaction. He called later that afternoon and left a message on
3432 my home phone that remarked that the theory must be wrong because it produced an
3433 algebraic dependence in latitude via the Coriolis parameter whereas the numerical
3434 solution appeared to have an exponential behavior.

3435 I couldn't figure out how the theory, being so simple, could be so drastically
3436 wrong, and looked forward to learning more the next day. I didn't have to wait for
3437 more than just a few hours because later I got another call from Hank excitedly
3438 saying that he and Jim had plotted their numerical results over the predictions of the
3439 theory. They appeared to match exactly! Could we talk and could I please explain
3440 the how the theory worked? I still remember the thrill of that evening. It was the
3441 beginning of one of the most exciting scientific periods of my life. We had really

3442 gotten a deep understanding of the inertial thermocline and our theory was so simple
3443 that it could form the solid basis for further extensions and elaborations.

3444 I believe that on one level, Hank was actually disappointed that an analytical
3445 model could work so easily. He often felt that doing work that required the computer
3446 was a mark of real professionalism and despite the fact that he was clearly
3447 recognized as the most productive and creative physical oceanographer of his time,
3448 or perhaps of all time, he sometimes felt that lack of a formal Ph.D. credential. So,
3449 he kept challenging our analytical solution and this turned out to be very stimulating.
3450 First he asked why we couldn't have a moving layer with a non-zero depth on the
3451 eastern boundary. In our initial model the layer interfaces each rise to the surface to
3452 satisfy the eastern boundary condition. It turned out not to be difficult and it led to
3453 the presence of a special region, the so-called Shadow Zone, in which the lower
3454 layer flow was at rest separated from the region of moving subducted flow to the
3455 west of the Shadow Zone boundary. I was secretly amused to use the word
3456 subduction to describe the process by which the Ekman pumping at the surface
3457 drove fluid down into the thermocline. I borrowed the word from the seafloor
3458 spreading and mantle convection work I had done with Frank Richter at Chicago.
3459 The term was naturally accepted in oceanography without a murmur.

3460 In addition to the Shadow Zone it was clear that one of the streamlines issued
3461 from the intersection of the outcrop line and the *western* boundary if the outcrop line
3462 was near enough to the zero line of the Ekman pumping and this delineated a region
3463 that could not be reached by any fluid subducted from the outcrop. Fluid within that
3464 contour seemed to be emanating directly from the western boundary. We therefore

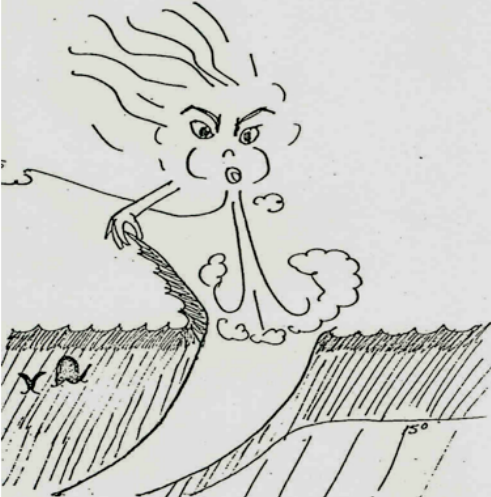
3465 proposed on physical grounds that the fluid, recirculating through a western
3466 boundary current (which was not part of our solution) would, a la Rhines and Young,
3467 evolve to have uniform potential vorticity. One thing that pleased me enormously
3468 about our solution was that it was *not* a similarity solution. The structure of the
3469 solution was different in each of the three regions of Subducted fluid, the Shadow
3470 Zone and the Pool of Potential Vorticity (which had a sort of New Testament ring to
3471 my ears) and put a final stake in the heart of the special solutions that had been
3472 found of the previous theories that assumed self-similarity in all regions.

3473 Hank's final challenge was his doubt that the analytical solution could go
3474 beyond the two-layer model and so I worked out the rather tedious three-layer
3475 model. The three-layer model naturally generated more special regions and I need
3476 not describe them here but it was clear that there was no limit, in principle, to the
3477 number of layers we could use and so approach a continuous model of the
3478 thermocline. In fact, our colleague, Rui Xin Huang, shortly afterward developed an
3479 ingenious computer version of the analytic layer model that could increasingly refine
3480 the vertical resolution with an enormous increase in the number of layers.


3481 At about this time Hank wandered down to my office and said with a ring of
3482 great satisfaction in his voice, "No one will ever think about the thermocline the
3483 same way again". It was not said boastfully; it was more in the nature of his
3484 measure of satisfaction that we had done a very fundamental piece of work that
3485 moved the field in a new direction.

3486 We sent our paper off to be published. Hank got considerable pleasure that we
3487 listed the authors alphabetically. Jim Luyten was up for promotion to Senior

3488 Scientist and being listed as first author on the paper would certainly help his case.
3489 Hank had a strong, rather paternal affection for Jim and at that moment we all felt
3490 warmly about each other. The Christmas holidays were approaching and we decided
3491 to give a public lecture about our work. Nancy Copley designed an announcement
3492 with a cloud-like figure pulling away the surface of the ocean while puffing a blast
3493 of air into “The Ventilated Thermocline”, the title of our paper.



A REVIVAL OF THE ANNUAL
CHRISTMAS LECTURE IN
GEOPHYSICAL FLUID DYNAMICS



“THE VENTILATED THERMOCLINE”

PRESENTED BY

JOSEPH PEDLOSKY

SENIOR SCIENTIST AND
DOHERTY PROFESSOR OF OCEANOGRAPHY
WOODS HOLE OCEANOGRAPHIC INSTITUTION

18 DECEMBER

4: 00 PM REDFIELD AUDITORIUM

COFFEE AT 3:30 PM

COCKTAILS AND DINNER* AT ENDEAVOUR HOUSE
FOLLOWING THE LECTURE

PLEASE MAKE RESERVATIONS WITH RAY SCHMITT*
W.H.O.I., 548-1400 EXT. 2426

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3498 A local production of Amahl and the Night Visitors allowed Jim, Hank and I

3499 to put on crowns I had borrowed from a participant in the opera and, to the musical

3500 score of “We Three Kings of Orient Are”, we were able to add an encore to the

3501 seminar in verse:

3502 We three kings of geostrophy are

3503 Tracing streamlines that travel far,

3504 The wind-driven gyre, (but not quite entire)

3505 Following yonder isobar

3506 (chorus)

3507 Oh, Oh, isobar of ideal lubricity, (Joe)

3508 Isobar of potential vorticity, (Jim)

3509 Westward swerving, still conserving (All)

3510 Guide us to beta helicity. (Hank)

3511



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The three Kings of Geostrophy

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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.

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3520

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

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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.

3524 Hank sent preprints of the paper to a number of colleagues in the US and
3525 abroad. Sometime after he did that I got an early Sunday morning telephone call
3526 from Hank who asked in an agitated tone of voice if I had seen the most recent issue
3527 of Ocean Modeling, a gray literature journal put together by Peter Killworth. I
3528 replied that I hadn't since my copy was sent to my office and not my home as
3529 Hank's was. I said I had not and he replied I should see it right away and that he
3530 would be right over. A few minutes later he was at the kitchen door in a rather
3531 agitated state. I was still in my pajamas and invited him in for a coffee and he
3532 replied, "No, no, you please take care of this" and thrust his copy of Ocean Modeling
3533 into my hands. What had disturbed Hank was the lead article in Ocean Modeling in
3534 which Killworth, its editor, attacked our solution with the Shadow Zone and claimed
3535 that any solution with a non-zero eastern boundary layer thickness for a moving
3536 layer had to be wrong. He then presented an ingenious argument. Expanding the
3537 solution in a Taylor Series from the eastern boundary and using the equations of
3538 motion he could show that each term in the Taylor Series in that layer had to be zero
3539 if the depth on the eastern wall was non-zero. When I first read the article my heart
3540 pounded in the anxiety that we might have made a brutal mistake although I couldn't
3541 understand where that might be. Then it struck me that the Taylor Series expansion
3542 was only valid up to the first singularity of the function and/or its derivatives. Of
3543 course, the Shadow Zone boundary was the line separating resting from moving
3544 fluid so at that boundary the layer thicknesses suffered a discontinuity in their first
3545 derivatives. So what Killworth had done was only prove precisely that the Shadow

3546 Zone had to be at rest! This had been already clear from physical considerations. I
3547 called Hank with the reassuring news.

3548 Hank's first reaction to the news was relief and then he became angrier than I
3549 have ever seen him before or afterwards. The fact that Killworth rushed his criticism
3550 into print without first contacting us, especially as his criticism was unfounded,
3551 struck him as unethical. He wrote a letter to Killworth that Jim and I co-signed, and
3552 his anger turned to outrage when Killworth suggested we reply by writing a letter to
3553 his journal as part of the correspondence dealing with the "controversy", as
3554 Killworth called it. Hank was so angry he sputtered that he would be damned if he
3555 would play into that and wrote to Killworth that there was no controversy only
3556 Killworth's mistaken criticism.

3557 The theory has held up well for 35 years to this point but the lesson learned
3558 again is that *nothing is easy*.

3559 One result of working so closely with Hank was the pleasure of drawing closer
3560 to him. It was natural for him to strike up friendships and deepen them through
3561 working together. He became increasingly candid to me about his opinions about
3562 other people and some of these opinions astonished me.

3563 When Hank turned 60 in 1980, he was feted by a wonderful party in Falmouth
3564 and the publication of 3 volumes of essays and reminiscences by his colleagues. It
3565 was a happy joyous moment and many colleagues from around country came to
3566 participate. Among them was Jule Charney who stayed over that weekend at our
3567 home. Jule was in apparent remission, after surgery, from a serious lung cancer and
3568 was still suffering the debilitating effects of the chemotherapy that he had recently

3569 undergone. There was so much affection shown for Hank at the party that I
3570 wondered subsequently whether Jule was annoyed that his own 60th birthday had
3571 passed largely unrecognized three years before.

3572 During this period of his remission Jule was often in his MIT office continuing
3573 his research and I recall that once he gestured towards some correspondence on his
3574 desk and said with a note of bemused resignation in his voice that after he received
3575 his cancer diagnosis he was determined to focus on only important things in life.
3576 “Now that I seem to be well again”, he said, “ I find myself going back to doing the
3577 same damn routine things that took so much of my time before”. Of course I was
3578 delighted that Jule seemed to be over his bout of the disease, which only illustrates
3579 my ignorance of the nature of the disease. I would learn more about that later.

3580 Ted Shepherd was Jule’s last Ph.D. student and during the spring of 1981 his
3581 thesis defense was scheduled at MIT. I was also on Ted’s thesis committee and so
3582 had travelled up to MIT to participate in the exam. Ted was an exceptionally gifted
3583 student and there was going to be no problem defending his work but it was an
3584 important rite of passage. Ted and each member of his committee gathered in a
3585 classroom near Jule’s office at the appointed time that I believe was in the morning.

3586 That is, all of us except Jule, his advisor. It was not unusual for Jule to be late
3587 but as time grew later and later we developed some concern that something was
3588 amiss. I stepped out into the hallway with the intention of telephoning Jule to see if
3589 everything was OK. As I entered the hallway I saw Ed Lorenz in the hallway and he
3590 beckoned to me and said that Jule was on the phone and wanted to talk to me.
3591 Thinking that there was some minor reason of his delay I affected a humorous tone

3592 of voice and asked, “ Jule, what’s cooking”. His chilling reply was, “ My fever is
3593 cooking”.

3594 Jule was hospitalized right away and was in very bad shape. We spoke on the
3595 phone but he was adamantly resistant to the idea of a visit. I wish I had visited
3596 anyway because he did not hold on much longer. However, during that period after
3597 the resurgence of the cancer until his death, Jule was deeply troubled by one thing
3598 and we had several lengthy phone conversations about it. In the volume, Evolution
3599 of Physical Oceanography honoring Hank on his 60th birthday, there was an essay
3600 written by George Veronis that deeply offended Jule. Aside from a gratuitous remark
3601 about Jule’s forgetfulness that might have possibly put Hank’s draft of his book, The
3602 Gulf Stream, in peril, he also implied that the basic idea for the inertial theory of the
3603 Gulf Stream, one of Charney’s most significant oceanographic contributions, was in
3604 fact, Hank’s idea. Jule in his last days was tortured by the idea that he had been
3605 portrayed as a computational assistant to Hank whom, Veronis said, had suggested
3606 the problem to Jule and another mathematician George Morgan at Brown. He talked
3607 to me heatedly about what he considered to be a slur in print and was deeply
3608 troubled.

3609 I have to say I was astonished. It is true that Veronis’ remarks were ungenerous
3610 (Charney was another scientist on George’s enemies list) but given the immense and
3611 magnificent nature of Jule’s contributions I couldn’t understand his focusing on this
3612 one paper on his deathbed. I have not yet had to face the experience of approaching
3613 death myself so I suppose I am not in a position to make that judgment but I was
3614 troubled that Jule was mentally so suffering. Jule’s Gulf Stream paper on the subject

3615 concluded with a warm and generous acknowledgement of the conversations he had
3616 had with Hank about the problem and how it could be said that “the problem solved
3617 itself” as a consequence. I was always stirred by that acknowledgment picturing two
3618 of my heroes linked in a heroic scientific friendship. “So”, I thought to myself, “
3619 this is easy”. I would go to Hank, describe Jule’s agony and ask him to talk to Jule
3620 and put his mind to rest on the subject. When I broached the issue with Hank I was
3621 astonished by his response. “ I won’t go”, he said, “it won’t do any good”. I then
3622 found myself in the middle between them carrying messages back and forth and I
3623 became deeply depressed by the whole business. It became clear that for all these
3624 years since Jule’s publication of his Gulf Stream paper in 1955, there had been a
3625 bitter falling out between the two of them. Hank thought he had contributed so much
3626 to the idea that he should have been asked to be listed as a co-author of the paper and
3627 Jule was equally adamant that the idea of a frictionless current was his quite
3628 independently. Hank had, in fact, tried to construct a frictionless model of the current
3629 but was limited to a constant potential vorticity model that was not able to close the
3630 mid-ocean flow through the boundary current; a key ingredient in the theory. It was
3631 also clear that Jule’s mathematical approach was his own and to what extent Hank’s
3632 previous effort suggested that attempt is something we will never know for sure
3633 now. Carrying messages back and forth and attempting a diplomatic rapprochement
3634 was more than I could accomplish. I was in the end astounded that my two heroes,
3635 indeed quite justifiably considered scientifically heroic figures, had lived all that
3636 time with such a feeling of bitterness between them. Since I was fond of and intimate
3637 with both of them I could only marvel how complicated the human psyche could be.

3638 Jule passed away shortly thereafter. He was only 64. It was for me like losing a
3639 second father. When I recall the effect Jule had on my career and my scientific
3640 development, one of his characteristics that impressed me the most was the
3641 generosity with which he supported the scientific efforts of his colleagues. When I
3642 first became aware of the work of Ed Lorenz, and this was before his work on
3643 deterministic chaos, I mentioned to Jule that I was concerned about Lorenz's use of
3644 arbitrarily truncated Fourier series to represent the solutions of complex nonlinear
3645 problems. Jule responded with warmth that I did not yet understand what an intuitive
3646 as well as mathematical prodigy Lorenz was. He emphatically maintained that
3647 Lorenz had the soul of an artist and that I should realize the great value of his
3648 contributions. Of course he was correct.

3649 Similarly, in the early sixties Akio Arakawa had developed an ingenious way of
3650 representing numerically the advection of vorticity in models of atmospheric flows
3651 so that important properties of the motion, such as energy and enstrophy[♦]would be
3652 conserved. It previously had been difficult to have the conservation properties of
3653 those quantities preserved in the numerical calculations leading to grievous errors
3654 that sorely limited their meteorological capabilities. Arakawa's technique heals that
3655 fundamental deficiency. After Arakawa had given his seminar outlining his new
3656 method, Jule turned to the seminar audience and praised Arakawa unstintingly
3657 remarking that he hoped we all understood what an important advance this was. Of
3658 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.

3659 For the next few years I worked on consequences and extensions of thermocline
3660 theory as well as some ideas on stability theory. But genuinely new directions were a
3661 few years off. In the meantime a new family adventure came to pass. As 1983 came
3662 to a close I felt the need for a sabbatical year away from Woods Hole and at first
3663 thought that another year in London might be enjoyable and scientifically profitable.
3664 My year in London in 1967-68 had been darkened by Carin's mental illness and the
3665 thought of enjoying the city with a healthy family was attractive. Fate intervened.
3666 One of my MIT colleagues, Professor Paola Rizzoli, mentioned that there was a
3667 small oceanographic lab in Venice, on the Grand Canal, where I would be a
3668 welcome visitor. I had been in Venice once before and had fallen in love with
3669 Venice, as who hasn't? It seemed like a provocative idea and Holly was game to try
3670 it. My idea was to use the sabbatical to do a second edition of my book, Geophysical
3671 Fluid Dynamics including new material (like the Ventilated Thermocline ideas) and
3672 to correct typographical errors in the first edition. I had also taken some Italian
3673 classes in the '60's in Boston when I was at MIT and the adventure sounded like a
3674 dream. A Guggenheim grant made the dream a reality and so in the fall of 1984
3675 Holly, Dove and I flew to Venice; Anna had preceded us by a month.
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3683 Photos: Family life and travels.

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3692 Holly 1970

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3699 Top: Holly at the beach in Woods Hole. Below: Holly, Anna and I in Woods
3700 Hole in 1974.

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3708 In our new home in Falmouth after moving to Woods Hole. Holly, Anna, Dove
3709 and Joe
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3712 My mother, Lillian, paying us a visit in the early '80's.



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3715 We were as happy as this picture shows. On our Saconesset beach around 1985.

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3723 Ca' Papadopoli on the Grand Canal, Venice where my visitor's office was for
3724 many years from 1984

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3736 Dove and Joe in 1984 from the Basilica of San Marco overlooking the Piazza

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3742 During one of our summer stays at Villa Cipressi in Varenna where Holly
3743 taught her photo workshops. The girl next to Holly is Dove's cousin Megan.
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As Holly films in Varenna, I try to be helpful and amuse the subject.

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3752 When Dove graduated from Boston University (with high honors) we were
3753 touched when Alberta Bianchini, Dove's pre-school teacher in Venice, and her
3754 husband, Memi, flew to Boston to be present at the ceremony.
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3760 One of the pleasures of the clarinet was playing in Falmouth's Town Band.
3761 Here I am shown with a more expert clarinetist, Joe Olivera. We were both in the
3762 first clarinet section but when the going got rough Joe O would cover my mistakes.
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Chapter 9

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Italy!

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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

3797 friends so that when we arrived in Venice in the autumn of that year we felt we were,
3798 to some degree, returning home.

3799 The two girls went to Venetian schools. Dove, who was 5 at the time, went to a
3800 “Scuola Materna”, a Venetian pre-school. The young girls had to wear white
3801 smocks, grembulie, that covered their clothes and Dove was very proud on the first
3802 day of school to be properly clothed for school, just a short walk from home. She
3803 was somewhat dismayed to realize everyone was speaking Italian but she quickly
3804 latched on to one of the teachers, Alberta Bianchini, who spoke English. Of course,
3805 after a matter of a few months Dove was doing fine in Italian but the transition
3806 wasn’t easy and on some evenings we could hear her in her room softly practicing
3807 one of the Italian songs she learned at school, “The serpent’s dance”. Alberta and her
3808 husband Domenico (Memi) became two of our closest friends in Venice and until
3809 their deaths remained important figures in the lives of all four of us.

3810 Anna, who was originally resistant to the idea of a year away from her
3811 American high school friends was persuaded to go to Italy a month before the rest of
3812 us and stay on the farm of a family who were connected to friends we had made in
3813 Falmouth, Rick and Paola Price. They had a daughter Anna’s age and the original
3814 idea was that the following year we would host their daughter, Elisa. Although that
3815 never came to pass we did become good friends with the family. We had visited the
3816 farm and family in Holly’s and my earlier visit in the spring and we liked each other
3817 immediately. I scored a hit with my primitive Italian gained from a few lessons with
3818 Paola that Holly and I took. When asked by some member of the family whether I
3819 would like to visit the burial place of Mussolini who came from that area, I replied to

3820 their evident approval that it was enough for me to know he was safely and
3821 permanently buried.

3822 When we were thinking about schools for Anna, who turned 15 the year of our
3823 move to Venice, it seemed that a Linguistic High School might be the most
3824 comfortable for her. In fact, Anna showed great facility in picking up the language.
3825 When we arrived to fetch her from the farm family, she was already fluent in Italian
3826 although her fluency was limited to the present tense. Her Italian only got better as
3827 time went on and she had a good year in Venice and considered, Amadea, the
3828 farmer's wife, her "Italian mother".

3829 Prior to the arriving in Venice I had received a very courteous letter from the
3830 Director of the lab asking me if I needed any special equipment or other material. I
3831 was touched by his hospitality and replied that since my goal was preparing a second
3832 edition of my book, I had need only of a good library, which I was certain the lab
3833 possessed. I was unaware that the Director was deeply unpopular among the
3834 scientific staff. The reasons were multitudinous and connected with the political
3835 situation in Italy, the threat of left and right wing terrorism and the Director was
3836 charged with keeping a lid on possible trouble in the lab. I never noticed anything
3837 that might have caused such concern although a few people were politically active
3838 on the left.

3839 The office I was assigned by the Director was a small office in one of the
3840 mezzanine floors of the palace. It was barren, cold in the winter, with a single small
3841 high window. Next-door was the office of a few technicians who appeared to spend

3842 their time loudly arguing, singing and occasionally drinking. Nothing bothered me. I
3843 felt I was in paradise.

3844 I made good friends with several of the scientific staff. Andrea Bergamasco
3845 became a great friend and the natural clarity of his Italian was a great help in my
3846 attempt at mastery of the language. He was also the person whose oceanographic
3847 interests were closest to mine. Another good friend I made was with Luigi (Gigi)
3848 Cavaleri who specialized in the measurements of wind-driven surface gravity waves.
3849 Stefano Zechetto who commuted from Vicenza daily was a particularly warm
3850 personality and we often visited him and his family at their home. The secretary for
3851 the lab was an American lady, Jane (Frankenfield) Zanin, married to an Italian and
3852 she was a great help to us in settling in.

3853 One of the rules for government laboratories was that they had to provide a
3854 cafeteria for mid-day meals. Since many of the labs, including the one in Venice,
3855 were in buildings that were unsuitable for a cafeteria, the employees were given
3856 tickets called “buoni” because they were good, for a small price (about the
3857 equivalent of a dollar in 1984), in some local restaurants where they were accepted
3858 as payment. That was my introduction to a very Italian activity: “beat the system”. I
3859 had asked whether I could join them when they went for lunch at the local restaurant,
3860 The Madonna, one of the best in Venice, especially for seafood. The reply was that
3861 they would have to check. At first I thought it would be up to the Director, but no, it
3862 was a decision made in Rome but I was told not to worry. The decision when it came
3863 was that I could not be given my own ticket book but I was told again that I was not
3864 to worry. There were many employees at the lab who also were in the same situation

3865 but everyone who wanted to dine at the Madonna could. It turned out that, in those
3866 days, the buoni were only good in restaurants; they couldn't be used for other
3867 purchases, not even food. So, several of the staff who went back to their homes for
3868 lunch had unused buoni and each day you could ask someone who was going home
3869 if you could use their buono and their name. With the buono and 1,900 lire (about a
3870 dollar in 1984) you could enjoy a four-course meal: first a pasta dish, then a main
3871 dish usually of meat, then a salad or other vegetable, a desert and espresso.
3872 Abundant wine was served with each meal. It was the task of the most recently hired
3873 junior scientist (that year it was Andrea) to keep track of the buoni to make sure they
3874 matched the number of diners and that the legitimate owner of each buono was
3875 correctly identified. So we "abusers" (abusivi) would turn our tickets in and respond
3876 to the surreal question, "Who are you today?" The meal was worth the daily hunt for
3877 a name that was "unused". It was great fun and the food was terrific.

3878 Again, the project of writing was deeply satisfying. There were sections of the
3879 book that needed rewriting and/or correcting. There were new sections I wanted to
3880 add, for example, a discussion of our new ideas of the thermocline.

3881 I worked each weekday but took advantage of the compactness of the city to do
3882 some sightseeing, either on the way to work or after work before heading home.
3883 From time to time I would meet Holly for lunch at one of the little restaurants we
3884 liked so much. The dollar was so strong that year that we felt comfortable dining out
3885 regularly. We spent a good deal of time together and enjoyed each other with a new
3886 sense of deeper intimacy.

3887 Holly had found a photography group in the Giudecca island in Venice and
3888 gradually developed plans to give photo workshops in Venice that became both a
3889 motive and a means for us to be able to come back regularly to the city we both had
3890 grown to love.

3891 The city is full of art and its easy accessibility was a delight. I remember one
3892 cold, cloudy day in the autumn I took a detour on my way home to our apartment in
3893 rio S.Felice in the Cannaregio section and went to the Frari church. The church is
3894 noted for its two paintings by Titian, especially its magnificent altarpiece showing
3895 the Assumption of the Virgin. Almost by accident I wandered into the Sacristy and
3896 discovered what became my favorite painting in Venice, a painting by Giovanni
3897 Bellini, one of his Sacra Conversazione. The painting glows and it has been in its
3898 current location for about 500 years. Although I am not a religious man I found the
3899 painting inspiring in its beauty and serenity.

3900 There were many such personal excursions. Holly used to tease me that I, a Jew,
3901 spent more time in Christian churches than she or anyone she knew. There were the
3902 parish churches of Tintoretto, Veronese; the Scuole or clubhouses of the various
3903 Confraternities that played such an important role in historical Venice, and, of
3904 course, sites like the Doges' palace and the Basilica of St. Marco. The list is nearly
3905 endless. The art museum, the Accademia, was an especial delight, especially the
3906 rooms with the Carpaccio paintings of the story of Saint Ursula in which her home in
3907 Brittany took on the flavor of a dream-like Venice.

3908 And, there was Vini da Gigio. It was a neighborhood *osteria*, a bar and a
3909 rudimentary restaurant, but the new owners, Paolo and Laura Lazzari, brother and

3910 sister, were determined to improve it and Holly and I found the atmosphere
3911 welcoming, the food good and the prices reasonable. Since the restaurant was just
3912 down the street from our apartment in Rio S. Felice, it became our favorite. That
3913 winter in Venice was an unusually cold one and the heating system in our apartment
3914 failed and it was some time before it could be fixed. One afternoon when the girls
3915 were at school Holly and I decided to go for lunch at Gigio's and explained our
3916 plight to Paolo. His reaction was immediate; "our kitchen warms the restaurant" and
3917 then indicated the electric heaters then used to heat the dining room. "Take the
3918 heaters and return them when your "riscaldamento" is fixed! It seems in retrospect
3919 that similar experiences of friendliness and generosity occurred regularly. The
3920 restaurant remains my favorite in Venice.

3921 We made friends with an older couple, Ernesto and Rosa, who ran the small
3922 outdoor fruit and vegetable shop near our house. We each developed a fondness for
3923 certain coffee shops (and the coffee). It was a splendid year.

3924 My life at the lab was equally magical. Housed in a beautiful palace on the
3925 Grand Canal, Ca` Papadopoli, it is now a luxury hotel. In spite of the cramped nature
3926 of my office I enjoyed my time there largely because of the friendly staff.
3927 Remembering the tactic that Charles Keeling used in Stockholm, I asked the Italian
3928 staff to speak to me only in Italian. They then asked me if I wished to be corrected
3929 when I made language mistakes and I assured them I did. As a consequence my
3930 Italian rapidly improved and that added to my enjoyment of my time at the CNR lab.
3931 I believe the other people in the lab were aware of the undesirability of my office but
3932 the fact that I had embraced it without complaint stood me in good stead. Indeed,

3933 one day a large group burst into my office and started singing. The reason? It was
3934 my Saint's name day and required celebration!

3935 As the year wore on we began to travel in Italy. I went numerous times to
3936 Bologna to visit the meteorology department there. We also went as a family to Pisa,
3937 Florence, Siena and, of course Rome. Towards the end of our year I was invited to
3938 the oceanographic lab at La Spezia and we took that opportunity to visit Liguria and
3939 Cinque Terre. Everywhere we went we were overwhelmed with the beauty of the
3940 countryside and the art and architecture of Italy.

3941 During the year Holly and I took very inexpensive courses in Italian for
3942 foreigners taught at the Venetian University and that helped our Italian and it was
3943 something that was fun to do together especially as we were normally the oldest
3944 folks in the class. We discovered that the younger foreigners had a less positive view
3945 of Venice and we grew to believe that the fact that we were a family and that our
3946 daughters were particularly winsome certainly helped us be accepted as Venetians
3947 "in training" rather than visiting strangers.

3948 One day in April of that year (1985) when I entered the apartment after walking
3949 home from the lab, Anna met me in an excited state. "Daddy", she said, "Jerry
3950 Namias called and said to tell you that you have been elected to the National
3951 Academy!" This was a moment that put the cherry on the sundae of the year that we
3952 were enjoying. I had watched a few of my colleagues like Peter Rhines and Carl
3953 Wunsch attain membership in the National Academy of Sciences and although I
3954 easily recognized the merit of their elections, I had to confess to a feeling of envy
3955 and disappointment that I had not been. So when I got the news from Anna and I

3956 relayed it to Holly when she came home, I told it to her with the phrase, “Finally!
3957 Now I don’t have to think about it anymore”. I think one is supposed to modestly
3958 express surprise at such news but I felt it more as a relief and a validation than
3959 anything else.

3960 Then, as the anxious person that I am, I began to wonder if it were someone’s
3961 idea of a joke but was reassured when a congratulatory telegram from my friend Carl
3962 Wunsch arrived. Holly told all our acquaintances in Venice and I was so deeply
3963 moved by her happiness and pride in my good fortune. The year seemed filled with
3964 golden experiences and we bonded so firmly with Venice and its people that it was
3965 the beginning of a lasting relationship with Italy. In subsequent years we would go
3966 back to Venice for at least a month, sometimes for the whole summer. Holly began
3967 teaching photography workshops for Americans, mostly middle-aged women,
3968 through the International Center for Photography in New York. The program helped
3969 pay the rent for the summer and gave both of us a structure to our summers spent in
3970 Venice.

3971 In 1988 I was invited to give a lecture at a summer school run under the
3972 auspices of the Italian Physics Society and its Enrico Fermi program. The course was
3973 held in the lovely Villa Monastero in Varenna on the eastern shore of Lake Como. I
3974 had been invited for the full two weeks of the school but I was reluctant to leave
3975 Venice for such a lengthy period. When Holly implored me to accept for the full
3976 period I questioned why should we give up the marvels of Venice for a vacation spot
3977 on the water given that we lived full time in just that environment in Woods Hole? I
3978 agreed to go for a shorter period. It was one of the stupidest decisions of the many I

3979 have made. When we got to Varenna and saw just how beautiful it was I sheepishly
3980 had to apologize for my insistence of a short visit.

3981 Holly discovered an equally beautiful villa next door, also along the lake,
3982 named Villa Cipressi for its many Cyprus trees. She quickly learned that the villa
3983 belonged to the town and that she could rent rooms in the villa for workshop
3984 students and from then on until her death in 2012 we would go to Varenna every
3985 other summer from our base in Venice until Varenna, as Venice, became a second
3986 home to us. We became friends with the locals and appreciated the local beauty and
3987 culture as a supplement to the beauty of Venice.

3988 Of course other cities of Italy attracted our attention and were visited as well
3989 and as our familiarity with Italy grew so did our understanding of the subtleties of
3990 Italian life in the public and private spheres. The former was frequently
3991 dysfunctional while the latter consisted of many tactics invented to circumvent the
3992 obstacles of the former. As an observer it was frequently amusing to experience
3993 both, while for inhabitants it was often exasperating but accepted as immutable.

3994 We returned to Woods Hole and Falmouth with a better sense of our place in the
3995 world and our daughters returned with a view of the world deeper than most children
3996 their ages. For Dove, our younger daughter, the return had temporarily traumatic
3997 overtones. When she enrolled in first grade she realized that the other children her
3998 age had learned to read in kindergarten. Confronted with her (temporary) inability
3999 she became convinced that she was “stupid”. When I tried to point out to her that she
4000 spoke a foreign language while none of her friends could, her response was that it
4001 wasn’t important. Of course, with a little extra tutoring at home she quickly caught

4002 up and ended her high school career as class valedictorian but that was off in the

4003 distant future.

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Chapter 10

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The Equator and other lessons

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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.

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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. 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.

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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

4029 energy. The higher modes with tiny amounts of energy did change the temporal
4030 behavior of the larger scale modes through their interactions in some cases rendering
4031 “interesting” chaotic behavior much more regular in the fuller system.

4032 A new direction for my work occurred in an unexpected way. I think it was in
4033 1986, attending a meeting of the American Geophysical Union (AGU) at its annual
4034 meeting in San Francisco, that I heard Harry Bryden talk about his recent work with
4035 his Joint Program student, Esther Brady. They had carefully analyzed the motion in
4036 the Equatorial Undercurrent, a subsurface current in the equatorial zone, i.e. the
4037 region within 2 or so degrees of latitude on each side of the equator. The major
4038 conclusion they came to was that the rising motion of water in the top 100 meters or
4039 so occurred as water flowed along surfaces of constant density that slope upwards
4040 from west to east. The traditional picture of this region of equatorial upwelling
4041 usually depicted the motion in the latitude/vertical plane so it appeared that the fluid
4042 was *crossing* density surfaces. That, in turn would require mixing of density and
4043 render the motion non-adiabatic. The power of a careless pictorial representation of
4044 reality places a scratch on the mind that is often irremovable and once the brain
4045 absorbs the picture of a mixing induced upwelling the governing dynamics is
4046 naturally *assumed* to be non-conservative requiring a recipe for the mixing. The
4047 Bryden/Brady picture instead suggested the possibility that a thermodynamically
4048 conservative dynamical model could be apt with all the simplifications that allows. I
4049 immediately thought of how such an equatorial model could be connected to the
4050 mid-latitude thermocline theory Stommel, Luyten and I had developed a few years
4051 earlier. I remember that my colleague, Nelson Hogg was sitting next to me during

4052 Harry's lecture and I could not but whisper to him my basic idea and he immediately
4053 asked how many layers I thought the model required and I said, "Two, just two".
4054 That lecture and the ideas that flowed from it all were stimulated by Harry's lecture
4055 and the irony was that I had to travel cross-country from Woods Hole to San
4056 Francisco to get the idea from someone whose office in those days was just across
4057 the hall from me in Woods Hole!

4058 When I got back to Woods Hole I quickly formulated the model and it was easy
4059 and natural to consider the problem, as in the mid-latitude thermocline problem, as
4060 an exercise in potential vorticity conservation. Once again the issue was what the
4061 relationship between the streamfunction and the potential vorticity would be. The big
4062 difference was that the form of the Equatorial potential vorticity was more complex
4063 because it now needed to include the relative vorticity and the streamline was
4064 connected to the Bernoulli function that now included the kinetic as well as the
4065 potential energy. Nevertheless, the relationship between those two functions, the
4066 potential vorticity and the Bernoulli function, was established in mid-latitudes for all
4067 streamlines emanating from mid-latitudes that reached the equator. The dynamical
4068 model gave rise to scales for velocity, depth and width that were more realistic than
4069 the extant representations of linear theories of the Equatorial Undercurrent (EUC),
4070 the strong subsurface flow that existed in the Pacific, Indian and Atlantic Oceans. I
4071 was thrilled to see the basic nonlinear theory emerge so easily. There was some
4072 difficulty in coming up with a representation of the layer *above* the EUC but with the
4073 encouragement by Carl Wunsch to whom I had mentioned that difficulty, I decided
4074 to reduce the dynamics to a simple condition on the interface between the two layers.

4075 The principal idea was more solid. The EUC in this picture was not a purely
4076 equatorial phenomenon. It was driven by its connection to the mid-latitude flow and
4077 I found it aesthetically pleasing that the continuation of ventilated thermocline
4078 theory to the equator explained the nature of this major current. Previous theories
4079 considered it a local equatorial phenomenon; and linear, and even previous attempts
4080 at nonlinear theories, considered the EUC in isolation from the rest of the ocean and
4081 I believed the *connection* of the Equator to the mid-latitude circulation was the most
4082 interesting part of the theory. There was some resistance to the new theory for that
4083 reason. Indeed, when one of my graduate students took up a post-doc at Princeton,
4084 he called me up and, in an embarrassed tone of voice, let me know that the first
4085 project he was assigned to do was to use a large numerical model to show this new
4086 theory was wrong because of the reliance on the connectivity of the two regions!

4087 The idea that equatorial dynamics was isolated from the rest of the ocean had a
4088 natural origin. Theories of large-scale waves in that region, starting with the earlier
4089 work of Dennis Moore, Mark Cane and Ed Sarachik, showed that these time-
4090 dependent motions were naturally trapped by the Earth's rotation to a narrow zone
4091 around the equator and the idea took hold that this was generally true for all
4092 dynamics. Indeed, people began talking about Equatorial Oceanography as distinct
4093 and separate from the rest of the ocean and for certain phenomena that are time
4094 dependent, like El Niño, that is largely true. For essentially steady motions, like the
4095 EUC, that trapping effect no longer holds and the flow of water to the Equator
4096 connects the mid-latitude dynamics to the equatorial domain.

4097 The following year, after the theory was essentially complete I was giving a
4098 seminar about it to the Oceanography Department at the University of Washington
4099 and as I was developing the ideas for the audience a helpful image suddenly
4100 occurred to me. In an improvisation on the spot, I asked the audience to consider the
4101 potential vorticity and the Bernoulli function to be two members of a “happy
4102 couple”. She is the Bernoulli function and he is the potential vorticity. In the rural
4103 area of the mid-latitude gyres she is mostly potential energy and he lacks any
4104 substantial spin of his own. I then asked the audience to think about the couple as
4105 they move to the region of the Equator where constraints are looser. She gains
4106 intense kinetic energy; his spin is less statically local, perhaps metro-sexual in
4107 character. Nevertheless, the relationship between the two of them is solid and
4108 remains what it was before as they superficially change form. The audience loved it.

4109 I did have one difficulty initially in completing the theory and I’m embarrassed
4110 to own up to it now. I originally solved the coupled non-linear differential equations
4111 for the layer depths and the velocity by starting the calculation in mid-latitudes and
4112 proceeding to the equator having given the depth of the thermocline *and* the zonal
4113 velocity at the starting points along a set of different longitudes. This was a mistaken
4114 formulation of the problem but I didn’t realize it immediately.

4115 As a consequence when I integrated the theory’s equations to the Equator I
4116 found a EUC that seemed to speed up and slow down, even reversed direction in a
4117 bizarre and physically unacceptable way. I was flummoxed.

4118 One day I mentioned to Holly that I was bewildered by this apparent
4119 fundamental deficiency of the theory and couldn’t figure out what to do. She

4120 listened, and suggested one morning that I come home for lunch that day instead of
4121 taking lunch at the lab. We often arranged such “dates” and the delectable lunch that
4122 she always prepared typically followed an intimate, romantic interlude while our
4123 daughters were at school. It was in the immediate aftermath of the interlude that day,
4124 while in the usual idyllic and dreamlike post-romantic state, that I turned to Holly
4125 and murmured, “ It’s not an initial value problem; it’s a boundary value problem!” In
4126 that moment of relaxation I suddenly realized that I should be specifying the
4127 thermocline depth at each longitude far from the Equator and the same constant
4128 Bernoulli function on the Equator at each longitude. That resolved the problem.

4129 I believe many of us have that experience where a difficult problem is hard to
4130 resolve because we fervently believe our initial approach to a problem is basically
4131 correct. When it fails to resolve, we try harder and harder on the original path to find
4132 a way to break through to the answer and remain stymied until we allow our internal
4133 censor to admit another approach, different than our original idea. It is in periods of
4134 relaxation when the censor lowers its guard allowing hitherto blocked lines of
4135 thought to arrive that progress can be made. At least, that is what I have experienced
4136 several times. It was just a bit more extreme and romantic in this case. Holly didn’t
4137 bat an eye; she was used to the intrusions of science gobbledygook at all hours.

4138 The Equator was also the site of one of my greatest embarrassments. I can think
4139 of two occasions in which I have published a paper than turned out to be very wrong.
4140 When I say very wrong I mean wrong not only as a calculation but wrong
4141 conceptually. The first example in this little museum of horrors that I revisit from
4142 time to time, usually in disturbed sleep, was my misconception about the nature of

4143 the spin-up of a stratified fluid. For a homogeneous fluid the flux of fluid flung out
4144 radially in the bottom viscous boundary layer, the so-called Ekman layer, rises in the
4145 viscous boundary layers on the sidewalls, the Stewartson layers. In my work on the
4146 stratified flow problems that I later continued with Victor Barcilon I was able to
4147 show that if the sidewalls were thermally insulated those boundary layers could not
4148 carry that mass flux. I jumped to the conclusion that it meant that the spin-up
4149 circulation would be choked off and the spin up process would proceed much slower
4150 by diffusion. I overlooked the possibility, as demonstrated by Gösta Walin, that the
4151 flow from the Ekman layer would circumvent that problem by issuing directly into
4152 the interior of the fluid via jets issuing from the corner intersections of the bottom
4153 and sidewalls. I was correct in arguing the sidewall layers could not carry the flux
4154 but incorrect in understanding the consequences. I was abashed and depressed when
4155 I realized my mistake. Of course, I try to give myself solace by reminding myself
4156 that when you are the first to try to do something really new it is not surprising that
4157 you can flub. More telling was that several people had told me as I was formulating
4158 the problem that I must have been making a mistake but my certainty was anchored
4159 on the idea that without the sidewall layer the spin-up would be blocked. Ignoring
4160 those criticisms in retrospect appeared more and more like intellectual arrogance and
4161 I hated to think of myself in those terms.

4162 Similarly, when I took up the problem of the existence of Deep Equatorial Jets,
4163 the jet-like flows on the equator beneath the region of the EUC, I had an intuition
4164 that forcing from the eastern boundary radiating its effect westward as arrested
4165 Rossby waves might be an explanation. I formulated an equatorial wave model

4166 recognizing that this was a dynamical regime that was new to me but was delighted
4167 when the calculations done in the application MATLAB gave a very good likeness
4168 to the deep jets. Later calculations by a group more adept than I am in such kinds of
4169 models showed convincingly that I was wrong. Checking and rechecking my
4170 calculations showed that the excellent but erroneous result I had obtained was due to
4171 a single misplaced parenthesis in my MATLAB program. It was a very dark day
4172 indeed. Those two papers out of an oeuvre of about 160 research papers remain
4173 seared in my consciousness as examples of my occasional hubris and the need for
4174 greater humility. I find it especially painful because as a rule, when I first hear
4175 criticism of some work of mine my very first inclination is to believe the criticism is
4176 probably true and my more serious problem is not to be swept away by self doubt. In
4177 those two cases I would have done better to listen to that inner skeptic.

4178 That brings me to another personal issue. About this time I was entering my
4179 50's, and although it looks to me like a relatively young age now, it was pretty clear
4180 that youth could no longer define my situation in the world. Indeed, I began to
4181 realize that with the scientific successes I had been fortunate enough to attain, really
4182 younger people began to interact with me in a different manner. When introduced to
4183 a new, young person in my field I could not help but notice a slight startle and
4184 change of demeanor. I don't want to exaggerate this but it was something
4185 inescapable. This, and the sense that I was no longer one of the kids, started me on
4186 an introspective train of thought and a conversation with myself.

4187 It was also initiated by an experience that made a profound impression on me.
4188 One summer, about this time, I was attending a seminar given by one of the founders

4189 of the GFD program in Woods Hole who was about 15 years older than me. A fairly
4190 good scientist in his youth he gave a seminar about some of his recent work that
4191 could only be called mediocre at best. It was actually embarrassing to hear him
4192 lecture. I almost felt sorry for him but his lack of self -knowledge didn't allow for
4193 too much sympathy. I was wondering about his inner thinking when the person
4194 sitting next to me leaned over and whispered to me, " His problem is that he thinks
4195 of himself as the Grand Old Man of GFD". The correctness of this diagnosis struck
4196 me as obviously true and I pledged to myself that as I aged I would do my best to
4197 continue to approach my work with the same attitude I had when I started out in the
4198 field. That is, to not ask for any special privilege in judging my work. If I had
4199 nothing really interesting to say I would keep quiet until I did, and not ask for special
4200 treatment based on past accomplishments. I know it's easiest to fool yourself in such
4201 evaluations but I promised myself to keep the example of that day's lecture in mind
4202 as a cautionary example of what can go wrong when you set yourself a different
4203 standard because of age.

4204 Another discussion I had with myself revolved around an experience that all
4205 scientists who have any self-knowledge have to confront sooner or later. How do
4206 you respond to a younger scientist who is clearly smarter than you? I have known
4207 some scientists in my field who out of envy or personal insecurity play the
4208 equivalent of the children's roughhouse game "king of the hill" where you do
4209 anything to prevent a competitor from reaching your level. There are many ways it
4210 can be done. People sometimes write destructive letters of recommendation about
4211 people they fear will compete with them or struggle over authorship precedence.

4212 In the conversation that I had with myself about the future I intended to behave
4213 quite differently. I think this was made easier for me by the fact that in all my school
4214 years I was *never* the smartest person in my class and in my professional life I never
4215 considered myself superior in capability to everyone else. When I thought of the
4216 giants in my field, e.g. Charney, Stern, Stommel, Munk and such others, my goal
4217 was simply to be able to play in their league. If I could be considered a valued player
4218 in that major league I would be happy with myself. Of course, in doing creative,
4219 science it is hard to evaluate what it means to be more or less smart than someone
4220 else. Its connection to “being smart” is less clear. Some people are better with
4221 abstract theory and advanced mathematics, others seem to have deeper physical
4222 intuition. In the end, what really counts is the quality and importance of your work.
4223 When I recognized younger people who I thought were clearly superior, I have been
4224 supportive and I feel that I’ve been rewarded by working with each of them and by
4225 their generosity in their interactions with me. Life is incredibly more pleasant when
4226 you are working to collaborate rather than working to thwart other people. It sounds
4227 simple but some people are so eaten by envy that they consider the appearance of
4228 new stars as a threat to the security of their self -image. Since I never had the
4229 experience of being able to consider myself the smartest person in my solar system
4230 or the brightest star in the scientific galaxy it was natural to content myself with
4231 feeling good about my successes on my own internal scale of measure.

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Photos: Teaching.



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Lecturing at the GFD summer course 2007.



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4246 The course in the MIT Joint Program that I enjoyed teaching the most was the first
4247 year course, which was an introduction to fluid dynamics. My classes were large
4248 with students from MIT, Woods Hole and Harvard. Here is the class in 2007. I am in
4249 the second row, second from the left.

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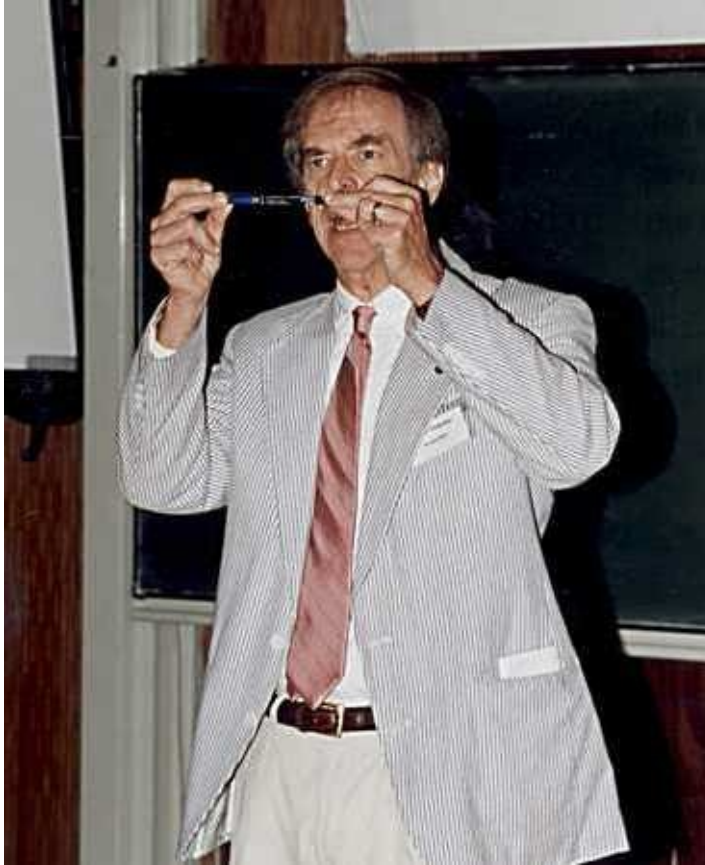
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4265 Lecturing at a course in Limerick, Ireland, 2000. I was fond of limericks and often
4266 made them up. One of my favorites referred to a lecture at a different meeting, in
4267 which the speaker, from Imperial College was attempting to simply represent the
4268 complicated behavior of atmospheric eddies. I thought the attempt was too simple:

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4270 There once was a man from Imperial
4271 Whose view of the world was ethereal.
4272 His theory is fine,
4273 It fits a straight line
4274 But his assumptions are many and serial.

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Chapter 11

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A Shattered Calm; instability and Islands

4281 One of the pleasures of academic life is the opportunity to work with a stream of
4282 bright young people whose strengths complement my own. In the period of the 90's
4283 I had that experience with several excellent students and post-docs. One of the
4284 brightest was Roger Samelson who came to WHOI as a post-doctoral scholar and
4285 stayed on as an Assistant Scientist until he was lured away by a faculty position at
4286 Oregon State University. I had become interested in the consequences of the
4287 variation in the downstream direction of the stability properties of a zonal flow.
4288 Roger and I looked at a simple idealized model in which topography, sloping to
4289 produce a stabilizing effect like beta, would satisfy the *sufficient* condition for
4290 stability everywhere, i.e. for all zonal stations, except for a relatively small region in
4291 the downstream direction. In that region the *necessary* conditions for the instability
4292 of the flow would be satisfied. It was unclear whether that would be enough to
4293 locally destabilize the flow. Our analytic work provided a surprising result. We
4294 found that if the necessary conditions were satisfied in a small region of length $2a$,
4295 the flow would become unstable even though the length a might be small compared
4296 to a deformation radius. It is not the place to go into the details of the analysis but
4297 since zonal flows are normally unstable to perturbations *greater* in length than the
4298 deformation radius it was natural to expect that small domains of instability would
4299 continue to be stable. The fact that it was not true was astonishing. One's intuition is
4300 built by the experience of a collection of examples and intuition can be misleading

4301 when the configuration of the problem seems familiar but actually has new elements
4302 outside previous experience.

4303 Some years later my colleague Bob Pickart, an excellent observational
4304 oceanographer, pointed out to me a curious region along the West Greenland Current
4305 where the current produces eddies from a limited region of the current flowing up
4306 the western coast of Greenland on its continental slope. It seemed surprising because
4307 that was also a zone of particularly steep slope, normally more stabilizing. However,
4308 the slope was so steep that in that short distance along the flow the current over most
4309 of its width was flowing over such a deep bottom that it did not sense the
4310 topographic slope. This seemed like a great opportunity to apply the ideas Roger and
4311 I had developed and another excellent post-doc, Annalisa Bracco, took up the
4312 problem. The new situation was more complex than the one Roger and I had first
4313 studied but Annalisa's careful numerical work showed the same behavior, i.e. locally
4314 enhanced instability in the domain in which the current does not experience the
4315 stabilizing effects of topographic slope. Her work led to an elegant explanation of
4316 the localized source of eddy energy in the West Greenland Sea.

4317 However satisfying my work was at that time, real life provided instability of a
4318 far more ominous character. After several medical examinations in 1990 it was
4319 discovered that Holly had breast cancer in her right breast. We found good doctors
4320 and treatment at the Dana Farber Cancer Hospital in Boston but, needless to say, our
4321 lives were overwhelmed by this terrible news and the news kept getting worse. At
4322 first it appeared that a simple lumpectomy would suffice but the results of that
4323 procedure required further surgery, a mastectomy and follow-up chemotherapy. In a

4324 seeming instant everything else in life fell away and the fear and anxiety of the
4325 cancer diagnosis took over everything. During the follow-up period of chemotherapy
4326 Holly continued the teaching of photography she had been doing at the school Dove
4327 was attending, Falmouth Academy. The pressures were great and the emotions were
4328 very raw. Shortly before the mastectomy Holly was standing in the bathroom one
4329 night before coming to bed and burst into tears at the thought of the breast
4330 disfigurement to come. When chemotherapy started, her normally abundant hair
4331 thinned noticeably but she avoided the need for a wig. Nevertheless, the pain, nausea
4332 and fatigue associated with the chemo were often overwhelming for her. She was
4333 amazingly stalwart during this period, never complaining to our daughters, keeping
4334 apparently serene in public but, occasionally, when there were just the two of us in
4335 the house she would burst into tears before recovering her composure.

4336 She joined a breast cancer support group of women who were going through
4337 similar trials and, in fact, she was the only woman of that group who survived her
4338 cancer. As time went on and periodic examinations showed no sign of the return of
4339 the cancer her confidence grew. The reconstructive surgery of the breast was also an
4340 important psychological boost and within a year we were traveling to Italy again and
4341 life seemed to return to normal. Of course, after cancer life is never normal again.
4342 The fear of recurrence hangs ever in the background. Holly was always a physically
4343 strong woman in excellent health and if the cancer did nothing else it destroyed the
4344 automatic self-assurance in her health. Still, she did survive that bout of cancer and
4345 her subsequent cancer 20 years later in 2011 was judged to be an independent and

4346 unrelated event. I will get to that sad story later but at this point in the history I am
4347 recalling, family life was restored and we had many happy years ahead of us.

4348 During this period of time an unexpected task was thrust on me at the
4349 Institution. Two fairly senior scientists, not in my department, were accused by two
4350 younger postdoctoral researchers of scientific misconduct. The younger scientists
4351 accused their older colleagues of stealing their ideas and publishing a paper on the
4352 subject without giving them proper credit. I was asked to chair a small committee of
4353 three to look into the matter and present a report to the Director so the appropriate
4354 action could be taken. The other two committee members, Stan Hart and Holger
4355 Janasch, both outstanding scientists, were eager, as I was, to get to the bottom of the
4356 matter. A preliminary investigation had already be performed and a report written
4357 and when I read the report my first reaction was to believe the accusation and I
4358 found myself in sympathy with the younger scientists. I will not go into detail to
4359 protect the identities of all involved but it required that we three investigate the
4360 matter in detail and interview everyone involved. The two younger scientists had left
4361 the Institution and they were invited to come back and tell their side of the story *in*
4362 *camera*, in confidence. The same opportunity was given to the more senior people.
4363 Somehow, in the process my original certainty became shaken and I grew to believe
4364 someone was not telling the truth and I decided to use a technique I associated with
4365 the fictional detective I most admired, Simenon's Inspector Maigret. I encouraged
4366 each of those involved to tell their story in greater detail, constantly encouraging
4367 them to describe things more deeply and at greater length. As I hoped, when the
4368 story telling went beyond the point of the originally prepared story, the people telling

4369 the truth had no difficulty in maintaining consistency. The others fell into increasing
4370 confusion and contradicted their own initial stories. We three on the committee were
4371 able to come to a conclusion that the original allegations had been so embroidered
4372 that the case against the senior people could not be maintained and said so in our
4373 report. I think we saved some innocent people some serious grief.

4374 Cancer touched not only our family but also one of my close friends and
4375 colleagues, Hank Stommel. We had remained close since our work on the ventilated
4376 thermocline and like many others of his colleagues I was dismayed when a colon
4377 cancer metastasized to his liver leading to his death in 1992. There was a period of
4378 remission between the initial cancer and its recurrence and during that time we had
4379 many occasions to talk about humanly deeper things than any particular
4380 oceanographic phenomenon. He mused aloud to me, during that time, that he felt
4381 basically lucky. “ Just think”, he once remarked to me, “ I could have been an apple
4382 and never even known I was alive”.

4383 He and I had been working on a paper concerning a mechanism, involving
4384 stratification dependent mixing, for self-excited inertial waves in the mixed layer. It
4385 was not a major piece of work but the physics was intriguing and it was fun for me,
4386 and I suppose an escape for Hank, to be working together again. When we had
4387 almost a full draft of the paper ready Hank came over to my house one Saturday to
4388 discuss the form of the final draft. We ended up standing in the kitchen while I
4389 prepared tea for the two of us. Hank was in a melancholy mood. The suggestion had
4390 been made to surgically remove the most cancer- affected region of the liver to
4391 perhaps give Hank more time. He was reluctant to undergo another surgery. He

4392 mentioned that his neighbor, Bill von Arx, had counseled against it saying that the
4393 time gained would be lost in the period of recuperation from the surgery. Hank
4394 sighed and said that his family was pressing him to have the surgery and he
4395 supposed that to satisfy them he would. He lamented that he was “tired of being
4396 everyone’s role model”. He was very depressed which of course was very natural in
4397 the situation in which he found himself. Death after all, was imminent. He shortly
4398 submitted to the surgical procedure and soon afterwards called me from the hospital
4399 to tell me that the surgery was to no avail. The surgery showed the cancer so
4400 widespread in the liver that no benefit would be obtained by removing any part of it
4401 and he asked me to pass on the news to his friends so he could be spared telling the
4402 same grim story over and over again. I did visit him once while he was recovering
4403 from the operation. He was in a deeply blue state and called himself a “deeply
4404 feeling person” and I think it was a way of expressing his anguish at what he was
4405 confronting. He died of a heart attack during the night soon after, no doubt a
4406 consequence of surgery. So passed the second of the two greatest scientists I have
4407 ever known personally, Charney and Stommel. My world is a much bleaker place
4408 without them.

4409 By this time I had spent more time at Woods Hole and the Oceanographic
4410 Institution than I had ever spent in one place in my life before. Holly and I found the
4411 atmosphere suited us perfectly. The beauty and serenity of Cape Cod, the annual
4412 excursions to Italy, the availability of Boston and New York and an increasing circle
4413 of friends and colleagues made both our lives richer and both more relaxed and
4414 stimulating. The connection with MIT and the excellent people there only added to

4415 my sense of satisfaction about our decision to move to Woods Hole. We considered
4416 ourselves immensely lucky.

4417 The decade of the 90's was also a particularly productive period for me
4418 scientifically. I worked with a number of excellent Ph.D. students and colleagues.
4419 Patrice Klein, from France, and I worked on fully nonlinear baroclinic instability
4420 where the condition of weak nonlinearity could be relaxed by employing direct
4421 numerical calculations. Roger Samelson and I showed how the radiation of energy
4422 from unstable jets could actually, and surprisingly, *destabilize* the basic flow. Karl
4423 Helfrich and I worked on models of atmospheric blocking as solitary waves and
4424 carried it further to include strong perturbations beyond the limits imposed by the
4425 solitary wave model. With Igor Kamenkovich, I was able to show how currents
4426 slightly tilted with respect to latitude circles could easily radiate energy to great
4427 distances as an explanation for the occurrence of strong eddy motions in oceanic
4428 regions with relatively weak background flows.

4429 Perhaps the most interesting and unexpected results came from work initiated
4430 with my colleagues Karl Helfrich, Larry Pratt and Mike Spall. We had become
4431 interested in planetary scale oceanic motions in regions containing large islands, e.g.
4432 Australia. Such large islands represent "holes" in the oceanic domain and present
4433 new mathematical and physical elements for the oceanic circulation. Karl and I had
4434 worked out the solutions for steady flow for a basin with a long island oriented in the
4435 North-South direction with only small gaps between the island tips and the basin
4436 boundaries. To our surprise the smallness of the gaps did not impeded a substantial
4437 circulation between the two sub-basins of the domain. That is, although the "island"

4438 nearly divided the basin into two nearly separate domains, dynamical conditions
4439 produced a full basin circulation. It also, even in linear theory, produced a region of
4440 recirculation on the eastern side of the skinny island. Karl's experiments validated
4441 the theory in a beautiful way. The video of he made of his experiment was always a
4442 crowd pleaser whenever I gave a seminar on the subject.

4443 Then with the addition of Larry and Mike to the team we began an ambitious
4444 investigation of the island problem in a more extensive manner. Our paper combined
4445 analytical theory, numerical modeling and laboratory experimentation in a way I
4446 thought was very fruitful and I enjoyed working with all three of my colleagues to a
4447 great degree.

4448 One of the results that puzzled us was that the magnitude of the mass flux from
4449 one sub-basin to the next seemed to not depend very strongly on how nonlinear the
4450 circulation was. Increasing the forcing led to major changes in the shape of the
4451 configuration but the streamfunction value on the barrier did not change much
4452 correspondingly. I wondered whether the use of the no-slip condition on the island,
4453 which vitiated much of the net relative vorticity transport was responsible for this
4454 and I wondered if a baroclinic model with potential vorticity that included thickness
4455 fluxes might alter the result qualitatively. Mike began to look at the same
4456 configuration in a 2-layer model where baroclinic eddies could transport potential
4457 vorticity without suffering the same constraints as the barotropic single layer model.
4458 He came into my office a few days after he had gotten started with a rather
4459 astonishing result. As expected, strong eddies were generated in the western
4460 boundary current on eastern side of the barrier that almost severed the eastern from

4461 western basins. Nevertheless, the western basin was also full of eddy activity and in
4462 the lower layer where the mean field was too small to mask the nature of the time
4463 dependent motion, there appeared to be fluctuations that looked remarkably like
4464 Rossby basin modes. How in heaven's name could such large-scale modes sneak
4465 through the two small gaps and dominate the western basin?

4466 Classical diffraction theory would seem to imply that an incident disturbance
4467 impinging on barrier from the east would be almost totally reflected if its north-south
4468 scale was much larger than the width of the two little gaps. I was very puzzled.

4469 Then one day not long after, probably when my interior censor was taking a
4470 brief break it occurred to me that applying Kelvin's theorem to the island showed
4471 how the region of the barrier between the two gaps evolved from being a barrier to
4472 being an antenna that radiated wave energy to the western basin. Using that idea I
4473 first calculated the coupled normal modes of the two sub-basins and Mike was able
4474 to verify the calculations using his numerical model. Some years later a summer
4475 student Fellow in the GFD program, Alexis Kaminski, carried out a laboratory
4476 experiment under Karl's guidance and she verified the very unexpected result. The
4477 large- scale modes squeezed through the two gaps like some kind of soft invertebrate
4478 animal squeezing through tiny cracks in a fence. A string of our papers followed the
4479 original one, each one demonstrating the striking power of Kelvin's theorem to
4480 allow this kind of unique wave diffraction result rendering what might be thought of
4481 as nearly impenetrable barriers completely transparent.

4482 Our daughters, Dove and Anna were now grown and no longer living at home
4483 except for visits and vacations. Anna had left New England for graduate school in

4484 San Francisco and discovered that she was, at heart, a natural Californian. She had
4485 some difficulty in finding a position, post-MBA, that was at once emotionally
4486 satisfying and financially sufficient. After many years in various management and
4487 administrative positions did she finally found an occupation that was emotionally
4488 satisfying, serving as independent contractor and counselor for people moving to the
4489 area around Silicon Valley. Dove was in her senior year at Boston University when
4490 she was struck by an illness that brought unremitting pain and an interruption to her
4491 personal life that lasted for over a decade. Never have I been so proud of her as how,
4492 during that time, she soldiered on with her life without becoming bitter about the
4493 limitations the disease placed on her. Finally, a doctor in Washington, DC, found a
4494 simple topical medication that helped with her pain and although she has needed to
4495 pay constant attention to her situation she has been able to recover a relatively
4496 normal professional and private life. Both girls married good men and are happy in
4497 their marriages to my own great satisfaction.

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4500

Chapter 12

4501

The new millennium and tragedy

4502 As we left the 20th century and entered the 21st, my life seemed established in a
4503 profoundly stable manner. My work continued to go well, our daughters were mature
4504 and independent, and Holly's and my lives seemed destined to continue in a
4505 comfortable and satisfying way in both our work and private lives. I was enjoying
4506 teaching in the MIT/WHOI Joint Program and my teaching seemed to please the
4507 students and that made me very happy. I was given the Arnold Arons award for
4508 teaching and mentoring and that gave me the opportunity to publically ruminate on
4509 the fundamental importance of teaching and how much it meant to me. My remarks
4510 on receiving the award are in Appendix C.

4511 My research work evolved and there were a series of new and interesting
4512 problems that focused my attention.

4513 It is impossible to describe all of the research problems and the collaborations
4514 involved in coming to grips with the challenges they presented. Among the more
4515 salient problems I examined at this time, was the problem of the instability of time
4516 dependent flows. When the basic flow is steady, as is often assumed in geophysical
4517 settings, there is usually a well-defined threshold of some parameter, e.g. the
4518 temperature gradient, or the degree of friction, that needs to be surpassed to make the
4519 flow unstable and generate self-sustaining perturbations. Working with graduate
4520 students Jim Thomson on one paper, with Francois Poulin on another and with my
4521 MIT colleague Glenn Flierl on yet another, we were able to show that flows of
4522 meteorological and oceanic interest could become unstable for values of critical

4523 parameters well below the normal thresholds established for steady flows. It was a
4524 very exciting time for me and the collaborations were continuously stimulating.

4525 With colleagues Igor Kamenkovich and Pavel Berloff, we suggested a
4526 mechanism for the generation of recently observed zonal jets or striations embedded
4527 in the large-scale ocean circulation.

4528 Perhaps the most unusual result from this period was a study I made of the
4529 development in time *and* space of baroclinic instabilities. I was interested in
4530 examining the spatial and temporal development of baroclinic wave moving
4531 downstream from an origin where small disturbances were introduced and the
4532 disturbances could grow and evolve in naturally unstable current. It turned out that
4533 for some values of the parameters, when the growth rate was small but no smaller
4534 than the viscous decay time, that the motion along rays in the downstream-time
4535 plane, would be chaotic, That meant that on two neighboring characteristics with
4536 slightly different initial values, the disturbance at long distances from the origin
4537 would have strongly different values as a consequence of the chaotic sensitivity to
4538 initial data. That in turn implied very rapid variations in space from one
4539 characteristic to another at a given time. I thought the results that led to what I called
4540 chaotic shocks represented something of fundamental importance that I had not seen
4541 before. However, since it was published in a journal of rather limited circulation (J.
4542 Marine Res.) that I chose in order to avoid page charges, it drew little public
4543 attention. Perhaps in some way it will be noticed in the future.

4544 Such considerations lost most of their importance to me because of the more
4545 pressing developments at home. Starting in the late winter of 2011 Holly started

4546 complaining of an upset stomach, usually after we had gone out for dinner. We
4547 thought little about it. Usually, Holly would check with me to see if I also was
4548 affected by the meal but since we rarely ate the same meal we put it down to
4549 something in the food she had eaten. When this began to occur more frequently she
4550 consulted our family doctor. Simple tests showed no problem and there was nothing
4551 suspicious in the food she was eating or the restaurants we frequented. She recently
4552 had her annual physical and everything seemed in perfect order with nothing
4553 suspicious in her blood work or the doctor's hands-on examination. Finally, after a
4554 few months of inconclusive tests the doctor ordered an abdominal scan.

4555 I still remember the phone call I received from Holly. The radiologist had
4556 discovered what he called "masses" in her liver. This was, of course, very shocking.
4557 I rushed home and we made an appointment to see a specialist, a Dr. Cobb, right
4558 away. He studied the radiologist's report and the weirdest thing was that he kept
4559 shaking his head and saying, "Very bad, oh, this is very bad" while all the time
4560 smiling as if he were having a fine time. I could have murdered him on the spot. The
4561 report said the liver was filled with cancerous tumors, over 50 of them. We were of
4562 course very shaken. Then Holly, bless her soul, decided to get in touch with a doctor
4563 at the Dana Farber in Boston who had shepherded her through a recent alarm of
4564 cervical cancer that turned out to be nothing. She had great confidence in him and
4565 she made an appointment to see him and we were able to do it the following day in
4566 Boston. I remember it was a Friday.

4567 The doctor read the radiologist's report and seemed annoyed at the diagnosis
4568 that assumed the masses were cancerous. He kept saying that they should just

4569 describe what they saw without jumping to conclusions about the identification of
4570 what the masses might be. Then he said to Holly, “ If these were cancers you’d be
4571 dead by now”. We took that as reassurance since clearly Holly was not dead but in
4572 good health. It’s clear we were desperate for reassurance that everything might still
4573 be ok. When Holly mentioned to him that a mesh had been inserted in her abdomen
4574 as part of the breast reconstruction after her mastectomy 20 years before, the doctor
4575 said with authority that it was probably the mesh that was being mistaken for
4576 cancerous masses. After further examination he said a hysterectomy might be
4577 advised but that it was nearly certain it was not cancer. My heart leaped and I
4578 shouted out, “Doctor, I feel like kissing you”. He replied that he’d much rather that
4579 Holly kiss him and I conceded the point.

4580 When the doctor told us we could go and that, to double check, he would
4581 show the radiology films to his radiologist but that he thought there was nothing to
4582 worry about, we left his office in a mood I can only call ecstatic. On the bus ride
4583 home we held hands and kept smiling foolishly to each other expressing our joy over
4584 and over again. Walking in Woods Hole from the bus to our car we bumped into a
4585 local friend, the jazz pianist, Glenway Fripp, and told him we had just dodged a
4586 bullet, that we were happy beyond measure and that it was a wonderful day. We had
4587 a splendid weekend together and felt that life had been restored to us.

4588 The following Tuesday I was in my office when Holly called and with a quiet
4589 and somber voice told me the doctor had called and said that his radiologist had
4590 confirmed that the masses were cancer tumors after all. It was a crushing moment

4591 and I rushed home to be with Holly as we started what was to be the journey into the
4592 darkness that followed.

4593 Holly's stepmother had died of cancer not long before, but Holly and her
4594 father had been favorably impressed by her stepmother's oncologist at the Dana
4595 Farber Institute. Holly called and made an appointment to be seen right away and
4596 had the films sent up to the Dana Farber for the doctor, Michael Rabin, to see them
4597 for himself. Our appointment was for the following Friday, July 3 and it took place
4598 at the Faulkner hospital just outside Boston which was easier for us to reach than the
4599 Dana Farber itself. We had a somber interview with Rabin. He told us immediately
4600 that the cancer had metastasized from an unknown organ to the liver. Although the
4601 cell type was a lung cancer type there was no sign of it in the lungs. He told us
4602 straight out that the cancer was incurable but treatable. We hung on to the work
4603 treatable and hoped it meant that death could be put off indefinitely with proper care
4604 and a good deal of luck even though he had also told us that the typical survival
4605 time, with treatment, was about a year. We both chose to believe in our naiveté that,
4606 of course, we would beat that dark forecast. Such is the hope born of desperation.
4607 Rabin then asked Holly when she wished to start the chemo treatments, whether the
4608 following week or sometime later and Holly, the fighter that she was, said, "Now!"
4609 Although it was the Friday before the Independence day weekend the nurses and
4610 doctor put aside any early leaving plans they may have had and got to work. Since
4611 the treatments were a 3-day affair we stayed overnight in the home of our friends in
4612 Milton, Morris and Shirley Carnovsky. Indeed, they were to be amazingly generous

4613 over the months of Holly's therapy. We had a key to their house and often stayed
4614 there by ourselves.

4615 The treatments were tough on Holly. As opposed to the chemo she had had
4616 earlier for breast cancer (and we were told this cancer cell type was not connected to
4617 that earlier cancer but was a second independent event) the physical effects were
4618 stronger. Her hair fell out almost immediately and completely. She cried to see that
4619 happen but gamely went with my wonderful secretary, Kathy Ponti, to a wig store
4620 where she let on that she enjoyed choosing a wig.

4621 The treatments became the center of our lives. We would drive up to the
4622 Carnovsky house, settle in, and go for the treatment. Holly bore the treatments well
4623 and, in fact, began to feel better as the tumors shrank.

4624 The pattern with Rabin was that he would come and talk to us about the
4625 evidence of how the treatment was going and then Holly would prepare for the
4626 following three-day treatment. There was a Legal Seafood restaurant between the
4627 hospital and the Carnovsky home and we would have supper there and, at least
4628 initially, it was not an unpleasant time if you could ignore the reason for what was
4629 happening. Indeed, before one of our early sessions, Rabin swept into the little
4630 examining room where we would anxiously be waiting and said. "You're doing
4631 great!" and we were thrilled. We told ourselves we were going to beat the cancer and
4632 with proper care our lives would go on much as before. Rabin quickly tried to dash
4633 those hopes when in answer to my question he emphasized that there was no
4634 correlation between the length of any remission and the initial success of the
4635 treatment. Still, we hoped.

4636 Once the protocol of the treatment was established we were able to transfer
4637 the treatments to a clinic in North Falmouth, just a few miles from home and this
4638 was a great benefit to both of us. The treatments continued until the end of October
4639 2011 when, on the advice of a local radiologist Holly decided to undergo radiation
4640 therapy to the brain under the theory that it could help prevent a spread of the cancer
4641 to the brain. I was skeptical because the therapy itself was so fatiguing but Holly,
4642 with her fighting spirit, wanted to do anything that might help increase the time of
4643 remission. That therapy took another month.

4644 The universe again demonstrated its love of irony. In the middle of this family
4645 tragedy that occupied our entire emotional life I received an email from the
4646 American Geophysical Union saying that I had just won the Ewing Medal, the
4647 society's highest award for oceanography. Were the situation normal it would have
4648 filled me with delight. As it was, and as I told our friends, Stan and Pam Hart, I
4649 would gladly have traded all such awards and any success in science to have Holly
4650 healthy again. But, one can't choose.

4651 The chemo treatments were going so well, and Holly was responding so well
4652 that with the improvement we decided to go together to San Francisco for the award
4653 ceremony. We also planned, with the Harts, a little vacation before the conference.
4654 We toured the wine country of Napa Valley. We stayed in a charming little hotel, ate
4655 well, and enjoyed visiting the vineyards and tasting their best wines. It was to be our
4656 last vacation together.

4657 The award ceremony requires an answering speech to the encomium
4658 describing the recipient's accomplishments. I used the speech, reprinted as appendix

4659 B, to publically thank Holly for her support over the years of our life together and to
4660 ruminate on the possible uniqueness of *intelligent* life in the universe and what that
4661 means for our responsibility to the cosmos.

4662 When we returned home the doctors suggested a scan to establish a baseline to
4663 compare the effects of further treatment. We were not too concerned about the scan
4664 since only a few weeks before, the cancer had retreated so strongly.

4665 We were therefore unprepared for the very bad news that the cancer had
4666 already sprung back and was, of course, resistant to further treatment by the first
4667 chemo treatment. A second and then a third chemical was tried and while the third
4668 type seemed to stay the further advance of the disease it was clear that Holly's health
4669 was failing. At the end of March we were advised to move to hospice care,
4670 fortunately possible at home. Our daughters rushed home and the late evening of
4671 April 5, 2102, Holly passed away as I was administering her pain medication. She
4672 had been in a coma for a few days, we were clearly at the end but the actual passing,
4673 as those who have experienced the death of a loved one know, makes painfully clear
4674 the difference between being alive and dead and is shockingly enormous.

4675 Now, seven years later, I continue to work and friends and children have been
4676 a great support.

4677 It has been a good and rich life and if I have added something to our
4678 understanding of the natural world, and if I have helped younger people make their
4679 way in science, and if I have helped my children find their places in the world, it will
4680 all have been worthwhile.

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Appendix A

4684

The Loyalty Oath

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4687

Woods Hole Oceanographic Institution

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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.

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September 7, 2004

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Reminiscences of the loyalty oath case of 1964-67 for the PAOC retreat

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2004 re: scientific controversies in Atmospheric, Oceanic and Climate

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studies.

4709

4710

I want to talk to you about the history of different

4711

sort of controversy that involved many scientists and

4712

other academics at MIT. It is perhaps not what the

4712

4713 **organizers of this retreat had in mind but I think you**
4714 **may find it of interest.**

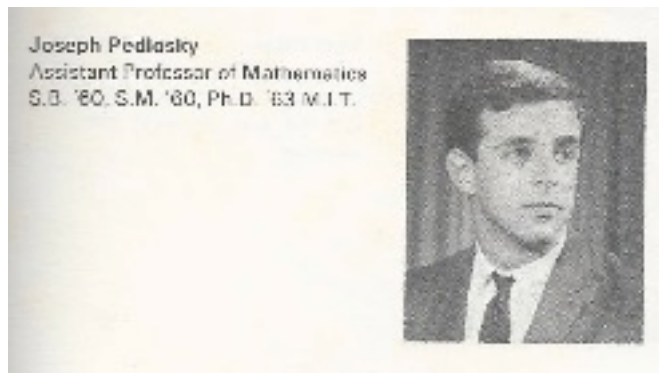
4715 _____

4716 _____

4717

4718 Imagine you are a young assistant professor of mathematics in the year 1964
4719 and it is October and you have already started to teach the fall semester with two
4720 sections of advanced calculus (18.075 today, M-351 in 1964).

4721



4722

4723 (Photo of JP from the MIT 1964 album of faculty)

4724 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.

92 State St
Boston 9, Mass.

✓
Mr. Louis
Nordlinger
LA-3-5580

Mr. Reuben
Goodman
HU2-8040

W. D. Wells
W. D. Wells

4725
4726
4727

4728

4729

.....
 (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 Massa-
 chusetts, 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

4730

4731 This is an instruction to sign an oath of allegiance and loyalty. Please note that
4732 you **all** would probably have to sign, as it, in principle, refers to professors,
4733 instructors, teaching assistants, and graduate assistants. The oath itself is rather
4734 simple and straightforward.

4735 It is an affirmative oath, not a disclaimer of the I-am-not –now – and –never –
4736 have –been –a -----Yankee fan (fill it in yourself).

4737 Instituted by the Massachusetts state legislature in 1935 over the strong
4738 protests of the state’s universities it had become, after 30 years a largely non-
4739 controversial rite of passage. So much so that although its signing was by law a
4740 condition of employment, since no one for decades had ever refused to sign it had
4741 fallen into obscurity and was routinely enforced *after* employment. After the law
4742 was passed there were several attempts to repeal the law but they failed.

4743 The history of the loyalty oath is interesting but too time consuming to describe
4744 in detail now in the time I have. But, in brief, beginning in the years right after the
4745 First World War and the Russian ‘revolution’ there was a tremendous anti-“radical”
4746 movement in the US with great xenophobia in response to the perceived “Bolshevik”
4747 threat. In fact, there were indeed a number of terrorist bombings and at least one case
4748 of multiple bombs sent through the mail to elected officials and fear was widespread
4749 of additional terrorist plots. The situation should sound familiar to us today. The
4750 response of the Attorney general of the time (Attorney General Palmer) was savage.
4751 Aliens were rounded up and deported without trial. Some members of the New York
4752 assembly were denied their elected seats and there was then started a movement to
4753 require oaths of allegiance of teachers to ensure their loyalty. In the 30’s, partly as a
4754 result of the increased social tensions associated with the Great Depression, partly
4755 again due to the menacing character of Stalin’s Soviet Union, such loyalty oaths
4756 became a common feature of many states. Sixteen states eventually passed similar
4757 measures. Massachusetts was neither the first nor last to pass such a law. Its
4758 enforcement in Massachusetts was weak and it prevented very few people from
4759 employment. In at least one case, though, a very famous literary critic and writer,
4760 Edmund Wilson was denied employment at Harvard because he refused to sign the
4761 oath.

4762 When this memo arrived on my desk without warning, my first thought was
4763 simply that this was a bureaucratic triviality that could be safely ignored. I knew
4764 without thinking that I would not sign the oath but my first thought was that I would,
4765 at worst, be considered eccentric and the whole thing would be ignored and
4766 forgotten. However, I could not be sure of that.

4767 As I said, mine was an immediate, instinctive reaction without forming a
4768 formal argument with myself. Why I felt that way is somewhat complicated to
4769 explain, even to myself. I always had a rather romantic and elevated view of what it
4770 meant to be a teacher and moreover, a college professor. Perhaps this was related to
4771 my Jewish heritage in which the Rabbi is both a teacher and the central ethical
4772 figure. I had a particularly *simpatico* and intellectually ethical Rabbi. Partly it ran in
4773 the family. My father was very much not an intellectual but as a grammar school
4774 teacher he had a strong and instinctive sense of the honor of his position. Partly it
4775 was a romantic identification with the Socratic story of the ethical role of the teacher
4776 in society. Whichever of these emotional strands was dominant it led to my decision
4777 not to sign. I was, as I said, only a bit worried about the consequences.

4778 So, as a card-carrying member of the American Civil Liberties Union, my first
4779 thought was to get in touch with them and ask for help and advice. It seemed like a
4780 natural for them and you can see on the copy of the notice my scrawled notes of the
4781 phone number and address to use to make contact. *The ACLU response could not*
4782 *have been more depressing*. And while waiting for the response a second memo
4783 from the Registrar, labeled SECOND NOTICE made me realize that MIT was not
4784 going to just forget about the whole thing if I didn't sign.

4785

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
CAMBRIDGE, MASSACHUSETTS 02139

OFFICE OF THE REGISTRAR

October 30, 1964

SECOND NOTICE

Professor Joseph Pedlosky
Room 2-375

Dear Professor Pedlosky:

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W. D. Wells

W. D. Wells

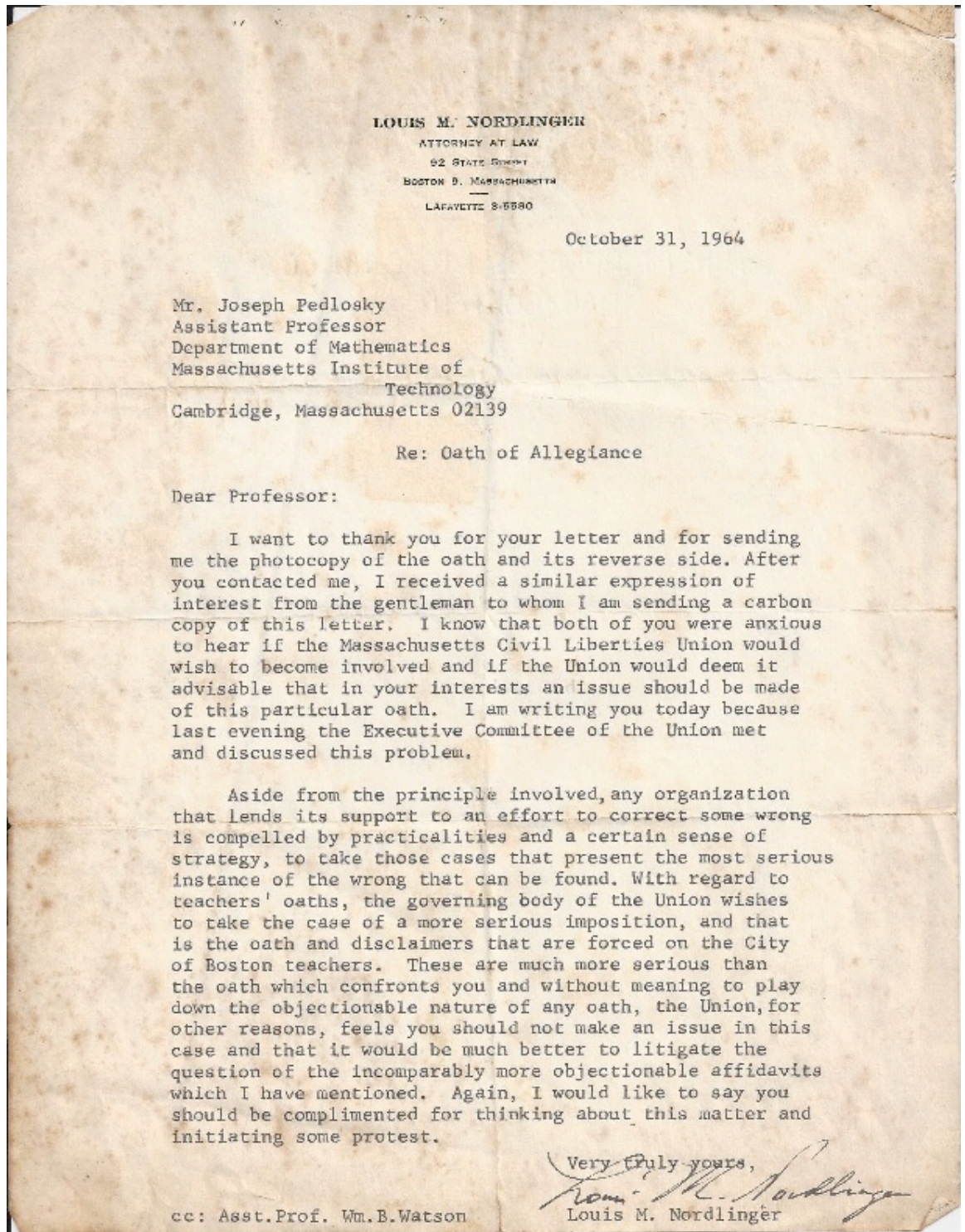
4786

4787

4788

4789

This was the first sign of trouble



4790

4791

4792 The ACLU response left me with the same feeling you might get if you called the
4793 Fire Department to report a fire in your house and they responded by saying that
4794 they were saving their engines for a *possibly greater fire that might, just might break*
4795 *out elsewhere*. So, sorry. (As it turned out there never was a case brought by a
4796 public school teacher; they were far too vulnerable).

4797 I was really rather scared now and did not know what to do. A third notice
4798 followed a month later, and then a fourth notice in December with its red heading
4799 that led me to call the attorney general's office to check with the civil liberties
4800 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 McIlwain (CA 7-4200)
Assistant Atty 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

4801

4802

4803

4804 At first, the pleasant Assistant Attorney General said she thought the oath did
4805 not apply to non-public employees (whew!) but then she found the section of the law
4806 that showed that it did and I was plunged back into deep worry.

4807 About this time, and this is the only benefit of the ACLU contact, I became
4808 aware that a second Asst. Professor at MIT, Bill Watson a historian in the
4809 Humanities department, had also refused to sign but his case was made obscure by
4810 the fact that as a teaching assistant earlier at Wellesley he had already signed the
4811 oath there and may not have needed to sign again. However, MIT was not willing to
4812 reappoint him without his signature when his current term of appointment was to
4813 end. Bill became my chief ally and his wider range of contacts in the world of law
4814 and civil liberties was crucial in what follows.

4815 Indeed shortly thereafter (although it seemed like a long time to me) a letter
4816 from the MIT Vice President M.G.Kispert arrived explicitly asking me to sign as a
4817 condition for continued employment, copies going to my dean, Jerry Wiesner and
4818 my department Chairman Ted Martin.

4819 In fact, two days later I got a call from one of the MIT lawyers (a Mr. Milne)
4820 warning me that the letter from would be the last written statement I would get from
4821 MIT officially. They were unwilling to commit anything else to paper because it
4822 would reveal that they had hired me in advance of my signing the oath and *they*
4823 would be in trouble. Further, if I did not sign in “the next few days” my employment
4824 would be terminated. This was a heavy moment for me. I felt very alone. No ACLU,
4825 no one to bat for me, and the weight of MIT bearing down on me. Furthermore, most
4826 of my colleagues at MIT that I might have expected to be sympathetic were rather
4827 reserved in expressions of support. If liberal, they were somewhat embarrassed that
4828 they had signed and argued the whole issue was really trivial and if they were more
4829 conservative they felt that I ought to sign. And, we were not so far removed from the
4830 McCarthy period where there was real worry about appearing less than completely
4831 loyal although the Federal Courts were now considerably more liberal. I also got a
4832 call from someone (I no longer remember who) on the MIT faculty committee on
4833 educational policy asking me what I was doing to deal with the situation and I had to
4834 admit to him and to myself that until that point I had done very little except to hope

4835 that it would somehow all go away. It was not and I realized I had to do *something*
4836 but it wasn't clear to me what.

4837 At this stage I was ready to throw in the towel. I am not by nature a martyr and
4838 while romantic, I have a strong attachment to the habit of eating and to sleeping
4839 under shelter. However, every time I was about to cave in something would so
4840 infuriate me that I would say "the Hell with it" and press on.

4841 For example, after the rather chilling telephone call threatening dismissal, I
4842 decided to talk to my Dean, Jerry Wiesner, well known for his liberal views and
4843 famous for his period as Kennedy's Science Advisor. I was ready to sign the Oath in
4844 exchange for a simple but public statement from MIT that they objected to being
4845 placed in a Police Role as the enforcer of the loyalty oath. That's all. I would
4846 settle for that.

4847 When I arrived at Wiesner's office for my appointment he was packing a
4848 suitcase. Without hesitating in his packing for very long he told me he was about to
4849 fly off to a disarmament conference in India and that the issue of disarmament was
4850 *inconceivably more important* than my troubles with the loyalty oath in
4851 Massachusetts. Well, I couldn't deny that and I didn't have the wit at the moment to
4852 remind him and myself that he *was* my Dean and this *was* an academic issue that
4853 was occurring in *his* School of Science. But as I left his office I got so angry at his
4854 indifference that I just knew I was not going to cave in, at least not then. Indeed,
4855 although frightened through much of this period, every time I was tempted to quit
4856 someone would do or say something that would make me so mad that I could not
4857 bear to face myself had I given up.

4858 Bill Watson and I also got in touch with the AAUP but they were also not
4859 interested in following up the case and giving any help and so Bill and I felt really
4860 trapped. That moment was the lowest moment and if it is possible to point to a single
4861 moment of moral courage it is when Bill said, "I think it's time to throw in the
4862 towel" and I said "let's wait just a bit longer before we give up".

4863 Unbeknownst to us, other wheels were moving. The administration was being
4864 asked by the faculty CEP (faculty committee on educational policy) why they were

4865 in such a hurry to decapitate us, and, urging calm and at this time someone with real
4866 moral courage came to our aid and I am forever grateful to him.

4867 Charlie Townes, the inventor of the laser, was the non-charismatic provost of
4868 MIT and his decision completely altered the tenor of the proceedings. It was only
4869 then, after about 6 months of high anxiety that I could start to breath again. I got a
4870 message left on my desk that Townes would give me a call and he did and set up an
4871 appointment to talk directly with Bill Watson and me. At the same time Bill, who
4872 was much more worldly than I, helped me find a lawyer since it was now clear that
4873 to protect ourselves we were going to have to go to court. Now enters the second
4874 major hero of the story after Townes.

4875 Our lawyer was Gerry Berlin. Onetime Assistant Attorney General in
4876 Massachusetts for civil liberties, Yale law school, WWII naval hero (bronze star for
4877 the invasion of southern France), this soft spoken Virginian with his delightful
4878 southern accent agreed to defend us, come what may, at no cost. He only asked that
4879 we somehow pay him for printing and legal research costs. A short note: An
4880 assistant professor of mathematics in 1964 made \$9,000 a year and this particular
4881 assistant professor was perpetually broke for personal reasons not pertinent for this
4882 talk. We eventually had to pay Gerry about \$3,000.

4883 We tried to get MIT to take the case as plaintiff to argue against the law but to
4884 no avail although the attempt burned up a good deal of time and effort. There were
4885 seemingly endless discussions about whether MIT was really a private or a Land-
4886 Grant Institution and the lawyers at MIT were unwilling to concede that MIT was
4887 truly a private institution.

4888 In our meeting in the spring of 1965 Townes outlined the following course of
4889 action MIT would take.

4890 First, since we could not take a moot case to court to fight the legitimacy of the
4891 Oath, which is what we were now committed to do. I would be fired at the end of the
4892 term (Spring 1965) if I did not sign.

4893 The case would go forward in my name and not jointly with Watson since his
4894 prior signing of the law would render the case obscure. However, and this was
4895 Townes' idea, MIT would put me on the research staff in a non teaching capacity, if

4896 necessary, at the same salary until the case was decided. It would then be up to me, if
4897 we lost, to decide what to do subsequently. So here was, at least temporarily, real
4898 relief.

4899 As spring turned to fall all decisions were delayed until MIT finally decided to
4900 not take the role of plaintiff and that it would be entirely up to us. The MIT lawyers
4901 delayed a final decision until just before the start of the 1965 Fall term when it
4902 suddenly became clear that I would have to go to court to get an injunction in order
4903 to teach that Fall. MIT and my department chairman were adamant that if the
4904 injunction was not in place by (I am not kidding here) by precisely 2:30 pm on
4905 September 20, I would not be able to teach the Fall semester and there was no
4906 elasticity allowed in the time. We were scheduled in court for that day but there was
4907 no certainty we could get a judgment by 2:30. My interview with my department
4908 chairman was very unpleasant and he suggested an obvious way out of my troubles
4909 was to just sign the oath and get it over with. The department chairman, the
4910 estimable Ted Martin, conveyed to me the time limit with great severity in the
4911 presence of his executive assistant. I only found out recently that he had gotten into
4912 some political trouble for his own very liberal views as a young man and was
4913 probably reluctant to be involved again.

4914 Berlin sprang into action and we rushed by subway to Superior Court in
4915 Cambridge. I signed our papers requesting an injunction to prevent my being fired
4916 while on the Green Line and we used fellow passengers to witness our signature. At
4917 1:30 pm we had the injunction so, for at least a while, I could teach. At the same
4918 time we filed for an injunction in Superior Court to find the Loyalty Oath Law
4919 unconstitutional. As Berlin noted at the time, this would be passed directly for
4920 decision to the State Supreme Judicial Court.

4921 We needed to do some fund raising at MIT and I formed a committee of
4922 people, one from each department, who agreed, without committing themselves to
4923 the correctness of our case to act as funnels for the money that would be held by a
4924 Episcopal minister at MIT for Berlin's costs.

4925

NIT COMMITTEE FOR THE LOYALTY OATH FUND

<u>DEPARTMENT</u>	<u>COMMITTEE MEMBER</u>	<u>NUMBERS</u>	<u>ADDRESS</u>	<u>PHONE</u>
ADMINISTRATION	WILLIAM C. GREENE	50	14N-411	4255
AERONAUTICS & ASTRONAUTICS	LEON TRILLING	50	33-410	4329
ARCHITECTURE	HENRY MELLON	30	7-308	4405
ATHLETICS	CHARLES BATTERMAN	20	W32-129	4918
BIOLOGY	SALVADOR LURIA	55	56-421	4707
CENTER-INTERNATIONAL STUDIES	DONALD BLACKBER	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	E52-2546	5637
ELECTRICAL ENGINEERING	RICHARD ADLER	160	13-3006	4651
GEOLOGY & GEOPHYSICS	RAYMOND HIDE	30	54-520	3396
HUMANITIES	ROY LAMSON	80	14N-413	4447
MANAGEMENT	MICHAEL BROWER	100	E52-956	2655
MATHEMATICS	NORMAN LEVINDOR	90	2-365	4387
MECHANICAL ENGINEERING	RONALD FROBSTEIN	90	3-254	2240
MEDICAL	SAMUEL CLARK	30	11-110	4488
METALLURGY	**CYRIL STANLEY SMITH	50	14N-317	3722
METEOROLOGY	ERIK HOLLO-CHRISTENSEN	20	54-1722	6281
MODERN LANGUAGES & LINGUISTICS	NOAM CHOMSKY	30	20C-152	3221
NAVAL ARCHITECTURE	J. HARVEY EVANS	20	5-230	4355
NUCLEAR ENGINEERING	ELIAS CYFPOPOULOS	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	15	7-202	2917
PSYCHOLOGY	RICHARD HELD	25	E10-014	5745
RELIGIOUS COUNSELORS	MYRON BLOY	<u>10</u>	317 N.D.	2326
		TOTAL		1380
<u>ASSISTANCE FROM:</u>				
HUMANITIES	DAVID SCHALK PETER GURNEY BERT DREYFUS			
MODERN LANGUAGES	DAVID PERLNUTTER			
MANAGEMENT	DOB FARRAR			
PHYSICS	AL LAZARUS			
** METALLURGY: MAIL TO	THOMAS KING	50	8-307	3303

4926

4927

4928

4929 Once all these people were involved the word of what was going on got out and
 4930 became public. First, articles appeared in the Tech



4931
 4932 and the Harvard Crimson (copy of Crimson 10/11/65)
 4933

Loyalty Oath Is Attacked At MIT

By ANN PECK

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

The first test of the oath since the law was passed in 1950 was initiated by MIT when Professors Joseph Pedlasky, in whose name the litigation will be conducted, and William Watson refused to sign the loyalty oath, which is a pre-condition 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

4935

4936

4937 and the Boston papers (see below copy of Dalton article from Boston Traveler, a

4938 now defunct conservative paper) not all of them sympathetic.

4939

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4942

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.



PEDLOSKY

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.

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 nucleus 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.

4943

4944 Even the NY times carried a small story in which my identity was

4945 conflated with Bill Watson's so, young as I was, I had already been

4946 teaching all this time at Wellesley. It is hard for me to take newspapers
4947 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.

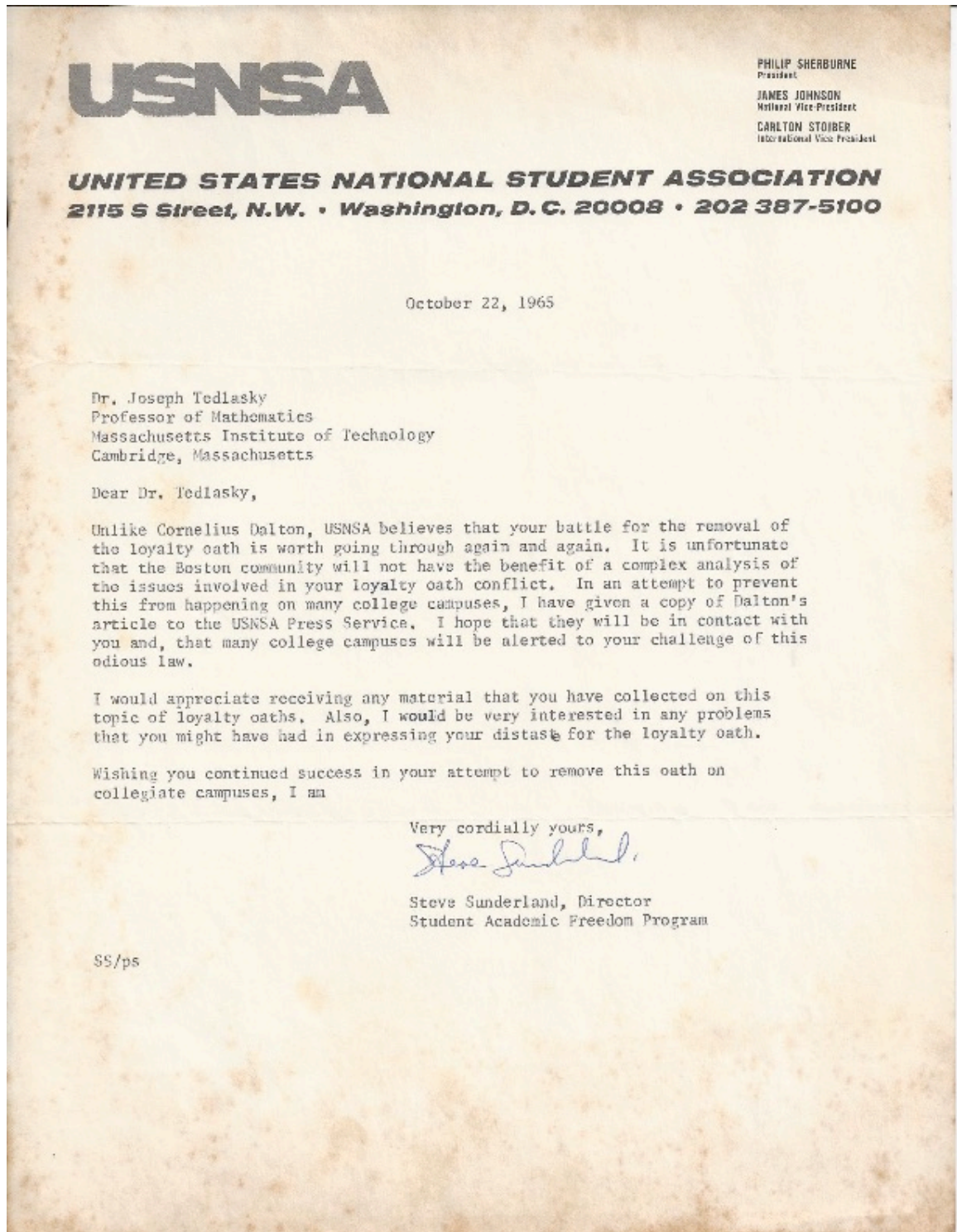
The New York Times

Published: October 14, 1965

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4949 When the story and the committee list appeared in The Tech one of the committee
4950 members hastened to write a letter to the paper protesting the portrayal of the list as a
4951 list of supporters. He did not wish to be identified as a supporter only as a good
4952 citizen facilitating the wishes of his colleagues. You get the picture. The Oath law
4953 was both trivial and its possible consequences frightening.

4954 After the story was in the papers I got a lot of mail, almost all of it supportive.
4955 Some even contained small checks as contributions for the defense fund.



4956



SOCIETY FOR SOCIAL RESPONSIBILITY IN SCIENCE

President
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Biomedical Electronics
37 E. Horman St.
Philadelphia 44, Pa.

Vice President
Norman E. Polster
Biomedical Research
855 Woods Rd.
Southampton, Pa.

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

Treasurer
Mark D. Shaw
Agricultural Engineering
1017 Houserville Rd.
State College, Pa.

Room 18-734
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 Responsibility 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
Program Chairman
MIT-SSRS

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

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THIS SIDE OF CARD IS FOR ADDRESS

Mr. Joseph Pedlosky, 2-375
Mass. Inst. of Technology
77 Massachusetts Ave.
Cambridge
Mass.

10/12/1965

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

WILLIAM G. HASLTINE
22 Fletcher Place
Belmont, Massachusetts 02178

4959

4960

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
WARREN, MASSACHUSETTS 02159

December 30, 1965

Dear Professor Pedlosky,

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

Yours sincerely,
William F. Field

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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)



THIS SIDE OF CARD IS FOR ADDRESS

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

2-375

So you want sign a loyal 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 cant
bring Hitler back. They
should deport you and
your kind to your beloved
Russia, you who owe so
much to America and lose
it so little you really
are not fit to live in
America

4968 “So you won’t sign a loyal (sic) oath. It is a shame so many decent boys died
4969 to save you from Hitler slavery. They must be turning over in their hallowed graves.
4970 It is a shame we can’t deport you and your kind to your beloved Russia, you who
4971 owe so much to America and love it so little. You really are not fit to live in
4972 America”

4973 It is probably at this point appropriate to mention that politically, I am left of
4974 center on domestic issues and probably considered slightly right of center on foreign
4975 policy and I really do love America. All during this process it was psychologically
4976 *impossible* for me to say that in public because in my own eyes it would have been
4977 equivalent to taking the loyalty oath and I wanted to make sure I kept the issue clean.
4978 That is, that I should be able teach and be accepted as a teacher without stating my
4979 loyalty just as a plumber at MIT need not declare his loyalty.

4980 As time dragged on with one or another delay the issue grew in size. A faculty
4981 member at Harvard refused to sign (Sam Bowles, the son of Chester Bowles) and his
4982 case was held in reserve in case we lost our own. The Faculty of the Harvard Law
4983 School were pessimistic about our chances and it is particularly delicious to see how
4984 wrong they turned out to be.

4985

4986

The Bowles Campaign

NO WRITER ATTRIBUTED

March 24, 1966

0 COMMENTS

EMAIL

PRINT

Like

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, an M.I.T. professor, is heard this October.

Pedlosky has contested the constitutionality 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.

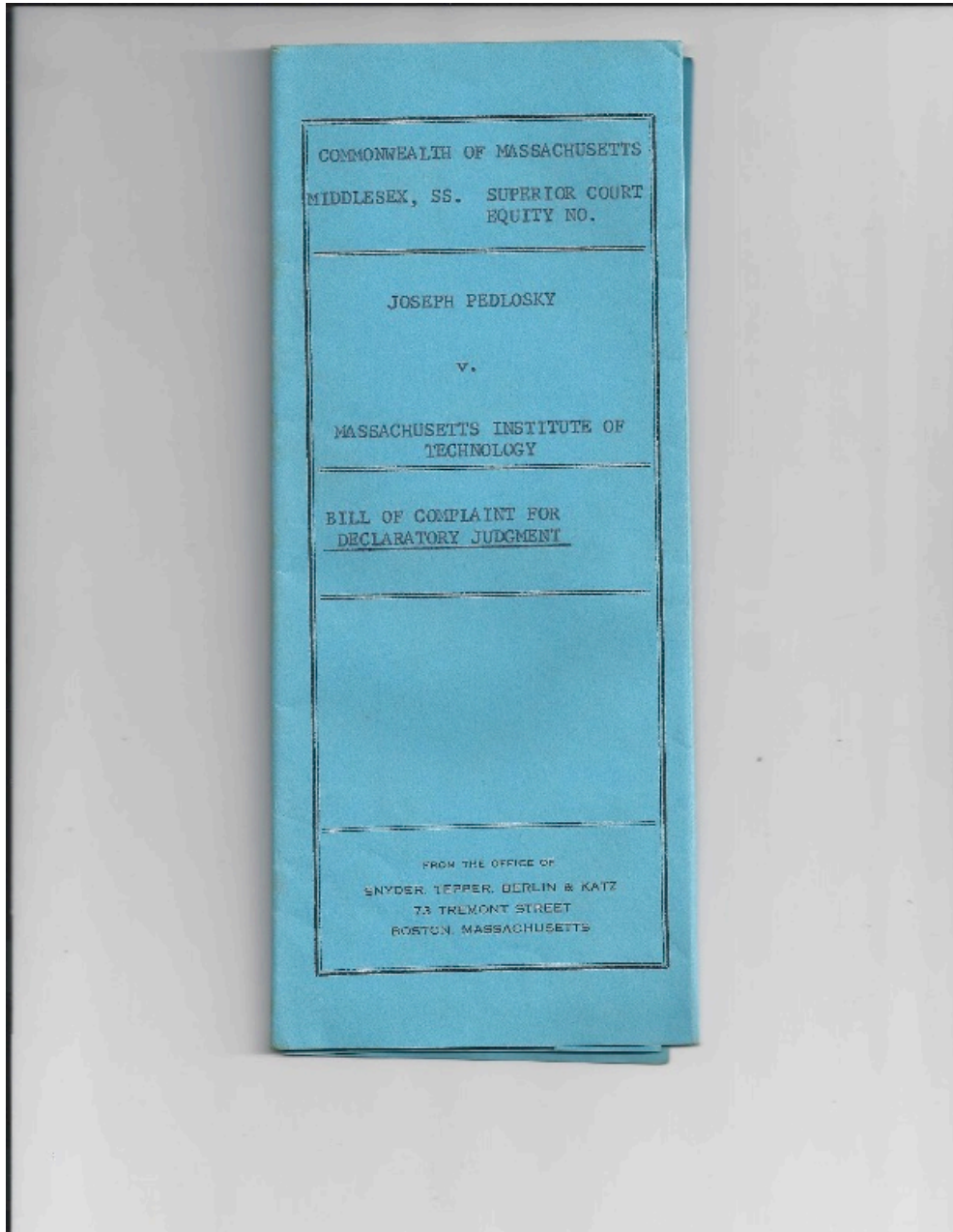
Pedlosky plans to appeal the case to the Supreme Court, and a long, perhaps futile, judicial struggle now seems inevitable. While the suit is being adjudicated--and in case it fails--Bowles, Pedlosky, and their supporters should consider the most effective remaining alternative: legislating the repeal 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

<http://www.the-simion.com/article/1966/3/24/the-bowles-campaign-pche-middlesex-superior/>

4988 The Massachusetts legislature was not inactive. Just to show we were not wasting
4989 our time the Massachusetts House voted 185-36 to **not** repeal the Loyalty Oath law
4990 even as our case was pending.

4991 So, after much delay the case was heard in the Massachusetts Supreme court
4992 December 8, 1966 over two years after I had declined to sign.

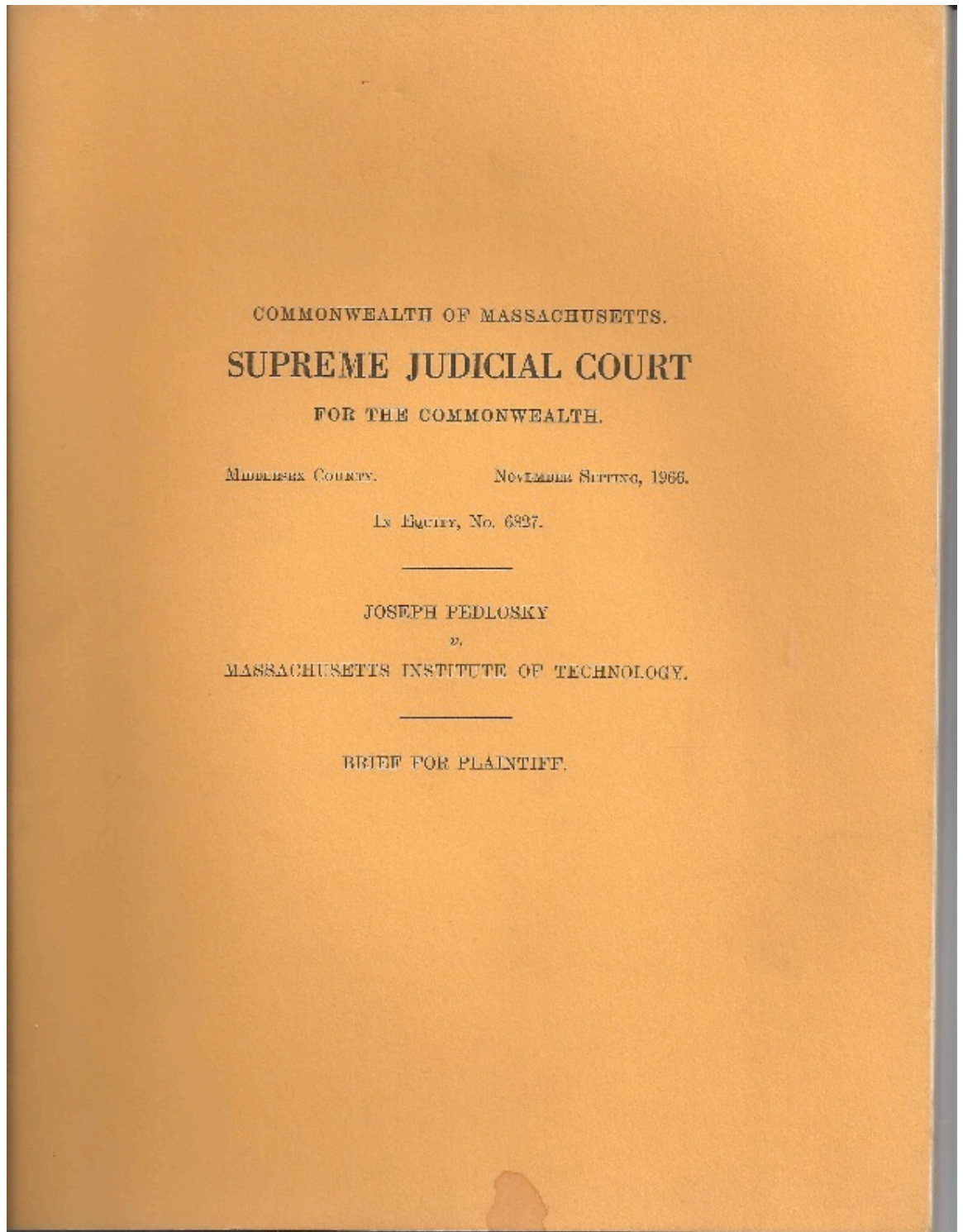


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Jesus F. Huidobro
Humanities Dept.
Dean Junior College
Franklin, Mass.
02038

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.

Sincerely,

Jesus F. Huidobro

J. F. Huidobro.

AMERICAN FRIENDS SERVICE COMMITTEE

Chicago Regional Office for Illinois and Wisconsin



431 S. Dearborn Street, Chicago, Illinois 60605

HAMMOND 7-2544

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

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

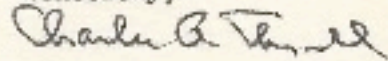
Dear Prof. Pedlosky:

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
Teaching Fellow

18 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

\$20

William A. Shureliff
Physicist
at the OSA, Harvard

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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

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 com-

munity 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.



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

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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 V. 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 5)

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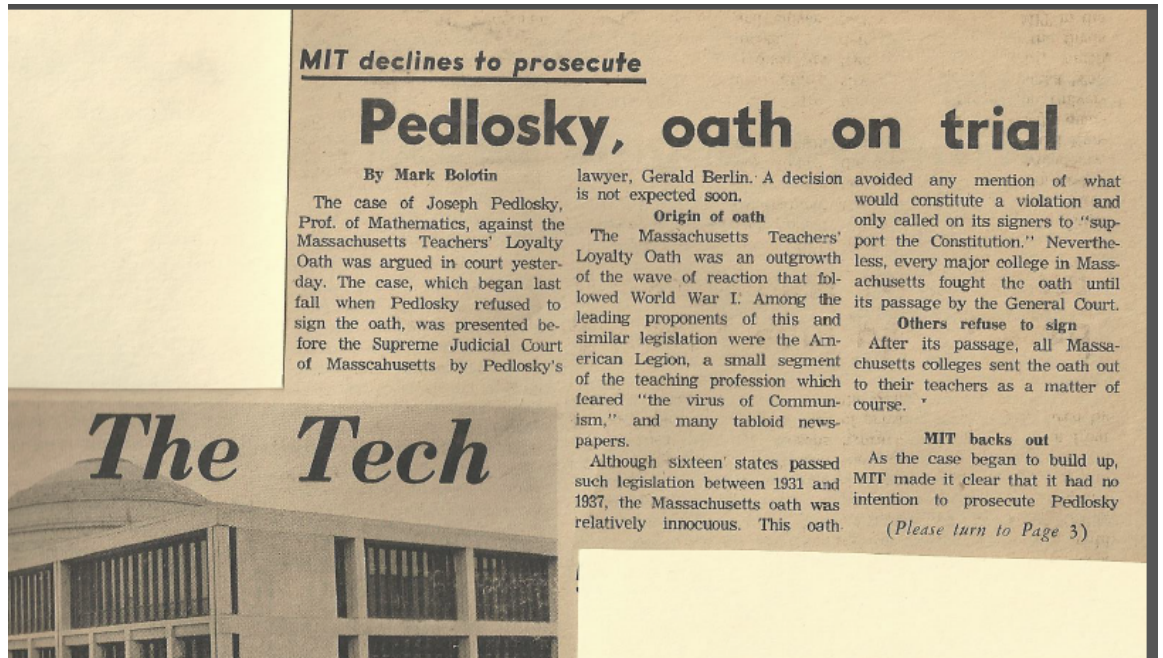
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5019 Pedlosky on trial!



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5022 This, I have to say was one of the moments when I was most proud to be an
5023 American. The courtroom was packed and I knew very few people there. I sat way
5024 in the back to try to see everything. When the Justices entered and the case began I
5025 had a profound thrill. I was an unknown, with no money and no influence and this
5026 whole process, the whole argument, unfolding before my eyes was happening
5027 because I had said "no" to a law I thought was unjust. Win or lose I felt so good that
5028 the machinery really was working the way I had always believed it would. I can
5029 never be cynical about the "system" after that.

5030 MIT refused to argue for the law so it was defended by the State Attorney
5031 General's office. The ACLU, two years after turning me down, filed a friend of the
5032 court brief and Gerry Berlin argued our case eloquently basing it on our view of its
5033 unconstitutionality because of its coercive demand for an oath of loyalty and for its
5034 legal weakness for its vagueness. How, for example, could I know if it could be
5035 determined that I was teaching to the best of my ability or if in making a statement
5036 about a political disagreement that I was not supporting adequately the constitution

5037 of the state of Massachusetts (whose provisions I admit never to have read, have
5038 you?). The arguments finished, I left the courtroom and waited.

5039 I have to remind you that in this two-year period I was still teaching and
5040 carrying out my research. Indeed, my work was a great solace during this time and
5041 was the basis of a deep friendship I developed with my colleagues Harvey
5042 Greenspan and Victor Barcilon. I recall that Harvey, who was my immediate mentor
5043 or boss, and who had a reputation for being a really hard case, asked me at the
5044 beginning of the whole deal whether I was really serious about carrying it through to
5045 the end. I said *I thought so*. He grunted what I took to be approval and we moved on
5046 to our work on Rossby waves and spin-up but he never ceased to support me with
5047 generous sensitivity.

5048 Finally, at the beginning of March 1967 I got a phone call from a reporter on
5049 the *Crimson* (they were *really* plugged in) who told me before anyone else could
5050 reach me that the Court had decided to throw the law out on the vagueness issue.
5051 While being a tad disappointed that they did not address the issues of the coercion of
5052 statements of loyalty I was delighted, simply delighted that we had won.

TEACHER OATH LAW IN BAY STATE VOIDED

BOSTON, March 2 (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

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The Weather
Partly cloudy, high 40's

vard Crimson

BRIDGE, 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."

The 500-word decision, written by Chief Justice Raymond S. Wilkins '68, 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.

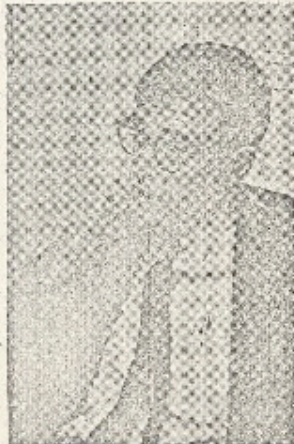
The limited scope of yesterday's decision produced speculation that the Massachusetts General Court (state legislature) might move to draft another teachers' oath without the illegal clause. "I don't know whether this is the first round or the end of the fight," said Gerald Berlin, attorney for Pedlosky.

Spit in Court's Eye

Mary D. Newman, Republican state representative from Cambridge, doubted



SAMUEL BOWLES
Interested Observer



JOSEPH PEDLOSKY
Winning Litigant

Cambridge Police Begin Square Button Struggle

By ROBERT J. SAMUELSON

Cambridge police have begun a low-key campaign to cut down on the sale of buttons that consider to be obscene or incite a riot. Since last Thursday, he said, police have visited the store four times, bringing a file inspector with them. The

State's High Court Kills Teacher Oath

The controversial 36-year-old teachers oath law is invalid, the Massachusetts Supreme Judicial Court ruled today.

The high court, in a decision written by Chief Justice Raymond S. Wilks, 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 1955 and received temporary injunctions barring M.L.T. from re-

fusing 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 \$100 fine penalty provided by the teachers oath law applied only to a violation of the oath regarding 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 physics 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

1919 as the result of a campaign led by the then Rep. Thomas Duran, of Dorchester. The law has been under attack ever since.

In his argument in behalf of Dr. Pedlosky before the Massachusetts high court, Atty. Gen. A. Bar's said,

"The oath was the product of a struggle of incalculable ferocity between an aggressive legislative and community segment brimming with patriotic fervor, intolerance of dissent and fearful of subversion in the classroom, on the 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.L.T. in July 1955 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 Woods Hole Oceanographic Institution at Woods Hole, Mass.

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.

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CLOUDY,
WARMER
FULL REPORT
SEE PAGE 20


Record American
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

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High Court Voids Oaths Of Teachers

The Massachusetts Supreme Court ruled invalid Thursday the state teachers' loyalty oath. In a unanimous 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, 1907

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 Peirce, happy as he too is with his victory, is a little 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.

5079 There were other satisfactions:
5080 The same vice-president who had threatened to fire me apparently had an
5081 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.

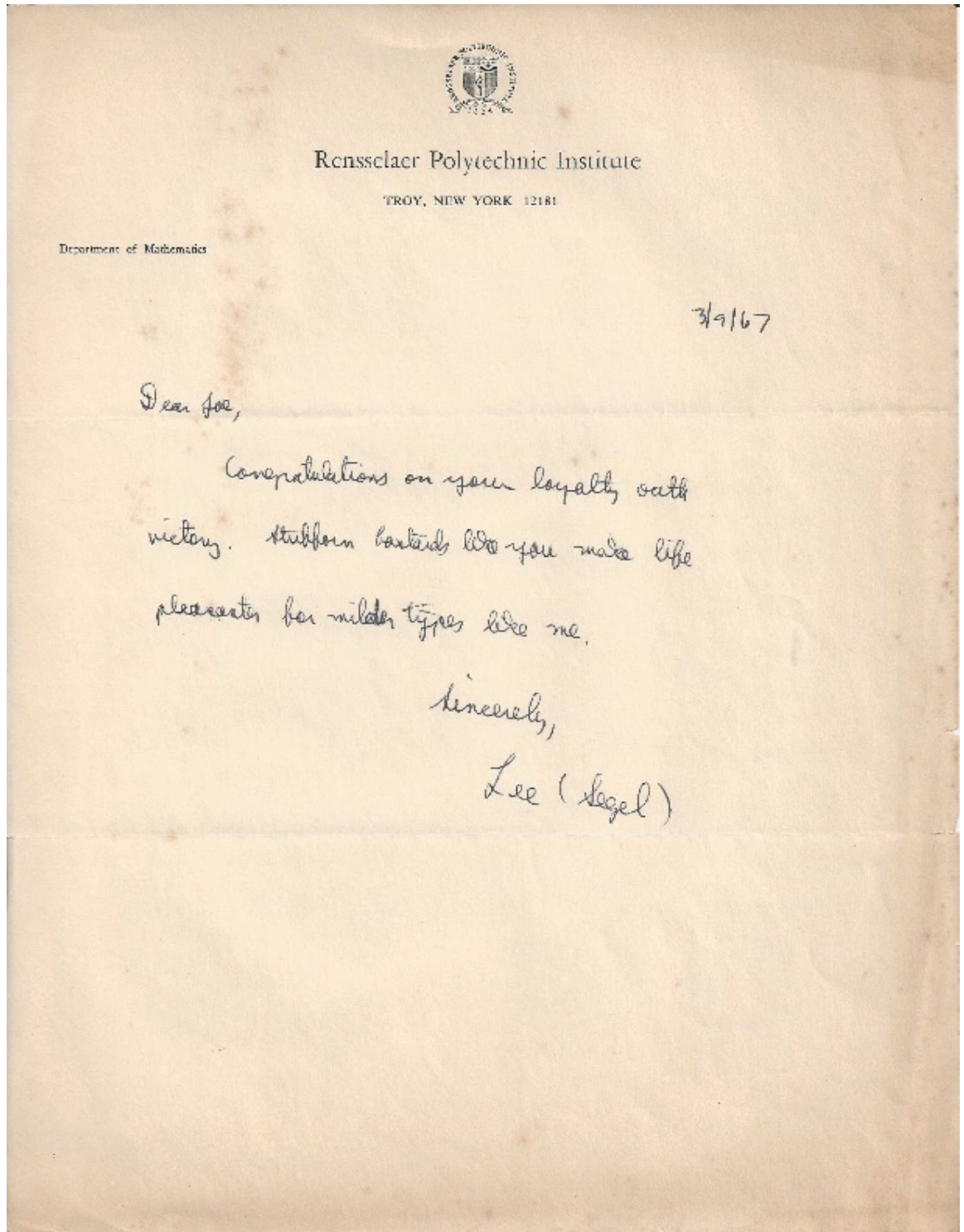
Sincerely yours,

M. G. Kispert
Vice President, Academic Administration

5082

5083

5084



5085

5086 Colleagues at other institutions offered congratulations (Segal, letter) and I even got
5087 a congratulatory letter from an old girl friend (that since I was married, I never
5088 answered).

Dear Joe,

You've come to mind
many times over the past few
years, but I never expected
to see you pop up on the
front page. So here is
one more piece of correspon-
dence to add to the old
mail you must be getting
like a mad.

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.

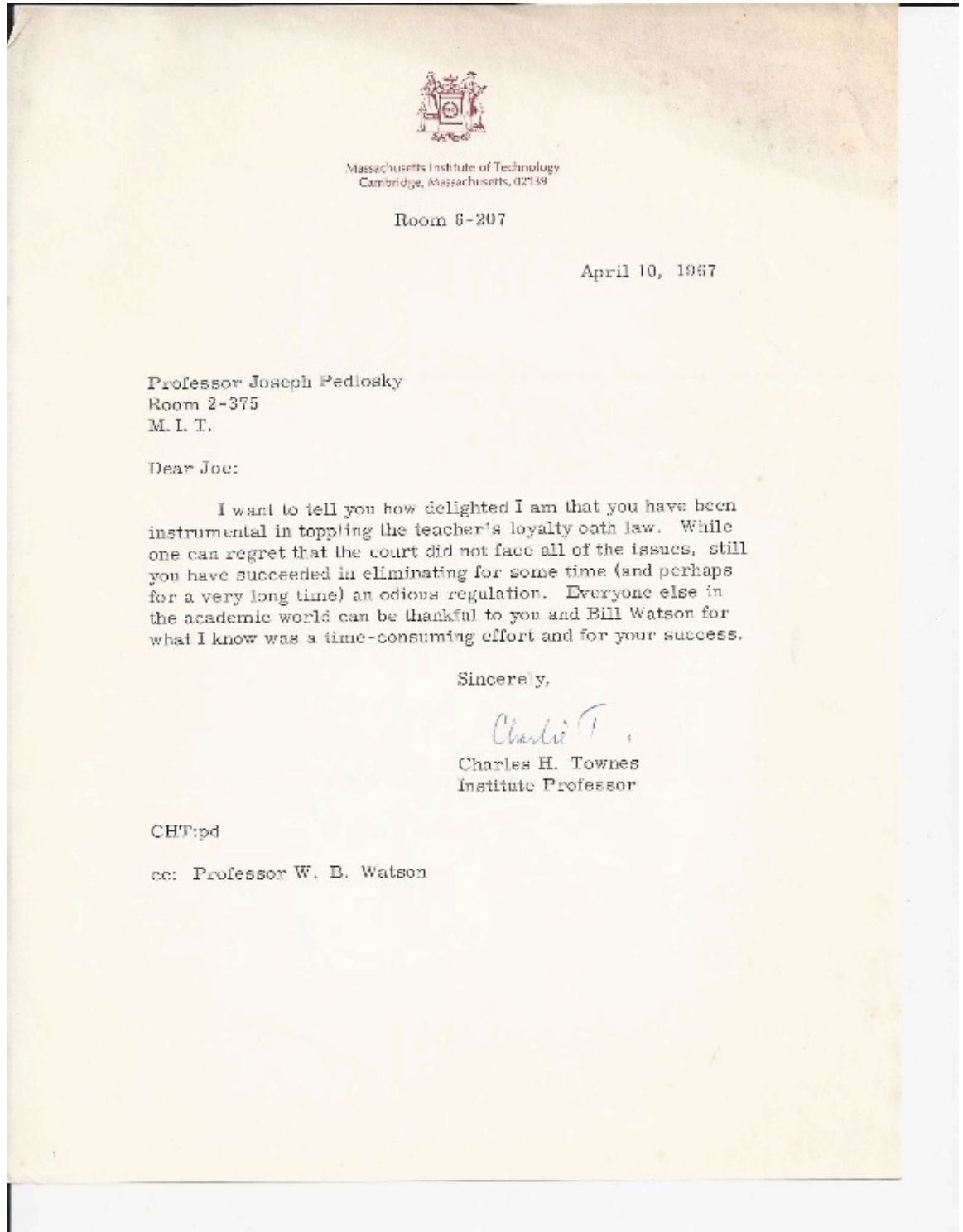
Imagine my embarrassment
if you're not you, but
how many Joe Pedlocks
can there be, just as how
many could there be of me?

Congratulations on
winning your case!

Best regards,

Carl S. Hall

5091 But, most presciently, I got a letter from Townes reminding me (us?) that such
5092 victories are never permanent.



5093

5094

5095 Even then the story was not clearly concluded. Some members of the legislature
5096 were eager to start the battle again. The notorious Louise Day Hicks of anti-busing
5097 fame supported a new Oath law. The vice chairman of the legislative committee for
5098 the American Legion, Jeffery Moulton, (quoting a Tech story), said he “represented
5099 80,000 veterans, ‘Give me a child for two years and I’ll give you a Communist. ‘
5100 Moulton said in an alleged quotation from Marx and Lenin. The public has a right to
5101 watch the teachers who build our “citizens of tomorrow”. At that time their efforts
5102 died.

5103 Was it worth it? Thinking about Townes’ letter, it *has* been now a very long
5104 time since that victory and maybe the lesson has faded. In the present climate of fear
5105 and with Federal legislation of the type of the Patriot Act, a Palmer-like Attorney
5106 General, and with the imprisonment of American citizens without trial or access to
5107 lawyers it is not clear that this particular controversy is ever permanently resolved.

5108 So, I will leave the answer of my question to you. Or better yet, pose the
5109 question: what will *you* do next time around?

5110

5111 **Appendix B**

5112 **The Ewing Award Response**

5113 10-25-11

5114 Thank you, Liu, for that very kind introduction. I am, of course, deeply honored by
5115 the Maurice Ewing award. The vast scope of Maurice Ewing's contributions can only make
5116 me feel humble in comparison.

5117 As always, I am grateful to my mentors: Jule Charney, Melvin Stern, Erik Mollø-
5118 Christensen and Harvey Greenspan. Some of them are, alas, now beyond my power to thank
5119 them directly. My gratitude to my colleagues at MIT, University of Chicago and Woods
5120 Hole extends to too many people to be able to name them all at this time but my debt to
5121 them is immense. I am delighted to express my gratitude to all my students for the pleasure
5122 to have seen them flourish as keen, independent scientists and particularly tonight to
5123 Zhengyu Liu and Paola Cessi who jointly nominated me for this award.

5124
5125 Now, though, I want to thank a very special person, my wonderful wife Holly. I have
5126 observed in the past that usually thanks are given to a spouse for putting up with an absent
5127 partner who spends many late nights in the lab or weeks at sea. As a theoretician, and one
5128 with limited mental stamina, I rarely worked in the evenings or on weekends, although
5129 brooding over a problem, or lack of one, can be full time. We know the cycle of theoretical
5130 work: formulation, painful perplexity, the occasional epiphany and the repeat of the cycle.
5131 What is less commented upon is the theoretician's anguish in searching for a new, good
5132 problem. The fear that the last good problem is in fact the *last* good problem is oppressive.
5133 Reassurance that one has always found a new problem before is not convincing since that
5134 speaks to history and not to the future. When Holly becomes aware that I am particularly
5135 grumpy, she knows it is because I am in that oneiric, half awake state, of confusion and
5136 anxiety. Somehow, for all these years, she has patiently helped shepherd me to the other side
5137 of that abyss where I can emerge into the desired state of constructive perplexity. So, I think
5138 she deserves most of the credit this evening.

5139
5140 One final thought. The calculation of the probability of intelligent life in the universe
5141 is a difficult one. A nearly infinite number of possible host planets and a near zero

5142 probability of intelligent life on any one of them means that the product could indicate a
5143 single event. Ours. After all, even here, the lengthy age of the dinosaurs produced no
5144 paintings of sunsets, no formulation of the Navier-Stokes equations (we had to wait for
5145 Navier and Stokes). If that is so, and we are alone in that regard, our responsibility is
5146 immense. It means that the universe is only conscious of itself by our agency. If we were not
5147 here the universe would be like a cinema showing Casablanca on an endless loop to an
5148 empty theater. It is only through us that the universe can be self-aware and if we were to
5149 blow ourselves up or render our planet uninhabitable for anything but the cockroaches the
5150 universe might as well be empty.

5151 We are often asked to describe the larger consequences of our work. I know of
5152 nothing more noble and important than serving as the self-awareness of the cosmos. Further,
5153 it is our *communal* effort, and especially for us as scientists, it is one we need to take as a
5154 sacred trust. I am proud to be part of that effort and to place my small contributions into that
5155 mosaic of understanding we are constructing together.

5156

5157 Joseph Pedlosky

5158 Scientist Emeritus

5159 Woods Hole Oceanographic Institution

5160 Woods Hole, MA 02543

5161

5162

Appendix C

5163

Arons Award for Teaching and Mentoring

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5165

September 15, 2005

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Acceptance remarks for the Arons Award September 24 2005

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Those of you who knew Arnold Arons, and his fierce commitment to

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teaching the meaning of things and not just the names of things, will appreciate

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how proud and delighted I am to receive the Arnold Arons award.

5171

I have many people to thank; many wonderful teachers and mentors have

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helped me on my way but first, I would like to say that to get any satisfaction from

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teaching you need to have good students. So, I would like to thank the Joint

5174

Program students, past and present for having made this part of my life so

5175

rewarding.

5176

Significant mentors and teachers for me go way back and I would like to

5177

mention just a few.

5178

Melvin Stern was my first advisor in the Geophysical Fluid Dynamics

5179

program here in Woods Hole. He combined brilliance and kindness and helped set

5180

me on my present path. My inspiring thesis advisor, Jule Charney, one of the two

5181

greatest scientists I have known, showed me how treating *all* his thesis students

5182

with dignity as true colleagues was consistent with challenging each to do

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fascinating work at his level. Harvey Greenspan, a superb mathematician, was my

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first mentor after my degree and showed how skepticism when combined with

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warmth and support could be a spur to greater creativity. Then when I moved to

5186

the University of Chicago my department chairman, Julian Goldsmith, showed me

5187 how it was possible to be a strong, shrewd and realistic academic administrator and
5188 remain a *mensch*, i.e. a person of warmth and integrity.

5189 A principal reason I came to Woods Hole, like many before me, was to have
5190 the chance to work with Henry Stommel, the second of the two stellar scientists I
5191 have known, and from whom I learned so very much. Nevertheless, I would have
5192 to say that I found myself really mentored by the *whole* Physical Oceanography
5193 department. Each of my colleagues has done his or her best to make me a better
5194 oceanographer and I am profoundly grateful to them as I am to all my mentors.

5195 Now, I mentioned Arnold Arons' *fierce* dedication to teaching. Why fierce?
5196 After all, many people think of teaching as a secondary, even an inferior activity to
5197 our research. Indeed, some people who don't understand how important teaching is
5198 to our own research have actually cautioned young scientists not to get involved in
5199 teaching too soon (whatever "too soon" means). Those of us involved in teaching
5200 *and* research know just how valuable teaching is to our own thinking but a
5201 commitment to teaching seems to me even more natural than that pragmatic
5202 explanation would suggest.

5203 I was trying to find a way to express that thought when I recalled a book I
5204 was lent by a friend and neighbor that consisted of a series of essays presented at a
5205 conference that was celebrating the 50th anniversary of Irwin Schrodinger's book
5206 "What is life?". You may recall that Schrodinger, the famous physicist, posed as a
5207 basic question what the mechanism was for the transmission of hereditary
5208 information from generation to generation, that allowed both change and stability.
5209 The biologists at the conference were, of course, celebrating the great progress

5210 made in our understanding of the mechanism of heredity, since Schrodinger's
5211 challenge, due to the Crick/Watson DNA revolution in understanding the genetic
5212 basis for heredity.

5213 Two of the speakers, Manfred Eigen and Jared Diamond pointed out that
5214 although the DNA mechanism is one we humans share with all living things,
5215 humans have developed another mechanism, unique to our species, for the
5216 transmission of accumulated information and that is speech. Diamond has pointed
5217 out that the great spurt of progress our species made about 70,000 years ago in
5218 development is only weakly correlated with brain size but is more likely connected
5219 with the development of our ability to speak complex thoughts. We are unique as a
5220 species in that I can tell you about another member of our species who lived
5221 hundreds of years ago, on another continent, who wrote music that is sublime and
5222 tell you just what it is. We can pass on how to make fire, how to fuse metals, how
5223 to plant and irrigate, how to paint portraits with oil and light itself and how we can
5224 use potential vorticity to explain the ocean circulation. We can *teach*.

5225 Seen this way, teaching is what makes us fully human. We *are* the teaching
5226 species. Teaching is what has allowed our species to survive and thrive in our
5227 environment. Looking at the challenges ahead of us we surely must realize that
5228 teaching, real teaching, must become even more valued as vital if we, as a species,
5229 are going to continue to survive and thrive. In a country like our own, in which
5230 more people believe in the folk tales of their bible than in the evidence for
5231 evolution or the origins of the universe, we clearly have our work cut out for us. It
5232 is not going to be easy.

5233 Arnold Arons was fully and fiercely aware of all of this and this is why I am
5234 so deeply honored by this award in his name. I treasure it. Thank you.

5235

5236