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From the Cold War to Global Warming: A Scientific Odyssey



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## A Scientific Odyssey

Charles F. Kennel

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**Death will come from the sky**
Consolidated-Vultee B-36


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*To my unhappy parents,*
*Archie Kennel and Betty Fitzpatrick,*
*whose loving discord conditions my life,*
*and*
*to my wife,*
*Ellen Jay Lehman,*
*without whom I have no life*

# Table of Contents

This book tells my personal story, but it also tells the stories of institutions (like NASA, UCLA, and the Scripps Institution of Oceanography) and scientific fields (like plasma physics and climate science). To aid readers in locating the material most interesting to them, this table of contents is annotated with brief overviews of the material in each chapter.

# Prologue: Remembrance of Things Past

*When to the sessions of sweet silent thought*
*I summon up remembrance of things past,*
*I sigh the lack of many a thing I sought,*
*And with old woes new wail my dear time's waste:*
*Then can I drown an eye, unused to flow,*
*For precious friends hid in death's dateless night...*

*William Shakespeare, Sonnet XXX*

Occasionally the memoirs of persons otherwise obscure are of interest because they convey something of the nature of the times. In my case, neither my accomplishments nor actions made a perceptible mark on either science or history, but history made its mark on me. Psychology, education, and circumstance made me a witness to the transformation of the central preoccupation of international science from avoiding world military catastrophe to avoiding global environmental catastrophe. A deep historical current carried me on a scientific odyssey, from astronomy at Harvard to thermonuclear fusion research at Princeton to space plasma physics at UCLA to satellite earth science at NASA to the ocean and climate sciences at the Scripps Institution of Oceanography to climate change and sustainability policy at UC San Diego and the University of Cambridge.

The historical currents carrying me also carried everyone else involved with the disciplines I worked in. Each of us had a personal psychological, philosophical, and political orientation to the work. At intermediate levels of depth and complexity it defies human capacity to trace the intermixtures of influences on our collective perceptions and the social decisions based upon them. But at the level of cultures, countries, and historical eras, historians have invented a term, *zeitgeist* (spirit of the age), meant to describe the pervasive attitudes a given generation brings bring to collective decision-making at a given historical and cultural moment. In the period I live in, we are transitioning from an era when the pervasive fear had been global military catastrophe to one when it is equally global environmental catastrophe. We are going from an era where war provokes environmental disaster to one where environmental disaster provokes war.

I have written an account of my personal odyssey in the language of general scientific discourse used when scientists convey the meaning of their research to a scientist in another discipline. This language presumes some familiarity with the basic concepts of the physical, biological, and information sciences, and with how the disciplines are organized institutionally. I avoid the specialized languages that every discipline deploys in private to discuss ongoing research. Results on the frontier are difficult for scientists even in neighboring subspecialties to comprehend at the depth that had been required to achieve them. Even within the disciplines, journal editors conduct the peer review process in part in the language of general scientific discourse since evaluation of societal implications is part of the review process. Funding agencies, university leaders, policy makers, and other decision makers all use the language of general scientific discourse. In most countries today, it is English.

The original ideas especially singled out by society for value come from somewhere. Einstein and the French mathematician Henri Poincaré used to say their best ideas inhabited them; ideas were invaders from outside. That may be, but personal relationships condition people to respond to certain ideas and not others. I like to think it is not narcissism's display but respect for the mystery of unconscious thought that drives me to disclose emotional reflections often kept hidden in a scientific biography. I relate aspects of my two marriages because this most fundamental form of human intimacy shaped my world of thought, motivation, and action. This aspect is often passed over in scientific biographies, perhaps because the institution of marriage is so ubiquitous that its commonplace details seem unimportant to scientific discovery.

Historians have long since abandoned the notion that science advances through the inexorable logic of ideas, that great ideas have few precedents, that great individuals originate great ideas via miraculous illumination. Ordinary people have had great ideas and great people have had ordinary ideas. Scientists are human like everyone else and respond to historical forces like everyone else. Scientists *are* human, and their human brains have evolved to respond to personified ideas. Scientific ideas were never abstract objects that I could rearrange in my mind like tinker toys. Ideas came to me connected to human beings. Many people come in and out of the story to follow. Some I stop along the way to characterize, others I leave to the reader to find what is missing. Yet were theirs my own life, I would resent how incompletely are characterized these others, or even worse, how many are left out when others are kept in. A murmuring crowd of the left-outs reproaches me; surely there must be a better way to capture where the precious flow of life took each of them who wove the fabric of my life.

Do not imagine for one moment that what follows is a complete or objective account. This is more like an after-dinner conversation than a scholarly disquisition. It recounts emotionally salient events that managed to survive in a once good memory now clouded by old age and biased by later experience. Events that achieved psychological salience were successes fertilized by the dead bodies of failures that memory mercifully buried. Writing one's own history elicits unattractive emotions: pride, grandiosity, solipsism, narcissism. An honest effort to be honest does not eliminate unconscious deceit. Self-explanation easily yields to self-justification. Words flow when writing gives pleasure, which makes self-deception an ever-present temptation. It is not possible for memory to reconstruct the feel and flow of events whose images in the mind's eye are framed by impressions acquired after the fact. The coherence of identity would not hold if artistic license did not occasionally rearrange once distinct events into more amenable patterns. Even were memory reliable, one person never sees enough. My excuse for adding to some poor scholar's burden is that I can provide what others cannot: what I remember now of what I thought at the time about events retrospectively important to me.

Taken separately, the pictures and the words I have included tell differently biased stories. Written memories are subject to the personal biases just recounted, whereas pictures, said to be worth 1,000 words, are subject in addition to selection and availability biases that reflect society's values and technological state. Because of that, there is much more in the photographic record from the twenty-first century than from the twentieth. Myself, I tend to trust the words more for intellectual issues, and pictures more for emotional state at a given moment in time. However, please realize that the events of the 1960s and are as vivid to me at those of the 2010s despite appearances.

There were occasions when visibly anxious UCLA graduate students would approach me before their oral examination for the PhD in physics. Assuming my most avuncular tone, I would reassure them that the anxiety of anticipation is constructive. Moreover, they should not over-interpret the anxiety provoked during the examination itself. Once one examiner starts a line of questioning, the committee will ask more difficult questions until you fumble one. Total collapse will seem near. Every line of questioning is ending in your failure. You will think a feeding frenzy has begun. What you feel is despair. *What your committee cares about is not your despair but how far you got, the things you got right before you failed.* That is the nature of a life in science and certainly mine; every answer you find is provisional, every quest for answers ends in more questions.

Rare is the author who does not have friends, neighbors, and family somewhere in mind when undertaking a memoir like this. There is much here that is personal, too, assuming they are curious about how my background, education, and life outside science affected my life in science, and how my life in science affected the person they knew. I hope the personal makes a story out of what otherwise is a catalog of events.

Charles F Kennel,
Started Ramona, California, July 13, 2020
Completed La Jolla, California, July 13, 2023

# 1

# On the Nature of Things

This was the voice of the one who did this to me. Waves—no, paroxysms—of fear, hot and cold fear, ran to my toes and back. Cringing like an abused dog I crawled to the safe space under the desk, between the legs of my mother and another person. I could see the crease of his trousers. Their disembodied voices floated in the air, quietly, darkly, ominously, talking above me. Mother's bright-edged voice I knew well. The other was the clipped voice of an angered god straining to be civil—unfeeling, unyielding, moralistic, imperious. The voice of Marvin Beatty, New England pediatrician.

This Marvin Beatty believed that infants need discipline from their first moment outside the womb. He had instructed Mother that I was to be fed every four hours on an exact-to-the-minute schedule, given precisely fifteen minutes to eat, and then the food be taken away even if I had not finished feeding. *That was the only time she should ever touch me*. There should be no other stimulation, no radio, no nothing. She was to leave me prey to thought in a darkened room, diapers changed only during the desperate minutes of feeding.

No wonder I panicked when Mother told me we were going to go to Dr. Beatty for a two-year checkup. There I heard her admit she had been unable to stay with the doctor's orders. She had spent hours outside the closed door of my room, weeping, calling to me, talking with me. Was this wrong, to talk to me? I had spent my infancy in that darkened room every day, my security depending upon figuring out what was in the world I had been sent into. I could hear her weeping, her voice a message from the world outside, the fifteen minutes of desperate sociability a taste of pleasures sure to be taken away.

Each time there was the terror of being sent back into the void. In the long sessions of silent thought, I sought form in the featureless, in the fleeting minutes of human contact—sweet tangibility. Return trips to the void forced a reimagination of the world, and each return from the void, a precious opportunity to check whether the same me had returned. You could never be sure.

Dr. Beatty had been right about one thing: I got discipline. I grew up intellectually precocious and physically backward. I could talk before I could walk. There is a family story about the day I revealed I could walk. The adults had been ribbing me about using big words (submarine) while still sitting on my fat buns. Their noodging really made me sheepish. When I thought they were not looking, I practiced walking across my playpen. One day they caught me at it. To much fanfare, I was lifted out and made to navigate the big space beyond the playpen on my own. I learned to read soon after that.

Talking, listening, and reading were ways to find out what was outside the darkened room. Plato's allegory of the cave makes sense; it tells where knowledge of the world comes from. My cave, that darkened room, silent but for Mother's weeping and calling, was where the journey from womb to world, wet to dry, enclosed to open, had deposited me. Mother's anguished voice was the only connection to whatever was outside the dark room; her words were clues to the unseen.

I do not recall my father, Archie C. Kennel, Jr., ever being in the darkened room. He had obviously been present when I was conceived during the holiday season of 1938, but I cannot remember that. I was disconnected from the security of the womb twelve days before war began, on August 20, 1939, and I have no clear memories of my father until after he had gone away on war assignment with the Lockheed Aircraft Corporation. I do, however, have a fleeting memory of an urgent male voice interrupting a Sunday afternoon radio concert with news of Pearl Harbor in 1941. I was a little over two years old. I am told I asked, who are the Japanese? Bad people, my parents said. Later, after we moved to Abington, I got addicted to war news; I would sit on the floor next to the big console radio to hear vivid descriptions by announcers like Fulton Lewis, Jr. Their words connected me to a world full of danger. I would also spread the Boston Herald on the floor to look at the maps. The maps showed our side in white, Hitler's in black. Every day, I checked whether the black splotch was getting bigger or smaller. There were good days and bad days. I remember the Battle of Stalingrad and in late 1944 being surprised that the Germans were still able to mount a ferocious counterattack—the Battle of the Bulge.

I must have been going on three years old when Mother and I moved to Abington, a small town in southeastern Massachusetts, to move in with Gran and Grandpa Charlie Fitzpatrick, and my favorite Aunt Charlotte. My father came home to Abington from time to time during the war. His visits were occasions for desperate sociability like the ones of my infancy. Of course, he came home for Christmas. I remember waiting upstairs in the dark one Christmas Eve, eavesdropping on my father and mother whispering as they set presents around the Christmas tree. After they had gone upstairs to bed, I sneaked downstairs to race his present, windup model cars, around an oval metal track. (He loved mechanical things.) I stayed up playing with them until sunrise. My parents said they never heard the sounds of the windup cars on the metal track downstairs, but I was sure they had.

Two of my father's other visits stick especially in memory. One summer day, just before he was to arrive, I subjected his balsa wood and paper model airplanes to a rigorous test procedure. Their propellers were driven by rubber bands that you had to wind, and I wanted to see how far the airplanes would fly for a given number of windings. I learned later that good engineers test things to destruction, and that is what I did. I crashed them all into the wall of the garage adjoining the back yard of the Abington house. We both cried when he saw the pile of balsa wood and paper that had been things he built with his own hands. He asked why I did it, but I am pretty sure he knew I was angry about his being away.

My father also came home for the birth of my brother John (December 1, 1943) during a wild hurricane. I was four years and a couple of months old. That time my father went directly to the hospital in Boston. I was left alone in the dark, enclosed verandah of the Abington house, no electricity, nose pressed flat on the rain-streaked window. I was straining to hear what the wind, rain, and thrashing trees were saying about my new brother. What could be so important that my father went to the hospital first instead of coming home to me?

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**Figure 1-1: 30 Morton Street in Abington, Massachusetts was the center of my world during World War II. Mother and I lived with her family when my father had a wartime assignment in Dayton, OH with the Lockheed Aircraft Corporation. I recall pressing my nose against the first floor windowpanes in anguish on the day that my brother, John, was born, December 1, 1943, during a hurricane. I asked the wind and the rain why my father went to the hospital to see my new brother instead of coming to see me. Photo by Ellen Lehman.**

Number 30 Morton Street, Abington, Massachusetts, was the center of my world during the war. My uncle Jim Queeny came to visit my Aunt Charlotte when he was stationed in Newport, Rhode Island, for training. They were in love. Charlotte did not go to Wellesley like her older sisters, Mother and Aunt Mary; wartime added urgency to every courtship and she married Jim in Jacksonville, Florida, when she was eighteen. Charlotte came to live with us while he was in the Navy. I can still visualize the snazzy blue Ford convertible Jim kept in Abington while he was in the service. The whole family had pooled its gasoline rationing coupons so Charlotte could commute to her job at the Toll House Restaurant in Whitman, Massachusetts. I bet she was a really, really glamorous hostess; I never went to the restaurant, but she brought me the first chocolate chip cookies I ever tasted: Toll House cookies. On her afternoons off, Charlotte would sun herself in the back yard and supervise my work on the Victory Garden.

I hung on Jim's every word when he talked about his life in the Navy. He chose glamour over safety when he volunteered for the PT (patrol torpedo boat) service, same as future President John F. Kennedy. After training, Jim was based in Portsmouth, England, where his mission was to rescue Allied flyers downed in the English Channel. Sometimes the German plane that shot down our plane would hang around to have a go at the rescue boat, but the PTs went out anyhow. Jim also played a role in history. On the early morning of June 6, 1944, before the Normandy invasion got underway, Jim's boat idled offshore for several hours under fire from the French coast, hovering above British divers who were disarming mines that had been set overnight.

Jim spent a long time with us in Abington after the Normandy Invasion while *en route* from England to the Philippines, where he was to support insurgent groups fighting the Japanese. His sailors mounted machine guns from downed B-24s on the stanchions of their PTs and dashed into inlets behind the Japanese lines, guns blazing. After Jim transferred to the Pacific, I searched the war maps of the Far East with the same hunger as I had the maps of Europe and Russia a year earlier. Surprisingly, I cannot remember what I felt about the atomic bombs at Hiroshima and Nagasaki, but I remember photos of the Japanese surrender aboard the battleship *Missouri*.

My world came to an end when the war came to an end. Life would be less interesting with no war maps in the newspaper. I do not remember my family rejoicing, but I do remember much apprehension about the coming inflation. My father came home for good and my parents found a small house at 7 Albert Avenue in Belmont, Massachusetts. We had lived on Benton Road in Belmont before Mother and I moved in with Gran, Grandpa Charlie, and Charlotte in Abington. I remember only one thing about the Albert Avenue house: that was where I learned about classical music. My parents had three large classical music albums on old 78 rpm vinyl disks; two complete operas, Bizet's *Carmen* and Puccini's *La Bohème,* and Beethoven's *Ninth Symphony*. The Beethoven symphony album, my father's favorite, was too heavy to lift easily, but I would listen to the two operas lying on the floor next to the loudspeakers of a large console record player. The operas came with little books, *libretti*, with French or Italian on the right-hand page and English on the left. I followed the sung dialogue reading the *libretti* and trying to figure out how these languages worked.

I never figured out why we spent only one year, 1946, in that little Belmont house. There is usually more than one reason for doing a big thing, and the most important things weren't talked about. I really don't know why we moved to Waban as soon as we did. I completed the first grade in Belmont, and then we moved to 31 Wilde Road in Waban, a lovely village in the city of Newton, about ten miles west of Boston. Mother told me that they moved because of me; the schools in Newton were so much better than those in Belmont, and I would be happier there. That was true, but I don't think it was the only reason for the hasty move. I overheard whispers of how my Uncle Bill, Mother's feckless younger brother, had been running through Gran's inheritance from my grandfather—that is, until my lawyer father put a stop to it. Gran bought a house next door to us at 25 Wilde Road so my parents could keep an eye one her and her finances. At least that is what Mother said; Gran may have thought she was giving up her life in Abington to look after my mother, whose alcoholism brought us to crisis in the first two years that we spent at Wilde Road.

Jim and Charlotte Queeny came to live at 25 Wilde Road with Gran when Jim enrolled in Harvard under the GI Bill. My parents did not think the government should pay for anyone's college education—if you cannot pay for it you should not go—but Gran put Jim and Char up even though she knew how my parents felt. Jim wrote a senior thesis about his PT adventures for the famous naval historian, Samuel Eliot Morison, and later got a master's degree from Trinity College, Dublin. After working another year in naval intelligence and living in Falls Church, Virginia, Jim and Charlotte decided they had had more than their share of the big wide world. He became a sixth grade teacher in Duxbury, Massachusetts, and sailing master at the Duxbury Yacht Club, and Char ran a popular daycare for preschoolers. They stayed the rest of their lives in tranquil Duxbury, raised five children, and gradually made their life together a thing of beauty.

The Queeny home in Duxbury became the *de facto* headquarters for Mother's branch of the family. I used to visit periodically during my high school and college years—in fact all my life until Jim died at 94, and Charlotte a couple of years after. When

Jim died, the whole town of Duxbury turned out—firefighters, police, teachers, former students, and friends—in a parade of tribute to their beloved schoolteacher. The Duxbury Yacht Club named the boat it uses to shuttle people around Duxbury Bay the *James F. Queeny*.

**Figure 1-2: 31 Wilde Road, Waban, Massachusetts. The Kennel family lived here from 1947 to 1950; it was painted grey in those days. This was the house in which my mother used to lock my brother, John, in his bedroom when she could not cope. Mother went away for quite a while for her "ulcer cure" and then we moved to a bigger house so my grandmother could have a separate apartment in it. Photo by Ellen Lehman.**

Life in suburban Waban was so peaceful in 1947 that Mother trusted me to walk the mile to Angier School by myself. I memorized the route and she didn't have to drive. Miss Pilibosian, my second grade teacher, was wonderful, but I did not like the third grade. I disrupted the class with smart-ass questions and the school called Mother in for a parent-teacher conference to complain. I do not know whose idea it was, but it was decided to skip me to the fourth grade with Mrs. Allen. Maybe I had been disruptive because I was smart and I was bored, as Mother said, but that was also about the time her alcoholism was approaching crisis and she had to go away, so maybe that was the reason I was acting out. Gran said Mother went away because of her ulcers. I don't know. In any case, the fourth grade was less boring than the third grade, and, despite Mother's worries, I did not have a problem with social adjustment.

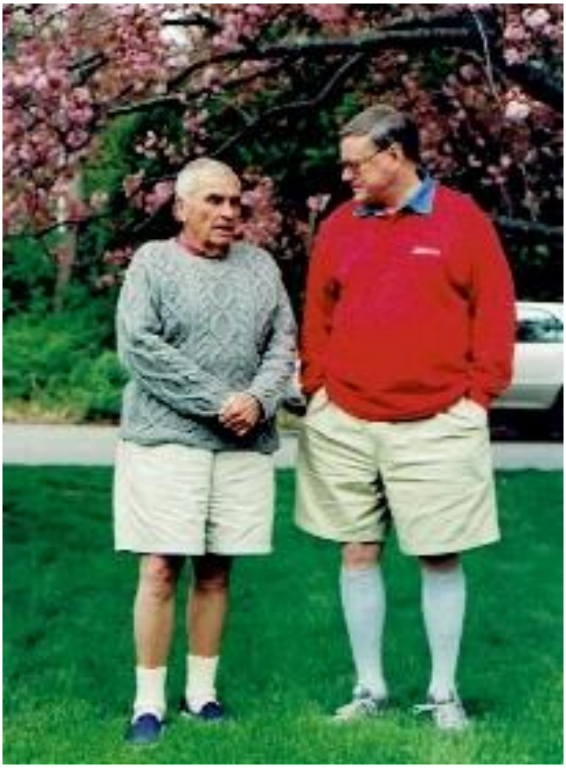

**Figure 1-3: Duxbury, Massachusetts, became my family home away from home, especially after my father died in 1956, and I made visits there all my life. My uncle, Jim Queeny (left) died twelve years later at age 94. Photo taken May 2003 by Ellen Lehman.**

I do not know when Mother became an alcoholic. Did she start at all those parties when she was at Wellesley? Maybe she was an alcoholic even when I was a toddler in Abington. She certainly was an alcoholic when my Uncle Jim and Aunt Charlotte came to stay in Waban with Gran after the war. No one ever called her condition alcoholism in my presence until I was in graduate school. Whatever it was called, the way Mother was *was* normality, the nature of things.

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Mother told me more than once how she had vowed never to inflict Dr. Beatty's scheduled feeding on my brother, John. She didn't, and it turned out even worse. John was a pest. He was always getting into trouble, like the time he escaped through our backyard in Waban to play with a neighbor boy. That boy stripped John naked and painted him red, head to toe. Mother and Gran took John to the hospital to get the red paint off “so his skin could breathe.” (Mother complained afterward that the neighbor boy was the son of a famous Harvard nutritionist, Frederick Stare. Harvard professors may be very bright, she said, but that does not make them sensible parents.) Lots of other times Mother couldn’t handle John. Many times. Then she would lock him in his room. I would stand by, inert, numb, fearing it might have been me. It never was me, I was the smart one, but I would escape to my own room in helpless sympathy anyhow to read something. There were many occasions like that and I did nothing to help him—but what could I have done? Call it survivor’s guilt; all my life a voice inside would accuse me that my mental absorption—my trips to the void—was allowing bad things happen to people I cared about.

John, when confined, would pick the wallpaper off the walls of his bedroom. He got creative; he took the drawers out of a clothes chest and used the chest as a ladder to climb higher. After a while the wallpaper was torn off within a few inches of the ceiling on all four walls. John stripped the whole room. He evidently moved his climbing chest around and put it back. My parents also had to nail shut his bedroom windows, because he would climb out; once after driving home from a shopping trip, Mother and Gran looked up from their car to see him on the roof staring vacantly into the treetops. He could have fallen. That was when Mother went away for a long time and a nice woman named Esther came to help us. Gran said Mother went away to have her ulcers treated. Ulcers made a kind of sense because I had seen Mother stop the car to get out and vomit; that was the “ulcers acting up.” That happened a few times. We all wanted Mother to get better so she could come home, and she finally did. I do not know how many weeks she was gone. None of us ever spoke again about the time she was away or about her ulcers. Mother never did beat her alcoholism, but she learned to keep it under control, and to keep mornings and early afternoons clear. Half a day was better than none. That precarious compromise set my daily routine until I went to college.

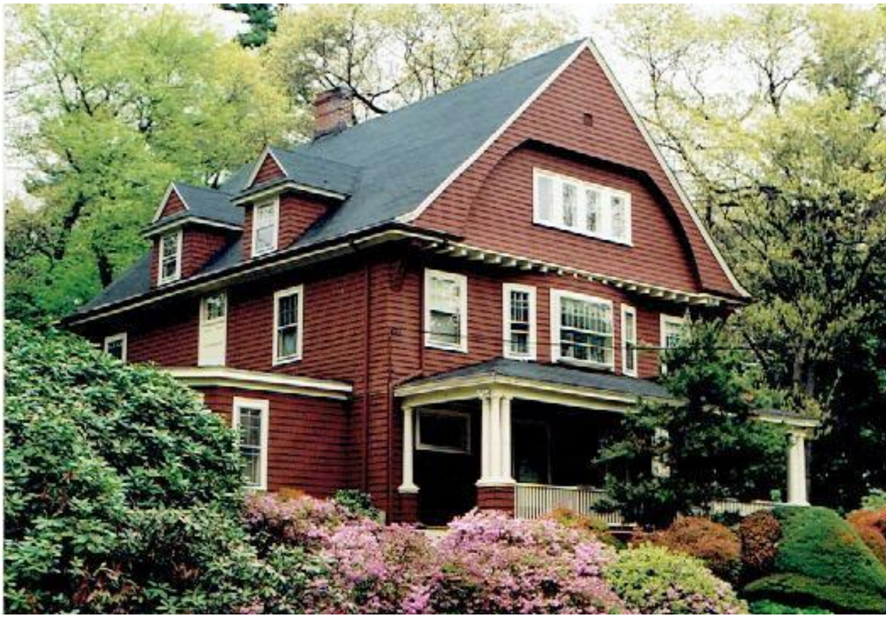

**Figure 1-4: 161 Pine Ridge Road, Waban, Massachusetts. The Kennels lived here from 1950 until six months after my father's death in August 1956. The house was painted gray then. My grandmother's apartment had the large bay window in front on the second floor, and my father's train room had the four windows in front on the third floor. My bedroom was next to my father's train room and had the rearmost window on the third floor. John's room had the rearmost window on the second floor. Photo by Ellen Lehman.**

There was another real estate upheaval a year after Mother came home from her "ulcer" cure. My parents and Gran sold their adjoining houses on Wilde Road and bought one great big house at 161 Pine Ridge Road, still in Waban. Gran had a separate apartment, and my bedroom was one of the original maids' rooms on the third floor. Pine Ridge Road was a little closer to Angier School, and the walk took me past the Waban Public Library, where I was to take out books on the way home from both Angier School and Warren Junior High School. Mother always worried about my social adjustment, and I did have a problem in junior high, but I solved it by helping Paul Kingston, the tough guy in my homeroom, with his homework, and he stood up for me with the other tough guys. But Kingston wasn't helpful with another social quandary. A lot of the guys in junior high were talking about a particularly buxom girl, but I couldn't really figure out what they saw in her.

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John and I were enrolled in Camp Tabor's full eight-week session when our parents were in Europe, and sometimes we got an extra week alone with the staff while camp was not in session. Mr. Browder, the camp owner, was a client of my father's and gave him a special price. Mother also liked that Mr. Browder hired mature schoolteachers and not college kids as counselors. Red-faced Mr. Browder, a choleric tyrant from Alabama, spent weekdays at his day camp in Needham, Massachusetts; when he came weekends to New London, New Hampshire, the whole camp had to drop everything and work on his crazy projects. One year we cut down trees and built a log cabin that nobody used. He had some other peculiar ideas, like requiring married counselors to take different days off.

Our summers in New Hampshire were especially hard for John, who was much too young to be sent away to boarding camp. He was five years old when he and I started to board at Camp Tabor; most of the kids could dress themselves, but John used to wander around with a sticky, dirty face wearing five undershorts, one on top of another, and a stained sweatshirt, a lost waif. Camp Tabor was like the times Mother locked John in his room; I felt crummy about him, but I went about my business. One year, John and I were sent early to Camp Tabor. A few days before my parents were to leave for the boat to Europe, I hid a message in the chest of drawers in my bedroom saying I might not be alive when they returned. Mother found the note and she and my father rushed to New Hampshire to check up on us. We were okay and they left for their trip on time, but I felt really guilty about what I had done.

Our parents endeavored to arrive home from Europe in time to meet John and me at Camp Tabor for my August 20th birthday, laden with exotic gifts—Hohner harmonicas, Türler Swiss watches—all kinds of things no other American kid had. I could not wait for them to bring the presents, which never quite made up for their being away. John and I would hungrily open the presents just after my parents arrived. We opened them under same blue sky from which on other afternoons I heard the bombers.

You could hear the bombers before you saw them. First you sensed a low-pitched hum coming from all directions, and then you looked up to see two, sometimes three, tiny silver airplanes so high in the sky that it was difficult to make out what model they were. I always could, though. They were Convair B-36s, huge planes with six pusher engines facing the rear. The first B-36s had only the six piston engines, but later models carried two jet engines under each wing to assist them in what they called JATO, jet assisted takeoff. They would fly over Camp Tabor's baseball field at about the same time in the afternoon, having taken off on long intercontinental flights, possibly from Loring Air Force base in Maine. Later the all-jet Boeing B-47 replaced the B-36 on the intercontinental training missions. These missions proved every day that the United States was ready to deliver nuclear devastation to Soviet Russia on a moment's notice.

The Cold War was like those B-36s and B-47s, an ever-present hum overhead that signaled annihilation. You could hear it even in the quiet tranquility of Camp Tabor's hilltop.

**Figure 1-5: Convair B-36s; I would watch them fly over the baseball field at Camp Tabor.**

# 2

# My Mother's World

I have luminous memories of coming home from Warren Junior High in bright mid-afternoon to find Mother stretched out on her bed. I think now she started with a drink or two around lunch. This special time after school was when we would have an intimate review of my day. Those were the best times we ever spent together, gentle replays of the desperate sociability of infancy. Her intelligent, educated, aware mind was clear, her voice still steady, her emotional inhibitions loosened on the way to the formless chaos to follow in the evening. I learned then the impossibility of divorcing knowledge from emotions. I learned that things that seem wonderfully clear in the early afternoon could become murky later in the day.

On those luminous afternoons we talked about my day at school, my friends, what I was reading, baseball, popular music, the Korean War. She related how nuclear weapons had changed her world; we Americans had felt free to do whatever we wanted, but now we could not do anything without looking over our shoulder at a permanent threat of war. She told me about the nativist rants of Father Coughlin before World War II, a time of extreme isolationism. She even questioned the social values she grew up with; we talked about the discrimination against Marian Anderson, an accomplished black contralto.

We talked about popular culture a lot. The late 1940s and early 1950s were halcyon years for baseball. The titanic struggles of the New York Yankees and the Boston Red Sox for the American League pennant provided numerous occasions for moral instruction. She said the New York Yankees were old pros who get the job done, not showy prima donnas like the Boston Red Sox. The technical skills of the greatest hitter in the history of baseball, Ted Williams, could not lift the Red Sox to the pennant; they always came up short in the clutch. It was in the clutch that aging, injured Joe DiMaggio would gather once more the skills that had won the adoration of the New York sports writers years before, and make the key play. It was not always his hitting; somehow DiMaggio knew how to inspire his teammates.

The post-war period also saw the decline of the Big Band Era. Mother liked the bouncy Canadian society dance band, the Casa Loma Orchestra, but did not like Benny Goodman's boisterous frenzies; Frank Sinatra had a certain cheap appeal, but Perry Como had an elegant grace. We talked about colleges; the Ivy League was the pinnacle. I remember her astonishment on learning that Stanford was beginning to rival Harvard academically. That a California school could do that did not seem possible to her, but another time she told me how Lewis Terman's studies of gifted children had guided her thinking about me. Terman was from Stanford.

We also talked about where I came from and the people who came before me. Actually, we talked mostly about where *she* came from, and where my father came from. I may be telling my story backwards; this kind of memoir usually starts with the parents and where they came from, but this is how, in beginning adolescence, I learned who I came from.

Mother's parents, Charlie and Mary McKenna Fitzpatrick, had led a prosperous life until the Great Depression. My grandfather Charlie and his brother, Jerry, owned a factory that was a major source of employment in Abington, where he and Gran lived. The factory manufactured shoe lasts, whatever they were. Gran had once been President of the Abington Women's Club. The Fitzpatricks also had a big summer place on the beach in Falmouth, where Mother volunteered at the Falmouth Playhouse as a teenager. (She once told me that she saw Geraldine Fitzgerald and Henry Fonda making out at the playhouse.) Mother and Aunts Mary and Charlotte were sent to an elegant girl's school in Montreal, the Villa Maria, located in the former residence of the Governors General of Canada. The nuns at the Villa found Mother's Boston accent deplorable and made her speak French from the moment she arrived. She had to learn it and she did. The one other thing she remembered being taught at the Villa was how proper young women walk gracefully in crowds; a book on your head, you stand straight, and rotate one shoulder toward the direction you want to walk, neatly dividing the crowd ahead. You can cut through without being jostled by the *hoi polloi.*

My beloved grandfather, Charlie Fitzpatrick, became an alcoholic when the family shoe last business failed during the Depression. He handled the failure honorably, delivering a Christmas turkey to each employee and telling each one personally the factory had to suspend operations. Then he started to drink with determination and persistence. Gran, ever in denial, could not deal with him, and my mother, the oldest child, was recruited to look after him in his cups; she would be the one to drive him to the sanatorium for treatment. She resented all her life having to clean him up, resented losing the carefree pleasures of adolescence. Alcoholism was the ultimate shame; he had fallen victim to the Irish disease. The drinking continued until I was born, at which point he morphed into the gentle, dried-out wraith I remember.

White-haired and emaciated, Grandpa Charlie Fitzpatrick died at 62 of a heart condition in 1944; the hospital did not allow children to visit but I was to be smuggled in to see him at 5 a.m. when fewer people would notice. I was awakened on the appointed day with the news that he had died the night before. I remember staring out the window into an empty, gray dawn at my favorite chestnut tree. There was a single announcement in the newspaper, and no map to tell me where he had gone.

Mother and I viewed my grandfather with different eyes. We saw him at different times of his life. She told me that when she was five years old she had cried in front of him over some childish vicissitude; he took her on his knee and said, young lady, you will never, ever cry again in my presence. And she never did. She also never forgot and told me the story many times. It somehow explained why she was the way she was, but my grandfather was not like that with me. When I was five years old, he would drive me on Saturdays to his almost-deserted factory across from the Weymouth Naval Air Station. We would count the blimps that were about to patrol the Cape Cod Canal in search of German submarines. We would pass an hour together in the office with the empty desk that smelled of damp wood. He would give me a nickel so I could buy a Coke from the Coke machine, which put up an amazing clatter as the bottle came down the chute. Then we would drive to a spaghetti lunch at Caruso's, where the waiters knew both of us.

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The next stop after Villa Maria for my mother was Manhattanville College in New York State, an elite school for young Catholic women, but one that did not answer my mother's yearnings. Towards the end of her freshman year, she drove alone to Wellesley College and talked them into admitting her for sophomore year. Her parents did not oppose her rebellion even though she was moving into a secular world. To hear Mother tell it, that started the three best years of her life. Wellesley was an intellectual paradise; there was far more to life than the nuns at the Villa Maria could teach. She was a social whirlwind; years later at a Wellesley reunion, one of her old house mothers greeted her with "Betty Fitzpatrick, the social butterfly!" Mother went out seven evenings a week; she loved dancing and dance bands; she parried a succession of suitors, all charming, until my father, older and more established, came along. He was a Protestant, but no matter, he was so strong and smart she could not resist. Nice Joe Russo didn't stand a chance.

My parents were married the week of her graduation from Wellesley, in June 1936. They took a long honeymoon in Europe that summer. My father was eager to see the resurgence of his beloved mother's homeland, but the world I was to be born into intruded on their idyll. They went to one of Hitler's Nuremberg rallies; Mother remembered that they had to put little American flags on their car for safe passage through the jittery crowds. That rally was an eye-opener; my parents were horrified by what they heard, but people back home did not want to listen.

Shortly after my mother and father returned from their honeymoon, Mother's maiden aunts, May and Kate Fitzpatrick, sent a Catholic priest to remind her that it was not too late to annul her marriage to my father. That priest's visit settled it; I was to be brought up in the most liberal Protestant church Mother could find—the Union Church of Waban. Each time I heard Mother's story about the aunts, the priest, and the annulment, I saw the tremendous cost in life energy of remaking the religious identity that circumstance had forced upon my mother.

I learned about my father's early life from my mother. At least, I learned what she thought was important about it. My paternal grandfather, Archie Kennel Sr., was about seventy-five when he died in July 1936, during my parents' honeymoon. Mother hardly knew him but saw him as a terrifying personage. He spent a dissolute youth gambling on the Mississippi riverboats, reformed, went to a local medical school, and set up practice in a big house on top of a hill in

St Louis, Missouri. He shared that house with his sisters. Then Dr. Kennel advertised for a wife. Frieda Piening, a young woman from the province of Schleswig-Holstein near Germany's Danish border, responded to Grandfather Kennel's ad and traveled across the Atlantic to marry him. He set her up in a small place at the bottom of the hill and made conjugal visits to his young wife. My father, as a boy, cowered under the table during the Sunday dinners my grandmother prepared for Dr. Kennel, who sat alone at table while she served standing.

My father worshiped his mother and I can see why, for her selflessness enabled him to succeed in high school, become a Missouri tennis champion, and graduate from Westminster College, where Winston Churchill was later to give his "Iron Curtain" speech. My father went on to earn a master's degree from Vanderbilt and a law degree from Harvard. His first job was with the Federal Reserve Bank of Boston after graduation from law school. Success at the Federal Reserve earned him an invitation to join the august firm of Choate, Hall, and Stewart as an associate. Choate Hall, founded in 1899, was located at 30 State Street, directly across from the Old State House in Boston in my father's time. Connections at Choate Hall got him the war assignment at Lockheed; Robert Gross, once at Choate Hall, had bought a struggling airplane manufacturing company from a gentleman named Allan Loughead (pronounced Lockheed). A visit to another Choate Hall colleague in Germany, Conrad Oberdorfer, led him to the Marklin model electric train business after the war.

It is not what you know, it is who you know, so said my mother.

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At a certain moment during our after-school conversations—I could not tell when—a switch would turn, and Mother would announce that she would have to get ready for my father's arrival home from work. That meant leaving her bed and going downstairs to the kitchen to lay out cans of Budweiser beer while she set the kitchen table; several cans would be finished before he arrived. She consoled herself that she did not drink Irish whiskey as the real drunks did at saloons. As soon as my father swept in, I could hear their passionate conversation, even from upstairs. Their conversations were punctuated by the sounds of beer cans being opened. They would finish off a case of Budweiser (24 cans) in one or two evenings; there were full cases stacked five feet high next to the back door. Gran, after she had moved in with us at Pine Ridge Road, never wanted to come downstairs in the late afternoons; she and John consoled one another, and often had dinner together, behind closed doors in her apartment. My parents' babble would go on for some time, until John or I or both got restive; then the food sitting cold on the stove would be rewarmed. Often, I did the warming and serving. (On scary occasions, my father would drop live lobster that he brought home from Boston in boiling water. I tried not to look at it from the lobster's point of view. I never really liked lobster, even though it was a New England specialty.) There was no dessert, and dinner dissolved as each of us drifted away from the table, my mother to fall into the bed she had left a few hours earlier, my father to stretch out on the couch in the TV room, and John and I to read or do something in our bedrooms.

Mother's long daily journeys into night could not fail to shape how I think. Her voice had connected me to the world, after all. The wish to organize her thinking was inescapable. Her daily falls from comprehension and empathy to confusion and complaint taught me that events have more than one interpretation. Outwardly strong people somewhere have weakness; illness could be the sign of the moral weakness that made them susceptible to physical infection. You find patches of empathy and generativity in the worst people and of selfishness and rigidity in the best. Motivations are always mixed, especially your own. You should always look below the surface but even there you do not find truth or purity. Rationality and emotion contend uneasily in the same mind: never put complete faith in either one.

How then are we poor humans to find reliable knowledge? By reaching beyond ourselves we can see facts and events as others do, but our own struggles tell us that others must be as uncertain as we are. Most of the mental constructs we invent to explain facts and events are not built according to the scientific method. The scientific method makes experiment and observation an arbiter of truth, and the scientific disciplines are there to adjudicate truth collectively. The collective judgment of your scientific peers is not only more reliable than your own but an extraordinarily powerful, if highly imperfect, social instrument for establishing knowledge. It is the best our civilization has learned to do.

I had seen Mother's coherence dissolve before my very eyes in her daily alcoholic trips to the edge of chaos. This is probably why

I do not find self-validation as other scientists do in the act of research. I cannot follow a line of reasoning wherever it leads and trust it takes me to a secure place. I yearn for validation from other scientists much as Mother did acceptance by Boston society. My judgment might be incoherent in some fundamental way, as Mother's was. I cannot be sure the ideas I bring back from the void are trustworthy. If scientists' approbation abandons me, I too fall into the void.

# 3

# The World of my Father

Occasionally, I would join my father on that couch in the TV room, and he would tell me wonderful stories about his adventures at Lockheed and imaginary stories about his model trains. His voice would catch as if he were on the edge of crying. I was a little old for children's stories, but I would listen to him as though I were five years old and it were the War again. Then, amazingly, after about an hour, he would rise from the couch and climb the stairs to the third floor, where he would work obsessively on his electric train hobby/business until 1 or 2 a.m. I would fall asleep in what had been the maid's bedroom next to his train room, hearing him working.

During the War, he had often traveled between Lockheed offices in Burbank, California, and Wright-Patterson Field in Dayton, Ohio, with side trips to Washington, DC, to negotiate airplane contracts with the federal government. He was really important: he had worked on the contract for the P-38, America's skittish but agile twin-engine fighter plane with the twin tails. He also worked on the P-80, America's first operational jet fighter plane, the Lockheed Lightning. I still have the model P-80 he gave me. That is how I know he was important.

This is my father's story of his last airplane flight: one day, he was in Dayton and had to go to California right away. Mother said he had had the equivalent rank of a brigadier general for making travel arrangements. Maybe. Anyhow, he called a real general he knew and asked him what flights they had going to Los Angeles. Come on down right away to Wright-Patterson, this general said, I have something going out right now. When my father got to the airfield, an unusually sleek four-engine airplane with a triple tail was warming up on the runway. What kind of plane is this, my father asked, climbing into the cockpit to join the engineers already there; a YC-121, they answered, the second test model of the Lockheed Constellation.

The big plane was ominously slow taking off, using most of the long runway, and climbed laboriously. After it reached altitude, the engineers finally had time to explain that the plane was loaded to twice its rating with aviation gasoline; that's what had been available as a load. The flight plan included feathering two propellers over the Rocky Mountains and measuring the rate of descent. That's what they did. The engineers, absorbed in data, took no notice of my father as they remarked to one another about the plane's rate of altitude loss. All Father could do was sit by. Eventually the engineers, having their data, declared it time to restart the other two engines and head on to California.

If you thought the feathered engines would restart, think again. The plane kept on descending, the remaining two engines whining and smoking. I do not think we are going to make it to Edwards Air Force Base, the pilot declared; those who want to bail out may do so; I will radio your positions. Some people did bail out; my father watched them jump. But the pilot asked my father, how much do you weigh? Well over 200 pounds. Too bad, the pilot said, the parachutes are rated for 180 pounds; maybe you can strap one under each arm; just remember to pull the cords one after the other or the parachutes will get tangled… On second thought, you had better stick with me; this is an expensive airplane, and I am going to try to land it in the desert, aviation fuel in the back and all. The pilot did put it down safely with two smoking engines and two dead ones; he was a test pilot, after all. After they landed, they ran from the airplane fearing an explosion; eventually a Jeep came to take them to Edwards.

That was the last airplane flight my father ever took. He took trains and boats after that.

My father also told me of how he fought a chess grandmaster to a draw during the War. The back story is important. Mother told me he came to marriage a twenty-eight-year-old bachelor with an addiction to chess. He used to bring his chess club friends home for silent, smoky evenings of chess, lasting well past midnight. Mother, then a newlywed, complained for years after that her only function was to supply coffee and empty ashtrays; besides, his scruffy chess friends were smelly. Her feelings were not a factor when he was on lonely travel during the war. On one fabled occasion, this grandmaster took on all comers in a multi-board chess tournament in some hotel where my father was staying. As the evening wore on, the other players dropped out, and my father was left to grapple with the grandmaster alone. They played on for a while, long enough to satisfy my father's pride. Finally, the grandmaster declared that he probably could beat my father, but it would take long after bedtime and he offered my father a draw. My father was proud of that draw, but he never played competitive chess again. He said he had achieved all he could. He never did teach me chess, and I never did like mental games.

He broke his chess taboo to teach my brother, John, but there was a strange tension in the air when they played. The whole thing made my mother anxious. I think now it was because Mother had come from wealth, and intellectual displays such as this were not done in high social circles; you show your intellect deftly and subtly, oblique humor its highest expression. Intellect is a tool of aggression and should never be used for self-aggrandizement. My father, risen from poverty, saw that his brains gave him Harvard Law School, a job with an elite Boston law firm, and the social cachet to attract my mother.

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My parents had essentially no social life at home. They went alone to the symphony and opera, and rarely invited friends home or went to parties. I remember only two occasions when they did have people to the house. Mr. and Mrs. Overly were important clients of my father's; actually, Mrs. Overly was, because her family owned the country's largest coal company, Peabody Coal. She relied on my father for advice. The four of them spent an entire Saturday afternoon drinking noisily on our porch while I was stuck playing outside. Then there was the time Mr. and Mrs. Lehman, rich friends of my Aunt Mary and Uncle Stanley in Pittsburgh, came to visit. My parents put the Lehmans in the big living room with the wine-colored wallpaper, the one we hardly ever entered. We even turned the lights on. There was a sober, respectful conversation in which I was shown off. I bet the Lehmans never suspected that little boy was a future son-in-law.

My parents were fundamentally different people. My father cared about possessions and my mother cared about social standing.

If you were from a once prosperous Boston Irish family like Mother's, you were endlessly susceptible to social slights completely imperceptible to others. Your whole world was shaped by your social inferiority. There were places you could never think of going, things you could not do, ways you should not speak, restaurants where you would not feel at home. The Protestant Boston elite were like English dukes and duchesses, the atmosphere in every room determined by the presence or absence of even one.


**Figure 3-1: The Kennel Family, about 1953, in the big living room at 161 Pine Ridge Road that we entered only on special occasions. In the back row are my father, Archie Kennel (left); my grandmother, Mary McKenna Fitzpatrick (center); and my mother, Elizabeth Ann Fitzpatrick Kennel (right). In the front row on the floor are my brother, John Archibald Kennel, (left), almost ten years old; the family poodle, Teddy (center); and Charles Frederick Kennel (right), fourteen years old. The landscape paintings on the walls, the lower parts of whose frames are visible, were acquired in France on my parents' annual trips to Europe.**

Mother feared that my father's love of food, of possessions, of tangibility, would reveal his immigrant background. She was on constant lookout for signs of it. Once, on coming home to Wilde Road from Angier School, I found Mother scattering the white shirts, normally neatly stacked in my father's bureau drawer, on the floor. Why does he need so many shirts she wailed; why does he need expensive Borsalino hats? Didn't he know that sharp dressing was for *nouveaux riches*? It wasn't only clothes; my father also loved cars. Once we all went together to a Cadillac dealership; he was a portly man (to be kind) and he really wanted a big, black Cadillac. We didn't get it. On the way home in our scruffy old Plymouth, my mother blistered him; didn't he realize that the people who matter don't care about cars? They don't have to show off, they keep their cars messy; they drive them into the ground. (All I know now is that I have had four Mercedes-Benzes; one is now forty-six years old. I never got a sporty model, but I yearn for one in the dealer's showroom each time I take one of my practical sedans for repair.) Another time, Father did get a car he wanted: a green 1950 Studebaker; you know, the kind with the fake propeller nacelle for a nose? As one of the first post-war cars with color, that military-green Studebaker with the propeller nose drew a crowd everywhere. People would stop him in parking lots while he proudly touted its modern look, and Mother would squirm in the front seat. We only had that car for a year. My parents said they got rid of it because it was a "lemon." I do not know whether this was true, but I do know Mother abhorred middle-class showiness. She yearned to radiate the Puritan values so serenely espoused by the Protestant elite that founded Boston and ruled Boston society until her Catholic forbears swarmed into Massachusetts after the Great Potato Famine in Ireland.

I think the only times my parents were happy was during their trips to Europe in the summer. On their first trip, in 1949, my father paid a visit to his German/American colleague Conrad Oberdorfer from Choate, Hall, and Stewart, then in the High Commissioner's Office in occupied Germany. It was Oberdorfer, I think, who sent my father to the factory near Stuttgart where Marklin electric trains were made. My father came home with the concession to sell Marklin products in the American market, as well as a fascinating collection of model trains in the then unfamiliar HO Gauge. He set up a layout that encircled the large basement of our house on Pine Ridge Road in Waban. l particularly liked to puzzle out the German instructions that came with the trains.

My mother and father took eight-week trips to Europe every summer from 1949 to 1955, ostensibly to pursue the electric model train business; it gave them a respectable reason to visit Germany, France, England, Switzerland, and Italy every year. Mother could not stop talking about the elegant people they consorted with in Europe. (I have a photograph of my parents with Count Antonio Giansanti-Coluzzi and his family; Giansanti's exquisitely beautiful teenage daughter catches my eye every time I look at that picture.) Europe glimmered over the horizon, a distant paradise, but sybaritic Europe could never surpass austere Boston in Mother's eyes. After all, Boston called itself the "Hub of the Universe." She was socializing with high society and my father had discovered an absorbing interest that connected him to his lost heritage and just might produce the financial success eluding him.

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Mother transferred her hopes for my father's career to me. My brains were her reason for hope. She made sure I skipped the third grade, encouraged my hobbies, talked the librarians into letting me read grown-up books from the Waban Public Library. But brains were not everything, at best they were only the beginning of everything. She wanted me to be "well rounded," which for her generally meant flexible general intelligence and a certain schooled confidence to handle unfamiliar people and circumstances. My future Harvard Dean, the aristocratic McGeorge Bundy, son of Harvey Bundy in my father's firm, was well rounded. John Kennedy, though he was Boston Irish, knew the value of appearing well rounded and did a good job of simulating the Bundy kind. Teaching me to be well rounded was part of Mother's strategy for relieving the conflict between the Boston Irish identity she was born into and the Boston Brahmin status she wished to die in.

Mother's Catholic forbears arrived in Boston after the Great Irish Potato Famine of 1845-1852. Irish Catholics swarmed into a city founded in 1630 by English Protestants who themselves were escapees from the intolerance of King Charles I. Protestant and Catholic—English and Irish—had never mixed comfortably in the old country and lived together uneasily in the new. The Irish occupied the South Shore of Boston and the aristocratic WASPs (White Anglo-Saxon Protestants) had removed themselves to the North Shore. I could distinguish North Shore people from South Shore people by their accents. On the South Shore it was "sho-wah" and on the North Shore, "schorrr." To paraphrase Winston Churchill, greater Boston had two great peoples divided by a common language, both of whom spoke it uncommonly well. The two mixed it up primarily in the tumultuous arena of Boston politics. The Kennels were neither this nor that: a Catholic-Protestant couple living in Newton, halfway between the North and South Shores; my father an immigrant boy from Missouri working in Boston's oldest law firm, and my mother from a formerly wealthy family now in lowered circumstances.

**Figure 3-2: The Old State House in Boston. The offices of Choate, Hall, and Stewart, my father's law firm, were located at 30 State Street in the building to the far left of this photo. I visited his office on the eighth floor on Saturdays from time to time. You could have been proud that he rose from an obscure Midwestern background to such an august position, as he was, or, as my mother was, ashamed that he lacked the social graces of his aristocratic colleagues in the firm.**

Boston Brahmins had built a mostly admirable culture of achievement and morality. The same group that was the first to lecture the rest of the country on the evils of slavery also had fixed presumptions about how one behaves in society. Aggression is expressed subtly and without bluster; power is unobtrusively expressed but relentlessly exercised. One does not visibly yield to human weaknesses—food, sex, money. Loyalty to social class is signaled through one's accent, schools, sports, clubs, and summer communities, and is never displayed outside the group. Mother complained repeatedly that my father's Midwesternisms rendered him invisible to the aristocrats in his Boston law firm. He was undoubtedly a brilliant technical lawyer (as demonstrated by his prominent wartime job with Lockheed and later selections as a judge of the Ames competition at the Harvard Law School), but he would never make partner. She, from a well-to-do Boston Irish family fallen on hard times, would never achieve the prominence in WASP Boston society for which she had danced the nights away at Wellesley College.

Indeed, my father never did make partner, and those Yankee Brahmins never deigned to notice the Kennels. We were church mice. I remember the time—it must have been 1954—when my mother and father came home in early evening from a garden party in the WASP enclave of Pride's Crossing on the North Shore. My parents threw things against the kitchen wall and yelled and screamed. My father had been in his law firm since I was born, and this was the first time that the leading partners of Choate, Hall, and Stewart had invited them to a social event. Apparently, at this party, while my father was busy losing money at poker, Mrs. Pengra, the doyenne of the firm and wife of my father's boss, took my mother on a walk in the garden of their seaside mansion. Mrs. Pengra was on a mission. My dear, she said to my mother, we like Archie (that was my father) very much; we think he is terribly smart, but, one thing you must understand, *we simply do not know the man*. Honestly, I do not know how each parent saw that remark, but I do know how I came to view it. A poor boy from Missouri never could be socially recognized in Boston. He was not a financial rainmaker; he was a brilliant backroom drudge. Brains were a dime a dozen; Winston Churchill, from a ducal family, called them boffins; my father was a boffin. He should have stayed with Lockheed when he had the chance.

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Father had his first known heart attack sometime in 1952 at age forty-five. I remember his sighing and moaning on the couch, and the heaving of his ample stomach. Mother had called the family doctor, "Turk" Thurston. In those days doctors came to the house, but little good it did that evening. He blew in, peeved at being called after dinner, and announced: Mrs. Kennel, he is having a heart attack; there is nothing I can do; either he makes it through the night or he does not. Call me in the morning either way. Without opening his bag, Thurston left. It probably was true, there was not much medicine could do in those days—you never heard much about cholesterol—but the New England Doctors Beatty and Thurston will live forever in my hall of infamy.

My father did get better that night, and family life went back to its version of normality without further mention of the heart attack. Thurston's abrupt departure and the ominous silence only confirmed that illness is an unspeakable moral weakness. And so four years were to pass with the unmentionable hanging over us all.

On the last day of my sophomore year at Harvard, my father drove from his office to Cambridge to take me, and my stuff, home to Waban. It was eerie: on the drive home, he said he had never expected to live to see that day. (*Not the day I graduated, mind you, but the day I finished my second year! How many other heart attacks had he had and not told anyone? Was that why he wanted me to go to Harvard early?*) About a month later, he had an unmistakably large heart attack, and this time they put him in the Massachusetts General Hospital on the Charles River in Boston. Mother and Gran enlisted me to drive them from Waban to the hospital every day to visit him. The hospital did not allow children to visit and at almost seventeen I was considered a child, even if I was going to be a junior at Harvard. So I spent hours inside the car in the hospital parking lot waiting for Mother and Gran to come down from my father's room. They did not say much to me on the way home, but they talked *sotto voce* about how sick he was. It would have been unseemly and pushy to try to find out what the hospital was doing to treat him, and they did not.

The last time I ever spoke to my father was on a telephone call on my seventeenth birthday; we chatted about how nice it will be when he returns and we can tell stories again about his electric trains. When the telephone in his electric train room next to my bedroom rang at 5:30 a.m. two days later, I guessed what it meant. I picked up the receiver, once again to overhear my mother and a doctor talking. *What shall I do?* she wailed. There really is nothing you can do, Mrs. Kennel, the doctor's voice said.

At that moment, a voice inside I did not recognize swore I would kill myself rather than be a failure like my father. Better to do it yourself than die a death of despair.

At my father's funeral, Mr. Pengra took me aside to tell me that he knew my father was an unhappy man but he, Mr. Pengra, thought he had found a solution to my father's dilemma. Mr. Pengra had been about to nominate him for the Securities and Exchange Commission where my father could use his great technical skills to advantage. He was Choate Hall's expert on how to float stock issues.

What Mr. Pengra said provided a kind of relief. My father wouldn't have died a death of despair had he gone to Washington.

I rarely spoke about the terrible vow made the day my father died. It was too dangerous to articulate in words. It stayed restless in the inner recesses of my mind, like a parasite that wormed its way into my soul and stayed. I secretly negotiated its terms; when have I done enough to be absolved of the vow? Boredom was a danger sign. Was it telling me it was time to carry the vow out? How will I know when I should do it? Mainly, I used the vow selfishly, to coerce myself. Lapses of creativity could be fatal, and I used the vow to override the guilt about science diverting my attention from other people's needs.

# 4

# Exile to Pittsburgh

My father died on August 24, 1956, between my sophomore and junior years at Harvard. I returned to Harvard three weeks after my father's funeral and proceeded to do poorly; I got a C-minus in Applied Math 202: Boundary Value Problems with Harvey Greenspan. It was my first graduate-level course, and I screwed it up. But I also took up with my first serious girlfriend, Penny Post, a senior at the Dana Hall School in Wellesley, a striking blonde destined to go on to Wellesley College where her mother was a trustee, and where my mother spent her happiest years. I got the family car, a Mercury, for a few months until it could be sold; I used it to drive from Harvard to Wellesley to see Penny Post. Penny's eventual apostasy was for a while a catastrophe like the loss of my father. I never saw it coming, but Penny suddenly left me for another man, a Yale man no less. Penny was destined to lead what the French call *une vie mouvementée*, but Penny and I remained friends, and in mid-life she became a kind of honorary aunt to my children.

Meanwhile, Mother, all in the space of six months, sent Gran to live near my Aunt Charlotte, packed up our family stuff, threw away a lot of my own, and decamped to Pittsburgh with John and our poodle, Teddy. Mother never called anyone to say goodbye or mentioned Boston friends she would miss. She did say she would not miss Gran's nagging, which I figured was about her drinking. Boston, which prided itself on being the "Hub of the Universe," had abandoned her, so she abandoned it. She cast herself, John, and our dog, Teddy, adrift, but she took her alcoholism along. From then on I had a new place to visit during vacations from college and graduate school. It was what I had to call home.

Mother, John, and Teddy found a dreary smoke-stained half-house on 6839 Thomas Boulevard in Pittsburgh, three blocks from Stanley and Mary Hahn's elegant home with a pool on 200 North Murtland Avenue, Pittsburgh. Mother and Mary had Wellesley College in common, Mother having blazed the trail. To hear my Aunt Mary was to hear that Wellesley had been the high point of her life too. Mary graduated as president of her class in 1938 with a lifelong taste for literature. Stanley's friends were a social crowd that partied almost every weekend, and Mary would escape his boisterous friends to read Shakespeare in peace and quiet.

Pittsburgh, America's "Gateway to the West," struck Mother as lacking sophistication compared to Boston, but nevertheless she adjusted to the ways of my Uncle Stanley Hahn's circle of friends. I found it curious that people in Pittsburgh had crowds they ran with every weekend. My first summer there, Stanley gave me a summer job working in the accounting office in his furniture store in East Liberty, and the next summer I spent working midnight to dawn on a high energy physics experiment at what is now Carnegie Mellon University. Mother's social skills found her work in fundraising with the Western Pennsylvania Hospital, and later with Pittsburgh's educational television station, WQED. She went to work semi-sober every day and drank every evening, the balance she had maintained since coming home to Waban from her "ulcer cure" in 1947.

There was something else. Stanley Hahn's father, Sig Hahn, was, and acted like, the patriarch who founded the chain of Hahn furniture stores in the Pittsburgh area. Stanley looked up to him with a mixture of admiration and terror; Stanley feared, probably justifiably, that he could never match his father in business. Sig's wife had separated from him at least thirty years before and was living in Florida; they never divorced, and the pain of separation was reenacted on their every contact. It all started when Sig, a lonely old man, invited Mother, a lonely still young woman, widowed at age 41, to dinners for mutual solace and companionship. I never asked whether it went further than that, but I am sure it did. (Who was I to deny Mother some satisfaction in life?) I did worry that their relationship would affect Mother's relationship with Aunt Mary and Stanley, her lifeline in Pittsburgh.

It was a relief to go back to Princeton after my vacations were over. At least I had a chance of solving the problems I encountered there.

Mother would abandon Pittsburgh in 1973 as abruptly as she had abandoned Waban in 1957. She returned to Plymouth, Massachusetts, about ten miles from Aunt Charlotte and Uncle Jim Queeny in Duxbury; she wanted to hear New England voices again. I never heard her regret that she was leaving friends and family behind in Pittsburgh. She died at age 60 in 1975, when I, and my family, were in Paris.

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I had met Ellen's mother and father years earlier when I was five years old and they visited my parents in Waban, Massachusetts. Mr. and Mrs. Lehman were half in and half out of the tight circle of Stanley and Mary Hahn's friends. Bud and Jane Lehman regarded the boisterous antics of the Hahn circle with a serene detachment that never conveyed impatience or disapproval. They sometimes even joined in. Aunt Mary was Jane's good friend, and Jane and Bud invited Mother to their stately home on Robin Road frequently. When I was on vacations from Harvard, and later Princeton, they made me welcome in their home, which was such a contrast to the dreary place where Mother and John lived. I also dated their niece, Carol Spear, Ellen's cousin, and a Vassar student. This relationship did not progress because I was not yet over Penny Post, and Carol's parents were serious alcoholics, which did not augur well. Meanwhile, the Lehmans had season tickets to the Pittsburgh Pirates baseball games, which I got to use on weekday evenings before going to work at midnight; the baseball field, Forbes Field, and the Carnegie Institute were close to one another. I would stop by Ellen's house to pick up the baseball tickets and we encountered one another frequently. Ellen was five years younger and seven years behind in school, so I did not think in courtship terms, with one memorable exception. Ellen was a senior at the Winchester-Thurston School and I a graduate student at Princeton on New Year's Eve 1962. Her parents had invited me to their party, and Ellen broke a date with someone else to be there. We stayed up until 3 a.m. in chaste conversation. I went home thinking that I ought to wait around for this girl;

however, I was to marry someone else first and waited twenty-two years for that girl.

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It took John half his lifetime to recover from his childhood to the extent he did. John went to Shadyside Academy in Fox Chapel during the day and came home to mother's babbling at night at Thomas Boulevard. He did not get to see mother's lucid moments. As in Waban, John bore the brunt of her drinking while I was spared. John's teenage years were excruciatingly painful; he would spend evenings away from home, driving around aimlessly with his equally unhappy friend, Norton Lewis. Then, John would return to the last stages of her alcoholic babbling. When he finished Shadyside Academy John enrolled in engineering at Cornell, but dropped out to go into the US Army after two years. He went to the Army language school in Monterey, California, and then was stationed at an intelligence listening post on the West German-East German border, where he heard German every day. After that, he enrolled in an IBM training center in New York State's Hudson Valley to become a manager/engineer in computer science. While there he had a six-week marriage with a woman I never met, who, my mother said, beat him with a frying pan and ran off with the family silver. John then took a position in information technology with the Swiss Federal Post Office and married a second time, a girl from Southern California, Caryn. John was as much in love with Caryn's parents and their family life as he was with her. I first met Caryn on a weekend in Venice, Italy. She was graceful and gentle, with only one salient defect: she was compulsively promiscuous. After many vicissitudes, Caryn left John for a German truck driver.

John shifted from the Swiss Post Office to a job in information technology and project management with National Cash Register's (NCR) branch in Frankfurt, Germany. In early midlife he had a problem with spasms of the aorta, which took him to the UCLA hospital for a consultation. While in Los Angeles, he told my then wife, Debby, and me that he was thinking of marrying again. The thing was, he had to choose between Elaine, a hard-driving American business colleague with NCR, and Heike, whom he had met in Germany. Elaine was more educated and professionally ambitious, but Heike was kind and sensible. Whom should he choose? I think he thought we would back the more intellectual one, but we said to take the kind one. He did in 1982, and Heike gave him the first stable family he ever had, as well as a fine son, Johann, my only blood nephew. Eventually, John and Heike moved to the NCR headquarters in Dayton, Ohio, a sign of professional success. John's life travails did not end there; he suffered a major heart attack while on business travel for NCR and ended up on permanent disability. John got his heart attack at about the same age that our father died. He and Heike remained in Dayton and devoted their last twenty years together to friendships, community affairs, and local Democratic Party politics.

In late 2012, I had a bladder cancer. A few weeks after my operation, John, who was not diagnosed as promptly as I, learned he was condemned to what became a losing battle with the same disease. During his chemotherapy, I beamed my lectures on climate change from Cambridge, England, to him in Dayton, Ohio, via Skype each Thursday, and the following weekend we would discuss the issues raised in my lecture via telephone. We also talked a lot about our father, whom John worshipped as a lost child would. John was deeply worried about climate change, and the depth of our discussions revealed how carefully he listened to each week's lecture. After he died in 2014 at age 70, I flew to Dayton to participate in his celebration of life. There I learned of the genuine admiration his friends had come to feel for him. He loved his neighbors, and they loved him.

He died instead of me.

# 5

# Education of a 1950s High School Student

I was almost eleven years old when the Korean War started in July 1950. I could check out war maps in the newspaper once again. A new generation of even faster jet fighter planes had taken over where my father's Lockheed P-80 left off at the end of World War II: the North American Aviation F-86 Sabre could do 650 miles per hour. I had a collection of WWII spotters' model aircraft hanging from the ceiling in my bedroom, so reminders of devastation by air accompanied me as I drifted off to sleep. By 1952, it appeared that the US and China had maneuvered the war on the Korean Peninsula into a stalemate that neither wanted to go nuclear, so most everybody could feel safe going about their business. Besides, the US was building great new bombers—the B-47 and the B-52—to instill nuclear fear in anyone thinking of bothering us. The bombers instilled fear in me, too, but I forgot it once I got to school; nobody there seemed aware that there was a Korean War.

Senator Joseph McCarthy was one who was obsessed by national security to the point of irrationality. Gran had a separate apartment on the second floor of our house on Pine Ridge Road, in Waban. I returned home from Newton High one afternoon in 1953 to find Mother, Gran, my brother John, and our poodle Teddy, gathered around Gran's television, watching Senator Kefauver's Army-McCarthy hearings. We cheered when my father's colleague from another Boston law firm, the elegant bow-tied Joseph Welch, badgered the uncouth Senator McCarthy without raising his voice or using crude language. What pride in Boston we felt when Welch destroyed McCarthy with a single question: *Have you no sense of decency left, sir?*

Mother had been right: they did have good teachers in the Newton School system. I took French in Warren Junior High School from the wonderful Madame Daudelin whose lilting voice made French sing like music. (Mme. Daudelin once told mother that I had amazing powers of concentration, and that class turmoil never seemed to bother me.) Newton High School (NHS), now Newton South High School, was paradise. Given the popular idealization of high school life in the movies it was counterintuitive, but NHS valued academics.

Mother told me that Newton High School was ranked one of the best in the country. Its department heads, many of whom had PhDs, were pioneering a special curriculum (the "Kenyon Plan," a program developed by Kenyon College, a small liberal arts institution in Gambier, Ohio) wherein gifted students could take college-level classes in high school. Newton High School's Kenyon Plan taught the mid-twentieth century academic canon, so education meant literary education: Greek mythology in ninth grade, Shakespeare in tenth, Charles Dickens and Thomas Hardy in eleventh, and American contemporary literature in twelfth. You could take French and calculus starting in tenth grade. You also had to take physics and chemistry, which I remember tolerating. I do not remember anything about "social studies," not yet called social sciences. Only seven of us had all our classes in advanced standing and we could enter college as sophomores if we were so inclined. Dorothy Swanton, Brenda Gordon, David Kanter, David Horvitz, David Sachar, Bernie Lettau, and I were the brainy crowd and hung out together.

Mother was impressed with the Kenyon Plan. It put NHS in her high school hall of fame, with New Trier High near Chicago, or Shaker Heights High near Cleveland. NHS Kenyon students were encouraged to read at least one extracurricular book every week and write a 2,000-word report on it as homework. Mother and I would discuss my choices and what I might say about them. She expressed true delight that, at last, I had found a group of fellow teenagers who would not see me as freaky, nerdy, or alien. My father was visibly delighted each time I brought home a perfect all-A report card; the one time I brought a report card home with a B-plus in one course, he asked playfully, *how come you got the B?* Only later would I discover the reason why he seemed so pleased at the visible signs of rapid academic progress.

As for me, I liked my friends and teachers. Mr. Taylor taught us college calculus in junior year. He threw a piece of chalk at me when I made a stupid mistake; it showed he cared. *It wasn't enough to get the general idea, you had to get it completely right.* Nor can I forget Miss Lanigan's passion, her heaving and sighing when reliving Eustacia Vye's unrequited yearnings in Thomas Hardy's *Return of the Native*. I made a lifelong friend in David Sachar, a son of the President of Brandeis University. David and I competed for the number one and two positions among the Kenyon Seven. He usually won, but not every semester. It was a matter of nuance anyhow, because we both had all-A report cards. David and I would get together in the afternoon at his house, which was near enough to Newton High to walk to after school. We would play silly adolescent games, which were continued on endless telephone calls.

One afternoon I arrived at David Sachar's house only to be warned we would have to be quiet: Professor Einstein was upstairs taking a nap. Speaking of Professor Einstein, you would not have predicted that I would become a scientist from how I spent my time in my early teens. I spent more recreational time on fiction than anything else until I lost the facility to read quickly from stopping to puzzle over details in mathematics and science. Until that happened, I read several novels each of C. S. Forester, Mark Twain, Charles Dickens, Thomas Hardy, Joseph Conrad, Thomas Mann, Leo Tolstoy, Fyodor Dostoevsky, Ivan Turgenev, and Alexandre Dumas, among others. When the librarian at the Waban Public Library called my mother to say I was reading books too old for me, my mother said not to worry, I could follow the plots, but I wouldn't understand the adult parts anyhow. From then on nobody tried to stop me from going into the grownup section of the library. My last literary infatuation was with *Alexandria Quartet*, the work of a twentieth-century British expatriate novelist, Lawrence Durrell, when I was sixteen and in college. I longed to know someone like his mysterious heroine, *Justine*; I read about her in a romantic haze on the banks of the Charles River in front of Leverett House.

I rarely read fiction by choice after getting into science, but after I became interested in the history of science at UCLA I turned to biographies of scientists and statesmen. I made an exception to the no fiction policy for the novels of Doris Lessing and V. S. Naipaul. Years later, Ellen and I read the complete set of Patrick O'Brian's novels about the adventures of English sea captain Jack Aubrey during the Napoleonic wars. I look upon it now as a form of nesting.

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NHS was also blessed with musical talent. Two schoolmates, Steve Kuhn and Roger Kellaway, went on to professional careers in what was called then "Modern Jazz." Another classmate, Richard Sudhalter, stimulated in me a lifelong interest in old jazz. I already was listening to the swing bands Mother and Aunt Charlotte had danced to—Glenn Miller, Benny Goodman, Tommy Dorsey, and Mother's favorite, the Casa Loma Orchestra—but Richard's passion for jazz opened a whole new dimension. Richard, the son of Al Sudhalter, who had played saxophone in Artie Shaw's swing band, would take me to ratty little record shops near South Station in Boston to look for recordings of 1920s jazz. Sometimes Jurgen Meyer-Cuno, a Sudhalter jazz disciple whose father worked in the German Consulate, would join us. We saw Richard tremble with ecstasy when he found a 78 rpm recording by the legendary cornetist, Bix Beiderbecke. Today Bix is considered second only to Louis Armstrong from that period. Meanwhile, I was hearing Armstrong almost every day on Bob Clayton's disk jockey radio show; Louis had, according to Sudhalter, sold out to commercial interests.

Richard played cornet like Bix, and after he moved to England Richard organized several musical groups that recorded the jazz of the 1920s and 1930s. A journalist in his working life, he authored biographies of Bix Beiderbecke and Hoagy Carmichael.[1] Richard believed that the white musicians who played in the musical shows and dance bands of the jazz era made musical contributions as important as the legendary black musicians like Louis Armstrong and Billie Holiday, whose predecessors started their careers in the brothels of New Orleans and on the Mississippi riverboats that carried black jazz to St Louis, Kansas City, and Chicago. At the listening and playing level Richard could sustain his case, but the life histories of the great black musicians shine a stark light on America's racial divide that elevates their stories to a different plane.

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[1] Richard M. Sudhalter, *Lost Chords: White Musicians and their Contribution to Jazz, 1915-1945* (New York: Oxford University Press, 2001); Richard M. Sudhalter, *Stardust Melody: The Life and Music of Hoagy Carmichael* (New York: Oxford University Press, 2003).

By the 1980s, many 78 rpm recordings that Richard Sudhalter would have swooned over were digitally cleaned and transferred onto compact discs, and I started collecting them. I concentrated on jazz, swing, the Great American Songbook, and jazz-inspired popular music from the 1920s to the 1980s. At first I found old jazz compact discs in commercial record stores, but after CDs became obsolescent in their turn I would find them in thrift stores or on the internet. Later, I discovered a fan of early jazz in the Cambridge market near Christ's College, Cambridge; from him I was able to acquire European and British jazz and popular music. Eventually, I recorded and catalogued the compact discs on Apple iTunes, and put the CDs away in boxes. By 2015 I was sure time and technology had passed me by, so you can imagine my pleasure when I learned that the UCSD library wanted the physical artifacts, perhaps for copyright reasons. It was not too difficult for me to give away the 5,000 or so discs that I had found because I could play them back on my computer. What is harder to bear is that they do not sound the same through my hearing aids. Nonetheless, I can still distinguish the great saxophonists Lester Young and Charlie Parker just from hearing a few bars.

In addition to jazz, there was baseball. The *Sporting News* arrived in the mail Thursday afternoons before I arrived home from junior and senior high school. The *Sporting News* was truly, as people called it, the "Bible of Baseball." I scanned its popular stories with passing interest, but I searched its small news announcements for clues to the addresses of baseball players; I wanted their autographs. I wrote for the autographs of *anyone* whose name appeared in the *Encyclopedia of Baseball,* which documented the batting or pitching records of everyone who played in the major leagues since the founding of the National League in 1876 (though not those who played in the fabled Negro League). Babe Ruth's widow cut his signature from a cancelled check and enclosed it with a nice note in her own envelope.

I could write to the baseball clubs of active players and eventually an autograph would arrive in the return mail, but the real challenge was finding retired players. One triumphant day I discovered that Lefty Grove, the Hall of Fame pitcher for the Philadelphia Athletics and later the Boston Red Sox, was still living in his hometown of Lonaconing, Maryland. I shot off a postcard to "Lefty Grove, Lonaconing, Maryland," figuring that someone there knew where he lived. A week or two later its detachable return postcard came back to me with his autograph. I still have the hundreds of autographs I collected before I went off to college, neatly ordered in albums by the player's lifetime record. These scrapbooks are my personal Hall of Fame.

Baseball fans were debating whether Ty Cobb in the 1910s and 1920s, or Ted Williams in the 1940s and 1950s, had been the greatest batter ever. When a modern team won the World Series, someone in the baseball commentariat would opine that this year's winning team was not as good as the 1927 New York Yankees. These arguments were entertaining, but there seemed no way to settle them. I myself wondered how my all-time heroes would fare playing against one another, and I tried to get at this question using what today we would call numerical simulation. I devised a game of dice-baseball using three dice, assigning to each possible triplet of integers from one through six a value: single, double, triple, homerun, ground out, fly out, etc. For each historical player, I made a table of values that statistically reflected his batting profile: batting average, home runs, etc. I assembled the player profiles into teams of all-time greats, played the teams of greats against one another in nine inning games, and kept seasonal batting and pitching records. A season comprised 154 games in those days.

I didn't need *The Sporting News* to know that Ted Williams was spending the baseball season in a house on Locke Road in Waban, a five-minute bike ride from my house. Every neighborhood kid knew that. One afternoon in 1950, I think, I listened to the end of a Red Sox game on the radio to guess the time Ted would arrive home from Fenway Park. I had parked on my bike in front of his house for perhaps fifteen minutes when a long, flamingo pink Cadillac El Dorado with nascent tailfins pulled up. I bet I know what you want, kid, he said. Here, help me take the groceries into the house, and I'll give you your autograph. The groceries took several trips back and forth. Then he sat me down in his kitchen, handed me a Coca Cola and a plate of cookies, and asked where I lived. We chatted for a while. I told him that my family and I lived about four houses from his teammate, the home run hitting shortstop, Vern Stephens. (The year before, Ted had hit forty-three and Vern thirty-nine). Ted taught me that baseball statistics and news stories correspond to flesh-and-blood human beings, and not to believe all you read about them. I thought he was truly gracious even though some Boston sportswriters professed to despise him.

Ten years later at Princeton, I would again create a personal hall of fame, this time of physicists. The games these heroes were playing had their mathematical aspects like dice-baseball but were altogether more serious.

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All of us in the all-Kenyon group at Newton High School had been doing college-level work for some time when, towards the end of our junior year, Bernie Lettau revealed the explosive news that he was going to Yale instead of spending his senior year at NHS. I reported the news about Bernie and Yale at the dinner table. I was jealous; if Bernie could do it, so could I—after all, he and I were both taking college-level work already, and my grades were better than his. My father listened for what seemed a long time, looked at me up and down, and shifting in his seat, finally announced with a mischievous smile that I could go to any college I wanted, *so long as it's Harvard.*

Later he said Harvard would give me the only chance I would have to learn things—philosophy, physics, Chinese—more difficult than I would ever encounter at NHS, and in life. Do not worry about grades, he said, just challenge yourself. It did not matter what subject, so long as it was challenging. There didn't have to be a plan; I would learn how to think. That way, when a complex challenge did come up in later life, I would not be afraid to take it on. This must also have been the time that my father made the astonishing statement that the President of Harvard was one of the ten most powerful men in America. *What? More powerful than a Wall Street tycoon? The Secretary of State?* Yes, indeed, because the Harvard President can convene the best minds in the world to work on every topic important to the human race.

Harvard had mixed appeal for Mother. True, it was high status and near home, but if you are going to leave Newton High, she said, why not go where you can acquire social polish? A year in a New England preparatory school would ready me for the social challenges of college. Harvard was pretty good, but it did not have the cachet of a New England prep school. So they reached a compromise; I would apply for freshman year at Harvard *and* senior year at Phillips Exeter Academy in New Hampshire.

I was required by law to take a course in Massachusetts history to graduate from high school; my father got a personal bill passed by the Massachusetts Legislature that removed that impediment to my application to Harvard. I have to admit to a shiver of satisfaction when the Harvard interviewer told Mother he wanted to interview me without parents in the room. Exeter's interviewer wasn't as scary as Dr. Beatty, but it did put both parents and child through their paces. Harvard said it was looking for people like me, while Exeter proclaimed what a privilege it was to be interviewed.

As things turned out, I was accepted by Harvard and rejected by Exeter. Harvard even gave me a National Scholarship, its most prestigious. The National Scholarship guaranteed that a combination of my parents' payments, the scholarship, and earnings from a work-study program would enable me to graduate without debt. This strange outcome was flattering, but counterintuitive. A member of the Exeter Board of Trustees was in my father's law firm and, without evidence, my mother wondered aloud whether *we* had been blackballed at Exeter because my father had failed to impress that prominent socialite. Fortunately, that cruel thought was only expressed in passing. She was impressed by the National Scholarship and consoled herself with the thought that I would make important friends at Harvard, just not as many as at Exeter. The most important social bonds are forged in prep school, not in college, she said, but no matter, Harvard was good enough.

How many times had I heard my mother say, *It's not what you know, it's who you know*? Her mantra made some sense, but it couldn't be the whole story; after all, it was my father's command of securities law that kept him with the august Boston law firm of Choate, Hall, and Stewart despite his lack of the social graces. How could I not wish for the way I lived my life to mediate my parents' conflict? Though "conflict" perhaps isn't the right word, for they were too much in love; maybe "harmonize their *a priori* emotional orientations to life's challenges" is a more accurate way to say it. In any case, the symphony of life was made dissonant by their discord. I was to be torn between creating knowledge and seeking social recognition, between solitary satisfaction in abstract science and social prominence through useful science. This dilemma never went away: when I am thinking I am not socializing, and when socializing I am not thinking. One or the other of the consciences implanted by my mother or father is sure to accuse me.

# 6

# Welcome to Harvard

It was decided: I would go to Harvard, but the tension between my parents' values played itself out throughout my four years there. Was the real goal of college intellectual growth or social positioning? I had chosen something intellectually challenging—science—as my father wanted, but to compensate I also became a student manager of the Harvard football and hockey teams. Two fellow managers indeed became lifelong friends, Warren McFarlan and Peter McElroy. Managing sports teams is not what your usual science major chooses to do, but I was really, *really* leery of being beaten about by boys two years older than I was. When I learned that managing a sports team satisfied the athletics requirement for graduation, I signed on as a manager of the football team in the fall and the hockey team in the winter. This preserved me from becoming a solitary bookworm with library pallor.

No one on the football team noticed when I, a lowly freshman manager, turned up in the locker room with my Revere tape recorder playing Harvard football songs. I take that back—one football player always did notice: Teddy Kennedy, Senator John Kennedy's brother. Whenever he saw me Teddy enthused about the songs and the tape recorder, and he even told a reporter—I think it was Will Cloney—who wrote that the fight songs and the newfangled tape recorder were my idea. My fifteen minutes of fame in the *Boston Herald* awakened me to the Kennedy family's clout in Boston. Since the Kennedys were Boston Irish aristocracy and not New England WASP aristocracy I wasn't sure my mother would be impressed, but I was.

How had I, a New Englander, missed out on hockey? Harvard was good in collegiate hockey in those days, and Harvard's Bobby Cleary, Billy Cleary, Lyle Guttu, and Bob McVey later played on the US Olympic team that won the Olympics at Squaw Valley in 1960. The Cleary Brothers were Boston Irish boys, Lyle Guttu was from—*get this*—Thief River Falls, Minnesota, and as I recall Bob McVey had played for Choate, John Kennedy's prep school in Connecticut. I travelled by bus with the hockey team in the dead of winter to desolate towns in far upstate New York, where we took on the fearsome hockey powers Clarkson and St. Lawrence. In my junior year, we were played with by the Russian National Hockey team in the Boston Garden, and we travelled to the Universities of Minnesota, Michigan, and North Dakota over the Christmas vacation. When the Harvard team bus arrived at Michigan State, a black limousine came to take me away. I had met Mary Hanna, the daughter of Michigan State's President, at Wellesley College, and we had agreed to have coffee together on my visit. It was a friendly and not a romantic assignation, but I still had a hard time living it down with the team. Nonetheless, I liked the social cachet of it all. My mother would have been pleased.

I ended my four years at Harvard as head manager of junior varsity football and varsity hockey, and, of all things, a member of the Harvard Varsity Club. The head hockey manager was required to submit an annual report to the Athletics Department. My written report said that the twenty-nine hockey games we played in 1959 created an undue burden on our players who, unlike those at some other schools, were not absolved from academic demands. What other college athletics department would pay attention to a student manager and reduce the number of sports events? Harvard's did. Chairing scientific committees in later life was a piece of cake compared to managing a bunch of rowdy young men who play a game where the judicious use of violence is good strategy.

During my sophomore year, I began to find envelopes slipped under the door of my room in Leverett House. These were invitations to be "punched"—to go to a reception where a potent alcoholic punch was served. Thus softened up, you were looked over for prospective membership in one of Harvard's final clubs. One day I told my mother over the telephone that I had received one of those envelopes. Who was it from, she asked. The Speakers Club, I replied. What did you do? I didn't go; I had too much studying. She made me promise to accept the next invitation I got. That one turned out to be from the Pi

Eta Club, the club for Boston Irish jocks like the ones I had been hanging out with every weekday afternoon during sports practices. Later, I learned my favorite uncle, Jim Queeny, had been a member of Pi Eta. The Pi Eta did not transform my daily life, nor did it materially advance my life prospects, and it certainly did not hone my intellectual skills, but I did enjoy going to the clubhouse late at night and reaching into the cooler for a can of beer. Isn't that what the "well-rounded" college man does?

Since then, Harvard has slowly but surely eased out its Final Clubs, and I am not sorry.

College is said to be a time of social awakening, and I am sure it is for many people, but I was too busy with my own affairs to have much time for ethical growth. I did make an exception for Joan Baez, the seventeen-year-old daughter of a physics professor at Boston University, who spent weeknights singing at the Mt. Auburn 47 Café, a short walk from Leverett House. She already had the presence and self-possession that I later recognized in the other folk singers who were to follow her in the 1960s. I fell in love with her from a distance and was gently introduced to the message of social justice that had not yet begun to preoccupy the many who came later.

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Football, Hockey, Pi Eta, and Joan Baez were still in the future when I reported to my advanced standing advisor, Mr. Hanson. I had to plan my curriculum for freshman year. After the usual pleasantries, he launched in: What do you want to do after you graduate? ...Well sir, I am not really sure, I said... Do you want to be a lawyer like your father? Again, sir, I really haven't thought things out... Well, Charles, in case you are thinking of law, I can arrange for you to graduate with an AB *and* an LLB after five years. You could be practicing law at age twenty-one! Of course, you would have to get started right now. Still not sure? ...Well let's at least not preclude the option. We will plan your first year as though you are going through with the double degree.

He had already written down Humanities 5, noting my high school interest in literature. *The most important thing is to get your science requirement out of the way*. Harvard has seven or eight general education courses in science; which one do you want? ...I guess the one on astronomy and geology, I said, Natural Sciences 7... I had read about astronomy in *Scientific American*. Good, he said, and wrote Nat. Sci. 7 on the card I would have to sign later. By the way, he went on, there is a wonderful course on the human sciences, anthropology and psychology; if you take that one too, you would not have to worry about science ever again. He wrote Soc. Sci. 4 on the card. In fall term you must take English A, but that ought not present a problem; in the spring you could take a language. Let's see, you already had three years of college level French at Newton High, so how about German? That appealed; my father's mother was German. That leaves your fourth course, he went on, hurrying to finish. ...Uhh, sir, my father wants me to take something really difficult, because it will teach me how to think... Mr. Hanson sighed: Oh well, I see you already had calculus in high school, how about second year calculus?

Well, that does it then. Welcome to Harvard.

Mr. Hanson *was* ambitious, and his reputation depended on how far he could push his young advisees. I thought there was a risk to his approach; Harvard was evidently too demanding for some people. I did not worry for myself, but when I went to him near the end of freshman year for another advising session, I told him I had been bothered by the number of suicides at Harvard that year. Mr. Hanson's response: *It's a competitive school; get over it.*

Mr. Hanson's two throwaway science courses, Natural Sciences 7 and Social Sciences 4, had the greatest impact of any I took during my four years at Harvard. I will have much to say about Natural Sciences 7 shortly, so I will start with Social Sciences 4. The anthropologist Clyde Kluckhohn and the psychologist Henry Murray co-taught Social Sciences 4. Kluckhohn's part was about A. R. Radcliffe-Brown's classic work on the Andaman Islanders, and Franz Boaz's work on the potlatch culture of the Northwest Coast indigenous peoples. The competitive gift-giving displays in potlaches were alien to a New Englander, but that was the point of the course.

I do not remember much about Henry Murray's part of the course, except that he invited about ten of us from class to dinner in his elegant home on Beacon Hill. It was an affair to tell my mother about: antiques, silver, candlelight, crystalware, elevated conversation. During dinner, Murray brought up the pioneering studies of adolescence done by his colleague, Erik Erikson. Erikson (1902-1994) was looking for student volunteers to be research subjects. You would learn far more about psychology volunteering for Erikson than you ever would in my class, Murray said. One of Erikson's postdocs was at that dinner to take names (I believe it was

Ken Keniston), and soon I found myself making regular visits to Harvard's Psychology Department.

Erikson's people wanted to know all about me. In my sophomore year, they gave me psychology tests and mini-therapy sessions. In my junior year, they wanted to know what I thought. I wrote up a philosophy of life, which the postdocs who had conducted the mini-therapies reviewed and re-reviewed with me to make sure we both understood what I was straining to express. The culmination was to be in my senior year. They said they wanted to see how two adolescents perceived each other's values. Another subject and I would interview one another about our philosophies of life, and afterwards the research team would conduct tests and interviews to see how much of the other's philosophy we had perceived. What communicated our deepest values: words, body language, something else? The interview was to be filmed. I was warned the filming would be distracting, which ensured that I would find it intrusive.

Today's institutional review boards for the protection of human subjects would never let Erikson do that experiment now. What I did not know was that my interviewer was to be a trained litigator from the Harvard Law School, who got a bounty each time he got me to take back one of the points the team had identified as a key element of my philosophy of life. I did not learn this until I was about to graduate. In the meantime, the Erikson team interviewed me about the changes over time in my reactions to that disastrous interview. They followed up with questionnaires after I graduated. The whole thing was so traumatic that to this day I have no wish to reconstruct what my philosophy of life had been. I am not even sure it is a good idea to have one.

What had been Erikson trying to do? He was pursuing the un-Freudian notion that psychological growth is lifelong and takes place when one confronts and resolves a series of crises, spaced at predictable intervals throughout life. In my case, Erikson was inducing identity crises in Harvard adolescents. It was truly painful to absorb Erikson's views so viscerally, but I learned that one's life prospects are not completely predetermined *in utero* or in infancy. Life's trajectory need not be inevitably downward; at each stage you can grow, but each growth opportunity presents itself as a crisis.

Isn't that what a humanistic education should teach?

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The American geologist L. Don Leet (1901-1974) and the Dutch astronomer Bart Bok (1906-1983) co-taught Natural Sciences 7. Today's student guides might call the course Astronomy and Geology for Poets. The main thing I remember about Leet is that he spent valuable class time histrionically debunking Alfred Wegener's theory of continental drift. It may seem from maps that South America fits into Africa, or that their fossils are related, Leet thundered, but it was science's obligation to view with unclouded eye what the popular mind sees as obvious. Since Leet outright dismissed questions my popular mind saw to be reasonable, he almost singlehandedly turned me away from science. Nine years later, a conversation on a train ride from Princeton to Washington with the great geologist Harry Hess would fill me in on the most important advance in earth science of the twentieth century: plate tectonics and its geographical consequence, continental drift. Leet was furiously defending the trailing edge of geology.

Bart Bok, on the other hand, was joyously on the leading edge of astronomy. In 1955, he was pioneering the use of radio telescopes in astronomy, and he took his Natural Sciences 7 class to visit a radio telescope Harvard had under construction a few miles west of Cambridge. (Six decades later, each drive past the Mullard Radio Astronomy Observatory on the drive to Cambridge University from London would call up associations with Bart Bok's observatory at the Agassiz Station in Harvard. Later, I would also associate the Mullard Observatory with the discovery of pulsars.) Bok's telescope was pretty neat, but it is not the main reason I remember Bart Bok.

One of his class requirements was to write a paper on our choice of topics in astronomy, and my paper elaborated on a topic that I had read about in high school. I was sure it was about the paper when, to my great embarrassment, Bok called out in front of the class, will Charles Kennel come see me? I made my way down the steep stairs from the last row of seats in Allston Burr Hall to meet the great man at the lecture table. Before Professor Bok could say anything I blurted out, I am sorry, sir, about the paper; what did I do wrong? I can rewrite it… The paper is why I asked you to see me, he said, but I want to know where you learned that much about the interstellar medium… You see, sir, in high school, I had a subscription to *Scientific American;* I did not understand most of its articles but I could read the ones on astronomy… Have you thought about concentrating in astronomy? Bok asked. I really hadn't… Even so, why not come to the next astronomy concentrators' dinner and see if we can convince you?

The astronomy concentrators' dinner was in Leverett House on the Charles River. I was in Matthews Hall in the Harvard Yard, where they put freshmen, so I did not know what the Harvard houses were like; that at least would make the dinner interesting. Jan Oort, the great Dutch astronomer, was Bok's guest that evening. After Oort finished his talk, Bok called me out again. When I arrived at the head table, Bok turned to Oort and said, Professor Oort, I would like you to meet Charles Kennel, our newest astronomy concentrator. Of course, I said something polite. After a few days, the astronomy concentration idea began to appeal to me. At least somebody wanted me, and Mr. Hanson would not rush me into Harvard Law School; that mattered a lot.

I did not learn why the Astronomy Department wanted me until later. Harvard's young Dean of Arts and Sciences, McGeorge Bundy, had come down hard on the Astronomy Department for poor academic performance. Bundy was the son of a senior partner in my father's law firm, Harvey Bundy, and later became Presidents Kennedy and Johnson's National Security Advisor. Mac Bundy was the kind of person I imagine my mother wanted me to be: cool, intelligent, forceful, socially impeccable. That Bundy had threatened the Astronomy Department with dissolution unless it improved its academic performance.

In response, the Department managed to recruit three undergraduate concentrators my year who would go on to be professional scientists: Peter Stone, who became a professor of atmospheric sciences at MIT; John Gaustad, who went on to teach astronomy at Berkeley and Swarthmore College; and me. I ranked myself the third of those three (Jerry Ostriker, who became the most famous astronomer from my Harvard class, must have majored in physics, not astronomy). We were pawns in an academic chess game, but undergraduate recruitment was never going to appease Bundy. By recruiting new faculty and striking up an alliance with the Smithsonian Astrophysical Observatory, the Harvard Astronomy Department managed to keep Bundy's wrath at bay.

**Figure 6-1: McKinlock Hall, Leverett House. I passed my sophomore, junior, and senior academic years (September 1957 to June 1959) with roommates Bob Brown and John Snygg in Leverett D-43, whose entry was beyond the right-hand edge of this picture. Our rooms were a ten-minute walk to the classrooms in Harvard Yard. I worked occasional evenings in the Leverett House Library.**

I do not remember what my advisor, Mr. Hanson, said when I told him I was going to concentrate in astronomy, but by then I was more concerned with what my mother and father would say. It took great forbearance for my parents to resist the warnings of one Colonel Bunker who wrote them a personal letter saying that I was about to be seduced into godless communism by the radicals on the Harvard faculty. Radicals were lodged in the sciences, and he named one notorious professor of physics, Wendell Furry. However, regarding my decision to study astronomy, my parents were together on one thing: was I aware that astronomers do not make much money? I wasn't thinking of a lifetime commitment, I answered, only of what courses I was going to take next year. I had to choose something, after all, and science was one of the subjects my father had recommended for mental growth. In any case, by entering college early I had earned extra time; I could go to law school after astronomy and still not be behind my age cohort.

Then, in October 1957, the Soviets launched the first artificial earth satellite, *Sputnik*. The news awakened Americans to the unthinkable notion that America could fall behind Soviet Russia in science and technology. In the nuclear age science and technology were matters of life and death. *Sputnik* catalyzed an immense national effort that was to propel my career forward, though I could not see that at the time. What I did see was that the nearby Smithsonian Observatory recruited fellow undergraduate, John Gaustad, to work on the computations of *Sputnik*'s orbit. John would go up to the Observatory after class and do real science; if he could do that, so could I.

*Sputnik* neutralized the anxiety my mother implanted within me about the social acceptability of science; maybe I was seeing a path to social success after all. And the space program involved the field my dead father worked in during the war in a big way. His yearning to return to Lockheed and to California was unspoken, but unmistakable. *Sputnik* took the pressure off the choices I had to make about the next two years, but it did not change the big question. I was not one of those, seduced by science in early adolescence, for whom there is no other path in life, whose unconflicted passion for science drives them to pursue details to the very end, even when no one else is interested. Instead, I was hyper-aware of what others were thinking all the time, which made my choice harder. How would I choose between law, business, or science? I may actually be one of those of whom it has been said that *Sputnik* was the reason they went into science.

Another thing I worried about but did dare not talk about was what life path would convince the kind of woman that went to Wellesley, like my mother—or Vassar, or Smith, for that matter—to marry me?

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The legendary Frances Wright called me in my Leverett House rooms one spring afternoon during my junior year. Miss Wright was the venerable Secretary of the Harvard Astronomy Department, and the first person every student encountered on visiting the Department; she was rumored to have more influence on departmental affairs than the chairperson. I was afraid her call was about the C-minus in Applied Mathematics 202, but instead she asked if I were thinking of going to graduate school. I had a hard time wrapping my mind around the notion at that very moment, so I said I really hadn't thought much about it.

Miss Wright persisted; we like to send our best students to Princeton. (*Best students? What about the C-minus?*) Some people from Princeton will visit in a couple of weeks to interview prospective students; would you like to meet with them? I am really not sure, I repeated, I need to think about it, can I call you later with my answer? I am afraid, Miss Wright replied, we have already put you down for an appointment; can you come to the Department at such-and-such a time? It was not a good thing to disappoint Miss Wright, so I yielded.

This graduate school idea only added to the confusion, but it gradually dawned on me that this was what had happened with Bart Bok two years earlier: *these astronomers actually wanted me*. Besides, I was two years ahead of other undergraduates and could spend a couple of years at another Ivy League school without losing anything but the time I had earned; two years at Princeton could not possibly harm my prospects if I chose to do something else. There was another thing: now I had to worry about finances. After my father died in the summer between my sophomore and junior years, Harvard cut the scholarship portion of my National Scholarship. When I, mystified, went to ask why, the student aid officer explained that my father's business debts had been taken into account in calculating the amount of my scholarship; after he died, my father's debts were liquidated along with his debt-ridden electric train business, and my widowed mother had been left with some cash. To this day, I am struck by the cruelty of what this student aid officer said: "Your mother has cold cash; make her spend it." She did, and when she died twenty years later she was nearly without resources. Harvard is the richest university in the world because its finances are managed by coldly realistic New England aristocrats, I thought bitterly.

Law and medical school suddenly seemed out of reach, and I knew science departments were providing graduate fellowships and teaching assistantships. I could spend a frivolous two years at a university my widowed mother could be proud of without burdening her financially. At sixty-one years' remove I can no longer remember Princeton's interview, except that I had been comfortable enough to pass muster. Uneasy about being rushed I also applied to Columbia University in physics, but was turned off when their acceptance letter informed me that seven years was their typical time to degree, and that intelligent students avoided walking in Central Park at night. In any event, Harvard and Princeton conspired through their recommendations to win me a Woodrow Wilson National Fellowship, so the money problem was taken care of and I could look forward at least one more year in a comfortable academic environment.

First-year students were housed in the Princeton Graduate College, a magnificent faux-medieval building patterned after Oxford and Cambridge colleges, with a porter's lodge, an interior courtyard, a playing field, a tennis court, a dining hall, and separate entries. Princeton evidently did not consider itself without peer as Harvard, the Hub of the Academic Universe, did, and turned to foreign academic traditions for inspiration. The Graduate College mimicked Oxbridge in other ways: there were cleaners to tidy student rooms, and students went to dining hall in academic gowns. I shared a set of rooms in the Graduate College with Morty Rubin, a passionate physics phenom who had taken a double major at MIT. Morty was already preparing for the PhD qualifying examinations, which most students did not take until the end of their second year. Morty was totally immersed in physics; he hunched over his desk, nodding and chuckling as he calculated away. He found humor in group theory!

He was born to it, a natural; I wasn't.

There were other graduate students at Princeton whose intellects were off the charts. Jack Douglas comes to mind. A sociologist, he had been in my class at Harvard but left a year early for Princeton. Jack was cursed with a photographic memory; he never read newspapers because he remembered the junk advertising. One long lunch he recited all of Shakespeare's *Hamlet* from memory, including stage directions. Jack told his astonished friends he was from Miami, the son of an unknown father, and that he had two half-brothers in jail. He had haunted the public library after school to keep warm until a Harvard graduate discovered Jack there, lent him his private library, and got him admitted to Harvard. At Princeton, upon being threatened with dismissal for not going to class, Jack straightway took his PhD exam and won the graduate school's highest dissertation award, the Procter Fellowship, all in his first year. Who could compete with a biological sport like Jack?

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I found myself studying the people doing science at Princeton as much as the science itself. I was invited early on to the homes of the two singular figures who dominated the Princeton astronomy community at mid-century. Martin Schwarzschild (1912-1997) was the son of the Astronomer Royal of Kaiser Wilhelm's Germany, Karl Schwarzschild, known to physicists as the originator of the most widely used solution to Einstein's general relativistic equations for gravity. Martin and his wife, Barbara, were at pains to tell his story at our first meeting, the horror of Hitler being only two decades past. Martin had been working on his doctoral research at the University of Göttingen when Hitler's edict banning Jews from participating in university life came down. The faculty were horrified but could only resist surreptitiously; in Martin's case, they resolved that he finish his degree as soon as possible and contrived to give him his final examination in a blacked-out basement room.

Upon his passing the secret examination, they handed Martin a sum of money and put him on a train out of Germany. Ultimately Martin made his way to Princeton and began a lifelong astronomical partnership with Lyman Spitzer. Science literally saved Martin Schwarzschild's life. In 2019, my space physics colleague Jörg Büchner took me to Karl Schwarzschild's former home in Göttingen, and I tried to picture Martin as a young person there. Göttingen's pastoral atmosphere reminded of Princeton—or maybe Princeton reminded me of Göttingen, I cannot tell which. There I also saw the house where another of my Princeton professors had lived as a young man, the Nobel laureate Eugene Wigner.

Lyman Spitzer (1914-1997) was from a well-to-do family in Toledo, Ohio, and blessed with a first-class education from Yale, Cambridge, and Princeton. His carriage was elegant and his speech refined, qualities my mother wished for me. Lyman and Martin wanted to keep Princeton's educational program in astronomy small; Princeton's students finished well before those in other astronomy departments. It was better to spend the years you saved as responsible professionals rather than as dependent students.

The challenge for Princeton was to supplement the small-group intimacy that was best for teaching and creativity with the technological resources needed for modern research. Lyman was among the first to foresee that astronomy was to become big science. The Princeton Astronomy Department, Lyman and Martin reasoned, was too small to afford a state-of-the-art telescope, so they designed their research around summer visits to Caltech's Palomar Observatory in Southern California. They preferred to travel to the best available telescope rather than make a dean happy by decorating the campus with an ordinary one. Lyman was willing to go even further than California for observing; in 1946, eleven years before the first earth satellite was launched, he wrote a report for the Rand Corporation that proposed launching a telescope into space. Later, Lyman brought to Princeton the management of the ultraviolet telescope on board America's first successful space observatory; he is the acknowledged godfather of the Hubble Space Telescope.

Space telescopes were not the only big science projects Lyman Spitzer originated. He conceived the Stellarator Nuclear Fusion Device and founded the Princeton Plasma Physics Laboratory, where I was to write my PhD thesis. Lyman's books on fully ionized gases and the interstellar medium were my introduction to the theoretical framework in which my plasma physics intellect was sharpened. Perhaps even more important, I absorbed an attitude toward energy that contrasted with what my contemporaries were learning from their teachers, or the media for that matter. He used to say that controlling nuclear fusion would make energy "too cheap to meter." Rather than be used to destroy civilization, fusion energy could sustain and advance global civilization into the indefinite future. Science could solve society's biggest problems.

Martin Schwarzschild taught his course on stellar evolution to three of us using his new book as text, and a young George Field guided a one-person Oxbridge-style tutorial on Lyman's book on the interstellar medium. Then very junior, George was later to found the Harvard-Smithsonian Center for Astrophysics. I was not afraid to ask him naïve questions, because he was close to me in age. The high points of the week were the journal club meetings when Martin and Lyman reviewed current events in astronomy. I learned from Martin what mastery of a subject was; he could put a new astronomical fact in perspective as soon as he heard it. Lyman would sit up in alert appreciation each time Martin fit a new micro-fact fact into the big picture.

Lyman saw science and technology as inseparable, and viewed nuclear fusion as the future of cheap, widely available energy. Martin would nod approvingly each time Lyman mentioned using the deuterium and tritium in seawater as fuel. There was an awful lot of seawater in the world and enough nuclear fuel for fusion to sustain human civilization for millions of years. Human civilization could live as long as a star! While this grand vision moved Martin, Lyman wanted to make it happen in his lifetime. Fusion energy was surely going to power economic and social development in the next generation. Those of us who listened in on their intimate dialogues were seeing pure science take the first steps on the road to social utility. Martin and Lyman were a true symbiosis; they worked side-by-side all their lives and died within weeks of one another.

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I learned only after the fact that emotional depression is commonly associated with mononucleosis; all I knew during my two-week confinement in the Princeton Infirmary was that indecision had again caught up with me. With time to think, it occurred to me that enrollment in graduate school implied a lifetime commitment to a discipline and mastery of its concepts and methods. I wasn't sure that I was ready for all that. I needed time off; perhaps I could use one of the years I had earned by entering Harvard two years early, but I couldn't ask my widowed mother for financial help. I would have to get a job. It was 1960, post-*Sputnik*, the apex of the Cold War, and the pages of *Scientific American* were filled with advertisements for scientists to work in the defense industry. The work would not be as demanding as graduate school and I could earn some money. Maybe I could even buy a car.

I was surprised how seriously Martin Schwarzschild took it when I broached the idea of working in industry to him. You are young, he said, and time off sometimes helps. He told me about the famous astronomer, Henry Norris Russell, who had fled from Princeton after thinking he failed his qualifying examination but came back to finish an outstanding degree and ultimately became one of the greatest astronomers of his generation. Give us a little time to think out a good place for you to go, Martin said. After a while he came back with a collective verdict. We originally thought the Meudon Observatory in Paris was a good choice, he said, but then we figured you might not want to come back from the undeniable attractions of Paris. They had also thought of sending me to California, but they feared I would never come back from California, either. But there is one place in your hometown that is doing very good work, he said: the Avco-Everett Research Laboratory. I had seen their advertisement in *Scientific American* and I filled out the application. Martin and/or Lyman made the necessary telephone call, and soon I had the first serious job of my life.

# 7

# Apprentice Physicist

Arthur Kantrowitz (1913-2008), a PhD student of Edward Teller, the "Father of the H-bomb," was the founder and Director of the Avco-Everett Research Lab (AERL). In 1955, Arthur left a professorship in aeronautical engineering at Cornell University at age forty-two to found AERL and brought his favorite PhD student, Harry Petschek, with him. The place had a mystique; people in the lab were already telling Avco origin myths when I arrived in the summer of 1960. Apparently, Kantrowitz's team had set up their first experiments before Avco's primitive cinderblock building was even completed; the staff had no furniture other than discarded wooden crates. Sometimes you could smell the outgassed fumes as you walked through the front door in the morning.

AERL's graceless building, built on recovered land under the Logan Airport landing pattern, was as finished as planned in 1960. Kantrowitz already had the air of quiet confidence that I later learned to associate with successful technology entrepreneurs. By 1960, the scientists and engineers had metal office furniture in cubicles that were frequently rearranged as projects changed. No Oxbridge or Princeton elegance for Avco. No elegance whatsoever.

The place was busy. AERL played a critical role in the US missile program, and the lab was created to work out the chemistry and radiation physics of missile re-entry, and then transfer that knowledge to larger companies fabricating intercontinental ballistic missiles. Most of Avco's work in high temperature gas dynamics was in the laboratory, but it also had a team on Kwajalein in the Marshall Islands to study the re-entry of missiles on intercontinental trajectories. The shock wave standing ahead of a re-entering missile creates a partially ionized plasma in the flow around the around the missile's nose cone, so AERL had to deal with plasma physics, and it had developed a reputation as an early entrant into the field.

However, the shocks AERL created with its partially ionized laboratory plasmas were collisional, as was appropriate to the missile re-entry regime (this meant that the charged ions and electrons comprising the plasma component of the flow would collide with the atoms of the unionized gas phase component.) Nuclear weapons and controlled nuclear fusion research had stimulated interest in fully ionized *collisionless* plasmas, a subject Lyman Spitzer had pioneered in his studies of the interstellar medium. The AERL team had tried and failed to push the operation of their shock tubes into the collisionless regime. Studying collisionless shocks in the laboratory seemed out of reach, and Harry Petschek had to put his interest in collision-free plasma physics to one side.

Harry Petschek (1930-2005) was the rebellious scion of a wealthy Jewish family that built its fortune in coal and banking in today's Czech Republic. The original family home, the Petschek Villa in Prague, has housed the US embassy since World War II. Harry's family escaped Hitler to New York City in 1938 with much of their art and fortune intact, and Harry, eight years old when he arrived, was brought up in urban luxury. (The Petscheks even replaced the works of art in the Prague mansion with copies and had the originals quietly shipped to New York to await the arrival of the family.) Harry's background was like Robert Oppenheimer's, another scion of a wealthy Jewish family from Central Europe. Like the Oppenheimer brothers, the Petschek brothers, Harry and Albert, went into physics, a notably meritocratic profession.

Harry refused all financial support from his family when in graduate school at Cornell. His young wife at the time could not understand why he insisted on living needlessly in poverty and divorced him, and research with Arthur had been Harry's solace during the divorce. By the time I knew him, Harry and his second wife, Barbara, a warm down-to-earth woman, were living a studiously typical suburban life in Lexington, Massachusetts. Harry had relented: unlike Henry VIII's Lord High Chancellor, Thomas More, Arthur Kantrowitz's wizard, Harry Petschek, no longer insisted on hairshirt poverty. Harry would live like other American physicists, but no better.

Harry and Arthur did not know what to do with a twenty-year-old kid with one year of graduate study. They originally put me to work on a blue-sky study of a new kind of re-entry vehicle, the “drag brake,” a missile nosecone that could vary its angle of attack during re-entry like an umbrella. It soon became apparent that the drag brake was not going to be successful, and the program was canceled. That was when Harry volunteered to take me on. I will never forget what he said in our first conversations: we at Avco have failed to produce collisionless shocks in the laboratory, but satellites have found a collisionless plasma in the geomagnetic field surrounding the earth *and* a supersonic solar wind upstream of the earth. The solar wind and geomagnetic field plasmas will interact to create a shock. The first collisionless shock will be discovered in space, not the lab. *If you teach me about the new observations*, he said, *I will teach you the relevant theory, and maybe we will be ready to understand the first collisionless shock observations as soon as they come in.*

Harry taught me how research is done. I was able to draw from memory the main diagrams from the observational papers I was reading. I did not need to take the journals with me; I carried the observations around in my head. Harry was a master of semi-quantitative dimensional argumentation. Starting with my descriptions of the data, he could frame the physics issues with simple estimations at the blackboard. I could visualize the physics behind each step in the developing calculation because he would first argue verbally, draw a sketch, and finally conjure an order of magnitude estimate. This taught me that size matters. If the size looked right, it would fall to me to redo the computation.

This was how I discovered what newborn research was really like: utter chaos. Nobody knew what they were doing; observations were messy, theories disagreed, people argued over the simplest of basics. The ugly stuff I was reading had none of the serene majesty that had seduced my Princeton particle physics friends. There is no well-ordered phalanx of physics heroes clearing your way; research is unknown people finding provisional order in the obscure. Research starts by figuring out what you need to know, Harry said, not with knowing lots of stuff. *It wouldn’t be research if we knew what we were doing.* Harry also said other helpful things. I thought I would never be as good as the mathematical *virtuosi* I had met at Princeton, but Harry freed me of this existential anxiety by telling me that I did not have to *invent* mathematics, I simply had to use it. He also told me that he got his ideas and criticized them on different days; I should never let the fear of a mistake get in the way of creativity.

The space science literature was rather thin in those days, so I had time to go beyond what little was known about collisionless shocks to read about other space observations. Something peculiar was happening in the Van Allen belts: electrons supposedly trapped forever in the Earth's magnetic field mirror were being lost to the atmosphere. Van Allen's electrons were making a mysterious flux of X-rays when they collided with the atoms in the upper atmosphere, and Kinsey Anderson's instruments on balloons high in the atmosphere were detecting them. I knew something analogous was happening in fusion research. The protons in magnetic mirror fusion devices had similar anomalous losses, but these were thought to be due to collisions with stray atoms. Since the plasma surrounding the earth had hardly any collisions of any kind, Van Allen's electrons should not have been doing what Kinsey Anderson's X-rays said they were doing.

Other than reading the space science literature, my main responsibility at Avco was to turn up at meetings where recent shock tube experiments related to missile re-entry and plasma flow experiments related to magnetohydrodynamic power generation were discussed. The group did not stop to explain things and I had to do the best I could. At one of those meetings a famous Soviet specialist in fluid flows, Leonid Sedov, gave an invited talk. Sedov brought with him a colleague, a young plasma physicist named Roald Sagdeev.

Roald and Harry had met at the 1958 Atoms for Peace conference in Geneva, and they spent a lot of time together during this visit to Avco. Their mutual admiration was palpable; there was a special light in their eyes when they spoke with one another. Harry had learned of Sagdeev's work at the Atoms for Peace Conference, and I already had read some, but after that Harry and I started to apply the theory of the whistler mode instability, ultimately published for fusion machines by Sagdeev and Shafranov in 1961, to the Van Allen belts.[2] We reasoned that the electrons in the Van Allen belts would have anisotropic pitch angle distributions; that their instability would generate plasma waves that resonate with the

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[2] R. Sagdeev and V. Shafranov, "On the Instability of a Plasma with an Anisotropic Distribution of Velocities in a Magnetic Field," *Soviet Physics JETP* 12, no. 1 (1961): 181.

frequency of gyration of Van Allen's electrons around the geomagnetic field; and the resonant wave-particle interactions would scatter electrons into the atmosphere, creating Kinsey Anderson's X-rays when they got there. By the time my fifteen months at Avco were up, I was pretty sure we were on the right track, a track that in four years would lead to the most widely cited research paper on which I have been an author.

But first I had to complete my education, so I returned to Princeton in September 1961. Harry's view was that the work mattered and not the false status of the degree, but he only half-heartedly opposed my return.

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Princeton physics was at the top of its game in 1961. I became friendly with a young French postdoc, René Turlay, and lived vicariously through the vicissitudes of his work with Jim Cronin's experiment on charge and parity violation, which was to earn a Nobel Prize for Cronin. We students were all aware of R. H. Dicke's pioneering work on the cosmic radio background, and the students thought Dicke the best experimentalist in Princeton. Dicke's student, Jim Peebles, was to win the 2019 Nobel Prize for work that started there. The great nuclear theorist, John Wheeler, who had written the key paper on nuclear fission with Niels Bohr in 1939,[3] was seductively claiming that black holes were more than just an elegant solution of Einstein's equations but actually exist in nature. The Institute for Advanced Study was a short walk from campus, and there one could find legendary figures including the physicists J. Robert Oppenheimer and Freeman Dyson. Einstein had only been dead for six years.

We students walked among these grand figures. My roommates and I invited Professor and Mrs. Wheeler to a dinner in our rented faculty house, cooked by graduate students, and they came. At dinner, the patrician Mrs. Wheeler was visibly shaken by the sound (and smell) of exploding bottles of student-brewed beer in the basement. That was when I noticed Mrs. Wheeler's instinctive grace, something my mother would appreciate. We also played evening pickup games of badminton in M. L. "Murph" Goldberger's

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[3] N. Bohr and J. A. Wheeler, "The Mechanism of Nuclear Fission," *Physical Review* 56, no. 5 (1939): 426.

backyard. None of us imagined that we were exercising with the future Director of the Institute for Advanced Study.

The mathematicians quibbled, but we students thought physics was king of the sciences. We were saying that with a physics education you could go into any other science—except maybe mathematics—but you could not come from something else into physics. The best of the mathematicians, John von Neumann, who had the first electronic computer in his office at the Institute, was half a physicist anyhow. There was a pecking order within physics, with high energy particle physics at the pinnacle and plasma physics too new to be ranked. Though it was not required for the degree in astrophysical sciences, I worked my way through the wondrous series of books on all of physics by the great Soviet physicists, L. D. Landau and E. M. Lifschitz. (Not a word of Landau and not a thought of Lifschitz, the students used to say.) Landau and Lifschitz used dimensional analysis to frame solutions to problems the same way Harry Petschek had. Their books told me that Russia was a *Shangri-La* of physics.

I also took a class in quantum electrodynamics from Eugene Wigner, later a Nobel laureate. Wigner's class was sink-or-swim for the theoretical physics hotshots, but I only needed to keep my head above water. I found quantum electrodynamics' combination of theoretical elegance and experimental agreement eerie. One memorable class, one of the young hotshots, Oscar Lanford possibly, brazenly interrupted Professor Wigner in mid-lecture; Landau solved the problem you just presented more compactly, the student said. Wigner, clearly annoyed, put down his glasses and addressed us for at least ten minutes. I know Landau, he said; Landau is very good, but he is not God. He has a facile way with problems, Wigner said, that can get him into trouble. You have got to think things through for yourselves and not rely on shortcuts, as Landau does. I have a very good forgettery said Wigner; I forget what other people have done, even what I have done, and I must work problems through each time from the beginning. Occasionally, you discover new things that way.

Wigner spoke many times of his article *The Unreasonable Effectiveness of Mathematics in the Natural Sciences.*[4] Wigner, not to mention the fearsome students in his class, convinced me I was not

[4] E. P. Wigner, "The Unreasonable Effectiveness of Mathematics in the Natural Sciences," *Communications on Pure and Applied Mathematics* 13 (1959): 1-14.

mathematically talented at the level demanded by elementary particle physics, so I would have to pursue a less mathematically demanding branch of science (like plasma physics). Nor was I like the great experimentalists who materialize answers to profound questions in devices and procedures. I did sense that I was better at verbal expression than my compatriots, so my personal advantage lay in synthesizing results obtained by collaborators scientifically more gifted than I was.

Fluency in the language of general scientific discourse became a survival skill for me. When discussing their research with collaborators, scientists speak specialized languages incomprehensible even to colleagues in nearby disciplines. To speak across disciplines they use the language of general scientific discourse, which presumes undergraduate-level familiarity with basic concepts, like force, evolution, the double-helix, relativity, the big bang, black holes, quantum theory, probability, pandemic, Fourier analysis, statistics, and so on; in recent years, words like environment, climate change, and sustainability have worked their way into the common vocabulary of science. Wherever the language of general scientific discourse is spoken, you can join the debate about research and its implications for society at large. Fortunately for me, its basis is English.

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My personal goal was to develop enough technical skill to bring the research Harry Petschek and I started to its conclusion, and for that I would have to learn plasma physics systematically. Luckily, Princeton was just then preparing to teach plasma physics systematically. I informed Martin Schwarzschild of my desire to transfer from the Princeton Observatory on the main campus to the new Princeton Plasma Physics Laboratory (PPPL), which housed an industrial-scale project on the nearby Forrestal campus. Lyman Spitzer, PPPL's founding Director, was overseeing construction of the first stellarator fusion device. In the experimental building there were enormous flywheels storing the energy used in powering the stellarator's magnets. If this wasn't big physics, I didn't know what was.

I believe I was the second student to enroll in graduate study at PPPL. Theorists like me were put in a modern but pedestrian office building that reminded me of Avco's quarters. PPPL's young plasma physicists became the first generation of senior figures in the field; Ed Frieman, Martin Kruskal, Tom Stix, Harold Furth, Carl Oberman, John Dawson, Ira Bernstein, John Johnson, and John Greene. During my first year back in Princeton, the young Division of Plasma Physics in the American Physical Society held its annual meeting at the Princeton Inn, across the lawn from the Princeton Graduate College where I had stayed before going to Avco. My eyes opened wide when a physicist from Los Alamos, Stirling Colgate, landed his airplane on the Inn's lawn and walked peacock-like into the meeting. (I do not know which was the more astounding—the airplane or the peacock walk.) All these young researchers, Colgate included, venerated a theorist of their generation then in California, Marshall Rosenbluth, who would play a big role in my life four years later in Trieste.

I did not know it at the time, but I was seeing the coalescence of the canon of modern theoretical plasma physics. I wrote my PhD thesis with the leader of the PPPL theory group, Ed Frieman. Ed was a New York City boy, fifteen years old at the time of Pearl Harbor, too young for the armed forces, but the right age to be rushed into and out of Columbia College to prepare for service in the Navy. He became an underwater demolition expert—one of the Navy's more hazardous jobs. This took him to the atomic weapons tests at Bikini Atoll in 1946. I do not know one scientist who had been at a weapons test who was not deeply affected in one way or another. It seems certain Ed was, though I never asked him; it was not something one talked about. After the Navy Ed did go into nuclear science, receiving an MS (1948) and PhD (1951) in physics from the Polytechnic Institute of Brooklyn under the guidance of Lloyd Motz. His PhD thesis was entitled *The Proton-Proton Reaction and Energy Production in the Sun*.

Ed's thesis must have caught the attention of John Wheeler and Lyman Spitzer, who were in the process of creating Project Matterhorn at Princeton, a secret program to achieve thermonuclear fusion for peaceful purposes. Ed left the City for bucolic Princeton and, in 1954 when Ed was twenty-eight, Spitzer asked him to head PPPL's theory division, the responsibility Ed had in 1961 when I became his student. The 1950s and 1960s were a time when physicists thought they could achieve anything, even controlled thermonuclear fusion—but controlling fusion was too hard, and both the Soviets and the Americans soon realized that peaceful fusion was so far off that it no longer needed to be classified. The political opening to today's international effort in fusion research was made at the Atoms for Peace Conference in Geneva in 1958. Shortly thereafter, Princeton converted Project Matterhorn into a research and teaching program within an expanded Department of Astrophysical Sciences. The University completed construction of the Princeton Plasma Physics Laboratory (forever to be called PPPL) in 1961, and Ed became its Associate Director in 1964.

Ed Frieman was a co-author of the most influential paper in the history of unclassified fusion research—the so-called energy principle for ideal magnetohydrodynamic stability—which provided a unified way to ascertain the basic stability of complex magnetic field and plasma configurations. Frieman, using his prodigious skill with long calculations, also did much to unify the field of plasma kinetics, using a sequence of reductions from the Liouville equation. This work did not make practical computations easier, but it enabled physicists to see how the various mathematical techniques to describe Coulomb collisions, most importantly Spitzer's collisionless idealization, emerge from fundamental principles. My impression was that Ed was seeking to harmonize complexity. My own thesis[5] was on drift waves in kinetic theory, a topic that Frieman and Paul Rutherford brought to near perfection a few years later. During his Princeton years, Ed expanded his presence on the national science policy scene as a consultant to government panels, the national laboratories, and industry—and, of later relevance to me, a relationship with NASA that endured until the end of Ed's life.

Shortly after I signed up with Ed Frieman, he subjected me to a surprisingly searching interrogation. Ed wanted to know what Harry Petschek and I knew about the Van Allen belts. One day, Ed said, somebody would inject energetic electrons into the Van Allen belts; would the injected electrons stay there without being lost to the atmosphere, as Hannes Alfvén's theory maintained? I answered that the electrons would not stay very long, even though Alfvén's adiabatic invariant theory suggested otherwise. You see, there was this whistler mode instability of Sagdeev and Shafranov that would produce plasma waves that scatter the electrons out of the belts. Ed probed round and round but eventually he relented, apparently satisfied.

Ed went away after my Van Allen belt interrogation. I didn't pay much attention; he went away a lot and I was preoccupied with

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[5] C. F. Kennel and J. M. Greene, "Finite Larmor Radius Hydromagnetics," *Annals of Physics* 38, no. 1 (1966): 63-94.

my thesis. I did notice when he came back from one of his trips with a suntan—in winter in Princeton, no less. When I asked where had been, he said he couldn't tell me. More months passed, and I forgot about the suntan. Then on July 9, 1962, the US exploded a 1.4 megaton nuclear device, *Starfish Prime,* at 248 miles altitude over Johnson Island in the Central Pacific. People couldn't miss the aurora lighting up the night sky above Honolulu: bomb-accelerated electrons hitting the atmosphere had created an artificial *aurora borealis*. Now I can tell you, Ed said; remember when the British astrophysicist A. C. B. Lovell had complained that US nuclear explosions will change the earth's radiation belts forever? Worried about political fallout, President Kennedy had asked a consulting group, with Ed as member, to meet in a sunny clime and recommend whether the *Starfish* test should go ahead or be called off. Clearly, the test went ahead. I never had the courage to ask Ed whether what I had said played any role in the decision to allow the Starfish experiment to proceed, but I had given him wrong advice. After a prompt initial loss, Starfish electrons were still detectable ten years later.

What had gone wrong with my prediction? In a later investigation with Richard Thorne, a colleague at UCLA, I figured out what had happened. The *Starfish* device was detonated at a low geomagnetic latitude, so the magnetic field lines on which the explosion took place did not extend far enough into space to expect whistler instabilities to occur, and the waves that were generated by those instabilities further out could not get to the *Starfish* lines of force. Eventually, as the *Starfish* electron belt slowly went away, so did my guilt.

No matter how airily diaphanous it may seem, I learned that theory, right or wrong, has real world consequences.

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The Cuban Missile Crisis, in October 1962, gave us graduate students a whiff of the world of the Cold War our professors had lived with for twenty years. A quiet panic settled over the Princeton physics community after President Kennedy announced that the Soviets had placed missiles in Cuba. Five of us were staying in a rented house on Harrison Street when Dick Roth returned from John Wheeler's class in nuclear physics. We all knew that Niels Bohr and John Wheeler wrote the history-making 1939 paper on nuclear fission that lay behind the development of the atomic bomb. Rather than go ahead with his planned lecture, Wheeler drew a map on the blackboard. He located New York, Philadelphia, and McGuire Air Force base, where H-bombs would be targeted. He drew circles of fifty-mile radii centered on each; Princeton was in a distant intersection of all three circles. Wheeler said that meant the shock waves from the explosions will not reach Princeton; radiation would, but the dose is survivable in a modest shelter.

Dick relayed Wheeler's estimate of the radiation intensity to us, and we turned our basement into a bomb shelter. We filled a bathtub with water, put down other supplies, and stuffed the basement windows with newspapers and anything else that had mass for shielding. Dick Roth calculated how much radiation protection we were adding. We were putting our education to use; we were our own best chance and were proud of it. Later, when the landlord saw the stuffed basement windows, he figured we were into "some kind of deviated pervert thing" and made us take the radiation protection out. You don't forget that kind of thing.

I do not remember why I was in New York City on a snowy weekend around then, but it was probably to visit Mary Beth Smith at Barnard College. At the end of a weekend in the city, I made my way to Pennsylvania Station to catch the last train to Princeton only to find that there was to be a major delay because of a blizzard. I sat on an old wooden bench with the other frustrated New Jersey travelers to wait for our train to be announced.

A little man with an English accent approached me while I was reading: You're a student at the University, aren't you? Mind if I sit down? I need someone to talk to. This turned out to be Freeman Dyson from the Institute for Advanced Study, one of my heroes of quantum electrodynamics from Eugene Wigner's class. Freeman spent what must have been an hour recounting how he wanted to be the first man on Mars; he was small and would not present a weight problem, and he was past his prime and willing to risk his life. He also described his work with Ted Taylor from General Atomics in La Jolla, Project Orion. You could lift enormous loads into orbit by exploding one hydrogen bomb per second inside an open semi-hemispherical cavity fastened beneath the payload. The momentum imparted to the ablated material in the exhaust flow could lift an Empire State Building into orbit. You could also carry enough nuclear bombs to propel a community of astronauts to Mars. Unfortunately, Freeman had just lost his chance to go to Mars: President Kennedy signed the test-ban treaty prohibiting nuclear explosions in the atmosphere. All this came out virtually nonstop until our train was announced.

It must have been 3 or 4 a.m. when we arrived in Princeton; Freeman had called ahead to his wife, Imme, to meet him at Princeton Junction; unknown to me he had asked her to drive me home too. I was staying with five other students in a rented house on Bear Brook Road, nowhere near the Institute. It was still snowing, but Freeman and his tired wife went out of their way to drop off a grateful graduate student. Recently, I read Freeman's last book, *Maker of Patterns,* where he recounts his life in the 1970s; it confirmed how kind and empathic he was. It also explained to me why despite his quintessential brilliance he did not win the Nobel Prize or match the deep originality of his contemporaries, Richard Feynman and Julian Schwinger. He was easily distracted by the feelings, hopes, and ideas of those around him in his family and work life. They do not give Nobels to polymaths or for caring for others.

Our paths crossed infrequently after that but Freeman's brilliant daughter, Esther Dyson, served with me on the NASA Advisory Council. Of course, I told her this story. Esther had tried to compensate for her father's frustrated ambition by training as a backup cosmonaut for a Russian mission to the International Space Station.

# 8

# Drafted by Marriage

Life had settled into a comfortable routine. I molted into an identifiable, if not inspiring, form of life: a graduate student. In addition to trying to master my subject I bonded with my roommates, and while I was not exactly a compulsive chaser I developed a serious relationship with Mary Beth Smith of Barnard College in New York. She was passionate about literature and her conceptual precision in discussing literature rivaled that of my physicist compatriots, without the benefit of mathematics. Our physical and mental passion was an unusually disorienting combination, and, assailed by ambivalence, I ended our relationship when I asked myself whether I was ready to be married.

Shortly thereafter I met the girl I was to marry: Deborah Bochner, a 1961 alumna of Smith College, who was the daughter of a Princeton mathematics professor, Salomon "Manya" Bochner. She had spent a year as a graduate student in history at Yale with the Renaissance scholar Roberto Lopez, and then transferred to the Harvard History Department. Debby's mother held the curious belief that her daughter would meet more marriageable men at Harvard than at Yale, and Debby submitted to her mother's irrational anxiety. I should have worried that Debby was so easily deflected from her course, but she had intelligence, culture, and phenomenal verbal skill—*and* she was from a prominent academic family.

My first meeting with my father-in-law to be, Salomon Bochner, was memorable. Debby and I had met at some graduate student party, and now I was to come to her house on Springdale Road to pick her up for a real date. I learned later that she was habitually late, so her father, who met me at the door, had already suspected he would have to entertain me. He led me into the living room and sat me down; he sat up straight, hands on his knees at ramrod attention, and stared quizzically at me, yet past me. If you wanted to know what the weather was like in fourteenth-century England, how would you find out? Other than hello, these were the first words my first father-in-law ever spoke to me. I fumbled: there must be weather and agricultural records; I wondered if there were harbor records at English ports... It is obvious you have not read your Chaucer; if you had, you would know that in the *Canterbury Tales* a group of friends are walking along a path in the country telling each other tales; between tales they remark upon the flowers growing aside the path. *Those flowers don't grow in Southeast England today; it must have been warmer then!* (Little did I know then that I would read professional papers forty-five years later that contained quantitative proxy evidence for the Medieval Warm Period.)

Bochner, as I came to understand, was incubating thoughts that went into his book, *The Role of Mathematics in the Rise of Science.* History of science was to become the principal topic of our conversations and I learned much that was deep about the scientific enterprise from him. He was imagining himself in dialogue with the great thinkers of the past. I imagined that in the dark of night he fancied himself a student in Aristotle's *Symposium*. I wondered if Bochner was inspired by his best friend Wigner's famous essay on the unreasonable effectiveness of mathematics in science. I wanted desperately to earn Bochner's respect and, up to a point, I did: Bochner told Debby that I was very bright and would do well in life; I was always going to be one or two years ahead of my field, and I would be "important," but, he said, do not expect me to be a seminal thinker who lays the foundation of a whole discipline.

Later, as I settled into the family, I was able to piece together Salomon Bochner's history, which earned my profound respect. Brought up in Kraków, Poland, his academic brilliance earned him a scholarship to an elite *gymnasium* in Berlin, where his stipend helped support his impoverished family. He completed his doctoral-equivalent degree at about age nineteen, but could not take an academic position because he had to help support his family. This he did by working at a relative's haberdashery, but he still went to the library at night to keep up with mathematics. There he found a paper by Harald Bohr, the famous Niels' brother, which he ventured to comment on in a letter to Harald. Bohr, impressed, invited Bochner to Copenhagen to join his research group. After Copenhagen, Bochner went to Munich and embarked on the joyous life of a brilliant young professor in the cultural effervescence and intellectual creativity of Weimar Germany. (Debby also told me he had a serious affair with an opera singer.)

It was not to be. A pall settled over Bochner's spirit that never lifted after Hitler was elected Chancellor. Bochner understood what the election meant and left Munich for Cambridge; there, the great mathematician G. H. Hardy eventually found him an academic position in Princeton. Bochner spent his first summers there travelling between Princeton and Europe, allowing himself no pleasure until he could persuade his family to leave Germany. He induced part of his extended family to settle in Hempstead outside London, and part in New York City, though his mother never did leave. Only then, at age forty (in 1939, the year I was born) did he permit himself to marry—a woman from New York, Naomi Weinberg, whom he met on one of his boat trips.

Manya and Naomi Bochner were living a quiet but busy social life when I first met them in 1963. The Princeton academic community was a world unto itself, and it was not clear to me whether those in it needed any other world. Bochner's closest friend was another émigré, Eugene Wigner, who lived around the corner. Einstein lived the next street over, but did not socialize much with the younger émigré crowd. The Institute for Advanced Study, then directed by the leader of the World War II Manhattan Project, Robert Oppenheimer, was a little further away than the Wigner's, but not much. The renowned art historian, Erwin Panofsky, was at the Institute, along with Bochner's mathematics colleagues. They all were acutely aware of one another; reputation in Princeton was more highly valued than reputation anywhere else. A visit to Princeton by a famous mathematician was more highly anticipated than a visit by the President of the United States. This was the world that Debby Bochner was brought up in.

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Shortly after I returned from visiting my mother in Pittsburgh during the 1963-64 winter break, Ed Frieman informed me that he would be away from campus on sabbatical. I was well enough along on my thesis, he said, that I could work productively with John Greene, who became an insightful assistant thesis advisor and the co-author of my thesis publication. But if Ed were not going be in Princeton, I did not see why I, too, had to stay in Princeton. I gave Harry Petschek a call and said I had been following the space physics literature, and no one had landed upon the idea that the Sagdeev-Shafranov instability was responsible for the anomalous losses of Van Allen belt electrons; we really ought to finish our work before it is too late. If what you say is true, Harry said, you have a job at Avco-Everett. I could finish the drift mode paper with John Greene at night, and work with Harry during the day. Somehow the thesis would get done, and besides, I could see what would develop with Debby Bochner, who was then living near Avco in Cambridge.

Ed Frieman made one attempt to deflect me from this risky course. I was not like his other graduate students, he said; I had broader interests, and I need not aspire only to conventional jobs. I know a place, he said, where they are developing an entire series of spacecraft, and I can get you in on the ground floor. Do you want me to set up an interview for you? Being both polite and curious, I agreed. Soon I found myself on another memorable train ride, this time to Washington DC, and this time not on a snowy evening. I rode with Harry Hess, one of the pioneers of plate tectonics, the most important conceptual development in twentieth-century geophysics. My own destination was CIA headquarters in Langley, Virginia, to see Ed's friend, Bud Wheelon. (I was aware of the historical relationship between Princeton and the CIA; Allen Dulles, the CIA's first Director, was a Princeton alumnus, and I imagined Ed was part of a Princeton-Washington pipeline.)

Once at the CIA front entrance, I needed a human guide to get to Wheelon's office because floors, corridors, and offices were not numbered or color-coded. What Wheelon told me when I finally found his office was astounding; they could trace the movement of every freight car in the Soviet Union from satellite observations. They were developing what became the famous *Vela* spacecraft series to detect X-rays from clandestine nuclear explosions in the atmosphere, and were looking for someone to diagnose the natural variability of the X-ray environment in the upper atmosphere. I would lead that part of the effort.

I had much to think about on the train ride back to Princeton. I would get in on the ground floor of what came to be called X-ray astronomy: neutron stars, stellar black holes, and active galactic nuclei were variable X-ray sources that could be mistaken for a nuclear explosion.

The CIA’s technology was impressive, but if I did something scientifically interesting, *would anybody ever hear of it?*

I decided to stay at Avco and settled into a bachelor apartment in Kenmore Square in Boston. Soon after, I got a letter from my draft board. I was ordered to report for a physical examination before being inducted into the armed forces. I freaked out, but of course, I showed up for the examination. I was stripped, measured, poked, prodded, and injected with things by medical orderlies for the better part of two hours, and at the end I was interviewed by a real medical doctor. You are older than the others, this doctor said*, Why are you here?* I explained about leaving Princeton and going to Avco; Princeton had evidently notified my draft board that I was no longer in attendance. Avco is an important defense contractor, this doctor said; I guess your defense deferment has not yet come through. (*By the way, the doctor added, I also went to Princeton.*) My, my, your feet are flat, aren’t they? I’m going to save the government a lot of confusion by giving you a 1-Y classification; they will not come for you unless there is a declared emergency. I put my clothes back on and drove back to Kenmore Square relieved that I had dodged a bullet—in fact, multiple bullets. The Vietnam war was in its early phases.

It is easy to say that all’s well that ends well, but I was pretty shaken by the whole thing. The next time I saw Debby Bochner I told her the whole story, poured out my anxiety, and speculated that maybe we should actually think of marriage; in those days married men were deferred from the draft. We had not talked about the important things like children, religion, money, where to live, how to finish our educations, how to balance our professional and family obligations, and the yet-to-be consummated intimate part of our relationship seemed strangely unnatural. Though I did not hide my mourning for my father, I probably had not yet revealed that I had an alcoholic mother, and Debby and I certainly had not discussed how any family of ours would cope with my mother's almost life-long alcoholism. But maybe it was time to start talking.

A day later when I next saw Debby, she announced it was all settled. She had called her mother and told her we talked of marriage. This was the mother who had hounded Debby into leaving Yale for Harvard because Debby would meet more men in Boston than in New Haven, and I was living proof. The news triggered a maternal frenzy, and before Debby and I even had a chance to talk further Naomi had reserved the Princeton Inn for the ceremony and reception and set the date. *Wait a minute! an interior voice screamed,* but the exterior voice said nothing; I might hurt Debby's feelings. That is not to say I did not have moments of resistance; I even went on two dates with other girls while having thus been engaged. Nonetheless, I watched myself slowly being dragged along the path to marriage by an ineluctable force. I got miserably drunk at the bachelor party the night before the wedding and turned up hungover and shaky at the ceremony.

The thing was quickly done. The Princeton Inn had been beautifully turned out by Debby's mother. There were only a few people from my side of the family. My mother was there, of course, and Aunt Mary and Uncle Stanley and their friends, the Lehmans, came from Pittsburgh; their teenage daughter, my friend Ellen, went to a movie with my roommates afterwards. Debby and I drove off in our wedding present to my childhood friend's, Tommy Gillespie's, wedding in Rhinelander, Wisconsin, where I was to be the best man. Had I been given a chance I would have chosen a more desirable honeymoon, but I was already committed to be the best man and Naomi had not consulted with either of us about the date of *our* wedding.

About three months later, Debby and I sought marital counseling. The intimate part of marriage was proving to be an unpleasant chore. She would go with me to counseling only once beyond this one time, though she must have known I got counseling on other occasions. She and I were to structure our separate responses to the familial and social challenges of adult life without ever working together on the shameful disjunction paralyzing the animating core of human life.

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The urgency to finish the thesis increased the moment Naomi chose the wedding date. It was crazy, but I managed to do it, working on drift waves with John Greene in the evenings and weekends, and the Van Allen belts with Harry Petschek during the day. To my chagrin the thesis was not finished by the wedding, but I did complete it soon thereafter and traveled back to Princeton in October to pass my final examination. By then I had learned enough plasma physics that I could carry out detailed calculations based on the sketches developed in conversations with Harry.

Arthur Kantrowitz, who depended on Harry's unusual ability to frame scientific issues, needed Harry full time during the day. To get Harry's real attention, I had to drive to Lexington from Cambridge (where my new wife and I were living) in the evenings, typically leaving Lexington around 1 a.m. That way Harry and I could have four hours of uninterrupted discussion, which, *mirabile dictu,* both young wives seemed to tolerate. I would spend the next few days at the lab carrying out the tasks we had set for ourselves in our evening marathons. One by one our calculations brought several streams of observational data into consistent focus, each adding to the case that the Sagdeev-Shafranov whistler mode instability was responsible for the unexplained loss of Van Allen's electrons to the atmosphere.

Harry Petschek invited me to accompany him to what was my second professional meeting at the NASA Jet Propulsion Laboratory in Pasadena. I was there to listen and Harry did the talking, but towards the end I gathered enough courage to ask a question of the session moderator, Tommy Gold. Gold was, like Freeman Dyson, a Cambridge-educated polymath. Gold asked if I would repeat my name for the stenographer taking minutes, and I replied, K-E-N-N-E-L, *as in dog...or...as a dog goes in.* The audience laughed, and Gold, gratified, exclaimed young man, with a name like that, you will go far. (Equally memorable was the lunch with the great biologist Max Delbruck at the Athenaeum on the Caltech campus during that trip. The only other time I saw Harry with that look of hero-worship in his eyes was when Roald Sagdeev visited Avco from the USSR.)

Gold must have been pleased with my remark, for he later invited me to give my first academic seminar to his research group at Cornell, where Harry had been a graduate student. My talk on the Van Allen belts that afternoon was unusually sparsely attended, though Gold graciously sat through it and provided the only commentary afterwards. My presentation had been interrupted several times by commotion down the hall. The reason, Gold explained, was that Dick Feynman was giving a seminar and I was hearing the laughter and applause. Gold, sheepish about the sparse turnout at my seminar, invited me to a public lecture Feynman was to give that evening—and much more meaningful, to join him, Feynman, and others in the pub in the basement of Willard Straight Hall after the lecture.

Tommy Gold had invited me to witness the making of physics history: this was Richard Feynman's first Messenger Lecture. Feynman wrote up the seven lectures in a series, *The Character of*

*Physical Law,* which became the most sophisticated introductory text in physics ever published, suitable only for the extraordinarily gifted. Even five minutes of listening to Feynman were enough to convince me that I was in the presence of an unusual mind. Complex physical processes that would take my Princeton physics professors weeks to work out were compressed in Feynman's mind into visual images that he could call up at will. He tried to capture these images in words and diagrams in his great but unusable book. I have never encountered anyone with a mind like his since; I understood everything he said but was unable to reconstruct any of it five minutes after leaving the lecture hall.

After Feynman's lecture an anointed few, herded by Gold, walked to Willard Straight Hall and down the long set of stairs to the pub for beer with Feynman. The picture in my mind is like Leonardo Da Vinci's *Last Supper,* with Feynman brightly lit in the center, attended by Gold, with sub-groupings of physicists engaged in desultory gossip or passionate technical argument arrayed to the left and right at a long table. The beer having arrived, the side conversations stopped and the physics disciples strained to hear what was going on at the center.

Gold had challenged Feynman to a contest. Each would pose a logical puzzle and the other would have to figure out the answer. It didn't work out so well for Gold, because as soon as he began to pose a puzzle, Feynman would see what the solution must be and gave the answer before Gold had even finished outlining the puzzle. This went on for a while with no material change in the result, so Gold changed the ground rules to languages. How many languages could each say something in? This too went on for quite a while, but it finally ended when Feynman challenged Gold with Mongolian. But Gold just wouldn't give up; finally, he thought of something he could win. It was time to call it a night, and Gold challenged Feynman to a stair-jumping contest on the way out, the disciples watching fearfully from the bottom. Gold, a diminutive man, won this one by at least two steps, and no geniuses died that night.

My path did not cross Feynman's much until 1988, when he was being treated in the UCLA hospital for his final illness. He took conspicuous leaves from the hospital to attend the meetings of NASA's failure review board for the *Challenger* accident, where he made the decisive intervention (about the O-rings), perhaps his last public act in science. We had arranged for a special office in the physics department for Feynman in case he wanted to wander up from the hospital for physicists' company; this office ended up being used by much of the world of international physics that came to visit Feynman in his final days. For a few brief moments, UCLA was the center of the physics universe.

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Roald Sagdeev made another visit to Avco in early 1965. Sagdeev's next stop on this American tour was to see my thesis advisor, Ed Frieman, in Princeton. A Soviet scientist couldn't walk around in the US unescorted, and Petschek arranged for me to travel with Sagdeev from Boston to Princeton. The ride was on the old Lockheed Electra, which at a certain threshold air speed became unstable due to a wing oscillation that could tear the wings off. For this reason, the Electra had been banished to the Boston-New York route and was required to fly at low speeds. I could hear the wings creaking, and I was imagining the lurches would send our plane into a dive where the air speed would exceed the instability threshold. Sagdeev was remarkably comfortable during the turbulence. I do not know whether he knew about the instability; they might be used to such things in Soviet Russia, but I was not.

Harry told me I would learn a lot on that airplane ride, and what I learned changed my life. On an alarmingly bumpy flight south, Sagdeev informed me between lurches that his young acolyte, Alec Galeev, would accompany him to a six-month workshop that he and the great Marshall Rosenbluth were organizing at the new UN International Centre for Theoretical Physics in Trieste, Italy. Roald wanted Alec to work with Rosenbluth, and so he, Roald, would need another young person to work with; would I be that young person? Sagdeev's scientific reputation was monumental and his personal magnetism overwhelming. Two years later, he would become among the youngest ever elected to the Soviet Academy of Sciences at age thirty-five. I had never thought about Trieste or the International Centre for Theoretical Physics, but it took me no more than two minutes to agree. I did not know then how serious a jolt it would be for my young wife, Debby, but I hoped the attractions of a few months abroad would compensate for the disruption.

The summer after the fearful plane ride, Harry dangled consultantships to lure a few of the younger space scientists whose papers I had been reading to Avco. If you really want to know what is going on in a research group, talk to the graduate students. Harry said that was my job; I will learn more than he would by talking to their professors. The Petschek “summer seminars" included Ian Axford, Neil Brice, Don Gurnett, George Siscoe, and Richard Thorne, then obscure figures who later became prominent in space plasma research. Axford became Vice Chancellor at the University of Wellington in New Zealand, and later the Director of what is now the Max-Planck-Institut für Sonnensystemforschung in Germany; Brice died young in an airplane crash, a promising professor at Cornell; Thorne and Siscoe became tenured faculty members at UCLA; Siscoe went on to Boston University, where Petschek worked at the end of his career; and Gurnett remained at Iowa with Van Allen, eventually to be elected to the US National Academy of Sciences.

I listened in on nearly every discussion, so I was up-to-date in space physics when I arrived in Trieste in September 1965 to work with Sagdeev. As I was preparing to leave for Italy, I gave a series of seminars to publicize Harry and my Van Allen belt work, including a memorable visit to Van Allen and his group at the University of Iowa. Harry and I submitted a comprehensive paper on the Van Allen belts to the *Journal of Geophysical Research* just before my young wife and I sailed to Italy.

Debby Bochner did what loyal academic wives do when their husband comes home with an important professional opportunity that requires a move: she carefully thought through our domestic needs while abroad for six months and carefully packed our bags. We drove to Princeton to stay with her parents the day before we were to embark from New York for Genoa aboard the *SS United States*. When everyone else was asleep, Naomi came downstairs and ransacked Debby’s suitcases, strewing the contents about on the floor. When we came downstairs the next morning for breakfast, we found Naomi ranting obsessively, attacking her daughter for her ignorant choices of things to take on our trip. Faced with imminent departure, Debby and I had to repack everything in a rush. Naomi had a hostile remark about each item as it went back into the suitcases. This manic critique, with its unusually elaborate particularity, continued nonstop until we were safely aboard ship.

I began to understand what Debby had to deal with as a child. I saw she would do just about anything to avoid one of Naomi’s obsessional tirades, maybe even marry me.

# 9

# Physics at the Crossroads of East and West

History made a hostage of the once prosperous port of Trieste after the end of the First World War. Trieste had been Vienna's outlet to the Adriatic Sea, but when the Austro-Hungarian empire was parceled out at the Versailles Conference in 1919, Trieste found itself exposed on the ethnic, cultural, and religious fault lines that have divided Central Europe for centuries. It was not clear to whom Trieste should belong: Slav or Italian, Catholic or Orthodox, Muslim or Jew, fascist or communist; all lived in uneasy proximity in Trieste. Vienna's once busy port city could no longer smooth over the old divisions with prosperity's emollient. So great was the Cold War tension between East and West that Trieste had to be administered as a United Nations protectorate from 1947 to 1954, and was declared part of Italy only in 1975. Trieste was only ten miles from Italy's border with communist Yugoslavia; in 1965, crossing into Yugoslavia to buy cheap groceries with Western currencies was a daring adventure. The red stars on the Yugoslav border guards' caps warned you that you were entering a forbidden world.

The conflicts of the past were never far away in Trieste. The first building of the International Centre for Theoretical Physics (ICTP) on Piazza Oberdan had been an SS headquarters during the Nazi occupation of Trieste. A Triestino medical doctor who treated me for a mild case of hepatitis told the following story about that SS Headquarters: all Triestini knew an Allied invasion was imminent and were especially jittery when a small American boat came to reconnoiter Trieste's huge harbor. That was when the SS decided to abandon Trieste, but first they rounded up a few more enemies and hung their bodies from the windows of the Piazza Oberdan building as they left. The next day, Allied ships filled the harbor to the horizon. The Triestini were terrified; after what the Nazis had done, what would the vastly more powerful Americans do? Soon a seemingly endless column of tanks, trucks, and Jeeps drove noisily from the docks past the hanging bodies and up Viale XX Settembre (where I lived) to the top of the hill overlooking the harbor. Only the children ventured out of hiding when the American column arrived at the top. The American soldiers gave the children the first chocolate bars anyone had seen in a long time.

**Figure 9-1: View of the port and Gulf of Trieste from the Castello San Giusto. Communist Yugoslavia was about ten miles to the east. Trieste, which was much in the news when I was a teenager in the 1950s, was on the line that separated the communist East and the capitalist West during the Cold War. The UN's International Centre for Theoretical Physics (ICTP) expressed a nuclear-age hope that that physics could build a new bridge over at least the newest of the divides. Prior to the Great War (WW I), when Trieste was the Austro-Hungarian Empire's outlet to the sea, it had been on the ethnic and religious line that divided Europe for centuries. Ellen Lehman took this photograph on July 23, 2013, while she and I were on side trip from Venice.**

Two scientific events of the Cold War, the American test of the first thermonuclear bomb at Eniwetok Atoll in 1952, and the Soviet launch of *Sputnik* in 1957, drove the international politics of the 1950s and 1960s that led to the creation of the ICTP. In his Atoms for Peace speech to the UN on December 8, 1953, President Dwight D. Eisenhower proposed that the Americans, Soviets, and British back away from the nuclear arms race, but in 1955 the Soviets detonated their first megaton-class thermonuclear bomb, which only increased Cold War tensions. The UN created the International Atomic Energy Agency (IAEA) in 1957 as a way of easing them. One of the first things the IAEA did was to organize the 1958 Atoms for Peace conference in Geneva, the largest international gathering until that time to focus on the use of nuclear energy for peaceful purposes. Among its attractions were presentations by Hannes Alfvén, Lev Artsimovich, Ludwig Biermann, Peter Thonemann, and Edward Teller on the physics of controlled thermonuclear fusion.

Many American and Soviet physicists had their first chance to meet one another at the Atoms for Peace conference. Marshall Rosenbluth met Roald Sagdeev at Geneva, and Harry Petschek met Sagdeev there too. Years later, my plasma physics elders in America, Britain, and the Soviet Union were to recall their astonishment at finding that classified plasma physics research had followed the virtually the same path in all three countries. Making a fusion bomb was relatively easy, but after trying it for a few years each country had learned that controlling thermonuclear fusion reactions was hard (and it still is). They could no longer see any reason to keep peaceful fusion research secret; since it was unlikely to be successful soon, it presented no imminent security threat.

All sides announced that they would declassify fusion energy research at Geneva, and plasma physics went public after the Geneva conference. In 1961 my PhD *alma mater*, Princeton, converted its classified Project Matterhorn into the Princeton Plasma Physics Laboratory (PPPL), the name it still has today. In 1964, the IAEA went on to create the International Centre for Theoretical Physics (ICTP). The IAEA had paved the way for the 1965-66 plasma workshop by organizing the first of a continuing series of international conferences devoted to peaceful fusion research beginning in Salzburg in 1961. My Princeton plasma physics professors—Ed Frieman, Ira Bernstein, Martin Kruskal, Carl Oberman, Harold Furth, John Dawson, John Greene, Tom Stix—considered the proceedings of the IAEA conferences to be the Bible of fusion plasma research. The

Director of the PPPL, Lyman Spitzer, didn't read the IAEA proceedings as assiduously as his junior colleagues did because he already knew it all, or so it appeared to me.

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Trieste was an iconic venue for a scientific institution whose political purpose was to bridge the communist, capitalist, and developing worlds. The ICTP, an offspring of the Cold War, has lived on to help incubate the global approach to science required by a world threatened by climate change and sustainability challenges. How fortunate I was—how lucky we all were—to be part of one of the first two ICTP plasma physics research seminars in 1964 and 1965! The scientific talks could be heard nowhere else, and our daily lives gave tangible witness to the way the Cold War divided international society.

The ICTP was the inspiration of the Pakistani Nobel laureate, Abdus Salam. Salam, a profound elementary particle physicist and a great humanitarian, had been alarmed that underdeveloped countries were sending their young physicists to the developed world to be educated, after which the young physicists stayed, enriching countries that were already rich rather than going home. Salam thought that developing world physicists could be kept at the top of their fields yet remain engaged with their home countries if they were provided postdoctoral appointments to work with senior physicists in residence at ICTP. The postdocs would return home to teach and would be guaranteed periodic returns to ICTP to keep up with the leading edge of research. Salam's vision was already special, but ICTP had a unique attraction for senior physicists in the developed world. ICTP, a UN IAEA organization, was the one place in the physics world of the Cold War where European, American, and Soviet physicists could collaborate for long enough to get serious research done.

Salam was at the peak of his powers: he was simultaneously a professor of physics at Imperial College, London, the Director of ICTP, and a science advisor to the government of Pakistan. He shuttled tirelessly between London, Trieste, and Lahore. You knew when he was about to arrive at ICTP because the lights would be on at 1 a.m. in the offices of the high energy physics group and in the administrative wing. ICTP's Deputy Director, Peter Rendi, and Salam's Personal Assistant, Moira Blessington, would be waiting in their offices for Salam's arrival. (Rendi, a UN employee, astonished me when he said he spoke seven languages; seeing a loaf of bread brought seven words to mind. I saw him switch easily from Russian and English in casual conversations.) The particle physicists would only have a day or two with Salam before he was off to London or Lahore but knew he would be back within a week or two on the return trip. People did not worry about the carbon cost of airplane travel in those days.

**Figure 9-2: First site of Abdus Salam's International Centre for Theoretical Physics, Trieste, Italy, in 2013. Number 6 Piazza Oberdan, SS headquarters during World War II, was pressed into service in 1965-66 as ICTP's temporary home until a new purpose-built building could be completed in the oceanside suburb of Miramare. The plasma physicists had offices on the third floor (American style) and the high energy physicists on the fifth. Photo taken by Ellen Lehman on July 23, 2013.**

Marshall Rosenbluth was America's leading theoretical plasma physicist from the mid-1950's to the time of his death in 2003. His unearthly talent in physics inspired in his colleagues the informal title "Pope of Plasma Physics." He co-invented the Monte Carlo technique of quantitative computation that underlies many of today's "big data" analyses used in science and commerce. He also made an important contribution to elementary particle physics (the Rosenbluth electron), and while working for the Los Alamos National Laboratory in his early twenties he had carried out the numerical computations that enabled the world's first test of a hydrogen bomb, the thermonuclear test explosion (MIKE) at Eniwetok Atoll in 1952. Its explosive power exceeded expectations.

Afterwards, Rosenbluth turned his back on all the other physics problems he could have solved to focus exclusively on peaceful fusion research. I have long speculated that his reluctance to discuss other topics in physics came from remorse over what he and his colleagues had done when they created the H-bomb. If endless prosperity powered by fusion energy was Lyman Spitzer's inspiration, a working fusion power plant would be Marshall Rosenbluth's redemption.

In October 1964, Marshall, then at General Atomics in San Diego, chaired the conference that opened the ICTP.[6] Roald Sagdeev, then from the Soviet Academy's Institute of Nuclear Physics in Novosibirsk, bonded with Rosenbluth again at that ICTP opening event, and the two apparently agreed to find a way to collaborate for longer than was possible at occasional international meetings. They hatched a plan for an extended fusion plasma physics seminar in Trieste for 1965-66. The plan was quickly approved by the IAEA, and soon after Marshall and Roald went scouting for seminar participants.

Sometime early in 1965, Sagdeev came to Avco-Everett to visit Harry Petschek, who had also been at the IAEA conference. As it turned out, while at Avco Roald recruited a younger collaborator—me—to work with in Trieste. A postdoctoral fellowship from the National Science Foundation covered my living expenses, and Senator Ted Kennedy added a personal note to his *pro forma* letter of congratulations (he remembered me from my days as a freshman manager of the Harvard football team). My young wife, Debby Bochner, and I, were among the first of the seminar group to arrive in Trieste. Marshall was already there and Roald came a few weeks later.

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The chance to hear Rosenbluth and Sagdeev lead a debate on the state of plasma physics attracted an unusually talented group of researchers to Trieste, who were or were about to become eminent figures in the field. Dave Book (who later ended up at NRL), Bruno Coppi (MIT), Wendell Horton and Herb Berk (UT Austin), Ravi Sudan (Cornell), Roscoe White (Princeton), and Tom O'Neil (UCSD) were part of the plasma group in residence. René Pellat and Guy Laval contrived to make so many visits from France that they were considered regular members. Ed Frieman visited from America, as

[6] *Lectures Presented at the Seminar on Plasma Physics Organized by and Held at the International Center for Theoretical Physics, Trieste, from 5-13 October 1964*, (Vienna: International Atomic Energy Agency, 1965).

did Burt Fried, the leader of the UCLA plasma physics group. (Burt broached the idea of my going to UCLA after one of the Trieste lectures.) Senior figures in the European plasma and space science communities also came on shorter, repeated visits. I first met three leaders of European space physics, Hannes Alfvén, Gerhard Haerendel, and Reimar Lüst, at Trieste. My collaboration with Folker Engelmann was carried out over several of his visits to Trieste.[7]

The seminar's attraction was not its political uniqueness, though that was undeniable, but rather that its leaders personified different intellectual traditions within physics. Marshall's approach was rooted in statistical physics, whereas Roald's was inspired by the great Russian tradition of nonlinear continuum mechanics. The two traditions confronted one another in Trieste, and plasma physics was the winner. When astrophysics and the recent observations from space were introduced into the mix at Trieste, one could begin to see the outline of the discipline of plasma physics as it is today. (Only the low temperature plasma physics used in the production of computer microchips was not represented at Trieste, and for a good reason—this was before the revolution in information technology, and nobody was making microchips.)

Ideas were in the air, and all a young person had to do was write them down.

The two babies of the Trieste group, Sagdeev's young proteges, Alec Galeev and I, shared an office. There I got to see how Alec worked, and I now think of Alec as the Mozart of plasma physics. Mozart was known to write out musical compositions without error, and Alec would write out mathematical calculations without pause, sometimes while conversing in English with me. Where did Alec's talent come from? It was daunting to know that such technical skill could exist. He was even more impressive than my Princeton Graduate College roommate, Morty Rubin, or my thesis advisor, Ed Frieman. Someone told me once that the small group of plasma physicists in Trieste started 110 papers in those few months that were eventually published. It is conceivable. Some of the Trieste publications had lasting impact, and Sagdeev and Galeev's Trieste lectures on nonlinear plasma theory guided the development of the

[7] C. F. Kennel and F. Engelmann, "Velocity Space Diffusion from Weak Plasma Turbulence in a Magnetic Field," *The Physics of Fluids* 9, no. 12 (1966): 2377-2388.

field for the next twenty years until numerical computations took over.[8]

Though I listened carefully to Marshall Rosenbluth's every word, a part of me did not want to be overwhelmed by his talent like the others in his circle of admirers. I did not participate in many of the intimate discussions on fusion research but concentrated instead on space physics. I sensed that Marshall understood my standoffishness.

How was space physics slipped into a UN seminar on fusion? It turns out that the fusion luminaries in Trieste were not far behind Harry Petschek in wanting to learn about space observations, and I became their guide to the literature. Space observations were brought up frequently during the twice-weekly two-hour Trieste seminars. I gave a talk on the pitch angle scattering by whistler mode waves in the Van Allen belts I had been working on with Harry.[9] This paper appeared in print on January 1, 1966, when I was in Trieste. I recall walking home for work on the day I first saw it in the ICTP library, thinking now that I have published, I will not perish. Later, Bruno Coppi, Guy Laval, and René Pellat presented their new theory on collisionless tearing mode magnetic field reconnection in space plasmas,[10] and Sagdeev and I developed a theory of collisionless shocks in so-called high-beta plasmas.[11]

Over the few months of the seminar, the group formulated problems that the space plasma physics community has worked on since: electrostatic and electromagnetic loss cone instabilities, velocity space diffusion in a magnetic field, radiation belt loss, solitons, quasi-parallel and quasi-perpendicular collisionless shocks, collisionless tearing mode, and magnetic field reconnection. The field has gone way beyond what we did, but at least we gave the problems names.

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[8] R. Z. Sagdeev and A. A. Galeev, *Nonlinear Plasma Theory,* (W. A. Benjamin, 1969).

[9] C. F. Kennel and H. E. Petschek, "Limit on Stably Trapped Particle Fluxes," *Journal of Geophysical Research* 71, no. 1 (1966): 1-28.

[10] B. Coppi, G. Laval, and R. Pellat, "Dynamics of the Geomagnetic Tail," *Physical Review Letters* 16, no. 26 (1966): 1207.

[11] C. F. Kennel and R. Z. Sagdeev, "Collisionless Shock Waves in High β Plasmas," *Journal of Geophysical Research* 72, no. 13 (1967):3303-26

One Saturday morning, I broke off doing library work at ICTP to go to lunch across Piazza Oberdan to the Birreria Forst. I took a copy of *Time* magazine along to read while eating. At some point, a youngish woman with an undefinable accent, seeing I was reading English, inquired whether she might practice her English with me; she sat down before I could answer her. *What are they doing in that building across the street,* she asked. When I replied that physicists from both sides of the Cold War were working together, she came to attention; when she learned I graduated from Princeton, her questioning manifested altogether too much familiarity with the town and university. An alarm bell went off and I got up to leave, taking *Time* magazine with me. As I crossed the street, I wondered whose side of the Cold War she was on. I didn't know whom to ask.

A few years later Sagdeev and Galeev were working full time on space physics, eventually becoming directors in succession of the Soviet Union's best space research laboratory, the Space Research Institute in Moscow. I was to visit Moscow frequently. René Pellat went on to head the French National Space Agency, among other French science agencies; he kept up the friendship started in Trieste by visiting UCLA frequently, and in 1975 I spent a sabbatical year with him at the École Polytechnique in Paris. Rosenbluth and Sagdeev saw one another frequently and used their friendship to advance international arms control, and Sagdeev became one of Soviet Premier Gorbachev's principal arms control advisors. The Berlin Wall came down in 1989 and the Cold War, which created ICTP, came to an end, but ICTP continues.

Do not get the idea that Trieste was like those august events memorialized in Victorian paintings on the walls of Oxford and Cambridge, or in Edwardian photographs of the attendees at the great Solvay Conferences on Physics. Marshall and Roald were young, thirty-eight and thirty-three respectively, as were the rest of us, and it was hard to distinguish work from play. We bonded as only young scientists could. Once, on a Friday, Sagdeev and I struggled fruitlessly to solve an integral equation. Oh, well, we said, let's go to the beach tomorrow and maybe relaxation will settle our thoughts. The next day at Miramare Beach we swam together to a raft offshore where, after a short conversation about our dilemma, Roald dove underwater. He stayed down for a longish time and then exploded to the surface: Charlie, I solved integral equation! Underwater! The discussion on Monday would prove him right. Most of our scientific discussions did not require a deep dive underwater, but this one did.

**Figure 9-3: The first of many trips to Venice. Here I am, twenty-six years old, on my first visit to Venice in 1965, on a weekend excursion from Trieste. How could I know that forty years later I would visit Venice frequently, not for plasma physics but for a subject that did not exist at the time, sustainability science?**

From time to time, Trieste alumni find themselves together and we reminisce about the deep dive into physics and life that was our few months together. We talk about what has happened to the work we did, and about Abdus Salam's vision. We talk about the parties at the Castello San Giusto and swimming at the beach in Miramare, about excursions in Yugoslavia to the Dalmatian coast, *datteri* at the Ristorante Bagutta and *salsicce* and *birra* at the Birreria Forst. Trieste enriched our lives, and it changed plasma physics. I have been back for other visits; the new ICTP building at Miramare is much nicer than the one at Piazza Oberdan, but without the same people ICTP never was the same for me—though the Miramare site does not have to live down the association with the SS that cursed ICTP's first building on Piazza Oberdan.

I returned to Avco from Trieste a different person, and a different Avco welcomed me back. The missile re-entry problem in hand, Arthur Kantrowitz conceived the ambition to build an artificial heart pump for use during cardiac operations (Arthur's brother was a cardiac surgeon). Arthur enlisted Harry Petschek's remarkable talent to frame scientific problems in the quest to bring advanced physics to medicine. Harry was immersed in the theory of complex flows in elastic pipes of fluids containing sticky, flexible, orientable particles, i.e., blood flow. Trieste having fledged me, as Harry realized it would, he had moved on.

I realize now that it must have been hard for Harry to separate from our collaboration since, towards the end of his career, Harry worked at Boston University full time on space physics. We would serve on an advisory board to together at the Geophysical Institute of the University of Alaska in Fairbanks shortly before he died.

# 10

# Life in the Los Angeles Aerospace Empire

My arrival at UCLA lives in memory as vividly as Neil Armstrong's landing on the moon. It is August 21, 1967, my third wedding anniversary, and my first day on campus as a just-turned twenty-eight-year-old tenured associate professor of physics at a research university. I am gazing westward towards the vast blue Pacific Ocean from the top of UCLA's Janss steps. I am in Los Angeles, where my father yearned to be, *and* I was going to become a father myself. On that first day on the UCLA campus, adult life opened the door and invited me in.

California was somewhere over the rainbow. California oranges were colorful messengers from a sunny world to winter gloom. California was where my father and his sister, Aunt Gladys, found it when they drove from St. Louis to California in the summer of 1928, after he graduated from Westminster College. California was where Lockheed made the P-38, the P-80, and the Constellation. California was where my father had flown on the second-ever Lockheed Constellation during the war; his dangerous test flight was legendary, a family myth. A mythical California had also been transmitted to my home in New England over the air; Jack Benny's radio program was recorded in California and flown to the East to be broadcast there. You could see strange pictures of freeway webs in *Life* magazine—definitely not descendants of the cow paths in our old New England towns.

All of this is not to say that New Englanders ever thought California normal; we could see for ourselves that it was not. Aunt Gladys, Uncle Stewart, and my cousin Skipper visited once from Monrovia, California, carrying fantastic tales of endless sunshine, of weeks and months without rain. That was hardly normal. Skipper told me you could go to the beach, mountains, and desert on the same day. *Who had deserts?* These California relatives were from an odd world; they needed help cracking lobster claws during the New England supper we put on for them.

California seemed pleasant enough in a pedestrian sort of way, but it was the home of kooky airheads and crazy cars. Mother's view was that other people might want to live in California, but no one *we* knew lives west of Dedham. She had a funny view at that time about places other than Boston. Mother's views would become more tolerant after she moved to Pittsburgh. Pittsburgh was the furthest west you could and still be in the East, the jumping off point for the earliest western migrations. She later had to deal with the fact that her grandchildren lived in California.

Several years before Aunt Gladys' visit, Mother, Father, my brother John, and I drove to New York City on a rare family excursion. Once beyond the Massachusetts border on the Merritt Turnpike, our car fell so quiet all you could hear was road noise. We were in alien territory. I looked out the window at the restaurants and motels passing by; I saw a McDonald's for the first time, a modern intruder standing out among the roadside "greasy spoons." Somewhere around Hartford, Mother shattered the spell: we had to go to New York because my father had a business appointment, she said. My brother John and I would like New York; we would enjoy the Rockettes at Radio City Music Hall and lunch at the Automat, but just remember: no matter how much fun we have, New Yorkers are different.

My father never went back to California after the war and I never went there until I was out of Princeton and working with Harry Petschek. Harry and I were invited to a workshop at the Jet Propulsion Laboratory in Pasadena in 1964. It was to be my first cross-country flight. I had flown to Europe several years before, but a flight to California was different. Mindful of my father's forced landing on that test flight of the Constellation, I asked Harry what our flight would be like. It would be bumpy over the Rocky Mountains, Harry said, but not to worry. (Over the Rockies was where my father's test flight ran onto trouble…) Harry went on:

flying was far safer than driving; if plane crashes were the sole cause of death, and I flew every minute of every day, I would live eight thousand years. Harry could always persuade me with quantitative reasoning. We ordered the airplane tickets and went to the meeting in far off Pasadena. California turned out to be surprisingly brown: no green trees, just runty little chaparral things, low bushes, and succulents. There were attempts to punctuate the brown drear by adorning the entryways to the Spanish-style apartment houses with showy birds of paradise.

Harry made two memorable introductions when we were in Pasadena. I joined him for lunch at the Caltech Athenaeum with Max Delbruck, a Nobel laureate molecular biologist. They had an intense conversation about a branch of science that I was unaware even existed. Harry also introduced me to Thomas Gold, the famous Austrian-British astrophysicist, then at Harry's PhD *alma mater,* Cornell, who chaired the JPL symposium.[12]

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LA's palm trees called up images of the South Seas and whispered "sin" in the soft breezes on that first visit, but different things became important when I knew I was going to live there. I had never encountered anything like Wilshire Boulevard, which had—get this—eight lanes running from downtown to the Pacific Ocean. One street, eight lanes, sixteen miles. They thought big when they designed Wilshire Boulevard. Speaking of thinking big, the new San Diego Freeway, built in 1957, went north from Orange County, past the LA airport, over the Santa Monica Mountains, and across the San Fernando Valley. The San Diego Freeway barreled over the mountains in a straight line; mountain topography had to give way. The smog in the San Fernando Valley looked like a cloud of bourbon. I saw my first Rolls-Royce in Beverly Hills, a car unimaginably far above Cadillac in the prestige rankings. They did not have cars like that in New England, but my Uncle Jim, who was a car buff, had told me about them. I saw lots of Rolls-Royces in Beverly Hills.

Los Angelenos really were different from New Englanders. They listened to the car radio all the time. I heard women radio

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[12] That symposium was the occasion of the anecdote related in chapter 8, when I was able to make an impression on Tommy and elicit a chuckle from the audience by making a pun on my surname.

announcers for the first time in Los Angeles, and something even weirder: the Forest Lawn Cemetery advertised for prospective tenants. Lawyers touted themselves on the radio. Stores didn't close at 5 p.m. on Sunday. I went to a food market at midnight just to see what it was like, and I found young people flirting with one another; even shopping was a social occasion. You could buy wine in supermarkets, and restaurants served the salad before the main course. Lots of houses had pools; only the biggest had basements or second floors, but most came with garage door openers and lawn sprinklers. You could press a button and the garage door would open, and you did not have to stand around watering the lawn with a hose. Where I came from, if you wanted something you should put in the effort to get it; garage door openers and lawn sprinklers were telltale indicators of dubious moral character.

People talked about house prices in California, a forbidden topic in my family, and Californians even discussed their salaries. If Burt Fried had not brought up salary while recruiting me, I would not have had the courage to ask what mine was to be. Even though I had health insurance through UCLA, a New England voice told me it showed moral weakness to use it—and, indeed, I did not use it when we had medical expenses, even though a deduction for insurance was taken out of my salary. This aberration lasted only a couple of years before I, too, was corrupted by the ease of life in Southern California.

Debby and I took a two-year lease on an apartment at 105 South Barrington Drive in West Los Angeles; it featured unusually low popcorn ceilings, exterior corridors exposed to the elements (such as they were), and inevitably, officious birds of paradise guarding the main entrance. After Debby learned she was pregnant she agitated for a real house, and so we sublet the apartment and moved to one of the more modest little streets, to 346 North Bowling Green Way in Brentwood, a fifteen-minute commute to UCLA in light traffic. For the first eighteen months of Matthew Bochner Kennel's life, Debby was absorbed in her new motherhood, and I in learning how to be a young associate professor at UCLA. When Sarah was born in 1972, we moved to a larger house at 1250 Norman Place in the same neighborhood. Debby would continue living there for the next fifty years.

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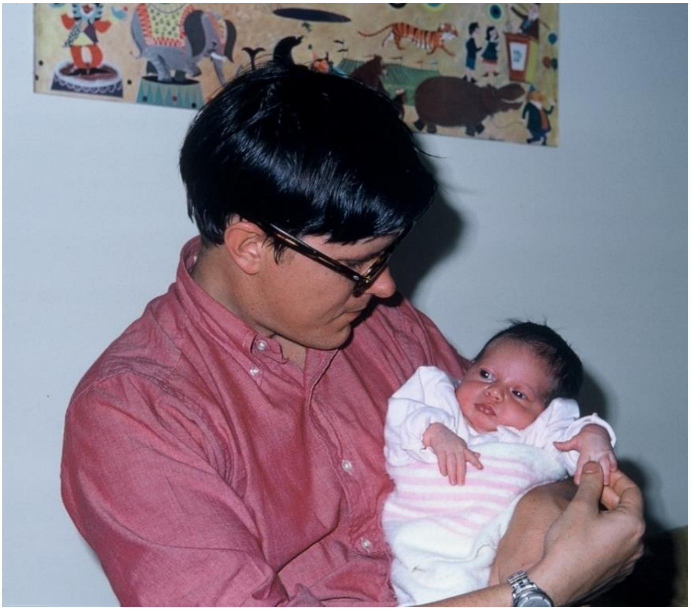

**Figure 10-1: Charles Frederick Kennel and daughter, Sarah Alexandra Kennel.**

**Figure 10-2: Deborah Bochner Kennel with her son, Matthew Bochner Kennel (left), and daughter, Sarah Alexandra Kennel (right), in Brentwood, about 1974.**

**Figure 10-3: Salomon Bochner, about 75 years old, and his grandson, Matthew Bochner Kennel.**

**Figure 10-4: Elizabeth Ann Fitzpatrick Kennel and her grandson, Matthew Bochner Kennel, about a year before my mother died at age 60.**

UCLA is the smallest of the University of California campuses, but it is the most beautiful. Given the image of Los Angeles as an urban dystopia, the beauty comes at a surprise. For that, you can thank Chancellor Charles E. Young, who enforced a unified campus aesthetic based on the Romanesque architecture of Bologna, Italy, during his extraordinary twenty-nine-year run as Chancellor. Not for UCLA is the casual anarchy of the Berkeley campus. The University campuses at San Diego, Santa Barbara, and Santa Cruz have three of the most prized oceanfront sites anywhere on California's mythically beautiful coast, yet once you turn away from the glorious ocean views you find only a few beautiful spots where the promise is realized, and others where you can only mourn the opportunities missed. The development of these coastal campuses, unconstrained by dense residential communities surrounding them, fell victim to opportunistic growth and undisciplined aesthetics, but UCLA was different. Coming onto the UCLA campus was somewhat like entering the Vatican, a hushed enclave of respect for knowledge and art amidst the noisy chaos of the gigantic metropolis all around.

**Figure 10-5: Royce and Haines Halls, UCLA. The serene beauty of the UCLA campus was a surprise to me, as it is to others who enter it for the first time. UCLA owes the magnificence of its physical surround to its greatest chancellor, Charles E. (Chuck) Young.**

Chuck Young became Chancellor in 1968, the year after I arrived. At thirty-six years of age he was the youngest university leader in the US, and I, eight years younger, was perhaps the youngest tenured faculty member in the University of California. Chuck and I were both born ahead of the World War II baby boom, but I was a beneficiary of the baby boom's stimulus to faculty hiring, which he was able to use to build the UCLA faculty.

Few of my teachers in the East would have predicted the excellence UCLA would achieve. Certainly, few of the senior faculty then at UCLA seemed to see the future Young saw, and I, fresh from Princeton's European émigré sophistication, looked upon my senior UCLA colleagues (with a few exceptions) as uncomplicated hicks who could not wait for the day to pass so they could go the faculty club for their "cocky-tails" at sundown. I had to remind myself that UCLA had hired some of the best young people in the country in plasma physics and space science.

UCLA had also recruited some of the best people in the country in basketball. Betty Rae Brown, the Secretary of the Plasma Physics Group, and her husband Marvin, were close friends with the legendary basketball coach John Wooden. About two months after I arrived on campus, Betty Rae invited me to a practice game between the freshmen and the varsity team that had won the national championship the previous two years. I was dubious; why go to a meaningless exhibition game? When the freshmen, led by a seven-foot center named Lew Alcindor (later Kareem Abdul-Jabbar) won handily, I knew something special was going on. They would rebroadcast the UCLA basketball games late at night on television and, for the next seven years, and I would sneak into the ends of games after my wife had gone to bed. Wooden won ten national championships between the 1964 and 1975 seasons. Curiously, Wooden never went on recruiting trips; athletes who wanted to play for him had to come to him. The coaches I knew at Harvard were circumspect, but they recruited more openly than John Wooden.

Then there was the Physics Department. The UCLA Physics Department's main ego problem was UC Berkeley's Physics Department. Berkeley already had a historically great department when a tiny UCLA moved to its Westwood campus in 1929. There seemed no way UCLA could replicate pioneers like J. Robert Oppenheimer and Ernest O. Lawrence, or incubate anything like the nation's two most important nuclear weapons laboratories, Los Alamos and Livermore. To its very great credit, the UCLA Physics Department set out to build a comprehensive program with excellence in many areas. Most other departments would have been satisfied, had they not had Berkeley for a big brother. Burt Fried built a plasma group that was arguably superior to Berkeley's, but the rest of the UCLA Physics Department believed that no matter how good UCLA plasma physics was, it made at best a marginal difference in the physics prestige sweepstakes. Particle physics was king at UCLA, just as at Princeton.

My main problem with the UCLA Physics Department was that it had no Harry Petschek or Roald Sagdeev to guide me over the rough spots. Tenure or no, I lacked confidence. Ferd Coroniti came to UCLA from Berkeley shortly after I did and played the Petschek role for me throughout the twenty-six years I was a professor there. Ferd thinks deeply, reads carefully, and calculates relentlessly. He ruminates. You can chew things over with him. He was my enabler, and I came to rely on his scientific judgment; an idea had to get by him before I dared let it leave my office. Ferd and I were to spend hour upon hour thinking about space plasma physics and astrophysics, gossiping about space physicists and astrophysicists, and smoking our pipes together. In a repeat of my years with Harry, Ferd and I used to work deep into the night at one of our homes about once each week until family obligations caught up with up with us. Years later, Ferd was to follow me as Chair of the Physics Department and combine it with UCLA's Department of Astronomy.

I was as attracted to the work in other departments at UCLA as I was to the work of Burt Fried's group; this was a problem because Burt was the one who recruited me. After my arrival, the disloyalty problem became worse. Professor "Venki" Venkateswaran of the Department of Atmospheric Sciences asked me to recommend a theorist they could recruit. I recommended MIT's George Siscoe and Richard Thorne, whom I had worked with in the Petschek summer seminars at Avco three years before. Much to my astonishment, the Atmospheric Sciences Department appointed both, such was the glamour of space research at that moment in time.

UCLA was off to a very fine start in experimental space plasma physics, the first of the disciplines spawned by the space program. Paul Coleman had built a fine group of young experimental space physicists in the Department of Earth and Space Sciences that specialized in the measurement of magnetic fields in space, the most fundamental plasma parameter in the space sciences. Chris Russell and Bob McPherron formed the nucleus of this group, and Margaret Kivelson joined it several years later. Margaret was to become its most distinguished member, elected to the National Academy of Sciences in 1999, and chairperson of the Space Studies Board in 2018 at age ninety(!). Her son, Steve, was also elected to NAS. How many mother-and-son combinations have been elected to NAS?

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Despite UCLA's efforts in space science, it occupied one of the smaller provinces in Southern California's vast aerospace empire of the 1950s to 1980s. The San Diego Freeway was the aerospace empire's Royal Road, connecting Edwards Air Force Base at the north end of the San Fernando Valley to the Douglas Aircraft Corporation in Orange County to the south, and the airplane and spacecraft manufacturers trucked components to one another along this stretch of highway. The San Diego Freeway reminded observers of Boston's Route 128 as a connector of technology and culture, but it had far more traffic and twice as many lanes. Particularly relevant to our UCLA programs were the world's leading space science groups at Caltech and the NASA Jet Propulsion Laboratory in Pasadena, east of the San Diego Freeway, and a first-class group in space plasma physics at the Aerospace Corporation on the freeway south of the Los Angeles Airport. The aerospace laboratory was part of the Los Angeles Air Force base, headquarters of the US Air Force Space Command.

The 110-acre campus of another major aerospace firm, TRW Systems, was a few miles to the south of the Aerospace Corporation off the San Diego Freeway. TRW Systems was working in Avco-Everett's line of business, missile re-entry, but its largest efforts were in spacecraft fabrication for NASA and intelligence agencies. Burt Fried had worked for TRW before he joined the UCLA faculty, and he arranged a consulting job for me with Fred Scarf's space plasma research group at TRW. Ferd Coroniti soon joined the effort.

TRW's first incarnation was as an automotive firm, Thompson Products in Cleveland, Ohio. In the 1950s, two engineers trained at Caltech, Si Ramo and Dean Wooldridge, developed a major aerospace business in Los Angeles that merged with Thompson Products in 1958 to form TRW Systems (using the founders' initials as the name of the new company). Wooldridge soon retired to Caltech, but Ramo remained a major force in the aerospace and defense world until his death at 103 in 2016. Dan Goldin, my old boss at NASA who revered Si Ramo, took me to a memorable lunch with Ramo shortly before the great man died.

The once proud Los Angeles aerospace empire has now passed its zenith. TRW systems is now Northrop-Grumman Space Technologies, and many of the legendary aerospace companies, like Douglas Aircraft, that caught the imagination of a young New England boy in the 1950s, now live only in memory. But *it ain't over 'til it's over,* as the baseball philosopher, Yogi Berra, once said. In 2002, Elon Musk chose to put his new company, SpaceX, near the old TRW campus in the South Bay, the fading capitol of the Southern California aerospace empire. In 2009 I was a member of a Presidential commission, the so-called "Augustine Commission," that recommended that commercial companies like SpaceX launch humans into space under NASA guidance. I knew that SpaceX would take human space flight in a new direction, but my enthusiasm may have been due as much to the memory of working with Fred Scarf and Ferd Coroniti at TRW in the Los Angeles aerospace empire at its zenith.

Fred Scarf had developed the first detectors of electrostatic plasma waves in space and was flying them on the first generation of missions to the earth's magnetosphere. One Wednesday a week for the next twenty years until Fred died, Ferd and I would drive the San Diego Freeway to TRW to work on the data that had arrived in Fred Scarf's suite of offices. Our job was to think about that data. Fred was not jealous about credit and Ferd and I were co-authors—and even first authors—on papers using an instrument that Fred had invented and brought to fruition,[13] such as the first detection of electrostatic plasma waves in the earth's magnetosphere[14] and in the earth's bow shock.[15] It was amazing to me that the theory I had learned at

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[13] A few of the better known papers written with Fred Scarf and his group at TRW: C. F. Kennel and F. L. Scarf, "Thermal Anisotropies and Electromagnetic Instabilities in the Solar Wind," *Journal of Geophysical Research* 73, no. 19 (1968):6149-65; C. F. Kennel, F. L. Scarf, F. V. Coroniti, E. J. Smith, and D. A. Gurnett, "Nonlocal Plasma Turbulence Associated with Interplanetary Shocks," *Journal of Geophysical Research: Space Physics* 87, no. 1 (1982):17-34; F. L. Scarf, F. V. Coroniti, C. F. Kennel, D. A. Gurnett, W. H. Ip, and E. J. Smith, "Plasma Wave Observations at Comet Giacobini-Zinner," *Science* 232, no. 4748 (1986): 377-81.

[14] C. F. Kennel, F. L. Scarf, R. W. Fredricks, J. H. McGehee, and F. V. Coroniti, "VLF Electric Field Observations in the Magnetosphere," *Journal of Geophysical Research* 75, no. 31 (1970): 6136-52.

[15] R. W. Fredricks, C. F. Kennel, F. L. Scarf, G. M. Crook, and I. M. Green, "Detection of Electric-Field Turbulence in the Earth's Bow Shock," *Physical Review Letters* 21, no. 26 (1968):1761.

Princeton, Avco, and Trieste—scribbles on paper—could come alive in the sophisticated instruments that Fred, trusting unworldly theoreticians like us, had devised. If plasma waves are in space around earth, why not at Venus, Jupiter, and the solar wind, too? Later, Fred discovered a continuous lightning storm atop a huge volcano on Venus. It was one thing to hear a professor preach that the laws of physics are universal, and quite another thing to find the vacuum of space filled with tangible, living plasma waves whose recordings fill heavy rolls of paper with data. Fred was extracting meaning out of the vacuum.

That may sound like an awful lot to learn from one person, but there was more: for Fred Scarf, there was no greater privilege, no higher form of existence, than to be a principal investigator (PI) on a NASA spacecraft. Fred spent hours every day on the telephone with other PIs working through the interpersonal, organizational, and engineering complexities of his missions. Getting a dozen or so specialized experiments squeezed together in the same tiny spacecraft was never going to be easy, and Fred spent those hours wheedling, cajoling, and flattering his counterparts in search of the give-and-take that enables experiments onboard to meet their minimum requirements.

From hearing Fred's tales, Ferd and I absorbed the technicalities of experiments that provided the cornucopia of plasma wave data Fred gave us to harvest. It was neither common nor easy for theorists to learn how experimentalists really think, but we had an insightful teacher. We learned that experimental issues are not settled by elegant debate in the conference room but by bending metal on the workshop floor. To the outside world, a spacecraft experiment may be a gadget, a metal box with complicated insides, but in Fred's world it was a collection of bright individuals whose notions and emotions must be harmonized before anything can be built. Fred brought us into that NASA world and gave us small but definite roles in one of the two greatest NASA space science projects of the twentieth century, the Voyager missions to the outer planets, Jupiter, Saturn, Uranus, and Neptune.

# 11

# When my UCLA World was Bright and Shiny

I had conceived the grandiose ambition to publish at least one research paper on every natural environment where plasma processes are important. I did not, and never will, know enough about all the different natural environments where plasma processes are important, but UCLA had students who could immerse themselves in the observational literature that I no longer had as much time to read. I could teach the students plasma physics and they could teach me what was in the literature, just as Harry Petschek had done with me.

I came to think of my students and postdocs as a little band of hunter-gatherers whose prey was publications. They were advance scouts, and I brought up the rear with the heavy weapons. That was the ideal anyway, though things did not always work out that way. The moral hazard is obvious when a professor supervises research in topics he or she knows little about, but I had worked out a rationale. When PhD students are learning something at research depth for the first time, you can either put them on a topic that you know well or one that you do not know well; students have to overcome the same degree of ignorance, whether or not their professor understands the subject.

I pacified my ethical qualms with the rationale that I would share the experience of discovery along with them. If I knew too much about their topic, over-directive professorial guidance would quash their creativity. I could help students around mental and methodological roadblocks, but I would not, and could not, supply the combination of desire and virtuosity that cuts through complex problems; they would have to find that by themselves. I would measure my success as a teacher not by the improvement in their technical skills, but by the growth in their independence and initiative. I did not always live up to this ideal, but my greatest pleasure in teaching came when I saw students prosper on their own. I knew they were ready to finish their thesis when they told me angrily that, because I had been away, they had to move ahead and undertake the next step in their research without my advice.

While plasma science has antecedents that go back to the nineteenth century, the study of fully ionized plasmas at extremely high temperatures began with classified atomic bomb research in World War II. The US government kept this research classified until 1958, so even in 1968 many of my scientific colleagues did not know what plasma physicists did. Plasma physics was considered applied physics since it was not on the quantum physics frontier. On the other hand, I could boast (at the time) that ninety-nine percent of the universe was in the plasma state (astronomers were then just beginning to learn about dark energy and dark matter). I could say that plasma physics applies to everything in the universe but the tiny cool enclaves where life exists, conveniently forgetting that life might just be the most important thing in the universe. The first satellite experiment was devoted to a plasma phenomenon, the Van Allen belts, and when I started work with TRW, fundamental discoveries in space plasma physics were still being made. Over the next nineteen years, my research was to get to the ionosphere, earth's magnetosphere, the solar wind, the plasma envelopes of other planets in the solar system, the acceleration of cosmic rays, neutron star magnetospheres, and active galactic nuclei before I ran out of psychological steam.

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The stacks of paper would be waiting on our desks when Ferd Coroniti and I arrived every Wednesday morning in Fred Scarf's offices at TRW in Redondo Beach. The stacks unfolded into sheets of machine-drawn graph paper, each fifty (or more) feet long. Fred would wait until we arrived before looking at what was on the piles of graph paper, but then a group of us—Ferd and I, Fred's colleagues Bob Fredricks and Gene Greenstadt, and Fred—would spend the morning poring over the machine-drawn figures. To the untutored eye the lines were meaningless scribbles, but to us they looked like data from the first detectors of electrostatic plasma waves ever flown in space.

Electromagnetic waves (radio waves are a familiar example) can propagate in empty space, but electrostatic waves propagate only in plasmas. Electromagnetic waves can reach the ground from space, but an instrument has to be inside plasma to detect an electrostatic wave. Fred Scarf had designed the first electrostatic wave detectors, his team at TRW had built them, and NASA had flown them on missions in the earth's magnetosphere and the solar wind. Almost everything we looked at was new to science, but we had to make sure the data were not some artifact of the way they were collected and processed. You couldn't make sense of the data without the people who made the detector being there, and they couldn't make sense of it without having people side-by-side who understood the theory, which was just then getting into textbooks. Ferd and I were those theorists. Fred's team would discuss the results over lunch, and we all would spend the afternoon deciding on the next data processing run.

I learned later how hard it is to do what came so easily then. We were the first to detect electrostatic plasma waves in the earth's bow shock, probably the first time it had been done in any plasma shock wave.[16] We made the first observations of electrostatic plasma waves in the earth's magnetosphere.[17] We rejoiced with Fred when he discovered lightning on Venus.[18] It is one thing to read about something, another to talk knowingly about it, and a whole other thing to get your hands on data collected in space by instruments made by a friend of yours.

Physicists had been studying so-called cosmic rays since the 1920s, but it was not until 1958 that James Van Allen made the first

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[16] R. W. Fredricks, C. F. Kennel, F. L. Scarf, G.M. Crook, and I. M. Green, "Detection of Electric-Field Turbulence in the Earth's Bow Shock," *Physical Review Letters* 21, no. 26 (1968): 1761; R. W. Fredricks, F. V. Coroniti, C. F. Kennel, F. L. Scarf, "Fast Time-Resolved Spectra of Electrostatic Turbulence in the Earth's Bow Shock," *Physical Review Letters* 24, no. 18 (1970):994.

[17] C. F. Kennel, F. L. Scarf, R. W. Fredricks, J. H. McGehee, and F. V. Coroniti, "VLF Electric Field Observations in the Magnetosphere," *Journal of Geophysical Research* 75, no. 31 (1970):6136-52.

[18] W. W. Taylor, F. L. Scarf, C. T. Russell, and L. H. Brace, "Evidence for Lightning on Venus," *Nature* 279 (1979):614-16.

measurements of plasma particles *in* space, rather than particles *from* space. As a result, Van Allen had become a household name that even my mother followed in the newspapers. I traveled from Avco to the University of Iowa in 1964 to explain to Van and his students why Harry Petschek and I thought the electron fluxes Van measured had the intensity they had and no more. Four years later, in 1968, I was invited to speak at the symposium honoring the tenth anniversary of the discovery of the Van Allen belts. Now the great man was telephoning me. He was a bit sheepish, actually; he said the Jet Propulsion Laboratory had already formed their Science Advisory Committee, and only then did they realize that they needed someone who can estimate the radiation hazard to the spacecraft they were planning to send on a reconnaissance of the outer planets. The mission did not have a name yet, but later it was to be called *Voyager*.

I was paralyzed by excitement at being asked by Van to join to the NASA Science Advisory Committee, but Ferd Coroniti's first reaction was more constructive: I think we can estimate the intensity of the Jovian radiation belts, he said. We will start with what is known about earth's radiation belts; we can apply the current theory of radial diffusion of Van Allen belt electrons, which would predict that electrons will become more energetic as they diffuse inward towards Jupiter; in Jupiter's case, they should reach relativistic energies as they near the planet, whereupon they should emit synchrotron radio emissions. Given a certain electron flux at a certain place in Jupiter's magnetic field we could calculate the intensity of the radio emissions, but we can also work the problem backwards. Using the intensity of Jupiter's radio emissions measured by radio telescopes on earth, we could calculate the intensity of the energetic electron fluxes near Jupiter. We can then work radial diffusion theory backwards to infer the radial profile of the energetic electron intensity elsewhere in Jupiter's magnetic field. If the electrons fluxes cross the Kennel-Petschek limit, we can assume they will be lost to Jupiter's Atmosphere. All this will give JPL's engineers enough quantitative information to calculate the risk to the spacecraft as it flies through Jupiter's radiation belts. The method for electrons would also tell us how rapidly protons diffuse inward. These ideas came together in an afternoon's discussion, and all that remained was to do the work, which took several years.[19] Later, Ferd

[19] C. F. Kennel, "Stably Trapped Proton Limits for Jupiter," *JPL Proc. of the Jupiter Radiation Belt Workshop* (1972): 347-61.

invited Richard Thorne, who had worked with us in the Petschek summer seminars, and who was now at my suggestion in the Department of Atmospheric Sciences at UCLA, to collaborate.[20]

No matter how sophisticated our theory was for its time, engineers would never trust a spacecraft to any theory. JPL's competitor, the NASA Ames Research Center in Silicon Valley, had already launched *Pioneer 10* on a flight to Jupiter—a much simpler spacecraft than the one JPL was planning. *Pioneer 10* would provide ground truth, or...space truth. My responsibility to JPL's Science Advisory Group was to be a knowledgeable interpreter of *Pioneer 10*'s radiation belt measurements. Ferd and I reviewed the measurements as well as our own theoretical work at a symposium in Frascati near Rome. Young scientists today would be surprised by the excitement this workshop created among the participants; this was the first time many European space scientists heard results from the first spacecraft ever to travel to an outer planet. It was also the last time Jupiter's magnetosphere seemed simple. A new world of space plasma physics was to be discovered by the Voyager missions then being designed by the Science Advisory Committee. Jupiter and its moons form a mini-solar system inside Jupiter's huge magnetosphere. The moons inject plasmas into Jupiter's magnetosphere, and also stir it up. One moon, Io, creates a permanent aurora in Jupiter's atmosphere as it orbits the planet.

A pattern had been set. I brought knowledge of an important practical problem to TRW and UCLA, communicated the problem to collaborators more technically competent than I, did the work with them, and then I framed the larger meaning of the results. I would also write a review article in hopes of increasing the visibility of the problem.[21]

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[20] R. M. Thorne and F. V. Coroniti, "A Self-Consistent Model for Jupiter's Radiation Belts," *JPL Proc. of the Jupiter Radiation Belt Workshop* (1972).

[21] C. F. Kennel, "Magnetospheres of the Planets," *Space Science Reviews* 14, no. 3 (1973):511-33; C. F. Kennel and F. V. Coroniti, "Is Jupiter's Magnetosphere like a Pulsar's or Earth's?" *The Magnetospheres of the Earth and Jupiter* (1975): 451-77; C. F. Kennel and F. V. Coroniti, "Jupiter's Magnetosphere," *Annual Review of Astronomy and Astrophysics* 15, no. 1 (1977): 389-436; C. F. Kennel and F. V. Coroniti, "Jupiter's Magnetosphere and Radiation Belts," in *Space Plasma Physics: The Study of Solar-System Plasmas. Volume 2: Working Papers, Part 1: Solar-System Magnetohydrodynamics* (1979): 454-558.

Not every good scientist bears obvious marks of genius. The young Albert Einstein did not. Strikingly ordinary people do extraordinary things. Success in science depends at least as much in *wanting* to solve a problem as in *knowing* how to solve it.

My very first PhD student put this set of beliefs to a direct test. Joe Kindel had a non-elite educational background and did not have conspicuous success on his PhD qualifying examination, but his enthusiasm and drive convinced me to take him on. He had a way of taking in things that he heard. I gave him a topic I might not have dared to take on alone: I had some inner resistance to an idea posed by the Nobel laureate Hannes Alfvén, an idea subsequently much elaborated in the literature—namely, that electrical currents flowing parallel to the magnetic field between the ionosphere and space would connect the plasma flows in space to those in the ionosphere. Alfvén's field-aligned currents were known to be subject to instabilities that would resist the current flow, and upward currents generate electric fields that accelerate electrons downward; these accelerated electrons were candidates to cause the atmosphere to light up in the *aurora borealis* and *aurora australis*.

Alfvén, however, had not specified which instabilities; besides, there might be different ones at different altitudes. I suggested that Joe calculate the instabilities of magnetic field-aligned currents as they flow upward through the upper atmosphere from auroral altitudes to outer space. Joe crushed the problem. Joe realized that the upper atmosphere is not one place, but a continuum of places with different dominant ion species and electron densities at different altitudes, and he adapted his instability calculations to the altitude continuum. The Kindel-Kennel paper is still cited fifty years later.[22] Joe would take a postdoctoral position at Princeton, and then go on to an important career in laser fusion with the Los Alamos National Laboratory and in administration with the Department of Energy in Washington, DC.

I had been working with Joe Kindel for some months when Ken Lee asked to become my next PhD student. When I asked Ken why, he said he heard from Joe that I was a really good advisor. That is not a good reason, I said. You only have to put up with me for a few years, but you have to live with plasma physics for the rest of your professional life. Talk to Joe and me, come back with a more

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[22] J. M. Kindel and C. F. Kennel, "Topside Current Instabilities," *Journal of Geophysical Research* 76, no. 13 (1971): 3055-78.

detailed proposal, and give me your reasons why the problem is worth your spending time on. Ken came back after about a month. We had discussed several problems he could do, he had thought about his reasons for the problem he chose, and I did not have the heart to discourage him further. He finished a good degree,[23] but not everyone can focus desire on a recalcitrant scientific problem until it yields, as Joe Kindel had done.

This was the problem Ken chose: In the 1920s, long distance radio communication had been made possible by bouncing short-wavelength radio waves off the plasma in the upper atmosphere, in a layer called the ionosphere. There had been active research on ionospheric plasmas since then. Don Farley from Cornell, using radars to probe the ionosphere, was detecting backscattered radar signals from plasma waves generated by an electrical current that is confined to a narrow band straddling the equator—the so-called equatorial electrojet. Farley had found that the electrojet plasma waves doing the backscattering were generated by an instability that occurred when the electrojet current passed a certain threshold strength. To find a way into Farley's subject, I suggested that Ken extend Farley's work to characterize the whole spectrum of plasma waves generated by the electrojet instability, not just the ones at long wavelengths that Farley was talking about.

One day a few years later, a glamorous young woman strode into my office on a mission. George Paulikas, who went on to become Executive Vice President of Aerospace Corporation, had told Mary Hudson that she should talk to me about getting a PhD. Mary had earned her bachelor's degree in physics from UCLA at the age of eighteen, and she had been working on satellite data at Aerospace with George for the past couple of years. She was already a professional and she left no question about motivation, so I agreed to take her on as my student. I wanted to learn about radar observations of the F region of the ionosphere, a hundred kilometers or so above Ken Lee's E layer, so I put Mary on that problem.[24] Our effort was not earthshaking, but I learned things I should have known about the F region. Mary finished her PhD in short order and went on to the University of California, Berkeley. Later, she called to ask if I had a

[23] K. Lee, C. F. Kennel, and J. M. Kindel, "High-Frequency Hall Current Instability," *Radio Science* 6, no. 2 (1971): 209-13.

[24] M. K. Hudson, and C. F. Kennel, "Linear Theory of Equatorial Spread F," *Journal of Geophysical Research* 80, no. 34 (1975): 4581-90.

student about to finish the PhD whom she could bring to Berkeley as a postdoctoral research fellow. Bill Lotko accepted Mary's job offer after he finished his degree,[25] and—lo and behold—after several years of working together they married. Evidently my supervision had not disabled all initiative. Not many have had students who became husband and wife.

I spoke at the ceremony honoring Mary's award of the American Geophysical Union's James B. Macelwane Medal for exceptional scientific achievement below the age of thirty-five. Bill and Mary recently retired after proud careers of teaching and research at Dartmouth College. Dartmouth, whose space physics programs were originally led by my contemporary Bengt Sonnerup, had been an early entrant into Van Allen belt research, and Mary's research with Paulikas had been in this area. On the sixtieth anniversary of *Sputnik*, in 2017, Mary received the John Adam Fleming Medal, the American Geophysical Union's highest award for research on the earth's plasma environment.

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To get from the Physics Department to the UCLA Faculty Club, I had to walk past an inverted fountain, a circular pool with gurgling water flowing from its circumference into a drain at the center. The water twinkled and glistened in the California sun. I walked past this fountain almost every day on the way to lunch. You could not miss the undergraduate women splashing topless in the inverted fountain, a playful assertion of the women's liberation movement that had been consuming my wife's friends.

Topless sunbathing wasn't the only thing new to this Easterner. One day, three stately young women with long blond hair interrupted a physics faculty meeting. Pictures of innocence in diaphanous white gowns, they processed across the meeting room carrying lit candles in each hand. Please, they pleaded, please stop making nuclear weapons on campus. It was heart-rending. We faculty said there was an old nuclear reactor about to be decommissioned, but UCLA had never made and would never make nuclear weapons *on campus*. We would never think of it. We did not

[25] W. Lotko and C. F. Kennel, "Spiky Ion Acoustic Waves in Collisionless Auroral Plasma," *Journal of Geophysical Research: Space Physics* 88, no. A1 (1983): 381-94.

tell them that the University of California was the only university in America that *did* make nuclear weapons, but at its Los Alamos and Livermore Laboratories.

The women's procession for peace happened at a time when students were protesting the Vietnam War at universities all over the country. UCLA's protestors were using departmental telephones to call their counterparts long distance, which was seriously annoying to some on the faculty. My Dean, Harold Ticho, saw that the students were sending us a message and believed, to his credit, that we should do something about it. He called me in, my first trip to a dean's office. You, the youngest tenured faculty member, Harold said, are the only faculty member with a chance to bridge the generational divide. (The students were saying they did not trust anyone over thirty, and I was just over.) *YOU* are going to co-chair the student-faculty committee that is to examine student grievances.

The undergraduate physics students on the student-faculty committee were remarkably open and warm for all their hostile rhetoric. Later, I realized that some of them wanted to be like us physicists. They did have an agenda though, and so we made a list of issues together. We agreed to meet for dinner once a week, take up one issue per meeting, and not break up until we reached consensus on what to do about that issue. The students knew their teachers: they plied us with good wine, temporarily bridging the generational divide. Predictably, the conversation would flag as one senior professor after another would fall asleep, until I was left alone to negotiate with cloyingly friendly young people carrying the hopes and fears of their entire age cohort. One time, we did not break up until 5 a.m. when I had to be up at 7 a.m. for breakfast with the children. But the group got a lot done, including something that lasted: we invented the faculty evaluation form that was used for decades at UCLA and other campuses of the University of California. We designed the form to be gradable by computer; it was hard for senior faculty to reject so explicit an appeal to modernity.

Harold Ticho also said I should do something about one of the students' main issues myself. They found physics teaching to be dry and impersonal, he said, and *YOU* will teach a new course in physics for non-science majors. There may be other inputs to your salary consideration, Harold said, but *mine* will be proportional to the enrollment in that class! Is there a stronger marching order?

Harold also asked Ernie Abers, a wonderful human being cursed with a surfeit of human empathy, to co-teach Physics 10 with me. Ernie and I wanted to tell stories about the lives of scientists, how they made discoveries, and where modern science came from. We devised a historical course that we named Mechanics from Aristotle to Einstein. Aristotle had started where students start, through naïve observations of the regularities of the natural world, like the apparent motions of the planets through the sky. Aristotle was actually just organized naïveté. You could also include electricity, magnetism, and light, and you could get to Einstein's special theory of relativity without taxing the students' limited mathematical backgrounds. Our course ended with a lecture on Big Bang cosmology.

Ernie Abers and I would teach Physics 10 as physicists insist. We would teach that quantitative agreement with experiment is the arbiter of scientific truth, but we could not get very far towards humanizing science without talking with historians of science. Ernie and I met with historians at lunch once a week for the next five academic years. The lunch regulars included Robert Westman and Norton Wise from UCLA's History Department, Robert Frank from the UCLA School of Medicine, and their guru, the great Israeli-American scholar of Medieval and Renaissance thought, Amos Funkenstein. Amos came to the lunches because he wanted to know how physicists think. Serious discussion was often diverted by politics, especially during the run-up to President Nixon's resignation, but all of us learned enough from one other that we kept at it for five years. Ernie and I even gave a paper at a history of science conference,[26] and we met some of the historians whose books we were reading. Bob Westman introduced us to the work of T. S. Kuhn and Kuhn's concept of ordinary science periodically transformed by a by paradigm shift. Kuhn's book, *The Copernican Revolution,* provided the organizing principle for Physics 10 and, I should add, much of the professional research in the history of science at the time.

Ernie and I wrote a textbook based on the lectures in Physics 10, *Matter in Motion: the Spirit and Evolution of Physics.*[27] I learned more writing that book than all the students put together did reading

[26] E. S. Abers and C. F. Kennel, "Commentary: The Role of Error in Ancient Methods for Determining the Solar Distance," in *The Copernican Achievement*, ed. Robert S. Westman (Berkeley: University of California Press, 2020), 130-36.

[27] E. S. Abers and C. F. Kennel, *Matter in Motion: The Spirit and Evolution of Physics* (Boston: Allyn and Bacon, 1977).

it. In retrospect, *Matter in Motion* lacked the careful craftsmanship that immersion in subject matter, dedication to pedagogy, and long experience in the classroom produce in works that find enduring value. But I enjoyed writing it.

I probably devoted as much creative energy to the history of physics as I did to research during those five years from 1970 to 1975. Why did I allow myself this diversion at the age of maximum scientific productivity? People often say that things always happen for a reason; actually, when big and mysterious things happen, there is more than one reason. Here are some that I have thought of: I did not know where the science I had committed my life to came from. I hoped I could justify my course in life. I had hoped I might find personal orientation in the life histories of other scientists; knowing I was a part of a larger historical movement could ease anxiety over my life's course. I could earn the respect of my father-in-law, Salomon Bochner, who was writing his book on the role of mathematics in the rise of science (I loved to hear Amos Funkenstein's brilliant conversation; he and Salomon Bochner had similar modes of reasoning). But it was the deepening disjunction with my wife that was the most urgent. Debby complained frequently that scientists were machine-like tools with ritually learned simulation of human feelings and, at best, acquired comprehension of social implications. By showing respect for her field and even trying research in it, I might earn the acceptance, perhaps the affection, of my wife. I might even allay the social anxieties implanted in me by my mother.

At fifty years' remove, I see that knowing something of the history of science illuminated the way I thought about science. Unraveling intellectual puzzles just for the joy of it was not for me. I could not worship technical virtuosity for its own sake as did my graduate student friends at Princeton. Working—however ineffectively—in an area that is historically significant would be more meaningful to me than triumphing single-handedly over complexities that no one else perceives.

# 12

# Emergency at the Center of Life

The trouble started before marriage, but it became permanent after our daughter, Sarah, was born on September 26, 1970. That was when marital relations ceased. It was not mentioned aloud; our day-to-day relationship did not become relentlessly acrimonious but rather retreated into resigned cooperation, for we had two human souls that depended on our care and guidance. Nonetheless, periods of outward peace would be punctuated by explosive disjunctions. While there were deep reasons for the fragility of our marriage, Debby was most concerned with her lack of progress towards a PhD in Renaissance history. My career was blocking her path to salvation through work.

I felt responsible. I had interrupted Debby's studies twice, but the pattern started even before I was on the scene. Her mother hounded her out of Yale, where Debby had made a promising start with the eminent Renaissance scholar, Roberto Lopez. Then I accepted Sagdeev's invitation without consulting her and tore Debby away from her studies at Harvard to pleasant but irrelevant Trieste. We returned to Cambridge in less than a year so she could restart her studies, but a year after that I condemned her to living in alien Los Angeles and finishing her degree at UCLA, an institution that she considered second-rate.

I believed that what most would afflict me was afflicting my young wife. Solve her work problem, I thought, and the rest would fall into place. My social surroundings reinforced my belief. The

1970s were an age of emergent women's liberation in America; I did not need the remonstrances of my wife's friends to convince me to support her career, especially since my path was now defined. Having achieved tenure at age twenty-eight, I would not need to search for jobs as other friends our age had to; I could stay at UCLA until she got her degree, and we would figure out together where to go next. In the meantime, I would leave my office in early afternoon for after-school lessons and childcare responsibilities; I would take the children away on weekends to give Debby unbroken hours to work; I was not going to see my wife fail if I could help it.

I vowed I would do all I could to free her time so that she could work. Little did I know that the conflict between the only two mortal vows I ever made would turn domestic life into a continuing emergency. I am not very good at abnegation. The unspeakable vow made the day my father died would not let me miss professional opportunities, yet I had also vowed not to smother the spiritual and intellectual potentialities of a highly intelligent woman. Some problems cannot be solved but can be managed; maybe this was one of them. Perhaps with unfailing effort, I could get through this life emergency.

Ultimately, I made a promise that I did not keep. Each lapse caused pain that got worse with time, and eventually I settled into a life of practical cooperation and emotional distancing. If she had not tried to cope, I would not have felt so guilty; her dissatisfaction with her life had to come out and did. She complained regularly that the alien vastness of Los Angeles decontextualized her, marooned her in a cultural desert at a second-class university with a second-class library.

There were weeks-long arguments; I could not figure out what I had done to trigger them. Equally painful but more concerning were the prostration episodes. On one occasion we took the children on a weekend excursion to Death Valley; the children and I were up and about at 7 a.m., but she stayed in bed until noon only to emerge from the hotel room wringing her hands, eyes wandering, vacantly whispering about the fearful landscape. During another handwringing event, she emerged from her room in a frenzy in mid-morning to throw away the children's clothes. Between cataclysms life was outwardly normal. I lived for those blessed intervals.

I numbed myself against the loss of intimacy, but I did not always succeed. I told myself that intimacy ended shortly after marriage began for most people. I developed fleeting crushes on intelligent women I met at parties; hopeless yearnings were secretly aroused when colleagues came to me to ventilate about their passions, thinking, I supposed, that my marriage was secure. I watched one married colleague confess his passion for a student (they had a forty-year second marriage); another confided in me his affair with a divorced postdoctoral student trying to start a career (he returned to his wife). Their powerful emotions raised echoes within me, but I did not allow myself the temptation of infidelity, nor did I ever disclose the feelings of helpless isolation stemming from loneliness within marriage.

I kept getting offers for summer vacation jobs in tempting places. I had fantasies that my little professional successes would enrich family life, but my little successes occasioned resentment and increased Debby's fear of failure. Each offer was the cause of painful marital negotiation. Debby did come with me to Los Alamos, New Mexico, one summer vacation when I visited its famed laboratory; she appeared to enjoy the young family pool parties, but on the way home from them she complained that she despised the unsophisticated parties and the uncultured people. None were kindred souls and almost all were scientists. On another occasion, I spent two summer months alone in a lovely house I had found near the mountains in Boulder, Colorado. I was co-running a summer school at the High Altitude Observatory with Ian Axford, and Debby stayed home to work. I still remember the loneliness of watching Fourth of July fireworks in the University of Colorado stadium surrounded by families with young children.

Thomas Jefferson made a vow to his dying young wife that he could not truly keep; he swore never to marry again. Though Jefferson never did marry again, he did yield to a passion for the artist Richard Conway's wife, Maria, when he was ambassador to France; in recent years, we have all heard about the children Jefferson fathered with his slave housekeeper at Monticello, Sally Hemings. I had often thought of Jefferson's vow and how hard it had been for him to keep.

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Six years after our arrival in Los Angeles, Debby decided that she would spend a summer in Basel, Switzerland, to follow a research lead on her thesis. She would have three months free to work, and I would stay home with Matthew, then four, and Sarah, two. We invited my mother to visit from Pittsburgh while Debby was in Europe. Mother brought her alcoholism with her to LA, but she managed to obey the House Rules and avoid drink until after the children were in bed. That summer proved the happiest one since I had come to Los Angeles. Colleagues who did not see Debby and me socially during the academic year invited Mother, me, and the children to barbecues and pool parties every weekend during the summer.

Mother and I took the children on excursions that I usually would have managed alone. One of these excursions was to Marineland, a beautiful amusement park overlooking the ocean in Palos Verdes, south of Los Angeles. Mother suggested we invite along Ellen Lehman, our Pittsburgh friend and the daughter of Aunt Mary's best friend, Jane Lehman. I had met Ellen in Pittsburgh on winter breaks from Princeton, and even spent a memorable New Year's Eve with her at her parents' party, but at that time Ellen and I were out of phase, since she was a senior in high school and I was seven years ahead in graduate school. Time had passed; Ellen now had a PhD from Cornell, and a psychology practice in nearby Santa Monica.

The sky over Marineland that day was a deep cloudless blue. The vast Pacific Ocean stretched to the horizon as Mother, Ellen, and I took turns pushing Sarah's stroller while Matthew darted about alongside. Passersby got the idea we were a happy family. That evening, after Ellen had gone home and Mother had her first drink, she finally blurted out what had been in her mind. *You have got to get out of this marriage,* she said. *I can't leave her,* I replied, *we have these two children*. *In that case*, Mother said, *you had better seek couples' therapy*—quite an admission given what New Englanders used to say about illness stemming from moral weakness. Life as a widow in Pittsburgh had changed her thinking.

I said I would seek marriage counseling. Debby and I went together, but after only one session she never went back. At the time, I interpreted this as one more signal that she did not want to cure the problems in the marriage, but later I arrived at the more charitable view that she was afraid of what she might learn if she persisted in therapy. I continued alone for a while, hoping it would help me see how to improve the marriage. My therapy sessions provided some personal relief but no pathway to a solution. Debby and I did not, and could not, discuss why she dropped out of therapy.

My responsibility for the children was the main reason I had to stay in the marriage, but not the only one. Divorce had been taboo to my parents. I recall a dinner table conversation about a colleague of my father's whose wife had run away with a Belgian count. Everybody in the law firm knew that my father's colleague was separated from his wife. She had been living with the count for years when she finally sued for divorce. When my father broke the news of his colleague's pending divorce, Mother's reaction was, oh dear, he is such a nice man; *now he will have to leave the firm.*

I still did not know what to do. Perhaps a year in another country would free us to explore new ways of being together as a family. Part of me wanted to show that I could keep my promise to make the time productive for Debby's thesis research; another part wanted to work near the leading edge of plasma physics research. In 1974 I had been at UCLA for seven years, which qualified me for a full academic-year sabbatical, but my hopes turned out to be large burdens to place on a simple one-year sabbatical.

I promised Debby that we would choose the location to suit her professional development. Debby wanted to go a back to Basel to follow up on her previous trip. I had no reason to believe that Debby's summer in Basel had not accomplished the goals she had set for herself, but my problem was that I did not know of any plasma physics being done in Basel. I could not see myself devoting all my time to childcare for an entire year with nothing scientific to do, especially when I thought there was a compromise. We could go to Paris and she could work at the Bibliothèque Nationale, which was undeniably strong in French Renaissance history, and I could work with my Trieste colleague, René Pellat, at the École Polytechnique. I would take the children weekends and afternoons; Debby could visit Basel from Paris if she needed to, and I would take the kids. Besides, I could get financial support for Paris and not Basel. This conflicted dialogue was repeated in many different ways, but in the end, Debby went along with Paris in a spirit of grudging resignation. Then she devoted her energies to making the best of the bargain.

# 13

# A Year in Paris

Paris epitomized romance for an American who had spent his high school years in the 1950s on romantic novels and old jazz. Paris in the 1920s was where F. Scott Fitzgerald and Ernest Hemingway wrote about bullfights in Spain and lost weekends on the Riviera; racially tolerant Paris was where American jazz musicians chose to go on their way to making their new musical form a global one. Paris was where the great black jazz singer, Josephine Baker, chose to perform, where Louis Armstrong found an adoring reception in the 1930s, where the first great European jazz soloist, the Romani Django Reinhardt, set up shop. George Gershwin and Paul Whiteman discovered jazz rhythms in the sounds of Paris traffic in *An American in Paris*. The wealthy Yale graduate and Broadway composer Cole Porter passed the time in Paris with equally wealthy Americans, Gerald and Sarah Murphy, whose only apparent distinction was an indisputably elegant lifestyle. Their way of living was quite beyond the earnest academics Debby and I had become, but perhaps our spirits could bottle some of Paris' elixir and take it home. Even if bottled romance were too much to expect, how could a yearlong stay in France not have its wonderful moments for a young family?

As soon as Debby acquiesced to Paris, I asked René Pellat, who headed the theoretical physics group at the École Polytechnique, if some financial support were possible; René wrote back with an offer of partial support for a year's visit. I stopped briefly in Paris on a trip to a scientific meeting in Sweden to investigate apartments and schools. I found a large 1930s fascist-modern apartment with enough rooms for children and their parents' studies. René offered me critical help in the negotiations with Madame Morel, the landlady, who, he said, would be too much for me to handle alone. (She was, he said, from the province of Berry, and everyone knew *ils étaient avares*.) Mme. Morel's rental apartment at 110 Boulevard de Courcelles was within walking distance of the Parc Monceau, an elegant little park known to me from paintings by Claude Monet. The children could play outside there, an important consideration for an American family accustomed to a backyard. I enrolled the children in the nearby École Internationale Bilingue, a small private school for the scions of parents working for multinational enterprises.

Meanwhile, Debby arranged study privileges at the Bibliothèque Nationale and thought through the children's clothing and other needs. Though we both could commute to work on the Metro, this American family would need a car for excursions into the country. I arranged to pick up a yellow 1974 Mercedes Benz 230 at the factory in Sindelfingen, near Stuttgart in Germany, two days after the family's arrival in Paris, and I reserved a space at an underground carpark at the Place des Ternes. I had noticed that Paris taxis were yellow 230s, and I inferred they were reliable. I had that yellow four-cylinder Mercedes for 48 years.

Mme. Morel's Boulevard de Courcelles apartment came with an added attraction named Eugenie. Eugenie, a raw-boned, good-humored fortyish woman from the Cabo Verde Islands off the west coast of Africa, lived in Mme. Morel's servant's quarters on the top floor of the building. Eugenie had lived most of her life in the seventeenth arrondissement and by then had chatted up every one of the merchants on the nearby market street who provisioned her cooking during the year. Not only did Eugenie make West African dishes, but she taught the children useful French, whereas in school they got only proper French.

I acquired the habit of leaving École Polytechnique in mid-afternoon to take the children to lessons or to Parc Monceau after school. The Paris public schools allowed their buildings to be used after hours for private music, art, and dance classes, and I took the children to a school on Rue Ampère. (I remember the name because André-Marie Ampère was the physicist for whom the unit of electric current is named.) I would leave Parc Monceau's walkways, where civilized Parisians strolled, and chased the children around its stately trees, thinking that American children were more used to rowdy physical exercise than French children. The children and I occasionally took the Metro to Neuilly-sur-Seine whose park had a better merry-go-round than the Parc Monceau. On one or two occasions, we went to watch French children sail toy boats in the little ponds of the Tuileries, my fantasy of what fathers and children do together in Paris.

The children had been attending École Internationale Bilingue for about six weeks when Matthew's *maîtrise* called for a parent-teacher conference. Matthew has emerged the best in his class, she said, in reading and writing. Surely you mean among the English-speaking kids, I said. (Matthew had been reading English for several years, so he knew what to do when faced with the printed word.) *Non! Non! Non! He is the best in the whole class in French!* Were he French, we would prepare him for École Polytechnique or École Normale Supérieure. As it is, we want to skip him a grade, said Mme. *La Maîtrise*. But Debby responded: Matt is only here for a year; you do not have to do that… We have already given him extra lessons, Mme. Kennel, Mme. *La Maîtrise* said; he has already finished our first year and he would have nothing to do. It was a *fait accompli*.

It was about then that Sarah enrolled in her *expression corporelle* class, a playful mixture of dance and gymnastics that started her on the path to a serious ballet career as a teenager. Both children had advanced more in French life than either of their parents. Their parents' inelegant French visibly embarrassed them, and Sarah refused to wear American clothes. Eight-year-old Matthew memorized the street maps in *Paris Indispensable,* and when we drove around Paris in the yellow Mercedes he would call out the names of the Metro stations we passed, assuring his anxious mother that *he* knew where we were and telling his father when to make the next turn.

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The École Polytechnique, known colloquially in France as "X," was in the ancient Latin Quarter of Paris when the Kennel family arrived but was about to move to more modern quarters in Palaiseau, south of Paris. (Near the end of our stay, I commuted by car and not by Metro.) The most distinguished engineering school in France, X is a blend of America's West Point (since it is run by the French army), MIT (because of its excellence in engineering), and, say, New Hampshire's St. Paul's School (for the social distinction of its students). Every month I would mount the stairs to the office of a colonel in the uniform of the French army where he and I would have a delightful conversation about French life, and he would slip me a packet of French Francs, the honorarium René Pellat had promised. My only formal obligation was to guide a couple of student theses.

The students had their own name for him: *René n'est pas la*, meaning René is not there. I saw him nonchalantly skip out of important bureaucratic meetings, but I also experienced first-hand his uncanny knack of turning up when it was important, like when he negotiated apartment rent with Mme. Morel. You would think his high-handed approach to administration would disqualify him for high position, but over his lifetime he directed the French National Space Agency, the French National Science Foundation, and the French Atomic Energy Agency. It is not as though Pellat spent all of his time in administrative positions others would kill for; he certainly did not. His example of independence was to be before me whenever I pondered changes in my own career direction. The fact that one is trained in a particular discipline does not preclude contributions to other disciplines; administration is not a charitable graveyard for researchers whose minds are prematurely dead, as my Princeton graduate student friends proclaimed. I saw René quite happily do both; our work together revealed his continuing joy in discovery.

In his relationship with me, René saw a way to protect his commitment to basic science from the entanglements of immediacy. He made so many visits to LA that, in 1989, he was named a part-time professor in the UCLA Physics Department. He was to strike up an exceptionally fruitful collaboration with Ferd Coroniti, and René and I deepened a personal friendship that started in Trieste and continued in Paris and Los Angeles. You can imagine the shock felt by his Los Angeles friends when we heard in 2003 that he died of a heart attack on vacation in the south of France. He was only 67. Ferd and I traveled to Paris for his memorial services, and UCLA composed a memorial that reads in part:

> René's words were often in conflict with his actions. He claimed to pursue science as a game just to have fun. Yet, no one was more passionate about science or more devoted to the search for scientific truth. He always used the power associated with the very high positions which he held in France to advance the progress of science. René was often rough of manner and brutally frank in his critiques. Yet he trained, nurtured, and found positions for a cadre of brilliant French students

to whom he remained unfailingly devoted. At UCLA, those who knew and worked with René became members of his extended "family," and came to treasure their relationship with him. He left us much too soon.

Though all this lay in the future; in 1975, Rene and I were young scientists with an itch to use our plasma physics to make a basic contribution to astrophysics.

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French life was less alien to Debby than Southern California's, and things between us generally seemed to go well. We started using the new yellow Mercedes to take day trips on weekends to nearby attractions like Versailles, Fontainebleau, and Compiègne, where World War I's armistice was signed. We took a long weekend to see the cathedrals in Auxerre and Vézelay on the November 1 All Saints Holiday, the day after America's secular Hallowe'en. The pleasures of exploring France as a young family were too great to miss and we went on other weekend excursions together, but there were also occasions when Debby and I went back to the way we had been in Los Angeles. Then Debby would have me take the children off her hands for a weekend; these excursions were invariably lonely.

One rainy winter weekend I drove Matthew and Sarah to Deauville on the Normandy coast. The children and I explored Deauville's coastal boardwalk on a tricycle called a *Deauvillaise,* visited the fishing boats in the lovely nearby port of Honfleur, and later had dinner together in our elegant old hotel back in Deauville. Our dinner was at the early hour when elderly people tend to dine, and those in the restaurant gave us curious glances, imagining, I supposed, a divorced American father in a rare reunion with his children. The children's French from school and Eugenie was so good that you really could imagine they had been living in France with their mother for some time. The loneliness I felt at those moments was not a good advertisement for divorce.

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I had the fantasy that in Paris, Mother could relive her summer-long trips to Europe with my father in the 1950s, so before leaving LA I purchased airline tickets for her April visit. Remembering our afternoon conversations about the popular music of the 1930s when I was in junior high school, once in Paris I landed tickets for the much-anticipated return to the stage of the fabled American expatriate jazz singer, Josephine Baker. That was not to be. Baker was to die a month before Mother.

Mother had achieved a compromise with alcohol after her return from the sanatorium in 1947. She was granted half the day with emotions stabilized yet perceptions freed in return for yielding to formless chaos in the other half. She held to this regime until her health failed. One day in early winter 1975, my aunt Charlotte called to say that I was to come to Boston; Mother had collapsed with a seizure. The seizure proved to be due to migration of metastatic lung cancer to her brain; she got the lung cancer from decades of chain smoking while drinking. Mother's physician at the hospital told me she had at most weeks to live. Did I want to tell her, or should he? I said I would do it. I spent the next two weeks at the foot of her bed in the Boston University Hospital, as I had done afternoons after junior high school in Waban. We reviewed our lives during those last long conversations. I heard her stories one last time. Finally, a switch turned, and she sent me away; we were done talking. I was sent away to look after my wife and children in Paris.

I plunged into depression; I was mourning while Mother still lived. Now the only parents I would ever have would both die deaths of despair. The low point of my own living despair was a terrible hangover after a party with *New York Times* reporters Clyde Farnsworth and Flora Lewis. Lewis had flown into Paris from Geneva where she was following the UN negotiations for the Law of the Sea. Her voice, drinking, and manner reminded me of my mother; her fascinating conversation evoked fantasies of what my mother might have become in a different world. I wandered the streets of Paris with queasy stomach and pounding head for three days after trying to drink with Flora Lewis. What had been music to George Gershwin, the sound of Paris traffic, had become painful cacophony. I telephoned Mother often, but she and I had both come to terms with the unavoidable and we let the shallow disguise the obvious during those last ritual calls.

Debby and the children had been housebound while I was in Boston, so we took them on a weekend excursion to see the Bayeux Tapestries in Blois; it was then that my Aunt Charlotte called with the news of Mother's death, about when the Josephine Baker concert would have been. Now both my parents were dead.

A story from Homer's *Odyssey* with a personal twist has haunted me ever since. Odysseus (aka Ulysses), sailing home from the Trojan War with his men, must navigate the treacherous waters between Scylla and Charybdis, a dangerous rock and a whirlpool, where there lived terrifying sirens—mortal danger either way the ship turns. The sirens lure unwary sailors to gruesome deaths with enchanting songs. Odysseus' sailors wore earplugs so as not to hear the siren songs, but he did not. He wanted—needed—to hear the singing. Fearing his own weakness, Odysseus had himself tied to the ship's mast. When Charlotte called, I felt, as he must have, the tightening bonds, the lurching ship, the unsettling nausea, the piercing rain, the roaring surf. The ship seemed close to breaking apart. I make out the sirens' voices amidst the roar of wind and surf. One siren sings in my mother's voice, and the other in my father's voice.

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It wasn't supposed to happen this way, but an ineffable light made the world come alive after Charlotte's call. Our French friends, reserved when we first arrived, now crowded our social schedule until they went away for their August vacations. In August, we visited places we had hoped to go when my mother's last illness called me to Boston. In Burgundy, we saw a roadside sign for the Château de Pommard and having never been on a winery tour in California, we decided to drive the long driveway to the Château. Monsieur le Comte de Pommard greeted us on our arrival; eyeing the yellow Mercedes, he inquired what had brought us to France. We were visiting École Polytechnique, I said, whereupon he said he had gone to "X" and his son was there now; perhaps we would like a personal tour of his winery? His vigneron, who had an impenetrable Burgundian accent, showed us unopened barrels of wine extending back to the Napoleonic era in his cellars. We bought two contemporary bottles out of courtesy, saying we would serve them at our farewell party for our friends. The wine stopped the conversational buzz; our guests, except for one (Guy Laval) simple scientists all, had heard of Château de Pommard but never had had any because you can only get it at the Château.

The TWA flight for the return to America featured loudmouth Americans shouting across the aisles at one another. Five-year-old Sarah announced that she would not speak French anymore since none of these people would understand her.

# 14

# Careers and Family in Precarious Balance

Debby Bochner, my wife, frequently claimed—sometimes in bitter resentment, sometimes in wistful resignation—that only one of us could prosper at a time. The trouble was that part of me, the preteen boy who abstracted himself in study when his mother was drinking her way to incoherence, feared his wife might be right. I proclaimed not to see it Debby's way, but I could not miss the fact that our careers and family lives were in precarious balance.

That balance was permanently and rudely altered on our return from Paris. Two years earlier, Judge Alfred Gitelson had ruled that the LA public schools must ensure racial equality in their assignment of children to schools by busing white children to minority schools until each had the same percentage of white students, at least averaged over the school year. In practical terms, this required white children to attend two or three different public schools during the year. In retrospect, Gitelson's arrogant presumption that physical proximity of affluent white students would transform the life prospects of underserved minority students was a trivial and shallow manifestation of what we now recognize as structural racism, but at the time the city was in turmoil because the authorities saw no way to ensure racial equality in their attendance statistics other than by busing. They did not want their statistics to be used in lawsuits.

Gitelson's ruling was eventually overturned, but not before it had permanently altered the course of Matthew's and Sarah's educations, and the lives of their parents. Within days of our return from Paris, I was in the office of the principal of Bellagio Road Elementary School, arguing to make permanent Matthew's grade acceleration that he had achieved at the École Internationale Bilingue. Bellagio's principal, knowing the high European education standards, went along easily. But he also informed us that his school was about to implement Judge Gitelson's order, which meant that Matthew would be admitted to Bellagio for only three months before being assigned to two other schools elsewhere in LA's vastness for the rest of the school year. The school system will provide bus transportation, he said, but since every school will be exchanging students, the bus system will be so overloaded that morning pickup times may be as early as 5:30 a.m. for an 8:30 a.m. start of classes. Parents who can do so may prefer to arrange carpools.

The family's return from Paris in early September made it almost too late to participate in the frenzy to find places in private schools. Rumors of available places traveled faster by word-of-mouth than they could be advertised. At this late remove I cannot recall all the possibilities that appeared and vanished, but suffice it to say that desperate parents from LA's West Side were placing their children in private schools in the San Fernando Valley—about forty minutes away in light traffic and twice that in rush hour. Eventually, a parent-designed ride sharing system required me to leave UCLA in the early afternoon to go to a point midway between the Valley and the West Side, where I would receive the relay of children from a Valley parent who was reluctant to go over the Santa Monica Mountains to the West Side. That was when I realized that not only my personal but also my professional life was hostage to the unaccountable vagaries of Los Angeles traffic. The children's school commute arrangements did not stabilize until Matthew was ready for the seventh grade and was admitted to the Harvard School in the nearer San Fernando Valley, and Sarah to its future sister school, the Westlake School. Even so, an unscheduled trip to the Valley meant professional disruption when the carpool routine failed.

The year in Paris gave the young Kennel family a coping option that not everyone else had: each of us had become used to living daily life in French. We found a wonderful Haitian woman, Fannie, who spoke only French, to be our LA Eugenie, and mundane family life would be carried out in French for the next five or so years. This meant additional weekly carpool duties for me because Debby panicked if Fannie arrived late on public transportation, so I drove

Fanny to and from her home to ensure domestic tranquility—hers and mine.

The French call the return from summer vacation *re-entry (La Rentrée)*. It is a distinct season of the year, much like Thanksgiving for Americans. The Kennel family's LA re-entry took several months, but eventually a routine was established, and Debby and I could think of returning to serious work. It took years for me to appreciate the significance of what then happened—another prostration event. One late afternoon, I found Debby riffling aimlessly through the boxes of notes she had carried home from the Bibliothèque Nationale. She was whispering to herself, dissociated, terrified; she no longer understood her notes. They were meaningless scribbles.

This was a turning point. Before, I could believe I was using my energies to unify career and family life, whereas now I understood that it would take all my energies to hold them together. I never dared ask if Debby made use of the research she had done in Paris. I had thought the sabbatical accomplished much of what I hoped for. Until that moment, I had no indication that my strategy for marriage stabilization in Paris had, despite appearances, failed. Debby and the children, with Eugenie, seemed to have found a balance that allowed all of us to progress. In fact, I thought I saw evidence that Debby was on a path to completion of her thesis. She had struck up a productive working relationship with the Director of the Institut de Recherche et d'Histoire des Textes. He even invited us to visit him at his summer home in Jonzac in southwest France. Debby and he did spend considerable time together on her thesis work on this visit, but he was also the rare humanist on the Board of the Centre National de la Recherche Scientifique, CNRS, the French National Science Foundation, and he wanted to spend part of a Sunday morning discussing science policy with me. I happily joined that discussion believing that propitiating him would support her, but a part of me worried that she would see it as my ambition again hogging the limelight.

I could no longer justify devoting time or resources to anything but career or children. I hardly ever talked with Debby about my own work for fear it would produce an anxious reaction—or worse, a dissociated one. I began to skip lunch at the Faculty Club because of the many afternoons I needed to leave for carpool duty; I had to use the time for students. I did not carry pocket money so as not to waste time in aimless browsing, but credit cards promoted sin; I left music stores itchy with guilt, apprehensive that I would be caught playing the recording I bought and wasting time that should have gone to the children. I escaped to my UCLA office after dinner, so that no one in the family would see me working. I tried to prepare my classes after everyone had gone to bed, and I missed sleep. There were Saturday afternoons I was so tired I fell asleep on the floor, guilty about escaping family responsibilities. Trips to professional meetings were occasions to lead an undivided daily life, but guilt pursued me.

In the meantime, there were children growing up whose needs could not be put off. Please do not think that part of it was a burden; some of my life's greatest moments were conversing with Matthew and Sarah when driving them to their activities. Matt's participation in the American Youth Soccer League did not work out because of a fanatical soccer father who mercilessly abused his son at practices. On the other hand, our trips to and from clarinet lessons with the Los Angeles Chamber Orchestra's gentle Gary Gray were more fulfilling. Matt had a lot of interests in scientific things, he did not need help in school, and I did not worry about him as much as I wish I had. On the other hand, remembering my mother and my wife, I was determined not to see Sarah fail. This primarily took the form of supporting Sarah's involvement with ballet. She had taken introductory ballet lessons in Paris, and we enrolled her in classes with the Los Angeles Ballet Company, thankfully located not far from home. That started a preoccupation that was to take up much of Sarah's and my time until she was a junior in high school.

I became a ballet father, sitting through classes with the ballet mothers. Sarah joined the Los Angeles Junior Ballet Company, which put on student performances at least once a year. I had the life privilege of working with its Director, Irina Kosmovska. Irina was the personal representative of the great George Balanchine to the Los Angeles ballet community; it was known that Irina was the conduit to Balanchine's New York City Ballet School and professional careers, the goal of most of the other parents sitting by. I was around the studio so much that I was elected President of Irina's Los Angeles Junior Ballet Company. Irina adapted for her junior charges the pieces introduced to European audiences in the early twentieth century by Serge Diaghilev's Russian Ballet and, later, in the 1920s by George Balanchine to American audiences. By the time I knew Irina, she lived only for dance. Dance may be beautiful to the audience out front, she would say, but at its core it is a mechanical craft embodied in automatic muscle memory. I stood backstage beside Irina during performances as she clapped out the rhythm, and her young dancers on stage repeated steps they had done hundreds of times before in a trance-like state, eyes glazed and absent. Later in her dance career, Sarah also performed with the West Side Ballet Company, especially at Christmas Nutcracker time. One of the West Side Ballet's two principals, Rosemary Valaire, had been trained in the Royal Ballet, so its performances were stylish architectural tableaux, while Irina's Russian pieces at the Los Angeles Junior Ballet appealed more to romantic sensibilities.

When the great Cuban ballerina, Alicia Alonso, or the famous Russian émigré dancer, Mikhail Baryshnikov, came to ballet class with Irina's students, we ballet groupies had something to talk about for a week. I got to know other fascinating personalities through Sarah's ballet, including Tatiana Riabouchinska, who had been one of the three so-called "baby ballerinas" of the Ballets Russes de Monte-Carlo of the 1930s. She had married another dancer, David Lichine, and was known to us as Tanya Lichine; her studio was in Beverly Hills. I never met anyone with flashier mental moves than Richard Feynman—well, actually, there was someone in Sarah's classes whose moves in ballet were as flashy as Feynman's in physics, Darci Kistler. Darci went directly to New York City Ballet to become the last dancer on whom Mr. Balanchine set an entire piece; when Darci retired, she was the last Balanchine dancer with the City Ballet. She ultimately married Mr. Balanchine's successor as City Ballet Director, Peter Martins. I got to see Darci at the peak of her powers when she was fourteen years old. Darci's high kicks were just a little faster than vision could follow, so that the memory of them exploded in your head. You wondered where they came from.

Sarah was involved in classical ballet from the first grade in France until her junior year at the Harvard-Westlake school, when she shifted to modern dance to make more time for academics. I did not share the obsessions of the other ballet parents and neither Sarah nor I regret that she left the dance. But by doing something so demanding so early in life, Sarah learned not to be deterred by difficulty. She grew up to be one of the most competent women I know.

The child-centered domestic life continued on its studiously conventional course until Matthew and Sarah were teenagers. Debby's and my efforts to hide our disjunction seemed surprisingly successful, and we thought nobody noticed. That wasn't entirely true; Debby's father noticed. Naomi had died, he had retired from

Princeton and was in Houston chairing the Mathematics Department at Rice University. This self-denying abstracted mathematician had become a consummate academic diplomat in his old age. My own father having not lived to age fifty, Bochner became my model of what it was like to live into old age. Manya never lost mental energy and he concentrated the philosophical thoughts of a lifetime into his last book, *The Role of Mathematics in the Rise of Science*. I picture him wishing he had learned at the feet of Aristotle in classical Greece.

Salomon Bochner had a full-time job in the Rice Mathematics Department into his eighties, and he would fly to Los Angeles to visit his daughter and grandchildren from time to time on weekends. Since Debby refused to waste time accompanying people to or from the airport, he and I were alone on one trip when he interrupted our casual chatter to burst out: *Please do not leave her!* Debby and I were in one of our calms between storms, and Manya's outburst came as a surprise. I allowed that we were working hard at the marriage but that I had no intention of leaving it. He chose to speak no more.

Manya Bochner was never to learn how his daughter's marriage would turn out. I had arranged to stay with him in Houston in 1982 for what was to be routine cataract surgery when he died following a heart attack on the operating table. There was more to his life than a transformation from abstracted mathematician in Princeton to consummate academic diplomat in Houston. A fortyish, handsome academic woman somehow heard of his passing and came to the hospital to weep alone by his deathbed. I let her weep in solitude. I never told Debby, who had remained in Los Angeles, about the mystery woman.

Debby's father was the one who preserved domestic tranquility all her life. From earliest childhood she had counted on his presence to counteract her mother's unimaginably destructive neuroses. Manya told me that when Naomi made life impossible, he would take himself to a nice lunch at Lahiere's Restaurant in Princeton, and then go home to praise Naomi's cooking to the skies. (There was nothing good to say about the cooking except that she cooked to disinfect; seeing the results, even that was questionable.) Manya's periods of mathematician's abstraction left Debby abandoned and disoriented and at the mercy of her mother's intricate obsessions and Debby's own unruly fantasy life. As for me, I could not imagine disappointing her father; I had yearned for his respect ever since our memorable first meeting. I took up the history of science as a way of signaling my respect for the social sciences to

Debby, but also to give me something to talk about with him. Bochner's death accelerated the slow process that was to transform my personal and academic lives. His outburst on the way to the airport may even have triggered it.

The child-centered life reached an apotheosis of a kind when I took Matthew and Sarah to Aspen, Colorado, for the summer of 1984; Matthew studied clarinet at the Aspen Music School, Sarah danced at the Aspen Ballet School, I did research at the Aspen Center for Physics, and Debby stayed in LA to work. As at Irina's Junior Ballet, you encountered young artists *before* they became famous; in Aspen, I heard the violinists Gil Shaham and Nadja Salerno-Sonnenberg before they rose to fame. (Shaham's father was a colleague at the Aspen Center for Physics.) Other young musicians would play impromptu chamber music on street corners after class; the town was magical. That summer at Aspen seemed to confirm the course we had been on.

The following year, 1985, Matthew received his letter of acceptance from Princeton; he and I went to a father-son event at somebody's Beverly Hills mansion organized by the Princeton Club of Los Angeles. When the hosts asked the newly accepted young teens what they wanted to do in life, Matt was the only one who said scientist; the large majority wanted to go into finance and, in LA, the performing media. Many brought guitars to the Princeton reception.

Sarah was in her junior year at Harvard-Westlake School when I took her to visit colleges in the East, culminating with a visit to Matt at Princeton. Westlake did not recommend Sarah for a formal interview at Princeton because her grades were mediocre. Instead, Matt asked a friend if she would take Sarah to class. On the airplane ride back to the West Coast, Sarah remarked that Princeton's was the most beautiful campus she had ever seen. *I want to go there*, she said, *even though Matt goes there*. Well, I said, there is a little problem: *your grades*. Her response amazed me. Oh, I can fix those. She could free up time for study by cutting back on classical ballet and enrolling in modern dance to stay in shape. She took a summer class in much-avoided chemistry, got an A, and never got even an A-minus again; she won an LA-wide prize for French and was admitted to Princeton, where she graduated *summa cum laude* with a prize for her undergraduate thesis.

Debby and I had accomplished the goal that the blind force of evolution sets for all parents—to raise children that can survive on their own—and we had attended to the most important requirement of membership in civilized society, to raise children who make productive contributions. Matthew and Sarah grew up to be ethical and accomplished human beings. Matthew got a PhD in physics from the University of California, San Diego, spent a postdoctoral year at the Oak Ridge National Laboratory in Tennessee, and returned to UCSD at its Institute of Nonlinear Science. Matthew is now a senior scientist with FICO, the analytics company that provides lending agencies with your credit rating. He lives near La Jolla, and I see him regularly. Sarah got a PhD in art history from the University of California, Berkeley, and became an Assistant Curator of Photography at the National Gallery of Art in Washington, DC, immediately after her degree. She is now Siskind Curator of Photography at the Virginia Museum of Fine Arts in Richmond, Virginia. She and husband, John Maggs, are raising two children, Ariel and Talia. John commutes periodically to Washington to pursue his career as a writer on economics with the Federal Reserve Bank, where my father had started his career.

Evolution was through with me.

I slowly lost faith that the course I had chosen would lead to PhD completion for Debby and marriage stabilization for us, or that the two were even related. One event shook my confidence to its foundations. Several years after our return from Paris, Debby was invited to give a paper on Renaissance history at a conference in Europe. I was delighted; maybe this was finally the breakthrough. While Fannie stayed home with the children, Debby worked in my UCLA office in the evenings during the summer before the conference to prepare her paper, while I sat nearby in the secretaries' bay to help with editing and minor scholarly logistics. To my shock and surprise, Debby was in a stupendous rage on her arrival at the LA airport after the conference. What had happened, I asked. Hers was voted the best junior researcher paper, she said, the only one to be included in the Proceedings. However, the conference organizers said that she needed to complete the footnotes. The rage persisted for about a month. She never finished the footnotes, and never did mail in an already accepted paper.

Did Debby think the success was not hers? Had I perversely been undermining? Did she fear she could not finish the footnotes in my presence? I began to believe our difficulties went beyond conscious and unconscious suppression of feminine autonomy by male hegemonic dominance, as her friends would unhelpfully suggest. I no longer had an explanation for Debby's inability to complete her work. Almost forty years on, I still do not have one, but I have gained sympathy for the mental world she lives in. An odd event—something in the news, a skinned knee, a bodily indisposition, a hostile landscape—could trigger a flood of improbable extrapolations that would carry her into unrecognizable psychological places.

Debby could not stabilize her perceptions. There were times when her surround was a jungle with untamable fantasies lurking in dark corners, their dangerous eyes looming bright wherever she looked. Primed by my mother's daily alcoholic journeys to incoherence and frozen into helplessness, I retreated into ritualized practicality when my wife got lost in her jungle of fantasies.

# 15

# Ground Down by the Paper Mill

I succumbed to recognition avarice after returning from Paris. I went on my first tour of duty with the National Research Council's Space Science Board (1977-1980), chairing its committee on space physics. I served as Vice-President Elect, Vice President, and President of the Division of Plasma Physics of the American Physical Society from 1987 to 1990, counting all the while the brownie points each would bring at National Academy election time. I supervised more graduate students and co-authored more papers while Chair of the UCLA Physics Department (1983-1986) than at any other time, a mistake for the students and for me. I violated my own instincts and employed students on topics that supported my grant objectives rather than their intellectual growth.

Students and postdocs were publishing so that I would not perish. More devoted to number of publications than to originality of content, I counted my publications each time I needed reassurance. A part of me hated this; the lengthening publication list could not appease the formless miasma that invaded at night. My father's half of the conscience implanted in me by my divided parents was excoriating me. By seeking social recognition more and intellectual originality less, I could not help but substitute conventional wisdom for original insight. My research group, my adventurous band of hunter-gathers, became a sedentary business operation, like a fast-food restaurant that occasionally offers something exotic to eat.

The paper mill did not stop grinding away because I had twinges of conscience. Dave Barbosa worked on plasma waves in the magnetosphere[28] and stayed at UCLA for the next ten years as a research associate, working largely with the UCLA spacecraft experimental group. John Engel[29] and Bill Livesey[30] assisted in the work that Maha Ashour-Abdalla and I were doing on electrostatic cyclotron harmonic instabilities. Both went on to jobs in industry. Pat Edmiston and Tohru Hada created computer-generated diagrams for a well-known paper that summarized what had been learned about collisionless shocks since Sagdeev and I worked on them in Trieste twenty years earlier.[31] Tohru went immediately into a tenure position at Kyushu University in Japan, and Pat chose to follow a career in industry. Pat and Tohru have written to me from time to time. I put Stanley Fujimura on pulsar winds,[32] my secret reach for redemption; insecure Stanley would wince when I guessed the answer to a problem eluding him. That, I thought, would not have happened had I not been so involved in pulsar physics myself; I was depriving him of the joy of discovery on his own problem so that I could make some progress on mine despite my other distractions. Stanley dropped out of UCLA in discouragement with both school and a personal relationship, but he finally finished.

Some people might think that it was I who recruited Maha Ashour-Abdalla to UCLA, but actually she recruited herself. Maha,

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[28] D. D. Barbosa, "On the Convective Properties of Magnetospheric Bernstein Waves," *Journal of Geophysical Research: Space Physics* 85, no. A5 (1980): 2341-45.

[29] J. Engel and C. F. Kennel, "Effect of Parallel Refraction on Magnetospheric Upper Hybrid Waves," *Geophysical Research Letters* 11, no. 9 (1984): 865-68; J. Engel and C. F. Kennel, "The Effects of Density Gradients on the Convective Amplification of Upper Hybrid Waves in the Magnetosphere," *Planetary and Space Science* 33, no. 11 (1985): 1331-57.

[30] M. Ashour-Abdalla, C. F. Kennel, and W. Livesey, "A Parametric Study of Electron Multiharmonic Instabilities in the Magnetosphere," *Journal of Geophysical Research: Space Physics* 84, vol. A11 (1979): 6540-46.

[31] C. F. Kennel, J. P. Edmiston, T. Hada, "A Quarter Century of Collisionless Shock Research," in *Collisionless Shocks in the Heliosphere: A Tutorial Review,* eds. R. G. Stone and B. T. Tsurutani (Washington DC: American Geophysical Union, 1985), 1-36.

[32] C. F. Kennel, F. S. Fujimura, R. Pellat, "Pulsar Magnetospheres," *Space Science Reviews* 24, no. 4 (197): 407-36; C. F. Kennel, F. S. Fujimura, and I. Okamoto, "Relativistic Magnetohydrodynamic Winds of Finite Temperature," *Geophysical & Astrophysical Fluid Dynamics* 26, no. 3-4 (1983): 147-222.

from a sophisticated Egyptian background, combined extraordinary determination with an elegant insouciance that won me over. She invited me to invite her to UCLA, but I went along because she could feed my addiction to brownie points. Maha hoped I would feed hers. I met her when I was visiting René Pellat at École Polytechnique in Paris in 1975. I also had a once-weekly consulting appointment with Roger Gendrin's group at the Center for Research on the Environment at Issy-les-Moulineaux in Paris' outskirts. I had been in Roger's laboratory no more than three hours before Maha made me the offer I could not refuse. I have, she said, a block of computer time that I would like to spend working space plasma problems with you. I had never worked with numerical computations, and she could do things that I could not, like use supercomputers.

I came to look forward to my weekly returns to Issy-Les-Moulineaux to see the magic Maha wrought with the machines she had tamed. I steered Maha into working out detailed solutions of the plasma wave modes first identified by my Princeton professor, Ira Bernstein.[33] Research by Vladimir Karpman in the USSR suggested that generating specialized analytic Bernstein Mode solutions was exceptionally burdensome, and I pushed Maha to work them out numerically for parameters pertinent to the Earth's radiation belts. She also became one of the first to simulate numerically the behavior of space plasmas, building on the pioneering work of UCLA's John Dawson for fusion plasmas and particle accelerators. Soon entire sessions at meetings would be devoted to numerical simulations of space plasma processes. I found them repetitive and boring, but Maha's audiences passionately followed every arcane detail of her work. I did not appreciate that small technicalities of computational technique could determine results, so I did not see why there were dozens of papers apparently on the same space physics issues at professional meetings. By force of personality and intellect she took my field in a direction I could not follow.

I had recruited to UCLA someone who brought alive the numerical methods that would challenge the centrality of the analytic techniques that made my career.

Maha was endowed with unfailing good humor and a human touch, as well as talent, and soon she led her own group on space plasma simulations. After she had built her reputation, Maha invited

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[33] I. B. Bernstein, "Waves in a Plasma in a Magnetic Field," *Physical Review* 109, no. 1 (1958): 10.

me to propose her for UCLA's tenured faculty, which happened in 1985. Our career directions would diverge after that; Maha went on to build an international school of numerical simulations of space plasmas. To me, numerical simulations were a visualization aide ill-adapted to conceptual innovation. Although the visionary part of me foresaw that they could become an important tool for discovery in space physics, the selfish part of me feared they would displace my way of doing things. My ambivalence marked a temporary stage in the development of space plasma physics. In 2016, at her memorial service, I recounted the Maha origin myth: How she and I met in in France and how she started space physics simulations at UCLA. I left my ambivalence out of it. Admirers from Russia, Germany, and Japan, as well as adoring former students and postdocs, had come to honor her memory. All saw how revered a teacher and inspirational a research leader she had become.

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**Figure 15-1. The UCLA space plasma group at its zenith, about 1985. We made this photo to commemorate a visit of my friend and mentor from our days in Trieste in 1965-1966, academician Roald Sagdeev, then Director of Moscow's Soviet Space Research Institute. Seated around the table are my student Pat Edmiston (left), Roald himself (center), and me (right). Standing in the rear to the left are my students, Tohru Hada, Dave Barbosa, and Bill Livesey. To the right standing in the back are two senior colleagues:**

**my alter ego, Ferd Coroniti, and my beguiling and charming colleague, Maha Ashour-Abdalla, whom I met in Paris in 1975. Word processing was just coming in, and we kept our research papers in bulky paper folders on the bookshelves behind us. Photo possibly taken by Melissa Licker, the long suffering and much loved secretary for the group.**

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The seed of redemption was planted in 1981 but did not bear fruit for until 1994. One quiet afternoon, an editor of the British Journal *Nature* called me in my UCLA office. Would I review the new book *Cosmic Plasma* by the only Nobel laureate in plasma physics, Hannes Alfvén? This editor tactlessly explained that he could get nobody better to do the review; he had contacted a few of Alfvén's contemporaries and they had all refused. Could I do it? Your experience was no surprise, I said to this editor. Alfvén didn't feature confirming someone else's ideas; Alfvén believes the fastest way to do something big in science is to attack received wisdom. Alfvén's arrogance put him on the outs with his contemporaries,[34] since they were the ones generating and receiving the wisdom, but I had heard that he was gentle with younger people. I, myself, have things to disagree with Alfvén about, I allowed, but I will do the review.

My review of *Cosmic Plasma* opined that there are in it some things that many will disagree with, and many things that some will disagree with, and even a few things that I disagree with, but there is one thing we have all learned: we ignore what Alfvén thinks at *our* peril. Alfvén was delighted with this review, and he called to invite Debby and me to visit him and Kirsten in La Jolla, the subtropical paradise they visited to escape Swedish winters. I prepared for the trip down the California coast as if it were a doctoral examination, preparing to dispute with the great man. It wasn't at all like that. Over a delightful lunch, he got to the point. How old are you, he asked. Forty-two. Alfvén went on, soon you will be called upon to work on the major problems facing international society. He spent the rest of a long afternoon speaking of the disarmament work of the Pugwash Conferences and, remarkably, of the new things being learned about climate change, 1981 being only two years after the

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[34] Alfven was most notable for attacking what has become the Standard Cosmological Model, then known as the Big Bang Model.

National Academy's Charney Report,[35] which put climate change on the world's policy agenda.

Shouldn't I prepare to use my talents to help end the Cold War, asked Alfvén, or combat global warming?

Alfvén's remonstrance still taunts me. Twice I had occasions to tell of my encounter with the only person ever to win a Nobel Prize for plasma physics. The first was when I was awarded the first Hannes Alfvén Medal by the European Geophysical Society in 1997, and the second, a commencement talk to the UCLA Division of Physical Sciences in 2015.

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[35] J. G. Charney, A. Arakawa, D. J. Baker, B. Bolin, R. E. Dickinson, R. M. Goody, C. E. Leith, H. M. Stommel, and C. I. Wunsch, *Carbon Dioxide and Climate: A Scientific Assessment* (Washington, DC: National Academy of Sciences, 1979).

# 16

# Gravitational Waves, Galactic Winds,and Pulsar Magnetospheres

I did not tell many people about my secret passion for astrophysics that just might produce redemption. It was a way to keep faith with the brilliant young physicists who had been in Eugene Wigner's class at Princeton. I was not part of an active group in the field, and PhD thesis topics for students did not suggest themselves quickly. It took years even to formulate problems I could work on, and years before I would reach the research level in a field that I abandoned once I had done so.

I saw my first opportunity to keep faith with my Princeton heritage by applying plasma techniques to Einstein's General Theory of Relativity in the late 1970s. The great Soviet physicist, Lev Landau, who co-wrote the wondrous books that taught me physics, had invented the signature process in the theory of plasma waves, Landau damping. I set Steven Gayer to work out the attenuation by Landau damping of the gravitational waves predicted by general relativity.[36] Maybe plasma physics could earn some respect in the basic physics community.

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[36] S. Gayer and C. F. Kennel, "Possibility of Landau Damping of Gravitational Waves," *Physical Review D* 19, no. 4 (1979): 1070.

Gravitational waves had never been observed, and physicists would debate how to detect them for decades.[37] It took forty years until gravitational waves were detected, and only then did Steve's and my paper move the prestige needle, too late to help Steve in his career. Gravitational waves generated by the rare but spectacular collisions of neutron stars and/or black holes have recently been detected coming from enormous distances. If gravitational waves propagate without being damped, they can reach the earth from any distance. On the other hand, gravitational waves can be damped if there is matter between the source and the earth. The Landau damping by intervening galaxies or dark matter became interesting to other physicists when it became possible to detect gravitational waves.

In 2020, Gordon Baym, an eminent physics colleague, inquired out of the blue whether I remembered my 1979 paper with Steve Gayer. Did I know it was the earliest paper Gordon could find on the topic? What scientist would not be gratified to learn that he and a student had written the first words on a topic that had lately become prominent? Was I justified at the time, however, in assigning Steve Gayer a topic with no community researching it to hire him—a topic that I had neither vision nor opportunity to pursue again? When does society benefit more, when an already active research community is further stimulated, or when a seed is planted that sprouts when your contemporaries are dead?

Another student, Steven Smith, wrote an astrophysics thesis with minimal guidance from me on active galactic nuclei,[38] a topic that did have a large research community pursuing it in the 1970s and 1980s. Steve connected with Ferd Coroniti, who developed an active interest in the subject and became Steve's primary advisor. This three-way collaboration, in which I was the least active, educated me enough that later I would be unafraid to meet with Cambridge's Martin Rees, the world's leading authority on active galactic nuclei.[39] This became a deep friendship that has sustained

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[37] C. F. Kennel, "The LIGO Controversy," *Science* 260, no. 5116 (1993): 1862.

[38] S. J. Smith, C. F. Kennel, and F. V. Coroniti, "Nuclear Winds in Active Elliptical Galaxies. I-Interaction," *The Astrophysical Journal* 411 (1993): 581-93; S. J. Smith, C. F. Kennel, and F. V. Coroniti, "Nuclear Winds in Active Elliptical Galaxies, II-Observational Signatures," *The Astrophysical Journal* 412 (1993):82-89

[39] I dreamed of meeting Martin Rees because of his 1974 work on pulsars with James Gunn.

my later years in important ways. In his own later years, Steve Smith turned his independent mind to climate change, where we would meet again.

Now I had come to the work that I thought stood a chance to earn me the respect of the students in Eugene Wigner's class at Princeton. After seventeen years of effort that missed the mark, my work on pulsar winds culminated in the second most highly cited publication of my career. With one exception, Stanley Fujimura, I did not involve students in it. Pulsars were not a problem where the application of plasma theory to a new plasma environment could yield a publishable result on the time scale of a PhD thesis. It was not even clear at the beginning that pulsars were a problem amenable to the kind of plasma physics I knew. The astronomy community had to work out the basics of pulsar phenomenology, and I had to teach myself relativistic plasma physics before I could apply my knowledge to pulsar winds. It took seventeen years to make pulsar physics "plasma ready" for me.

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The discovery of pulsars in 1968 was a science fiction writer's dream.[40] Anthony Hewish and his student Jocelyn Bell[41] had rigged the then new Mullard Radio Telescope in Cambridge, England, to look for time variable signals from the cosmos. The student, Bell, looking for something else, spotted in the noise a source of mysterious radio signals that repeated precisely every 1.3 seconds. Nothing in nature had ever been so precise; Bell herself playfully speculated that the signals were from "little green men." Bell and others soon found several other pulsing radio stars—pulsars—coming from our galaxy.

It did not take long for Tommy Gold, the Cornell astrophysicist who invited me to give my first academic seminar and to attend Richard Feynman's first Messenger Lecture, to argue that the radio signals were organized like the rotating beams of light from a lighthouse. No astronomical object acting as lighthouse could complete a rotation in a few seconds unless it were so compact and dense that its gravity could hold it together against the immense

[40] A. Hewish, S. J. Bell, J. D. H. Pilkington, P. F. Scott, and R. A. Collins "Observation of a Rapidly Pulsating Radio Source," *Nature* 217 (1968): 709-13.
[41] Later Dame Jocelyn Bell Burnell.

centrifugal force of rotation.[42] The central object holding the radio transmitter had to be a huge, super dense, quasi-atomic nucleus with the mass of an entire star—a neutron star. Gold's brief paper made it evident that the discovery of pulsars announced a new era in astronomy.

Bell's story has become a *cause celebre* in the world of science: she made the original discovery yet did not share in the Nobel Prize awarded to her male thesis advisor, Anthony Hewish.

I had learned in Martin Schwarzschild's class at Princeton that stars close to running out of nuclear fuel are liable to explode. Some stellar explosions generate so intense a light that a new star, a supernova, appears temporarily in the sky. Occasionally, a supernova is so bright that people on earth can see it without a telescope. I told the students in my history of physics class how excited European astronomers had been about the supernovae of 1572 and 1603, because they proved there were more things than dreamt of in Aristotle's philosophy. The nuclear physicists of 1968 knew all about supernovae because they had built small versions of them during World War II—atomic bombs. They went on to build bigger hydrogen bombs during the Cold War. My Trieste mentor, Marshall Rosenbluth, made a critical contribution to the development of the hydrogen bomb. The world had stopped testing H-bombs in the atmosphere in 1963, but by no means had physicists stopped thinking about them. They discussed weapons in hushed tones among themselves and disguised the topic by referring to supernovae. It seemed fairly transparent to this graduate student.

The physics of a supernova explosion only gets complicated when you try to make one yourself. The outer layers of an exploding star are blown outwards; the light generated in the expanding shell of hot plasma is what looks like a new star in the sky. The inner core of the star, on the other hand, collapses inward—implodes—just as in a bomb. The implosion will not stop until a solid neutron star forms at the center. If that doesn't happen because there is too much matter falling in, a black hole is formed. Neutron stars are essentially giant atomic nuclei some ten kilometers in diameter, nineteen orders of magnitude larger than an atomic nucleus. Neutron stars had existed on paper since 1939 when J. Robert Oppenheimer, as much as anyone the father of the atomic bomb, wrote the foundational paper in

[42] T. Gold, "Rotating Neutron Stars and the Nature of Pulsars," *Nature* 221 (1969): 25-27.

1939.[43] Jocelyn Bell's 1968 discovery of pulsars meant neutron stars exist in real life, not only in Oppenheimer's obscure twenty-eight-year-old paper.

European observers left no record of the supernova of 1054 in the Crab Nebula, but Chinese and Arab astronomers did. It was visible to the naked eye *during the day* for twenty-three days, and at night for two years. European astronomers did pick up the diffuse "nebulous" light from the expanding supernova remnant eight centuries later. From the eighteenth century CE forward, the Crab Nebula was never far from the minds of professional astronomers. Charles Messier's famous 1774 catalog of diffuse astronomical objects lists the Crab Nebula as M1. Twentieth-century astronomers, noting that the supernova remnant was expanding, traced the expansion backward in time to the 1054 supernova in Chinese records. The rapid variations in the structure of the nebula were particularly puzzling; there had to be a power source near the center of the nebula that was stirring things up. Tommy Gold identified that source as a rapidly rotating neutron star.

The international astronomy community put together what became the standard model of the Crab Pulsar and Nebula within a year after Bell's discovery of pulsars. The Italian astronomer, Franco Pacini, argued that the 1054 supernova must have created a pulsar,[44] and in the same year, Richard Lovelace found Pacini's radio pulsar using the Arecibo Radio Telescope in Puerto Rico.[45] This association raised pulsars to an entirely new level of significance. The Crab Pulsar's parent neutron star was rotating not once per second, as Jocelyn Bell's had, but thirty times per second. Subsequent observations showed that virtually the whole range of electromagnetic wavelengths open to astronomical observation was pulsed at thirty-three times per second, from low energy radio waves to optical light to the high energy gamma rays. Not only that, but the period between pulses was increasing, which meant the rotation of the parent neutron star was slowing down; the pulsar's rotational

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[43] J. R. Oppenheimer and G. M. Volkoff, "On Massive Neutron Cores," *Physical Review* 55, no. 4 (1939): 374.

[44] F. Pacini, "Rotating Neutron Stars, Pulsars and Supernova Remnants," *Nature* 219 (1968): 145-46

[45] R. V. E. Lovelace, J. M. Sutton, and H. D. Craft, *IAU Circ. No. 2113* (November 18, 1968); R. V. E. Lovelace and G. L. Tyler, "On the Discovery of the Period of the Crab Nebular Pulsar," *The Observatory* 132 (2012): 186-88.

energy was being delivered to the nebula. There were only educated guesses about what form the energy flowing from the neutron star to the nebula would take.

The plasma physics action became fast and furious. The imploding portion of the supernova plasma would carry the precursor star's magnetic field directly into the neutron star, compressing it to the point where the magnetic force became as strong as the nuclear force that holds atomic nuclei together. The atoms in the outer layers of neutron stars are cigar shaped because of the super strong magnetic force, but deeper in neutron star interiors the neutron density is so great that cigar atoms are squeezed together into a soup of pure neutrons. You can think of a magnetized neutron star as almost entirely a super dense rotating liquid ball with a bar magnet in its interior. There is no reason why the implosion would leave the bar magnet and the star's axis of rotation aligned; misaligned, a rotating magnet in vacuum would radiate an enormously powerful electromagnetic wave. The energy carried away by the wave would be equal to the rotational energy being lost by the pulsar. The wave could be so strong that it could accelerate high energy cosmic ray particles.

All in all, pulsars were as much a problem in electrodynamics as in nuclear physics, and, moreover, they had magnetospheres as in space physics.[46] My future friend and colleague Martin Rees (and James Gunn) made a proposal that ultimately proved to be the one closest to today's thinking. Rather than a wave, the pulsar emits a highly energetic version of the solar wind that interacts with the surrounding supernova shell, much as the solar wind interacts with the interstellar medium. This was incredibly exciting: I knew enough to do something interesting in one of the most glamorous of all topics in high energy astrophysics!

But first, the field had to decide whether the Crab Pulsar created a super strong plasma wind or a super strong plasma wave. In either case, you could be sure of one thing: the energies involved were so large that either the wind or the wave would require the use of Einstein's Theory of Relativity. The theory of relativistic plasmas was an arcane topic worked on because it was needed to design high energy particle accelerators and hydrogen bombs. The UCLA Physics Department did not teach it in its plasma physics curriculum. The

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[46] P. Goldreich and W. H. Julian, "Pulsar Electrodynamics," *The Astrophysical Journal* 157 (1969): 869.

challenge for me was to master relativistic plasma physics, apply it to plasma winds and plasma waves, and figure out which was the right model to apply to the Crab Pulsar.

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Tom Wilcox, from the UCLA high energy physics group, George Schmidt, a visiting plasma physicist from the Stevens Institute of Technology, and I, made a first try at mastering the theory of super relativistic plasma waves in 1973.[47] We worked out a nonlinear theory of an electromagnetic wave so strong that both the ions and electrons comprising the plasma in which the wave propagates are driven to super relativistic energies. Our basic conclusion was easy to state but not easy to reach. There is a critical plasma density below which such a wave will propagate and above which it will not. This result made me focus on the question of the plasma density in the Crab Nebula.

I should also have asked what the plasma is made of. Mal Ruderman[48] and Peter Sutherland of Columbia University guided me to answers to both questions.[49] Their reasoning was remarkable for following a problem from beginning to end in the same paper. A pulsar can be thought of a rotating magnet; a rotating super strong magnet generates a super strong electric field; that electric field literally rips iron ions out of the solid surface of the neutron star and accelerates them to super relativistic energies along the pulsar's magnetic field lines into space; the ions are so energetic that they emit photons of light as they go around the curves in the pulsar's magnetic field; those photons are themselves so energetic that they create electron positron pairs out of the vacuum, a process I learned about in Eugene Wigner's class at Princeton (a positron is identical to an electron, except that it has a positive rather than negative charge). This suggested to me that the Crab's plasma could be predominantly positronic. I did not know whether the positron

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[47] C. F. Kennel, G. Schmidt, and T. Wilcox, "Cosmic-Ray Generation by Pulsars," *Physical Review Letters* 31, no. 22 (1973): 1364.

[48] Mal became a personal friend. In our later years, we would meet summers when Mal would visit La Jolla from New York to attend the meetings of the defense consulting group, the Jasons, doing what well-known physicists do.

[49] M. A. Ruderman and P. G. Sutherland, "Theory of Pulsars: Polar Caps, Sparks, and Coherent Microwave Radiation," *The Astrophysical Journal* 196 (1975): 51-72.

density was above or below that for a super strong wave to propagate, but it made me take the wind option seriously.

I also had to come to grips with the physics of super strong electromagnetic waves in positronic plasmas. During my sabbatical year in Paris at the École Polytechnique, René Pellat and I worked out sample solutions for a variety of super relativistic plasma waves. One solution was in closed form even for a wave of arbitrary amplitude.[50]

The other task was to master the physics of highly relativistic stellar winds. The task was again simple to state, but there being no numerical computations, difficult to carry out. I needed to apply relativistic magnetohydrodynamics to Eugene Parker's theory of the solar wind.[51] I asked my student Stanley Fujimura and Isao Okamoto, visiting UCLA from Sendai University in Japan, to join me in a long paper on relativistic winds that ultimately appeared in 1983.[52] This mathematical exercise sharpened the tool that I would have to use to make sense out of the observations of the Crab Pulsar and Nebula. In the meantime, Stanley Fujimura, René Pellat, and I summarized the status of the wind vs. wave question in a 1979 review article.[53]

Theoretical machinery in astrophysics has little value unless it explains observations. It was time to ask the important question; if I put together my best guesses (wind not wave; positronic plasma; a simplified Parker model of relativistic winds; termination in a shock wave when the relativistic wind hits the expanding supernova remnant), can I explain the observed profile of light emissions in the wind zone? In other words, can a relativistic positronic wind with the energy flux and magnetic field expected of the Crab Pulsar explain the strength and spatial profile of the Crab Pulsar's light emissions? I started to work on this problem alone, but I had a new tool, a Texas Instruments handheld programmable calculator. It would take all night for one computation to run; morning after morning, the pattern would slowly emerge. The seemly endless repetition of almost identical calculations was almost too much to bear.

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[50] C. F. Kennel and R. Pellat, "Relativistic Nonlinear Plasma Waves in a Magnetic Field," *Journal of Plasma Physics* 15, no. 3 (1976): 335-55.

[51] E.N. Parker, "Cosmic Ray Modulation by the Solar Wind," *Phys. Rev. 110* (1958): 1445.

[52] C. F. Kennel, F. S. Fujimura, and I. Okamoto, "Relativistic Magnetohydrodynamic Winds of Finite Temperature," *Geophysical & Astrophysical Fluid Dynamics* 26, no. 3-4 (1983): 147-222.

[53] C. F. Kennel, F. S. Fujimura, and R. Pellat, "Pulsar Magnetospheres," *Space Science Reviews* 24, no. 4 (1979): 407-36.

I would have found motivation, inspiration, and confidence flagging had I not asked Ferd Coroniti to join me in the pulsar work, hoping to rejuvenate the working relationship we had had when we first came to UCLA. That was not in the cards. Ferd's wife, Pat, was afflicted with the autoimmune disease that has tied her to a wheelchair and a breathing apparatus for the last forty years until she died in 2022. Ferd's devotion to Pat never lapsed and he carried on his career with unusual grace. I weep to think what he might have done in science had he not been so burdened. As it was, he was, I think, the best chair the UCLA Physics Department has had since I have been associated with it. Ferd Coroniti brought to our pulsar research his unusual skills at close analysis, and together we brought the work to completion. We were even able to account for light emissions created when the positronic plasma wind collides with the surrounding supernova remnant. Our results[54] achieved a better match with observations than anyone had before, but of course have been superseded by now.

The publication of our two Crab Nebula papers in 1984 turned out to be a pivotal point in my life, but not for the reasons you would think. A few days after I put the papers in the mail to be published, Debby inquired about what I had been working on. Normally I never talked about work and would deflect the question, but this time, I allowed that Ferd and I had just completed two very good papers. I even said they could be as good as the one I had completed with Harry Petschek when Debby and I were engaged. For the next two months I was subject to verbal tirades more intense and obsessive than any I had experienced in the marriage. I wondered what I had done. Finally, it occurred to me that I had radiated the deep satisfaction the pulsar work had given me. That must be what did it, I thought. Debby often said that only one of us could prosper at a time; the years I was working on the Crab Pulsar were years Debby was not completing her thesis.

I never worked on the Crab Nebula again. In later life, I had occasions to learn from other senior scientists how they made their discoveries. I had worked on the Crab Nebula as they had, patiently assembling evidence and mastering technique until an answer came

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[54] C. F. Kennel and F. Coroniti, "Magnetohydrodynamic Model of Crab Nebula Radiation," *The Astrophysical Journal* 283 (1984): 710-30; C. F. Kennel and F. V. Coroniti, "Confinement of the Crab Pulsar's Wind by its Supernova Remnant," *The Astrophysical Journal* 283 (1984): 694-709.

into view. Debby's reaction, or maybe my interpretation of it, had completely shut down all my interest in the Crab Nebula. Ferd carried on and put our work on much more solid footing.[55] Twenty-five years later Fiona Harrison, a younger colleague from Caltech, asked me whether I knew my 1984 papers with Ferd had become classics in her field. I replied I had no idea what happened to them.

[55] F. V. Coroniti, "Magnetically Striped Relativistic Magnetohydrodynamic Winds: The Crab Nebula Revisited," *The Astrophysical Journal*, 349 (1990): 538-45; F. V. Coroniti, "The Crab Pulsar and Relativistic Wind," *The Astrophysical Journal* 850, no. 2 (2017): 184; F. V. Coroniti, "The Crab Pulsar: Origin of the Crab Nebula's Radio Pairs," *The Astrophysical Journal* 871, no. 2 (2019): 252.

# 17

# Erikson's Midlife Crisis Arrives

Erik Erikson's group at Harvard had stimulated a premature adolescent identity crisis in me, and I was destined to endure Erikson's midlife crisis, too. He argued that fundamental psychological development occurs not only in early childhood, as Freud did, but in stages throughout life. Adult development is not a genetically designed response to universal challenges in life development, as it is in infancy, but there is a predictable sequence of psychosocial crises in the lives of adults in twentieth-century Western culture. Whether you thrive in adult life depends on how you deal with each crisis. Not everyone moves beyond crisis; I was seeing colleagues frozen in intellectual and emotional development, unable to progress beyond their midlife crises.

Divorce, which the law calls an event, is really a process. Years before the legal event, I comprehended that my strategy of freeing Debby to realize her professional potential was not working. It was arguably the wrong thing to do. Though the periods of pain were intermittent, they continued, and my misgivings slowly crystallized into intent. The midlife crisis the misgivings foretold materialized in 1984 and 1985. That which seemed alien at age nineteen when I learned of it from Erikson at Harvard arrived on schedule at age forty-five in Los Angeles. I could count many reasons to find joy in living, yet corrosive dissatisfaction was eating away at life's foundations. Neither before nor since have I felt such dissonance between inner and outer life.

The 1984 Olympics marked a high point for quality of life in Los Angeles. LA may have lost some of its 1967 off-beat charm, but it had acquired a new cultural sophistication, too young to be world weary. You could still find off-beat charm whenever you wanted, but now you could also find an impressive center for the performing arts downtown where—you guessed it—the Santa Monica, San Bernardino, Golden State, and Pasadena Freeways converge. The John Paul Getty Museum, blessed with an enormous endowment, had to be careful not to trample on its competitors by overpaying when adding to its collections of old master paintings and photographs. The Los Angeles County Museum of Art became a home for works created in Southern California.

Legendary musicians had chosen to emigrate to LA from Hitler's Europe—composer Arnold Schoenberg, violinist Jascha Heifetz, and cellist Gregor Piatigorsky, among many others, lived near us, and their presence internationalized the city's classical musical life. The charismatic Zubin Mehta conducted the LA Philharmonic before becoming successor to the comparably charismatic Leonard Bernstein at the New York Philharmonic. Movies and television continued to have a pervasive influence on LA cultural life, as they had done since the beginning of the twentieth century, but you did not have to go to the movies or watch TV to enjoy the benefits. Talented performers could support their classical art habits by working part time for the commercial media. The Los Angeles Chamber Orchestra, founded in 1968 by the English conductor Neville Marriner, featured instrumentalists the equal of the LA Philharmonic—and, as its aficionados liked to say, of any orchestra anywhere. Local church choirs presented major choral works. If you lived in Southern California, you did not have to go to New York or London for good theater; the town was crawling with talented actors and actresses. High-quality performances at ninety-nine-seat neighborhood playhouses provided new performers and plays with friendly local audiences, yet managed to keep the loyalty of the tired businessman with popular offerings.

You did not have to be a cultural elitist to enjoy LA's golden age. The joys of eating out had progressed beyond fresh salads. Restaurants no longer featured only simple American food, though hamburgers had reached a new level of rococo elaboration; you did not have to go far to find French, Italian, Chinese, Japanese, Korean, Thai, even Ethiopian cooking; culinary *cognoscenti* enthused about a new California fusion cuisine. Yet withal, the ease of ordinary life that attracted so many servicemen to Southern California after the war was still there. The easy living also attracted academics, and my colleagues commuted to UCLA on uncrowded freeways from a variety of distinctive little communities in the vast Los Angeles basin. They were free to choose a lifestyle and did; real estate was destiny. Debby and I had been less creative; we lived in nearby Brentwood, a version of the proper Eastern towns we had grown up in—Newton, Massachusetts, and Princeton, New Jersey—but with palm trees.

How LA organized the 1984 Olympics presaged a different future. The Olympics were designed around the freeway system, LA's signature contribution to modern urban life. Olympic sports venues were placed miles apart at existing facilities to avoid the expense of new construction, and because of this the LA Olympics finished with a financial surplus whereas other Olympic cities had spent billions. The LA authorities, worried about the growth in traffic from the huge number of Olympic visitors, urged residents to take their vacations during the Olympics. That was when I took the children to our glorious summer in Aspen. Enough others did as well, and traffic volume was reduced by five to ten percent, enough to unlock traffic jams. The newspapers reported later that visitors and stay-at-homes had had a surprisingly pleasant time of it.

Later, the LA basin would suffer from progressive traffic seizure dystopia (PTSD). A traffic seizure could happen any time, even at three o'clock in the morning. You had to allow extra time to get to the airport because a traffic seizure might happen. LA's PTSD was isolating people and disconnecting communities. Los Angeles' Golden Age would end in a cacophony of car horns.

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It was in this high point of Los Angeles' Golden Age that I first heard the low voice. Matthew had received Princeton's letter of acceptance. The voice was whispering *your job is done, your job is done, your job is done*. That wasn't entirely true; Sarah had two more years until she, too, would be accepted by Princeton, but the low voice kept intruding when I least expected it. The voice was there when I went to lunch with my Pittsburgh friend, Ellen Lehman, which I used to do about once a year. She had completed her PhD in child development at Cornell, and by 1984 she progressed to Therapist and Training Analyst in a private practice in Santa Monica. Our lunches were moments of respite for me. It was not taboo to talk about work, but I never discussed what was going on in my marriage; Ellen told me later she never suspected.

Ellen related how she very nearly bled out from a hemorrhage on a flight home from a professional meeting. She had been within minutes of dying when a colleague rushed her to the emergency room after they landed. Hearing of Ellen's close call stirred up feelings first aroused at her family's New Year's Eve party twenty-two years earlier in Pittsburgh, when we had stayed up until 3 a.m. in passionate, if chaste, conversation. The low voice whispered of the toll taken by fourteen years of celibacy. For me, marriage had been the graveyard of passion. I did not know but suspected that most marriages turned celibate after a while, their passion smothered by obligation and routine. I strayed only once; I had a brief one-month affair with my college girlfriend, Penny Post, but that was it for the fourteen years. Otherwise, I had been scrupulously faithful. Nonetheless, the helpless yearnings for women I met at parties would take weeks to suppress.

It was then or never, and Ellen and I started our affair. There followed months of dither, in which I took long early morning walks hoping to puzzle out what to do. Before Debby and I were married, I entertained a romantic fantasy of taking a postdoctoral fellowship in Brazil, but I had been shanghaied into marriage by Debby's mother, and my Brazil fantasy never stood a chance. I had recently applied for and won a Fulbright fellowship to spend three weeks visiting space research laboratories in São José dos Campos, São Paolo, Rio De Janeiro, and Brasília; it was to be my summer vacation. Debby did not want to accompany me even on the interesting trips. This trip looked like a discreet opportunity for Ellen and me to be together, and we coordinated travel plans.

Debby finally figured out what was happening, and on Bastille Day, July 14, 1985, as I was leaving for another morning dither walk, she ordered me not to come home. Ellen took me in. We spent our first full day together at a pool party in Mandeville Canyon in West LA. I had forgotten there were people who did not spend every minute of every day on obligatory tasks. Those people around the pool were taking time off. They were enjoying the California sunshine.

Separation from Debby was harder than telling Mother that she had only a few months to live. It took years for Matthew and Sarah to accept the separation, and it caused a temporary emotional meltdown in Debby. About a week after the pool party, Matthew and Sarah called to say that their mother was having a breakdown and I had to look after her. I arrived at my old home in Brentwood to take her to the UCLA Neurosciences Institute. At least three physicians there asked whether I would reconsider the separation, but with hardened clarity, I replied that I had no intention of going back to the marriage. Three weeks later, Ellen and I left separately for Brazil. When I arrived in our hotel room after an eleven-hour flight from Los Angeles, I threw my luggage and myself down on the hard hotel bed. The impact herniated the L5 disc in my lower back, and I hobbled around in intense pain for the next three weeks in Brazil and three months in Los Angeles. Even then I saw the physical pain as an externalization of emotional stress.

I gave my Fulbright lectures on space plasma physics in São José dos Campos, Rio de Janeiro, and Brasília while hobbling about on the bad back.[56] The scientists at the National Space Research Institute in São José dos Campos (INPE) also hoped I could help them with another problem. They showed me photographs taken from high-flying NASA airplanes documenting the destruction of the Brazilian rainforest; it was clear as day: roads cut into virgin forest create a herringbone pattern of deforestation when seen from above. The INPE forest scientists pleaded with me; couldn't I do something? They were afraid that governments around the world would not believe their estimates of the losses, which were smaller than the highly publicized estimates made by international advocacy organizations. The Brazilian scientists did not want to be accused of lack of objectivity.

Little did I know then that I would return to São José dos Campos a few years later to negotiate with a Brazilian Air Force general the access rights for a NASA Blackbird airplane to photograph the rainforest from high in the stratosphere. I did not suspect that the few hours spent with INPE's rainforest scientists marked the moment when my scientific focus began its shift away from space physics and towards climate change. That trip to Brazil also presaged a major change in life's texture; it was the first of many that Ellen and I were to take together. Many trips were to have a common theme besides science—the cultures and arts of indigenous people—in

[56] Years later the memory of it enabled me to sympathize with Walter Munk when he hobbled into my Scripps office with a failed knee replacement.

Brazil, Alaska, New Mexico, California, Australia, Canada's Northwest Territory, and Baffin Island.

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I am fortunate in many ways in the choice of Ellen Lehman, but at this point in the narrative, I will mention only one of the ways. Ellen is a psychologist. She believes in working things out by talking things through. Ellen's is a non-doctrinaire branch of psychology, self-psychology, which leverages emotional change through inter-subjective interactions in therapy about the specifics of the patient's circumstances and history. She recommended a colleague, Mel Schwartz, to help me find the path to the other side of my midlife crisis. Mel and I worked together for the next seven years until I left UCLA for NASA, and space physics for earth and climate science. The pathway to adult crisis resolution is history dependent, and serious deviltry *and* serious opportunity hide in the details. At first, my therapy's preoccupation was crisis intervention—pain relief—but the focus shifted to figuring out what kind of person I could become in the next phase of life. I will not speak much about how the transformation took place; in fact, I can't. One feature of successful therapy is that the patient cannot remember what life was like before the therapy began, nor can the patient articulate the psychological changes that took place, so internalized had they become. While I see now that my personal and professional lives evolved at their own paces, the changes in each started when I started therapy.

A physicist might put it this way: the theory that had structured my emotional life for the past twenty years had just failed and the void beckoned again. The first task was to repopulate that void with a new set of facts and experiences from which a new identity could be fabricated.

It all started with little things. The texture of daily life does matter. Not all an intimate relationship's time need be spent on goals that are presumptively consonant but separately pursued. Easing the immediacy of the guilt about my conscious and unconscious failures in marriage freed me to participate in day-to-day life. I carried money. I began to buy things, starting with a three-dollar coffee mug for Ellen. We went to a favorite little restaurant in Pacific Palisades. I did not have to commit every minute to purposeful activity; I could watch UCLA's great basketball teams on TV without being accused of boorishness or selfishness; it was not selfish to take naps, not neglectful to take a Saturday off. Your friends need not occasion your partner's obsessive criticism. Joint friendships might even be possible.

Ellen gave me a new suitcase to say it was okay to travel to professional meetings. I could even go places for the sheer aimless pleasure of it. Ellen and I were soon exploring the California desert, the Sequoias, Santa Barbara, and the Central Coast. Like Freud, Ellen had a deep interest in indigenous native cultures, and our travel repertoire expanded beyond Brazil to the American Southwest, Australia, and our two most frequent destinations, Alaska and the Canadian Arctic. My own interest in native arts was awakened during our surreptitious trip to Brazil, where we bought the Yanomani feather headdress that still hangs on our walls.[57] As we settled into our new life, we made frequent trips to New Mexico, where I was involved in a project at the Los Alamos National Laboratory; we visited pueblos together, notably Santa Clara, where we bought pottery from the artists. We got to know several, including Ray Tafoya, who, had he not died young, could have become as famous as the Santo Domingo potter Maria Martinez. Ellen and I haunted museums and galleries in Santa Fe and acquired bronze sculptures by the Native American artist Allan Hauser. We also could visit *my* family. We visited my brother John and his wife, Heike, in Königstein, near Frankfurt, Germany; after they left Königstein to celebrate Christmas with Heike's family near Hamburg, Ellen and I drove along the Moselle River to the old Roman town of Trier, keeping warm with *glühwein* at Christmas markets along the way.

Life had a new unity. Ellen tolerated, even welcomed, my professional friends. She invited René Pellat to stay with us in her house in Pacific Palisades when he visited UCLA, which he did more and more frequently until he got a tenured part-time professorship in the Physics Department. Ellen laughed at René's distinctive habits, and they became close friends.

After my term as Chair of the Physics Department was over and Debby and I separated, I cared less about number of published papers and more about seeing creativity flourish around me. Harry Petschek's work on Alfvén waves in complex flowing plasma configurations and Roald Sagdeev's on nonlinear waves were inspirations for what happened next. I had reached a stage in my

[57] An art dealer told us that another physicist, from Caltech (Nobel laureate Murray Gell-Mann), had a similar piece. That made it extra special.

career where showing enthusiasm was enough to start a local research mini-boom, and a group of graduate students, postdoctoral researchers, and visitors interested in linear and nonlinear Alfvén wave physics accreted around me in the late 1980s. My students, Tohru Hada and Bob Hamilton,[58] and a visitor from Tromsø in Norway, Einar Mjølhus, coined the affectionate term "Alfvén Wave Mafia."[59] Other foreign visitors, Bimla Buti[60] from India and Abraham Chian[61] from Brazil, were honorary members of the Alfvén Wave Mafia. I was more a godfather than a hands-on technical leader, and it gave me joy to see the *mafiosi* happily at work.

In 1989, Ellen gave me a fiftieth birthday party featuring wonderful music performed by musicians suggested by her flutist friend, Anne LaBerge, who had moved from Los Angeles to Amsterdam with her composer husband, David Dramm to find work as avant-garde musicians. That party signaled to me that Ellen accepted my scientist friends and marked a release in tension that enabled me to deal with a second dimension of the midlife crisis that had to wait until I worked through the first.

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I had put off dealing with feelings common to many scientists at midlife. Half your scientific life is gone. You have achieved not nothing, but less than you hoped. You have experienced the challenge of research, and you are having a harder time marshaling the ruthless concentration you now know discovery demands. You have begun to

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[58] R. L. Hamilton, C. F. Kennel, and E. Mjølhus, "Stability and Bifurcation of Quasiparallel Alfvén Solitons," *Physica Scripta* 46, no. 3 (1992): 230; T. Hada, R. L. Hamilton, and C. F. Kennel, "The Soliton Transform and a Possible Application to Nonlinear Alfvén Waves in Space," *Geophysical research letters* 20, no. 9 (1993): 779-82.

[59] Bob Hamilton, Tohru Hada, and Einar Mjolhus struck up an enduring friendship; I heard after the fact that Einar had arranged a reunion on a musical excursion to Norway's Lofoten Islands.

[60] C. F. Kennel, B. Buti, T. Hada, and R. Pellat, "Nonlinear, Dispersive, Elliptically Polarized Alfvén Waves," *The Physics of Fluids* 31, no. 7 (1988): 1949-61; T. Hada, C. F. Kennel, and B. Buti, "Stationary Nonlinear Alfvén Waves and Solitons," *Journal of Geophysical Research: Space Physics* 94, no. A1 (1989): 65-77; T. Hada, C. F. Kennel, B. Buti, E. Mjolhus, E., "Chaos in Driven Alfvén Systems," *Physics of Fluids B: Plasma Physics* 2, no. 11 (1990): 2581-90.

[61] A. L. Chian and C. F. Kennel, "Self-Modulational Formation of Pulsar Microstructures," *Astrophysics and Space Science* 97, no. 1 (1983): 9-18.

experience unenthusiastic peer reviews. You realize your students will never see the opportunities you did. In capitalizing on a few opportunities you missed others; the missed opportunities loom as large as the successes. Yet half your professional life remains. You know innovation is for the young, but you are still a scientist, still curious. You wish you could hit it big one more time. You are becoming impatient with the slow procedures required for rigor in science. Your impatience is existential; you want to see the answers to questions that you now sense will be denied to you by the time you have remaining. You wish could spy on the future like a science fiction author.

Half a lifetime is just about enough to hit it big one more time. There is a multiplicity of worlds of science, called disciplines. Each has its salient facts, organizing principles, identifying dialects, behavioral norms, respected figures, hallowed events, sacred spaces, and animating legends. I yearned to perceive the essence of other scientific disciplines, to befriend scientific heroes, to see other places where I might have lived and worked. I wanted to be in the special places where knowledge is being generated, where one's sensory surround brings forth ideas before they could be expressed in words. I wanted to know and be known by the chiefs of the tribes of scientists who lived in science's special places.

Ellen, having lived a single life, did not feel betrayed or abandoned when I was away. She encouraged me to explore opportunities by making research visits. I returned to Princeton and stayed in a pleasant faculty apartment overlooking Lake Carnegie; once again I learned, in Thomas Wolfe's words, that you can't go home again. I spent three months as one of the last Fairchild Scholars at Caltech. A Caltech colleague told me he wished he could die, go to heaven, and return as a Fairchild Scholar; it was, he said, the best job at Caltech. I learned the physics of cosmic ray acceleration from Roger Blandford there, and Ellen and I made a friendship with Roger and Liz that was maintained until they moved to Stanford in Northern California.

I shared gloomy winter months in Moscow with the cockroaches in the old Soviet Academy of Science's hotel in October Square. Roald Sagdeev was Director of the Soviet Academy's Space Research Institute, and I collaborated with several of his talented staff. I made Russian friends who would later visit Ellen and me in Santa Monica as *glasnost* and *perestroika* unfolded, until she finally tired of taking them to Disneyland. *Perestroika* gave them permission to visit America, but it did not cure decades of consumer deprivation. Ellen and I found the way to social success was to feed visiting Russians Texas-sized steaks and take them to Disneyland, where Khrushchev had pounded his shoe. Finally, enough was enough for Ellen, and we have not visited Disneyland since. But we saw, and did our part to cause, the flight of Soviet scientists to the West at the end of the Cold War. More time passed and I went to NASA Headquarters as an Associate Administrator and later tried being a university Executive Vice Chancellor.

All these things had to happen before I was ready to cope with Hannes Alfvén's remonstrance.

# 18

# The Path to a New Life Led through the Arctic

**Figure 18-1: View from an island in the Beaufort Sea north of Canada's Arctic Ocean Coast. This picture was taken in 1981 by Ellen Lehman when she accompanied Canadian Wildlife agent, Richard Barnes on his rounds. Richard and Sandy Barnes remain close friends.**

Ellen Lehman created a personal relationship with the Arctic for both of us that has lasted to the present day. We were married in

1991 in Homer on Kachemak Bay in Alaska, but Ellen's attachment to the Arctic started well before that. Ellen had visited friends, Richard and Sandy Barnes, first in Kenora, aka Portage-aux-Rats, in rural Ontario, and later in Inuvik in Canada's Northwest Territory. While Ellen was visiting Inuvik, Richard and Sandy arranged a side trip to Fort McPherson to attend the Eskimo-Indian games; a souvenir of that day is the large wall hanging from Paulatuk which is the first thing you see on entering her professional office in Santa Monica. On that trip, Richard, a Canadian government wildlife agent, also arranged for Ellen to accompany him on a search in the Beaufort Sea for illegal whale kills; her stories of the trip told me of her taste for exotic travel.

**Figure 18-2: The first thing I see when entering Ellen's professional office in Santa Monica: the Inuit wall hanging from Paulatuk. This display spoke volumes about her interest in the Arctic and its indigenous cultures and arts, which became mine as well. Photograph by Ellen Lehman on March 28, 2016.**

Our joint interest in Alaska started when Ellen flew there before or after my summer visits to the University of Alaska's Geophysical Institute. Here was a place we could *both* be interested in. We traveled there at least once per year (and sometimes three times per year) between 1988 and 2019, after which the Covid pandemic limited travel. We did not rough it, we slept in beds every night, and almost achieved our ambition to drive everywhere Alaska's primitive road system went. This took persistence because the many of the smaller towns' road systems extended only ten or twenty miles into the surrounding countryside, so we often had to fly to the towns before renting wheeled transportation. We drove out of Fairbanks or Anchorage to Girdwood, Seward, and the Kenai Peninsula in South Central Alaska; to Palmer and Glenallen in the Matanuska Valley north of Prince William Sound; to Denali National Park; we flew to Nome and Kotzebue on the Bering Sea in Northwest Alaska; we got a memorable view of a vast caribou herd when we drove out of Nome to Teller to visit a native artist; we visited Chitina, McCarthy, and the Wrangell-St. Elias National Park on Alaska's border with Canada's Yukon Territory; and to rainy Ketchikan and Juneau in southeast Alaska. We flew to Kodiak Island in the Gulf of Alaska and the British Petroleum facility on the North Slope. We made especially sure to attend the annual meetings of the Alaskan Federation of Natives (AFN) in October. Ellen and I have made, we guess, sixty trips to the North, and soon had visited more of Alaska than many of my colleagues at the Geophysical Institute. Unlike Edwardian era explorers we did not go to prove we could brave the wild, we went to see a people emerging from the wild.

Alaska whetted our desire to see more of the Arctic and its peoples, and our northern travel repertoire expanded to include two visits to Iqaluit on Baffin Island in the then new Canadian Territory of Nunavut and six trips to Yellowknife in Canada's Northwest Territory. We did not neglect the European Arctic. I drove in Sami territory from a conference at the Geophysical Institute in Kiruna, Sweden, to Narvik on Norway's northern coast; when I was with NASA, Ellen and I visited the Norwegian polar research station at Ny-Ålesund on the island of Svalbard, halfway between Norway's Arctic coast and the North Pole. Ny-Ålesund was our personal furthest north, featuring midnight sun until August 21, the day after my birthday. We never did get to the Siberian Arctic.

**Figure 18-3: Homage to the great polar explorer Roald Amundsen, whose monument is on the Norwegian island of Svalbard (approximately 78 degrees north), near where he died in 1928 in a plane crash on a rescue mission over the Arctic Ocean. In 1925, Amundsen, with his backer Lincoln Ellsworth and pilot Hjalmar Riiser-Larsen, flew the airship *Norge* to Alaska via the North Pole in the first trans-Arctic flight across the Pole. Some birds, Arctic Terns, make even longer flights twice every year, from the Antarctic to the Arctic and back. Terns were evidently in the habit of also paying Amundsen tribute—the only kind they could—on the top of his head. Afraid we humans might disturb their nests, which were on the ground, whole teams of terns would dive-bomb alien visitors. Photo by Ellen Lehman.**

At first our travels were inspired by the landscape and the arts, but we found ourselves returning for the people and the culture. More valuable to us than all the works of art in our home are the pictures in our minds of the unique culture of America's forty-ninth state and its people. Soon our visits evolved a pattern that enabled us to spot change more easily than full-time residents for whom the changes seemed imperceptible. Within minutes of arrival, we would switch into our Alaska lives and the things that had changed between trips would stand out. Call it time lapse ethnography.

The occasional visitor might not get to hear the words that resident Alaskans use among themselves to assert their regional identity. "Outside" means places that are not Alaska, usually the other forty-nine American states; "Cheechako" conveys a feeling about tenderfeet from Outside that needs no explanation. You don't outgrow the Cheechako label until you have lived through at least one

Alaskan winter. “Great Land,” originally a native term, conveys how special Alaska’s geography really is. Tourist brochures boast that Alaska is more than twice Texas in area, or that the distance from easternmost southeast Alaska to the westernmost Aleutian Island, Attu, would stretch across the forty-eight continental states. But size is a given; it is the landscape that catches the imaginations of resident and Cheechako tourists alike. Landscape is everywhere. Within a few miles of Anchorage—or no matter what little settlement—is a landscape whose beauty would occasion a national park outside.

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**Figure 18-4: A few miles outside of virtually any Alaskan settlement you can find landscapes worthy of a national park in the lower forty-eight states. Photo by Ellen Lehman, who took this picture from the road a few miles north of Anchorage looking east on Sept 28, 2019.**

**Figure 18-5: Sunset over the Chena River, about three miles from the center of Fairbanks, Alaska. Interior Alaska, with its flat Taiga-like landscape, is the easternmost extension of Siberia. Were it not for the Bering Strait, Siberia and Alaska would be connected by land, but as it is, they share similar climatic and topographic characteristics. It is late October, and the freeze-up that will last all winter is just beginning. Photograph by Ellen Lehman, October 26, 2013**

The term ultimately most meaningful to the repeat visitor is "Last Frontier," but "frontier" means different things to the resident and the Cheechako. Today's Alaskans live far closer to nature than Cheechakos; danger is a furnace breakdown in winter; faculty colleagues catch or kill part of the family diet. If you live in urban Anchorage, "frontier" means the moose munching on the flowers in your front yard or the bear going through the garbage in your back yard. For all Alaskans, the frontier is the imaginary line separating comfort from danger that encloses every settlement, and flies with you between settlements.

The "Last Frontier" is as much an idea as a place. Alaska was already the geographical frontier of European exploration when the British navigator, Captain James Cook, arrived in the eighteenth century. You can find a monument to Cook in Anchorage, at the head of Cook Inlet, and if you are of a mind to think this way, you feel a kinship with Hawai'i and Australia where they, too, have monuments to Captain Cook. In nineteenth-century America, the frontier was a wild untamed place where the crowded East sent its restive and potentially dangerous youth west to build railroads; to find oil, silver, and gold; to convert land seized from natives into ranches, farms, and lately, shopping malls; and to build wealth for the New York or London investor.

"Last Frontier" also means that Alaska is the last place where restless Americans can go without being bothered by other people. To sensitive observers like the photographer Edward S. Curtis, "Last Frontier" conveyed a certain wistful fatalism; Native American tribes in the American West seemed destined to be wiped out to the last person by disease and violence by the end of the nineteenth century. All these notions were applied to twentieth-century Native Alaskans, whose gold, copper, furs, fish, lumber, and lately, oil, provided an incentive to tame a distant wilderness. The economic incentives were the same, but native Alaskans had a much better fate than their nineteenth-century counterparts.

Just as the federal government sent troops to the Western Frontier in the nineteenth century, it sent the US military to the Last Frontier in the twentieth. In the nineteenth century the troops were sent to subdue natives, but in the twentieth the military's job was to defend American soil. (The only battle fought on American soil during World War II was on Attu, in Alaska's Aleutian Islands.) This time native peoples were on the military's side; they volunteered for a Territorial Guard under the leadership of the now-legendary Major 'Muktuk' Marston at a time when natives were not allowed to vote. (One should not be surprised that all the natives in the Territorial Guard, each a subsistence hunter turned soldier, won sharpshooter awards.) Later, during the Cold War, the US Air Force developed a major presence in Alaska as it stood watch over the polar flight paths to Soviet Russia. It is a commonplace that the arrival of the US military during and after World War II transformed Alaska's society and economy; oil came later.

Not many Cheechakos, or even Alaskans, realize that one of the more profound influences on Alaska's native societies may well have come from the events triggered by a 1960 visit by that iconic cold warrior, physicist Edward Teller. In 1958, Alaska came within a hair's breadth of being invaded by the US Atomic Energy Commission (AEC). The AEC conceived *Project Chariot* one year before Alaska achieved statehood; *Chariot* was to explode a string of nuclear weapons on Alaska's north coast near native communities. Its stated goal was to build a deep-water port serving tiny communities far from commercial shipping lanes, the kind of port that elsewhere a supply ship visits once per summer season. Edward Teller visited Alaska several times to tout *Project Chariot* among Alaska's white business community. A Fairbanks newspaper called Teller's project a tribute to Alaska's promotion to statehood.

The apparently not self-evident fact that little commercial ship traffic frequented that ice-bound part of the world meant *Chariot* could not credibly sustain that economic rationale. *Chariot's* true goal was to explore the effects of fallout radiation on the ecology (and subsistence livelihoods of the surrounding Inupiat native villages). You can imagine *Chariot* was not very popular with the citizens of Point Hope, the village most directly in the line of fire. What happened next must have come as a profound surprise to the sensibilities of the late 1950s. On arriving at Point Hope, Teller encountered a non-submissive native community so angry that his pilot recommended that they get out of there in a hurry. Natives all over Alaska started and won a crusade. The crusade was led by Howard Rock, who founded *The Tundra Times,* the newspaper that organized native tribes across Alaska in opposition to *Project Chariot*. In 1962, the AEC quietly shelved *Chariot,* and Teller did not come back.[62]

Willie Helmsley, another great native leader of the period, founded the Alaskan Federation of Natives (AFN) in 1966 to organize and represent the views of Alaska's native tribes to the federal and state governments on the management of ancestral lands. While native societies had no conception of land ownership, they depended on their lands for subsistence. Unschooled tribal members were faced with the double challenge of organizing politically over an area that rivalled the lower forty-eight states in extent without benefit of reliable telephone service, much less the internet, as well as mastering legal arguments and political stratagems in English. AFN was to see success in a surprisingly short time.

In 1968, oil was discovered at Prudhoe Bay on Alaska's North Slope; the oil interests, wishing to establish clear title to the land before they commenced drilling, supported the passage of the Alaska Native Claims Settlement Act (ANCSA) by the US Congress in 1971.

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[62] I knew much of the Teller-Helmsley story when I attended a symposium of the National Academy of Sciences in 1994 commemorating the fiftieth anniversary of the world's first nuclear explosion. Teller was a featured speaker, then so elderly he had to be helped into his chair. His mind was still vigorous, and he was as aggressive verbally as a thirty-two-year-old congressional staffer.

Not many outside Alaska know ANCSA was the largest private land transfer in US history. ANCSA was also a considerable improvement in provision and execution over the earlier American Indian treaties; Alaskan tribes were granted clear title to larger portions of their ancestral lands than nineteenth-century Indians. The Alaskan tribes were also compensated for loss of the rest of their lands by the endowment of native corporations, which invest on the behalf on their tribes in the economies of the other forty-nine states as well as Alaska. Ellen and I heard at the 2022 AFN meeting that New Zealand had been inspired to reach a similar settlement with the Māori.

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Willie Helmsley was an honored senior leader of AFN when Ellen and I started to attend its meetings in the late 1980s. His Ivy-League-educated daughter carries on his leadership. Natives from communities all over Alaska gather at AFN to discuss issues from subsistence hunting to domestic violence to federal infrastructure support. Natives, who comprise eighteen percent of Alaska's population, are a major political force, and both senators, Alaska's one representative to Congress, the governor, and the mayors of Anchorage and Fairbanks regularly speak at AFN.

Native artists also come to AFN from all over the state to sell their work. It was at AFN that we fell in love with the arts of Alaska's indigenous people—the Inupiat near the coast and the Athabaskan Indians in the interior, as well as the Haida, Tlingit, and Tsimshian tribes of Southeast Alaska. We are not financially able to participate in the London or New York art markets, but we were able to acquire best-of-genre works directly from native Alaskan artists. Better yet, we got to know the artists. We also acquired works of Western tradition painters like the Englishman, Sydney Laurence, and Eustace Paul Ziegler, a Yale divinity graduate, both of whom who came to Alaska in the Gold Rush era, or Fred Machetanz, Barbara Lavallee, and Rie Muñoz, who arrived after World War II from Ohio, Iowa, and Holland, respectively, to create distinctive artistic genres reflecting the lives of both natives and non-natives in transition from ancient to modern ways.

You could also see the modern encroaching on the ancient by who comes to the AFN meeting to recruit natives. Thirty years ago, government agencies and big corporations sought skilled laborers—truck drivers, heavy equipment operators, and so on. Now, universities—Harvard and Dartmouth as well as the University of Alaska and various community colleges—come to recruit students. This was already having an impact. When Ellen and I were in Kotzebue in the mid-1990s, we heard an impassioned diatribe against the herding of native students into the government school at Mt. Edgecumbe from a native who was a PhD graduate of Michigan State University. When I served on the Pew Oceans Commission in the early 2000s, I heard a sophisticated PowerPoint presentation on native fishing rights by a native Alaskan with a graduate degree from the Harvard Business School. This kind of thing was happening more frequently as time passed.

Alaska asserts its cultural identity through art. The state has a "One Percent for Art" program that devotes a portion of state revenue to public arts. You recognize the art at airports, public schools, and hospitals and, as noted, AFN attracts artists and artisans from all over Alaska. We started collecting directly from the artists at the AFN show and at the gift shop of the Alaskan Native Hospital. Ellen has photographed the artists with their pieces, many every year; she now has thick notebooks containing photos annotated by name of artist and date. This is where my Ellen has made her mark; by now Ellen's collection has scholarly value. Her photos comprise a thirty-year chronicle of Alaska's native arts community, wherein one sees not only one's friends age, but traditional art forms reinterpreted by younger artists with formal training. Ellen also has hundreds of books on Alaskan life, many self-published in editions not available elsewhere. She is now helping to build the collections

**Figure 18-6: Ellen Lehman with doll maker Ursula Paniyak at the annual meeting of the Alaskan Federation of Natives, Anchorage Alaska, October 21, 2011. Photo by Charles Kennel.**

on Alaska at the American Philosophical Society, which has a long-standing interest in American Indigenous peoples.

Our search for the arts of northern native people soon extended to Arctic Canada. There followed two summer trips to Baffin Island on Canada's eastern Arctic coast and at least six to Yellowknife, a town of about 20,000 that is the capital of Canada's Northwest Territory on the Great Slave Lake on the flat, rocky Canadian Shield. Yellowknife's landscape is a stark contrast to the extravagant mountains of Alaska or the lushly forested glories of Jasper, the ski resort 675 miles to Yellowknife's south. Yellowknife is an outpost of modern Western creature comfort in the featureless Canadian Shield, and a group of native soapstone carvers had migrated there from impoverished settlements like Tuktoyactuk on the Arctic Ocean, about 1,900 miles to the north. Seeing the success of Baffin Island sculptors, Yellowknife's set out to establish a distinctive western Canadian style of soapstone carving, which we found more dramatic and less rigidly stylized than the more famous Baffin Island style.

Canada's paternalistic approach to its native populations meant there had been fewer calls for a decidedly independent organization like the Alaskan Federation of Natives, and we found native society there the poorer for it economically and spiritually.

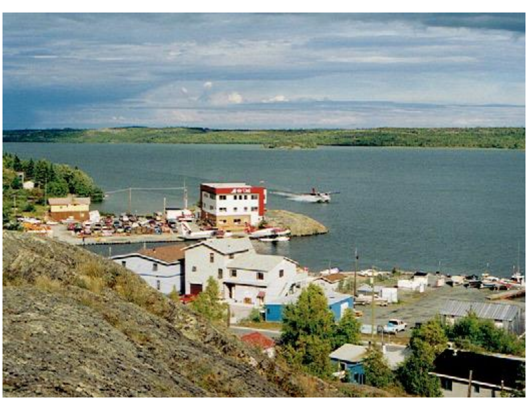

**Figure 18-7: The non-native district of Yellowknife, a settlement situated on an arm of the Great Slave Lake in Canada's Northwest Territory. Our native friends lived in the less elegant town center, perhaps a mile distant. Photo by Ellen Lehman (2002).**

**Figure 18-8: Charles Kennel (right) with soapstone carver friends Eli Nasugoleak (left) and Derrald Taylor (center) in the Frozen Rock Studio, Yellowknife, Northwest Territory, Canada, September 7, 2018. Photo by Ellen Lehman.**

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The parents of our native Alaskan friends lived hunter-gatherer lives full time. Despite automobiles and airplanes, the mindset of prehistoric hunter-gatherer communities persists in Alaska's dispersed native communities. Alaska's overlay of twentieth-century consumer culture is deceptive. Many native Alaskan families have modern homes in Anchorage or Fairbanks and ancestral homes in the villages. In addition, white settlers arriving from Outside less than a century ago were creating tiny communities where face-to-face communication ruled and travelers carried both welcome news and unwelcome sickness. (The one was worth risking the other for.) These early settlers lived in hamlets with less social structure than rural communities of eighth-century England.

By 1959, World War II and the airplane had changed Alaska enough that people Outside could be convinced to convert Alaska from a territory to a state. There were (and still are) Alaskans who needed to be convinced of the virtues of any relationship with the federal government, but airplanes and television did not stop working their transformational magic. By 1988, when we started visiting, residents were just beginning to call Anchorage "Los Anchorage" (not, I hasten to add, a term of approbation). The invasion of big box stores from the lower forty-eight had been underway for about ten years and people began to find fresh vegetables in the supermarkets of Fairbanks and Anchorage in winter.

In 1988, Alaska had not yet completely adapted to the twentieth century. Guns had been necessary for subsistence and dispute resolution, and there was still a restaurant in Fairbanks that required you to check your guns at the door. Some colleagues at the University of Alaska still lived without running water at home; the Geophysical Institute had to provide showers for them.[63] These intrepid souls, scientific descendants of fur trappers and gold prospectors, did things like fly measuring instruments in small airplanes into the mouths of active volcanos.

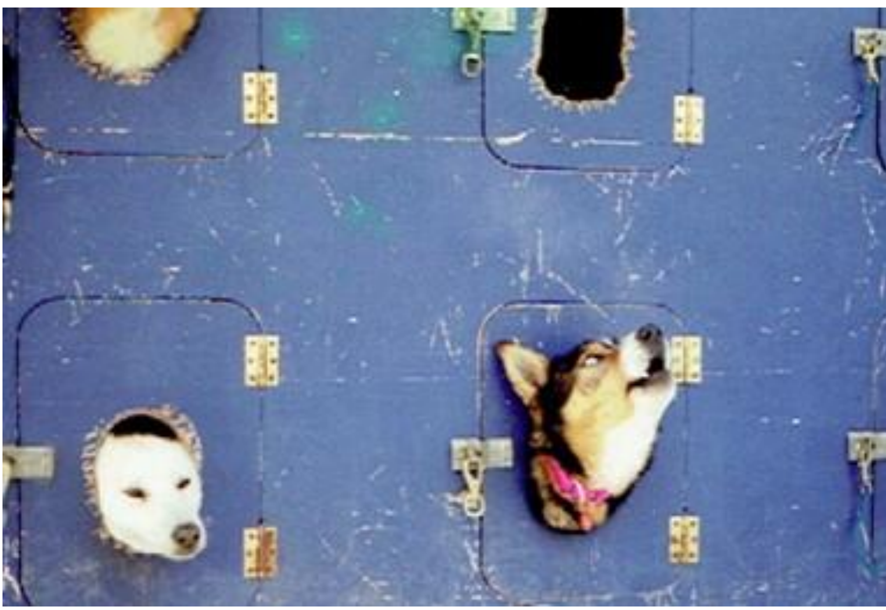

**Figure 18-9: At the ceremonial start of the Iditarod dog team race, Anchorage, Alaska, 1997. Animal welfare advocates argue that the 1,049-mile Anchorage to Nome dog race in the cold of Alaskan winters is cruel and unusual punishment. Nobody asked the dogs. Here are members of one team waiting excitedly to be harnessed to their sled. They seemed impatient to be on their way. Photo by Ellen Lehman.**

[63] Scripps, by contrast, provides showers for the students who go surfing in the ocean at lunchtime.

Today's Alaska does what it can to acknowledge its full membership in the union of American states through sports. Its weather and terrain do not bless football and baseball, but ice hockey and basketball are popular. Basketball was a Cheechako sport, but now many towns have teams and statewide competition is enthusiastic. Ellen and I attended a season-opening collegiate basketball tournament (in which UCLA participated) in Anchorage's Sullivan Arena, and we hope to find a collegiate hockey game there when we visit every October for the AFN meeting. Nearly all Alaskans agree on the dog-team race from Anchorage to Nome, the Iditarod, and the less well-known race from Fairbanks to Dawson in Canada's Northwest Territory. Nonnatives were reminded of Alaska's frontier heritage, and natives had dogs in the family.

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In 1992, Ellen and I flew the 547 miles from Anchorage to Kotzebue, which is astride the Arctic Circle. On our arrival, we ventured outside in a heavy downpour to visit the National Park headquarters where a friendly park ranger dried our wet gear on radiators. We took the opportunity to ask him to refer us to native artists, and he sent us to meet George Goldy, his uncle. We hit it off with George and his wife, Shirley. The next summer, George and Shirley invited us to stay with them in Kotzebue. It was George who could not stand living in Anchorage; Shirley, on the other hand, was from Anchorage and yearned for urban conversation. Their lithe eldest son, Patrick, sixteen years old, was raised as a hunter on native food, while their indolent youngest daughter was raised on television and potato chips. When we arrived at their home, we found Patrick on the living room rug butchering a caribou, while his plump sister sat in a padded sofa watching a quiz show on television. On our next trip in the fall to the meeting of the Alaskan Federation of Natives (AFN) in Anchorage, we introduced ourselves to Shirley's mother, Sara Tweet, an accomplished seamstress and doll maker. Later, Sara made for Ellen a perfect-fitting *kuspuk* (a native calico dress) from memory without needing measurements, so intuitive were her skills. We have sought out Sara Tweet at AFN many times since.

A rumor was going around Kotzebue's native community that pike, an aggressive game fish, were biting at the junction of the big Noatak River and one of its tributaries, perhaps thirty miles from Kotzebue. George's blood was up. He collected his family, his visitors from California, and a neighbor family, and soon after he finished his workday with the telephone company, he herded us all into his large flat-bottom boat with twin Mercury engines. There would be enough light when the boat got to the fish because in that season it never gets dark at night. George gave Patrick the tiller and stretched out in the bottom of the boat. Even with his eyes closed, George could tell where we were from the sounds the boat made in the varying depth of the shallow water. He identified the landmarks we were passing to Patrick. To me it was a featureless flat landscape, but to George it was alive with memories—his own and cultural—of where people had had close escapes and where friends and relatives had found food.

After we had passed the fish hatchery, George warned Patrick that we were nearing the junction of the two rivers and Patrick turned off the engines. We glided silently to where the rumors said the fish were. George signaled with a head motion and everyone on the boat dipped lines into the water. There were ten or twelve bites in the first minute or two and then nothing. George declared aloud that this was going to be it for the evening; the fish knew about us. By then it was nearing midnight, and the sun was setting—not in the west but to the north.

The environment shaped the humans in the hunter-gatherer days. People were constantly on the move to places where the environment could feed them. It shaped how they lived, how they thought, what they felt, where they traveled. They were hostage to the whims of the weather. With the population of the globe now at seven billion and climbing, you may think there is no place left where people live like that—and you would be almost entirely right. In today's Anthropocene Era, there are hardly any places on earth where human culture has not decisively shaped the environment, including in the Arctic. But that was not true as late as the turn of the twentieth century, for then Euro-American explorers penetrating frozen Arctic wastes encountered small bands of Inuit *who had never met other humans*. And today, when you go to the circumpolar north (Alaska, Canada, Greenland, Scandinavia, Siberia) you encounter communities in transition, with vestiges of the ice ages surviving in the ways people think and feel. These northern societies are leading the globe in the transition to a warming climate.

Our half-native friend, Ellen Paneok, personified Alaska's imperfect reconciliation of the old and new. She had a remarkably nuanced if unschooled command of the English language, and a surprisingly culturally aware sense of humor. The daughter of a father from Outside and a native mother, Ellen ran away from a foster home at age fifteen to take flying lessons from the pioneer aviator Noel Wien. She became a bush pilot famous for delivering passengers anywhere in any weather during her tumultuous youth. The Smithsonian Air and Space Museum honors her as the first licensed native Alaskan pilot. In her more sedate post-teen years, when we knew her, she divided her creative time between traditional carving in ivory and writing up her flying adventures for aviation magazines.[64] Her flying made her a heroine in certain circles in town, and she loved to hang out where the Anchorage cops ate breakfast in the early morning. Ultimately Ellen could not hold the two halves of her life together, and she died of alcoholism at age forty-eight. In her last two years, the two Ellens spoke on the telephone nearly every day. Ellen Paneok willed some of her shares in CIRI (Cook Inlet Regional, Inc., one of Alaska's twelve native corporations) to my Ellen Lehman, who in turn is planning to will them to the daughter of one of our native friends.

To paraphrase a thought John Maynard Keynes once applied to Isaac Newton, Ellen Paneok was the last of her kind to look out on the world through the eyes of the ancient hunters. She was also the first of her kind to look down on her world through the eyes of a professional pilot.

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Ellen Lehman had been e-mailing Alaskan friends to fix times to meet for more than a month. Her excited anticipation extended to practical details: warm clothing and presents piled up waiting to be packed, and we bought a portable wheelchair in case my breathing

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[64] Ellen Paneok scrimshawed a small piece of walrus ivory with an image of a native hunter and his spear looking to the horizon, an aurora winding sinuously overhead. You can almost hear the crackling sound the natives insist is made by the aurora overhead. I asked her, what did native children in the old days think about when they saw the aurora? Ellen had quick responses to the intrusive curiosity of Cheechakos: she said if you are bad, the aurora will come down and bop you on the head! An image of Ellen's scrimshaw became the first slide in my 1993 UCLA Faculty Research Lecture on the physics of the aurora.

problem reasserted itself (which it did). We were about to depart on a ten-day trip to Alaska, our first since the pandemic began. Ellen made especially sure that we meet with Fred and Randi Hirschmann as soon as we arrived. Fred and Randi ("Frandi"), an inseparable couple, met when they were national park rangers in the lower forty-eight states, married, and came to Alaska. They bonded with my Ellen (and me) in the aftermath of Ellen Paneok's death. "Frandi" had bought an elegant log house with oversize bay windows in Wasilla, forty miles north of Anchorage. Over the decades since they got their dream house, Wasilla had accreted big box malls, hardware stores, thrift shops, medical clinics, and food markets. Wasilla was a Bakersfield (California) transplanted into Alaska by stealth during dark winter nights when no one was looking. Fred couldn't stand it and they built a second house in remote Sutton, well up in the mountains and a world away from Wasilla's wannabe California freeway culture.

We agreed to meet in "Frandi's" once-upon-a-time Shangri-La in Wasilla. It had snowed ten inches in Sutton overnight, and when Fred arose, he had to put chains on his big truck's tires and plow his and several neighbors' access roads, many a good part of a mile long. Ellen and I put off our visit to Wasilla for a day so he could complete his neighborly chores. Fred arrived in Wasilla, where Randi had been preparing for our visit, about fifteen minutes after Ellen and me. He brought with him two rowdy dogs and a stew of moose meat and handpicked vegetables. (Which was more remarkable, the contents or that he had found time to make it?) But it was not his cuisine but our after-dinner conversation that confirmed him as a latter-day Leonardo da Vinci, a relentlessly curious genius not in the fifteenth-century Florentine hills but in the twenty-first-century Alaskan mountains. Like Leonardo, Fred habitually works the night through, taking catnaps if fatigue catches up with him. Like Leonardo, Fred aspires to omnicompetence. A Fred explanation makes the otherwise inert sparkle with life. He masters twenty-first-century technologies midst the practical challenges of life in America's Last Frontier. He needs Alaska's practical challenges to feel alive.

Fred earns part of his living as a photographer. When words fail him, he expresses his thoughts in images that belie hidden layers of complexity below the surface. He has created sequences of high spatial resolution photographs (about twenty per second) that the human eye sees as a moving picture. Detail is made to dance in his moving pictures of clouds and the aurora. When words failed

Leonardo da Vinci, he too turned to imagery to express the complexity beneath the commonplace; Leonardo's notebooks are filled with many drawings of turbulent flows in water. As I watched Fred's films of clouds, flowing above and swirling around mountains, and of flickering auroral arcs—rippling, twisting, and turning in the night sky—I found myself wishing I could overhear a conversation between these two restless geniuses about nature's mysteries. I wished I had had Fred's auroral imagery when I gave the UCLA Faculty Research Lecture twenty-nine years before. Fred was amazingly conversant with the terminology and concepts I had used with UCLA's scientifically literate audience. It was as though he had been at the lecture.

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The 2022 meeting of the Alaskan Federation of Natives was the first held in person since the pandemic began. Once again, Ellen and I visited with artist friends, now a diminishing number from death and infirmity, at the AFN craft show. One, June Pardue, received an AFN President's Award. We attended presentations made by political figures either in, or contending for, state and federal office, cabinet secretaries, university presidents, business CEOs, and AFN leaders. Climate change was on Alaska's mind; there had been an enormously destructive storm on the coast and major collapses of crab and salmon fisheries so important to both subsistence living and economic prosperity. Alaska's pleas to the federal government for assistance in disaster response and infrastructure maintenance had greater force this year.

AFN's theme for 2022, "Celebrating our Unity," meant different things to different people. We are together again after the pandemic; we natives have achieved much by sticking together; and finally, we must not succumb to the murderous resentments that afflict politics Outside. Native Alaskans have good reasons to resent social and economic discrimination, yet they want to see bridged the divisions afflicting the nation they are part of.

# 19

# Moscow Gloom

It surprises me when I think of it, but there were a few years during *glasnost* and *perestroika* when Moscow was my second-most-frequent destination for scientific travel after our own nation's capital. The political atmosphere had become more welcoming under Mikhail Gorbachev's policies, and I spent the late winter and early spring of 1986 at Moscow's Space Research Institute, then directed by Roald Sagdeev, my mentor during our few months together at the International Centre for Theoretical Physics in Trieste, Italy.

My close colleague and collaborator, TRW's Fred Scarf, had one of his plasma wave instruments chosen for flight on a pair of Soviet Mars probes. In 1988, when Fred died unexpectedly at age 57 in Moscow, NASA selected me to be the successor principal investigator of Fred's experiments, thinking that I, at least, could work the science politics with Sagdeev. I was a figurehead PI and the technical work continued without insightful input from me, but I attended team meetings in Moscow. The Soviet Union fell apart in 1991—as did my colleagues' lives in once orderly Moscow. The social disorder that followed occasioned the great diaspora of Soviet scientists to the West, and the next chapter relates anecdotes told me by my Russian colleagues about life in the Soviet Union. My last visit to Moscow was as an Associate Administrator of NASA in 1995; after that I had no desire to go back, and never did.

I was rudely greeted by the Soviet authorities on my first trip the to the USSR in 1971. The occasion was to give a talk at an international meeting on space science. Two MiG fighters escorted my British European Airways flight wing-to-wing, from its entry into

Soviet airspace to Sheremetyevo Airport, where immigration agents with drawn automatic weapons boarded the airplane to greet the passengers. They pointed their weapons at us as they turned our passports over in their hands. Two lines of Soviet Army troops formed a human corridor for us passengers to walk through from the airplane to the terminal. The soldiers followed each one of us with automatic weapons as we walked by. You can imagine my relief when I saw my Trieste officemate, Alec Galeev, waiting for me in the terminal.

I remember little else of this visit except that we did not have glass 35mm slides in those days, and the overly bright Soviet projector burned a small hole in the middle of each of the photo-slides that I had carefully prepared in Los Angeles. Though I tried not to be inflammatory, the image of the burn-through on the screen expanded as I expounded, edges smoking. I hurried to call for the next slide before the one I was talking about burned up. The audience, straining to follow my English, was more absorbed in the hole with the smoking edges than in what I was saying.

During my 1975-76 sabbatical, Roald Sagdeev passed through Paris on his way to Toulouse, the center of the French aerospace Industry, to accept an honorary degree. He had persuaded his minders in Soviet intelligence that wives are expected to attend degree ceremonies in the West, and so Tema Sagdeev got permission to accompany him to France. My then wife, Debby, and I took them to the Louvre, Jeu de Paume, Musée de l'Orangerie, and other great Parisian museums on their ways to and from Toulouse. Tema wept on seeing the pictures; she knew them from books, she said, but she had never ever expected to see them in person. Later, at our farewell dinner, this intelligent, cultivated woman admitted that she couldn't bear the primitive *apparatchiki* that Roald had to deal with.

Cold War hostility was less rude than in 1971, but no less profound in 1977 when I made an official trip on behalf on NASA. The telephone had rung in my UCLA office with an invitation to join a NASA team going to the Soviet Union to negotiate a cooperative agreement for research on NASA's forthcoming Space Shuttle and the Soviet Salyut missions. Noel Hinners, another Princeton PhD who was then NASA's Associate Administrator for Space Science, chaired the NASA team. The proposed Shuttle-Salyut mission was to be a follow up to Apollo-Soyuz, the first crewed international space mission, which was thought to have served a useful diplomatic purpose. I took my responsibility seriously and polled several

American colleagues for ideas about what to do. It was good to have a reason to call up important people.

Before leaving for Moscow the NASA team assembled in Washington for preparatory discussions. The morning of our departure we had breakfast in the Cosmos Club with an undersecretary for Soviet affairs in the State Department. This undersecretary carried a distinct message: Gentlemen, he began, you will be the only official US delegation in Moscow during the sixtieth anniversary celebration of the great 1917 Soviet Revolution. Ordinarily, the US has ten or so teams in Moscow negotiating something. The State Department does not care *what* the teams talk about, but we do care about the *tone* that is set. Surely the fact that you will be the only US team in Moscow ought to tell you something. In case some of you still miss the point, I will be explicit: *relations between the United States and the Soviet Union are bad, getting worse, and we have no interest in improving them.*

With official pessimism ringing in our ears our NASA team left for Paris, where we were required to rest before starting a US government negotiation. We did not tell the other guests at Jacques Blamont's[65] dinner what we learned at breakfast in the Cosmos Club. Roald Sagdeev had evidently received a similar briefing, for upon our arrival in Moscow he told me quietly it was unlikely we would get down to discussing serious science, so I might as well spend my time in Moscow visiting scientists. That is when I met two scientific heroes from my personal pantheon, the astrophysicist Iosif Shklovsky,[66] and the physicist Vitaly Ginzburg.[67] At the concluding banquet, the Soviet contingent disguised their disappointment at our failure to reach agreement by joking about Russian idiosyncrasies in their toasts to friendship, which were numerous. I left for America somewhat the worse for wear, pleased for myself, and discouraged by the state of the world.

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[65] Jacques Blamont (1926-2020), founder and Technical Director of the French National Space Studies Center (Centre National d'Études Spatiales, CNES). My Trieste and UCLA colleague, René Pellat (1936-2003), was also to direct CNES.

[66] Iosif Samueilovich Shklovsky (1916-1985); I had read a translation of his 1962 book on life in the universe.

[67] Vitaly Lazarevich Ginzburg (1916-2009); Nobel laureate, one of the fathers of the Soviet hydrogen bomb, known to me as the author of an amazingly concise paper on the great problems of physics. It was one of the things students would carry around with them.

It was 1986 and the Soviet Union was undergoing great changes, yet Moscow in winter was strangely familiar. In truth, it only seemed that I had seen its chilly gray gloom before; I had read about Moscow gloom as a child. World War II correspondents used to write how the gloom settling in over Moscow set the atmosphere for the tense discussions our diplomats would have with the hard men who ran the affairs of America's enigmatic ally. (If in wartime you do not choose your enemies, your enemies also choose your friends.) The five-year-old newspaper reader and the forty-seven-year-old scientific visitor were in knowing communion as this visitor watched, from high above, as white ice fog laced with black pollution lay its greasy film over everything below rooftop level. From my eighth-floor rooms in the Soviet Academy of Science's hotel on October Square, I could see the sunny winter sky above the sharp upper boundary of the ice fog. The roofs of the buildings glistened in the bright sun above the gray ice fog; it was reassuring to know the sun was still there. Pre-communism Muscovites had painted their buildings in bright pastels—lime green, rose pink, peachy orange—to counter the winter gloom. The Stalin-era buildings (like the one I was in) were not all that cheery.

The middle-aged American scientist looked down from his window on the tiny figures milling about at dawn. He had been awakened by the screeches of the streetcars in the switching yard behind his hotel where commuters were getting off onto the sidewalk. He knew from experience they were struggling on the ice. Sidewalks not kept in repair were a hazard in the best weather, but now they were covered with a five-inch layer of ice with serious hills and valleys. You had to look down and choose where to place your feet. Moscow was magically beautiful in the morning when it had snowed the night before, but the sidewalks were even more treacherous. There were, withal, some places where it was safe to walk, but you had to keep your head down to see where you were going. You were safe from slipping when you finally got to Moscow's fabled underground transit system. Subway stations were communism's temples. To get to the trains, though, you had to brave a long trek in muddy corridors smelling of urine not yet congealed into ice. I did that every morning to get to the Soviet Academy of Science's enormous Space Research Institute several miles to the south on Profsoyuznaya, number 88. I learned to call the Institute by the sound made by its Russian initials, IKI, pronounced "icky."

Spoiled American visitors were never prepared for the food in Soviet Russia. A person sitting alone in one of Moscow's cavernous restaurants could wait all evening without ever being approached by any of the waiters lost in conversation near the door to the kitchen. There might not be any other customer in view. I did not feature spending whole evenings in search of food, so I brought packets of dried mountaineer's food from America, which I warmed in my hotel room with an immersion heater. This being *glasnost*, I would puzzle out TV documentaries on Stalin's crimes during what passed for dinner. You could be more confident of being served in the cafeteria of the Space Research Institute, but my mother would have disapproved of the greasy, battered aluminum dinnerware. The cafeteria on an upper floor of the Bolshoi Theater was a glorious exception to this almost universal rule. My colleagues eagerly sought permission to take me to performances of ballet and opera there so they could dine at the best restaurant in Moscow. I soon learned how wonderful it was to oblige. It didn't happen all that often, though, and many a weekday afternoon my train of thought would be interrupted by food fantasies.

**Figure 19-1: The Space Research Institute, Profsoyuznaya 88, Moscow. The grey gloom was typical.**

Like Moscow's colorful old buildings, IKI's scientists offset the gloom of communist life with color, but of the personal emotional kind. Every human relationship in their country had suspicion and mistrust somewhere, but once suspicion was tested and set aside, friendship was what there was to warm chilled lives. I was there because of a friendship I had made with IKI's Director, Roald Sagdeev, when at the International Centre for Theoretical Physis in Trieste twenty years earlier. I had been his young acolyte for a few months in 1965 and 1966. The American and Soviet governments knew of this relationship, and I can only surmise that they both thought it suited their purposes to see the friendship continue.

Roald had assembled a team of the USSR's best and brightest to carry out the Soviet Union's space science program—like the Moscow subway, it was communism's reach for excellence. In the vast Institute building they made the scientific instruments to be carried into space by rockets, which were launched not by a dedicated scientific agency like NASA but by the military. The Institute scientists worked in Roald's and my field, space plasma physics, but also in planetary science and astrophysics. Although IKI's real business was to produce working experiments, the Institute had a wonderful collection of theoreticians with whom I worked. They were there so that IKI could hold its own in the company of the other great theory institutes in Moscow, like the Institute for Physical Problems where the great Lev Landau had worked, or the Lebedev Institute where Vitaly Ginzburg worked. It turned out the Soviets had a physics aristocracy much like the one I was brought up by as a Princeton graduate student.

The change in political atmosphere that Roald's college friend, Soviet Premier Mikhail Gorbachev, labelled *glasnost* and *perestroika* (openness and reconstruction), had encouraged me to visit the Soviet Union in the late 1980s. People were willing to say things that had been dangerous ten years before. Sagdeev was devising stratagems large and small to promote *perestroika*. One day, just for laughs, he introduced me as a member of his staff when an American group chaired by David Hamburg came to discuss arms control; I sat in on their discussions on the Soviet side of the table. Senator Alan Cranston of California told me that he realized something interesting was going on when he encountered one of his constituents sitting on the Soviet side.

A much less showy encounter revealed to me why the international atmosphere was so different. One day at IKI, I spotted a room full of unopened boxes of American personal computers. I asked Roald about the boxes. We Soviet scientists, he said, were afraid we were falling behind America in computing, and we invited a panel of Japanese experts to give us advice on how we could recover. On their flight from Tokyo to Moscow, the Japanese group previewed what they might find. They thought their final report would say the USSR was behind but could catch up, but after seeing the situation, they told Sagdeev that the USSR would never catch up. He was able to use the Japanese report to get permission to buy Western computers. Sagdeev's colleagues had already begun using them. I recall being in an animated group standing around the great mathematician, V.I. Arnold[68], as IKI's George Zaslavskii[69] ran clever computations in chaos theory on out-of-date American PCs. Arnold seemed more interested in the PCs than in the mathematics.

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Although I was never near the center of the historical stage, my twenty years of trips to the Soviet Union gave me a front-row balcony seat from which I could see the end of the Cold War approaching. One evening shortly after my 1986 stay in Moscow, my wife-to-be, Ellen Lehman, and I were at home in Pacific Palisades, California, when we got a telephone call. Charlie, this is Roald! Where are you? I asked. Santa Monica, at the Rand Corporation, he said. We have just finished our discussions; can I come by for dinner? He arrived shortly after, alone, without the "friend" from Soviet Intelligence who usually accompanied him in America. This was new: a prominent Soviet physicist, a known friend of Gorbachev, discussing arms control at the Rand Corporation alone, without a minder.

After dessert, Roald asked if we could reach a new friend of his, the popular American singer John Denver. (There were no public telephone directories in Moscow, and you asked your network of friends to help you connect with people; otherwise, you had to pay ten kopecks at a kiosk where they wrote down why you wanted to make the call.) Ellen and I tried to oblige Roald, but directory

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[68] V. I. Arnold (1937-2010), mathematician and one of the founders of chaos theory.

[69] George Zaslavskii (1935-2008), mathematical physicist, made notable contributions to Hamiltonian chaos theory.

assistance in Aspen, Colorado, told us John Denver's number was unlisted.

Roald understood America. Bonding with John Denver was the kind of thing that could really help end the Cold War, but there was something even bigger. Roald was to marry an American princess, Susan Eisenhower, the former president's granddaughter. This was a sign of new times. I would seriously miss Roald's first wife, Tema, a sensitive and intelligent woman, but I soon came to appreciate Susan's incisive personality. Later, in the mid-90s when I was at NASA Headquarters in Washington, I visited Susan and Roald at their lovely estate in West Virginia, and years after that, when I was working on climate change, Susan asked me to speak at the Eisenhower Institute. She even got President Obama's science advisor, John Holdren, to attend.

**Figure 19-2: The last great celebration of space science during the Cold War. Shown here are some of the scientific luminaries gathered to celebrate the thirtieth anniversary of *Sputnik* on October 4, 1987. This picture shows those in attendance probably listening to an account of the Soviet Union's successful 1986 mission to Comet Halley. In the front row, from left to right, are Fred Scarf, from TRW; Stamatios (Tom) Krimigis, from the Johns Hopkins Applied Physics Laboratory; possibly Lou Lanzerotti, from Bell Laboratories; Charlie Kennel, from UCLA; and a young Lev Zeleny, future Director of the Russian Space Research Institute. Directly behind me in a gray suit is Konstantin Gringauz of IKI, the last person to touch *Sputnik* before it was launched. To Gringauz's left (right in the picture) is Andy Nagy from the University of Michigan, who sent this digital copy to me.**

Shortly after his Rand Corporation visit, Sagdeev organized a symposium on October 4, 1987, the thirtieth anniversary of *Sputnik*'s first orbital flight. Roald got the Soviet airline, *Aeroflot*, to fly hundreds of his international friends to Moscow. It was a grand event, a gathering of the international space science clan, a happy celebration of what enabled the careers of all present.

**Figure 19-3. Twins in space plasma physics, A. A. Galeev and C.F. Kennel, at the celebration of the 30th anniversary of *Sputnik*. The ineffably talented Alec Galeev and I were the babies of the plasma physics group at the International Centre for Theoretical Physics in 1965-66. Here we are 21 years older; Alec will become Roald Sagdeev's successor as director of Moscow's Space Research Institute. Photo by an unknown participant.**

*Sputnik*'s thirtieth birthday party was the last best celebration of Soviet space science. The Chernobyl nuclear disaster seventeen months earlier had already begun to eat away at the legitimacy of Soviet governance, the Berlin Wall would fall in 1989, and Boris Yeltsin's freewheeling capitalism would replace Mikhail Gorbachev's humanized communism in 1991. No matter how great a physicist Sagdeev was, it is too much to ask whether he foresaw all this. How could he suspect his grand celebration was a swan song for a way of life about to disappear?

The realization would come soon enough. In 1989, the Berlin Wall came down and international attention turned elsewhere, leaving ordinary Russians to sort through the fall of communism. I happened to be in Moscow in 1991 when Boris Yeltsin ousted Mikhail Gorbachev on CNN; my Russian colleagues joked that this was when the gangsters abandoned government employment for the private sector. My trips to Russia in the 1990s confirmed what they had been saying: privatization was diffusing gangsterism everywhere. This was at a time when some Western economists were urging faster privatization.

Privatization may have been good for oligarchs, but it was not good for Russian scientists. Public order frayed. One evening, I noticed bars on the outside-facing windows of Mischa Malkov's eighth floor apartment. I asked him about the bars. He said that robbers had taken to rappelling down from the roofs of apartment buildings to break in through upper-floor windows. Mischa also showed me the metal bars he had installed across the main door to his apartment to keep gangs from chopping in with axes. Even homes with little to protect had to be fortified like castles. IKI was not immune; business gangsters brazenly invaded the Space Research Institute, one of the sacred places of Cold War Russia, and occupied IKI's technical floors rent-free.

Gangsters also took over the airlines. Later, I was part of a US National Academy group attending a conference in Stalin's favorite resort of Sochi in the Crimea. We knew there would be trouble when, during the transfer from Moscow's international airport, Sheremetyevo, to its regional airport, Vnukovo, airline personnel demanded payment in Western currency for a flight that the Russian Academy of Sciences had already paid for in rubles. Our Russian handlers wanted dollars. A call to the Vice-President of the Russian Academy, my plasma physics colleague Evgeny Velikhov,[70] was required to settle the matter. Our return flight was cancelled because again the airline said our trips had not been paid for. Again, a demand for dollars; again, calls to Moscow. This time it took a day, during which the wife of a Russian colleague conducted an admirably well-

[70] Yevgeny Velikhov (1935-) co-wrote the paper by Vedenov, Velikhov, and Sagdeev from which I learned the quasi-linear theory of weak turbulence in plasmas.

informed tour to a nearby tea plantation. After that one-day delay, we were told we had to be at the Sochi airport at 5 a.m. for the only flight to Moscow. Waiting on the tarmac when we arrived was one of the huge double-deck Ilyushin transports, which we were told to board immediately. We duly climbed to the upper passenger deck. We were nearly all the passengers flying that day; there were many empty seats. The airplane did not take off until about 4 p.m., eleven hours later, during which time we heard laughing voices in the lower deck, people coming and going. A month's allotment of airline food and drink for the month's passengers had arrived at the Sochi airport, and the crew and airport staff were partying while dividing the stash up among themselves. That day's passengers never got food or water, nor did any others that month. Our pilot was visibly inebriated as he shuffled down the aisle to the cockpit to fly us to Moscow.

My only official trip to Moscow as NASA Associate Administrator came in 1995. By that time, NASA officials were required to be met at the airport by an armed driver with an unmarked, nondescript car. Mine had a beat-up Volvo; my driver was instructed to take a circuitous route to the US embassy. He looked around nervously throughout our trip. On our arrival, the gates of the embassy swung open, let us in, and quickly closed. Once inside the embassy courtyard, I was directed to another NASA employee in the cafeteria. The poor man was on the verge of tears. An engineer from Texas, he had been in country for about two months, his wife had only recently arrived, and their furniture had come a couple of days before. NASA had rented an entire apartment building for its employees where this husband and wife stayed in sparse circumstances waiting for their furniture. Thieves turned up the moment the furniture arrived, held the engineer's wife knife-at-throat, and diverted the entire shipment as it was unloaded. A second shipment was due the next day and when the same thing happened, the poor man's wife told him that not only was she leaving Russia, she was leaving *him*. An informed guess is that one of the *dezhurnayas,* the "hostesses" who monitored comings and goings on every hotel floor, alerted gangster friends to the furniture shipments.

I can see why by the turn of the millennium many ordinary Russians preferred Vladimir Putin's restoration of orderly criminality to continuing with Boris Yeltsin's entrepreneurial chaos. By the mid-1990s, many of my colleagues had joined the diaspora of Russian scientists in the West. Rashid Sunyaev went to Garching in Germany; Volodya Krasnoselskikh to Orleans in France; George

Zaslavsky to New York University; Michael Gedalin to Israel; Ildar Khabibrakhmanov to IBM; Mischa Malkov, Vitali Shapiro, and Yuri Shevchenko to the University of California San Diego; and even Roald, their leader, to the University of Maryland. In 1993, Mischa Malkov and his pregnant wife, Olga, stayed in Ellen Lehman's house in Pacific Palisades. At our urging, they chose to have in America the child they had tried for and failed several times in Russia. Their baby, Anna, got American citizenship, and Mischa ultimately joined the research staff in the UC San Diego Physics Department. He and Olga now live next to the Pacific Ocean in the town of Oceanside.

Russia has lost its scientific and personal attraction for me. I have not traveled back since the mid-1990s, but it would have been nice to catch up with the friends who stayed—especially my Trieste officemate, Alec Galeev, who succeeded Roald Sagdeev as Institute Director, and Lev Zelenyi, Alec's student, who succeeded Alec. I chose not to go to Lev's sixtieth *Sputnik* anniversary celebration in 2017. I missed seeing him, but I had no desire to go back to Moscow.

# 20

# My Soviet Colleagues' World Collapses

It was *glasnost:* my Soviet friends were relieved to get their stories off their chests, especially to an American. You could feel the emotional toll that the USSR's death agony was taking on my friends. These are some of the stories they told.

Alec Galeev once proudly showed me a photograph of his high school graduating class in Alma Ata (now Almaty), Kazakhstan. One among the three rows of smiling teenage faces had been scratched out with a sharp point; who did that, I asked. *My mother,* Alec replied. *The guy was an enemy of the people.* Such was the faith that ordinary people once had in communism.

I especially remember a story told by Lev Zelenyi[71] because I can visualize what happened. There was a deserted Russian church with an unkempt courtyard 500 meters south of the Academy Hotel on October Square. The church's courtyard was notable for its overgrown grassy mounds in casual disarray. Afflicted with travel insomnia, I would retreat to the eerie quiet of the courtyard with the grassy mounds to escape the early morning streetcar clatter next to the Academy Hotel. When I described the courtyard and its eerie quiet and grassy mounds to Lev, he said *I know that church. You must be its only visitor*. (It was true, I never saw anyone else there). *You*

[71] Lev Matveevich Zeleny, aka Zelyoni, (1948-); Space plasma physicist, Student of Alec Galeev and Galeev's successor as Director of IKI, now the Russian Space Research Institute.

*probably remember also walking by the children's toy store, Detsky Mir?* Just across from Detsky Mir is Lubyanka prison; in Stalin's time, they shot prisoners there and dumped their bodies in that courtyard almost every night. People would come in the morning to bury them. *Those grassy mounds are the prisoners*. *Nobody ever goes there.*

One Saturday morning, on emerging from the Moscow subway with a young colleague, Ildar Khabibrakhmanov,[72] I spotted a line of people carrying tied packages of newspapers, patiently waiting in the rain outside a bookstore. What are they doing, I asked Ildar. Recycling, he replied; they get books in return. The books are the government's way of saying it is safe to throw newspapers away. It is a reaction to things that happened in Stalin's time. He went on: in those days, people saved newspaper accounts of Stalin's speeches. You at least saved the issues in *Pravda* with articles about Stalin, though to be extra careful you saved other newspapers, too. Some people carefully cut Stalin's speeches out and put them in scrapbooks, but others just kept every issue of *Pravda* in piles next to the walls of their apartments. You dared not throw away a newspaper that might have something Stalin said in it; your neighbors would denounce you. Besides, the newspapers were insulation against the cold.

My 1986 visit to Troitsk, a secret Soviet city, was memorable. In preparing papers on electrostatic electron cyclotron waves with Maha Ashour-Abdalla in the 1970s, we had discovered that Vladimir Karpman had done good work on the same topic; I was also familiar with his book on non-linear waves in dispersive media.[73] I asked Sagdeev to arrange a visit to Karpman. At first, Roald's staff were hesitant and made vague promises. They seemed surprised when they got permission, and one day a black car with flags arrived to drive me to Karpman, about two hours outside Moscow. I was struck by how many bent-over peasant women clothed in black were walking by the road, not taking public transportation; Moscow was, despite the unfavorable comparison with Western capitals, an island of prosperity. When the official car arrived at Troitsk, the broad gates to the Institute where Karpman worked swung open and I found ten or fifteen people waiting inside the courtyard, surrounding Karpman,

[72] Ildar had just finished his first degree in space physics and now works as a software engineer at the IBM Watson Research Center.

[73] V. I. Karpman, *Non-Linear Waves in Dispersive Media: International Series of Monographs in Natural Philosophy* (Oxford, United Kingdom: Pergamon, 2016).

who was holding out his hand in greeting. He and I were meeting for the first time, though we knew one other by reputation.

The crowd listened attentively as Karpman and I started discussing physics then and there; Maha Ashour-Abdalla had verified some of Karpman's analytic results with her numerical calculations, so there was much to talk about. The crowd walked along with us as we continued the physics discussion inside a cafeteria, listening to our English with evident understanding. It was clearly important for them to hear everything, for reasons, I assumed, of science, curiosity, and security, or all three. Tears were in Karpman's eyes when I stepped into the black car for the slow trip back to Moscow: one of Karpman's colleagues whispered in my ear that Karpman had learned the night before that his father was on his deathbed but had delayed his departure for home until after my visit. I could not imagine that a simple scientific visit could mean that much.

*Glasnost* continued to unfold and several years later, Karpman and I were able to have dinner together at the Rossiya Hotel in Moscow without the congregation of listening ears. Karpman then told me about his father; the relief in being able to tell his story outside his family was palpable. The story was about his father's sleeplessness. Karpman's father rarely got enough sleep during World War II. Despite being a Jew, he had been deputy mayor of Minsk; he attended to his regular duties during the day and at night he slept in his office with Stalin's red telephone by his side. Stalin was said never to sleep. He would call government officials all over the Soviet Union late at night, and if they were not there to answer he could have them arrested and killed; what's more, the secret police would go after their families and friends. Karpman's father's insomnia was one more thing that convinced me that Stalin's USSR had been a truly evil empire, just as Ronald Reagan had said. Brezhnev's sclerotic USSR was not far behind.

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It was spring vacation in Princeton, and I invited my son, Matthew, to join me for a few days in the USSR. Roald Sagdeev's striking redheaded daughter Anya was recruited to show Matthew the sights in Moscow. I had fantasies they might fall for one another but wishing did not make it so. I had previously scheduled a visit to a cosmic ray physics institute in Leningrad (now St. Petersburg) so that Matthew could visit Leningrad. Matthew and I left Moscow in time to catch the departure of the 9:30 p.m. overnight express to Leningrad, the *Red Arrow* (*Krasnaya Strela*). Upon our arrival at Moscow's Leningrad station, we found ourselves staring at the posted train schedules, transliterating the Cyrillic to find our track, when a deep voice behind us said: You want track twenty-three. When we turned around, the deep voice had blended into the crowd. Foreign visitors never needed fear being alone in the Soviet Union.

When Matthew and I arrived at our Leningrad hotel at 9:30 a.m. the next morning, its cavernous bar was already filled with a boisterous crowd handing opened bottles of vodka to one another over their heads. They were not yet so far gone that they were spilling it on each other, but they were close. These were Finnish tourists who made a day trip for the cheap five-ruble vodka; their boat from Helsinki had just arrived at the dock in front of the hotel and they started drinking at once. When we got back to that hotel that afternoon, their boat was about to depart. The bar had emptied out and its odorous rug was covered with broken bottles. The hotel staff was resignedly cleaning it up—apparently all in in a normal day's work at that hotel.

The trip to Leningrad was more interesting than I found the physics there. I had a stiff conversation with the director of the cosmic ray physics institute, a typical *apparatchik*. Matthew and I then returned to Moscow so he could fly back to Princeton to finish his academic term. I lingered in Sheremetyevo airport to watch his takeoff; armed soldiers were on the tarmac patrolling his airplane. The little voice inside said maybe Matthew knew something I didn't; it would be weeks until I could return to America and freedom. I countered this gloomy thought with the cheerier one that I would soon visit Jumber Lominadze in Tbilisi (Tiflis) in Soviet Georgia. At last, I might get some good food.

Unlike the Karpman visit, it was a piece of cake for Sagdeev's staff to arrange a visit to Jumber Lominadze[74] in Tbilisi. Go, Roald said, the food and wine are the best in all the USSR, and Lominadze will show you a good time. Lominadze was Secretary of the Georgian Academy of Sciences; by controlling the Academy's words sent to the bureaucrats, he controlled the Academy. Others were happy to leave him this dismal labor. Jumber met me at the Tbilisi Airport with a

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[74] Jumber Lominadze (1930-2014), astrophysicist, Secretary of Georgian Academy of Sciences, politically active when Eduard Shevardnadze was president of the independent country of Georgia.

squadron of three black limousines, all with flags, the first for the two of us and the other two for the highly conspicuous security detail that followed us into town. He never went anywhere without this pod of sycophants. Jumber's apartment in Tbilisi had been assembled from two or perhaps three separate units. It was large enough that he could invite forty to lunch every day, which he did each of the four or five days I was there.

Lominadze had become a Georgian knock-off of a Russian oligarch and was acting like one. There was an apparently famous circus troupe visiting Tbilisi at the time, and Jumber invited the whole lot to lunch too. He told me that the ten or so ladies in black wearing kerchiefs doing the cooking for the big crowds at lunch were from his wife's family farm.

I met Lominadze's sister at one of his luncheon galas. She spoke French so it was easy to converse with her, and she volunteered to take me on a tour of Tbilisi. She met me in an ear-to-ankle fur coat. When we got to Tbilisi's main cathedral, she related that the Georgian Orthodox Christian Church was founded in the third century CE, before the Roman Catholic Church. She added that the Islamic faith was founded later, in the seventh century of the Christian Era. Then she said: *We Georgians started killing Muslims then, and I see no reason to stop now*. Muslim Azerbaijan is only 548 km from Georgian Tbilisi.

We were to celebrate the end of my stay in Tbilisi with a party at Lominadze's wife's farm about an hour's drive out of town. On the appointed day, Jumber dispatched his entourage to deliver a lamb to be cooked all morning in an underground pit, and he took me to a museum honoring Georgian artists to pass the time. When we arrived at the farm in mid-afternoon the party was already on full-blast, and we were handed large stone goblets into which waiters wearing red caps with tassels poured a thick red wine from goatskins.

The lamb was plenteous, the waiters omnipresent, the wine endless, the crowd boisterous. Not used to such a rich life, I asked Lominadze if we might escape the din and catch some air. He took me for a walk in the agricultural field behind his wife's farmhouse. We engaged in the babble that a forgiving person would let pass for intelligent conversation in such a circumstance. Several hundred meters into the field, Lominadze spotted an old lady in black bent over with a hoe; she might have been ninety years old. Come meet her, he said. I have the impression that I got the gist of their conversation in the Georgian language, but out of consideration for his visitor, he translated into English. Grandma, he said to her, meet an American physicist. Tears dimmed her eyes as she reached to touch my sleeve. She pulled at my shirt, too: I never thought I would ever meet an American, a physicist no less, she said; I never thought I would see such a day; now I know my grandchildren will be safe.

I didn't think I would ever see such a day, either. Nor have I seen one since.

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Those of us of a certain age cannot forget the "beep-beep" from humanity's first orbiting earth satellite, *Sputnik,* in 1957. My parents heard the magic beep-beep; it softened their reservations about my going into science. *Sputnik* awakened a lethargic American public to the shocking possibility that the Soviet Union could overtake the United States in science and technology. This fear amplified the uneasy military confrontation that the US and the USSR inherited from World War II that we called the Cold War. Within two years, a mobilized America created the vast programs of education and research in which I came of professional age. I had thought no further than becoming a Boston lawyer like my father, but the beep-beep—the Cold War's siren song—was heard everywhere, and 1959 found me enlisting in programs of controlled thermonuclear fusion and space plasma research, both thought important to winning the Cold War. I progressed along the path designed for people like me, and in the fullness of time, I became a professor of physics at UCLA and a consultant for a satellite manufacturer, TRW Systems, in the aerospace capital of the Western world, Los Angeles.

What the other side was doing behind the wall of secrecy held a kind of mystical fascination for those of us in the field, and having been to the Soviet Union was a mark of professional distinction. It was on my first or second trip in the 1970s that I met the man who created *Sputnik's* beep-beep. Konstantin Gringauz[75] was the last person to touch *Sputnik* when he fastened into position the radio transmitter his group had built. If anyone was a true hero of the Soviet Union, Gringauz was. He had been trapped in the siege of Leningrad during World War II. Later in the war he designed small radio transmitters for use by the Soviet tank corps. This attracted the

[75] Konstantin Iosifovich Gringauz (1918-1993)

attention of the father of the Soviet Space program, Sergei Korolev,[76] with whom Gringauz studied for an advanced degree after the war. Even gruff matter-of-fact Gringauz had to admit it was surreal to visit his thesis advisor in prison. Stalin controlled scientists, who were suspected of being free-thinking, that way.

Korolev had given Gringauz the responsibility for achieving one of the most important political goals of *Sputnik*—international radio communications from space. When I met Gringauz at IKI, he was still doing important research on the propagation of radio waves through the ionosphere into space. Before *Sputnik,* the feeling had been that the plasma layer in the ionosphere would prevent radio waves from a satellite above from reaching the ground, but Korolev and Gringauz reasoned that the waves would find a propagation path from the satellite to the ground through the layer's chaotic irregularities in plasma density.

Gringauz and I shared an interest in art, and I asked him if he would take me to Moscow's Pushkin Museum. Why do you want to go to the Pushkin, he protested. It is full of impressionist art collected by rich capitalists; you can see that kind of art any time you want in Paris or New York. What you cannot see anywhere else is Socialist Realism; we are going to the Tretyakov Gallery! What followed is one of the most arduous days of my life. We lined up at the Tretyakov before opening time and left at closing time. Without a break, Gringauz spent a full fifteen minutes before each picture, giving an accomplished art history lecture (in English!) on communist iconography and the different schools of Socialist Realism. It sounds unlikely, but under his tutelage, I came to appreciate a style of art that was dismissed by *cognoscenti* in the West. Gringauz's passionate dedication to the ideals of communism shone through every word he said that day.

I saw Gringauz many times after that, but as we got older our visits to museums got shorter. The last two times I saw him stand out in memory. Gringauz and I had been invited to speak at the symposium in Stockholm honoring the award of the 1989 Crafoord Prize to James Van Allen by the Royal Swedish Academy of Sciences. Restaurants in Stockholm were expensive even for Americans, and my wife-to-be, Ellen Lehman, and I noticed that he avoided joining the speakers' group at dinners. Was he even eating? We contrived to

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[76] Sergei Pavlovich Korolov (1907-1966), chief rocket designer and father of the Soviet space program.

order pizzas to share with him in the conference hotel, which he accepted as a matter of fact, but clearly his pride took a hit. The last time I saw Gringauz was in Moscow in 1991. We did not go to a museum this time, but he took me to a circus, another specialty of Soviet life; we stayed only until intermission because he was getting bored. We went outside and sat on a bench under a stand of trees to wait for a bus to central Moscow. Then it all came out. Eyes glistening, shoulders bent, hands trembling, voice quavering, he who once so proudly proclaimed the virtues of socialist realism admitted he had lost faith in communism. He used to think communism's defects were due to the undisciplined Russian character and not to communism *per se,* but every place communism had been tried—even in sunny Cuba—it failed to produce wellbeing for the masses despite the most admirable of intentions. There was, he said, something in communism that ran contrary to human psychology.

The Soviet Union disintegrated in December of that year and the man who made *Sputnik* go beep-beep died in 1993. I still hear his beep-beep.

# 21

# Farewell to Space Plasma Physics

I should have been happy. I had been elected to the National Academy of Sciences in 1991 after half a decade of annual attacks of status anxiety at election time. The UCLA Physics Department gave me a lovely party after the announcement, and my family made a happy reunion out of my formal induction to the Academy a year later, yet these celebrations left me with an eerie feeling of emptiness.

The void beckoned again. Perhaps it was time to begin a serious scientific treatise. I started a deeply theoretical one on the magnetohydrodynamics of planetary and astrophysical systems but abandoned it halfway through. I wanted desperately to tell others that the subject I had devoted my life to has the generality, precision, and elegance of the quantum electrodynamics I had encountered in graduate school. When I could not, I blamed my lack of mathematical talent; my disgust with myself came out in itchy squirmy sensations.

I abandoned the theoretical treatise for an essay of historical character that did get finished.[77] No mathematics anywhere, this book became an account of the observations underpinning the then modern understanding of the earth's magnetosphere and the *aurora borealis,* a synthesis of observations made on the ground over centuries and those made in space since the beginning of the space

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[77] C. F. Kennel, *Convection and Substorms: Paradigms of Magnetospheric Phenomenology* (New York: Oxford University Press, 1996).

age. I could use history to try to legitimize my life's work, if not with the elegant precision of mathematics.

**Figure 21-1: Family celebration on my induction into the US National Academy of Sciences, Washington, DC, 1992. Shown left to right in the front row are my Aunt Mary; me; my son, Matthew; Ellen's sister, Jo Brandt; Ellen Lehman; Ellen's father, Bud Lehman; and my Aunt Charlotte Queeny. In the back row, left to right, are Jo Brandt's husband, Bill; Ellen's mother, Jane Lehman; my brother, John Kennel; and my uncle, Jim Queeny.**

I had made summer visits to the Geophysical Institute of the University of Alaska between 1988 and 1993 to get a feel for how observations are made on the ground and for who makes them; to go where work is done that explains things you can see with your own eyes; to read in my Alaskan colleagues' eyes where uncertainty remains. It was the kind of feel for a subject that you cannot get from the literature or committee meetings.

Modern space scientists have found a new kind of answer to a question that occupied philosophers and astrologers in centuries past: do events in the skies affect human affairs? Philosophers had thought, along with Aristotle, that a special kind of material fills space—ether—which transmits the influence of events in space to the earth. Ancient philosophers sought to identify those events. They thought, for example, that the planets influence human affairs by transmitting thus far unheard signals through the ether. If so, observers on earth might be able to infer something about the future from the planets' apparent positions in the sky.

By carrying measuring instruments into space, satellites were bound to change the view of what is in the space surrounding the earth. The new celestial material we call plasma. Modern solar system plasma physics has its origin in the mid-nineteenth century, when scientists started detecting slight variations in the earth's magnetic field using navigational compasses. The magnetic variations proved to be telltale signatures of a specific kind of event on earth. In 1859, the British astronomer Richard Carrington observed a bright solar flare, followed a few days later by what is now called a magnetic storm. What connected the flare on the sun to the storm on earth was not made clear until Konstantin Gringauz's experiment onboard the Soviet Spacecraft *Luna 1* detected the solar wind in 1959, a century after Carrington. We now know that plasma flowing outward from the sun interacts with all the planets in the solar system, carrying the variable solar magnetic field with it. The compasses were picking up the changes in magnetic field induced when the solar wind, suddenly strengthened by the solar flare, collided with the earth's magnetic field.

We also know where the solar wind ends and interstellar space starts. There is a discontinuity in the solar wind magnetic field and plasma density—a shock wave—where the solar wind collides with the interstellar medium. It was the magnetic field variations at this far outer plasma boundary of the solar system that told us that the Voyager II spacecraft had entered interstellar space in 2018, forty-one years after its launch on my birthday in 1977. We also know, as we did not in 1995 when I published *Paradigms*, that many stars have stellar winds and planets. Recent results from the James Webb Space Telescope support the view that most nearby stars have planets. Those planets with magnetic fields should have *magnetospheres*, where the planetary magnetic field forces the wind from the parent star to flow around the planet.

In the 1980s, there was much that we did not know about the solar system and nothing about extrasolar planets, but we had already learned a great deal about the solar wind's interaction with the earth's magnetosphere. Thirty years after Konstantin Gringauz's *Luna*, the driving question for terrestrial magnetospheric research had been refined to: how are variations in solar wind dynamic pressure and magnetic field related to the observed changes in the structure and behavior of the earth's magnetosphere and aurora? This form of the question was the starting point of *Convection and Substorms: Paradigms of Magnetospheric Phenomenology*.

From reading Thomas Kuhn's *The Structure of Scientific Revolutions*,[78] I realized that space scientists had been working out details of two modern paradigms. We were using provisional organizing concepts, paradigms to Kuhn, to guide us through the complexity of observations in space and those made from the ground to which they are linked. The magnetospheric research community gave the paradigms names, *Convection and Substorms,* the title of the book that I was inspired to write by working with colleagues at UCLA who were observing plasmas in space, and colleagues at Alaska's Geophysical Institute who were observing the *aurora borealis* from the ground. I wanted to see the extent to which observations supported the theoretical picture generated by Ian Axford, Harry Petschek, and George Siscoe at the Petschek summer seminars.

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When they have no other information, scientists often picture space as empty, even if it is not, when it is harmless to ignore the irrelevant things in it. If we learned anything from the first satellite observations, it is that outer space is almost but not quite empty; the space around earth is full of electromagnetically active plasma. From the human point of view, the plasma is almost not there. The best laboratory vacuums have a particle density millions of times larger than that of the solar wind (five protons per cubic centimeter). Humans had never encountered anything so tenuous before; it was as close to nothing as you could get without being nothing. And yet we space physicists were claiming that this almost-nothing influences events on earth.

The space physics community made sure to phrase its key question in a form it could answer. Could variations in the magnetic field and plasmas in the space around the earth be transmitted along the earth's magnetic field lines to the earth's atmosphere? If so, the main effects would be found near the north and south magnetic poles. The earth's polar atmospheres are like television screens that show

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[78] T. Kuhn, *The Structure of Scientific Revolutions* (Princeton, NJ: Princeton University Press, 2021), pp. 176-77.

blurry images of what is happening in space on the magnetic field lines that connect to the aurora. Only with the greatest luck could a spacecraft be on the field line where interesting things were happening in the atmosphere, but by following auroral activity with cameras on the ground you might be able to fabricate a picture of what is happening in space. You would have to do a mathematical mapping along field lines from the atmosphere to space, but given how spottily satellites sampled the space environment, it was worth the effort. The aurora was a symptom of changes in a magnetosphere that you could not see all of.

The main goal of the Petschek summer seminars was to understand the interaction of the solar wind with the earth's magnetic field using modern concepts from plasma physics. The hot plasma in the sun's outer atmosphere—the solar corona—expands into space forming a wind that carries some of the sun's magnetic field with it. The University of Chicago astrophysicist, Eugene Parker, had taken insights of Syun-Ichi Akasofu's mentor, Sydney Chapman, and fashioned the definitive theory of the solar wind in the late 1950s.[79] That wind is sometimes a breeze, sometimes a hurricane, but it is always there. It interacts with each of the planets in a distinctive way. The solar wind is deflected around the earth by the geomagnetic field and an even lower-density cavity surrounding earth—the magnetosphere—is created. Vast electrical currents flow in the boundary between the magnetosphere and solar wind to adjust the changes in the wind and the geomagnetic field to one another. Some of these currents flow along geomagnetic field lines into and out of the earth's upper atmosphere and ionosphere at high northern and southern latitudes. The currents cause the night sky in those regions of the earth's atmosphere to light up. The *aurora borealis* and *aurora australis* are signatures of events in the space surrounding the earth.

The founding campus of the University of Alaska is in Fairbanks, close to the geomagnetic latitude of maximum frequency of auroral occurrence. This, along with Alaska's volcanoes, earthquakes, and glaciers, makes Fairbanks a prime location for the country's only permanent geophysical research station in the Arctic. The Geophysical Institute was founded by congressional legislation

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[79] E. N. Parker, "Cosmic-Ray Modulation by Solar Wind," *Physical Review* 110, no. 6 (1958): 1445-49; E. N. Parker, "Interaction of the Solar Wind with the Geomagnetic Field," *The Physics of Fluids* 1, no. 3 (1958): 171-87.

in 1946. The great British geophysicist, Sydney Chapman, who postulated the existence of the solar wind in the 1930s before its discovery, saw the Geophysical Institute as a place where you could use observations of auroral activity to understand the origin of magnetic storms and other more subtle variations of the earth's magnetic field. His work was continued by his student, Syun-Ichi Akasofu, the Director of the Geophysical Institute when I made my summer visits (1988-93). Akasofu had characterized a subtle event he called a substorm to distinguish it from Chapman's magnetic storm. Chapman's magnetic storms are due to a sudden increase in the mass flux of the solar wind, and Akasofu's substorms, to a sudden shift southward in the direction of the solar wind magnetic field. Understanding what happened to the magnetosphere after the southward shift was the main issue that preoccupied the Petschek summer seminars.

Numerical computations were not yet up to calculating the changes in the geomagnetic field and aurora that followed the southward shift, and Akasofu worked empirically, relying initially on sequences of photographs of the aurora taken from the ground. This simpleminded empiricism did not make him popular with his space physics colleagues, who hoped for a higher standard of rigor. Nonetheless, Akasofu had invented an idea that preoccupied much of the magnetospheric research community. I wanted to see what was in Akasofu's idea beyond inspired guesswork and had many arguments about substorm mechanisms with him and his protégé, Joe Kan. I also hoped to absorb the ethos of observers who worked on the ground and not on satellites. Magnetospheric physics was in its pre-rigor assembly phase; researchers were hoping observations would pin down issues of principle enough to enable them to design a program of research with sufficient rigor to meet the standards of more developed fields. There were many kinds of observations, and we really did not know which would be decisive; we needed a way to sort them into relevant and irrelevant bins. That was where Akasofu's substorm became a paradigm to be tested; if observations seemed consistent with it, we could sort them into the consistent evidence bin. Akasofu, thinking he was tweaking Sydney Chapman's ideas, had invented a notion that every auroral and magnetospheric observer was checking out.

I participated in the gestation of the second paradigm of space plasma physics by listening to Ian Axford, Harry Petschek, and George Siscoe as they worked together in the Petschek summer seminars in the 1960s. The interaction of the solar wind magnetic field with the magnetosphere sets the plasma inside in motion, and the coupling of motions inside the magnetosphere to the ionosphere in the upper atmosphere shapes the behavior of the aurora. Ian Axford called those motions *convection*, and that was the term the Petschek summer seminars adopted during the discovery years. Axford's convection picture became another paradigm used to organize the magnetosphere's phenomenology. The two paradigms were closely related since the event that triggers a substorm sets the plasma inside the magnetosphere into convective motion.

The book *Paradigms* summarized the observations organized by the convection and substorm concepts, but it did not assess the extent to which theory was tested quantitatively, which would have required numerical computations. The book was all about the pictures in my colleagues' minds when they examined data and how they referred to their mental pictures when they met at meetings. By committing to Kuhn's paradigm concept, I was committing to sifting evidence as a social scientist would, examining each piece for consistency with the two guiding hypotheses rather than seeking a computational formulation that would replace the need to consult a whole raft of evidence. I was not seeking physics glory; I was seeking justification of the course my research life had taken. The book would be outmoded from the beginning, but I imagined that historians might take an interest in how the observations that had organized the field were viewed before rigorous theory was possible. Moreover, my extraordinary student Vassilis Angelopoulos was writing his pioneering PhD thesis, and I was quietly checking out the observations so that my advice to him would have a sound perspective.

While the laws of plasma physics have not changed, today's space plasma research employs an organizing methodology that combines the substorm and convection paradigms. It replaces data analysis by assertion of plausible hypothesis with case-by-case numerical modeling. In the thirty years since *Paradigms* was published, there have been many more spacecraft and a longer and larger data record. The precision of today's measurements was not only unheard of in the 1980s but would have been useless given the way the data were analyzed at the time. Space plasma physicists no longer ask whether the data fit Akasofu's substorm model or Axford's convection picture, they ask whether their data are explainable by numerical modeling of events in the magnetosphere and aurora. This capability makes the field ready to predict the weather in space. I first heard the term "space weather" from old friends, George Siscoe and Lou Lanzerotti, who came to Washington when I was with NASA to advocate programs to forecast plasma events in the solar system. Lanzerotti, who had absorbed the practical ethos of AT&T's famed Bell Labs, was the most aware of my colleagues that magnetic storms create serious hazards to communication spacecraft in orbit and long-line communication on the ground.

*Paradigms* was emblematic of an intermediate stage of research. Without the consolidation of a secure empirical picture, space physicists would not have had the confidence to propose expensive space weather programs and governments the courage to fund them. While predicting space weather is more prosaic than what ancient philosophers or sixteenth-century astrologers aimed to do, space physicists now routinely use observations of the sun and solar wind to forecast magnetic storms, substorms, and auroral displays on earth. Auroral forecasts are shown on Alaskan TV. My research had become as routine as weather prediction, a sign of the maturity of the subject. This comforting sentiment was probably in the back of my mind when I was writing *Paradigms*. In any case, I was writing its dedication to Ellen at the very moment Dan Goldin, the NASA Administrator, called from Washington.

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Vassilis Angelopoulos is a natural experimentalist. During his PhD studies in the late 1980s, he collaborated with many experimental groups in the US and abroad. My role as his thesis advisor was to point out interesting things to do, to use the insights acquired from working on experiments with Fred Scarf, and to use the savvy I acquired from government committees to guide Vassilis in the organizational intricacies of experimental research done in space. In a repeat of my intimate conversations with Harry Petschek and Ferd Coroniti, Vassilis and I spent hours gossiping about his work on what he called "bursty" bulk flows, as well as the people and organizations who were getting the data he used. I poured into these conversations all that I had learned about the science and politics of space physics. I did not hold anything back because I was unconsciously preparing to leave the field.

Vassilis' PhD thesis was the beneficiary of two decades of worker-bee routine work. He was able to access different data sets acquired by different spacecraft to exploit a key difference between plasmas and ordinary fluids. Theoreticians like myself often use the mathematics developed for ordinary fluid flows to describe the bulk motions of plasmas, even though we know what we do is a first approximation. To that first approximation, fluids have viscosity and plasmas have none. Viscosity tames fluid behavior; it smooths irregularities in fluid flows while plasma flows can be impulsive and inhomogeneous. Or bursty, Vassilis said. He constructed data sets in forms that enabled him to diagnose flows at the plasma level for the first time. His paper on bursty bulk flows in the earth's magnetosphere is the third most cited in my Google Scholar bibliography.[80] It accounts for numerous puzzles in the interpretation of observations, but I think its significance for understanding the inherently impulsive spatio-temporal structure of plasma flows has been overlooked.

Vassilis, like Mary Hudson, went to UC Berkeley after his degree, where he became an unusually youthful principal investigator of a spacecraft project, *Themis*. Vassilis, like Mary Hudson, won the AGU Macelwane Award for achievement before the age of thirty-five. He has returned to UCLA as a professor in the Department of Earth, Planetary, and Space Sciences. I like to think of him as my space plasma physics successor. Indeed, Vassilis recently replaced me as US representative to COSPAR, the international Committee on Space Research. Typically, he made an audacious proposal to this group of graybeards at his first meeting.

I gave the seventy-fifth UCLA Faculty Research Lecture, the Academic Senate's highest faculty honor, in the spring of 1993. Here was an opportunity to tie up the different threads of my career before a friendly audience of humanists, social scientists, lawyers, and doctors, as well as scientists and engineers. If the work I had been doing had any human meaning at all, this was the time to say so. This was the opportunity to place modern space plasma research in its historical and cultural context. I gave it my all. I connected the early history of solar terrestrial physics with the progress made by modern space plasma physics. The lecture was a tribute to my colleagues at Avco, UCLA, in Alaska, and in Russia. There were lots of drawings

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[80] V. Angelopoulos, W. Baumjohann, C. F. Kennel, F. V. Coroniti, M. G. Kivelson, R. Pellat, R. J. Walker, H. Lühr, and G. Paschmann, "Bursty Bulk Flows in the Inner Central Plasma Sheet," *Journal of Geophysical Research: Space Physics* 97, no. A4 (1992): 4027-39.

and diagrams, historical woodcuts and prints, modern line drawings, photographs of the aurorae taken from the ground at the University of Alaska, and from the Space Shuttle by Ellen's cousin, astronaut Jay Apt.

I started the Faculty Research Lecture with an image of Ellen Paneok's scrimshaw of a native Alaskan hunter with an aurora overhead keeping him company in the polar night. I concluded with warm thanks to my colleagues and UCLA for having given me so interesting an academic life. My feelings were genuine, yet a perceptive observer could not miss the elegiac tone. Not many in the audience would have known the reason why; I was about to be installed as Associate Administrator for Mission to Planet Earth at NASA headquarters, never again to return to research in space plasma physics.

**Figure 21-2: This scrimshaw etched on a small piece of walrus ivory was made by Ellen Lehman's and my Native Alaskan friend, Ellen Paneok, the first female native Alaskan bush pilot. Photo by Ellen Lehman.**

Recently, I engaged in an e-mail round-robin with my equally venerable space plasma physics colleagues Jacob Bortnik, Richard Horne, and Bruce Tsurutani. They evidently had been gossiping about the past, and Richard formulated the question that had been bothering them: I know you did all that work on space physics a while ago and I know you moved on to do other things such as Director at

Scripps and more on climate. Was there something in particular that happened that made you want to change fields? Or was it something gradual, or a desire to move more into management or position of influence?

This in part was my answer: You ask a question that I have asked myself many times and have gotten different answers each time. Richard has proposed one of the answers. With the prospect of empty pandemic time looming ahead, I decided to write about something I still knew something about—me—and the result came to 400 pages in which I still was unable to answer Richard's question. It is the best I can do, and it is still not good enough.

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There is no such thing as a clean break with one's past. In the coming years, I became an elder statesman of space science. I was to be the rare theoretician to serve as Chair of the NASA Advisory Council; I had published at least one paper in the four main areas of space research—space plasma physics, planetary science, astrophysics, and earth science—and had accumulated enough name recognition to chair the National Research Council's Space Studies Board. I also served on a Presidential Commission on the future of human space flight during the Obama Administration.[81] All this lay in a future whose day-by-day routines were of a totally different character.

Farewell to research in space plasma physics also meant one-by-one farewells to friends and colleagues as the professional opportunities to meet faded away. The most profound of these partings came in April 2003 at a meeting of the Scientific Advisory Board of the Geophysical Institute in Fairbanks, Alaska, the last time I saw my first physics mentor and my first scientific soulmate, Harry Petschek. Harry was to die in 2005. This time our parting was for good.

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[81] My recollections of my involvements in planning NASA space projects, including the Voyager Missions, the Hubble Space Telescope, the International Space Station, the Space Shuttle, the Falcon Space X launcher, and the James Webb Space Telescope, were published on August 5, 2022 by the American Geophysical Union: C. F. Kennel, "50 Years in Space Science," *Perspectives of Earth and Space Scientists* 3, no. 1 (2002).

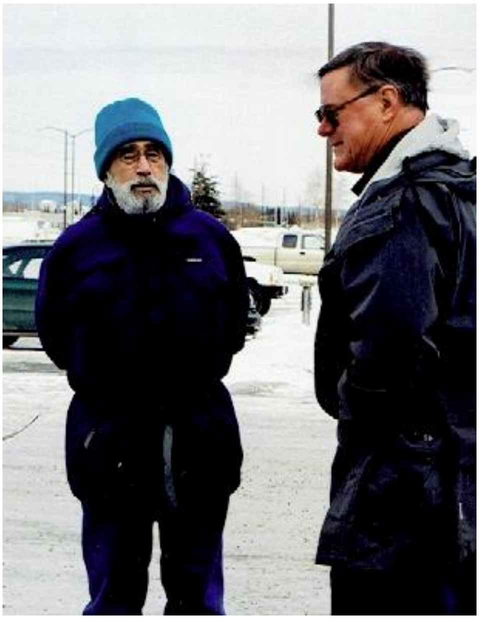

**Figure 21-3: Charlie Kennel (right) again listening to Harry Petschek (left) in the snow outside the University of Alaska's Geophysical Institute in Fairbanks in April 2003. This was the last time I was to see him. Photo by Ellen Lehman.**

# 22

# Close Encounters with Physicists of the Best Kind

Over the years, I have had memorable encounters with physicists who were heroes when I was learning the subject. Any historian of science will tell you that the reminiscences of senior scientists are filtered through decades of progressively faltering memory. What survives is overlain by later impressions and altered by frequent retelling; facts are left out, invented, or rearranged for dramatic effect. Why did this anecdote stick in the storyteller's mind? How was that story distorted by the wish that was the reason for its preservation? In my case, I preserved these anecdotes to convince myself that I was a member in good standing of the physics fraternity.

My PhD completed in fall 1964, I returned to the Avco-Everett Research Laboratory where Hans Bethe (1906-2005) was a consultant. Bethe visited infrequently from Cornell and the scientists at Avco jealously competed for time with the great man, who was to win the 1967 Nobel Prize in Physics.[82] I was flattered to be put down

[82] Other physicists wondered why Hans had taken so long to win the prize. The prize rewards a singular achievement, not general excellence, and the Nobel committees had difficulty choosing which of Bethe's achievements to cite; their

on Bethe's schedule on one of his visits. I recall walking into the sacred interview room to be greeted in a gruff German accent: Zo, you are Kennel, ze plazma physicist vrom Prinzeton? I remember vhen I vas an AEC commissioner und Lyman Schpitzer came to us vith his proposal for ze Shtellarator; I vas skeptical, because I beliefed zat something zat far out of termal eqvilibrium vould find a clever vay to ezscape to ze valls. Tell me, how is the project doing? That broke the ice, and Bethe and I had a good discussion about both fusion and space plasmas. I had to tell him that so far his skepticism about fusion was spot-on.

In 1967, after I had been in the UCLA Physics Department for a few months, I was summoned to the Director's Office at the UCLA Institute of Geophysics and Planetary Physics (IGPP). Willard Libby's (1908-1980) purpose was to invite me to join IGPP, which I did a few years later. On entering Libby's office, I was greeted in a flat American accent: So, you're Kennel, the plasma physicist from Princeton! Then he launched into a well-rehearsed litany: You know, I *made* the fusion program. Bethe had been skeptical about Spitzer's Stellarator proposal, but I, Libby, persuaded him, and the third AEC commissioner, John von Neumann (I believe) went along. (Von Neumann [1903-1957] had died and I would have no chance to hear his side of the story.) Libby went on: the Princeton Plasma Physics Laboratory would not exist without me(!). I read this as saying that Kennel would not exist without Libby. I thanked him for his offer to join IGPP and said I will think about it.

It did not take me long to turn Libby down. He had won the 1960 Nobel Prize in Chemistry for the invention of the carbon-14 dating technique, which had revolutionized research in geology, archaeology, and paleoecology, among other fields; of course, I would consider his offer, but there was something I did not trust about him. He and his wife, Leona Marshall, had reputations as fall-down drunks; I had heard stories of their having to be carried home after faculty parties. Perhaps it was my experience with my alcoholic mother that spooked me.

The story was completely different several years later when Libby's successor as IGPP Director, Leon Knopoff (1925-2011), invited the paleobiologist Bill Schopf and me to join IGPP as one of his first official acts as Director. Leon Knopoff was an uncomplicatedly great

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silence was getting scandalous, and they finally decided to honor his work on the chain of nuclear reactions that powers the stars.

gentleman and scholar. He had been elected to the National Academy of Sciences at an unusually early age for his work in seismology, and he also had an adjunct appointment with the UCLA Music Department. Leon and his wife, Joanne, were graciously hospitable to my first wife, Debby Bochner, and later to my present wife, Ellen Lehman. I recall the fondness with which Leon and Joanne spoke of meetings of the American Philosophical Society (they simply called it "Philadelphia") in the 1980s; later, when I was at Scripps, Walter and Judith Munk also spoke of "Philadelphia" with the same pleasure as Leon and Joanne. It struck me as something one does not talk about too much, as very few can become members. Two decades later, after Schopf and I were both elected, I recall how proud Leon was to greet "his boys" in Philadelphia. I am proud to have been his boy.

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Richard Feynman (1918-1988) was not the only master player of mental games who won the Nobel Prize; his co-winner, Julian Schwinger (1918-1994), proved to be one also. It was the month of May in the late 1970s, and I was in Paris waiting in Charles de Gaulle airport for my flight to Los Angeles. The center of attention in the waiting area was a group of fit young people, tennis players who had competed in the French Open. One of the youngest, the tall one with the Central European accent, shyly introduced herself: Martina Navratilova. While I was standing around in the group admiring the young tennis warriors, a compact middle-aged man came up to me and asked, you're Charlie Kennel, aren't you? Julian Schwinger had been in the UCLA Physics Department for a few years, but we had never had a personal interaction. Julian was looking for company and hoped we could change seats and sit together.

Once on board, Julian pulled out an elegant leather-bound flask filled, as I recall hazily, with good Scotch whiskey, and offered me some. This continued until the flask was exhausted somewhere over Canada; he did not turn away the flight attendant's drinks either, but I think she did have to turn away his last request. No matter. We quizzed one another on baseball pretty much full time. We competed on old-time baseball, (Wee Willie Keeler, Cap Anson, Napoleon Lajoie, "Tinker to Evers to Chance," Honus Wagner, Cy Young, Christy Mathewson, Rogers Hornsby), the 1927 Yankees (Babe Ruth, Lou Gehrig, Tony Lazzeri, Red Ruffing) and Red Sox heroes (Lefty Grove, Jimmy Foxx, Vern Stephens, Ted Williams, Mel Parnell, Ellis Kinder). Our headbutting continued while consciousness held out. I cannot recall which of us faded first.

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I had often heard it said that Paul Dirac's theory of the positron was the single most elegant theory in all of quantum physics. By the time I was in graduate school in the early 1960s, Dirac's 1928 theory was encased in textbooks, and he was encased in legend. There was nonetheless great excitement among the Princeton graduate students when it was announced that Dirac (1902-1984) would give a seminar, and we all trooped over to the Institute for Advanced Study to hear it. Its Director, J. Robert Oppenheimer (1904-1967), who was famous for explaining beautifully what the speaker was about to say, introduced Dirac. Oppenheimer paid elegant homage to Dirac's great theory, and then explained that today we would hear about a problem Dirac had started on at the same time and had returned to after many years of thinking about other things. Dirac proceeded to give his lecture; the audience was polite, but you could hear the fidgeting.

On the walk back to the university, my physics graduate student compatriots outdid one other in scorn for Dirac's backwardness. He *was* over the hill; however, I was thinking that even if he is over the hill, none of us is likely to do what Dirac had done. Maybe the immense concentration required to create his theory had burned him out; he deserved a lifetime's honor and comfort for that one singular achievement. Professorial tenure is not only to protect intellectual freedom but also to encourage young scientists to go all out as Dirac had. Dirac never knew he had a secret defender that day.

Dirac lived up to his reputation as a selective mute when years later he came to an intimate dinner at my house in Brentwood near UCLA. Dirac had married Mary Wigner, Eugene Wigner's sister. Eugene Wigner (1902-1995), who had taught my class in quantum electrodynamics, was my then wife, Debby Bochner's, father's best friend in Princeton. The Wigners lived on Ober Road perhaps five houses from the Bochners on Springdale Road. Why else would the Diracs visit a young faculty couple they had never met? After we shook hands at the door when they arrived, Dirac stepped to one side, pointed to his wife, and said, "She's Wigner's sister." These were three of the four words he spoke all evening, the other being "goodbye." Fortunately, Mary Wigner Dirac turned out to be a gracious and sophisticated conversationalist, and our three-way conversation revolved around Princeton and Cambridge and physics and mathematics gossip. Dirac followed every word his wife spoke with his eyes.

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It was not as imposing as the National Academy of Science's 1995 symposium on the fiftieth anniversary of the first nuclear explosion at Alamogordo, but those of us in the smaller world of plasma physics and computing thought the 2003 anniversary event at Los Alamos was just as meaningful. It was to celebrate the fiftieth anniversary of the publication of the great paper on the Monte Carlo method by Nick Metropolis, Marshall and Arianna Rosenbluth, and Edward and Mitzi Teller.[83] Their research, begun near the end of World War II, is one of the foundations of modern computing; the paper's acclaim has risen to even higher levels in our present age of big data computing. The symposium was held on hallowed ground, the Los Alamos National Laboratory, where the work had been done.

Though I had not contributed to the field and had no speaking role, I wanted very much to attend to honor Marshall Rosenbluth (1927-2003), then at UCSD as I was. I knew that Marshall was mortally ill; concerned that no one else seemed to be doing it, I had arranged for the American Institute of Physics to dispatch an oral historian to record his thoughts.[84] Sarah Rosenbluth had encouraged me to do so, saying that it would give him something to do during his chemotherapy sessions. I hoped that Marshall would cover the achievements he made before he became the "Pope of Plasma Physics" that I knew: Marshall was co-leader of the great 1965-66 plasma workshop at the International Centre for Theoretical Physics

[83] N. Metropolis, A. W. Rosenbluth, M. N. Rosenbluth, A. H. Teller, E. Teller, "Equation of State Calculations by Fast Computing Machines," *The Journal of Chemical Physics* 21, no. 6 (1953): 1087-92.

[84] I remember one morning in the SIO Director's Office, sunlight scattered from the ocean waves coming through the big window, feeling restless. Sheepish even. No one seemed to be thinking about Marshall's physics legacy. Surely someone must be. After a few days of hesitation, I called Sarah Rosenbluth and asked if she thought Marshall would mind giving an oral history interview if I could arrange it. She said no, Charlie, he might like it, he complains he has nothing to do during chemotherapy sessions. I made the necessary telephone calls.

in Trieste. His Trieste counterpart, Roald Sagdeev, was also going to attend. It would be good to catch up with him.

**Figure 22-1: Los Alamos, shrine of twentieth-century physics. The slanting sunlight captures the atmosphere that Sagdeev and I walked into after hearing about the days when physicists put aside their qualms to invent a weapon that would bring World War II to a close.**

No matter how much the physicists at the Los Alamos symposium had drifted off to other subjects, or had seen their collegiality ripped apart by politics, they agreed on one thing: Los Alamos had done some miraculous physics in the early days. They seemed to astonish each other with what they did; the symposium was a collective reverie about a magical time for physics that would never come back. Much was made of Nick Metropolis' leadership. They also reminisced affectionately about how the first atomic bomb computations were done—not on an electronic computer, but in a room full of mechanical adding machines operated by physicists' wives. The bomb implosion calculation was split into a sequence of micro-steps, each carried out at one adding machine station and passed on. The ringmaster of the whole effort was the young Richard Feynman, the only one quick enough in all of Los Alamos to detect and correct errors as they were being made. (Besides, Feynman could repair the adding machines if they broke down!) I had heard this legend many times, but it had an entirely different quality when recounted to one another by those who had been there.

The symposium was over much too soon for my taste. I was in a pensive mood when I walked with Roald Sagdeev out of the dark meeting room into the bright New Mexico sunlight. There, a young woman looking for someone to talk to came up to us; she turned out to be a reporter for Scottish BBC. She interviewed Roald extensively about his experiences in the Soviet Union. Then she turned to me. How has physics changed in the past fifty years because of these great events?

Physics was different then, I replied. When I was a student, thousands of physicists worked their entire lives to do experiments whose data kept a small number of ineffably talented geniuses meaningfully occupied. Feynman was one of those ineffably talented geniuses. In those days, the progress of physics could be judged by the progress made by perhaps two dozen godlike theoreticians. I said that computer networks have now succeeded in enabling a thousand physicists do the work of one Richard Feynman. Governments prefer to work with the thousand because they can control the thousand, but there was no chance with Feynman.

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There was a time in pre-adolescence when I read a lot of science fiction—the names of authors Robert Heinlein and Isaac Asimov ring in memory. It wasn't science fiction that lured me into science, but I was fascinated by tales of intelligent nonhuman creatures, of galaxy-wide civilizations, and of planets that acted and thought like individuals. I did not devote much time wondering whether these fantasies could be real. I later switched to *Scientific American,* which talked about science non-fiction. There, the only articles I found understandable were those on astrophysics and cosmology; it was science fact and not science fiction that brought me into the field.

One science fiction idea—thinking planets with civilizations on or in them—must have slept in my unconscious through college, graduate school, and teaching "Physics for Poets" at UCLA. It was reawakened one evening in the mid-1970s when Philip and Phyllis Morrison were at my dinner table in Brentwood. They were visiting UCLA from MIT so that Phyllis could work with the famous designers Ray and Charles Eames. Philip Morrison (1915-2005) was a legend in the science world, his Los Alamos work already in the history books. But that was history; I knew him as the book editor of *Scientific American*. Physicists in those days thought themselves able to understand any field of knowledge. Morrison was the real deal, the ultimate polymath who wrote *Scientific American's* book reviews in all fields of science. He read prodigiously, forgot little, and conveyed wonder. Those who knew him had told me there was no one quite like Phil Morrison. They were right.

I had invited some up-and-coming luminaries of the UCLA faculty to be my guests. The cocktail hour chatter centered on what you might expect from young faculty— examination questions, faculty meetings, playgrounds, babysitters. Busy as I was attending to my guests, I was late in sensing that sociable chatter had been replaced by an eerie quiet. People had formed a circle around Phil, with Phyllis sitting quietly by. They wanted to hear his answers to their questions. Whether trivial or cosmic, Phil answered each in full sentences with the same joy in knowing things as he showed in his book reviews. The chatter resumed as people moved to the table and dinner was served. But come dessert, one of the faculty spouses who had been too shy to ask earlier finally found the courage. Professor Morrison, she said, you know so much, what do you think the future holds for the human race?

Just look around, he said. You can see the future if you look for it in what is happening today. *You recall that computing machines were first networked together here at UCLA?* Yes, heads nodding around the table. That was the beginning of something really big, he said. Soon computers will be connected networks of networks; the first *global* network will be used in finance; science and defense will pale in comparison. (This, remember, was in the 1970s.)

LA is the smog capital of the world; you can see it with your own eyes. I can smell it; Something will have to be done about air pollution, Morrison said. The expansion of economic activity enabled by the global financial network will create so much pollution that the network will have to manage the economy and environment synergistically. People will want day-by-day, even minute-by-minute, financial and environmental information that they neither have patience to gather nor quickness of mind to process. They will connect their pollution monitors directly to the network. The satellites you are working on will evolve into ones that provide a global view of the planet and its natural processes. No one will be able to manage the immense volume of data by hand; into the network it will go. In fact, humans will not even be able to program the codes that process the data; the network will take on the programming job, and it will start to make routine decisions. People will eventually have a hard time figuring out how it thinks. Morrison went on, humans will have created a planet aware of its own internal processes, a planetary consciousness. Humans will go about their lives as before, but it will be that planetary consciousness that lives long enough to engage in dialogue with similar entities on planets across the galaxy. A deep silence as our dinner guests digested Morrison's vision. Then it was time for the long goodbyes.

Fifteen years later at NASA I would be caught up in materializing a primitive version of Morrison's planetary consciousness.

# 23

# Mission to Planet Earth

The telephone rang as I was writing “to Ellen” in the dedication to *Convection and Substorms,* my farewell to space plasma physics, as it would turn out. The call was from Dan Goldin, the NASA Administrator, no less. That does not happen every day. Charlie, he said, I want you to come to Washington. You mean to run your space plasma physics program, I asked. (We had spoken earlier about that.) *No! No! No! To run my earth science program, Mission to Planet Earth.*

Earth science? Why are you asking me? *Come to Washington and I can explain it to you in person*, Dan said. If you are working in the space program, you do not turn down a personal invitation from the NASA Administrator. A couple of weeks later, Dan and I met over a weekend in his apartment in the Watergate complex. He had undergone an operation for a detached retina and was under medical house arrest, not a congenial state for him. He stalked back and forth restlessly in his cowboy boots as we talked about space science, small spacecraft, the Earth Observing System, and climate change politics as the dim light of afternoon faded into to the darkness of evening.

Why was Dan asking me rather than a real earth scientist? Was it because I had advocated the use of fleets of small spacecraft for space plasma physics? Knowing this, Ed Frieman had given my name to Dan, but it was not only my advocacy of small spacecraft that persuaded Dan. You are, he said, a reputable scientist, a member of the National Academy of Sciences, which gives you credibility. More than that, you are from another field and your decisions will not be about colleagues to whom you feel loyal.  You will have to cut the Mission to Planet Earth budget and Congress and the earth science community will see you have no personal bias when you do so. Then he said something I will never forget. *When you testify before Congress, half the members will not believe a word you say*. That half thinks climate change is deceitful hogwash, and the other half is afraid NASA is using the threat of climatic catastrophe to take money away from other things they value.

I want you to make your judgments based on science, not politics, Dan said. *I want the world to know that* science *is in charge of Mission to Planet Earth.*

That did it for me. I returned to UCLA, turned my research grants over to colleagues, and prepared to go to Washington. Dan had persuaded me to leave space plasma research (and research itself) to work as an administrator overseeing two national laboratories (the Caltech Jet Propulsion Laboratory and the NASA Goddard Space Flight Center) in a field in which I had no training, and when I had never managed anything more complex than the UCLA Physics Department. Who in his right mind would ask anyone to do that, and who in his right mind would do it?

Big things never happen for one reason. Going to NASA was, of course, a big thing. I would give up the security of an established position in a recognized discipline for a high position in a field that some considered illegitimate. People at space physics meetings knew me on sight; I was getting invitations to speak at conferences without much exertion other than pulling together already prepared slides at the last moment. I would give all that up.

The lifestyle implications were considerable. Ellen and I would sacrifice the way of life we were making for ourselves to a quirky impulse. She could not move her psychology practice to Washington. Patients are not fungible widgets, she used to say; successful treatment requires long-term commitment from both therapist and patient. She would stay, I would go. But she argued that we could turn NASA into a cultural adventure by meeting weekends in the middle of the country. About once a month we met for three-day weekends somewhere. We visited galleries in Santa Fe and Chicago; NASA travel took me to Houston and Pasadena where we also met. We went to Cape Kennedy in Florida for shuttle launches; we stayed in Palm Beach with her parents over the Christmas holidays. Ellen visited me in the apartment I rented in Dupont Circle and we explored the Washington galleries and the Virginia countryside together. So we found a way to make a bi-coastal relationship work. Others working for the government had done so, which leaves the question: why would I, or any scientist in his right mind, leave a field where he was doing well for one that was politically suspect?

**Figure 23-1: A man is not a prophet until he leaves home. UCLA took notice when I left for Washington to lead NASA's Mission to Planet Earth, and I was invited to give a commencement address in 1995. Shown here is a gathering of loved ones I left behind. From left to right in the back row, they are Marguerite and Len Mautner, friends of the physics genius Richard Feynman, as well as my personal friends; Joyce and Mel Schwartz, Mel was my psychotherapist from 1986 to 1993; the Associate Administrator himself in his Princeton academic gown, and René Pellat, colleague and confidant in both Paris and Los Angeles. Front and center is my wife, Ellen Lehman, in her Cornell PhD gown.**

In truth, for the previous three or four years I had not been in my right professional mind, growing not exactly bored but less excited by space plasma physics. The most mature of the disciplines created by the space program, it was no longer the jewel in NASA's crown, no longer getting the public recognition that swayed my parents. I was uninterested in the fine details in papers on numerical simulations that began to fill whole sessions at meetings. Space physics lacked the creative messiness of its early days. The Moscow Space Research Institute, IKI, was not the same exciting place after the Berlin Wall came down. Fred Scarf had died and my new consulting at TRW on plasma techniques to separate nuclear isotopes was less interesting. So was UCLA; I knew what faculty colleagues would say before the conversation started. I also got an ominous review of a proposal to renew the NASA grant I had been getting since 1967; the proposal was of the high quality that the community had come to expect from the applicant (me), this reviewer said, but wasn't it time to give an opportunity to younger people?

The grant was renewed, but my scientific life was emptying out. I had been itching for change for quite a while. However, I did not want to change jobs until one piece of unfinished business had been taken care of: I was not yet a member of the National Academy of Sciences, the ambition of almost every American scientist, and certainly mine. Roald Sagdeev had told me it was about to happen, just be patient, he said. When the telephone rang one April morning in 1991, Ralph Cicerone, an earth and climate scientist who went on to become President of the Academy, was the first to congratulate me on my election. I was freed to do something new but soon became depressed because I did not know what to do. I had done some interesting work, witnessed some interesting events, and been to some interesting places, but what had I *really* done? My space physics contemporaries were already doing important things. René Pellat in France, Gerhard Haerendel in Germany, Syun-Ichi Akasofu in Alaska, Atsuhiro Nishida in Japan, David Southwood in England, and Roald Sagdeev in the USSR were leading major research organizations; what was wrong with me? Two years later, Dan Goldin offered me the chance to prove I could do it too. I went to Washington.

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Everyone has one. A hometown. A hometown is where you grew up. A hometown is where you know how the people think—or perhaps it is the other way around, they know how you think. Your hometown has a special word for those who have a different hometown: *stranger*. If your hometown is in New England, like mine, you recognize other New Englanders by telltale differences in the way they pronounce vowels, even if neither of you has lived in New England for half a century. New England is a collection of proud old hometowns, all similar to the stranger but each one distinctive to the native. When a teenager I had an ear for accents and could tell people who came from the North Shore ("schorrr") of Boston apart from those who came from the South Shore ("sho-wah"). They lived only fifty miles apart, but they thought differently. After three years in Washington my ear could identify people who lived fifty miles apart in Maryland and Virginia by their accents, though that did not tell me how they thought.

People in the District of Columbia had all kinds of accents. The Founding Fathers realized that if America were to be governed as a union of states, its national capital had to be no one's and everyone's hometown. The national capital couldn't be in a state, it had to be in its own something else, a district perhaps. A district is a place that is part of a larger place. Not for America could its capital be like London, Paris, Berlin, or Moscow, the biggest and richest city in the country, the city where ideas come from, the city where money is made and spent. We Americans have tried to separate commerce and government by putting our state capitals in cities whose primary business is government. We have kept Albany apart from New York City, Springfield from Chicago, Sacramento from San Francisco and Los Angeles, and Harrisburg from Philadelphia and Pittsburgh. A visitor to these capital cities finds them oases of public order. You see it in the low buildings and orderly flow of automobile traffic. Paris and New York traffic may inspire symphonies for auto horns, but in Washington the traffic hum comes from tires on pavement.

American capitals are not notable for their intellectual or artistic creativity, though creativity is not absent. Duke Ellington came from Washington, for example. Ideas in science and technology also have hometowns—aerospace in Southern California and Seattle, information technology in Silicon Valley, biotech in San Francisco and Boston. Whatever their hometown, all ideas come to Washington as strangers and must be acculturated to a town where everyone comes from a different hometown. Not only that, but you have to re-acculturate them every four years. Ideas cut off from their roots are judged according to the logic of political power and not the inner logics painstakingly worked out before the ideas were mature enough to leave their hometowns. This is hard for the scientist to appreciate, but almost every idea making the rounds in Washington, including the ones competing with yours, had a virtuous conception. All are treated in the same rough way.

Washington is different when Congress is in session, as if someone has turned on the electricity. I used to picture Congress as a leviathan in the depths, its slightest motions churning the water at the surface above. I could tell when Congress was in session because acquaintances would tell me what this congressman or that staffer had said a couple of days earlier at a committee meeting or cocktail party. The trade journals told me what I would be testifying about next week. I had read of the power of committee chairs and knew the senators or representatives who would take a special interest in my program. It is true that some people's views are more important to you than others, Dan Goldin used to say, but no member of Congress is unimportant. All are elected and all deserve your attention and honesty.

Moving from the trendy media culture of Los Angeles to Washington's culture of influence, control, and power was more illuminating than I had anticipated. I found myself saying that I wished I had learned twenty years earlier what I knew then about how my country is run. I would have made fewer mistakes.

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Washington had been so afflicted by heat waves before air conditioning that little government work got done in the summer. Those who could left town, just as British colonials fled the summer heat of Delhi for their Himalayan resort of Shimla. In 1979 the National Academy of Sciences published the report of a panel chaired by the MIT atmospheric scientist Jule Charney that predicted that greenhouse warming would have a clear impact on measured atmospheric temperature by the year 2000. In 1988 there was a spectacular heat wave, even for Washington, and Congress held a hearing on it. Some in Congress wondered whether this heat wave came from the global warming that scientists had warned of. Was this 1988 event a harbinger of worse to come?

Every climate scientist today will be careful to say that science is only beginning to predict a particular heat wave with confidence, that science can only say with assurance that the ongoing accumulation of greenhouse gases in the atmosphere makes heat waves more likely. Today's restraint was not so binding on Jim Hansen, a climatologist at the NASA Goddard Institute for Space Studies at Columbia University, who came out with it and said the extreme heat could be due to global warming. Physicians are often panicked at the first appearance of a new disease largely, I think, because it is natural to fear most what you do not understand.

Hansen's heat wave was the first highly publicized weather event attributed to global warming by an acknowledged authority. NASA, seeking an issue that could place the agency again at the center of the government's policy agenda after the end of the Apollo program, saw an opportunity and proposed what would have been the most expensive scientific project ever built.

The Earth Observing System (EOS) was originally conceived to be three pair of large shuttle-launched spacecraft in earth polar orbit, one pair launched every five years for fifteen years. The huge shuttle-launched spacecraft were to carry twenty-four instruments to track changes in the variables thought diagnostic of climate change and its impacts on ecology and the environment. Though EOS cost estimates were notional, building and operating the full system for fifteen years would have cost about eighteen billion in 1990 dollars, more than the Superconducting Supercollider in the US and the Large Hadron Collider at the European Center for Nuclear Research. This made the original EOS the largest scientific project ever proposed up to that time.

The first George Bush administration had been spooked by the climate threat enough to fund this outsize reaction. The laws of politics are not nearly so precise as those of physics, but every action provokes a reaction; the Clinton administration's reaction took the form of objections to the huge cost of EOS. Dan Goldin, President Bill Clinton's new NASA Administrator, was given two big tasks: bring Russia into the International Space Station collaboration to help settle the Cold War and achieve EOS' scientific goals at lower cost.

Goldin's first EOS challenge was to devise a strategy for reducing cost. To that end, he appointed a panel of experts chaired by my thesis advisor, Ed Frieman, then Vice Chair of the White House Science Council. The main EOS cost driver was instrumenting spacecraft whose capabilities could justify using the huge lift capacity of the Space Shuttle, which cost about a half billion dollars per launch. Using the Shuttle optimally required filling a large spacecraft with a large number of large instruments. Some of the instruments had not been built previously and it was likely that their costs were underestimated. There was also a one-in-twenty-five risk that the spacecraft, all experiments onboard, and science critical to policy, would be lost on launch. Finally, deploying numerous complex instruments on the same crowded spacecraft, even when their requirements are individually well understood, presents a major cost risk because unforeseen requirement conflicts are likely to force expensive modifications during payload development and integration. That risk could not be calculated, but experienced engineers saw it as the cost cuckoo in the nest.

There was a way out of the requirements conflict dilemma, but it required new ways of thinking. The launch and requirement conflict risks would both be mitigated if fewer instruments were placed on more but smaller and cheaper spacecraft. While this made sense from the engineering point of view, the principal question was whether the small spacecraft version of EOS would produce the information needed by science. The Frieman panel observed that one major observational requirement driving the initial EOS design could be relaxed with what appeared to be an acceptable loss of scientific content. If it were no longer required that all instruments observe the same volume of the atmosphere at the same time, instruments on multiple smaller spacecraft could gather the types of data that recent research in earth system science indicated was needed. You would have to create new synthetic capabilities in data analysis and modeling to offset the loss of simultaneous observations, but if you could do that you could solve the cost problem. Small spacecraft would both diminish the cost (in this event by a factor three) and spread the risk—and they did, but the real advance was the change in philosophy.

I had never run a big organization or overseen a multi-lab engineering project; Dan Goldin gave me two crackerjack deputies, Mike Mann for management and Bill Townsend for engineering, and we ran Mission to Planet Earth as a triumvirate. Our main task was to reduce the cost of the EOS while retaining as many of its original capabilities as possible, but NASA's other earth science programs could not be kept on hold while we grappled with EOS. My main role was to work with the EOS project team, the larger earth science community, the Space Studies Board of the National Academy of Sciences, NASA's EOS partner agencies (NSF, DOE, USGS, and NOAA), and foreign partners to develop a system acceptable to the scientific community, the Clinton Administration, and Congress. It took three redesigns in three years, but we arrived at an earth observing system whose capabilities were acceptable to the science community and whose budget was acceptable to Congress.

We made use of a degree of freedom never before (or since) available to government satellite designers. EOS missions were not ends in themselves. I came to perceive that an earth observing system was not coordinated launches of spacecraft with related suites of measurements, it was an information management system. Whereas in the past and the future Congress would approve satellite missions on a case-by-case basis, Congress had authorized EOS as a twenty-four-instrument system of measurements, and NASA had the freedom to architect a system of multiple small satellites to carry those instruments. Congress proved to be receptive so long at the total system cost came down with each redesign, and the scientific community did not object. With the new data analysis capabilities coming online, smaller spacecraft could achieve enough of the basic scientific goals of the original EOS. Ghassem Asrar, my successor as Associate Administrator, got EOS built on his watch. The engineers at Goddard Space Flight Center even figured out how to fly satellites in formation and recover some of the capabilities lost in downsizing. The system cost was still substantial: six billion dollars.

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Many NASA scientists and engineers were put off by Dan Goldin's abrupt style and angry outbursts on encountering resistance to his ideas. Engineers valuing orderly process would be mystified by Goldin's orders to study an option and its opposite on successive days. Their unease came across as mutters about Dan's "faster, better, cheaper" mantra. You can do one, perhaps two, but never all three, they repeated out of Goldin's hearing. Ellen was the one who helped me deal with Dan about this attitude, which was common among those doing the work. She suggested I agree to carry out whatever study Dan wanted and tell the engineers he was comparing options; if an option turned out not to answer Dan's hopes, tell him so and have a feasible alternative ready. Bill Townsend, when he brought yet another Goldin request to restive Goddard engineers, would patiently explain when they rolled their eyes that Goldin just wanted to choose between technically sound alternatives.

I learned a new branch of science by osmosis. In the mid-1980s, NASA had led the organization of a new multi-discipline, earth system science, to place earth observations from space in context, to understand natural climate variability, to forecast anthropogenic climate change, and to support assessments of its environmental impacts. Concepts from atmospheric science, oceanography, geology, ecology, glaciology, marine biology, and other earth science subdisciplines were woven into a knowledge network that mirrors the interconnection of processes determining climate variability on the ten- to one-hundred-year time scale. I had the best possible teachers—EOS science teams and NAS committees. The leaders of earth system science had to explain to me the science behind the measurements; they had to tell me not only why they were doing what they were doing but also their visions of the future of their fields.

Earth system science is a syncretic field whose progress depends significantly on how it is organized. This meant I could contribute even if I had not been trained in any of its subjects. Being an Associate Administrator placed me at a node of a knowledge network that processed the views of scientists, engineers, managers, policy experts, congressional staff, the White House, and NASA leadership. This knowledge-to-action network was an instrument of collective multidisciplinary thought that I charged with the redesign of EOS. The participants in this network could not speak the specialized languages of their primary disciplines but had to use the less rigorous language of general scientific discourse to communicate with one another. The views expressed by the participants depended upon one another and changed when others' views changed. Frequent face-to-face interactions ensured that each notion under discussion was accompanied in transmission with the speaker's emotional urgency, which surely speeded the crystallization of collective views.

As we were working, our perceptions of the future power of information technology evolved. The EOS teams saw how much more they could do in their own fields with the emerging technologies. What kept the debate from being an abstract game was the requirement to submit an EOS budget to Congress very year. Engineering feasibility and system cost had to be as credible as NASA could make them each time. One of my functions was to explain the annual budgets to Congress. With the support of a dedicated NASA congressional relations staff, I would review NASA's updated version of EOS with congressional committee staff, brief individual members, and testify before the House and Senate Science Committees. I was looking for the sweet spot where we had reduced the cost enough that Congress would be content to see EOS completed.

Congressional support was the *sine qua non*, but Congress was not the only arena in which the future of EOS was debated. The first Bush administration created the US Global Change Research Program (USGCRP), which has coordinated the support of US and international climate policy by US government science agencies for the past three decades. The principal USGCRP agencies were NASA, the National Science Foundation (NSF), the Department of Energy (DOE), and the US Geological Survey (USGS), listed in approximate order by size of earth system science program. At the time I participated in official USGCRP affairs, 1994-96, NASA played the dominant role because of its big research budget, though everyone understood that intellectual and financial contributions are not the same thing. Program coordination was achieved by an expedient simple to describe but difficult to achieve: the Federal Office of Management and Budget (OMB) mandated a unified interagency budget (in bureaucratic parlance, they "fenced" the USGCRP budget) which, to the astonishment of many, was successfully enforced in USGCRP's early years. I do not believe the level of interagency collaboration achieved by the early USGCRP was ever achieved again as climate politics, and politics in general, became more conflicted.

It did not take long to see the international dimensions of earth system science. I participated in the CIA's MEDEA group, whose task was to judge whether declassifying observations taken on CIA satellites in the 1950s and 1960s could provide baseline information about subsequent climatic and environmental change. I was also the NASA principal at meetings of the international Committee on Earth Observation Satellites (CEOS). Lisa Shaffer, the Director of International Relations for Mission to Planet Earth, accompanied me to every meeting. CEOS revealed Lisa's passion and talent at their most effective. In 1984, when she was with the National Oceanic and Atmospheric Administration (NOAA), she had been one of the prime movers in the organization of CEOS. CEOS, which meets at the head-of-national-program level, provides a forum where projects are coordinated technically and administratively without requiring formal intergovernmental negotiations. CEOS also coordinates national launch schedules to optimize global observational coverage. I was welcome in CEOS because NASA was providing half the world capability in observations of the earth from space at the time; Lisa was welcome in CEOS because she made things happen. Recently CEOS conducted its thirty-fifth plenary meeting, this time virtually because of the pandemic. It became the model for a more ambitious global effort devoted to climate change and sustainability—the Global Earth Observation System of Systems (GEOSS).

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Until I was in middle age, I had unrealistic notions about what happens when you try to turn scientific ideas to useful account. I thought I knew a lot and expounded a lot, but I had never actually worked with the types of people who make real the fantasies of people like me. I did not know how these mysterious doers thought and what was important to them. This changed when I went to NASA. If the preoccupations of my early twenties led me to make an ordered list of physics heroes as I had done with baseball in my early teens, the urgencies of middle age made me add engineers to my personal pantheon of heroes. At the top of list for me were the man who recruited me to NASA, Dan Goldin, and his infinitely patient accomplice, Bill Townsend, whose job as Deputy Associate Administrator for Mission to Planet Earth was to explain to his fellow engineers what Dan Goldin and I really meant when we asked them to imagine our ideas in engineering terms. Then there was my counterpart at the German Space Agency, Heinz Stoewer, whose accomplishments when I first met him would have placed him on the list with Goldin and Townsend, but whose subsequent academic accomplishments made system engineering rival physics and biology in my estimation. Heinz and I first met at Cape Kennedy, where he travelled to because his agency had shared with NASA the development of the Shuttle Imaging Radar C (SIR C) on STS 59.

When Heinz Stoewer was president of the International Council on Systems Engineering (INCOSE), he invited me to give a plenary presentation at its 2004 General Assembly in Toulouse, France, the home base of the French aerospace industry. I was honored to share the platform with the chief designer of Airbus. I called climate change the biggest system-engineering problem of them all, and described the then current international efforts to design the global earth system of systems (GEOSS).

One day, Dan, who was hard to meet with when in Washington, invited me to accompany him on a flight in the NASA airplane to Houston (where he was to give a presentation) "so we could have a real discussion." I believe it was on this flight that I learned that Dan had a number one on his heroes list: Si Ramo (1913-2016), the "R" in "TRW, Dan's boss, and a prime mover in the development of the intercontinental ballistic missile. When Dan and I were in retirement, Dan arranged a memorable lunch in Los Angeles for us with Ramo, whose remarkable emotional and intellectual vigor Dan so obviously yearned to emulate. Ramo was then nearing one hundred years old. A small piece of the universe fell into place for me when Heinz Stoewer invited me to the ceremony in San Francisco honoring his winning of the 2018 Simon Ramo award by the Institute of Electronics and Electrical Engineers (IEEE). The presentations covered Heinz's wide range of experience, but the slide reproduced below shows the piece of life experience he and I shared: Heinz congratulating an astronaut upon completion of a successful mission. Heinz and our wives became lifelong friends; Ellen and visited them in their home in Den Kaag in Holland, and even at age 82 he is a distinguished visiting scientist at NASA and CalTech's nearby Jet Propulsion Laboratory, which gives other opportunities to meet.

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I shared the adventure of human space flight in ways that most space scientists do not. I was senior NASA officer at Cape Kennedy for several Space Shuttle launches; I spoke at pre-launch press briefings; I shared pre-launch anxiety with Heinz Stoewer at the STS-59 launch of the Shuttle Imaging Radar (SIR-C). I was senior NASA officer at the Cape for STS-59, on which my wife's cousin, Jay Apt, was an astronaut. Normally I would have been part of STS-59's decision-to-launch process, but I recused myself because of the family connection. Nonetheless, I was in the Kennedy Space Center control room as an observer during Jay's launch countdown, while Ellen sat in the viewing stand with other family members.


**Figure 23-2: At Kennedy Space Flight Center in Florida during the events associated with the launch of the Space Shuttle for NASA Mission STS-59 (April 9-20, 1994). Ellen's cousin, Jay Apt, was a crew member.**

During the countdown, one of the eighteen (as I recall) launch readiness teams registered a formal protest that the launch director ultimately overrode. (The winds at an emergency landing site in Spain were out of specification and just barely came into compliance during the flight.) I was glad I did not have to be part of that decision. I did not tell Ellen about it until after Jay had landed. Ellen and I participated in many astronaut family group activities both at launch in Florida and at touchdown at Edwards AFB in the California desert. I got to experience the human side of NASA as scientist, administrator, and astronaut family member. No longer could I regard the human space flight program as an expensive public relations ploy as some scientists did.

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All the EOS science teams understood that their obligation was to create knowledge in earth system science and not to advocate political action on climate change. Nonetheless, meetings with EOS scientists had an underlying urgency that came from their understanding of the consequences of climate change. Fear of climate change came up in nearly every conversation; I particularly remember one with Jerry Mahlman, the Director of Princeton's Geophysical Fluid Dynamics Laboratory (GFDL), one of the three principal US climate modeling agencies. One day, Jerry said, GFDL had chosen, just for curiosity, to forecast the climate out to the year 2200 rather than the usual 2100, assuming greenhouse gas emissions continue their present trajectory. That moment with Jerry Mahlman burns bright to this day. What did you find? I asked. It was *horrendous*, he said, *so horrendous that we vowed never to do it again. No one would believe how bad it could become.* I, too, did not believe a single forecast that far in the future, but a bomb went off in my mind at that moment anyhow. Jerry had just told me global civilization has no more than a century to solve a problem that could be mortal.


**Figure 23-3: Associate Administrator Charles Kennel (right) and Administrator Dan Goldin (left), June 5, 1996, NASA Headquarters, Washington, DC. This picture was taken at my retirement party from NASA. I was to return to UCLA to be Chancellor Chuck Young's last Executive Vice Chancellor.**

# 24

# On and Off the University President Track

NASA had rented me from UCLA. NASA paid the University of California the top salary in the Federal Senior Executive Service—the going rate for an Associate Administrator, the highest NASA rank before Senate confirmation is required. UCLA continued my academic salary, health care, and retirement benefits (which meant it took a loss) and I was allowed University of California work rules while on loan to the government. This freedom enabled me to arrange an extra day off to meet Ellen somewhere in the country every few weeks. I had the best of both worlds, government expenditure authority and academic work rules.

I knew there was a limit to how long I could be away from UCLA. I just didn't know what it was. I had read of Henry Kissinger's negotiations with Harvard about extending his term of service as Secretary of State. That little voice inside was whispering that staying at NASA much beyond three years would be pushing things. Eventually, the government would be more comfortable with billion-dollar expenditure authority in the hands of a federal employee. Besides, there was just so much Ellen and I could sacrifice to a long-distance relationship before we would encounter trouble. When the White House science advisor, Jack Gibbons, interviewed me, he warned that relationship trouble typically emerged (if it did) in one's second or third year alone in Washington. The others in government I interviewed for the NASA job emphasized the political delicacy of managing a controversial program, but the one with the highest rank, Jack, emphasized the human cost of taking a Washington job. I respected him for that.

Ellen and I were not experiencing relationship difficulty, but we were aware of the growing risk, and we started to discuss when I should come home. I was not sure that I should return to UCLA in the same position I had left two and a half years previously; there must be some way I could turn my NASA administrative experience to account. When I became aware that Chancellor Young was looking for a new Executive Vice Chancellor, UCLA's number two executive officer and chief academic officer, I applied to the search committee. I even had in mind a notional reply to the question the UCLA search committee would inevitably ask about my limited administrative experience: Mission to Planet Earth and UCLA had similar budgetary expenditures, about two billion dollars per year, and MTPE did not collapse because of my mismanagement. Somehow, NASA had coped.

I must not have felt entirely ready to return to Los Angeles because there were several delays in scheduling my interview with the UCLA search committee. The date was finally fixed for the last day of the search. There was a blizzard in Washington the day of my flight to LA, taxis were not going to Dulles Airport, and I ended up taking the Washington Metro part way and a bus the rest of the way to the airport. I arrived within minutes of my flight's departure. The flight got me to LA around 9 p.m. for an early morning interview the next day. I do not remember what was said at the interview, but I must have said something right because Chancellor Young offered me the job the day after. So the die was cast; I would return to UCLA in June 1996 as Executive Vice Chancellor.

The next four months at NASA were taken up refining the last EOS redesign to be made on my watch as we assembled 1997's Mission to Planet Earth's budget request. NASA put on a lovely retirement party. My NASA colleagues, unaccustomed as they were to an academic in the leadership role—a theoretician no less—had marveled at my penchant to debate the pros and cons of every issue.[85]

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[85] I did learn to restrain my curiosity somewhat one day soon after arriving at NASA headquarters. I interrupted a discussion in my office saying: I know this is not on topic, but what would happen if we did X? No one at the table knew the answer, so we moved on. Two days later I got a telephone call from the Jet Propulsion Laboratory. We hear you asked a question about X in at a meeting in your office, the person said; we looked into it, and here is the answer! Be careful what you ask for.

Many of their skits at the party had the theme, when, oh when, will Charlie ever make up his mind? If my main task had been anything but EOS redesign, which demanded endless questioning, I would have been an abject failure.

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Chuck Young was far and away UCLA's greatest chancellor—respectful of academic culture, consistent in philosophy, adroit in administration, cordial to all, and forceful when required. A capable administrative team had evolved in his image, and I looked forward to a few quiet months to absorb the basics of my new job from the denizens of the Chancellor's Office. That was wishful thinking. Soon after I arrived Chuck Young's wife, Sue, was diagnosed with terminal cancer. Sue had shared, supported, and amplified Chuck's passion for UCLA and the UCLA community. The University of California had even created a special title for her, Associate of the Chancellor, to recognize the spousal role in high university administration. Wanting to be available for Sue in her time of trial, Young decided to retire. Characteristically, he turned his departure to account by coordinating the announcement of his retirement with that of the UC Berkeley Chancellor, Chang-Lin Tien, in a joint protest of the UC Regents' decision to eliminate affirmative action in student admissions. There followed a year unlike all others at UCLA. Young stayed on while the Regents searched for a successor (Al Carnesale, Provost at Harvard). The Los Angeles community celebrated Young's achievements in event after event, all while he was preoccupied with the suffering of his wife. One day I heard him weep in his office, which was near my own.

The Executive Vice Chancellor had authority for UCLA's academic resource allocations. The leaders of all academic units reported to me except for Gerry Levey, the Vice Chancellor for Health Sciences (which had half the budget of the entire campus), who reported to God, the chancellor, and me, in that order; I saw no reason to contest a successful working arrangement.

One of my responsibilities was to integrate the plans of our academic units into a unified vision that could be communicated to UCLA's many and diverse stakeholders: the President of the University; the Board of Regents (which included the governor); leaders of other UC campuses; faculty and students; alumni; and, critically, donors. Just as at NASA, vision was articulated through the budgeting process. After twenty-eight years as Chancellor, Young was still seeking ways to use the budget process to shape UCLA's priorities and actions. When I arrived, he was contemplating shifting from incremental budgeting, wherein one decides puts and takes from a previous budget, to zero-based budgeting, wherein one reassesses the value obtained from the entire expenditure before making specific decisions. My task was to explore the acceptability of a zero-based budget with UCLA's academic units. To that end I met with deans, departmental chairs, and senior faculty, both individually and in larger meetings. Nearly all of them had to explain the basics of their subjects, which was fine because they and I knew that their disciplines were new to me. Was Young's a characteristically tactful way of teaching me how the campus worked?

As the budget consultations wore on, I learned a lot, but I also encountered a reaction like the one at NASA. One evening the Provost of the Undergraduate College, Brian Copenhaver, invited me to a quiet dinner at an elegant restaurant in nearby Westwood. After almost enough good California wine had flowed, Brian revealed his purpose. Charlie, he said, you are taking too great an interest in our subjects; we do not want you to debate the merits, we want you to be Delphic; we want you to make decisions we cannot make. You do not even have to tell us why you decide something, but please make up your mind in private.

Young eventually abandoned his zero-based budgeting experiment, not because it set loose a flood of delicate questions, but because it was seen as a threat to faculty autonomy—a sacred value at the University of California. Incremental budgeting was suited to the problems at hand and the cast of characters in charge. Not much changed. The strong units continued to do what they thought necessary to remain strong. Departments and schools remained unaware of what the others were doing. It remained hard to get them to articulate anything more than an anodyne vision of UCLA's future. Young was working on his legacy, hoping UCLA could internalize an actionable vision of its role in the life of Los Angeles and the world at large. Aping Berkeley was not the route to greatness for Charles E. Young. UCLA's psychological Berkeley complex and addiction to external recognition were to continue after Young stepped down.

Just as the admiral of the fleet cannot order the flag captain to change the ship's course, neither can chancellors and vice chancellors order academic units to change course. Chancellors and vice chancellors do make a difference at the margins or universities would not have them. In my case, I was able to shelter (to a certain degree) the School of Nursing from the hegemonic behavior of the School of Medicine. Also, Rory Hume, Dean of the School of Dentistry, carefully sought my approval before restructuring his faculty's practice plans in order to encourage patient service; we both knew faculty compensation was a delicate topic. I encouraged him to take the risk. Rory went on to become Vice Chancellor of the University of New South Wales, and after a troubled term there he returned to the University of California as Vice President for Health Sciences.

Chuck Young had been the first UC campus leader to undertake private fundraising when Ronald Reagan's political philosophy supplanted Lyndon Johnson's, and state funding started to fall short of UC's ambitions. Young's decision began UCLA's reach for the light outside Berkeley's shadow. As a junior faculty member, I first noticed the upgrade in elegance of the Faculty Club and other public-facing facilities. Young created a formidable fundraising operation, which fitted Ellen and me smoothly into its event planning. Chuck, I was told, entertained 20,000 people each year, many at the hospitality tents at UCLA football games in the Rose Bowl. I enjoyed the football games, my time as a Harvard football manager having conditioned me; Ellen was not so enchanted. There were, in addition, two or three social events each week at the Chancellor's House. By and large Ellen and I did not resent them, because we were schmoozing with accomplished people who could have been gambling their money in Las Vegas but instead were looking for intelligent ways to give it to us.

Had it been a different time, I might not have been drawn into areas of general campus life where Young ordinarily took leadership. The day I arrived from Washington, I learned of a snafu that reached all the way to the Chancellor. The federal Office for the Protection of Human Research Subjects (OPRS), which regulated the operation of the institutional review boards (IRBs) that monitor experiments on human subjects, had found UCLA's management of its IRBs in non-compliance. The OPRS was so angry with a faculty member's supercilious resistance to their demands for compliance that it threatened to withdraw *all* federal research funding from UCLA. This hit where university administrations live, in the budget, and Young initiated a full-court press to deal with the crisis.

The university research and federal regulatory cultures are not naturally attuned in the best of circumstances, but in this case the problem was exacerbated by the actions of a highly productive professor who resented what he saw as bureaucratic nitpicking. He had defied the federal regulators more than once before they came down on UCLA. I believed that society must enforce its ethical standards, and a compliant solution would have to be found. I also believed that faculty would respond better to other faculty. My contribution was to recruit a faculty advisory group, led by a much-respected professor of philosophy, David Kaplan, to work with UCLA's IRB operations. I came to respect David so much that he was welcome in the Executive Vice Chancellor's Office at any time. Peer pressure seemed to settle things, and ultimately this oversight function devolved to the Office of the Vice Chancellor for Research.

The ever-present possibility of demonstrations on campus can never be far from a chancellor's mind. One afternoon we were notified that an armed group was coming to campus to attend a planned demonstration that evening. Young had seen altogether too many of these kinds of things, and in his time of mourning and impending retirement, he was tired. He notified the campus and LAPD that I would be in charge, and he went home. It would be good training for me, he said. I minded the constant telephone traffic in the Chancellor's Office all evening until the crisis subsided. By agreement, the campus police monitored but did not interfere with the demonstration, and as the demonstrators left the campus in their cars the LAPD stopped them on suspicion of traffic violations. An arsenal was found stashed in the trunks of their cars. The lack of publicity was our signal of success: it had been just another day at the office.

On another evening as the sun was setting, I was absent-mindedly leaving the Chancellor's Office complex only to be blinded by flashbulbs. A microphone was pushed into my teeth, and I was asked on camera whether I knew a sexual predator was stalking the halls of one of our science departments. I had to admit I did not but would look into the matter. A senior professor and a younger subordinate researcher had struck up a consensual relationship that had gone sour, and the subordinate was seeking legal redress for harassment. The surprise TV interrogation was a lawyer's Hollywood-style ploy. Later I learned the faculty member's wife had left him and he had left campus. It was a bad situation for all concerned. The University of California had not yet developed firm policies and procedures that forbid intimate relations between members of the University in asymmetric power positions. Later, at

UC San Diego, I would have another reason to appreciate that it eventually did so.

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It was off to one side at a University of California Regents meeting that I convinced the University's Vice President for Administration, Wayne Kennedy, to repair the UCLA chancellor's residence during the interregnum caused by Young's impending retirement. I did not want to force the new chancellor to order repairs. Young himself had not dared to do it; a UC Santa Barbara chancellor had even gone to jail for spending public funds on his private residence where he entertained officially.

The UCLA chancellor's house was used only for official business; Chuck and Sue Young lived in their own place at the Sherwood Oaks in the San Fernando Valley. The empty UCLA house was in such ill repair that its kitchen could not be used for entertaining the thousands of people who came to events there every year. Caterers had to set up outdoor kitchens in the parking lot for each event. Ellen and I could see the caterers as we walked to events. Young never asked me, but I took on chancellor's residence repair as a problem that needed to be solved. Wayne Kennedy and I addressed it by taking the management of its repairs out of UCLA's hands and giving Wayne decision authority. Thus, Al Carnesale, Young's successor, moved into a freshly repaired classic Bel Air house, whereas the new Berkeley Chancellor, Bob Berdahl, got bad press—and fortunately nothing worse—for ordering repairs to his house after he arrived. A similar problem arose later at UC San Diego with the arrival of Marye Anne Fox as Chancellor. University presidents' and chancellors' houses are a curse.

Many new leaders want to choose their number two executive, but Al Carnesale graciously assured me that he wanted me to remain. It was evidently not for my administrative *legerdemain* but for my attunement to UCLA culture, since he regularly checked with me when pondering decisions. He also used a technique I presumed he learned in arms control negotiations. He would probe potential reactions by floating trial balloon proposals and waiting for feedback. I would get one of the early trial balloons. Al also told me about the summer course at Harvard for incoming university presidents; if there were only one thing they should pick up from that beginner's course, Al said, it was that those who tried to create radical change in their first year were gone in their second. Al and I spent our first year together not making this mistake. Since I thought I might be someday in the same spot as Al, I watched what he did carefully. Budget restructuring now last year's fashion, I spent much of my time working with the Academic Senate on the joint faculty-administration oversight of the academic promotion process prescribed in the founding documents of the University of California.

I had been educated at East Coast private universities, Harvard and Princeton, and had a sense of their ethos, but until I was in the UCLA Chancellor's Office, I did not fully comprehend what made the University of California the greatest public university in the world. Each branch of the University is its own expression of California's regional and cultural diversity, each having the size and role of a flagship campus of a state university elsewhere.

In those days, the number two campus officers could attend the monthly meetings of the UC Board of Regents as observers. All University funds and property are held in the name of the Regents, who represent the University to the governor, legislature, and public of the State of California. The Regents protect the University's independence in academic governance and while promoting political responsiveness and social responsibility. Their importance to the University is comparable to the Supreme Court's to America. It was a life privilege to see them in action.

The administrations of the different campuses of the University of California share a common culture of academic governance. It didn't have to be that way; it was done by design. Each campus has the same leadership hierarchy: a chancellor, vice chancellors, deans, directors, etc. The chancellors, vice chancellors, and other high administrators with similar spans of authority at the different campuses meet monthly. I faithfully attended the executive vice chancellor/provost meetings; the campuses vied with one another to provide instructive venues for the meetings.

The dinners on the first evenings were high points. We shared gossip and gripes, and though our socialization did not create identical responses to common problems, it did promote shared perceptions and values that enable a flexible university-wide response in times of stress. The campuses may have behavioral norms in common, but each has creative freedom. There is sibling rivalry and campuses regularly raid each other's faculties. Until climate change came along, there was little taste in the President's Office for creating university-wide academic initiatives, and the campuses were as different as the brothers and sisters in any large family.

The President of the University of California, Richard C. Atkinson, called me three times during my time as Executive Vice Chancellor at UCLA. The first was to ask me to chair a system-wide committee to propose to the Regents the creation of a new branch of the UC library system, a digital library. As I think about university libraries now, I realize that I have seen information technology transform the way the scholarly world documents its thoughts.

The old Fine Hall Mathematics Library at Princeton was sacred to mathematics and physics graduate students, open twenty-four hours a day, and busy all night. You could walk into its library spaces and find inspiration by randomly examining books and titles. Each aisle in the stacks had a different smell of dust; books had unconscious tactile associations. When I arrived at UCLA I expected to use, and did use, the Physics Branch Library in Kinsey Hall the same way—at first. The Xerox machine changed all that; I copied articles and stored them in metal filing cabinets that soon crowded my office. When I arrived in the mornings, I smelled outgassed Xerox ink. I had been a few years at UCLA when the UCLA librarian asked me to join a faculty committee on the library of the future—partly, I suppose, because I was known to work with historians of science. The issue was whether some UCLA branch libraries could be closed to free up funding for technology and service improvements. My senior colleagues in the Physics Department were reflexively opposed to closing the Physics Branch Library in Kinsey Hall, but when I learned that they routinely sent their graduate students to make Xeroxes, and that the students wanted more efficient services, I voted for technology and service.

Now the issue was digital publications. The University wished to consolidate its buying power to negotiate prices for digital journal subscriptions for all the campuses. The campus librarians had done an excellent job informing their local committees, and it was an easy matter for me to chair the committee that proposed a university-wide digital library to Dick Atkinson. When Dick called me the second time, he agreed with everything in our proposal but the name; we had called it the *University of California Digital Library*, but Dick wanted *California Digital Library* because it would serve all citizens and not just students and faculty.

The California Digital Library was an intermediate step in the transformation of information management services ahead. Ten years later, when I went back to research after the Scripps directorship, my desktop computer connected me to a global library, and Google Scholar became my librarian. The global digital library environment has become as critical to today's students as the physical library environment had been to the students at Princeton's Fine Hall. The new ease of retrieval permits meta-studies of unprecedented scale and facilitates research across the boundaries of knowledge, but the more profound effect may prove to be quite subtle. Search engines create their own associations between the forms and substance of knowledge. Their algorithms may impose unsuspected changes in the structure of scholarly discourse.

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The third time Dick Atkinson called was to inform me that the search committee for Chancellor Young's successor had given my candidacy to become his successor at UCLA considerable thought. This came as a surprise since I had not applied for the job. Dick said they had concluded I had not had enough experience, but that in the fullness of time, maybe after a couple more years as Executive Vice Chancellor, I was sure to become a chancellor at one of the UC campuses, if not UCLA.

Methodical people, when facing decisions, write lists of pros and cons, as many as necessary for a decision to settle in the mind. I have never been like that, and I wasn't this time. In fact, I wasn't even aware that I had a decision to make. Dick Atkinson's telephone call had the opposite affect; Dick's call, meant to pacify, stirred up internal unrest. Parts of me were in conflicted debate about the course to follow for the next ten years before retirement. Should I commit to the university president track, or should I go back to science? If so, which science? Issues set down on methodical people's pads of yellow paper entered my mind through the back door and found shelter in hidden corners of my unconscious mind. They would emerge from their lairs at night and do battle with one another. The days had the comfortable routine established by Chuck Young during his twenty-nine years as Chancellor.

Events kept reminding me of Dick Atkinson's telephone call. A member of the Board of Regents of the University of Texas called to have lunch with me in the UCLA Faculty Club. He inquired whether I might be interested in applying to be Bob Berdahl's successor as Chancellor at UT Austin, Berdahl having assumed Chang-Lin Tien's position at UC Berkeley. I responded with polite enthusiasm and did not apply. I realized that a continuing number two at a big university with a new number one was a natural target for headhunters, and this Texas thing was likely to happen again. If a UCLA search committee asked me, I would probably take the Chancellor job because of my long embedding in campus life but going elsewhere as Chancellor—even to another University of California campus—was strangely unappealing, even though my mother's part of me would have loved the social prominence and enjoyed the pageantry. I did not think universities fungible and did not look kindly on those who climbed the prestige ladder by committing serial presidencies.

Events also kept reminding me of the scientific career not yet safely in the past. Dan Goldin called from Washington to ask if I could arrange a quiet visit with my UCLA friend and colleague, Bill Schopf, a world-famous expert on the evolution of the earliest life forms on earth. There had been a report of what looked like microscopic animal tracks in a meteorite of Mars origin that had made it to earth. A NASA announcement of the beginning of life on another planet would create a worldwide sensation, and Dan wanted to be sure of the interpretation of the measurements before he signed off on any announcement. After spending a week in Schopf's lab looking at the evidence of early life on earth, Dan decided not to make the announcement.

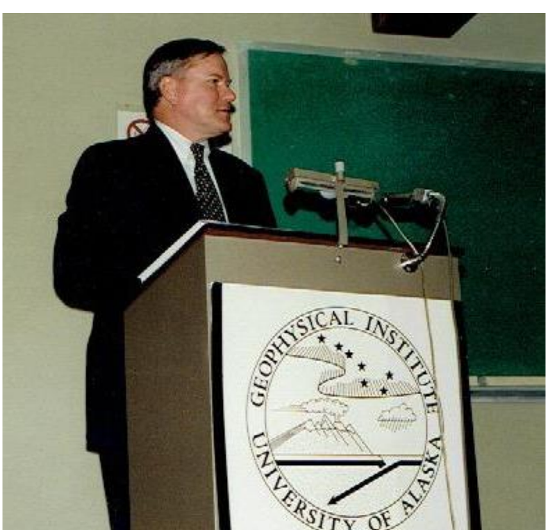


**Figure 24-1: Here I am looking presidential while giving a talk on February 17, 1997, with the pompous title "The Future of Universities," at the Geophysical Institute in Fairbanks, Alaska, which Ellen and I had visited in many summers in the decade before. I would have considered an offer to be UAF Chancellor since we loved Alaska so much. Photo by Ellen Lehman.**

When I had a chance, I sat in on some of the Schopf-Goldin discussions; I hope someone will write an article about them. The issues ranged from what was known about the earliest microscopic life forms on earth, how their tracks in mud were fossilized, how fossil-containing fragments could be blown into space from Mars' surface, and how meteoroids found on Earth's surface could have migrated from Mars. Their dialogue reminded me how much I missed science's conceptual fascination, but it was not clear that I could go back to science. It had been six years since I had last done research; who would take me? Should I go back to space plasma physics, which I was already finding repetitive, or ahead to earth system science, where I had neither formal education nor experience of discovery?

At the end of the day, there was something vaguely unsatisfying about being UCLA's Executive Vice Chancellor. The job had its satisfying parts; it embedded me in UCLA life, Los Angeles society, and University of California tradition; it was a good job for somebody, but not for me. Nonetheless, I was not eager to change jobs. I found the whole idea unsettling. I had found a reasonable path to the end of professional life, but now what I describe next had to happen.

**Figure 24-2: On the university president track: Stanford President John Hennessy (left), Charlie Kennel (center), and UCLA Chancellor, Al Carnesale (right).**

**Figure 24-3: University of California President Richard C. Atkinson (left) and UCLA Executive Vice Chancellor, Charles Kennel (right).**

# 25

# On Becoming Scripps Director


**Figure 25-1: Collared by Scripps. I had been Director for five years when this picture was taken. I was still not feeling fully secure there since I am wearing a suit when the Scripps faculty style ran towards a T-shirt and bathing suit.**

Ellen planted the Scripps idea in my mind. Toward the end of my stay at NASA, we made a trip to New York for me to be interviewed for Director of the Lamont-Doherty Earth Observatory and the Earth Institute at Columbia University. The two jobs are now each full time, but at the time it was thought that I might spend half the week in the City for the Earth Institute and the other half eighteen miles north in the riverside town of Palisades at Lamont-Doherty. Michael Crow, Columbia's charismatic Vice Provost who later founded an entire school devoted to sustainability at Arizona State University, had the notion that the subject was big enough to warrant organizing a university around it. He started at Columbia by connecting the social sciences at the New York campus with the earth sciences at Lamont-Doherty. Neither New York City nor Palisades appealed to Ellen, either for our residence or her psychology practice. I found the Lamont-Doherty campus dilapidated relative to Chuck Young's UCLA. Ellen asked why not wait until Ed Frieman retires and then go to Scripps, and that did it for me. We would return to California. Nonetheless, that I could have gone to Columbia and worked on climate change contributed to the restlessness I felt as Executive Vice Chancellor at UCLA.


**Figure 25-2: Ellen Lehman made a great sacrifice for me to take the Scripps Director job. While she continued her psychology practice in Santa Monica, where she was known as "Dr. Lehman" half the week, she commuted 130 miles to La Jolla to assume the "Mrs. Director" role the other half of the week. She made the weekly commute from March 1998 until March 2020, when we decided to spend the pandemic in San Diego and this memoir was composed. Here she is speaking to the E. W. Scripps Associates in August 2003.**

Ed Frieman retired as the Director of the Scripps Institution of Oceanography (SIO) in 1996, and he and Dan Goldin badgered me until I applied to be Ed's successor. I was reluctant to leave UCLA after such a short time, but Frieman urged me to visit Scripps to take a look. One does not readily refuse an invitation from one's thesis advisor.

What I found was a spectacularly situated campus with elegantly shabby buildings, tattered by use and scattered in insouciant disarray on gray-green hills that sloped down to a wide beach bordering the vast blue expanse of the Pacific. Scripps had what had to be the loveliest director's office in the world, perched on the seawall a few feet from the beach. This office featured a floor-to-ceiling bay window through which you could see the surfers on the waves rolling in.[86] On windy days, that window would be spattered with spray. The adjoining administrative offices were quiet and reassuringly unimposing, and my predecessor Bill Nierenberg's by then slightly scruffy rose garden was out front. Halfway up the hill was an elegant little aquarium with children and parents at play. Frieman took me to another big bay window in the building named for SIO's legendary oceanographer, Walter Munk. The Munk Building was high enough up to see the vast blue expanse of the Pacific Ocean. I pictured Japan to the west beyond the horizon and palm trees on islands in the South Seas. The peaceful murmur of the surf filled the air, which was redolent with the perfume of flowers. Frieman remarked softly, *this could be yours.*

Ellen and I had already been debating how we wanted to live the next years of our lives. The time was coming when we would not want to take the weekend trips that had been our recreational pleasure. The 1984 LA Olympics were thirteen years in the past; LA was growing more crowded and its traffic intolerable. It was time to find a weekend place in the mountains, and we had been discussing alternatives.

One weekend, Ellen and I decided to explore the beautiful town of Ojai, about 75 miles north of UCLA, where there is a well-known music festival in the summer. I did not even tell Shannon White, my assistant in the UCLA Chancellor's Office, where Ellen and I were going. So far as I could tell, no one knew. About an hour after we arrived at our motel there was a phone call for me in our room

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[86] Some waves, I was later to learn from Walter Munk, came from as far as the South Pacific.

from Dan Goldin in DC. How did you find us? I asked. White House operators can find anybody, Dan said. Then Goldin bored in: how many people at UCLA could do your Executive Vice Chancellor job tomorrow? I dunno, I replied, perhaps twenty-five? How many, he asked, could head a world-leading scientific organization with global responsibilities? Fewer, I replied. There's your answer, he said; you've got to take the Scripps job.

That did it. I took a pay cut and moved south to La Jolla as UC San Diego's Vice Chancellor for Marine Sciences and the Director of Scripps. Ellen, who made the bigger sacrifice, spent Thursdays to Sundays as "Mrs. Director" in La Jolla, and Mondays to Wednesdays as "Dr. Lehman" in Santa Monica for the next twenty-two years. In 2003, we bought that place in the mountains—not in Ojai but forty-one miles northeast of La Jolla in Ramona.

I told people later I could not get climate change out of my head; that was true but not the whole truth. The way to work on something truly big is to work on something bigger than yourself. Hannes Alfvén had planted a seed that took sixteen years to bear fruit. It took seeing the Berlin Wall brought down on television and being in Moscow when Gorbachev was brought down on CNN. It took seeing the Cold War's end through the eyes of friends in Moscow and seeing their scientific and personal lives scattered to the four winds. It took seeing a personal hero, Roald Sagdeev, doing more than even Alfvén hoped for to end the Cold War. It took seeing the Cold War context that that shaped international science disintegrate. It took seeing a chance to have the winds of history at my back a second time.

Chancellors Carnesale of UCLA and Dynes of UCSD agreed that I would divide my time between the two campuses for a six-month period, and for those six months I did two jobs badly. Ellen and I commuted the 130 miles between Santa Monica and San Diego every week, and when in La Jolla we stayed in a small apartment in Bob Dynes' chancellor's residence. My first "official" appearance as SIO Director was in October 1997 when Ellen and I appeared at Walter Munk's eightieth birthday party at his and Judith's home in La Jolla. I soon learned that an invitation to the Munk home was a rite of passage for earth scientists.

During one of our visits, Ed and Joy Frieman gave us an elegant reception at their house in the hills overlooking the ocean. When it was time for me to speak, I praised Ed's persuasiveness but said that, really, Scripps sold itself. I also spoke of the first time UCSD had tried to recruit me. This story starts in Trieste in 1965 when Burt Fried, the *magister ludi* of UCLA plasma physics, also tried to recruit me; Burt continued his pursuit into the following year when I returned to Boston and the Avco-Everett Research Laboratory. Burt and I ultimately agreed that I would become an associate professor of physics at UCLA with tenure at a nine-month salary of $11,000, beginning July 1, 1967. Burt said the University of California bureaucracy grinds slowly, and I would have to wait a few months before the deal is official; *in the meantime, he said, if anyone gives you a better offer, let me know and I will top it.*

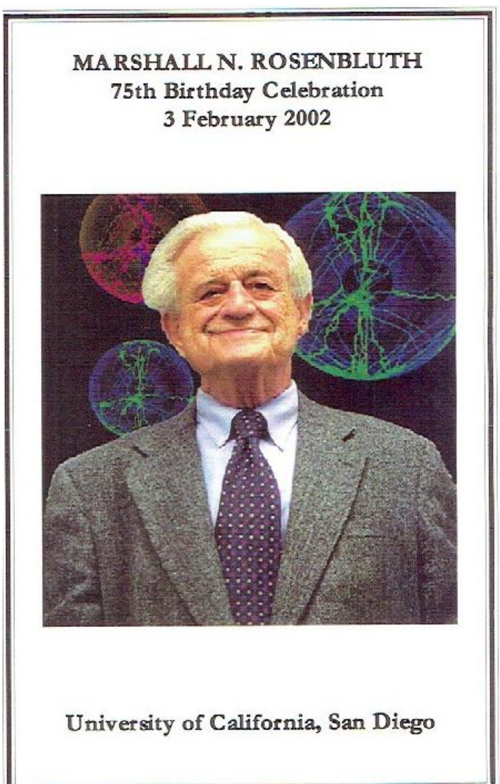


**Figure 25-3: "The Pope of Plasma Physics," Marshall Rosenbluth, became one of my exemplars in 1961 when I saw how my Princeton professors revered his contributions to fusion research. Marshall and Roald Sagdeev co-chaired the 1965-66 plasma physics workshop at the UN's International Centre for Theoretical Physics, where I was introduced to international science. Though we never collaborated, our professional lives would be intertwined for the next 42 years until Marshall died in 2003. Even more impressive to me than his unusual talent in physics was his single-minded passion for fusion research; He wanted to harness the energy source of the stars for humanity's benefit. Twenty years later, we still do not have a working fusion reactor, but there is another reason for keeping at it: climate change**

A few months passed without incident, but three weeks before I was to be officially notified by UCLA, my Trieste colleague, Marshall Rosenbluth, the "Pope of Plasma Physics," called. I am about to leave UCSD for the Institute of Advanced Study, Marshall said, and there are some people here who think you should take my position; will you drive to La Jolla and give us a talk? One doesn't say no to popes, and soon I found myself on the Scripps lawn having lunch by the sea with Marshall, the plasma physicist Bill Thompson, the astronomer Geoffrey Burbidge, and the geophysicist Walter Munk. I *was* tempted. La Jolla in 1967 was a subtropical paradise; the ocean calm, the sky flawless blue, the cool evening air perfumed with the scent of desert flowers, the San Diego weather even better than LA's. As if paradise were not temptation enough, my space physics colleague from the Petschek summer seminars, Ian Axford, had decided to go to UCSD. I owed it to Burt Fried to let him know I was being propositioned. I told Burt about Marshall, the lunch, Ian Axford, and the paradise. Burt's next communication with me was an improved offer with a salary of $11,600. I went to UCLA for the extra $600.

I told the people at Ed and Joy's welcoming party that I had wondered what life would have been like had I gone to UCSD rather than UCLA in 1967. Things in 1997 could have been the other way around. Ellen and I could be leaving La Jolla rather than coming to it. We might be attending a UCLA welcoming party instead. One thing was certain: that party could not possibly be as nice as Ed and Joy's.

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Wolf Berger became the SIO Interim Director after Ed Frieman stepped down, and he kept what I was to learn was a highly temperamental institution at peace for two years until he handed the reins to me. I heard no complaints about his leadership, except that he felt unjustified as an interim director in committing to new faculty appointments. I soon came to appreciate that he was one of SIO's most honored faculty members, his work in paleo-oceanography having earned no fewer than eight major international awards, but Ellen and I had the special privilege of getting to know his lyrical side.

Ellen soon settled into the routine she kept for twenty-two years until the global pandemic started in 2020. She would spend Mondays through Wednesdays seeing patients in her Santa Monica office and drive to La Jolla on Thursdays. We often celebrated Ellen's arrival by going for burgers and beer at the nearby La Jolla Sea Lodge (now the La Jolla Shores Hotel), where we often found Wolf and Karen Berger also celebrating Thursday together. There, Wolf's restless romanticism filled our conversations with pleasure. He had published children's books and San Diego County trail guides. No one of my acquaintance, foreign or native born, has had such a rich command of the English language. This came as a surprise since Wolf still spoke with a slight German accent. In retirement Wolf wrote a book, *Ocean*, which was as much a story as a textbook, wherein he lavished the same gentle lyricism on his science as he did on his friends, walks in nature, and young children. When he died in 2017, Ellen and I wrote this public tribute:

> We will miss Wolf. And so will Scripps, more than we know. Wolf wrote a lyrical book on oceans and oceanography that told the story of ocean research and the people and institutions who did it. His deep humanity shone through, as did an almost poetic writing style. We had a fine human being and a great scientist with us for more 50 years. Wolf is one of the reasons that Scripps is the legendary place it is to oceanographers and earth scientists.

A familiar face was there to greet me when I made my first visit to Scripps' administrative offices as the new Director. Tom Collins had been the business manager of the UCLA Physics Department when I had been its Chair, and though more than a decade had passed we took up where we left off. Tom had the romantic notion that the Scripps support staff was a family, and he cared for the staff individually and collectively as if this pleasing fantasy were true. About a year after I arrived at SIO, Tom brought Doug Bennett into the Deputy Director's Office. Doug and I had worked together in Mission to Planet Earth at NASA, and when Tom retired and Doug took over his responsibilities, Doug and I were also able to take over where we, too, had left off.

Although Doug did not see himself as romantically as Tom did, Doug was equally caring in his own way. A gentle and kind man, Doug took ordinary people's issues to heart. When staff came asking for time off for family emergencies, Doug already knew that I believed family comes first, and we would always grant paid leave, saying we knew they would make up the missing work when things got better for them. In many ways the staff understood Scripps' mission better than the complicated and self-involved faculty did. While my and Doug's work together was not free of emotional stress, the stresses were about jobs getting done and not about staff morale.

The birthday celebration for a much-loved faculty member, Fred Spiess, was held in a tent near the glorious Director's Office during my two-campus period. It was a loud, beery affair with energetic and remarkably ungraceful dancing. The out-of-town VIPs were consigned to a "head table" at the quieter far end of the tent. The new Director, one of those out-of-town VIPs, was seated next to a friend of Scripps from the Office of Naval Research in Washington who knew us well. Scripps never stints its guests and there was plenty of beer even for those far from the action. At some point this friend who knew us well turned even more garrulous, leaned over to me, and shouted over the laughter and music, *SIO is a pretty close-knit place, isn't it?* He bet that even if I were Director for ten years, I would not be fully accepted.

Over the years, I have pondered what this august gentleman meant. If you ask me now, I would say there is an independent streak in Scripps faculty that values the freedom to investigate the useless arcane and delights in knowing the uncommon. By emphasizing societal issues like climate change, my oft-stated desire that we make a historical mark threatened the unusually independent spirit that was what they loved above all else about Scripps.

During my weekly visits to La Jolla from UCLA I closed the deal on the house on Mt. Soledad that Ellen and I still have, and in March 1998 I transferred to full time at SIO. My installation had its complexities. I had taken a salary cut, and to compensate for the financial loss Ed Frieman arranged a one-day-a-week consultancy at the defense company SAIC, where he was a vice president. I ultimately did not take his offer. Ed and Joy had a lovely apartment on the Île Saint-Louis in Paris, and as Ed's own term wore on they spent more and more time there. His absences aroused resentment in some SIO faculty, including Russ Davis. I came to appreciate Russ' blunt honesty, but this first time it took me aback. He had learned that I was going to consult for SAIC, and he warned me that the faculty did not want another director spending part time on the job. I immediately informed Ed that I would not take up his consulting offer.

Shortly after turning down the SAIC offer, I learned that my Israeli space plasma physics colleague, Arkee Eviatar, had arranged for the award of a Wolf Prize to me. It required about two weeks in Israel giving lectures on space physics, and after Russ Davis' scolding

I could not justify time away from campus on something that would not add to Scripps' reputation in ocean sciences. I turned Arkee down.

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It would take an entire book to recount all the things beside a prickly faculty that make Scripps a great institution. Though SIO tolerates theoreticians and has had good ones, after the experience of World War II it became a ruthlessly hardnosed sea-going organization that reveled in the murder of beautiful theories by ugly facts, to steal words from Charles Darwin's acolyte Thomas Huxley. Scripps operates four ocean-going research vessels on behalf of the National Science Foundation and the US Navy, and researchers from around the world meet in San Diego to go down to the sea in Scripps ships. There is no discernable difference between night and day on a research cruise, and sea-going researchers forge unusually close emotional bonds as they stand chilly watch in lonely parts of the ocean where commercial ships rarely go. Many of SIO's research ideas (and attitudes towards land-bound leaders) are incubated far from home.

SIO is the oldest oceanographic institution in the US. It was founded as a marine biology association in 1903, the year of the Wright Brothers' first flight of a heavier-than-air ship, and the year the Ford Motor Company was founded. In its early years, SIO's research projects were supported out of pocket by the newspaper magnate Edward Willis (E. W.) Scripps. E. W. loved his marine lab, and he even drove his own tractor to clear the road down the hillside to the shore. It was his half-sister, the renowned San Diego philanthropist Ellen Browning Scripps, who convinced E. W. to make a more sustainable financial arrangement. In 1910 he gave the San Diego Marine Biological Association to the University of California along with a sailing vessel for which he paid $2,000, 170 acres of virtually deserted land in La Jolla Shores for which he paid $1,000 and a promise that his family would continue to support the University of California's new research station.[87] Every succeeding generation of the Scripps family has kept his promise, and the land

[87] It is an SIO legend that E. W. Scripps, who owned the local newspaper, intimidated other bidders so that our property sold for the auctioneer's minimum price of $1,000.

he bought for $1,000 would now pay for several aircraft carriers. That land by the sea is SIO's greatest endowment.

Scripps became the largest private oceanographic research institution in the US. Shortly after Scripps' second Director, the geologist T. Wayland Vaughan, arrived in 1924, the University of California renamed E. W.'s biological station the *Scripps Institution of Oceanography* to acknowledge the breadth of research taking place there. SIO now has first class programs not only in marine biology and physical oceanography, but also in biological oceanography, atmospheric chemistry, marine chemistry, geochemistry, seismology, volcanology, glaciology, hydrology, climate science, marine conservation, marine archaeology, and more. A NOAA laboratory devoted to fisheries management is on the Scripps campus. The J. Craig Venter Institute placed a laboratory on the Scripps campus specializing in genomics. Scripps' 2020 annual expenditures are close to $200 million per year, that of a small liberal arts college.

Scripps evolved into a single-department school divided into affinity groups defined only approximately by research specialty. The groups' compositions reflected past factures of collegiality. At an institution like Scripps, your greatest critic is not found at professional meetings but down the hall. There is no equivalent institution to escape to when interpersonal conflicts become intolerable, but you do not have to encounter your nemesis at group meetings. Though the organization of SIO's disciplinary groups made general sense, the exceptions to the rule could only be understood as the residue of past battles still living in the minds of the antagonists and carefully related to the new Director. On the positive side, SIO's loose administrative structure mirrors the intellectual structure of earth system science, the multi-disciplinary construct designed in the 1980s to organize observations of the earth from space and to provide a framework of concepts to describe global climatic and environmental change. Earth system science evolved after most research universities had already committed to narrower teaching departments in the classical geophysical sciences, but UCSD and Scripps had been under no such constraint.

Dollars and cents provided the common language of discourse when I oversaw Chancellor Young's academic budget reviews at UCLA, but what measures should I use to judge Scripps' progress? Faculty tended to cite various measures of professional reputation, of which the ratings by the National Academy of Sciences were valued most by science faculties. (The ratings of the *U.S. News and World Report* were touted more by development offices.) Scripps was doing well enough in such categories. As I reflected on the virtues of top ten departments in the Academy rankings, however, I could not tell the difference between, say, number three and number eight, despite number eight's loudly stated ambition to close what it saw as the critical gap separating it from number three. Excellence at top-ten level is accomplished in ways that cannot be measured by polling; other things matter. Dan Goldin emphasized SIO's proud history and impact on global society; my first father-in-law, Salomon Bochner, talked about foundational advances in a fundamental branch of knowledge; Ed Frieman told me he concentrated on increasing SIO's funding. Which should SIO aim for: financial security, disciplinary recognition, or societal impact? The chancellor cares about the first, the faculty cares about the second, and the public cares about the third. Surely, I thought, societal impact is the ultimate arbiter of excellence. Of course, no one knew how to measure it, but we were smart enough to know it when we saw it. Trouble is, you can't have the third without the other two.

The first thing was to trust an institution at the top of its game to make good faculty choices. Ed Frieman had stepped down as Director two years before I assumed full-time leadership, and a backlog of open positions had built up through retirements and separations. Wolf Berger did not feel he should make new appointments, so faculty rejuvenation was on faculty minds when I arrived. While I could not and would not evaluate individual candidates, I could influence the way we went about searching for them. I hoped to use the search process to promote multi-disciplinary interactions that could strengthen SIO's presence in policy and public advocacy circles.[88] We went on an appointments binge; we aggregated about ten open appointments into a single search, and several previously authorized searches took advantage of the cornucopia of applicants the general advertisement attracted. That advertisement did not specify a subject but listed all those pursued

[88] I had been particularly put out by the fact that while Ramanathan and Crutzen had spearheaded the INDOEX project, which showed a vast cloud of pollution generated by open fire cooking by the poor in India covering the entire north Indian Ocean, Ram was invited to a speak at a major symposium attended by the president of India at Harvard's Kennedy School. While happy for Ram, I asked myself why, if we did the science, were we not alert enough to reap the recognition for seeing its policy implications?

at SIO. Each disciplinary group's search committee would appraise candidates, but the Institution would choose the most promising regardless of field, with a preference for the assistant professor level. The full faculty would vote on each appointment. It took finesse to organize the interviews, seminars, and group discussions that followed, but I hoped the discussions surrounding our appointments binge would help people get to know about fields other than their own. Today, with more than 100 tenure-track faculty; over 200 postdocs, project scientists, and researchers; and more than 400 graduate students, Scripps can deploy about 700 professionals on problems in earth system science.

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I never was quite sure how the Scripps community believed SIO directors should behave. I believed that I should build SIO's presence in influential circles. Here I took cues from the examples of two of my predecessors, Roger Revelle and Ed Frieman. Walter Munk told me that Revelle had loved San Francisco's Bohemian Club, an enclave of the Republican aristocracy. I joined the club and went to its summer encampments at the Bohemian Grove, where I gave lectures on ocean conservation and climate change, hoping to influence the members' thinking. (I especially recall a conversation on climate change with Mr. Justice Clarence Thomas.) For influence in Washington, I could not top Ed Frieman's membership on the President's Science Advisory Council, but I did chair the NASA Advisory Council, a position which kept me connected to high government thinking about science policy. I dropped out of the Bohemian Club after three years but stayed on the NASA Advisory Council until 2007.

**Figure 25-4: One of the main tasks of Scripps directors is to go to Washington to advocate the funding of research programs, preferably Scripps' own. This photo, taken on May 10, 2002, in Washington DC's Cosmos Club, shows, left to right, the eighth Director of the Scripps Institution of Oceanography, Ed Frieman; the ninth Director, Charlie Kennel; Joy Frieman; SIO's great oceanographer, Walter Munk; Judith Munk; and apologies to the unidentified woman on the right.**

# 26

# Scripps and the Global Ocean

It was to be a celebration, but I am afraid I turned it into a wake. I was speaking at a reunion of an extraordinarily tightly knit group of SIO's doughty sailor-scientists. They used to spend months away from home, first with the US Navy during World War II, and then together on long SIO cruises to map the circulation and bathymetry of the Pacific Ocean in the 1950s and 1960s. With the unpretentious practicality of WWII Navy veterans, they made scientific contributions of truly historical significance. I said they had carried on the great tradition of global oceanography started by the British *Challenger* expedition of 1872-76.

This praise was received by the proud old salts as their due. Then I deviated from my prepared remarks on a riff about the historical significance of the era they had helped create. Their voyages were the last expressions of the great age of European exploration begun by Prince Henry the Navigator in the fifteenth century. Science provided a reason for ocean voyages in the late seventeenth century, when Edmond Halley led three cruises of the *Paramour* to test his friend Isaac Newton's theory of gravitation. Pacific Ocean explorers like James Cook and Louis Antoine de Bougainville carried on the tradition of exploration science in the eighteenth century. One Cook voyage went to Tahiti to observe an astronomical eclipse. Ocean science itself became the objective of the *Challenger* expedition. At the turn of the twentieth century, the polar explorers Nansen, Scott, Shackleton, and Amundsen were still exploring parts of the earth where Euro-American explorers had not been.

The Scripps research cruises of the 1950s and 1960s were the last great scientific voyages of the great Age of Exploration. The great Age came to an end sometime between 1972, with the first Landsat space mission, and 1992, when the Topex/Poseidon mission, a joint venture of NASA and France's CNES, measured the height of every ocean wave with one-centimeter precision. Scripps veterans had told me their most frequent question when wandering the trackless Pacific expanse had been "where are we?"; now the global positioning system locates ships to one-centimeter accuracy and not one square centimeter of land surface remains unmapped. The oceanographer's life-or-death question is no longer "where are we?" but a more distantly threatening "what is changing?" The questions had changed, I said, but these doughty sailor-scientists could be sure that the young explorers of today's Scripps were pursuing the new questions with the same passion as they did during SIO's Golden Age.

SIO's *emeritus* oceanographers still get squirrelly whenever someone brings up the space program. That satellites 100 miles up could sense the rhythms of the ocean and feel its moods seemed alien, even illegitimate, to them. You can't feel the rhythms of a computer voyage into a mathematical sea. There is no salt air. There is nothing like being there oneself.

Russ Davis (1941-2022) made Scripps' identity crisis real to me. He was in a plaintive, even wistful mood in this particular conversation. When he came to Scripps, Russ said, sea-going physical oceanographers were the heart and soul of the Institution, and now physical oceanography is a handmaiden to the younger earth science disciplines. All the others want now, he said, is our routine data so they can do their work. They do not need us to generate new ideas. The Scripps that had become great in the 1950s and 1960s had been hollowed out. I did not intervene to remind Russ that his own work with ARGO was continuing SIO's leadership in physical oceanography, but I could see his point. ARGO was supported for its importance to climate change research, and the intricate questions physical oceanographers and navies are interested in no longer dominated the decisions about Argo's funding.

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Walter Munk (1917-2019) was a physicist's geophysicist and a geophysicist's oceanographer. He had the same semi-divine status in his field that the founders of quantum electrodynamics had with my physics graduate student friends at Princeton. The 2021 Nobel laureate in Physics, Klaus Hasselmann, went so far as to publish the series of worshipful interviews he conducted with Walter in 2010. The interviews gave Klaus an excuse to visit Walter as Klaus had done in the 1960s when he wrote some of the papers that would contribute to his prize.[89] I myself saw Klaus conducting his interviews at the round table in Walter's garden—you know, the one with the spectacular view of the Pacific Ocean.

By attending events celebrating Walter's achievements I came to see that every seven or so years in a 70-year career(!), Walter would establish an intellectual beachhead in a new area, do enough foundational work to consolidate its viability, leave the follow-up to the others who flocked in after him, and look for the next opportunity for an original contribution. As Walter neared the end of his long and remarkably productive life, the local newspaper, the *La Jolla Light,* began to call him the "Einstein of the Oceans." Oceanographers felt the term did Einstein honor.

One day I caught a glimpse of the profound drive obscured by Walter's Viennese gentility. He hobbled into the Director's Office to inform me that he would be absent from SIO for two weeks. (Notifying the Director of long absences from campus was a quaint courtesy from another time that was ignored by every other faculty member.) Walter had to return to the Boston surgeon who had done his earlier hip replacement to have another done, the last one now having obviously failed. Walter was away for twice two weeks, but I did not think much about it as I was not in the habit of policing faculty absences. After his return Walter *strode* into my office to apologize for his extra-long absence; his surgeon had been called away in an emergency, and Walter's hip replacement was deferred for about ten days. What did you do while you waited for your surgeon to return, I asked. I went down to Woods Hole and gave lectures, he said. I later learned that he gave a whole series of lectures hobbling about on his bad hip. He was in his eighties at the time.

Walter and his longtime second wife, Judith, were mainstays of the La Jolla community for decades, and an invitation to their home with the garden looking out over the ocean and the theatrical stage in the garden was initiation into full membership in an exclusive club

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[89] Klaus was, along with my Russian colleagues, Vedenov, Velikhov, and Sagdeev, an originator of the quasi-linear theory of wave turbulence that I used in my work with Harry Petschek.

of oceanographers. In Walter's view, whoever directed Scripps was an oceanographer, and Ellen and I—and even our Skye Terriers, Tammy, and later Forbes—were frequent guests. Walter and Judith probably entertained guests from the scientific and San Diego communities three or four times a week. That was one of the secrets of Walter's emotional resilience: he never missed a party if he could help it. Sociability was the foundation of his remarkably productive old age. He knew one outlives the intellectual and social milieus nurturing one's creativity, but the simple social lubricants of wine, food, and conversation would enable the next enlivening milieu to self-assemble around him.

When I became Director in 1997, Walter was investigating a new way of measuring the increase in ocean temperature due to greenhouse warming. It is unfair to focus on one of the few areas he originated where he did not come out first, but Walter's work on acoustic tomography presented the first deep problem I had to deal with as the new Director. I have long thought that Robert Hooke's greatest tragedy was to have lived in the same scientific era as Isaac Newton, for otherwise Hooke would have been the most important English scientist of his generation. The dispute between Newton and Hooke about the nature of light is legendary in the history of physics. The conflict between Walter Munk and Russ Davis on how to measure ocean temperature brought the Hooke-Newton controversy to mind. Like Hooke, Russ Davis found himself in a more famous scientist's shadow.

It was scandalous in an era of global warming that the temperature of the ocean below the surface was not well measured. My SIO colleague, Tim Barnett, had complained to me about the inadequacy of ocean temperature observations with unusual passion, even for Scripps. Theory was suggesting that up to ninety percent of the energy added to the earth system by anthropogenic greenhouse warming should be taken up by the oceans. Denialists and skeptics could use the apparent absence of ocean temperature increase to argue against the significance of the human role in greenhouse warming.

The observational challenge was that the heat capacity of water is so large and the oceans so huge that it takes a vast amount of energy to raise the average temperature of the ocean by even 0.01° C. To see the expected increase, oceanographers had to measure tiny changes in temperature over an immense volume. Moreover, the spatial coverage of the measurements was not up to the job because measuring instruments dropped from commercial ships are confined to commercial sea-lanes and miss important parts of the global ocean circulation. The bottom line was that oceanographers could not say definitively whether the upper layers of the ocean were warming as expected. Oceanography was failing its biggest test, I thought.

Walter Munk had been obsessed with ocean waves since World War II when he was tasked with predicting the wave environment for the Allied Normandy invasion in June 1944. Years later, he pioneered an elegant way to measure ocean temperature by generating low frequency sound waves that propagate over huge distances in a waveguide mode, confined to the upper few hundred meters of the ocean. Since the sound wave speed depends on temperature, measuring sound travel times between two points gave the temperature averaged over the wave propagation path. Moreover, that temperature would be measured at the depths where greenhouse warming was expected to be strongest. Munk had already shown that low frequency sound could propagate intercontinental distances by launching sound waves from Heard Island in the south Indian Ocean and detecting them thousands of miles away at Scripps on California's coast.

To make enough sound observations for the climate change problem, systems of transmitters and receivers would be necessary in every ocean of the world. Munk's acoustic thermometry project hit a snag when the US Navy, which wanted to use low frequency sound waves, this time to communicate orders to launch nuclear missiles from submarines, built a much more powerful transmitter than Munk's temperature system needed. Whales, who had employed the waveguide transmission channel to communicate with one another for thousands of years, formed cetacean tribes that communicated in distinctive languages. Scripps' John Hildebrand had listened in on whale conversations and could hear them discussing where their next dinner could be found. Even the engine noise of distant ships interfered with their conversations, but the Navy's intense transmissions drove a particular species of whale, beaked whales, crazy—crazy enough to beach themselves and die.

Pictures of dead whales attract attention. The environmental community swung into opposition to Walter's project. Advocacy groups opposed Munk's project where it hurt, in the opinion of the environmentally responsible public. Before my arrival at SIO, Walter had asked the Director's Office for help in explaining that the potential value of acoustic tomography to the diagnosis of ocean warming was greater than the damage to whale hearing from his low-power transmissions. His system used much less power than the Navy needed, and its sound levels were less likely to drive beaked whales out of their minds.

Nonetheless, acoustic tomography was intentional creation of sound pollution, and this added to its ethical burden. I recognized that solving one environmental problem can incur other environmental costs that have to be counted as the price of progress. I certainly did not enjoy public conflict with conservation biologists, but that is not the only reason why I felt ambivalent about acoustic tomography. To the physicist in me, Munk's was an extraordinarily elegant technique, but it measured temperature averaged along the sound propagation path. The measurements would be open to multiple interpretations. In an age of climate change denialism, the devilry in this detail would complicate the maintenance of credibility so essential to the public reception of climate change research.

Walter's method, properly implemented, would have answered a very important scientific question: is the global ocean warming as a whole? But I had recently arrived from NASA and an additional issue was in my mind. Acoustic tomography could not map where the ocean was warming and where it was not; it could not identify except in general terms the coastal regions most at risk from sea level rise; and it would not provide the related oceanographic data that would enable ocean modelers to guess where changing ocean currents will cause sea level to rise in the future. In other words, his method would enchant other scientists but it would not help future decision-makers cope with the consequences of ocean warming they will face. At the time, around the millennium, the earth science community was designing global observing systems that strained the financial and technical capacities of international science. You could only build one at a time. It was better to start with designs with broader conceptions than to build one whose expense still precludes other initiatives.

Walter Munk's acoustic tomography is now being used, but it has not advanced climate change research as decisively as the technique invented by his in-house rival, Russ Davis. Russ Davis' approach to measuring ocean temperature is a satellite-era extension of what sea-going oceanographers have been doing since the late nineteenth century. In the 1980's, Davis found a way to fit the temperature, density, and salinity instruments ordinarily deployed from ships into automated submersible floats. These little robots were programmed to carry instruments to depths of up to 1,000 meters, reside at depth for about two weeks while traveling with the currents, and then return to the surface, where their stored data would be telemetered to a spacecraft passing overhead. The paths followed by the instrumented floats gave hitherto unavailable information about the ocean currents at depth, as well as the record of temperature, density, and salinity measured beneath the surface. Knowing the temperature you could calculate the heat content of the upper ocean, and with worldwide coverage you could assess the rate that ocean heat content is changing due to greenhouse warming.

Russ Davis and his longtime Scripps collaborator, Dean Roemmich, proposed that national ocean agencies deploy a portion of a global system of floats and share all the data acquired by the system. This project was given the name *ARGO* after *Argonautica*, the ancient Greek epic poem. The elegance of ARGO was not in its gigantic elaboration of fundamentally brute force measurement techniques but in the enchantment of its social design. A worldwide system of ARGO floats required international coordination and funding, exactly what would be needed if society were to make progress in fighting climate change. The US National Oceanic and Atmospheric Administration (NOAA), then led by D. James Baker, together with the UNESCO Intergovernmental Oceanographic Commission (IOC), directed by SIO graduate Patricio Bernal, had taken up the challenge of securing and organizing international participation in ARGO.

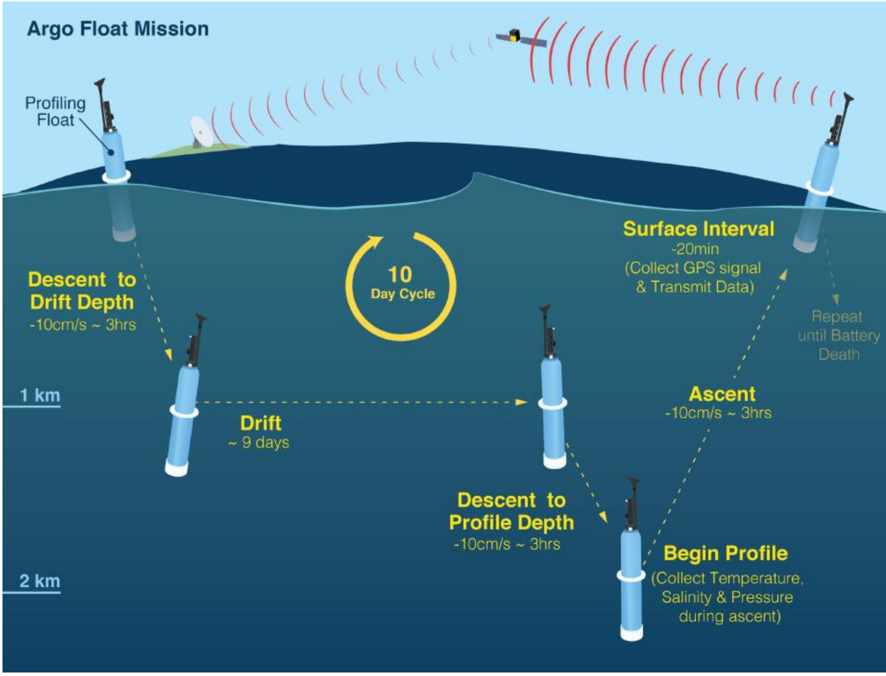


**Figure 26-1: A schematic of the operation of an Argo profiling float. The little robot submerges, drifts with the ocean currents, and then dives deeper to record the temperature profile from 1,000 meters as it rises to the surface. When the float arrives at the surface, it runs out an antenna and communicates its stored data to a satellite passing overhead. It will repeat this cycle until it runs out of battery power.**

SIO's Director of International Relations, Lisa Shaffer, and I took it upon ourselves to carry the torch for ARGO. At NASA, Lisa and I had worked with the International Committee on Earth Observations, which coordinates space agency earth observation programs, and we believed the directors of ocean institutions needed a similar forum to coordinate ocean observations. We started by lining up Bob Gagosian, then Director of the Woods Hole Oceanographic Institution. With the two most famous ocean institutions in America aligned, Lisa and I went on to recruit the Director of the National Oceanography Centre at Southampton in the United Kingdom, at the time John Shepherd, soon after, Howard Roe. This was enough to convince Scripps graduate Patricio Bernal at the UN to host a larger recruitment effort. The Intergovernmental Oceanographic Commission (IOC) would sponsor an exploratory meeting of what Lisa called the Partnership for the Observations of the Global Ocean (POGO). Later Lisa and I visited the Japanese

Agency for Marine-Earth Science and Technology (JAMSTEC) in Yokohama, and China's First and Second Institutes of Oceanography (FIO and SIO) to further promote international coordination of ocean research through POGO. Jesse Ausubel of New York's Rockefeller University shared our thinking and, crucially, secured funding from the Lounsbery Foundation for the first three POGO meetings. After that, POGO would become a dues-paying organization. Scripps hosted the first official meeting, POGO-1, on its campus in 1999. POGO now has forty-nine institutional members in twenty-eight countries. Jesse Ausubel was recently awarded the Nierenberg Prize, Scripps' most important, for work on the Census of Marine Life and POGO.

ARGO has become a system of almost 4,000 floats. I am not alone in believing that ARGO is the most significant observational capacity created for the diagnosis and modeling of climate change in the first two decades of the twenty-first century.

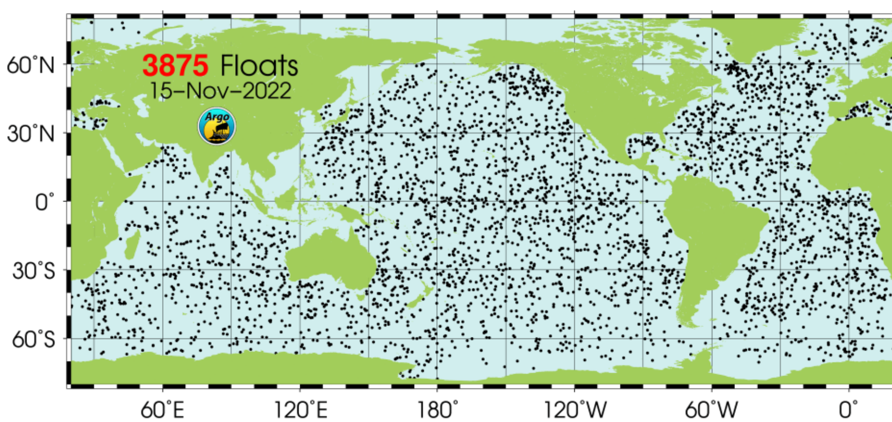


**Figure 26-2: This map shows the positions of Argo floats on November 15, 2022. The natural circulation of the ocean does a pretty good job of distributing Argo's observing floats around the global ocean.**

# A POGO Rogue's Gallery

**Figure 26-3: Lisa Shaffer conceived the name *Partnership for the Observation of the Global Ocean* as much as anything for the enchantment of the acronym formed from its initials, POGO. She took this picture in 1999 of three of POGO's early instigators, Charlie Kennel of Scripps (left), Bob Gagosian of Woods Hole (center), and Jesse Ausubel of Rockefeller University (right). There has always been friendly competition between Scripps and Woods Hole, but the two have a tradition of agreeing when it is scientifically important. Without the agreement of the two best-known US ocean research institutions, POGO would have been dead in the water. Jesse's devotion to ocean issues was inspired by his work on an academy committee with my predecessor as Scripps Director once removed, Bill Nierenberg. Jesse found the financial support that made POGO feasible.**

**Figure 26-4: The G (for global) is essential to the name, POGO. POGO would have been a nonstarter without the blessing of at least one international oceanography research center. The UK's National Oceanography Centre at Southampton, Britain's largest, stepped up to the plate, and its Director, Howard Roe, became an ardent POGO supporter. In January 2008, the Bermuda Institute of Ocean Sciences (BIOS) hosted a celebration of the founding of POGO. In the front row are the four people BIOS honored; left to right, they are Shubha Sathyendranath, POGO Executive Director; Howard Roe, former Director, Southampton Ocean Center; Charlie Kennel, former Director, Scripps Institution of Oceanography; and Jesse Ausubel, from Rockefeller University in New York. In the second row is my successor at Scripps, Tony Haymet (left); my apologies for not remembering the gentlemen from the Woods Hole Ocean Institution (center); and Tony Knap, then BIOS Director (right). I am holding an Argo float.**

**Figure 26-5: POGO is now over two decades old. The Scripps Institution of Oceanography hosted the first meeting of the Partnership for the Observation of the Global Ocean (POGO) on Dec 1-3, 1999, and hosted POGO-19 on Jan 23-25, 2018. Shown here is the view taken of the Scripps Director's Office building with POGO-19 participants in front, the Scripps pier in back, and the Pacific Ocean stretching to the horizon.**

# 27

# Scripps and Climate Change

My first presentation to the Scripps faculty in October 1997 began with a cautious quote from the 1996 Assessment of Climate Change of the Intergovernmental Panel on Climate Change (IPCC):

> *The balance of evidence suggests a discernible human influence on climate.*

I had been warned many times that talking about climate change alienated donors, puzzled friends, disconcerted faculty, and provoked politicians, but I hoped the Scripps faculty would accept climate change research as an institutional commitment because of the pioneering work of Roger Revelle and Charles David Keeling. I did not understand then as I do now that climate change is not your usual academic subject where you measure success by the growth of knowledge in books; it is not a subject whose peer group is defined by common educational backgrounds and research experiences (like oceanography and astronomy); its goals are not exclusively set by people with common motivations; it is a broad social commitment, like healthcare, that enlists a wide range of academic specialties in organizing the knowledge it needs to take action. Responsibility for both knowledge and action creates issues that few of the basic sciences encounter. Which is most important: understanding basic causes, characterizing what needs doing, or organizing the doing of it?

Scripps prides itself on its commitment to basic science. There are many SIO faculty who take visceral delight in knowing odd things, believing their arcane expertise might someday be valuable. This group, seeing the widespread participation of non-scientists in setting climate priorities, feared that my priorities would bias academic resource decisions. Some SIO faculty may have feared that I secretly wanted them to drop everything and switch to climate, or, given my NASA background, to abandon ships in favor of spacecraft. How could they have known that I love to see creativity flourish whatever its form, and that those who prize their prickly independence need only tolerate those in their midst doing climate work? It is SIO's responsibility (and not theirs personally) to integrate the knowledge in the many fields needed to deal with climate change.

Walter Munk spoke frequently of two SIO directors, Harald Sverdrup, Walter's thesis advisor, and Roger Revelle, Walter's closest friend and my greatest predecessor. Munk's admiration for Revelle bordered on hero worship largely, I think, because Revelle worked so differently than other scientists. Roger conceived of things beyond science's reach, like founding UCSD. I met Revelle only once,[90] but whether because of Walter or UCSD iconology, Roger Revelle was an animating presence through my whole time as Director even though he died in 1991, six years before I came to Scripps. A local journalist, Neil Morgan, and his wife, Judith, wrote a hagiography of Revelle;[91] though I read it I did not have to, because Neil and Judith would take me aside at Scripps events to tell me another thing I needed to know about Roger.

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The modern phase of global warming research started with Roger's 1957 paper with Hans Suess, but before that, in 1896, the Swedish chemist and 1903 Nobel laureate, Svante Arrhenius, had proposed that greenhouse warming driven by $CO_2$ emissions from

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[90] I was introduced to Revelle at a party at Harvard Law School before I came to California; Debby Bochner's parents knew the Dean of the Law School. I made some callow remark and Revelle turned away disdainfully. I hope no one will portray this as a fateful meeting.

[91] Judith Morgan and Neil Morgan, *Roger: A Biography of Roger Revelle* (La Jolla, CA: Scripps Institution of Oceanography, 1996).

industrial fossil fuel burning could increase atmospheric temperature. For the next 60 years, it was believed that the oceans would absorb nearly all the $CO_2$ industrial society could produce and that not enough $CO_2$ would remain in the atmosphere to create greenhouse warming large enough to worry about. That belief was based on laboratory measurements of the diffusivity of $CO_2$ in distilled water. Roger worried that the complex chemical processes in salty seawater could buffer the $CO_2$ absorption rate, so he and Suess set out to measure $CO_2$'s absorptivity in seawater. From their results they calculated that a molecule of $CO_2$ would remain in the atmosphere twenty years after leaving the smokestack before it diffuses into the ocean. This meant that industrial-origin $CO_2$ should be accumulating in the atmosphere, and anthropogenic greenhouse warming should be occurring. Revelle seems to have grasped immediately the profound implications of his work with Suess; their paper concluded with a remark reproduced many times over the decades:

> Human beings are now carrying out a large-scale geophysical experiment of a kind that could not have happened in the past nor be reproduced in the future.[92]

Roger's next question was whether the $CO_2$ concentration in the atmosphere is increasing at a rate consistent with observed industrial $CO_2$ emissions and $CO_2$'s atmospheric lifetime. To go after this question, he recruited to Scripps one of the few in the world who knew how to make to make measurements accurate enough to answer it, the young Charles David Keeling. The data that Keeling was to collect over the next fifty years would become the single fixed point in the conflicted debate about greenhouse warming.

Dave Keeling made the implications of the long lifetime of atmospheric carbon dioxide come alive to me. By the time I met him, the best estimate of its lifetime had increased from twenty to one hundred years; $CO_2$ was an even more tenacious adversary than Suess and Revelle originally thought. It survives most of the things that happen to other atmospheric constituents; it does not rain out in

[92] R. Revelle and H. E. Suess, "Carbon Dioxide Exchange Between Atmosphere and Ocean and the Question of an Increase of Atmospheric $CO_2$ During the Past Decades," *Tellus* 9, no. 1 (1957): 18-27.

storms; it has enough time for the winds to carry it around the earth many times, so it should be evenly distributed. This last property meant one place should be as good as any other to measure $CO_2$ concentrations, which made it practicable for one person at one location—Dave—to make the measurement, provided he was willing to devote a lifetime to the task. It also means that no matter where $CO_2$ is emitted, it creates the same climate change risk for everyone on earth. Not only that, but if you should make the effort to reduce your own $CO_2$ emissions, society will not realize clear benefits until after you are dead. Since it is a global problem, you cannot attack it on your own, and your contemporaries must be persuaded to make an expensive effort without expecting to enjoy the benefits either. They would have to take you on faith, and all you can say is trust science and the data.

Today, we have a clearer idea of how Roger's one-time experiment is turning out. It is not a pretty picture. In the millennium year, 2000, Veerabhadran Ramanathan's INDOEX collaborator Paul Crutzen, with biologist Eugene Stoermer, captured the totality of humanity's impact on the planet in a single word: *Anthropocene.*[93] By this they meant that human activities have already altered the planet's land surface, atmosphere, and oceans so completely that future scientists will label our time a new geological era. The time we live in is an accelerated version of other cataclysms in earth history when mass species extinctions accompanied the switch between geological eras. The most striking of these earlier extinctions occurred when an asteroid about nine miles across hit Mexico's Yucatan Peninsula and wiped out the then dominant dinosaurs as well as a large fraction of all the other living species, though tiny burrowing mammals found a way to survive the thousands of years of climatic disruptions that followed.

I believe Roger's grasp of the implications of climate change was one of the things that drove him to create a full-service campus of the University of California on the bluffs above Scripps in La Jolla. Very few people thought like Crutzen and Stoermer when Revelle and Suess wrote their paper. Six and a half decades after Revelle and Suess, and two decades and a third after Crutzen and Stoermer, it is

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93 P. J. Crutzen and E. G. Stoermer, "The Anthropocene," *Global Change Newsletter* 41 (2000): 17-18; P. J. Crutzen "The Anthropocene," in *Earth System Science in the Anthropocene*, eds. E. Ehlers and T. Kraft (Berlin: Springer, 2006), 13-18.

not considered unreasonable to argue that coping with climate change will, like medicine, demand university-wide knowledge resources. Both my universities, the Universities of California and Cambridge, have created comprehensive climate initiatives, as have for example, Columbia, Arizona State, and Boston University in the US, and many other academic institutions elsewhere in the world. Revelle saw that this would have to happen, and he wanted it to happen in San Diego first; he told his faculty that SIO needed a full-service university nearby to realize the full value of its research to society. Much as the beauty of a jewel is enhanced by its setting, SIO would be seen as the jewel it is if a university were nearby. Woods Hole's main limitation, Revelle argued, was that Harvard and MIT were fifty miles away.

SIO was going to incubate a new university up the hill in La Jolla if Revelle had anything to do with it. Roger did not have what Abram Sachar, President of Brandeis University and father of my best high school friend, David Sachar, called an 'edifice complex.' For Roger, it was "build the faculty, and students, buildings and funding will follow." The extraordinary beauty of the La Jolla site and Roger's vision of what could be done on it were enough to recruit the physicists, Nobel laureate Maria Goeppert Meyer and her husband Joseph, as well as the distinguished chemist James Arnold, even before there was a University of California campus to put them on. Roger made way for them on SIO's campus.

**Figure 27-1: Roger Revelle (1909-91), Director of Scripps, 1950-64. "...neither politicians nor scientists command much respect in society, and yet between them they hold in their hands the future of mankind..." I introduced talks about Scripps with the thought that all the scientific heroes of my youth are safely embalmed in the history books except Roger. He was asking life and death questions that we still don't know how to answer today.**

The UC Regents and Clark Kerr, then President of the University of California system, on the other hand, did have an edifice complex; they selected not Revelle but the nuclear physicist Herb York, the extraordinarily young first Director of the Lawrence Livermore National Laboratory, to be UCSD's first Chancellor. Herb's prodigious skill in managing Livermore, an institution as large as a typical UC campus, was among his assets. Thus, a Cold War leader, Herb York, was chosen to build UCSD, whereas the climate warrior, Roger Revelle, left Scripps for Harvard, where the future US presidential candidate Al Gore was to be his student. My mother used to warn that a prophet is not recognized in his own home.

The relationships between SIO and UCSD ("the upper campus") remained tense as the campus that Revelle conceived (and SIO sacrificed to create) grew to ten times SIO's size. In its urgency to create world-leading programs of its own, UCSD paid lip service to SIO; SIO's reputation was already where UCSD's new departments wanted theirs to be. Nonetheless, physical monuments to Roger's vision could be found at SIO in the ship and building named for him. Revelle also left behind his influence on the way that Scripps faculty thought collectively despite themselves. Among other things, much of SIO had come down with Roger's case of multiple discipline disorder. Revelle, when asked whether everything done at Scripps is truly oceanography, is said to have replied, "Oceanography is what we do at Scripps." The question may have had hostile intent since the mandate creep it implies threatens to dilute budgets and change disciplinary balance, but Revelle's arrogant response explains how SIO evolved into a comprehensive earth system science institution: by hiring in areas that SIO's research led it to and by valuing talent over orderly organization.

Towards the end of my years as Director, the Master of Revelle College on the upper campus asked me to give his undergraduates some idea of who Roger had been. In my talk, I said that Roger had been counted among the best scientists of his day, like the genius-heroes of quantum electrodynamics I revered at Princeton (Feynman, Schwinger, Dirac, Dyson). Roger did not see himself that way; he said his main accomplishment was helping smart people do important things. Roger's self-appraisal grossly underestimated his intellectual capacities, I said. The accomplishments of the quantum pioneers are safely interred in history books while we still are asking questions Roger raised through his scientific work at SIO and policy work at Harvard. We look upon Roger's physics contemporaries as we do portraits of America's founding fathers, inert reminders of a great heritage in period clothing. Roger is all around us at UCSD, an ineffable, animating presence, groping along with us for the key to sustaining the human experiment on earth.

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If anyone asks me today what one thing convinced me to give up a bigger title and larger salary to go to Scripps, it was the Keeling Curve.

Dave Keeling had already been at Scripps for forty years when I arrived. As Director, I got used to visits from him. His issue was the same each time: his research funding was under threat. Dave did me the profound courtesy of assuming I would respond to a scientific rather than financial argument. He never used manipulative language and always explained in depth the significance of his results before we got to his immediate problem. Most scientific grants have three-year terms, and usually that is enough time to produce a new result. Government contract monitors are judged by what they support; adding three years to Dave's by then four-decade record of $CO_2$ accumulation in the atmosphere was low on their novelty list. They may also have been turned off by what stimulated me: Dave Keeling's stubborn insistence on doing measurements his way. Seemingly slight changes in procedure threatened data continuity, and the alternate approaches his contract monitors yearned to fund he believed unreliable. In short, Dave Keeling looked stubborn, a trait unappealing to people of power. In fact, he *was* stubborn. I could not provide him substantial advice, but I think Dave wanted to know the Scripps Director had his back, as did Roger Revelle, Bill Nierenberg, and Ed Frieman before me. Dave routinely escaped funding disaster on his own. It seemed a miracle each time.

When Dave Keeling was awarded the National Medal of Science, I felt that he and his family deserved a celebration in Washington as well as at SIO. I used my own funds to arrange one at the Cosmos Club, uncertain whether university funds could be used for an event at a private club. Shortly after, Dave died unexpectedly at his beloved mountain home in Montana, and it fell to me to preside over his memorial service at Scripps. I said there that on only two other occasions in the history of Western science had data altered the course of history. The first was Tycho Brahe's observational data on the planets' orbits and the subsequent use of these data by Johannes

Kepler and Isaac Newton to create the laws of motion and gravity. The other occasion was when Albert A. Michelson measured the speed of light; his data made clear that Einstein's theory of relativity, however counterintuitive, had to be correct. Einstein's theory made nuclear weapons conceivable, which ended World War II and started the Cold War.

Dave Keeling's data may prove even more transformational. The time series of $CO_2$ concentration in the atmosphere started by Dave, continued by his son Ralph, and now reinforced by other programs, remains the one fixed point in the highly conflicted debate about climate change. Half a century from now, society will have undergone a global political, ethical, economic, and technological transformation because of Dave Keeling's stubbornness.

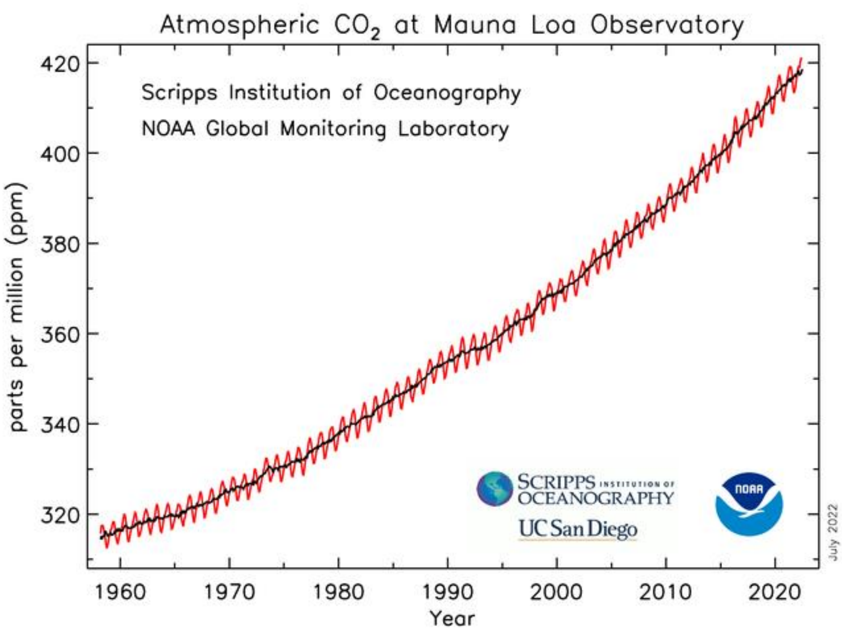


**Figure 27-2: The most consequential data set of the twentieth century. Shown here are the $CO_2$ data acquired by Scripps Oceanography's Charles David Keeling, his son Ralph, and the National Oceanic and Atmospheric Administration (NOAA) at Hawaii's Mauna Loa Observatory between 1957 and 2021. The fact that the Keeling Curve bends upward indicates that global $CO_2$ emissions are increasing with time; the ambition of all advocates of climate action is to see the Keeling's Curve bend downward. Ramanathan called this "Bending the Curve."**

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Many mathematicians of my acquaintance quantify their hero worship; they will tell you that so-and-so is the fifth geometer in the European Union, or some such thing. This method satisfies the desire for order but fails when talent extends into dimensions of human activity where quantification does injustice. So it is with my Scripps colleague, Veerabhadran Ramanathan (you can call him Ram; atmospheric scientists, the President of the University of California, the Governor of California, the President of India, the Director of the UN Development Program, the Dalai Lama, the Pope, and the local minister all do). Ram chose to widen the mainstream of climate science rather than go with the flow in the middle of the channel. His first research paper made him an international leader among climate scientists, but, like Picasso, his capacities and repertoire evolved decade by decade. His mid-career research on short-lived climate pollutants (SLCP's) led directly to policies implemented by the US government and the United Nations. Lately he has achieved a synthesis of thought and action that blends science, human development, and religion.

In 1975, Ram showed that the chlorofluorocarbon compounds (CFCs) responsible for the ozone hole are super greenhouse gases, tens of thousands of times more potent per molecule than $CO_2$. CFCs were in common use at the time as refrigerants. The Montreal Protocol on Substances that Deplete the Ozone Layer, which went into force on January 1, 1989, has led to a notable reduction in greenhouse warming even though that was not the goal of the protocol. Ram followed up his CFC work by showing that atmospheric methane, nitrous oxide, and tropospheric ozone, all of which are produced in air pollution, are individually powerful, if less abundant greenhouse gases than $CO_2$, which was getting the lion's share of policy attention. Ram led the international assessment that showed that non-$CO_2$ greenhouse gases collectively comprise about forty percent of the atmospheric warming caused by humans.

You could not assume that the greenhouse gases in air pollution remain in the atmosphere long enough to be uniformly mixed as did Keeling for $CO_2$; you have to know where they go once they leave the smokestack before you can figure out how large a global impact they have. When I was at NASA, my colleagues reported frequently on Ram's Central Pacific Experiment (CEPEX), the purpose of which was to find the least polluted spot on earth. CEPEX found no place where air pollution was undetectable. Pollution from China was blown west to east by prevailing winds, and roughly fifty percent of the air pollution above one kilometer altitude over Los Angeles could be traced back to Chinese cities. What I found interesting was that the pollution from a given city would remain in an identifiable layer across the Pacific, and that each city's layer has a distinctive chemical signature. This meant to me that intelligence agencies could use airplanes standing far off the Chinese coast to determine which city was making the pollution affecting Los Angeles.

When I became Scripps Director, Ramanathan and Paul Crutzen were directing the Indian Ocean Experiment (INDOEX), a CEPEX on steroids. INDOEX, a *tour de force* of research management, integrated the work of hundreds of scientists using field stations, oceanographic ships, research airplanes, and weather satellites. The most visible component of air pollution, black carbon haze, has been reduced in the industrial world but still afflicts the developing world. Black carbon particles in the lower atmosphere absorb sunlight and are a significant potential warming agent. INDOEX established that black carbon generated by open cooking fires in India affects climate on a continental scale. What Ram called the "Asian Brown Cloud" was a vast cloud of black carbon pollution spreading south from the foothills of the Himalayas to the equator.

INDOEX's technical achievements pale before its geopolitical implications. The international balance of responsibility for climate change shifted: no longer could it be said that the developed world is solely responsible. I recall a telephone conversation with the former US Secretary of State George Schultz, one of the architects of the Montreal Protocol on limiting chlorofluorocarbon emissions. I described how much of the world's warming is caused by non-$CO_2$ greenhouse gases and black carbon pollution. This changes everything, Schultz said; developing nations are no longer solely passive sufferers from the $CO_2$ sins of the advanced world, they have ethical responsibilities of their own. The developing world would become a full partner in the 2015 Paris Climate Accord, whereas it was absolved of responsibility in the 1997 Kyoto Protocol that George Schultz had negotiated.

Ramanathan's scientific successes enlisted leadership, administrative, and political talents that originate in an ethical dimension of his character. The welfare of the poor who are the most vulnerable to pollution and climate change is his central preoccupation, one that moved him from research to action on black carbon. A non-profit company he and his children created, *Nexleaf*, is providing clean-cook stoves to the poor in rural India, protecting their health while demonstrating reduction of black carbon emissions in realistic social circumstances. Ram sees the developing and developed worlds as bound together by the climate crisis. While many have said developing nations will suffer more from climate change, this older way of thinking exacerbates the international division that some said doomed the Kyoto Protocol. Ram has found a way to communicate with both the developed and developing worlds. There are vulnerable poor at risk even in the richest countries, and Ram finds it more persuasive ethically to focus on all poor people—the "bottom three billion" wherever they are. A surprising number of them are in America.

Ramanathan's humanitarian impulse found powerful expression in a forum co-sponsored by Rome's Pontifical Academies of Science and Social Science. With the Cambridge economist, Sir Partha Dasgupta, Ram co-led one of the most consequential academic conferences of recent times, *Sustainable Nature, Sustainable Humanity: Our Responsibility*. Artfully designed, it linked the ethical, social, and ecological components of the world's sustainability crisis. Its wide-ranging analyses informed the scientific framework of Pope Francis' encyclical *Laudato Si',* which helped create the atmosphere of acceptance for the 2015 Paris Agreement, the high-water mark of international cooperation on climate change.

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Even an advanced technological state like California had only begun to worry about the reduction in freshwater availability expected from the reduction of Sierra Nevada snow cover caused by global warming. Anyone who has seen the 1974 film *Chinatown* will know that the settlement of America's most populous state depended on the delivery of fresh water from the Sierra Nevada to its farms and communities, especially in the parched southern half of the state. Today about seventy percent of the water for Southern California's twenty-three million citizens comes from the spring runoff of the winter snows in the Sierra Nevada in the northern part of the state. *Chinatown* told of the vast water project at the turn of the twentieth century that, by bringing water south to the San Fernando Valley, triggered the enormous population growth of the Los Angeles Basin. The life work of California's iconic historian, Kevin Starr, was centered on the role water has played in the politics and economics of California. I often introduced public talks on climate change with

Starr's thought that that the history of California is written in water, not ink.

In 2002, the California Energy Commission set aside funds for research on the regional impacts of climate change, the first US state to do so. When I was a member of the California Council on Science and Technology, I had advocated funding the program, and I made sure Scripps researchers took advantage of the opportunity. Dan Cayan, Mike Dettinger, and their students and postdocs took on the Sierra Nevada snowmelt problem. Within two years, they had estimated that ninety percent of its spring snow cover would be gone by the end of the twenty-first century. This striking result got front page and full inner-page coverage in the Los Angeles Times, and I soon found myself in the state capital, Sacramento, explaining it to legislators. I was asked repeatedly, aren't climate change models uncertain? Yes, I replied, the models are uncertain, but their directions of change are consistent, and by the time the State is ready to do something the models will have improved.

I underestimated the speed with which the California State Legislature and Governor Arnold Schwarzenegger would act. California passed Assembly Bill 32 in 2006 which committed the State to reduce greenhouse gas emissions, the first time any US state had done so. California was ahead of the US federal government and other national governments. (The UK passed organic legislation in 2008.) The first article of the preamble to Assembly Bill 32 stated why the Legislature stuck its neck out: the loss of the Sierra Nevada snows predicted by Scripps.

The State also committed to ongoing periodic assessments of the impacts of climate change on California's natural resources, agriculture, and industries. Over the years, a library of research on climate change impacts on California's environment, agriculture, industry, and commerce has built up from these assessments. Californians had thought that their wineries could have problems from pests, wildfires, and drought, but never imagined they would be made worse out by climate change. Scripps' wonderfully low-key Dan Cayan became the scientific godfather of California's climate change assessments over the next fifteen years. The County of San Diego soon undertook even more specific planning in light of the statewide climate change assessments, and Dan and I, along with others from Scripps, were brought in as advisors to San Diego County's planning team. Scripps was walking the talk: we were helping turn scientific knowledge into action.

After I stepped down as Director in 2006, I began to make winter term visits to the University of Cambridge and soon found myself in discussions with Dick Fenner, Professor of Engineering Sustainability, and his student Grant Kopec, who had chosen California water issues as part of his dissertation topic. As with automobile air pollution, California was showing leadership in environmental issues.

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Most academic leaders would jump at the opportunity to recruit a Nobel laureate but recruiting Mario Molina was not to be an unalloyed success for me. The presence of Ramanathan, plus San Diego's proximity to Mexico, attracted Paul Crutzen's Nobel co-laureate Mario Molina to UCSD from MIT. When Ram informed me of Mario's interest in UCSD, I called my Chancellor, Bob Dynes, to ask whether he would like to recruit a Nobel laureate, but I did so before I consulted the SIO faculty—never a good political move.

Mario ended up with a joint appointment between SIO and the UCSD Department of Chemistry. Mario, Mexico's only Nobel laureate, also headed a major institution dealing with air pollution in Mexico City. The faculty commentary preceding Molina's appointment was how I sensed unease with his appointment. While faculty found it difficult to oppose the appointment of a Nobel laureate, the fact was that a precious faculty resource would go to someone who spent much of his time away from SIO. Some faculty wished we could recruit someone younger. There is no doubt in my mind that the world benefitted from UCSD's appointment of Mario Molina, but the primary beneficiary may have been Mexico City, not SIO.

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*El Niño made me do it.* That was the title of a ditty composed and sung by the then Director of the Birch Aquarium at Scripps, Ned Smith, who had quite a good singing voice. Ned's ditty had its moments of popularity at Aquarium events. Nick Graham of Scripps had predicted a major *El Niño* event for 1998, and the weathercasts on local television were full of warnings of beach erosion, storm surges, downed telephone lines, and power outages. The San Diego Gas and Electric Company, whose president was on a Scripps advisory board, took the warnings seriously enough that they rescheduled maintenance projects from inland to the coast in anticipation of storm damage. SIO predicted that *El Niño* storms would occur in the winter months; December came, January came, no *El Niño*. People remembered the damage wrought by *El Niño* in 1982. The local newspapers showed photographs of a sandbagged SIO Director's Office and taunted us: *when is the Scripps El Niño going to arrive?* The Scripps media relations office took to repeating that the most likely month was February, just be patient.

When it came, that *El Niño* was the strongest ever recorded by modern instrumentation. That made a definite impression on me: twenty-two years later, Elena Yulaeva and I would work out reasons why the 1998 *El Niño* signaled a structural change in Pacific trade wind dynamics and ocean climate.[94]

At the time, numerical greenhouse warming models predicted specific climate events even more poorly than they do now, so the idea of forecasting specific *El Niño* events was highly provocative, especially to my predecessor, Ed Frieman. Mark Cane of Columbia's Lamont-Doherty Earth Observatory had developed the basic theoretical model of *El Niño* in the 1980s.[95] Given satellite measurements of the ocean surface temperature in the western Pacific, Cane's model predicts when an *El Niño* event should occur on the coasts of Peru or California. SIO's Nick Graham had the only operational model applicable to the coming 1998 event, and both Columbia and SIO depended on Nick for their press releases. Frieman, seeing all this, had proposed to create a joint institute to forecast *El Niño* events and study their impacts. I inherited Frieman's negotiations with Columbia and flew several times to New York to discuss the joint institute with Michael Crow—the same Michael Crow who tried to hire me two years earlier to Columbia's Earth Institute. The 1998 *El Niño* occurred before he and I could reach agreement. Columbia decided to go it alone and pirated Nick Graham away, which I found treacherous, conveniently forgetting that University of California campuses raid each other all the time.

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[94] C. F. Kennel and E. Yulaeva "Influence of Arctic Sea-Ice Variability on Pacific Trade Winds," *Proceedings of the National Academy of Sciences* 117, no. 6 (2020): 2824-34.

[95] M. A. Cane "El Niño," *Annual Review of Earth and Planetary Sciences* 14, no. 1 (1986): 43-70.

Landing SIO's first satellite project was disillusioning. While bringing UCSD its first spacecraft project might have been a career plus in 1998, it looked like a political liability to a risk-averse chancellor, Marye Anne Fox, in 2006.

During a telephone call with my old boss at NASA, Dan Goldin, I learned that NASA was contemplating a small earth observation spacecraft that would have a new and different vantage point from which to make climate measurements. There is a neutral gravity point between the earth and sun where, with powered orbital corrections, a spacecraft can remain on the earth-sun line as the earth orbits the sun. Instruments on the sun-facing side could stare full time at the sun and collect long sequences of data about the solar wind, while those on the earth-facing side would look continuously at the sunlit face of the earth. A spacecraft in such an L1 Lagrangian orbit could do a better job of measuring the earth's radiation balance than the Earth Radiation Budget Satellites that Ramanathan had worked with in the 1980s. By calculating the imbalance between the flux of incoming radiant energy from the spacecraft's sun-facing side and the outgoing energy flux of infrared radiation from the earth-facing side, you could measure the rate at which greenhouse warming was adding energy to the earth's climate system. In other words, you could measure the rate of global warming, which in 1998 people disputed even existed. I saw this to be as fundamental a measurement as one can make, on the order of Dave Keeling's measurements of atmospheric carbon dioxide concentration.

I did not yet know all the researchers at Scripps, so I inquired whether anyone would be interested in proposing a satellite to NASA. Francisco Valero volunteered. Valero and the team he assembled was ultimately awarded a NASA grant to build the spacecraft, which was named *Triana* after the first of Christopher Columbus' crew to sight the New World. *Triana,* later renamed *DSCOVR*, was to blight the next 16 years of Francisco's career, only to reward his efforts after retirement. The problem was that Vice President Al Gore took an interest in the project. A camera on the earth-facing side of *Triana* could beam a TV image of the rotating earth to every schoolroom in the country, or in the world for that matter. This politicized everything. I have only hearsay about what happened the first time the project came up for congressional markup. Tom Delay, the "hammer," the Republican majority leader of the House of Representatives, burst into a routine committee meeting to ask whether it was going approve “Gore-Sat.” What looked like a smooth approval was tabled in favor of a National Academy study of *Triana’s* science return, given that it had entered only recently into NASA’s planning queue. The San Diego congressional delegation helped save *Triana* for another day by engineering the compromise. The Academy report opined that the quality of its science was consistent with that of other projects in NASA’s planning queue.

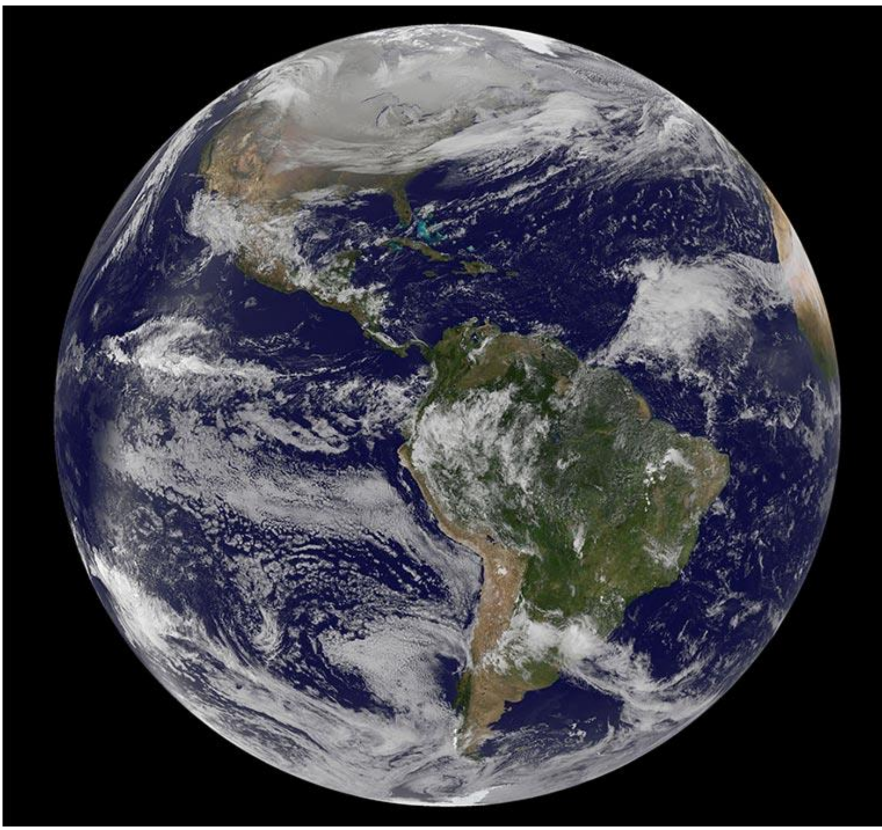

**Figure 27-3: The View of the Americas from DSCOVR (aka “Gore-Sat”). What would have ordinarily been a plus to a Director’s record of achievement turned out to be a liability—at first. It was a life satisfaction to me to see Francisco Valero’s dedication and hard work vindicated without disguising the contributions his mission would make to climate science.**

Ultimately NASA, under continuing political pressure, decided to put an almost completed spacecraft in mothballs at Goddard Space Flight Center, where it remained until 2014. Then NASA’s partner agency, the National Oceanic and Atmospheric Administration, needed to replace its spacecraft, *ACE*, that it relied on to measure solar wind for space weather applications. It would be less expensive to refurbish *Triana* than to build a new spacecraft, and the project went forward with a new name, *DSCOVR* (Deep Space Climate Observatory) that does not disguise its purpose. I heard a rumor that some members of Congress wanted to disable *DSCOVR*'s climate instruments but keep its solar wind instruments, but if so, nothing came of that idea. This illustrates how deep the hostility to climate change was in some quarters.

Valero recently wrote me that *DSCOVR* had had a third special session at a meeting of the American Geophysical Union devoted to its results. Francisco and I often speak of how much better our professional lives would have been had politics not delayed *Triana/DSCOVR*. Even fifteen years late, it was giving good results. Francisco's story epitomizes an important moment in time when publicized denial of climate change could paralyze scientific initiative. Many people, including me, have urged Francisco to write a book about the modern *Triana*'s adventures and misadventures.

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Early in my term as Director, Naomi Oreskes, then recently appointed to the UCSD Department of History, asked if Scripps might make a research office available to her. She wanted to work on the history of oceanography in the SIO archives, the best in American oceanography outside the Library of Congress. Her PhD thesis had been on the history of plate tectonics, and it was evident she could have become as accomplished in earth science as she became in history of science. Our first conversation told me it could be enriching to have someone like her around, and I found a small office in a corner of the administrator's complex.

I did not foresee how important Naomi Oreskes' impact on the climate change enterprise would be. While all historians write history, Naomi *made* history. Her 2004 paper was a landmark.[96] The mainstream media, used to reporting on a politically divided country, customarily presented an individual scientist's opposed view on climate change along with that, say, of the Intergovernmental Panel on Climate Change. Journalistic ethics require balance, but the public

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[96] N. Oreskes "The Scientific Consensus on Climate Change," *Science* 306, no. 5702 (2004): 1686.

perceived that climate science was controversial *within the science community itself*. After Naomi's paper, which documented a strong consensus within the community of active climate scientists, the mainstream media endeavored to indicate the strength of the scientific consensus when presenting opposed views. The sober conclusions of the Intergovernmental Panel on Climate Change were less often paired with showy outlier opinions. The mainstream media had changed how it applies one of its ethical standards.

Naomi Oreskes' later book with Erik Conway, *Merchants of Doubt,* brought home to me the implications of denialism for science. She and Erik had included my predecessor once removed, Bill Nierenberg, among the prominent physicists who became climate change skeptics, if not outright denialists.[97] Bill, SIO's longest serving Director (1965-1986), had been extremely kind to me. He invited me to lunches about once a month where he would give me "director lessons." He would read something I had said and ask whether I had checked my facts. Though an NAS study he chaired made an important contribution, his physicist's style of questioning made him appear hostile to climate change science; indeed, Naomi Oreskes counted him among the denialists. In truth, the science of climate change is a half-theoretical, half-empirical enterprise dependent on under-sampled data and never adequate computations. It could not achieve the rigor of the physics Nierenberg and I had grown up with, but the world could not do without it. Climate change science was being asked to come up with actionable results before Bill could be comfortable.

As Director, Nierenberg had frequently made himself available to faculty and students in the rose garden in front of the Director's Office. He tended his rose garden faithfully, but it had fallen into decline since neither Frieman nor I were into gardening. I figured that UCSD ground crews would pay more attention if the garden were named. Without going through a formal naming process, I had a wooden sign made that declared it the *William A. Nierenberg Rose Garden* and invited his local friends to an informal dedication ceremony. It was probably the last time Bill saw many of them because he died two months later.

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[97] Bill Nierenberg's children, who founded Scripps' most important lecture series, the Nierenberg lecture, deeply resented Naomi's characterization of him in her book, and my defense of her work.

The Nierenberg family has endowed SIO's most important prize, which is awarded for science in the public interest. I was privileged to preside over the award of the first Nierenberg Prize to the great ecologist, E. O. Wilson, in 2001. The prize's lengthening list of laureates is becoming a pantheon of twenty-first-century science. Of the eighteen winners up to 2021, only two have been in physics and astrophysics.


**Figure 27-4: Bill Nierenberg's rose garden with a view of the one-story Scripps Director's office in the background.**

# 28

# Marine Biology and Biodiversity at Scripps

The Scripps Institution of Oceanography started life in 1903 as the Marine Biological Association of San Diego. Its first Director, William Emerson Ritter (1912-1923), was a UC Berkeley professor who made summer visits with his students to study the biology of the La Jolla Cove. The newspaper magnate Edward Willis (E. W.) Scripps adopted Ritter's project, funded the Marine Biological Association, and in 1910 gave 170 acres of La Jolla shorefront land, an oceangoing sailing vessel, and an endowment sufficient to create a research station of the University of California. T. Wayland Vaughn, SIO's second Director (1923-1936), broadened its research scope to oceanography, but marine biology remained the dominant presence in the institution. Walter Munk's thesis advisor, Norwegian Harald Sverdrup (Director 1936-1948), an expert in chemical and physical oceanography as well as marine biology, continued the broader oceanography theme. The change to "big science" physical oceanography occurred under Roger Revelle (Director 1951-1964), who secured Navy funding for ocean-going ships and SIO's legendary Pacific Ocean cruises. Walter told me that Revelle had to endure a vote of no confidence, instigated by restive biologists who were alarmed by what they saw as Revelle's headlong rush into physical oceanography. By the time I arrived in 1997 SIO's biologists were finally feeling comfortable, though if I listened carefully, I could hear grumbles stemming from the submerged identity conflict.

I heard other voices, too. Information technology and molecular biology had made huge strides since I was in graduate school, and biology was now in a real competition with physics to be the queen of the basic sciences. I had been present at a White House ceremony where US President Clinton and UK Prime Minister Blair congratulated Craig Venter and Francis Collins for leading the efforts that decoded the human genome. Even though SIO biology felt itself to be doing well, I worried that if it did not master modern genomics, an information science, it would fall behind just like the physics departments of the 1920s and 1930s that did not embrace quantum mechanics. As one might expect, Scripps' biologists had a natural inclination to biological oceanography, which is concerned with the interactions of marine populations among one another and with their chemical and physical environments. Maybe it was time to nudge the balance of SIO's interests towards the new discoveries in fundamental biology. The challenge was to persuade a research group comfortable at the top of its game that the game is about to change.

After my appointment was announced, there was a six-month period when I held both the UCLA Executive Vice Chancellor and SIO Director jobs and commuted weekly between Los Angeles and La Jolla. When I finally settled in full time at Scripps in March 1998, I asked SIO biologist Peter Franks to organize a weekly lunchtime personal seminar to educate this physicist about what SIO's biologists were doing. I no longer had youth's advantage of learning when the human brain is consolidating the pathways of adult thinking, but I felt I had to make the effort: forty percent of an institution I had committed to lead was in biology, a subject in which I had not had even a beginning course. Ellen, always curious, attended these lunch seminars, which facilitated personal relationships with people I would not have occasions to socialize with at professional meetings.

Sometime during the bi-campus period, I received a call at UCLA from the Nobel laureate biologist David Baltimore, who had just become Caltech's President. With David's new job came Caltech's responsibility for NASA's Jet Propulsion Laboratory (JPL), whose Director, Ed Stone, had reported to me when I was at NASA headquarters. David, noting that JPL's budget dwarfed Caltech's, said he knew perilously little about space science and how NASA operated under Dan Goldin; would I come to Caltech for lunch and give him a brief tutorial? I replied that I had just assumed responsibility for an institution at which nearly half of the work was in biology, and that I would accept his invitation only if we could spend half our lunch on space and the other half on biology. It became evident during my half of the lunch that Baltimore had a good picture of what SIO did in biology (already a promising indication). He filled me in on the genomics revolution as asked.

I had been full time Director for perhaps three months when the legendary biologist Sydney Brenner, who came from Cambridge University to La Jolla and was now at the Salk Institute, walked unannounced into my SIO office. Sydney had a provocative message: Did I know SIO was about to make its greatest error of omission in decades? Did I know that the Pre-Cambrian origin of multi-cellular life in the oceans was about to be unraveled and SIO might miss the opportunity to be in at the beginning? Sydney urged me to build SIO's strength in modern biology and offered to help.

A few days later I got the same message from biologist Rita Colwell, then Director of the US National Science Foundation. Rita was returning to Washington from Japan via California, and she wished to take a personal day with SIO's marine biologists. She delivered the Colwell version of Brenner's message to me as I drove her to campus to meet with our biologists. I sensed that after her visit that they wanted more, so I also arranged for another eminent biologist, Eric Davidson from Caltech, to spend a day at SIO, give the faculty a seminar, and me advice. I remember asking Eric what faculty recruitments in his field cost at Caltech; he said about a million dollars each, mostly for lab setup.

**Figure 28-1: Craig Venter as I knew him, with his beloved boat in the background, overlooking some ocean or another.**

The co-decoder of the human genome, Craig Venter, also stopped by to describe his plans for an expedition in his yacht to collect microbial samples for genomic analysis. SIO's Farooq Azam had taught in my personal seminar that the marine microbiome, the million or so microbial species per cubic centimeter in the sunlit upper ocean, is the base of the marine food chain. The food chain in turn regulates the ocean's absorption of carbon dioxide from the atmosphere, and, thus, climate change. Venter's "shotgun" genome sequencing technique was particularly adapted to diagnosing microbiomes and could be important to climate change research.

I had a couple of questions ready for Venter to demonstrate serious interest in his line of work. One was why minority species in the ocean microbiome survive. Do minority species enable the microbiome to rapidly adjust both its species distribution and waste product chemistry to the changing environment when ocean currents carry it into new regions?

When Craig published the results from his cruise, he emphasized the discovery of new species as a classical biologist might do, but when I learned from Farooq Azam and Peter Franks that the ocean microbiome's composition differs with each meter of depth and that Craig's data were under-sampled, I concluded that there was a good reason for marine biologists to enter the genomics field and act like oceanographers when they did so.

I asked Craig if I could visit him at Celera Corporation in Rockville, Maryland. I invited one of my personal tutorial mentors, Doug Bartlett, to come with me. When I saw the scale of Craig's decoding and computational efforts, I saw that genomics is as much a problem in bioinformatics as in wet biology. Venter had the taste for big science of the physicists at Princeton.

I tried various ways to connect SIO's biologists to genomics programs that were centered largely on medical applications at the time. I worked with Larry Smarr, the Director of the then new California Institute for Telecommunications and Information Technology (now the Qualcomm Institute), to secure a grant in marine bioinformatics from the Gordon and Betty Moore Foundation.

Following Eric Davidson's advice, I applied the whole of a million-dollar donation from Lou Simpson, Warren Buffett's business partner, to the recruitment of Terry Gaasterland from the Rockefeller Institute. Here, Sydney Brenner again enters the story. Sydney and I were both members of the First Thursday Club, a monthly dining club for La Jolla scientific luminaries created after Francis Crick arrived at the Salk Institute from Cambridge.[98] I came late one evening to one of its Thursday dinners, which by chance was held the evening that Sydney's Nobel Prize was announced. On entering the dining room, I

[98] Later, in Cambridge, I learned that the La Jolla First Thursday Club dinner was modeled on a similar event hosted by the Royal Society of London.

found Sydney surrounded by a claque of congratulating admirers. He interrupted their animated chatter to call across the room as I entered: Charlie, has Terry accepted SIO yet? If she is still hesitant, tell her I will find her a place in my lab at the Salk, too! Terry Gaasterland did come to SIO and has been a fine faculty member.

David Baltimore, Sydney Brenner, Rita Colwell, Eric Davidson, and Craig Venter are heroes of modern biology, much like the heroes of quantum electrodynamics so impressive to me in graduate school. I interpreted their messages to me the same way: SIO needed to catch up with the revolution in genomics. This view underestimated the Scripps biology faculty. An alert faculty like SIO's would have gotten into it on its own, but perhaps a year or two later. However, I did have fun getting to know some of the great figures in biology, and SIO's biology faculty never doubted this astrophysicist's respect for their field. And I learned about a discipline that has a major role to play in addressing the coming world sustainability crisis.

Craig Venter differed with his medically oriented competitors; he believed that the applications of genomics to the environment will be as important as they are to medicine. He eventually severed his relationship with the Celera Corporation because Celera's financiers were more interested in moneymaking opportunities. Craig was a UCSD graduate and San Diego native and might be tempted to return to San Diego. It did not take much to convince Bob Dynes, UCSD's Chancellor, that we ought to persuade Venter to bring part of his operation to UCSD. Venter's response to our overture was that he always had an ambition to return to La Jolla (because Francis Crick had gone there?), but his wife, the distinguished biologist Claire Fraser, loved the East Coast; they kept their boat there (not to mention her laboratory). He turned us down.

I settled for persuading Craig to join SIO's advisory board. He didn't have to attend ceremonial events, but I hoped he would talk to our scientists from time to time. Three or four years later, I got a call from Craig: Charlie, do you remember that impediment to my return to La Jolla? It's gone now. (He and Claire Fraser had divorced). By that time, I had also recruited UCSD's IT visionary, Larry Smarr, to SIO's advisory board. Smarr and Venter both came to one of its meetings; each found in the other a kindred technical soul. Seeing this, I suggested to Bob Dynes that UCSD rev up its recruitment engine again. This time Venter was enthusiastic.

The last stop on the way to formal UC approval of the Venter Institute was an SIO faculty vote. At the faculty meeting before the vote, I reminded our biologists that Craig was thoroughly disliked in some quarters of the La Jolla biology community; would that pose a problem for them? Their reply was that it was *my* job to deal with Craig; all they knew was that he had young people they wanted to work with. That did it for me. I forwarded the positive faculty vote (Craig recently told me he had heard it was unanimous) to Bob Dynes. Dynes got the UC Regents to approve a lease for the Venter Institute on almost the last free parcel of SIO land with a good ocean view. Craig built an unusually environmentally correct building on the road connecting SIO, the Birch Aquarium, and the upper campus. Our site was not as spectacular as that of the Salk Institute, where Francis Crick was, but pretty good.

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My physics background had given me some idea of how to deal with genomics, but I was on unfamiliar ground when Nancy Knowlton and Jeremy Jackson approached me with their proposal to create a Center for Marine Biodiversity and Conservation (CMBC) at Scripps. Nancy and Jeremy were asking me to take a famously hardnosed research organization into alien territory. What Nancy and Jeremy advocated would require judgments about not only the scientific world of ecology, but also the policy world of government regulations and marine protected areas, the advocacy world of nongovernmental organizations, and the glitzy world of public fundraising. Nancy and Jeremy argued that the existing marine advocacy and policy worlds, though professing to use the best available science, were far from making the best use of the available science. A new generation of students, trained in both marine ecological science *and* its policy and public dimensions, would in time address this shortcoming.

Nancy and Jeremy's proposal presented a fundamental organizational identity problem. Conservation science ran counter to Vannevar Bush's paradigm of disinterested basic science; conservation biology was something that non-governmental advocacy organizations did, not academic departments. Many, maybe most, faculty at SIO would see CMBC as a diversion of resources from fundamental research. Many, maybe most, would feel unqualified to make academic promotion judgments about conservation biology. I had doubts about my own judgment given that I had not been educated in biology or acculturated to conservation science and policy.

Nancy and Jeremy skillfully softened me up. Our discussions prepared me to accept the invitation from the Pew Charitable Trusts to be a member of the Pew Oceans Commission. Leon Panetta was to do his customarily deft job as Chair of the Commission; no wonder, Leon had been President Clinton's Chief of Staff and knew how to deal with opinionated people. (Leon would later become President Obama's CIA Director and Secretary of Defense.) I was one of three scientific members of the Pew Oceans Commission, along with Jane Lubchenco and Kathryn Sullivan, both of whom went on to become Administrators of NOAA. The Pew Commission would lend me an aura of *gravitas* that might give my marine conservation initiatives at SIO some credibility. I was a conservation neophyte faced with managing the consequences of major decisions with no reason to feel my judgment secure. Perhaps the Pew Commission could teach me enough to make knowledgeable decisions for SIO. Membership on the Pew Commission was to provide both Ellen and me with great personal pleasure; we bonded with Tony Knowles, the sitting Governor of the State of Alaska, (our favorite travel destination and Ellen's passion) and his wife Susan, who was a Vassar graduate like Ellen.

The Magnuson-Stevens Fishery Conservation and Management Act, which became law in 1976, was scheduled for renewal. The appointment of a national commission to assess the state of US marine fisheries was delayed, and the Pew Trusts, fearing the lack of a formal assessment prior to the congressional debate on renewal of the act, decided to proceed with its own assessment. Eventually the Pew and the later national commission collaborated on their recommendations to Congress.

As background, the UN Food and Agriculture Organization had found, after biased reporting from China was corrected, that there had been a systematic worldwide decline in global fisheries catch for decades, presenting a threat to the world's food security. The Pew Oceans Commission's job was to examine the state of the fisheries in US coastal waters and recommend improvements in their management. The Magnuson-Stevens Act created eight NOAA fisheries management councils: in the north Pacific, Pacific and western Pacific; the Gulf of Mexico and Caribbean; and the south Atlantic, mid-Atlantic, and New England. Each council has roughly equal representation from the marine ecology community and the fisheries industry. The fisheries management councils' primary responsibility is to use the "best available science" to develop and update fisheries management plans in light of the trends in fish harvests. To that end, each council has a scientific advisory committee.

My particular interest was to examine the effectiveness of the interactions between the scientific advisory committees and the management councils. What I learned shook further my faith in the view, implicit in Vannevar Bush's philosophy, that the best basic science will *automatically* produce the best outcomes for society. The path connecting knowledge to action needs explicit attention. The government's management councils were constructed to balance the interests of the fishing industry and the environmental community, and the two groups had roughly equal representation on the management councils—a prescription for deadlock in a democratic society.

The science advisory committees' task was to examine recent trends in fisheries productivity and ecosystem health and recommend the next year's fisheries catch. The advisory committees' recommendations, whatever they were, were highly likely to split the management council in half and create gridlock, especially when the data were changing and uncertain, as they usually are. The hope that the councils would enable flexible, data-driven responses to changing environmental conditions was often defeated, not by ill will but by the dynamics of the decision process, given the equal balance of interests among the deciding parties. Yet no decision was worst of all, and the committees' NOAA manager had to recommend a fisheries' catch every year. The default was essentially to return to the last time the council had reached agreement and recommend that.

I saw that educating PhD-level marine ecologists to work in decision arenas like fisheries management councils, as Nancy Knowlton and Jeremy Jackson proposed Scripps do, could facilitate society's uptake of marine science. The Center for Marine Biology and Conservation they founded sponsored an academic curriculum unlike any previously at Scripps—its first professional master's degree program. CMBC students would take the basic courses that PhD students take in the first year, as well as an introduction to the policy and advocacy dimensions of conservation science. Their academic year would start with a summer "boot camp" taught by volunteer faculty from across SIO and UCSD. The students' year would conclude with a “capstone project” and a final oral examination designed to be like a press interrogation. CMBC, hoping to draw working professionals as well, also offered a fee-based certificate program for those taking only the boot camp. It was hoped that the mixture of experienced and aspiring professionals in the boot camp would enrich the student experience as well as help pay expenses.

**Figure 28-2: Dame Jane Goodall, winner of the 2004 Scripps Nierenberg Prize for Science in the Public Interest. I had two reasons for my delight that Jane Goodall was the Scripps Nierenberg lecturer for 2004. I believed she would appeal to the La Jolla and Scripps donor communities, and I wanted the Scripps faculty to appreciate that Scripps was making a commitment of academic resources to biodiversity and conservation and a new professional master’s degree program.**

Its first year was a heady one for the new program. There was pent-up demand for CMBC’s offerings and CMBC got the lion’s share of the best applicants to SIO biology in its first year of graduate student recruitment. This caused a reaction among SIO’s basic biologists that required serious attention. There were master’s students with non-research goals in biology classes. Moreover, faculty were being asked to donate to CMBC the added teaching burden posed by the additional students. It was not easy working out how credit for teaching should be allocated. There was a financial issue, since certificate students were paying fees that were returned only to CMBC. Nancy was taken aback when I told her that CMBC could not live on charity forever and that an agreement to share the burdens more equitably would have to be found. There followed an impassioned many-sided negotiation that I find difficult to reconstruct at this remove. Suffice it to say, CMBC survived and prospered, even after Nancy and Jeremy returned to the Smithsonian Institution. Perhaps the most telling signal of success is the quality of the CMBC directors that followed them, Lisa Levin, Stuart Sandin, and Jennifer Smith.

In May 2023, SIO formally dedicated the Ted and Jean Scripps Marine Conservation and Technology Facility. At last, CMBC had its own building that it will devote to conservation research and laboratories for the more than 400 undergraduate majors who come down the hill from the upper campus to study marine biology and conservation. CMBC's new building is artfully contoured to fit in a spectacular hillside site overlooking the Pacific. It was a pleasure to learn that it was designed by the same architects, Safdie Rabines, who did so well with the Scripps Seaside Forum during my term as Director.

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The National Academy of Sciences created a new section, Section 63, to recognize excellence in research on the environmental sciences and ecology in 1998 (I believe). I chose to affiliate with this new section, as well as Section 16 (geophysics) as soon as it was announced. After I was stepped down as SIO Director, I received a telephone call from Simon Levin of Section 63, a Distinguished Professor of Ecology and Evolutionary Biology at Princeton. Would I join him in the preparation of the case for election of Nancy Knowlton to NAS? CMBC would be an important part of the case. Nancy was elected in 2013 and Jeremy in 2019. Rare is the married couple who both are elected to membership. When you see such a couple on the dance floor, it is hard to make out who is leading.

The receptivity of the scientific community to CMBC's kind of work has evolved in the two decades since CMBC was founded. Senior Scripps faculty on my watch had been as wary of interacting with non-governmental advocacy organizations as with the media or politicians. Scientists, they argued, must personify objectivity in both personal and professional life, and avoid taking sides on issues. The historian of science, Naomi Oreskes, once remarked to me that even experts' bringing up an issue is a political act. CMBC, anchored in science, brings up issues. It introduces students to a social dimension they will need to understand as they grapple with the coming world crisis of climate change and sustainability.

# 29

# Scripps as an Institution

You can't always spot academic excellence right away: It takes time for encrusted ideas to fall away and new perceptions to take shape; it takes time for schooled people to recognize originality. Mannerisms masquerade as brilliance. Ordinary individuals do extraordinary things. Elegant proposals are not always matched by elegant achievement. Intellectual excellence is not invariably accompanied by social skills.

If academic institutions cannot guarantee that they unerringly recognize excellence, they can build integrity into the processes they use to evaluate excellence. It is thus a glory of the University of California that faculty judge faculty in academic personnel decisions. The primary appraisal by a faculty review committee follows two independent decision paths, one in the Academic Senate and the other in the administration. There are no resolution mechanisms when Senate and administration disagree; they must duke it out on a case-by-case basis. If the most consequential thing I could do as Director was recruit talented faculty, then my most important responsibility as Dean of Marine Sciences was to ensure the integrity of SIO's academic promotion processes. To that end I had almost daily conferences with Ray Weiss, a particularly independent-minded faculty member in the Scripps tradition of prickliness. I had asked Ray to be Associate Dean with the sole responsibility of ensuring the integrity of every academic promotion case, regardless of institutional needs. Ray served in this capacity for many years, which was an indication of the trust his colleagues had in his judgment—and his integrity.

Scripps had a tacit financial understanding with the UCSD campus it had incubated. When UCSD was founded, Scripps was permanently allocated (as I recall) only eight state-funded full-time equivalent faculty positions that carried the possibility of tenure. However, Scripps would be free to run its own affairs in return for not making serious demands on State of California funds, which would be used primarily to build the new campus. Since Scripps was to rely on grant funding for growth, the UCSD administration looked the other way when SIO reduced its teaching load to encourage greater capacity to support grants. UCSD distributed the overhead funds derived from SIO's grants to SIO (after taking a small tax), which could use the growth in overhead return to finance new initiatives. Scripps had the freedom to hire in any sub-discipline of earth science. This business model, together with the absence of a requirement to support an undergraduate curriculum, was what enabled SIO to grow multi-disciplinary research programs.

Scripps' unwritten business model was approaching its limit. Over time, the research faculty financed purely by grants came to rival the state-funded teaching faculty both in number and quality. The difference in status and security between faculty and researchers with comparable records of accomplishment strained collegiality and created inequity. The dollar allocations corresponding to the academic hard money positions were divided up into fractional pieces, and professionals with functionally indistinguishable research records ended up with different mixtures of hard and soft money for their financial support. It was hard to say what a fractional tenure appointment would mean in an emergency, and no one wanted to address the question unless forced to; hoping for the best was in everyone's short-term interest. Meanwhile, the status inequities among and between SIO faculty and researchers were growing harder to justify.

The passage of time strained SIO's relationship with the upper campus in another fundamental way. The vanishingly small teaching loads of SIO faculty were becoming harder to justify to a UCSD administration that had forgotten about the original deal. Since state funding for faculty positions used an allocation formula that counted only the number of undergraduate students taught, SIO had not received a new state-funded faculty position since 1975, whereas the campus up the hill was awash in new positions. No appeal to the global importance of SIO research could or would change that; international esteem does not generate funding. In addition, the academic pecking order within UCSD had evolved: some young departments on the main campus were now just as prominent in their fields as SIO was in its, and the teaching load inequity between SIO and the main campus lost us friends in the central administration.

SIO's principal investigators had to do all they could to maintain their research grants—but SIO as an institution needed to grow new revenue sources to evolve intellectually. SIO had already added new research fields and new grant funding opportunities, and now it had to pay serious attention to undergraduate teaching to expand its research remit. The immediate reason was that we could be allocated new faculty positions carrying tenure, but I had other reasons for not fanatically protecting SIO's envied low teaching load. I believed that creating long-term public support for solutions to the world's climate change problem required an undergraduate education program. If we were to pursue Roger Revelle's sustainability vision, SIO would eventually need to work with the social sciences on the UCSD campus, and SIO's standoffish stance would be a barrier to collaboration. We could not diversify the faculty without new full time equivalent faculty positions (FTEs), and SIO couldn't get new FTEs without teaching undergraduates. Still, many SIO faculty at the turn of the millennium feared a large expansion of teaching would dilute the concentration on research that had taken SIO to the top of its game. They feared we were becoming just another department in a huge university.

SIO got its first allocation of new faculty FTE since 1975 on my watch as Director. They were only a token. SIO's commitment to undergraduate teaching developed slowly, but now, seventeen years later, SIO manages UCSD's Earth Science and Marine Biology majors. Moreover, my successor once removed, Margaret Leinen, has frequent access to new FTEs, and SIO has divided some of them into interdepartmental appointments. Of 125 faculty positions, twenty-eight are now shared with other departments, from engineering to public health to archaeology. Pradeep Khosla, who succeeded Marye Anne Fox as Chancellor, has created a strategic plan that places SIO at the lead of one of UCSD's four main areas of institutional research commitment, *Understanding and Protecting the Planet,* in furtherance of Roger Revelle's vision.[99]

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[99] University of California, San Diego, *Strategic Plan Report: Defining the Future of the Public Research University*, accessed October 30, 2022, https://plan.ucsd.edu/report#goal-3


**Figures 29-1 & 29-2: At the 2016 Conference of the Parties (COP) meeting of the UN Framework Convention on Climate Change (UNFCCC), Marrakech, Morocco. Top photo: the main meeting hall; I hoped and still hope that one day Scripps could host a COP meeting at its Seaside Forum. Bottom photo: the poster advertising the Blue Zone, where many advocacy organizations including Scripps had displays. Photographs by Charles Kennel, November 9, 2016.**

I also sought to expand access to international opportunities for both faculty and students at SIO with the help of Lisa Shaffer. Lisa was the only person brought to SIO as part of my recruitment package in anticipation of the international responsibilities SIO would have in an era of global warming. When she directed international relations for NASA's earth observation programs, I had seen her unusual ability to build cooperative programs like the Committee on Earth Observing Satellites (CEOS). In addition to ARGO and POGO at Scripps, Lisa organized a joint program with John Browne's British Petroleum, and by seemingly endless back-and-forth she engineered the accreditation of the University of California to the annual UN Framework Convention on Climate Change's Conference of Parties (COP) meetings, which will have perhaps the most enduring impact. SIO sends about ten faculty and students to the annual COP meetings, which are held all over the world. COP meetings give students a flavor of the larger world their work will take them into. The support of student travel to the COP meetings has become attractive to donors, and Ellen has contributed personal funds to it for many years.

While Director, I could not justify taking the place of a student in a COP meeting, but in November 2016, I paid my own way to the COP meeting in Marrakech, Morocco. Many in the SIO group stayed up all one night to hear the results of the American election. In the next few days, very tired students came to me one-by-one, sometimes two-by-two, with versions of the same question: will we get jobs working on climate change with Donald Trump as president? I replied that Trump's election had made their work more important than ever, and if they did not do it, who would? Besides if they are worried about the loss of their civil liberties, the State of California will protect them.

In addition to growing the faculty and developing its educational programs, SIO also needed updated facilities that reflected its status as a world-class institution. In my first few days as Director, I learned of a plan to build a conference center on the beachfront land next to the Director's Office. At first, I thought La Jolla had enough meeting facilities, but ultimately, I was persuaded that SIO should be identified with international leadership in the socially beneficial deployment of earth and environmental science. A thought leader in a collection of sciences of profound public consequence needed a place where scientists from around the world *and* the public valued coming.

A conference center on the site of the old machine shop abutting the sea wall just south of the Director's Office would make a memorable impression. A "temporary" Quonset hut had housed SIO's machine shop on the site since World War II. To experimental scientists a machine shop is hallowed ground, so we first had to find a replacement site that would appease our experimental scientists.

Once we moved it to a centrally located parking lot, a multi-purpose forum was built next to the beach. It includes a 250-person auditorium, conference rooms for twenty, forty, sixty, and eighty, a clubhouse where students can gather, and a small restaurant where faculty, students, and beachgoers can take breakfast or lunch. We chose not to use public money for construction because the restrictions on the use of public funds would have made the Robert Paine Scripps Seaside Forum look like a prison. In the event, in 2010 the American Institute of Architects awarded Safdie Rabines Architects a prize for the Forum's integration with its natural environment. Their building won two other awards that year as well.

Private fundraising for the Scripps Seaside Forum had already started before I became Director. After SIO obtained an estimate of construction costs from the UCSD architect's office and had raised the required amount, we put the project out for bid; the lowest bid was far more than what we had raised. After we thought we had raised enough additional to cover a new cost estimate, we again put the project out for bid; again, the lowest bid was far more than the estimate. I wondered how many in the Chancellor's Office thought we were incompetent managers. This *contretemps* occurred at the peak of China's building boom and the runup to America's mortgage crisis, and we were able to show that construction prices were rising faster than financial interest rates, so SIO's business office argued that it would be cheaper to borrow to finish quickly than to delay once more to raise additional money. Scripps would repay the loan from fees for private use of the Forum for weddings and other events.

Groundbreaking took place during my last year in office. Chancellor Fox joined me in the public ceremony. I do not know how aware she was of the financial deal her real estate office had struck, but I did know she would resent so visibly committing to a project obligating her to pay off debt for a nonessential and showy project.

Despite her misgivings, the Forum completed construction on my successor's watch in 2009. As things turned out, I was to co-chair its first international conference. SIO ended up with an elegant conference center on an exquisite seaside site and few now remember the struggle to build it. In retrospect, I am comforted that we did not blow an opportunity to go first class.

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As you make your way toward the Birch Aquarium at Scripps (BAS), you walk past an inverted fountain that reminds me of the similar one outside UCLA's Physics Department in Kinsey Hall. The Aquarium's fountain features bronze sculptures of two breaching whales leaping high out of the water. Once past the leaping whales, you pass an admissions kiosk into an intimate courtyard, with a gift shop and classrooms to the left and administrative offices to the right. The glass doors at the front of the courtyard lead into an auditorium, where there are doors leading to museum displays on the left and fish tanks on the right. Keep walking through the auditorium and you find yourself outside again in another courtyard with a panoramic view of the Pacific Ocean. Here is a shallow pool in which children can feel sea creatures with their hands. The younger children, their eyes lifted up to their docents, never failed to give me a moment of pleasure. No matter how stressful the day, visiting the aquarium settled my mind.

**Figure 29-3: The breaching whales in the fountains at the entrance to the Birch Aquarium at Scripps before the millennium.**

The Birch Aquarium is SIO's main instrument of public outreach and its main way of supporting K-12 education. Before the Aquarium was established, Scripps had had an informal aquarium in one of its research buildings. In 1985, the Stephen and Mary Birch

Foundation started fundraising for the present aquarium on its own site, which opened in 1992. The Birch Aquarium acquired its own professional staff and management structure as part of the informal to formal transition, but its role and identity within SIO and UCSD were still evolving, as I soon discovered after arriving in late 1997.

The Aquarium's role in contributing to the broad goals of SIO and UCSD had never been well articulated. BAS staff felt isolated from the academic enterprise, and SIO academics were not aware of what the aquarists, no longer co-located with them, were doing up the hill. Although this situation was far from ideal, that concern was not dispositive—until a budget problem forced a change in leadership. Tom Collins, SIO's Business Manager, and I believed it was necessary to install a new director if the Birch Aquarium were to be put back on track. Tom's main concern was, properly, budgetary mismanagement, but I had a broader misgiving. The Birch Aquarium Director at the time thought his main job was fundraising (he and his wife hungrily pursued La Jolla social life), while I believed that Birch's main job was to represent SIO's research to the public. The climate change denial movement was active in the late 1990s, and Scripps needed the Birch to communicate what was happening to the oceans and climate.

Tom Collins, working closely with the UCSD budget office, arranged to have BAS taken into receivership, whereby the BAS gift shop and certain other revenues would go directly to the central administration until its deficit was retired. In turn, UCSD would temporarily fund its operating deficits. Since BAS was unlikely to attract a professional aquarium director until the deficit was taken care of, we had to find an interim director from within SIO. Though there would be a salary increment for whoever became the interim director, there would also be salary savings that could be applied to deficit reduction. Tom had worked well in the past with Jeff Graham, a shark physiologist on the research faculty, and recommended that I persuade Jeff to take the thankless job. I did not need much persuasion, because I had already seen that Jeff had the right instincts. One memorable Saturday, Jeff mesmerized Ellen's and my pre-teen nephews in his lab with hands-on (and hands-in) instruction in shark physiology. Jeff agreed to take on a job that would not advance his research career, a testimony to his devotion to Scripps.

The budget reforms in place, it remained to Jeff to keep BAS together until it was out of intensive care. Jeff first set out to solve BAS's morale problem. Its budget crisis had made it almost impossible to create new displays, but Jeff started discussing with the staff what they could build anyhow. Not enough money for new displays? Jeff did it the old Scripps way, assembling materials at hand with his own two hands. The work got done, and BAS made it through. Just as important, Jeff articulated to his staff the unique role they played within SIO as outreach professionals. Here was a professional scientist listening to the downtrodden staff's ideas!

Jeff, with my encouragement, also created a tradition that has prospered to this day: he invited his SIO colleagues to give public lectures on their personal research. A lot of us had feared that busy scientists would resent the imposition on their time, but the famously hard-nosed SIO faculty loved to explain what they were doing to the public, and they crafted their popular talks with care. I see now that this reflected a new attitude about scientists' public responsibilities. After about three wearing years doing two jobs, Jeff really had to return full time to his laboratory. There was little I could do to thank him, but I named the Aquarium lecture series the *Jeffrey B. Graham Perspectives on Ocean Science* Lectures in his honor. It was Ellen's inspiration to donate the coffee, tea, and pastries served at the Graham Lectures, which creates a family-like atmosphere. Jeff's family regularly attend, and it is everyone's pleasure to see them there.

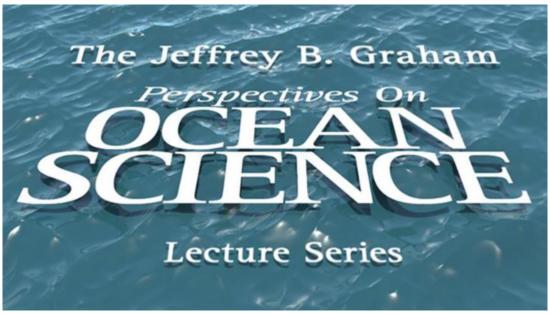


**Figure 29-4: The artful introduction to the University of California Television (UCTV) presentations of the Jeffrey B. Graham Perspectives on Ocean Science Lecture Series.**

Jeff has been dead for ten years now. I like to think he would be proud of his living memorial; the Graham lectures are recorded and released by the University of California television network, and surpassed one million views. There are now 220 video recordings of Graham Lectures given from 2001 to 2022 available on the UCTV website. Each lecture is a personal account of research just after it was done by the one who did it. The Graham Lecture Archive is already a resource for historians of ocean science.

The Birch Aquarium's financial deficit in hand, it became possible for Tom Collins and me to recruit a permanent BAS Director. For us, there was only one choice: Nigella Hillgarth. Nigella is an Oxford PhD ornithologist who spent her younger years on distant research expeditions (already a plus for Scripps), and in midlife became the Director of the Tracy Aviary in Salt Lake City, the largest bird park in the US. Ornithologists are by nature gentle people, but those at the Tracy expressed forceful regret at her leaving for La Jolla. They thought she was wonderful, as I came to as well.

**Figure 29-5: Nigella Hillgarth, the Director of the Birch Aquarium at Scripps, flanked by Sir David Attenborough (left) and Charles Kennel (right) on the occasion of the award of the 2005 Nierenberg prize to Sir David. Ellen Lehman took this picture looking west over the Pacific Ocean from the balcony of the Birch Aquarium.**

Nigella carried on the work of integrating the Birch Aquarium and public communication into the soul of the Scripps Institution of Oceanography. BAS was soon recognized as one of San Diego's important tourist destinations, at a level just behind the world-famous San Diego Zoo. BAS provides an intellectually sophisticated alternative to San Diego Sea World, which has become a commercial theme park with fish despite having started life as a research organization. The Birch is what most out-of-town visitors think of when they hear the word "Scripps." If tourists come to Birch, then Birch goes to schoolchildren; BAS sends vans loaded with specimens, educational materials, and aquarists who explain the displays to local schools.[100]

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Scripps' centennial year, 2003, was a banner year for me personally. Admiral Conrad Lautenbacher, the Administrator of NOAA, invited me to the Earth Observation Summit, a meeting at the minister of state level to kick off what became the Global Earth Observation Systems of Systems (GEOSS). Though I had no government position, I was invited because I had overseen the redesign of NASA's Earth Observation System, which would become the foundation of GEOSS.

I was also elected to America's oldest and most distinguished scholarly society, the American Philosophical Society (APS), founded in 1743 by Benjamin Franklin. The ways people are elected to honorary societies are mysterious and never more so than to the one elected. Judging from their enthusiastic greetings when I appeared at my first meeting, UCLA's Leon Knopoff and SIO's Walter Munk must have had something to do with it. Election to APS has meant more to me than any other distinction, prize, or honor. The APS recognizes excellence in all fields of intellectual and social endeavor and convenes presentations in diverse fields at every meeting. No matter the topic, there are always more than a few in the audience who can carry on an informed debate. Ellen's joy in the people we meet[101] and the things we talk about more than doubles my own. Ellen has donated her collection of books on native life in Alaska and the Canadian North to the Philosophical Society's library. While APS's collections are strong in many aspects of indigenous American life, its collections lacked materials in the post-statehood social and cultural development of native Alaskan life. There is where she is filling a void. Her pleasure in doing so is tangible.

Scientific friends and colleagues from around the world came to La Jolla to celebrate Scripps' centennial. I took advantage of the

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[100] Ellen and I were saddened but understood when Nigella left the Birch in 2014 to direct the New England Aquarium in Boston; conflict with my successor, Tony Haymet, had taken its toll. She has since returned to La Jolla and has been caught up once again in Scripps' destiny: the Climate Science Alliance named her a Climate Art Fellow for her bird photography.

[101] We became especially fond of the climate scientist Warren Washington and his wife Mary.

gathering of luminaries to get some ocean work done. D. James Baker[102] and Conrad Lautenbacher,[103] Administrators of NOAA during the Clinton and Bush administrations, respectively, agreed to make the case for including ocean observations in GEOSS, which would be created in 2005 by intergovernmental agreement. As a consequence, POGO was accredited to GEOSS.

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**Figure 29-6: The logo for the Scripps centennial celebration.**

I could feel the anticipation building a year beforehand. First it was excited comments in the hallways, then a delegation coming to say it was time to start planning the centennial celebration. The community of long-time Scripps faculty commenced work on an affectionate theatrical reminiscence to be performed on June 9, 2003, now available on the internet. Kevin Hardy took the lead with the wider community; his bumptious enthusiasm and uncomplicated love of Scripps helped make SIO's celebration the grand occasion it was. Kevin started by inspiring the creation of historical displays that were shown around San Diego County. He convinced the Marine Technology Society to hold its 2003 meeting in San Diego and to devote several sessions to Scripps oceanography. I invited Jane Lubchenco, my colleague from the Pew Oceans Commission, to present the 2003 Nierenberg Lecture at the Marine Technology Society meeting. The Del Mar Fair devoted several rooms to celebrating SIO's history and presented a show of underwater cinematography.

Ellen Lehman presided over an intimate centennial event designed for the traditional Scripps community. A pair of propellers from the retired SIO research vessel, R/V *Horizon* (1949-1968) was put on public display overlooking a courtyard on the Scripps campus. Walter and Judith Munk had donated *Horizon's* propellers as a Centennial gift

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[102] D. James Baker, Undersecretary of Commerce and NOAA Administrator (1993-2001), and consultant to the UNESCO Intergovernmental Oceanographic Commission, which Patricio Bernal, a Scripps graduate, directed.

[103] Conrad C. Lautenbacher, Admiral USN, was NOAA Administrator and Undersecretary of Commerce, 2001-08.

and it was Ellen Lehman's idea to name the propellers *Walter* and *Judith*. A typically practical US Navy officer invited to the dedication ceremony naturally experienced in ship christenings had warned Ellen to stand back when she swung the champagne bottle lest it explode over her dress. It fell to me to preside over the public event to celebrate Scripps' centennial the next day.

Scripps' centennial day, September 26, 2003, opened bright, sunny, and cool as the crowd gathered. The sound of the surf blended with the susurrus of the crowd. I looked out at the expectant crowd from the reviewing stand facing the new Pawka Green, which we had newly landscaped for the occasion. There were so many that we had to keep ambulances standing by for emergencies.

**Figure 29-7: Three women who made Scripps special, September 25, 2003. To the left is the ever-radiant Judith Munk, wife of Walter Munk; to the right is the ever-optimistic Ellen Revelle, widow of Roger Revelle; another Ellen, Ellen Lehman, wife of the 2003 Director, preparing to swing a champagne bottle to christen the propellors from *R/V Horizon (ret.)*. Photograph by Charles Kennel.**

There were mothers, fathers, children, aunts, uncles, cousins, friends, and neighbors; scientists, engineers, sailors, and surfers; students; colleagues from the US and abroad; politicians, admirals, and agency officials. All were there to celebrate the hundredth birthday of a local landmark that is almost as much a San Diego institution as the US Navy.

That day marks the high-water point of my life at Scripps. The tide of events that carried me to Scripps was about to turn. In 2004, Bob Dynes, the physicist who hired me, left UCSD to become President of the entire University of California. Bob's successor was to be Marye Anne Fox. My effectiveness as Scripps Director was over but I had no way to know that.


**Figure 29-8: The Scripps centennial celebration. Photo by Ellen Lehman.**

# 30

# On Being Resigned

I met Bob Dynes when we served together on the advisory board of what is now the Kavli Institute for Theoretical Physics at the University of California Santa Barbara in the 1980s. The dreamy Santa Barbara theorists had recruited Bob, an experimentalist at the fabled Bell Laboratories in New Jersey, because they recognized the streak of constructive optimism in him, as I came to do. His relentless optimism could even be what got him named Chancellor. Later Bob's informal management style got him into trouble, but I found it congenial, for he encouraged initiative. Bob was to follow another UCSD Chancellor, Dick Atkinson, to the Presidency of the University of California. The fellow physicist who had put Ellen and me up in his own house, whose wedding we had attended, was about to move up.

I avoided the political melee among Bob Dynes' direct reports surrounding the search for his successor, with one exception. I notified Marcia McNutt, a Scripps graduate, that the UCSD Chancellor's job was open. (Marcia eventually became President of the National Academy of Sciences.) I believed the case for Scripps spoke for itself and I tried to be equally hospitable to all visiting candidates for Bob's successor; I did not commit to anyone, fearing a wrong choice would compromise my standing with the eventual winner. I was disappointed when negotiations with my secret favorite candidate did not work out, but I had no reason to fear the accession of Marye Anne Fox from North Carolina State University in July 2004. Later, I learned that North Carolina State's faculty senate had censured Fox.[104] There was also something incongruous about her husband, especially in a university atmosphere: he was a right-wing political ideologue given to intemperance and prejudiced rants at social events.

I had been naïve. From working closely with Chancellors Young, Carnesale, and Dynes, I came to trust that chancellors would encourage initiative, or they would not be chancellors. This was another beautiful theory murdered by ugly reality. One event conveyed to me how different Marye Anne Fox's attitudes towards risk would be. SIO's work with the Venice Gates project (Chapter 33) had attracted the interest of Dennis DeConcini, Senator from Arizona from 1977-95, who loved all things Italian. Dennis and his wife, Patty, escaped Phoenix's frightful summer heat to their seaside home a short walk from Scripps, and Ellen and I saw them occasionally when they were in La Jolla. The President of Arizona State University, Michael Crow,[105] was going to visit Dennis, a trustee of ASU, and Dennis asked me to arrange a dinner invitation to Marye Anne so that she and Michael Crow could meet. The dinner came and went without notable incident, except that Marye Anne was careful to relate to me her reaction. I had to understand, she said, that she could never commit as much to climate change as Crow. Crow had taken over a third-rate university so the world would not suffer a big loss if he failed. If she tried the same thing at UCSD, it would be more serious, she said. She would never take the risk.

Fox's and my differing attitudes towards risk were at the root of my demise. Marye Anne listened for signs of discontent whereas I listened for signs of enthusiasm. Permissiveness in leaders was an inherent source of risk to her: the more important the leader, the more visible the issue, the larger the consequence of failure. My disjunction with Fox would have been containable except that some restive Scripps faculty could appeal to this aspect of her character. The keystone issue with the traditional Scripps community might

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[104] Aniesha Felton, "Faculty Senate Censures Fox," *Technician*, May 2, 2003, p. 6, accessed November 2, 2022, https://ocr.lib.ncsu.edu/ocr/te/technician-The-Last-Page-2003-05-01/technician-The-Last-Page-2003-05-01.pdf.

[105] Michael Crow had interviewed me about directing Columbia University's then new Earth Institute and venerable Lamont-Doherty Earth Observatory; Crow went on to the presidency of Arizona State University in Tempe, where he reorganized an entire campus around climate change and sustainability issues.

well have been ships, and not the risk of going big-time into climate change. SIO's then newest research vessel, the *R/V Roger Revelle,* had had its first research cruise in 1996. When I took over in late 1997, it was already time to position SIO to get the next generation vessel since it would take a decade to lobby the oceanographic community and Washington for a new class of research ship. I left this sacred task largely to SIO's veteran sailor-scientists who had done so well in the past. SIO never did get a commitment to a new vessel on my watch as Director.[106] My *pro forma* attention to ships undervalued traditional SIO's successful way of life.

**Figure 30-1: My farewell cruise, somewhere off the two Californias' coasts. On the left is John Hildebrand, who taught me much about whale conversations on my one earlier cruise to Hawaii. John had the endless curiosity about the arcane that was characteristic of the best of Scripps. At right is Bob Knox, Associate Director for Ship Operations and Technical Support until his retirement in 2007. Bob personified the quiet competence of Scripps seagoing oceanographers, many of whom had been in the US Navy.**

That intoxicated friend of Scripps at Fred Spiess' birthday party may have been onto something when, loosened by alcohol, he opined that I would not be accepted even after ten years on the job.

[106] SIO got one of the first two research vessels in the new class in 2016. It was named for my UCSD and Augustine Commission colleague, Sally Ride, the first US woman astronaut. Sally and my daughter, Sarah, were both graduates of the Westlake School in Los Angeles.

It was not as though I misunderstood SIO science, but I did not personify a disciplinary identity, the kind shaped by shared experiences in distant and dangerous waters—months away from home, exotic foreign ports; lonely watches on board; storms at sea. I could learn oceanography's words but not its body language.

As a kind of farewell tribute, I was invited on a brief cruise, only my second, (off the coasts of California and its Mexican sister state, Baha California) toward the end of my term as Scripps Director. By then, it was too late.

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Chancellor Fox secured the departures of all but one of Bob Dynes' vice chancellors within two years of her arrival. The only common feature in the epidemic of resignations seemed that the departed had independent public visibility. The architect of UCSD's successful financial development campaign was gone within months. Even more striking were the hasty departures of the most powerful vice chancellor of all, Ed Holmes of Health Sciences and his wife, Judy Swain. This was clearly unintended on their parts because that very year Judy had given up her position as Chair of the Department of Medicine at Stanford to join him at UCSD as the founding Dean of Translational Medicine at UCSD. If that were not enough warning, I should have been suspicious when Ed Frieman, who I knew spoke regularly with Marye Anne, proposed to work with the NOAA Administrator, Conrad Lautenbacher, to place me as first Director of the Global Earth System of Systems at the World Meteorological Organization in Geneva. It would have been a tactful way out, but all I wanted was to stay at Scripps. I could think of no other academic institution with such a proud history in climate change research.

My scheduled five-year review had been delayed until Marye Anne was in office, but now my dossier was put out for review. The review committee report came back with an altered conclusion page in another typescript recommending tersely that I be terminated. The altered page seemed to replace what must have been the committee's lengthier recommendation. The rest of the review, though it did not contain paeans of praise, did not seem like a mortal accusation either. Chancellor Fox appointed a chemical oceanographer, Tony Haymet, whom Ellen and I had met in Hobart, Tasmania, when he directed the CSIRO Marine Laboratory there. Dan Goldin said I was foolish to stay on until Haymet arrived, but I had a hard time letting go. When

Haymet did arrive in September 2006, he announced to me—and others—that Scripps was a steppingstone to much, much bigger things for him.


**Figure 30-2: Kennel Way. When universities have the departure of a senior leader in prospect, their development offices have an occasion to raise money for a building named for the nearly departed. California's legendary automobile traffic makes naming streets arguably as honorable as naming buildings. For me, UCSD named the street that goes by the Director's office on the Scripps campus. From what I heard, the struggles with the University and State of California's naming bureaucracies argues that Scripps' Development office ought to stick with buildings. Photo by Ellen Lehman.**

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SIO's transition from dependence on Navy funding for defense to dependence on NSF funding for geophysics and NOAA funding for the environment contributed to a crisis of institutional identity that played itself out on my watch as Director. At seventeen years' remove, it appears it has not proven possible to hold back the tide of change in oceanography. Ships remain a fundamental element of discovery, but the NSF and Navy integrate them ever more into a single national fleet under government direction. University departments can now make serious contributions to oceanography without having a ship docked near their campus. (Astronomy departments need not be located in space to have experiments on spacecraft, after all.) Ships still go on extended voyages of discovery, but their scientists make brief flying trips to their research destinations. Today's oceanographers have briefer opportunities to bond with shipmates, and are becoming like faculty members in more normal departments. Moreover, SIO's old long-voyage argument for major relief from teaching looks increasingly hollow. Finally, in addition to defense, oceanography now serves a new priority—climate change—that connects it to a cast of characters and institutions whose personalities and priorities take adjusting to. All this adds up to a comprehensive change like the one that I had seen earlier with the introduction of space observations and remote observatories into astronomy.

The migration of government funding priorities also changed the qualifications desired of SIO leaders. My two predecessors and I started professional life as physicists, and my two successors as earth scientists. Ed Frieman had been a true cold warrior, a member of the JASONs,[107] a member of the President Reagan's Science Advisory Council, and a vice president of SAIC, a major defense company. My immediate successor, Tony Haymet—a chemist, not a physicist—had directed Australia's largest seagoing oceanography center, but for obvious reasons had not established a major presence in Washington. Tony's successor, Margaret Leinen, headed *two* US oceanography institutions *and* the Directorate for Geosciences at the National Science Foundation. She is an earth scientist with clout in Washington. The two appointments reflected the rising political influence of the earth sciences as climate change began to compete with the Cold War as a driver of science policy. No longer did Scripps need to borrow the clout in Washington that physicists once had during the Cold War.

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[107] JASON stood for the months—July, August, September, October, November—that a group of extraordinarily elite scientists chose to meet in La Jolla to advise the Defense Department and intelligence agencies on classified national security issues.

# 31

# Reconstruction

It did not come as a surprise when Chancellor Fox handed me her draft press release announcing my departure. I suppose she thought I would vet the list of accomplishments. Dark thoughts were to live in hidden crevices of my mind to spread their miasma at night, but my daytime reaction on being notified of my resignation was to salvage what I could. The list of accomplishments was not my primary concern. *The press release did not say what I was going to do next*. I persuaded her to say that she had asked me to explore the feasibility of an Environment and Sustainability Initiative (ESI) that links SIO with the upper campus during my lame duck last year in office, one more step towards accomplishing Roger Revelle's vision for UCSD.

Chancellor Fox did not commit resources to ESI other than the chance to make the case. Shortly after the press release appeared I visited Ellen Revelle, Roger's widow, who still kept their home near Scripps. You know, she said, they did a similar thing to Roger when he was passed over for Chancellor after having founded UCSD; they gave him make-work at Berkeley, which persuaded us to go to Harvard. She hoped I would not be driven out as well. Roger's Ellen offered me a small donation to try to make something of the ESI Initiative. It encouraged Lisa Shaffer and me to host a lunch for potentially interested faculty. I have found a paper copy marked "draft for discussion" prepared by Lisa and me, dated August 2, 2005, with the following abstract:

> UCSD, by converging its strengths in the biological and biomedical sciences, engineering and information sciences, and earth sciences, is developing practical sustainability solutions through rigorous science-based approaches to decision-making. Its sophistication in the study of human affairs connects a unique set of scientific strengths to the social and economic aspects of sustainability.

With this statement of scope, the ESI idea passed muster at Ellen Revelle's lunch, and Lisa and I had a list of potential participants in the Initiative. We wanted ESI to be a true collaboration between the upper campus and Scripps, and to that end Paul Linden, the Chair of the Mechanical and Aerospace Engineering Department, joined me as ESI co-Director. Larry Smarr found an office suite in the Atkinson Building of what is now called the Qualcomm Institute, and Lisa became ESI's Executive Officer. We had fabricated all the appearances of a sanctioned academic program. Now all we needed was a program.

It is a good thing that I had that Qualcomm office, because I no longer felt welcome on the SIO campus. I was not asked explicitly to stay away, but I eventually learned that Tony Haymet forbade SIO staff to contact me or Ellen. Of course, some did; we sympathized and made suggestions but did not intervene with Tony or anyone else. This was an essential courtesy owed one's successor. Ellen and I value our friendships with the staff as one of the great pleasures of the Director's job and we have abiding affection for those who work hard for SIO without much recognition. In some ways, the staff understood SIO's role in society better than the faculty. We did not know they were placing their jobs in jeopardy by speaking with us.

Bureaucratic maneuvering had done what it could with Chancellor Fox, but it would come to naught unless a beached, sixty-eight-year-old former Scripps Director could reacquire intellectual depth. I was less prepared for an independent research life than a recent PhD graduate, or even a high school student. I had not attempted research while NASA Associate Administrator, UCLA Executive Vice Chancellor, or SIO Director. I could think about other people's research but not my own. Thirteen years' absence from research had robbed me of the mental tenacity that grinds down resisting details and smooths the way to answers. I had lost facility with mathematics, lost currency in space research, understood earth and ocean science only as an administrator, and knew less than high school freshmen about digital tools like Wolfram Mathematica, PowerPoint, Excel, Google Images, and Google Scholar.


**Figure 31-1: Lisa Shaffer outside the Scripps Director's Office on May 9, 2005. Lisa was the most impressive staff person I worked with at NASA; she knew how to translate ideas into action. Lisa became SIO Assistant Director for International Relations and later she would choose to venture into the unknown territory of the UCSD Sustainability Solutions Institute.**

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I had negotiated a termination-of-service sabbatical leave with Bob Dynes that I divided between winter quarter 2007 at Christ's College, Cambridge, and spring quarter 2007 at the School of Public Policy at UCLA. I ended up convinced I had no choice but to start with what I had done at NASA and SIO: communicate the implications of research in the earth and environmental sciences to policymakers and the public. If I started there, I might find the capacity to originate contributions to climate change policy.

After my June 2007 return from UCLA, Paul Linden and I proposed to Marsha Chandler, UCSD's Executive Vice Chancellor, that she authorize a few joint faculty appointments to support the ESI. Marsha, sympathetic, said she could not do it herself, but suggested

Paul and I visit department chairs to drum up support for aligning multidisciplinary appointments around climate change solutions. We did so. Al Gore's film, *An Inconvenient Truth,* had appeared in 2006, and we found general enthusiasm at UCSD. Our effort came to grief during the 2008 financial crisis, but one alert school, now the School of Global Policy and Strategy, recruited two faculty before the funding disappeared, one of whom was David Victor from the Stanford Law School. David was to teach me the realities of international policy making and how science does and does not play into policy.

I began to revive my moribund scholarly habits with more careful preparation of public talks. While private speculation was deeply entertaining, I dared not speak in public without certification by others more expert than I. Bill Nierenberg's admonitions to check my sources weighed heavily on me, and I relied on consensus studies for public presentations. Seemingly minor technical details of climate change science were being attacked in the popular literature by an ideologically aligned "denialist" opposition. I could see how people even in the discriminating public might not know what to believe. Like other neophytes, I needed a mentor to lead me past the rough spots. I could always talk to Ramanathan (and later, David Victor) during episodes of serious disorientation, but my day-in-day-out mentor was the UN Intergovernmental Panel on Climate Change. Students absorb quality standards from their mentors; I was absorbing from IPCC assessments not only the extent of climate knowledge, but also how secure climate science believes it to be. This helped me endure the cacophony of contrary voices in the public media.

Invitations to speak kept coming despite my removal from high position. My topics over the next two to three years included space research, earth observations, the coming sustainability crisis, sustainability informatics, the impacts of climate change on California, and ocean conservation. The venues were varied but included Stanford, Princeton, and Cambridge among alumni groups, San Diego non-profit groups, San Francisco's Bohemian Club, the San Diego World Affairs Council, a lunch group of California legislators, the Presidents of the Association of Pacific Rim Universities, the Netherlands Space Day, the International Council on Systems Engineering, the National Defense University, and the Ministers of Finance of the Americas. I was invited to give Graham Lectures at the Birch Aquarium at Scripps. It gives me special pleasure to record that

I was a speaker at Ellen's fiftieth reunions at Vassar and mine at Harvard. (In case you ask, I enjoyed Vassar's more.)

Most presentations started or ended with a question meant to tantalize: *what is the most important question in environmental science?* My answer—*what will happen to me?*—was my way of saying that we scientists were talking too much about theoretical mechanisms and too little about human impacts. It was time human concerns were placed front and center, time for nearly self-evident platitudes like *the adverse impacts of climate change fall most heavily on the most vulnerable* to be replaced by actionable guidance like *these are the risks my community should anticipate, and this is how it could prepare for them.*

UCSD's ESI was ultimately formalized and renamed the Sustainability Solutions Institute (SSI) but neither Linden nor I was named its leader. Paul had begun exploratory discussions about a donation of foundational scale with a prospective donor, a prominent San Diego entrepreneur (and a friend of Al Gore) with a deep interest in climate change. This prospective donor had asked for an indication of support from Chancellor Fox. Hers was lukewarm at best; Paul complained to Fox and was rebuffed.

The founding donation never came to pass. If Paul and I had doubts earlier, it was now clear: Fox did not want the program we conceived (and found the funding for). Paul decided to return to Cambridge to work on a successful effort there, as would I in due course. I had already been resigned once by this Chancellor but decided to stay. I was nearing retirement, and Ellen and I did not want to leave La Jolla. I learned Tony Haymet did not want me on the Scripps campus, so I continued to need the SSI office in the Qualcomm Institute. By that time my scholarly life was getting interesting, and I was enjoying working with SSI's committed support staff. Working with Kristin Blackler, Michelle Session, and Kelsey Lamberto was a continuing source of pleasure and satisfaction. We did two things that make me proud: the Venice Sustainability Advisory Panels and UCSD's first massively open online course, *Climate Change in Four Dimensions* (I will describe both in due course).

Until recently I did not perceive that ESI/SSI actually played a role, if transitory, in preparing the campus for the commitment to action on climate change that I had had hazily in mind when I proposed the ESI to Chancellor Fox on the day she resigned me. The SSI became a temporary focal point for those who wanted to mobilize UCSD's resources to address the global environmental crisis. Under a new UCSD chancellor, Pradeep Khosla, climate change became one of UCSD's four main strategic priorities.

Ramanathan led a climate solutions initiative on behalf of the entire University of California in 2016. He called it *Bending the Curve*.[108]

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The American climate science community was clinging to the hope that it could make the case for greenhouse gas emissions reduction self-evident. However, few other sciences have a public following that spreads doubt in the popular media as soon as new research appears in the scholarly media. Climate science was trapped between its strongest norm, communicating objectively the nature of climate change, and the hounding by denialists disputing the science in public media.

To add to that, climate science's own adaptation taboo was depriving it of one of its more effective persuasion tools. The adaptation taboo's manifestation was a collective reluctance to attribute the increasing frequency of extreme events to climate change before being able to attribute a particular event to climate change. A second manifestation was a corporate uneasiness about communicating the risks of climate change (what is going to happen to me?) without the rigor that would pass peer review, even though public decisions relating to human security are often made without anything close to that rigor. The science community's otherwise admirable insistence on its standards of rigor also had a cost also in timeliness of decision making, which itself increased the risks of climate change. Finally, some feared that advocacy for adaptation could weaken the public's support for mitigation: emissions reduction would then devolve from an ethical absolute to a morally neutral comparison of the relative costs of mitigation and adaptation.

California's experience taught otherwise. In 2006 the California Legislature, once it had absorbed Scripps' forecast that climate change will threaten Southern California's water security, passed Assembly Bill 32, the first to reduce greenhouse gas emissions

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[108] Ramanathan, V., Allison, J., Auffhammer, M., Auston, D., Barnosky, A.D., Chiang, L., Collins, W.D., Davis, S.J., Forman, F., Hecht, S.B. and Kammen, D.M., 2016. Bending the curve: Ten scalable solutions for carbon neutrality and climate stability. *Collabra*, *2*(1).

enacted by any American state. This led me to believe that once scientists begin talking about adapting to threats people care about, there will be political action.

One smokestack is as good as any other when devising climate change mitigation strategies, but that is not so for adaptation. To illustrate: the headwaters of the eleven largest rivers in Asia are in the Himalaya-Hindu Kush mountains. Tim Barnett of Scripps, among others, pointed out that the loss of Himalaya-Hindu Kush snows would affect the cities, towns, and farms in China, India, Bangladesh, Vietnam, Pakistan, and Central Asia, putting about two billion people at risk. An adaptation assessment would have to forecast the changes in water availability in each river basin and warn its communities of what to prepare for. It would start by projecting the changes in snowmelt driven by global greenhouse warming, include the local geophysical, technological, and political factors that determine water availability on a river-by-river basis, and tailor culturally aware recommendation for each riverside community.

The knowledge-to-action networks that could govern such a program lay in the future, but it was not too early to scope the issues that could arise. To that end, I attended three workshops on mountain snows, glaciers, and water availability, and one on African water availability. Martin Rees, Paul Linden, and I arranged the funding for the first workshop at Scripps from the Gordon and Betty Moore Foundation in 2009. Its theme was to compare the Sierra Nevada/Southern California and Himalaya-Hindu Kush/Asia water availability issues. Ours was the first international conference held at the Scripps Seaside Forum. Martin Rees, Lord Julian Hunt (former Director of the UK Meteorological Office) and I organized an African water availability workshop in Cambridge, England. Two other workshops on the decline of mountain snows and glaciers followed the one at Scripps: one was held in 2010 at the Institute for Advanced Sustainability Science in Potsdam, Germany, organized by Klaus Töpfer, and the other at the Pontifical Academy of Sciences in Rome, Italy, in 2011, convened by Veerabhadran Ramanathan and Paul Crutzen.

I came away with a series of questions from these exchanges of expert views. Even if scientists articulate the problems faced by riverside communities caused by mountain snow decline in Pakistan, Peru, or California, even if engineers design workable solutions, even if policy is framed in locally actionable terms, and even if resources are there, what kinds of relationships should foreign experts forge with local leaders? How can local leaders be persuaded to invest in building resilience to offset abstract risks in the distant future when their constituents are coping with tangible risks in the here and now?

Trust in science and scientists is essential to counter the time displacement disincentive to all climate action, whether for mitigation or adaptation: those who initiate action are unlikely to see publicly meaningful results in their lifetimes. I had been introduced to the disincentive to mitigation action in discussions with Dave Keeling and David Victor, and to adaptation action by my private tutor, the climate change assessment literature. Fortunately, I was not dependent on the assessment literature; I had a Scripps office next to Ram's in Nierenberg Hall and made sure to join the pilgrimages of scientists who wanted to get ideas from him.

**Figure 31-2: Giri Ramanathan (right), Veerabhadran Ramanathan (center) and Ellen Lehman (left) on July 31, 2012. In 2007, when SIO staff inquired where I wanted my faculty office after losing the Director's Office, I requested that it be near that of my friend and mentor, Ram. Ram and Paul Crutzen co-chaired the Vatican symposium on the disappearance of mountain snows that Ellen and I attended in 2011.**

# 32

# Convergence of Science and Climate Policy

I met David Victor in Ram's office in 2008 or 2009. Proximity to Scripps was one reason David chose to forsake the Stanford Law School for UCSD's young School of Global Policy and Strategy, and he often came down the hill to our offices in Nierenberg Hall to check out recent developments in climate science. Our discussions covered the whole range of issues as we knew them. I later organized my thoughts on what I heard in these conversations by composing a narrative for a generally educated audience, leaving out computations and technical language. David and Ram were gracious enough to lend their names to my "training wheels" essay, *Coping with Climate Change in the Next Half-Century,* published in the *Proceedings of the American Philosophical Society*.[109] That essay had no impact on either research or policy, but the writing of it set a pattern for me: organize perspective in conversation with those more informed than I, read the literature, and extract a contribution to the professional literature. My path to climate enlightenment was destined to be fertilized by the dead bodies of failed articles.[110]

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[109] C. Kennel, V. Ramanathan, and D. Victor, "Coping with Climate Change in the Next Half-Century," *Proceedings of the American Philosophical Society* 156, no. 4 (2012): 398-415.

[110] I was to compose a fifteen-chapter exposition on climate science. I thought of it as a guide to IPCC assessments for the interested amateur; my reviews from the MIT press convinced me I was not ready to write something of that breadth

David Victor's analysis of the failure of the Kyoto Protocol made me see he was privy to a way of understanding I had been blind to. On one of his afternoon visits David brought xeroxed drafts of his 2011 book, *Global Warming Gridlock,* which he asked Ram and me to review. In it, David discussed the origins of the Kyoto Protocol, diplomacy's first major agreement to limit global greenhouse gas emissions; the reasons for its failure; and the consequent gridlock in climate diplomacy. David went on to a propose a flexible bottom-up approach that recognizes the particularities of each country's situation, capabilities, and readiness to act. David's book helped frame the negotiation strategy adopted for the 2015 Paris Climate Accords, but that was in the future. It struck me right away that the Kyoto Protocol was what I, a scientist, might have designed. it seemed almost self-evident that the world would respond to the authority of science. It had done so at the beginning of the nuclear age and made heroes of the physicists who taught me in graduate school. The incessant carping of climate change denialists was an annoying manifestation of the disappearance of the world in which I came of age.

I was also struck by David Victor's observation that Ram's work on short-lived climate pollutants (SLCP's) created a new opportunity for diplomacy at a time of diplomatic gridlock. Ram had injected into our discussions the important idea that there are non-$CO_2$ greenhouse gases whose removal requires different institutional capacities and negotiation strategies. Much of Ram's life work had been on the connection between air pollution and climate warming. He had been a leader in the research that showed that only about sixty percent of the current atmospheric warming rate is due to the $CO_2$ from industrial fossil fuel burning; the remainder is due to atmospheric constituents in air pollution.

Nearly all climate policy attention had been focused on carbon dioxide for the understandable reason that a transformation of the world's fossil fuel-based economy is required to reduce $CO_2$ emissions materially. A $CO_2$ molecule once in the atmosphere remains about a century; the long lifetime leads to an almost confounding time displacement disincentive: those who spend time and treasure reducing $CO_2$ emissions will not see material reductions in

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with the clarity and depth that comes with experience. It was not succinct enough to appeal to the general reader and it did not advance the researcher's understanding.

atmospheric $CO_2$ in their lifetimes. The time displacement disincentive does not apply to Ram's SLCPs. SLCP lifetimes are ten years or less, depending on the molecule, so politicians can see results in their terms of office. Moreover, both developed and developing countries have air pollution agencies that could attack SLCPs with feasible adjustments of their procedures.

Ram and David Victor started the public debate on short-lived climate forcing agents in an article in the *New York Times* in 2010.[111] Ram had worked with Achim Steiner, Director of the UN Environmental Programme (UNEP), on the INDOEX collaboration that discovered the Asian Brown Cloud, the vast pall of carbon particulate pollution that extends from the southern slopes of the Himalaya-Hindu Kush Mountains to the equatorial Indian Ocean. It itself contributed to atmospheric warming. A recent UNEP assessment had confirmed the readiness of SLCP science for policy prime time, and in January 2012, David, Ram, and I wrote Steiner to urge that UNEP take the lead in limiting SLCP emissions.

We believed it was time to convey the emerging scientific consensus on SLCPs to the wider diplomatic community. Each of us had a different reason for proposing our article to *Foreign Affairs*. Ram believed that action on SLCPs would reduce the risks of climate change in the short term while the world waits for $CO_2$ emission reductions in the long term; David believed that a short-term success might loosen the diplomatic gridlock; I believed that future generations would accuse ours of malfeasance if we did not pursue a way of slowing climate change that we knew about.

David Victor took the lead in drafting *The Climate Threat We Can Beat*.[112] *Foreign Affairs* called David with its acceptance just days after we had submitted our article towards the end of 2011. Its editors must have suspected what was brewing, since US Secretary of State Hillary Clinton announced in February 2012 the formation of the Climate and Clear Air Coalition (CCAC), a UNEP program to exchange SLCP technical knowhow. Cities like London and Los Angeles had shown that while there is no magic solution to air pollution, diverse small solutions, relentlessly pursued, can be successful. CCAC would

[111] V. Ramanathan and D. Victor, "To Fight Climate. Change, Clear the Air," *New York Times*, Nov 27, 2010.

[112] D. Victor, C. Kennel, and V. Ramanathan, "The Climate Threat We Can Beat," *Foreign Affairs,* May/Jun 2012, https://www.foreignaffairs.com/articles/2012-04-20/climate-threat-we-can-beat.

be there to help pollution agencies include SLCP's in their policy portfolios.

We heard a rumor that our *Foreign Affairs* article was circulated at the G-8 meeting at Camp David in May 2012 as part of an effort to encourage countries to join CCAC. CCAC was a small step, but it showed at a time of discouragement that climate negotiations need not be condemned to gridlock. David was encouraged to continue thinking about what could replace the moribund Kyoto Protocol. Ram was named a 2013 UN Climate Champion. I figured I might have a future in this field after all.

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I did not foresee the six-year train of events triggered by a simple request to give an invited lecture on "some topic in global warming" at the April 2014 meeting of the American Philosophical Society (APS). I was truly honored to share the podium with the father of climate change economics, Nobel laureate William Nordhaus. The APS ultimately paired the recording of my talk with one given in 2013 by my Scripps colleague, Walter Munk, and posted them together in a podcast series commemorating its 275$^{th}$ anniversary, "Great Talks at the American Philosophical Society."

I chose to speak to this scholarly audience about a technical controversy that I feared would become lodged in the political mind. I had been following, as internet recreation, the parallel discourse in non-technical media involving climate science. Within several months of a portentous peer-reviewed article's appearance, it would be reinterpreted in more accessible forms (the colloquial word is "spun") for a mixture of motives. The contributors to this derivative literature ranged from denialists who wanted to undermine the credibility of climate science to administrators who wanted to tout their programs. Somewhere in the middle were those whom David Victor called hobbyists, who wanted to understand for themselves what the climate fuss was about. Even prior to the emergence of *Facebook*, hobbyists were carrying out passionate debates about climate change amongst themselves on the internet.

There had emerged a series of observational results that were construed as undermining the notion that fossil fuel burning leads to global warming. In particular, the unusually strong *El Niño* of 1998 had marked a transition from persistent growth in measured global temperature since the mid-1970s to slower, irregular growth since the year 2000. This slowdown in temperature increase, colloquially called the "global warming hiatus," was occurring when the growth in atmospheric greenhouse gas concentrations was, if anything, accelerating. The hiatus in temperature increase was used as an argument against action on global warming. Denialists argued that you cannot believe climate models, and skeptics asked why hurry to reduce fossil fuel burning when the climate is no longer warming? We noted, however, that other impacts expected of climate change were taking place as though nothing had happened. In particular, the Arctic continued to warm faster than the earth as a whole; sea ice, land ice, and snow cover were retreating as before; and there was no evidence that extreme weather events decreased in number and intensity during the hiatus.

The turmoil in the popular literature could not be disregarded and various technical explanations for the hiatus had been ventured; even the 2013 Intergovernmental Panel on Climate Change felt constrained to weigh in. In my APS talk, I went through several discarded explanations of the hiatus and speculated myself that atmospheric circulation driven by the warming of the Arctic Ocean was cooling the tropical Pacific Ocean. The cooler ocean in the tropics reduced the measured global average temperature in ways not captured by existing climate models. In short, the hiatus gave no good reason to doubt that humans were changing the climate, but good reason to question how humans were modeling the changes in climate.

By the time the "Hiatus" talk was published in the APS *Proceedings,*[113] David Victor and I had already published our controversial "Ditch" paper in *Nature*. It all started when I invited David to lunch at the Estancia Hotel up the hill from Scripps. I led off our lunch conversation by saying the whole hiatus issue bothered me: even though the measured global temperature was not increasing as rapidly as before, other adverse impacts of climate change were still increasing. It was disingenuous to claim that action on climate change was no longer needed, and dangerous if action were delayed by such a claim. David observed that when the idea of limiting the growth of global temperature to two degrees centigrade relative to the pre-industrial era was first brought up, it was as a device to focus

[113] C. F. Kennel, "'Hiatus' in Global Warming: Paradox and Complexity in Climate Science," *Proceedings of the American Philosophical Society* (2015): 367-408.

the minds of busy negotiators. There had been little scientific work done on how the global temperature measured by scientists relates to the risks of climate change that diplomats work to avoid. There had not been enough studies to justify picking one temperature goal over another. Without a lot of work, global temperature would not tell negotiators what risks their countries are exposed to, what position to take in climate negotiations, and what temperature increase the world should work to avoid.

We decided to submit an opinion piece to *Nature*. To get started, I drafted a *precis* of the APS talk that David redrafted in policy-pungent terms. Journal editors play a more active role with opinion pieces than with peer-reviewed research, and our *Nature* editors certainly did so. They went so far as to suggest a provocative title, "Ditch the 2° C Warming Goal." David and I debated a more anodyne title—and Ram urged us not to adopt their title at all—but in the end we stayed with *Nature*'s. Our article was also accompanied by an even more provocative illustration. Some unhappy critics later accused us of claiming the intent of the Paris negotiations could not be achieved. We did not mean that the negotiations had an unworkable goal, but that climate diplomacy is not well-served by science if success is defined solely by a temperature goal. Other complex multilateral negotiations, like those in global trade, are supported by baskets of indicators which, by facilitating asymmetric perceptions of risk, enhance flexibility in negotiation.

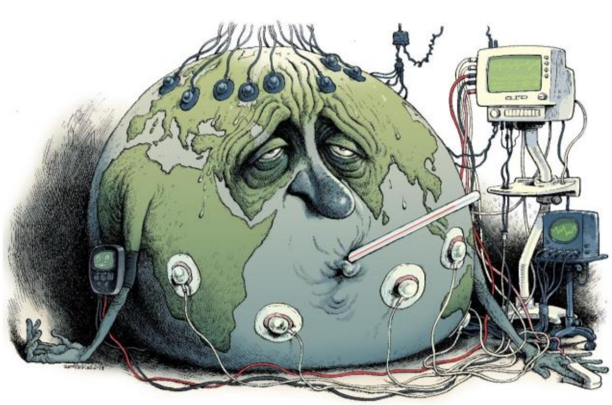

**Figure 32-1: This illustration by David Parkins of a soft, bloated, overfed Earth with a headache and fever accompanied David Victor's and my opinion article in the October 1, 2014, special issue of *Nature*.**

Our *Nature* article was caught up in the whirlwind of publicity preceding the December 2015 Paris Conference on Climate. An article in the *Wall Street Journal* construed our paper as among those questioning the feasibility of the Paris Conference's goal. That was not our intent; we thought climate negotiators should be provided with a basket of indicators on how the severity of climate change depends on the temperature target. In the event, the 2015 Paris Agreement responded to the demands of small island countries (and the developing world) by declaring desirable the more ambitious temperature target of 1.5° C. This inspired a comprehensive assessment of the impacts expected in a 1.5° C world. Comparison of the 1.5° and 2° assessments does provide a roundabout way to accomplish the outcome we had in mind, but not a compact basket of indicators whose use is common practice in other complex negotiations.

David Victor attended the Paris conference while I stayed home in San Diego, but we were in frequent contact as I pumped him for news that I could relay to a San Diego civic group at a luncheon speech. During one of my internet exchanges with David, he reported he had met my colleague from my NASA days, Stephen Briggs. Their conversation initiated a train of events that enabled Stephen to move our opinion closer to action. Stephen was the Chair of the Global Climate Observing System (GCOS), an international planning group that harmonizes national observing programs and defines the requirements for monitoring climate change.[114] The three of us wrote an article in *Nature Climate Change* arguing that a basket of risk indicators should be provided to climate negotiators.[115] I gave a talk at a GCOS meeting in Amsterdam on what we called planetary vital signs; Stephen, through GCOS, did the hard work of developing a community list of indicators and getting it adopted by the World Meteorological Organisation.

Diplomats are not the only ones who need to judge the risks posed by climate change. Stephen Briggs believed it would soon be possible to create risk assessments for transport, industry, and commerce. Stephen and I had participated in discussions with Emily Shuckburgh, then of the British Antarctic Survey, about using big data methods to link the risk to human enterprises to changes in

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[114] GCOS is cosponsored by the World Meteorological Organization, the Intergovernmental Oceanographic Commission of the United Nations Educational, Scientific, and Cultural Organization, the United Nations Environment Programme, and the International Science Council.

[115] S. Briggs, C. F. Kennel, and D. G. Victor, "Planetary Vital Signs," *Nature Climate Change* 5, no. 11 (2015): 969-70.

climate. David, Stephen, and I included this suggestion in a 2016 article in *Science*[116]. Later, Stephen and I attended a private meeting with reinsurance industry executives that was arranged by my former NASA boss, Dan Goldin. In Toronto, Stephen and I conveyed what we thought could be done to estimate the long-term risks to industries and businesses, given the state of climate modeling and big data technology. It was a respectful conversation, but Dan later explained that the reinsurance executives did not want to risk their working relationships with the companies that did their short-term risk estimations, so little came from this meeting. Had the Toronto meeting taken place after 2019, its outcome might have been different, for that was the year the financial industry awakened to climate change.

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There is more to climate change than is dreamt of in any one discipline. It has at least four dimensions: scientific, environmental, economic, and social. These areas are usually taught in different departments, and rare is the student whose curriculum includes all four dimensions. Yet students trained in one are likely to encounter all four as their careers unfold. As things stand today, they will lack familiarity with the foundational concepts and terminology of three of the four when they get their PhDs. It is unreasonable to require them to get four PhDs, but not unreasonable to hope they learn enough to be unafraid to study further if the need arises. This basic idea motivated Ram, David Victor, Naomi Oreskes, and me to originate a one-quarter course, *Climate Change in Four Dimensions*, in which we each gave five or six ninety-minute lectures. The only requirement for admission was graduate status in any department. Our departments would not grant teaching credit, so we volunteered to teach it as an overload, though they did list it as an optional special topics course. Though the course was not required for any degree, a respectable number of students signed up.

I do not know who learned more, the professors or the students, but enthusiasm was not exhausted after our first attempt to teach *Climate Change in Four Dimensions*. The second time, we

[116] C. F. Kennel, S. Briggs, and D. G. Victor, "Making Climate Science More Relevant," *Science* 354, no. 6311 (2016): 421-22.

experimented with making it UCSD's first massive open online course (MOOC); it was professionally managed by UCSD Extension and distributed by *Coursera*. SIO assigned it a classroom for two lecture periods in a row, the first devoted to setting up the audiovisual recording equipment. In the meantime, Naomi Oreskes had left for Harvard, and our SIO colleague, Richard Somerville, joined the team. Kim McIntyre, from the Sustainability Solutions Institute, skillfully managed the interface with students from many different time zones, and neighborhood friends in my time zone told me that they had listened in. The course was a success for UCSD's international reputation, but its fees did not cover UCSD Extension's expenses, and it was quietly dropped. I reused the work put into the MOOC in 2014 when I gave the first lectures in what became the Christ's College series on climate change and sustainability. In 2016, the financial fiasco forgotten, the University of California honored me with a Dickson Professorship for achievement by an emeritus faculty member, citing among other things my work as organizer of the MOOC.[117]

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I remained uneasy about the speculative argument about the hiatus in global warming that I presented to the American Philosophical Society in 2014, but I did not have the research tools to develop the argument further. It was not clear that the modeling community did either, since climate models had been unable to explain certain observational trends during the hiatus, including slower global temperature increase, faster losses of Arctic Sea Ice, stronger central Pacific trade winds, emergence of central Pacific *El Niños*, and cooler sea surface in the equatorial central and eastern Pacific. The magnitude of Arctic sea ice loss and the strength of the Central Pacific trade winds were unprecedented in the observational record; I wondered whether the two were related. It was a naïve idea that sticks in the mind even when there is little evidence for it.

[117] Though it was called a professorship, the Dickson is basically an award for productivity as an emeritus faculty member. One (or more) is awarded each year at every campus of the University of California.

**Figure 32-2: First-year Arctic Sea Ice observed near the end of summer, September 9, 2009. "Plumes" of ocean-warmed moist air rise at end of summer when the atmosphere above the ocean cools. This image is taken from a PowerPoint presentation by C. Kennel and E. Yulaeva, "Arctic Sea Ice, Atmospheric Plumes, and ENSO," given at the Birch Aquarium at Scripps on April 18, 2017.**

Numerical climate models having apparently failed, Elena Yulaeva and I tried data analysis to tease out possible relationships between Arctic warming, sea ice loss, and Pacific Ocean cooling. I was ignorant of the statistical methods atmospheric scientists use, but Elena came to my rescue. Her PhD supervisor had been one of the nation's premier atmospheric scientists, John Wallace of the University of Washington; she taught me how empirical climate analyses were done, and she did and redid them until a picture emerged that we could understand. That picture was an extension of one associated in my mind with Jennifer Francis[118], now of the Woods Hole Research Center, though a wide variety of others contributed.[119]

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118 Francis, J. and Skific, N., 2015. Evidence linking rapid Arctic warming to mid-latitude weather patterns. *Philosophical Transactions of the Royal Society A: Mathematical, Physical and Engineering Sciences*, *373*(2045), p.20140170

119 Cohen, J., Screen, J.A., Furtado, J.C., Barlow, M., Whittleston, D., Coumou, D., Francis, J., Dethloff, K., Entekhabi, D., Overland, J. and Jones, J., 2014. Recent

The warming of the Arctic Ocean due to loss of sea ice cover drives convection into the upper troposphere. The rising air accumulates there and forces Arctic air to flow equatorward. The popular media call the resulting intrusions of Arctic air into the mid-latitudes "the polar vortex"; Elena and I asked whether polar vortex dynamics also strengthens the trade winds over the tropical Pacific Ocean. The trade winds drive the circulation of ocean heat, and Arctic-forced strengthening of the trade winds could cool the equatorial ocean, thereby driving down the globally averaged surface temperature. Others will have to decide whether the *El Niño* cycle is modified. Our 2020 paper[120] was my last attempt at rigorous science.

Arctic amplification and extreme mid-latitude weather. *Nature geoscience*, *7*(9), pp.627-637.

[120] C. F. Kennel and E. Yulaeva, "Influence of Arctic Sea-Ice Variability on Pacific Trade Winds," *Proceedings of the National Academy of Sciences* 117, no. 6 (2020): 2824-34. As or June 2023, this article has received 16 references in per-reviewed journals according to *Google Scholar*.

# 33

# Sustainability in Venice

When towards the end of the Roman Empire, the inhabitants of the towns and fields in northeast Italy eluded their Germanic and Hun tormentors by settling in the vast marshland now known as the Venice Lagoon, they relied on nature to be their provider and the sea to be their protector. Their descendants deforested the neighboring shores and islands to build an entire city above the water on wood pilings, pilings that continue to serve this purpose today. By the early Middle Ages, Venice was a trading and financial center that extended its soft and hard power to a network of satellite cities in the eastern Mediterranean. Its leading city, Venice, had no walls, so the Lagoon was the principal element of its military security. The Venetians—their homes, markets, and churches all perched a few centimeters above the water—had learned to respect the vagaries of sea level, which varied with the regularity of the tides and the unpredictability of storm surges.

The Venetian Lagoon, the largest wetland in Europe, is protected from the Adriatic Sea by an almost continuous barrier island. The Lagoon exchanges water with the Adriatic through only three passages, each approximately 100 meters wide: the Lido, Malamocco, and Chioggia, at the northeastern, center, and southwestern ends of the barrier island, respectively. I do not know when Venice first conceived the ambition to control sea level in the Lagoon, but in 1501 the Magistrato alle Acque, the Venice Water Authority, was constituted and given powers that would make modern water management districts jealous. The Water Authority Director was second in power only to the Doge. The Magistrato had the authority to dam and/or divert the flow of the streams and rivers entering the Lagoon, and used these powers to exert some control over the seasonal flows of water into the Lagoon from the land. The goal was to keep the water in the Lagoon deep enough that troops could not march in and shallow enough that war ships could not sail in. Still, Venice had no way to control the Lagoon's exchanges of water with the Adriatic, and the historical city was subject to flooding when a storm surge caused by a southeasterly wind, the *Scirocco*, pushed water up against the Lagoon's barrier island. Adriatic high water would then pour into the Lagoon through the three passages, flooding the city. The *Scirocco* season is autumn and there have been many windstorms that flooded the city in November when the astronomical tide is highest. Its inhabitants have had to endure high water events (*acqua alta*, plural *acque alte*) in November for centuries.


**Figure 33-1: An old building sign in the headquarters of the Magistrato Alle Acque, Venice, Italy, founded 1501. A rough translation is "Regional Supervisor of Public Works." Photo taken by Ellen Lehman, July 3, 2012.**

The extreme high-water event of 1966 was a harbinger of events to come. The tremendous publicity surrounding the flooding alerted the international world to the artistic treasures put at risk by high water events. The Arno River flooded the inland city of Florence at the same time, doubling the media exposure. An alerted Venice soon became aware of the increased frequency of dangerous high-water events (but not yet sea level rise and climate change) and asked with new urgency an old question: what can be done to protect the city from *acque alte*?

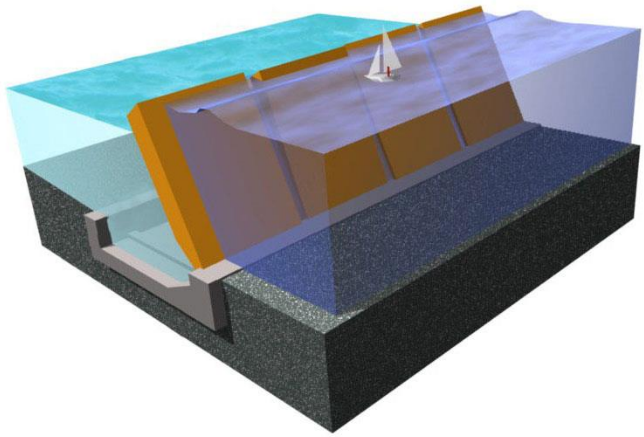

**Figure 33-2: A schematic of one of the Venice gates. Do not imagine for one minute that Venice's resident population uniformly welcomed the project. Aesthetes saw it as a clumsy technocratic intrusion on Venice's cultural heritage; one London art critic pleaded with scientists to stop meddling and let the city decline elegantly as it had for the past 300 years. The well-known author, Donna Leon, could not stop complaining about the gates in her novels.**

No scientist in modern times has done more to answer this question than Scripps' Walter Munk. In 1970, Walter was among the four organizers of the first international scientific conference devoted to Venice's high-water dilemma. Walter worked out statistical methods to estimate the frequency and amplitude of the storm surges in the Adriatic that cause Venice's high-water events. In 1973, Munk conceived the basic outline of an engineering solution. The primary constraint beyond flood control was aesthetics; the engineering works must be out of sight except at times when flooding threatened. In 1975, Munk spent a sabbatical studying changes in water circulation and wave actions resulting from the opening and closing of hypothetical gates at the three entrances to the Lagoon, gates that when raised could keep storm surges at bay. Each would be divided into hollow upper and lower chambers; when both chambers are filled with water, the gates rest horizontally on large hinges anchored to the lagoon floor; when a high-water event is imminent, a capacitor bank discharges its energy into pumps that replace the water in the upper chamber with air in about thirty minutes; the buoyancy of the upper chamber rotates the gate on its hinge from horizontal to almost vertical. When all three gates are fully raised, they can hold off the Adriatic up to 110 cm above local mean sea level.

Constructing the gates was within twentieth-century engineering capabilities, and in 1987 the Magistrato appointed a consortium of four marine engineering companies, the Consorzio Venezia Nuova (CVN, Consortium for a New Venice), to build gates that would normally rest horizontally on the ocean floor, to be raised in anticipation of high-water events. Construction started at all three entrances in 2003.

**Figure 33-3: Thetis Corporation headquarters in the morning hours. Thetis was responsible for the construction the MOSE gates at the time UCSD's Venice Sustainability Advisory Panels met in this building. Photograph by Ellen Lehman on September 15, 2011.**

The Venice Gates Project (called MOSE, "Modulo Sperimentale Elettromeccanico," or "Experimental Electromechanical Module") was perhaps the largest environmental project in Europe with an estimated cost of six to seven billion euros. An industrial plant constructed in the Lagoon worked twenty-four hours a day, seven day per week, to produce the cement for the gates. Millions of cubic meters of sediments were dredged to make way for the structures that held the hinges anchoring the gates to the Lagoon floor. This is where Scripps came into the story. Venice is a UN World

Heritage Site, which meant that the sediments could not be removed from the Lagoon. Instead, the sediments would be used to build new intertidal flats, like the many apparently naturally occurring mud flats, tidal shallows, and salt marshes that currently occupy about eighty percent of the Lagoon's area. However, the bottom sediments had accumulated over centuries, and there were fears that toxic chemicals deposited near Porto Marghera during the operation of chemical plants from the late nineteenth to the mid-twentieth centuries would be released by the dredging. There were even worries that pathogens from the Middle Ages to which people were no longer immune might be released back into the water.

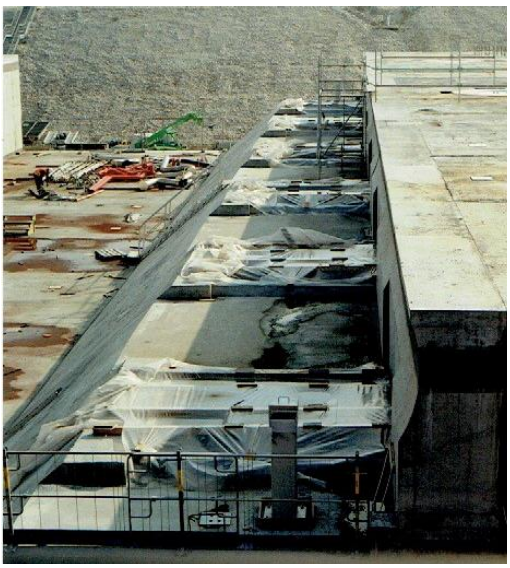

**Figure 33-4: The Venice Gates under construction. In April 2011, the Thetis team escorted the Venice Sustainability Advisory Panel on a tour of the construction at the three entrances through the barrier island protecting the Venice Lagoon from the Adriatic Sea. Shown here are what eventually will raise the gates. A cement plant had been in 24-hour per day operation since the project began to produce enough cement to build the gates. Photograph by Ellen Lehman.**

About a year after construction began, the project asked Scripps for scientific advice. The CVN had already engaged an MIT group to consult on the hard engineering of the gates, and it now needed SIO's advice on the new "soft" subject of ecological engineering.

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One day in 2004, I had a free moment to gaze at the ocean through the floor-to-ceiling windows in the Scripps Director's Office. The waves never stop rolling on the beach and I never stopped being calmed by the sound of the surf. My reverie—perhaps it was that all directors should be so lucky to have an office on a sea wall—was interrupted by what turned out to be a welcome visitor, Ozzie Holm-Hansen, a faculty member. Though naturally gregarious, Ozzie did not visit the Director unless he thought it important. Would Scripps be interested, he asked, in a million-euro research grant? Good Director that I was, I replied without hesitation: a million euros? You are speaking my language, but what language do I have to speak? Ozzie asked whether I would talk (in English) with a couple of representatives from the Italian consortium building the Venice gates. Warm memories of my postdoctoral year at the International Centre for Theoretical Physics in nearby Trieste flooded my mind; I did not tell Ozzie that I would have done the project for nothing to find a serious reason to spend time in Italy.

In that first interview I asked the gentlemen from the CVN why they wanted SIO's advice. No other ocean research institution, they said, had the combination of expertise in biological and chemical toxicology and circulation modeling that they needed. They also said that Walter Munk was the one who originated the basic design of their project. I was disposed to find their answers credible, and without further thought put Scripps' skillful Assistant Director for International Relations, Lisa Shaffer, to work on negotiating a collaboration between the University of California and a foreign private enterprise. (My impression was that it was more challenging for Lisa to deal with University of California bureaucracy than Italy's.) It turned out that Venice's answer to my question "Why Scripps?" was the truth, but not the whole truth. Rosangela Mazzacurati, the Italian-American wife of the CVN President Giovanni Mazzacurati, had a home in La Jolla that they visited from time to time.[121]

Scripps' first task was to assess the chemical and biological toxicity of the sediments used in building new salt marshes. While our senior researchers were intrigued by the opportunity to work in the Venice Lagoon, none wanted the intercultural and linguistic challenges of representing the Scripps team to the CVN, so I asked a linguistically capable junior researcher, Dmitri Deheyn, if he would act as coordinator. I persuaded the senior members of the team to accept his role, and the Scripps team got to work. Fortunately, the

[121] Ellen and I became close friends, particularly after they settled permanently in La Jolla after Gianni had to resign his position, of which more later.

worries about residual toxicity proved not as serious as feared and the dredging and the project could continue. This happy conclusion meant that our involvement with the Venice Gates Project would continue.

I boasted to the Scripps Advisory Council of our Venice project; it was the kind of thing that board members find glamorous. Ultimately the Council and I decided to organize a trip to Venice, where the MOSE project arranged helicopter and boat tours of the three construction sites, lectures, and, of course, some wonderful dinners. Italians know how to do such things well. Salah Hassanein, a member of the Council, arranged for the Council also to visit London and Christ's College, Cambridge, where his friend (soon to be my friend) Bill Fitzgerald was a Fellow; that visit to Cambridge was precursor to the most important new institutional relationship I would develop during my retirement. I was careful not to use UCSD or project funds for the Council trip, and every participant, including Ellen and me, paid their personal expenses and some extra for staff support. Even so, my new Chancellor, Marye Anne Fox, frequently objected to the optics of a fat-cat spree to a famous tourist destination associated with her public university.


**Figure 33-5: Working with the MOSE project had undeniable attractions. One of the most memorable was joining the group invited to watch the Venice Regatta from the balcony of the Mayor's apartment. Photo taken by Ellen Lehman in the morning of September 8, 2008.**

SIO's initial work had whetted the MOSE project's appetite for more, but I had been resigned as Scripps Director in September 2006 and had to explain that I would no longer be SIO Director for the next phase of the work. I suggested the project might wish to work with my successor, Tony Haymet; the project replied that it remained comfortable with me. I had negotiated with Chancellor Fox a post-directorship mandate to explore a new environment and sustainability initiative on behalf of UCSD. Paul Linden and I ended up co-founders of the UCSD Environment and Sustainability Initiative, a mighty sounding name for a threadbare operation now defunct. Paul and I were co-principal investigators on UCSD's subsequent grants with the Venice Gates Project. Our responsibility was to convene, oversee, and contribute to the technical work of international advisory panels on sustainability issues: the Venice Sustainability Advisory Panel (2007-09), The Venice Marsh Restoration Panel (2008-11), and the Venice Lagoon Sustainability Management Panel (2010-14). Naturally, these panels drew on Scripps and UCSD talent, but their international members added to their capabilities and perspective.

Saving Venice, a UN World Heritage Site, is a global responsibility, and the people we contacted responded accordingly. The various advisory panels took me to Venice several times a year, and Ellen occasionally, until 2014, when human frailty—a financial scandal—scuttled the group managing the gates project. This delayed project completion by another seven years. On December 2, 2021, the NASA Earth Observatory released images from space of the gates in successful operation during the high-water season.[122]

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An interesting issue came up once SIO's team had completed its assessment of the toxicity of the dredged sediments. Since the sediments could not be exported from the Lagoon, a World Heritage Site, the MOSE project planned to use the sediments to build new tidal flats. Would the artificial tidal flats differ from the "natural" ones that had been in the Lagoon longer than people could

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[122] A. Voiland, "A Long-Awaited System of Movable Floodgates is Starting to Protect the City from the Highest Tides," NASA Earth Observatory Image of the Day, December 2, 2021, https://earthobservatory.nasa.gov/images/149151/venice-holds-back-the-adriatic-sea.

remember? The MOSE project was sensitive to accusations of hubris for thinking it could replace nature. In the event, after the artificial tidal flats had been in place for a few years, UCSD/SSI's Marsh Restoration Panel found that the ecologies of the new and old flats could not be distinguished by the ecological science of the day. The interesting question is why anybody should have been surprised. There is no pristine state of nature in the Venetian Lagoon, whose ecology has been human-dominated for more than a millennium. Probably everything in the Lagoon is a residue of past human activities. As time passed, I came to see Venice and its Lagoon as a miniature forerunner of earth in the Anthropocene Era.

The Venice Conference (September 12-15, 2011) was the high point of the Sustainability Solutions Institute's work with Venice. The *Consorzio Venezia Nuova*, the MOSE project, and the *Magistrato alle Acque*, wishing to benchmark their approach relative to similar efforts around the world, asked the Sustainability Solutions Institute to convene an international workshop to compare best practices. There had been conferences in which scientists and engineers discussed sea level management, and occasions when management leaders shared their experiences among themselves, but rarely had scientists, engineers, and managers met as one group to assess an approach to adaptation to an expected impact of climate change. Our meeting convened marine scientists, engineers, and management leaders from Australia, Bermuda, Canada, Egypt, Italy, Finland, Germany, Japan, the Netherlands, Thailand, the United States, and the United Kingdom. This group reviewed the technical, political, and social challenges that urban coastal regions like the ones they were

**Figure 33-6: The Venice Conference: improving the capacity to adapt to climate change in urban coastal regions. This conference was the high-water mark of SIO/UCSD's work with the Venice Gates Project. Photo by Ellen Lehman.**

responsible for could expect from climate change. The group perceived that a broadly similar methodological philosophy had emerged as regions around the world were preparing on their own for the sea level rise all knew was coming. Venice's leadership, principally the and *Magistrato,* heard that their own preparatory efforts met international standards of best practice.

The personal high point of my involvement with Venice was the moonlit reception for workshop participants and guests that Ellen arranged on the roof of the Venice Guggenheim Museum. We had known Richard Armstrong, the Director of the Guggenheim Museums, from his Pittsburgh and La Jolla days, and he arranged for the roof of the Venice Guggenheim to be available for the reception. Then Ellen made the rest happen: hors d'oeuvres, drinks, dinner, music, and moonlight dancing on the roof.[123] The moon was bright, the air soft, and the sounds from the *vaporetti* on the water below mixed with the music above and echoed from the buildings across the canals. It was magical.

**Figure 33-7: Walter Munk in the limelight with Charlie Kennel (center) and Giovanni Cecconi (right) in the shadows, at the event marking the conclusion of the Venice Conference on the roof of the Venice Guggenheim Museum, September 15, 2011. Photo by Ellen Lehman.**

[123] We were careful to pay for the expenses personally, so as not to be accused of spending university-administered funds on a showy event.

The Venice Gates were not started because of climate change, but climate change became part of the conflicted debate about them. How long would the seven-billion-euro gates protect the venerable city against storm surges in a future of sea level rise? This question was in the back of many minds. In an unpublished 2011 analysis, our team, led by Scripps' Dan Cayan and Sam Iacobellis, selected climate models from the 2007 assessment of the Intergovernmental Panel on Climate Change that performed particularly well in the Adriatic, and used those models to forecast the change in storm surge frequency under climate change conditions. They then combined these results with astronomical tide data under various sea level rise scenarios to estimate the annual frequency of gate closures in various climate change scenarios.

Their basic result was that the gates would protect Venice until global sea level rise reaches 80 cm relative to the preindustrial era. When that happens, nearly every day of Venice's year will resemble a November *acqua alta* event, even with the gates raised. The city will be effectively uninhabitable, although its half-flooded buildings and monuments will remain visible above the waterline (as they do during today's high-water events), and the dying city might retain a ghoulish attraction as a tourist destination. When would global sea level rise reach 80 cm? Our answer, then as now, was that it depends on the rate of loss of freshwater ice from Antarctica and Greenland; it might be 2100, but it could also be as soon as 2050. The losses of Antarctic and Greenland ice were accelerating, but we could not predict their future rates; should global sea level rise reach 80 cm in 2050, the gates might not last as long as the time it took to build them.

Our panel recommended vigilance at the press conference; sea level rise and the ecological health of the Lagoon should be assessed regularly. Considering how long it took to plan, organize, and construct the gates, early warning is crucial. Sea level will rise, and storminess increase, decade upon decade; given that it took the MOSE project twelve years from conception to approval, 16 years from approval to the beginning of construction, and another twenty years to complete, visioning what to do next should start immediately.

An ominous milestone will have been reached when typical storm surges rise above the lowest elevation point on the barrier islands protecting the Lagoon, for then the gates fail in conception. The system of barrages in the Thames Estuary has a multi-layered plan of defense against sea level rise, but I did not hear what Venice will do when the gates fail—other than evacuate the population, as it is slowing doing. Even before global sea level rise reaches 80 cm, climate change could have a debilitating impact on the city's habitability and attraction to tourists. In 2022, climate change produced a surprise not dreamt of in 2011's philosophy: the Lagoon was flooded, not by a storm surge from the Adriatic in the south but by the runoff from torrential rains in the hills to the north.[124]

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Ellen and I flew to Venice in summer 2014 for what we expected to be a team meeting. While we were in the airplane over the Atlantic, teams of financial police were fanning out all over Italy to impound the records of the *Consorzio Venezia Nuova;* work on the MOSE project stopped instantly. We had nothing to do but go to the Venice *Bienniale* and speculate fruitlessly on why the work was stopped. We still do not know the real story, but we do know that the President of the CVN, Giovanni Mazzacurati, who I saw as a great visionary, retired in disgrace to La Jolla, his health broken. We continued to see him and Rosangela in La Jolla at a time when many Italian friends avoided contact; Rosangela's daughter, Nina, moved to the US to be near them and has started a new life as an importer of high-quality Italian wines. Gianni died in 2019, without vindication.

[124] On November 26, 2022, The Washington Post published an article by Chico Harlan and Stefano Pitrelli, with photography by Carolyn Van Houten and a video of the Venice gates in operation, that relates a contrary view of the sea level rise issue dominating the 2011 Venice Conference. C. Harlan and S. Pitrelli, "An Engineering Marvel Just Saved Venice From a Flood. What About When Seas Rise?" *The Washington Post*, November 26, 2022, https://www.washingtonpost.com/climate-solutions/2022/11/26/venice-floods-mose-barrier-climate.

**Figure 33-8: Giovanni Mazzacurati (left) and Paolo Rosa Salva (right) in happier days. Paulo is married to Cristina Nasci, who was in charge of interfacing with UCSD's Venice Sustainability Advisory Panels on behalf of the Thetis Corporation, and became a lifelong friend. Photograph taken by Ellen Lehman on June 10, 2009, from the balcony of the Mazzacurati apartment overlooking a canal at dusk.**

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Work had started on the gates in 2003 and by 2013 was eighty-five percent complete. Then the financial scandal brought the work to a halt. The MOSE project was eventually reorganized and restarted, but it missed its revised 2018 completion deadline. In 2020, the first set of gates was operated successfully on a trial basis. Had all the gates been operational in 2018, they would have protected the city from the high-water event of 2019, which rivaled the 1966 event in sea level rise but not in impact on residents; the population living in the city had dropped from about 275,000 in 1966 to 53,000 in 2019, and so fewer permanent residents were threatened.

The NASA Earth Observatory released the following statement, accompanied by satellite images of the gates in operation, after a high-water event in 2021:

> On the afternoon of November 3, 2021, the flood gates were raised as a storm brewed in the Adriatic Sea. At the time, forecasters warned that

water levels might rise 140 centimeters above normal when high tide peaked and strong sirocco winds battered the Venetian coast. Water at that level is enough to flood 60 percent of the city, including the iconic St. Mark's Square, the lowest part of the city.

While some of the barrier gates at the Lido inlet were kept closed for the duration of the high-water event, the gates at Malamocco and Chioggia inlets were retracted during low tide to let water out of the lagoon. The [...] Multispectral Instrument (MSI) on Sentinel-2, shows sediment stirred into a zigzag pattern as the barrier gates at the Malamocco inlet retracted on November 4, 2021. Two days later, the Operational Land Imager (OLI) on Landsat 8 captured an image [...] showing the same inlet with the barrier gates fully activated and standing above the water surface. At the time of the Landsat overpass, strong winds (61 kilometers per hour/38 miles per hour) blew from the east, stirring up sediment on both sides of the barriers.

Activating the flood gates proved successful in this case. While high tide water levels rose above 130 centimeters in the Adriatic Sea, they reached just 83 centimeters in Venice, enough to prevent major flooding. MOSE has been used several times in recent years as engineers test it and work toward making it fully operational by 2022. The floodgates were activated five times in 2021 and 20 times the previous winter. In 2019, before the system was available for use, more than 25 high-water events swamped Venice, including a November flood that proved to be the second worst on record.[125]

[125] Excerpted from "Venice Holds Back the Adriatic Sea," NASA Earth Observatory, accessed November 2, 2022, https://earthobservatory.nasa.gov/images/149151/venice-holds-back-the-adriatic-sea.

**Figure 33-9**: **Satellite imagery of the Venice floodgates open, on November 4, 2021 (left), and closed, on November 6, 2021 (right).**

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I view our work with the MOSE gates project as a pioneer Knowledge-Action Network, in which international scientists work personally with local leaders to build resilience to climate change.[126] Venice's example teaches how important it is for us in the scientific world to understand the decision atmosphere in which local leaders live. In Venice's case, the local population was deeply divided by the intrusion of foreign technocratic ideas (and a disruptive project) into a city whose self-image had been shaped by art and literary culture since the Renaissance. Moreover, since Venice became part of Italy, it has been ruled by a byzantine governmental bureaucracy that complicates decisions and encourages informal workarounds and deal making. That said, Venice is blessed with a high level of talent in marine engineering and ecology, and intellectual comprehension of the technical issues is not a serious challenge.

The fate of Venice and its cultural treasures in the century to come is, however, in the hands of the international community;

[126] I had introduced the Knowledge Action Network notion at a meeting of the Science and Technology in Society Forum in Kyoto in 2009.

Venice is simply too small to defend itself from the sea level rise of the future. The ongoing flight of the resident population to the Italian mainland means that the next time something big needs to be done, Venice will lack the economic and political influence within Italy to protect itself.

Venice's centuries-long battle with the sea provides an object lesson for the rest of the world. The global sea level rise already set in motion will endure for centuries even if society succeeds in reducing carbon dioxide emissions. Every citizen of every coastal city will feel insecurities that Venetians have felt for the past thousand years. Venice's complicated experience with its gates project is an object lesson of another kind. It is not for lack of talented marine engineers, experienced ocean scientists, qualified coastal ecologists, local industrial capacity, or adequate financial support that the gates may well end up having been completed too late to protect Venice for long enough to build its next line of defense against the sea. Human failings—unceasing political controversy, complicated decision-making, financial scandal—weighted down a valiant effort to defeat a relentless opponent. The story of the Venice Gates, despite (or perhaps because of) the travails, will find a hallowed afterlife in the world to come. Like the Thames' barrages, it is one of the first investments in major infrastructure for adaptation to the most enduring impact of climate change.

# 34

# The California Council on Science and Technology

When I arrived at UCLA in 1967, California's population was below the State of New York's, but by 1970 California had taken over as the nation's most populous state. According to 2020 census data, California's population exceeds thirty-nine million, about thirty-five percent larger than number-two Texas' twenty-nine million. California's 2012 gross domestic product, $3.37 trillion, would rank fifth in the world were it an independent country. No other American state has anything quite like Silicon Valley or Hollywood. Silicon Valley is home to some of the world's largest corporations by market valuation: Apple, Alphabet, Intel, Facebook, and Twitter. Companies of their scale are considered to have the weight of sovereign nations in international regulatory circles. California's equable weather attracted the movie industry at the turn of the twentieth century and Hollywood became the home of moving picture studios with global reach. Only native tribes have been in California for a long time; everyone else comes from families that immigrated there, most after the Gold Rush of 1849, and many within living memory. The dynamic mix of California's many cultures of origin underpins Hollywood's appeal to cultures around the world.

The plasticity of its culture made California receptive to new technologies. Less visible, but crucial to the success of the moving picture and television industries, are their creative services companies in the Los Angeles Basin. California's creative atmosphere also incubated aerospace and defense industries (Lockheed, Northrup, Hughes, Douglas, TRW, and the Aerospace Corporation, among others) that grew to enormous scale during World War II and the Cold War. Los Angeles, perhaps the first major city built largely in the automobile era, experienced urban sprawl and traffic congestion ahead of other cities. Freeways replaced the squares of older cities elsewhere as enablers of human discourse. The San Diego Freeway connecting the Los Angeles Basin with Orange County and San Diego was the royal road of the Southern California aerospace empire during the Cold War.

Academic culture joined commerce, and California boasts some of the most highly ranked universities in the world, both public and private. For example, in the 2023 Times Higher Education rankings Stanford tied for third, Caltech was sixth, UC Berkeley eighth, UCLA twenty-first, and UCSD thirty-second out of 1,799 universities in 104 countries. In other words, California has about one in six of the top one percent of universities worldwide, and one in seven of the world's top two percent. For reference, the United Kingdom, whose 2020 population was sixty-seven million compared to California's thirty-nine million, had one in six of the world's top one percent universities, and one in six of the top two percent, according to the Times Higher Education rankings.

Each of the ten campuses of the University of California is similar in size and scope to a flagship campus of a Midwestern US state university. In terms of the number of highly cited authors in peer-reviewed research publications, the University of California San Diego ranks eighth in the world, best in the University of California system. California's other public university system, the California State University, has twenty-three campuses, making it America's largest educational institution of its kind. California's private universities rival the University of California in impact, if not size. Stanford incubated Silicon Valley and Caltech, the California aerospace industry; the University of Southern California is the largest private employer in the City of Los Angeles.

Federal research laboratories and private research institutes settled around California's centers of academic excellence. California has four US Department of Energy National Laboratories, and three NASA flight centers. NASA's vast Jet Propulsion Laboratory in Pasadena is a nine-mile bicycle ride from tiny Caltech, its sponsor. The high technology and medical research companies clustering around California's universities and laboratories are too numerous to mention here, but for two pioneers of the biotechnology industry:

Genentech, founded in 1976, and Amgen, in 1980. Cambridge, Massachusetts, is the only US rival of California's San Francisco Bay Area as a center of biotechnology and biomedical research. Moreover, La Jolla's Salk Institute and Scripps Research Institute are among the best biomedical research organizations anywhere, and San Diego County follows closely behind the Bay Area in medical research.

All in all, I think of California as comparable in size, scope, and international influence to one of the European countries (viz., the United Kingdom, France, Germany, Russia) that were called Great Powers in the World War I era.

California also has the environmental challenges of a sovereign country. California agriculture, the largest in the nation by dollar volume, is critically dependent on water supplies that are under threat from climate change. Los Angeles is hostage to the vagaries of the winter snowfall in the Sierra Nevada. Seventy percent of the water for the Los Angeles Basin is transported hundreds of miles from the Sierra Nevada Mountains in Northern California. Scripps climate scientists predicted that global warming may cause the Sierra's spring snowmelt to disappear by the end of the twenty-first century. Monterey Bay's Pacific Sardine Fishery, once the largest in the world, is a shadow of its former glory. California's gloriously beautiful coast is subject to beach and cliff erosion made worse by sea level rise. Already, cliff erosion has caused traffic suspensions on the rail line that hugs the coast in Del Mar near San Diego.

California's state government has created unusually competent agencies to deal with the State's longest standing environmental issues—air quality and water availability—the California Air Resources Board (CARB) and the Department of Water Resources (DWR), respectively. CARB, established in 1967 by then Governor Ronald Reagan within the state's Environmental Protection Agency (Cal-EPA), became the only US state agency with federal EPA clearance to set its own emission standards. California's air pollution policies set an example for other states and the federal government, which, with some delay, follows California's lead. Automobile manufacturers around the world pay attention to what California does, and it is no surprise that the electric automobile revolution got underway in California ahead of the international auto industry.

CARB found it natural to view greenhouse gas emissions as a climate pollutant. In the early 2000s, the California Energy Commission started to fund grants for research on greenhouse gas emissions reduction, and like an agency of sovereign country, struck up cooperative agreements with a number of foreign agencies. In 2006 California became the first US state to enact climate change legislation. Assembly Bill 32 set an emissions reduction goal that matched contemporary international standards and mandated assessments every third year of the impacts of climate change on California's water resources, agriculture, ecology, and environment.

Smog had prepared California for climate change. In 2018, I was invited to speak at one of the many ceremonies honoring the fiftieth anniversary of the California Air Resources Board. I congratulated CARB for reducing smog levels dramatically despite the enormous growth of population and traffic over those fifty years.[127] The methodically relentless way CARB had achieved success gave California's Legislature (and many of its constituents) the confidence to pass globally pioneering climate change legislation.

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Rare is the mid-career California scientist who has not served on panels convened by the National Research Council of the National Academies of Sciences, Engineering, and Medicine. For most of the twentieth century, California science and technology prospered without a Californian analog of the US National Research Council. Indeed, California got used to winning competitions for federal research funding by relying on the National Research Council. One of California's areas of presumptive superiority was in earthquake research. No other state had the great San Francisco earthquake of 1906 in its recent past, or the continuously active San Andreas fault on which it occurred. The San Andreas does not give rise to the largest quakes, but its continuous activity makes it a fine field laboratory for seismological research. Caltech founded its seismology research center in 1921, and in the following years the San Andreas fault became the most instrumented earthquake fault in the US. The University of California created its own multi-campus Institute of Geophysics and Planetary Physics in 1946 with a heavy emphasis on seismology. With all that, you can imagine the surprise of California's seismology community when New York State won a federal

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[127] In the 1950s, London and Los Angeles competed to have the worst air pollution in the world, a distinction that neither wanted and both happily relinquished. The UK passed analogous climate legislation two years after California.

competition for a seismology research center. I did not participate in the seismology competition, so I do not know all the reasons for California's loss, but I do know that our seismologists believed they failed to get the attention of California's government, whose active participation was a key element for success.

This discontent incubated the founding of the California Council on Science and Technology (CCST) by an act of the California Legislature in 1988. CCST became the nation's first state science advisory board through the guidance of one person, Susan Hackwood. Susan was its second, not its first, Executive Director, but she was the one who made today's CCST possible. She took a quasi-organization with no permanent headquarters and a floating staff dispersed around the state, and shaped the volunteer efforts of busy scientists into a science advisory body that governors, legislators, and agencies would listen to—and, eventually, pay for.

An electrical engineer from the UK, Susan had been the founding Dean of the College of Engineering at the University of California Riverside, where she retained a professorship at ten precent time during her twenty-three years (1995-2018) as CCST's Executive Director. Susan understood the innovation process, was an innovator herself, and took an entrepreneurial approach to establishing CCST's presence in California policymaking circles. This was easier said than done. California's proud agencies were not in the habit of working with the scientific community.[128] Though small, CCST's administrative costs were not zero; even though CCST panelists serve *pro bono* there were costs of administration, travel, publication, and outreach, and state agencies did not have budget lines for obtaining scientific advice. It was going to take time to persuade the state government that scientific advice was valuable enough to pay for.

Susan Hackwood's first task was to persuade California's technology, agricultural, and environmental interests to weigh in. She took a characteristically entrepreneurial approach. Through her wide acquaintance with scientists, she identified policy issues of interest to California and tuned them up in meetings of the CCST Council. Thus reinforced, she sought private funding for studies

[128] When the California Energy Commission (CEC) started its program of research grants on climate change, CCST took it upon itself to advise the energy commission on how to conduct a peer review process, since it became clear that the CEC did not know how to fund external research projects.

whose expert participants she recruited personally. Study results were presented at subsequent Council meetings to which appropriate representatives from state and local agencies and sponsors were invited. There must have been times that Susan was overcome with discouragement, yet never once did I see her lose her constructive enthusiasm. And so, year-by-year, the California Council on Science and Technology almost imperceptibly emerged from obscurity.

**Figure 34-1: The California Council on Science and Technology (CCST). A 2010 photo of CCST Board Chair Charles Kennel (left), California Senator Dianne Feinstein (center), and CCST Executive Director, Susan Hackwood (right).**

The second-most important individual in bringing CCST to the verge of maturity was Karl Pister (1926-2022), former Chancellor of UC Santa Cruz and long-time Chair of the CCST Board. The Board, as stipulated in the organic legislation that founded CCST, comprises representatives appointed by the University of California, the California State University, the California Community College System, Stanford, Caltech, and the University of Southern California, who pay annual dues that provide CCST core support. The scientists these institutions appoint to CCST's Board rival in prominence those on senior panels of the National Research Council. In 2005, six California-based federally funded research laboratories were added to the Board as affiliated members: the Lawrence Berkeley National Laboratory, the Lawrence Livermore National Laboratory, the Sandia

National Laboratory, the Stanford Linear Accelerator, the NASA Jet Propulsion Laboratory, and the NASA Ames Research Center. Their affiliations add decisively to CCST's ability to sponsor studies with deep technical content, provided funding could be found.

Karl Pister had an unwritten responsibility: to be Susan's sounding board as she sought new opportunities for CCST service and (hopefully) funding. Karl and Susan worked together in this fashion for two decades until Karl expressed the wish to step down. At this point, Susan nominated me as Karl's successor.[129] Because of the way that Karl and Susan had interacted, I inherited a personal direction-setting role for CCST when I took over as Board Chair. Karl left me a passive Board, content to preside over what Susan and Karl were doing, which invariably had admirable intent if not impeccable execution. The old Board's formal responsibilities had been largely fiduciary: hold funds, authorize expenditures, set standards for report reviews, approve review panels, and the like. My CCST Board took a more active stance; I preferred more lively Board meetings and encouraged debates about policies and procedures, which had the indirect effect of promoting more active connections with CCST's sponsoring institutions. Most of the time, the Board's vigorous engagement made my life easier, and on a couple of occasions, harder, but there were few who thought our meetings uninteresting. The CCST charter and Governing Board are beyond all doubt its greatest endowments, but as the years wore on and the range of its activities grew, the fact that CCST had to raise funding through philanthropy for activities that benefit the state became a serious limitation.

Inspired by the legislative fellows program of the American Association for the Advancement of Science, Susan created the CCST fellows program, the single most important thing CCST has done to strengthen the relationships between California's scientific community and the State Legislature. It is Susan's masterpiece. She secured funding from the Gordon and Betty Moore Foundation to place ten postdoctoral scientists in legislative offices for a period of one year. A new group of legislative fellows was appointed yearly. CCST ran a "boot camp" at the beginning of the legislative year, and

[129] I had been a member of the Council in the early 2000s, eventually to chair it; my job as Council Chair was to help Susan set the agenda for and preside over Council meetings. My job as Board Chair was to preside over the formulation of CCST's initiatives and policies.

the fellows met regularly with their CCST mentors during the year. The aim was to create a cadre of politics-aware science policy specialists and science-aware politicians, as well as corporate memory of science policy in the legislative staff. Increasingly, legislators turn to the young scientists in their daily environments when an issue with science content comes up; if an issue lies beyond the expertise of an individual fellow, CCST seeks appropriate experts to pinch-hit.

Many former CCST fellows have gone into state government or into the government relations offices of universities and industries. I noticed a significant improvement in the Legislature's reception of CCST ideas in the second year of the Fellow's program. Just as in Kyoto, when the RACC group developed the notion of Knowledge Action Networks, the importance of person-to-person relationships in conveying science to decision makers was brought home to me.[130]

Susan Hackwood, a former engineering dean, brought a high technology and innovation approach to CCST projects, and I, as a former Scripps Institution of Oceanography Director, brought a climate change, environment, and sustainability orientation in my roles, first as Council Chair and later as Board Chair. CCST undertook studies of California water issues and, starting in 2011, a series of pioneering studies of California energy practices and policies in the age of global climate change. The first in this series made me aware that the optimum path to greenhouse gas emissions reduction was through electrification of as many energy uses as possible, particularly automobile transportation.

CCST's climate and energy studies would not have been possible without the leadership of Jane Long, then Associate Director of the Lawrence Livermore National Laboratory. As CCST's series developed, Jerry Brown, Governor from 1975-83 and 2011-19, who was deeply committed to action on climate change, consulted Jane frequently on energy issues.[131] Towards the end of his term, Governor Brown took to inviting CCST Board members and National Laboratory

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[130] The special sessions of the Kyoto Forum on Science and Technology in Society on Regional Section on Climate Change (RACC) will be taken up in the next chapter.

[131] Brown truly enjoyed talking to scientists, and consulted intimately with Scripps' Veerabhadran Ramanathan on climate change and air pollution policy, as well.

directors to lunch in the Governor's Mansion when CCST met in Sacramento. Quite remarkably, Brown knew something about the specialties of each of the scientists at his lunches and questioned each with enthusiasm, but it was obvious he reserved his greatest passion for energy and climate issues. I left our last lunch meeting feeling that CCST must be doing something right.

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And yet,...and yet. Though increasingly pleased with the content and quality of CCST's advice and flattered by its personal reception by the governor, I worried that CCST's efforts, though increasingly respected, were not producing enough financial support to ensure enduring political influence. I was simultaneously Chair of the Space Studies Board, one of the National Research Council's largest boards, and saw how much organizational support is needed to formulate policy recommendations in actionable terms, communicate them to the chain of pertinent decision makers, and follow the recommendations through to implementation.

Putting knowledge into action is not cost free. CCST was at a delicate point. CCST had built receptivity, but now it was time to build capability. Susan Hackwood's CCST, which she and Pister created out of whole cloth, grew when and where opportunity allowed. Its participating scientists lived all over the state, and so did its staff. People were performing enduring CCST staff functions on contract: full time occasionally, part time frequently; long term rarely, CCST staff were dispersed at those campuses that could provide office space and other organizational support; the only identifiable headquarters was wherever Susan Hackwood was. Moreover, the social network, CCST, did not have a physical presence in the one place where one would think it essential: Sacramento, the state capital. It is ironic, because Susan had created a decisive reason to have a CCST presence in Sacramento—the legislative fellows.

One of my duties as Board Chair was to negotiate the terms of Susan Hackwood's five-year contracts for the ninety percent of her time she spent on CCST affairs, and forward a Board recommendation to University of California President, Janet Napolitano, for final decision. I had a delicate issue to bring up with Susan when we negotiated her last five-year contract. It was now time, I said, to make her accomplishments permanent. She should use her next five years to recruit individuals who could manage CCST-critical functions without her.

Susan, a victim of her own success, was now overloaded with work and needed a deputy to whom she could grant real discretion. To Susan's very great credit, she set out to do what must have been hard for her: bring in a serious competitor for the leadership role as Deputy Director, Amber Mace. I had worked with Amber when she had been Executive Director—and I the first Chair—of the California Ocean Science Trust, where she too had to build a support organization from scratch. While Susan's experience and inclinations were academic, Amber's came from working with government agencies. Amber's kind was needed to achieve CCST's immediate organizational goal—to position CCST to undertake studies commissioned and paid for by the California's legislature and agencies.

Then Susan did something that surprised me. She gave Amber the crown jewels, the responsibility for managing the CCST fellows program. Soon a suite of offices was found in Sacramento, CCST had an adequate headquarters for the first time, and the fellows had a home in the capital where legislators and their staffs could drop by to see what was going on. Amber's CCST gradually built up a talented team in residence. CCST's progressively interactive Board, meanwhile, was in the middle of a revolution of rising expectations. While I as chair got general support from it, a vocal minority believed we were proceeding too slowly on the professionalization of CCST. I, however, was reluctant to push Susan faster than she was comfortable, despite my recognition of CCST's structural problems and commitment to solving them. The vocal minority, I believe, saw me as an obstacle to progress.

That may or not be, but I saw a way to trigger the larger transformation both I and the Board hoped to see. I could time my retirement from CCST to coincide with Susan's, and work with the Board to accomplish its desired restructuring as part of the broad leadership transition. I worked closely with Board member Bruce Darling, the Executive Director of the National Research Council, to design the searches for Susan's and my replacements. The CCST Board carried out a national search for Susan's successor and ultimately appointed Amber Mace, my secret favorite candidate; Susan and I stepped down. On July 1, 2018, The Board composed a vote of thanks to Susan that contains the following words:

> Absolutely nothing was easy, and everything depended on your leadership and judgment. We learned to admire your style. You take on—with enthusiasm and energy—whatever needs to be done, no matter how menial (even serving as a transportation hub!). Your style was contagious; CCST's wonderful staff has learned the words 'can-do' from you...CCST owes its existence to you.

The change in CCST's top leadership provided a fundraising opportunity. Longtime Board member and former President of the National Academy of Sciences (1993-2005), Bruce Alberts, worked with the Gordon and Betty Moore Foundation to put funding of the fellows program on a more secure financial basis. The Board appointed a true policy professional, Peter Cowhey, the Dean of UCSD's School of Global Policy and Strategy, as my successor. Peter's appointment was itself a major step towards the professionalization of CCST's policy work.[132] On July 1, 2019, the California Legislature voted to help support the CCST legislative fellows program financially. On January 1, 2022, Amber Mace was given the title of Chief Executive Officer, reflecting the growth of CCST's activities and influence.

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[132] As outgoing Board Chair I was entitled to another year on the Board, but I was reluctant to interfere; then in 2019 I had open-heart surgery and did not attend.

**Figure 34-2: CCST is institutionalized. Shown here is incoming Executive Director Amber Mace (left), outgoing Board Chair Charles Kennel (center), and outgoing Executive Director Susan Hackwood (right), with the proclamation celebrating Susan's leadership.**

It is often said that the State of California has the scale and scope of a European Union country. Now, like one, California has its own advisory board for science and technology. It is far too much to ask that it achieve as much as Britain's Royal Society, which was founded in 1660, or the French *Academie des Sciences,* founded in 1666, but CCST's (or its successor's?) best chance for historical influence lies in the evaluation of emerging technologies for application to future problems as they manifest themselves in California. Its example could then extend beyond California's borders.

# 35

# The Kyoto Forum on Science and Technology in Society

The July 2003 Earth Observation Summit in Washington, DC, was led at the minister-of-state level and attended by leaders of earth observation programs from thirty-three countries. I was invited because of my role in overseeing the redesign of NASA's Earth Observing System in the mid-1990s. The ministers of state recommended the creation of a Global Earth Observation System of Systems (GEOSS) and its governing body, the Group on Earth Observations (GEO); by 2006, 64 countries had joined GEO. GEOSS facilitates the assembly and harmonization of nationally collected environmental data in ways that promote their use in decision-making around the globe. GEO agreed to measure success by assessing the contributions of GEOSS data toward managing environmental threats in nine societal benefit areas, which at the beginning were loosely defined. Since 2006, when GEO and GEOSS came into being, observations have improved in quality, quantity, variety, and precision; communications have improved in coverage and capacity; scientific understanding has evolved; and management has acquired sophistication and articulation. GEO today measures success by the contributions of earth observations to eight (better articulated) societal benefit areas: disaster resilience, public health surveillance, energy and mineral resource management, water resources management, infrastructure and transport, sustainable urban development, food security and sustainable agriculture, and biodiversity and ecosystem sustainability.

If there is to be a sustainable global civilization, it will have a GEOSS or its follow-on. It is difficult to imagine what that globally inclusive GEOSS will be like, just as those who pioneered radio communications in the 1920s could wish for but not picture today's World Wide Web. It is less risky to guess that one evolutionary step will be incorporation of artificial intelligence and machine learning into environmental data management, not only to spot complex spatiotemporal configurations of environmental threats but to evaluate—eventually in real-time—how well current remediation strategies are working. There will be a celebration each time a corrective step is automated in that GOESS: that there has been another global consensus on how to manage the environment. There will inevitably be worries about handing discretion over to machine intelligence, people will argue over its discretion, and the system will contain a patchwork of special accommodations to local sensitivities. At the moment, however, this seems the only pathway to making earth a thinking planet, aware of its internal processes.

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Koji Omi, former Japanese Minister of Finance and Minister of Science and Technology, confidant of Japanese prime ministers, founded the Kyoto Forum on Science and Technology in Society (STS). Once every year since 2004, on the first weekend of October, the STS Forum has convened science ministers, university presidents, Nobel laureates, industry chiefs, scientists, engineers, and medics from all over the globe, advanced and developing worlds alike, to debate the "lights and shadows" of science and technology. In Omi's lexicon, "lights and shadows" means the threats to—and opportunities for—peace and prosperity created by science and technology, with emphasis on issues where international cooperation or collaboration is important. There is no cooperation without communication, and STS meetings provide an opportunity for busy leaders to develop perspectives on issues they may have to deal with at home. What they do with the insights they acquire is up to them; the Forum takes no political positions, other than raising issues in the first place.

**Figure 35-1: The Kyoto International Conference Center; the Kyoto Protocol was signed here in 1997. The protocol was the first global agreement reached under the auspices of the UN Framework Convention on Climate Change and the one every climate scientist's hopes were on when I started to work in the field. To me, the Conference Center was hallowed ground, an entirely appropriate venue for the annual meetings of the Forum for Science and Technology (STS). Photograph taken by Charles Kennel on October 4, 2012.**

There is no hardship to attending an STS Forum other than international travel. The Forum is held in the magnificent Kyoto International Conference Center, set next to a lake in a forested park overlooking Japan's urban jewel, Kyoto. The Conference Center acquired mythical status with me when I learned the Kyoto Protocol on Climate Change was adopted there in December 1997. I thought I had been lifted bodily into science policy paradise at my first STS Forum in 2006. In meeting rooms all around me were international panels debating issues I had encountered but not mastered. The views I heard were occasionally notable for their originality but invariably impressive for who was expressing them. From what the luminaries talked about you could judge a policy issue's readiness for prime time.

You cannot just walk into an STS meeting as there are the security procedures expected when high government ministers meet. One might reasonably wonder why I was invited. Dan Goldin, who was on Mr. Omi's Advisory Board, had suggested global earth observations as an STS topic, and told me he had done so. Lisa

Shaffer, Scripps' Assistant Director for International Relations, had been promoting networked global earth observations ever since she had assisted John McElroy, then NOAA's Assistant Administrator for Satellites, in organizing the Committee on Earth Observation Satellites (CEOS) under the auspices of the G-7 group of industrial nations in 1984. I had participated in CEOS plenary meetings as NASA principal between 1993 and 1996. These experiences helped Lisa and me to see GEOSS as a necessary advance. We prepared a white paper and PowerPoint presentation for the 2006 STS Forum that concluded with these thoughts:

> As civilization becomes increasingly globalized and challenged environmentally, our need for a global earth observing capability will grow. The creation of an international framework to guide the evolution of GEOSS is an essential and promising beginning but only that. The full value of earth observations will not be realized until observations from distributed ground networks are intimately related to space observations, connected to relevant social information, and peer-to-peer interaction between small users and providers is enabled. Achieving this next step requires that the governmental and intergovernmental concepts and procedures that brought GEOSS to its present stage of development be supplemented by methods similar to those used to stimulate the growth of the internet, which were in that case independent and non-governmental.

I attended STS plenary sessions faithfully but paid more attention to the breakout sessions on environment and climate change. When leaving one of these sessions with me, Dan Goldin remarked with appreciation how many participants were from developing countries. Dan's remark fell on fertile ground: I had been thinking of concentrating my newly-freed time on climate change adaptation, which the mainstream climate science community was downplaying in its urgency to convince the world to reduce greenhouse gas emissions. The adverse impacts of climate change were going to fall heavily on an underprepared developing world. Not only that, but the impacts were going to fall differently on different regions and communities. The STS Forum offered an opportunity to articulate adaptation strategies with scientific leaders and decision makers from many regions of the developing world. It seemed designed for a dialogue that was not taking place in other scientific gatherings.

At the 2007 and 2008 STS meetings, I followed the "Harmony with Nature" series of plenary and breakout sessions. At the 2007 STS Forum, I took the opportunity to advocate sessions on climate change at every STS meeting. In response, the 2008 STS Forum sponsored a planning breakfast that convened experts on international development and the environment. Its purpose was to advise Mr. Omi and the STS Board on the proposal in a white paper that I had prepared, "Think Globally, Assess Regionally, Act Locally."

Dan Goldin and I proposed that STS devote sessions to climate change adaptation on an ongoing basis. The breakfast attendees recommended that a theme for STS 2009 be developing an international framework for regional climate change assessments (with an initial emphasis on water). Dan and I then asked for volunteers to serve on a steering committee to prepare a formal recommendation to submit to the STS Board on January 10, 2009. Dan and I discussed over lunches in La Jolla the arguments that STS cover climate change at every one of its meetings. Even a large meeting like STS does not have the capacity to deal with all the topics in science and technology of importance, and STS policy was to avoid long-term commitments to any one topic. Despite that, the STS Board designed a workable approach that made possible an annual return to topics in climate change.

Dan Goldin and I argued at the planning breakfast that dedicated sessions would provide an ongoing opportunity for scientists on the front lines of adaptation to compare their efforts with global best practice, and for people like me, whose scientific experience was in places like California, to acquire essential understanding of what fighting on the front lines in the developing world would be like. By itself this would serve an unmet need, but there was more. The decision makers who understand best what their communities care about live in those communities, but they usually do not know many scientists. These community leaders are not the leaders of industry, finance, and government who plan how to decarbonize national economies, negotiate global treaties, or commission satellite systems. The central decision makers getting the lion's share of IPCC's scientific advice can fit into the hotels of a medium-sized city, as they do at the annual UN Conference of the

Parties meetings. There is no functional equivalent that serves the millions of local decision makers responsible for adaptation to climate change.

**Figure 35-2: Charlie Kennel (left) and Dan Goldin (right) in discussion in 2009, around the time they proposed the special adjunct sessions devoted to Regional Action on Climate Change (RACC) at annual meetings of the Kyoto Forum on Science and Technology in Society (STS Forum). Ellen Lehman took this picture from the balcony of the Birch Aquarium at Scripps looking west with the Scripps pier below and the Pacific Ocean stretching to the horizon.**

In its wisdom, the STS Governing Board decided that STS convene adjunct meetings devoted to Regional Action on Climate Change (RACC) at every STS meeting. In addition, the RACC international advisory committee could nominate individuals to attend the general meeting, both to be held in Kyoto on the same weekend. Dan Goldin and I ended up as co-chairs of the first RACC advisory committee, which decided at its first meeting in 2009 to compose a declaration that would be read out at the plenary concluding session of the general meeting. Composing a declaration was then new to STS.

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I had begun to think about what the international science community could do to encourage decision makers in the developing world to act on scientific knowledge. It was hard enough to tease out the answer to this question in California and it was en elusive task in Venice. What did we in California or Venice know about the decision cultures of other countries? Local leaders need to trust what they hear from scientists before they dare risk resources and reputations on scientific advice. Seeing our Scripps teams work with the California state government and San Diego County, and UCSD's with Venice, convinced me how important interpersonal interactions are even to committed and funded decision makers when they are faced with putting unfamiliar knowledge to work.

Regionally specialized climate forecasts are the first step. But after that, even more specialized knowledge about potential impacts is needed to guide community leaders, who react acutely to threats to the things their communities care about. To avoid being ignored, professional climate scientists need to understand the issues specific to the communities they are working with. In numerous discussions with Dan Goldin in La Jolla and with Sally Daultrey, Dick Fenner, Grant Kopec, Julian Hunt, and Martin Rees in Cambridge, and with Paul Linden, Lisa Shaffer, and Kim McIntyre of the UCSD Sustainability Solutions institute, I arrived at the notion of knowledge-sharing social networks that immerse international scientists, regional decision makers, disaster managers, and local community leaders in dialogue about adaptation risks. Only later did I realize ours was a particular realization of ideas developed by the Nobel laureate economist Elinor Ostrom, who had been one of the first speakers Lisa Shaffer invited to speak to the fledgling Environment and Sustainability Initiative at UCSD.

I introduced the Knowledge Action Network concept at the 2009 STS Forum. I wanted to hear what developing-world leaders had to say about it. The reception was sufficiently positive that a small group worked at night to develop the ideas expressed in the daytime sessions into the first RACC declaration. In the subsequent 2010 declaration, an augmented group of RACC participants returned to Kyoto to clarify the Knowledge Action Network concept.

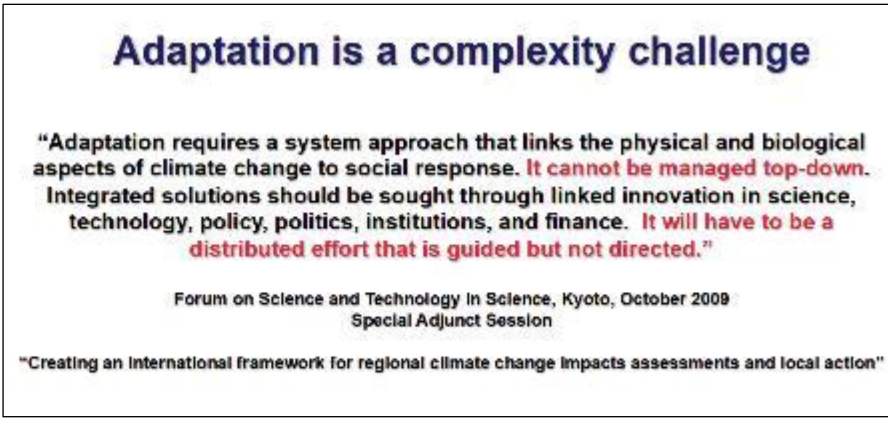


**Figure 35-3: In December 2010, I gave a presentation to the International Drylands Conference in Cairo, Egypt, on the conclusions reached at the STS Forum in Kyoto a year earlier. Above is one of the PowerPoint slides I presented there, wherein I conveyed the notion of Knowledge Action Networks to support local leaders as they deal with the complexity of regional climate change.**

The 2009 declaration could not have been written by any one of us. The group that composed that first RACC declaration had leadership experience not only with research but also with science as an institution. The group's diversity and wisdom reflects well on Koji Omi's vision for the STS Forum. John Boright was Executive Director for International Relations of the US National Academy of Sciences; Meghan Clark was Director of CSIRO, Australia, the Australian National Science Foundation; Ismail Serageldin had been Vice President of the World Bank and was the founding Director of Bibliotheca Alexandrina, the new Library of Alexandria in Egypt; Alexander "Sascha" Zehnder was the Scientific Director of the Alberta Water Research Institute in Canada; Dan Goldin as NASA Administrator had overseen programs that provided about half the world capacity to observe the earth from space; and Jim Falk of the University of Melbourne was ahead of most of us as the author of several books on climate change governance.

An STS adjunct session on Regional Action on Climate Change has been convened every year since 2009 by an international advisory group comprising some of the original founders listed above as well as new recruits. I stepped down as founding RACC Chair in 2013 in favor of Canada's Gordon McBean, who, in 2014, would become the Chair of the International Council for Science. I promoted Gordon's nomination because I was grandiosely beginning to feel that

RACC would not become permanent unless it could prosper without me. Gordon was well placed to communicate the Knowledge Action Network agenda globally, and his International Science Council's Future Earth project did indeed adopt a version of the Knowledge Action Network concept.

Knowledge Action Networks are a mechanism to link the intellectual power of global science to the social agency of local leaders. They have acquired an acronym, KAN, signifying mostly that they have been the subject of repeated debate in RACC circles. No one has come up with a universal design, since KANs conform to highly specific social and environmental circumstances; this means there is no standard definition of a KAN to test in double blind social experiments. The lack of rigor makes peer reviewers critical, but it seems intuitively obvious that interpersonal reinforcement increases the motivation to act, especially when the decision maker's comprehension of unfamiliar knowledge is imperfect.

I needed a visualizable example wherein the same global climate forecast would motivate different adaptation decisions in different regions. Scripps' forecast of long-term decline in the Sierra Nevada snowpack and spring water runoff and the consequent threat to California water security remained fresh in my mind after I was stepped down as SIO Director. Asia was going to have a much bigger snowmelt water availability problem than California. The eleven largest rivers in Asia have headwaters in the Himalaya and Hindu Kush Mountains. Climatic conditions in this snowy mountainous region, sometimes called the Third Pole, affect the populations in a good fraction of Central and East Asia. Tim Barnett of Scripps, among others, had pointed out that climate change would impact the cities, towns, and farms in China, India, Bangladesh, Vietnam, Pakistan, and Central Asia that use the water from those eleven rivers, putting about two billion people at risk. Shouldn't science be forecasting the coming changes in water availability in each river basin? Each community along each river will face its own unique challenges, but they all will be doing so simultaneously. Will generic policy provisions be enough? Or will each community require specialized scientific advice?

Over the next three years, I helped arrange two workshops and participated in two others on the impacts of climate change on water availability. Martin Rees and I visited the Gordon and Betty Moore Foundation to secure funds for workshops at UCSD and Cambridge. The first, which opened the new Scripps Seaside Forum, compared the threats to water availability from the decline of the snows in California's Sierra Nevada and Central Asia's Himalaya and Hindu Kush Mountains. We followed with a sister workshop in Cambridge led by Lord Julian Hunt, former head of the UK Meteorological Office, on African water availability issues. There are many threats to African water availability, but we found no common threat except for the fragility of its infrastructure. Later, I participated in two workshops on the decline of mountain snows and glacial retreat, one at the Potsdam Institute for Advanced Sustainability Studies, led by former UN Environmental Programme Director, Klaus Töpfer, and the other in Rome at the Pontifical Academy of Sciences, led by Veerabhadran Ramanathan and Paul Crutzen. Among other things, these workshops confirmed that mountain snows and glaciers were retreating virtually everywhere, even in the tropics. Contrary to denialist claims in the popular literature, the impacts of climate change were already being experienced globally.

The risk to water availability is situation dependent, and there is no one adaptation assessment methodology that fits all. IPCC's approach to climate change assessment, designed for mitigation, is not easily ported over to adaptation. In our example, decision makers in each river basin need highly specific forecasts. At the 2009 Scripps workshop, we learned that central planners in Beijing had sophisticated management plans for China's major rivers, including especially the Yangtze and Yellow Rivers.[133] The fact that central planning was admirably sophisticated did not guarantee that its mandates would be carried out in the provinces. Nepal's concern was to prepare villagers for the glacial lake outflow floods resulting from the collapse of ice dams that hold back the water from melting glaciers. Other examples confirmed that climate change impact assessments would need to be further specialized by community and economic sector, and methods found to translate abstract scientific knowledge into community action. Initially, face-to-face expertise networks can communicate the needed knowledge, but as risks and causes start to interact, knowledge integration will increasingly rely on electronically facilitated networks of human expertise.

[133] David Victor pointed out that however detailed the work of Beijing's central planners might be, the real issue was whether their plans would be adhered to in the provinces.

I conveyed the lessons these workshops taught me in a lunchtime talk at a meeting of the Ministers of Finance of the Americas in Lima, Peru, in May 2010, prior to the UNFCCC Conference of the Parties meeting that fall. Indeed, Peru itself is highly vulnerable to climate change; Peru west of the Andes Mountains is a vegetation-free desert that gets what freshwater it does from about forty rivers and streams with headwaters in the Andes to the east. Peru faces its own version of the problems the populations near Mount Kilimanjaro in Africa and along the Ganges and Mekong Rivers in Asia are facing.  California's water problems suddenly seemed small.  But perhaps we could help.

RACC's focus on adaptation emphasizes ecological and environmental issues and not the mitigation issues of energy and finance that preoccupy the leaders of finance and industry at UNFCCC COP meetings, or the main STS Forum, for that matter. Shinzo Abe, the now deceased longest-serving Prime Minister of Japan, saw an opportunity to build on the opening made by his friend, Koji Omi. In 2014, the government of Japan sponsored the first annual "Innovation for Cool Earth Forum" (ICEF) immediately following the STS Forum. STS luminaries needed only take a three-hour ride on the bullet train from Kyoto to Tokyo to participate in ICEF. Japan was signaling its belief that climate change will be a major stimulus of technology innovation. I gave an invited paper at that first ICEF meeting in a session on the role of the public sector; I accompanied my good friend and collaborator David Victor as he spoke a ICEF plenary sessions in both 2015 and 2016 on diplomatic strategies for addressing climate change. David was in his element at ICEF.

I wonder now why innovation for climate change is primarily thought of in the context of mitigation and not adaptation; perhaps it is because the gentle people who go into ecology and love watching nature unfold on its own by and large are less influenced by the restless acquisitiveness of those trying to cure the world's energy problem.  But most of earth's creatures will feel climate change as an adaptation problem and human innovation in this area is just as critical to their futures, and indirectly our own.

# 36

# Cambridge

There was something odd about the light. The gathering darkness of long evenings in Britain was familiar. That wasn't odd. It was the flickering shadows, the dancing hues that shouldn't be there—lime green, deep purple, pale yellow, bright orange. I walked towards the shimmering sky, past the closed merchants' tents in Market Square, approached Great St. Mary's Church, stopped momentarily by the windows of Ryder and Amies (where I bought my Christ's College scarf), and finally pushed out into Trumpington Street. There were almost as many people there as on Saturday mornings when the tourists come to look at the home of the King's College Choir or to gawk at the curious brass cricket clock nodding away in a window of Corpus Christi College. Locals also came into town in families to shop on Saturdays. The people in tonight's crowd didn't seem like tourists or locals; they were mostly academics, standing around, gazing like sheep in the same direction at the images of faces projected onto the walls of the Senate House. The soft recorded voice on the loudspeakers barely overcame the quiet murmur of the pensive crowd.

I had been to other sound and light shows, but this one was different. The others featured the lit-up building itself—Notre Dame in Paris, the Coliseum in Rome, the Pyramid in Giza. This one featured people projected in twenty-first-century Technicolor. Not just any people, mind you, but the three that Cambridge University in 2009 counted its best and brightest in its 800-year history: Isaac Newton, Charles Darwin, and John Milton. You couldn't complain about the choice, I thought, but I bet there were ten times as many that this crowd would not quibble about. How about John Maynard Keynes or Bertrand Russell for starters? I wondered if any of my contemporaries would make it to the 900th anniversary list, say Fred Sanger, Alan Turing, Martin Rees or Stephen Hawking. It was not too hard to think up other names.

I must admit to a little shiver—a *frisson*—of pride when I think that two of the three up in lights, Darwin and Milton, were from "my" college, Christ's College. Christ's took me in at a moment when life seemed without direction, made me its C. P. Snow Lecturer in 2007, and kept on finding a place for me until I felt I actually belonged. I found myself making annual visits for the entire Lent (winter) term. I founded a lecture series on climate change in 2014, and now, in 2022, Christ's Board of Governors has elected me a Fellow Commoner with all the rights and privileges of other College Fellows, except that I do not hold an academic position in the University. The College has elected only eight Fellow Commoners since 1994.

**Figure 36-1: Fellows Building, Christ's College, Cambridge, on a rare snowy morning. Bill Fitzgerald's second story window was just to the left above B-entrance (on the right). Photo taken by Charles Kennel on February 5, 2012.**

Visitors must walk across Second Court on the path; only Fellows may walk on the luxuriant green grass. I would knock on Bill’s door around a half hour before dinner almost every day. The first time I knocked I did not know what to expect and found myself in the digs of a Victorian academic: fire burning in the fireplace behind a big iron grate, tattered Turkish rugs strewn higgledy-piggledy on the floor, others arranged more neatly over the big couch, dimpled leather easy chairs facing the fire. A low table covered with open mathematics books and folded two-week old newspapers stood between the couch and chairs. There was just enough open space to put your wineglass on the table. On Bill’s walls hung exotic musical instruments; someone told me there were fifty-six of them and that Bill could play them all. This big room with the rugs and instruments was where Bill tutored engineering undergraduates. He was very particular about his time, particularly on student supervision days, and he would tell me to come for wine at 19:03 or 19:10, and on early days at 18:45. I never ran into a student coming down the stairs as I was going up; they must have left Bill’s room a minute or two earlier. The wine would be waiting on the table and Bill would be ready with a question to start the conversation. Dinner in Hall would follow at 19:30 where I had to contend with Darwin’s questions.


**Figure 36-2: The Cambridge Mystique. US Air Force veterans and many of my scientific heroes con-sider the Eagle Pub a hallowed site. During World War II, US and RAF bomber pilots wrote their girlfriends’ names in lipstick on the metal ceiling. You could see them 65 years later. The decoders of DNA, Francis Crick and James Watson, came from their laboratory to the Eagle to announce to all who would listen that they had just solved the mystery of life. I went to the Eagle occasion-ally, hoping that some of the Watson-Crick magic would rub off on me. Photo taken by Ellen Lehman on December 30, 2017.**

They do not do it anymore, but then the Christ's College Fellows used to line up for High Table in precise order of seniority: those sworn in as Fellows a few minutes earlier on the same day took precedence over those sworn in minutes later. I never questioned that, nor that I, as a visitor, should walk at the end of the procession of gowned personages into Hall. It sometimes took a while for the whole procession to be seated, so as the last in line I had a chance to see the students standing out front, waiting hungrily for the Latin benediction. The death mask of John Milton next to the corner of the Fellows' High Table had the smooth face of a teenager; I haven't dared ask whether I am the only one who thinks it eerie. My favorite portrait of Charles Darwin was on the wall overlooking the long dining table. I would contrive to sit across from Darwin at dinner; in the occasional interval of silence, Darwin, without moving his lips, would ask me archly whether I had thought deeply that day. (I never dared answer.) David Norman, a Christ's paleontologist, had been put in charge of the College's celebration of Darwin's bicentenary, also in 2009. David arranged to have Darwin's room furnished with early-nineteenth-century period pieces, some of them Darwin's, and turned the room into a secular shrine. The tourists come to visit Darwin's room on Saturdays. The College had also commissioned a life-sized sculpture of Darwin as a student, seated reading at a bench in Third Court. You could sit and read beside the young Darwin. I did so from time to time.

**Figure 36-3: Charles Darwin's spirit is everywhere in Christ's College. Here is a life size portrait as he might have appeared to fellow undergraduates. Anthony Smith (matriculated in Christ's in 2002) spent a year crafting this sculpture of Darwin as a young man, which was unveiled by HRH Prince Philip, Duke of Edinburgh, on February 12th, 2009—Darwin's 200th birthday. I used to sit on the bench next to Darwin in vain hopes that some of his magic would attach itself to me. Photo by Ellen Lehman.**

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How did I end up a friend of Bill Fitzgerald? The story starts in La Jolla in 2004 when an imposing friend of UCSD, Salah Hassanein, made me an offer I could not refuse. I have just finished raising money for the new cardiac care hospital at the UCSD Medical School, Salah said, and I now would like to do something equivalent for Scripps. Are you interested? I said *we were* and asked Salah to join the E. W. Scripps Associates.[134] At one of the Associates' meetings, Salah took me aside to say I really must meet his friend, Bill Fitzgerald. Bill had done Salah a tremendous favor (I never learned what) and Salah thought the world of him.

I annoyed Chancellor Fox when I took the Associates on a showy summer trip to visit SIO's Venice Gates Project.[135] The group flew to Venice through England and Salah asked Bill Fitzgerald to host us in Cambridge. Bill arranged a visit to the Cambridge Botanical Gardens where the Associates heard described the work of John Stevens Henslow (1796-1861), Charles Darwin's Cambridge mentor and lifelong friend. To conclude our Cambridge visit, Bill arranged a reception for the Scripps group in the Christ's College Garden, where he and I agreed to have lunch together on Bill's next trip to visit Salah in San Diego.

The lunch was on one of those glorious Southern California days: gentle warmth, mild Santa Ana winds, the scent of flowers in the air, cloudless blue skies. Bill and I sat outdoors at a little Italian restaurant in La Jolla Shores near Scripps. Our conversation lasted until four o'clock in the afternoon. Though I had met Bill only twice, and though we were in different fields, we bonded, and I revealed that I was about to step down from the Scripps Director's job and had a sabbatical coming. I asked Bill whether a visit to Christ's College were possible. I had no idea of the magnitude of what I was asking.

Bill pulled off a miracle of College politics. The lower half of a faculty house next to the playing fields on Huntington Road, a forty-five-minute walk from the College, was found in time for my arrival in January 2007. My visit proved timely because the previously agreed C. P. Snow Lecturer had bailed out, and I was pressed into

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[134] Before he retired, Salah had been the President of Warner Brothers International theaters; Bill later told me Salah had owned many of the movie theaters in London's Leicester Square. Salah had another deep connection to England; his wife was the elegantly quirky fashion and theater designer Zandra Rhodes. They divided their time between San Diego and London.

[135] The showy appearance was the problem, not the expense to UCSD; the board members paid for all trip expenses.

service. Over the next seven years, Bill Fitzgerald initiated me into the intricacies of Oxbridge collegiate politics. Bill made sure I got to know the key people in the life of Christ's College—the Master, the Senior Tutor, the Bursar, and sundry legendary personalities. The Master's Assistant, Clare Kitcat, provided an occasion for delightful chats in mid-morning. Bill also introduced me to colleagues in the Department of Engineering, especially Nick Kingsbury and his wife, Jane, who have become close personal friends.

The title of my C. P. Snow Lecture borrowed from the Cambridge graduate and poet Andrew Marvell: *Had we World Enough and Time: Global Earth Science and Sustainability*. The lecture touched on themes that have stayed with me ever since: the sustainability challenge, the environmental crisis in the past fifty years and what was expected of it in the next fifty years. I emphasized universities' obligation to face the sustainability challenge. I said that the earth sciences will be central to the science of sustainability and that the individual earth sciences were already self-aggregating into a comprehensive earth system science. I spoke of the UN's Global Earth Observing System of Systems, the international institutional network that provides data for environmental decision-making recently created by intergovernmental agreement in 2005. I also emphasized that information technology was going be decisive for managing the impacts of climate change. The talk ended with a genuine plea to bridge Snow's two cultures before it is too late.

At the reception following the C. P. Snow Lecture I had a brief conversation with Alison Richard, then the Vice-Chancellor of the University. The conversation was fleeting, one of thousands she would have had, but it set the tone for what was to come for me. She related that Cambridge had a chance to host what became the Hadley Centre for climate modeling and passed it up. She asked if they had made a mistake. I replied no, the UK government had done a fine job with climate forecasting, but Cambridge would find its moment when it was time to find solutions to the climate problem, for much more than climate models would be required, and the breadth of excellence of the University would come into play.

Lord Snow, a Fellow of Christ's College for fifty years, had been a major government executive and an author of fiction as well as a chemist. Snow's novel *The Masters* was a thinly disguised account of the election of an earlier Master of Christ's College, Lord Todd. Some of Snow's portrayals of the Fellows of Todd's time were not entirely flattering, and Snow did not have an impeccable reputation among the current fellows. I did not know of Snow's mixed reputation in the College until after my talk, but I was honored to give the lecture. As a Harvard undergraduate in 1959, I attended Snow's lecture in Memorial Hall about the deepening divide separating the sciences from the humanities in mid-century academic culture; now forty-eight years later, in 2007, I had stood on a similar, if less imposing, stage.

The dinner in Christ's College after the C. P. Snow Lecture remains a pleasant blur in memory except for one thing: I was positioned near Milton's death mask with Darwin's portrait looking down at me from on high, when one of the senior fellows leaned over to whisper in my ear, *you know, Snow really was not a very good chemist*... Good chemist or no, C. P. Snow is interred in the garden of Christ's College.

I did not return to Christ's for an extended visit in 2008, but I did come to town for a brief visit. By chance I ran into Frank Kelly, then the Christ's College Master, who greeted me with, Charlie, you've not been using your privileges! *Privileges?* I thought to myself: *Professors have privileges?* Yes, indeed, Frank said; we voted you a distinguished visiting scholar so you have dining rights seven meals a week in College. And you have a priority in the allocation of visitor accommodations. That sounded like a pretty good deal, and I contrived to visit nearly every Lent term from 2009 until 2020, with reduced time in 2013 for treatment of bladder cancer. I chose winter term for my annual visits because I imagined other visitors would prefer the spring term and there would be less competition for accommodations in the gray rainy English winter. You don't go to Cambridge for the weather anyhow; you go for the people.

Sometime after I told Bill Fitzgerald about my 2012-13 bout with bladder cancer, he revealed that he had been having "déjà vu" events where he experienced, as though immediate, vivid sensory recreations of past events of his life. The déjà vu events were growing more frequent, and he was going to seek treatment. The events proved due to a brain tumor of the worst kind, a glioblastoma. Jonathan Gillard, a medical doctor and Fellow of the College, quietly told me there was no hope and outlined the stages of Bill's inevitable decline. Bill did not want his regular colleagues to see his debility, but he did not feel that way about me. He wanted me to visit him earlier and earlier for our pre-dinner conversations. His childhood Irish brogue, overlain by decades in Cambridge, reappeared. At a certain moment, Jonathan and I suggested that the College arrange a quiet lunch to which Bill's colleagues would be invited, and he died shortly after. Bill was the friend who guided me through the delicate complexities of college life, and now he was gone.

It was not until Bill Fitzgerald's memorial service in 2015 that I appreciated how important a scientist he had been. One of Fitzgerald's students, Mike Lynch, a British version of a Silicon Valley tycoon, who founded several hi-tech companies using Bill's ideas, venerated Bill as much as did Salah Hassenein. In our pre-dinner conversations, Bill had tried to teach me the use of Bayesian statistics in what are today called big data analyses, but I had been too insecure mathematically to follow him into the thicket of Markov Chain Monte Carlo analyses. (I did learn how important was the work of my plasma physics hero, Marshall Rosenbluth.) I have not stopped being impressed by the philosophical implications of Bayesian probability: that we humans start with imposing a conceptual structure on reality—a *priori*—and then test the extent to which reality conforms to the structure. In other words, we leap to conclusions and only then ask statistics to prove we were right.

I suspect early humans evolved brain circuitry that reacted instantly to the growl of a saber-toothed tiger tracking from behind; it was safer to assume the growl meant saber-toothed tiger and not anything else. We have turned this alarm reaction into a habitual mode of reasoning; once we think something through, we store the thought nugget in our minds. We humans have learned how to array thought nuggets in ever more complex structures. We do not waste mental energy rethinking every step through from the beginning, especially if a mode of thought made sense in the past. Neither do computers; every time we receive a message suggesting we might like to buy such-and-such, some computer somewhere has already categorized our likes and dislikes based on our behavior and is matching them to products for sale. It may be through this reasoning pathway that societies impose cultural norms on behavior.[136] We learn from clues of behavior, accent, and dress which of our fellow humans is trustworthy and which—usually outsiders—are not.

[136] J. Henrich, *The Secret of Our Success* (Princeton, NJ: Princeton University Press, 2015); see also S. Reinert, M. Hübener, T. Bonhoeffer, and P. M. Goltstein, "Mouse Prefrontal Cortex Represents Learned Rules for Categorization," *Nature* 593 (2021): 411-17.

Behavioral and ethical norms do not enter our minds by rigorous mathematical reasoning.

Bill Fitzgerald has given me years of philosophical pleasure, tinged with regret that I had not appreciated his field when my career as a scientist started. The biggest debts can never be repaid.

# 37

# Cambridge Dialogues: Black Holes to Climate Change

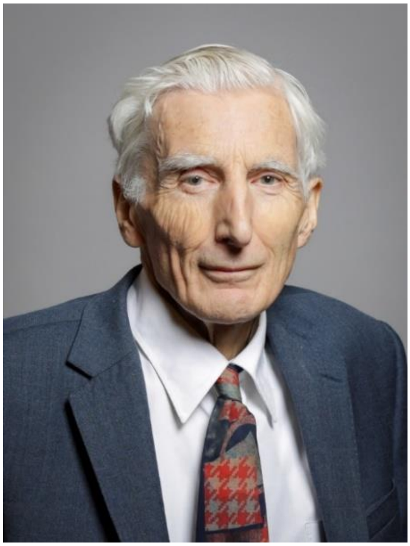

Figure 37-1: Martin Rees, Baron of Ludlow. We would discuss the state of science and the world for hours at a time in his rooms at Trinity College or his study at home.

Martin Rees is the most renowned scientist after Charles Darwin to graduate from the Shrewsbury School in Shropshire, founded in 1552. Were it not for the extraordinary life story of Stephen Hawking, Martin would have been an easy choice for the leading Cambridge astrophysicist of his generation. Martin succeeded Fred Hoyle as the Plumian Professor of Astronomy and Experimental Philosophy in 1973 at the age of thirty-one, and he became Astronomer Royal in 1995. In his early career, Martin worked out the physics of accretion disks surrounding ultra-dense astrophysical objects like black holes and neutron stars. My Princeton graduate student friends and I had listened with polite skepticism to John Wheeler's arguments that black holes were more than solutions of the Einstein equations, they were the engines powering active galactic nuclei. Only fifteen years later Martin had formulated the intricate physics of how accretion disk materiel diffusing toward the black holes generates the beams of X- and gamma-rays whose extraordinary luminosity makes them identifiable. To do this, he made a heroic assumption that only someone trained in fusion plasma physics (as I had been) would see as reasonable: that the diffusion rate would be much faster than classical models of the diffusion process predicted. I sat up and took notice when Martin's papers were published in the 1970s.

My 1984 papers with Ferd Coroniti on the magnetosphere of the Crab Pulsar had developed a 1974 theoretical picture proposed by Martin and James Gunn, and I had worked with Martin's former student Roger Blandford as a Fairchild Scholar at Caltech, so I did not feel out of place inviting myself to visit Martin during one of Ellen and my trips to visit her sister, Jo, and her family, then living in Surrey outside London. Ellen and I went up to Cambridge and had lunch with Martin, who was at King's College at the time. Two decades later, when I got to Christ's, Martin was Master of Trinity College and President of the Royal Society of London. Within the tightly knit Cambridge academic community, the Master of Trinity, the only college master appointed by the Queen, has a status comparable to Prime Minister, who is also appointed by the Queen.

Martin has an unusually compact understanding of many scientific disciplines and a visionary bent that resonates with visionaries in many other fields in and out of science. At first I thought that his work with the Royal Society enabled him to acquire his extraordinary breadth of knowledge, but I soon realized that he was an effective President of the Royal Society because he had extraordinary breadth of knowledge to start with. Martin is also the most expeditious person in my acquaintance and attends assiduously to an unusually large scientific acquaintanceship. On my first few visits with him in 2007, we identified people in Cambridge I ought to meet. During the ten minutes it took me to walk back from his rooms in Trinity College to mine in Christ's College, Martin would have emailed them, so that when I emailed later to set up a meeting the people we hoped I would meet already knew who I was. Martin also introduced me to the British climate science community by inviting me to the Royal Society's review of the 2007 IPCC climate change assessment, to be placed at its ceremonial dinner with Sir John Houghton, the founder of the IPCC, a memory I cherish to this day.

Life's trajectory had at long last carried me from Princeton to Cambridge's famed Cavendish Laboratory—not to work with physicists but rather with social anthropologists. Each time I was to climb the circular staircase in the Mond building, I was aware that the Mond had once housed Pyotr Kapitsa's big magnets. Kapitsa was, until Stalin called him back to Russia in 1934, a prized protégé of the legendary Director of the Cavendish Laboratory, Ernest Rutherford, the father of nuclear physics.

You really ought to visit the Mongolia and Inner Asia Study Unit (hereinafter, MIASU) at the old Cavendish,[137] Martin Rees had said; they are doing some interesting work on climate and society. Martin's wife, Caroline Humphrey, had devoted the proceeds of a MacArthur Fellowship award to founding MIASU. You would like this notion, Martin said: there is a spot in Middle Asia where the borders of Mongolia, Kazakhstan, and China come together; you can see the national borders from space. The landscapes look different from above because of cultural differences in environmental practices.[138] These traditional societies had transformed their environments on a large scale without the aid of tractors and dynamite, and the people at MIASU are studying how traditional cultural practices affect the climate.

Make sure you meet Hildegard Diemberger, Martin had said; she is reconstructing the climatic history of Tibet using records preserved in monasteries. Hildegard became a good friend whom I made sure to see several times on my annual trips, and I was careful to attend as many of MIASU's seminars as I could. Hildegard, the daughter of a famous Swiss mountaineer, had created an adventurous high-altitude life of her own by working in the Himalayas and on the Tibetan Plateau. Tibetan monks kept records of their observations of the snow cover on certain "sentinel" mountains in the Himalayan foothills, whose summits would be brown in warm and dry years and white in wet and cold years. These monastery records help to infer a history of water availability on the Tibetan plateau.

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[137] The original Cavendish laboratory was moved to West Cambridge in 1974, and an even more modern one was under construction in 2021.

[138] You can see the border between Mexico and California, too, because of differences in wildfire management practices, too. In Mexico they tend to let wildfires burn, whereas we in California engage in active wildfire suppression.

This research prompted Hildegard and her social anthropology colleagues to ask whether culturally inherited land management practices affect the rainfall or snowfall on the Tibetan Plateau. They teamed up with Hans Graf, a climate modeler from the Cambridge Department of Geography. It turns out that grazing practices do make a difference to the weather at high altitude. The air is normally dry and transparent; what moisture there is condenses at night on the ground, evaporates in first sunlight, and rises to altitude during the day where it forms clouds; the clouds provide some shade, which tends to warm the ground less. Before the Han Chinese took over ruling Tibet the local herders looked after yaks, which trim the ground cover to a height of few centimeters. The ground cover provided a large surface area for atmospheric moisture to condense on overnight. The Chinese government imported goats, which trim plants to the ground. The net result was hardly any nighttime condensation and a dryer, cloud-free, less rainy atmosphere.

**Figure 37-2: The Mond Building, originally part of the famed Cavendish Laboratory at Cambridge University. The Cavendish, legendary in the history of twentieth-century physics, housed the Mongolia and Inner Asia Study unit when I visited in the twenty-first century. The Mond's 1920s art deco style was new when Pyotr Kapitsa, the favorite student of the father of nuclear physics, Ernest Rutherford, placed his large research magnets there.**

I was impressed that MIASU's social anthropologists had teamed with Cambridge Geography's climate modelers to run a climate workshop. Social scientists dislike being dragged into climate research as an afterthought, and here they were initiating climate research on their own and for their own reasons. This was new. I thought this kind of thing ought to happen more often, and I hoped to encourage MIASU's interest in climate change by making frequent visits to the Mond Building and leading informal seminars on new results in climate change science and policy. I occasionally sat in MIASU's professional meetings as well.[139] It was particularly enjoyable to meet another prominent social anthropologist, Barbara Bodenhorn, with whom I was able to talk knowledgeably about the lives of Native Alaskans because Ellen and I had spent so much time with Native people during our visits to Alaska. (Barbara had Alaskan and Canadian Inuit culture as one of her specialties).

Listening to Cambridge's social anthropologists gave me a visceral sense of how intimately are entwined the human and natural drivers of environmental change.

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Great mathematicians and scientists like Isaac Newton may find their own intellectual company more stimulating than everyone else's, but even Newton had to acknowledge his debt to the giants whose shoulders he stood upon. However, he was communing across centuries with the few other geniuses like him in history. The vast majority of us need other people to talk to in the here and now. Solitary thought soon encounters roadblocks of its own manufacture. It creates contention within itself that cannot be resolved before resolve runs out. The resolution for most of us is to entice or provoke colleagues into providing what is missing: new information, the freeing idea, or just plain encouragement. Science usually does not advance until this happens.

Cambridge University may be slower than others to create new curricula and research centers, but it compensates by cultivating an extraordinary rich seminar culture. Cambridge's late afternoon seminars are timed to match scholarship's diurnal cycle. Cambridge's thirty-one colleges have meeting rooms that make it easy to convene

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[139] C. F. Kennel, "Afterword: Speaking Scientific Truth to Power," *The Cambridge Journal of Anthropology* 31, no. 1 (2013): 150-55.

a group somewhere to debate some new topic. Debates start at the canonical time of 5pm, when teaching and tutorials are finished for the day, and faculty, their critical faculties fatigued, are ready to listen to something different. Cambridge for me was a ferment of ideas being tried out; they would come and go and only a few would survive trial by seminar. When some idea seemed worth looking into further the University would subsidize an organized series of seminars, sometimes followed by a dinner with more opportunity for talk sustained by good wine. Martin Rees and Paul Linden co-chaired one such series on sustainability that I attended when in residence. Their purpose was essentially to find out what was in it for the University should it choose to develop formal programs of research and instruction related to sustainability. Sustainability, a trendy word, means many things to many people. To tie down its meaning to the University, the Linden-Rees seminars invited representatives of local and national government, industry, and commerce. This being Cambridge, the chosen witnesses nearly always came when invited, often from London.

**Figure 37-3: Ellen Lehman and Charlie Kennel celebrating Ellen's 75th birthday, February 21, 2019, in the Fellow's Combination Room at Christ's College, Cambridge. The picture in relief over the mantelpiece shows two mythical animals, Yales, that are part of Lady Margaret Beaufort's Coat of Arms. Lady Margaret, the mother of King Henry VII, was a major benefactress of Cambridge and Oxford; she was to Cambridge what Ellen Browning Scripps was to La Jolla. Lady Margaret founded Christ's in 1505.**

I was also invited to be the first senior fellow of Cambridge's Centre for Science and Policy (CSaP), which works principally with executives of the UK government who visit the University with issues they want to think through. Here the convening tool was lunch: CSaP convenes lunchtime seminars on science policymaking for graduate students from different departments. I may have looked like a senior scientist at these long lunch seminars, but I was acting like a graduate student. I participated occasionally in another of Martin Rees' inspirations, the Centre for the Study of Existential Risk (CESR), where I talked of Philip Morrison's vision for the long future of the planet. Once I accompanied Martin and some CESR students to London where the students had organized a seminar on climate change for members of Parliament and their staffs in the House of Commons.

It got so I did not want to leave Cambridge. The museums and theatres of London were forty-six minutes away by fast trains that ran every half-hour, but I chose to spend most weekends in Cambridge when there was unscheduled time to think and write. I did faithfully attend the Sunday evensong choral service in the Christ's College Chapel for an hour of mindfulness. Ellen and I would talk by Skype twice a day in my afternoons and evenings, and she would take vacations in England from her psychology practice in LA when we did go around London. We also visited the Isle of Skye in Scotland where the breed of our Skye Terriers, Andy, Tammy, and later, Forbes, originated. We had planned a visit to Cornwall and Devon in 2017, but I was called back to California because of Ellen's ovarian cancer; we celebrated her recovery and 75$^{th}$ birthday in February 2019 with an elegant dinner put on by the College.

I returned to Cambridge in 2020, six months after my own cardiac bypass surgery, to write an essay on the Anthropocene Crisis, a new kind of paper for me. I returned early to California on March 11, 2020, the day the US closed international air travel because of the pandemic. Ellen and I have led confined lives in La Jolla and Ramona since then. I did not visit Cambridge in winter 2021 or winter 2022. The Christ's College Lectures were conducted via Zoom.

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I had been visiting Cambridge since 2007 but only in 2014 did it occur to me that it was time to give something of academic substance back. In creating UCSD's massive open online course

(MOOC), Climate Change in Four Dimensions, we had created a course in what in my undergraduate days at Harvard we would have called general education for graduate students. I proposed to Christ's College that I give a similar series of lectures for the general educated public at Cambridge's canonical seminar time, 5pm. I had the MOOC's PowerPoint presentations on climate change adaptation ready to go. The College responded positively by assigning a lecture room on Thursday evenings during Lent Term and providing a wine and cheese reception for all who wished to stay afterwards to converse, or who needed inducement to stay until the end. In 2014 I gave the entire series of lectures, which I called "Avoid the Unmanageable, Manage the Unavoidable," about what science thought the world could expect from climate change in the coming decades.[140] I do not know whether we should have been surprised, but my first lectures drew audiences of about sixty, primarily of academics from the University and citizens from the town. Many attendees returned every week; one person came in from Southampton. In later years, the lectures moved to the larger Hamied Auditorium in Christ's College.

Thus was started the Christ's College series of lectures on Climate Change and Sustainability. I had exhausted my prepared material in the first year, so the next year, 2015, we decided to expand beyond describing climate change impacts to suggesting solutions. I recalled my brief conversation with Alison Richard, the Vice-Chancellor, after my C. P. Snow Lecture, when I had remarked that when the world got around to devising solutions Cambridge would shine. Focusing on solutions was the freeing move for the lecture series, since we could invite presentations on topics ranging from energy to urban planning, diplomacy to environmental economics. The science of climate change, though never absent, receded into the background, and my role changed from presenter to moderator. The Center for Science and Policy joined Christ's as a cosponsor, and Rob Doubleday, CSaP's Director, used his extensive contacts in business and government to help recruit speakers,[141] most of whom came from Cambridge itself or government agencies in

[140] My brother John, who was slowly succumbing to the same bladder cancer that I had survived, asked if he could hear the lectures. I set my portable computer in front of the big screen showing my slides and beamed a video to him via Skype. He was my most perceptive critic.

[141] A list of the lectures may be found in the CSaP website.

London. There was no shortage of speakers, the Cambridge seminar ferment having produced many people in many departments who were thinking about solutions. The Cambridge audience proved eager to learn how its government's agencies were thinking.

Emulation is the sincerest form of praise: Trinity College started its own more technology-oriented series under Hugh Hunt, and Hugh and I decided to divide Lent Term between the two colleges, so there is now a Cambridge Climate Lecture Series that runs throughout the entire Lent Term. When Frank Kelley was about to step down as Master of Christ's, to thank him for his support and to encourage the continuation of the Christ's lecture series, Ellen and I arranged a gift to Christ's endowment whose income could, for example, pay for post lecture receptions or travel expenses for speakers from abroad. We have made donations every year since.

**Figure 37-4: A dinner at Christ's College, Cambridge in February 2023 to celebrate the tenth anniversary of my lecture series. Seated around the table from left to right are: Jonathan Gillard, Kate McNeil, Jane Dominey, Richard Mortier, Dame Theresa Marteau, Rob Doubleday, Sir Partha Dasgupta, Frank Kelly, Mike Housden, Lord Rees, and Richard Banks.**

Remembering my conversation with Vice-Chancellor Alison Richard following the 2007 C. P. Snow lecture, I chose to call the 2016 Christ's Climate Change Lecture Series, Bending the Curve. The term of art, Bending the Curve, meant doing what is necessary to get the rising curve of carbon dioxide emissions from fossil fuel burning to bend downwards. Bending the Curve was the name of the University of California's climate action plan that had been developed by a committee of about fifty faculty from all ten campuses and four national laboratories. The committee had been convened by

University of California President Janet Napolitano and chaired by my Scripps colleague, Veerabhadran Ramanathan. I secretly hoped that hearing about the University of California's action plan would advance the climate debate within the University of Cambridge.

How will Cambridge, the greatest university for science in the history of Western civilization, choose to address what UN Secretary General António Guterres called the defining issue of our time? Martin Rees led a vigorous debate among the audience after the final lecture of the 2016 Christ's College series on what the town and University of Cambridge could do about climate change. I perceived from the audience's responses that people who had yet to come to grips with the climate crisis were beginning to formulate specific views. In the next two years, I invited my UCSD colleagues Veerabhadran Ramanathan and David Victor to give the final lectures in the Christ's College series. Their lectures were successful in different ways. Ram spoke on humanity's ethical obligation to protect the world's three billion most vulnerable from the impacts of climate change; when he spoke of his grandmother's life and his daughter's work in villages in India, he held the audience in the palm of his hand. If anything, David Victor's was even more successful given Cambridge's technical orientation. David spoke with good sense and great clarity about what will be required to elicit an international diplomatic response to the climate challenge broad enough and large enough to cope with the climate crisis. David's observation that a new technology can unravel diplomatic snarls was like catnip to the science and technology wing of the Cambridge community.

I continued to watch the Cambridge community make up its mind about what to do. I was invited to more and more seminars. I was accosted one day on the street by a passionate group of students who wanted me to sign a petition asking the University to divest its fossil fuel investments. I responded that I was a guest of the University and preferred not to sign, but that I agreed that universities have a special responsibility to lead by example. The divestment issue proved to be provocative as is often the case when money is involved.

In October 2017, Cambridge University named its 346$^{th}$ Vice-Chancellor, Stephen Toope, from the University of Toronto's Munk School of Global Affairs and Public Policy. Toope had indicated serious interest in climate change and the University commissioned a planning study for his arrival on campus. My Christ's colleague and former Pro-Vice-Chancellor for Research, Ian Leslie, was a member of Cambridge's planning group. Ian had attended many of the Christ's climate seminars and had given several. The Christ's seminars may be what prompted him to visit UCSD to see what we were doing about climate change. I worked with my Scripps successor once removed, Margaret Leinen, to arrange a week's appointments with UCSD faculty and administration for Ian, who visited in May 2018. On my next visit to Cambridge in January 2019, I attended Toope's Vice-Chancellor's Lecture, which was given by Lord Nicholas Stern, the principal author of the pioneering 2006 UK government review of the economics of climate change. In his introduction to Stern's lecture, Toope announced that Cambridge University would commit its academic resources to finding solutions to the climate crisis. He promised he would frequently include climate change in his speeches. The University would of course seek to manage its estate (campus) sustainably.

A university-wide academic and outreach initiative took shape around the diplomatic commitments that the UK and other like-minded countries had made to arrive at zero-carbon emission economies by 2050. It is now clear that only major transformation of the global economy can keep the growth in global temperature below $2^{o}$ C, one definition of the threshold of intolerable risk. There are many ideas on how to get started, but only a few are ready for implementation. The "Cambridge Zero" initiative announced by Toope provides a multi-disciplinary forum in which the policy relevance, economic potential, diplomatic implications, and social feasibility of research related to the zero-carbon emission economy goal can be identified, evaluated, coordinated, and communicated.[142]

Emily Shuckburgh, from the British Antarctic Survey and Darwin College, is Cambridge Zero's first Director.[143] In January 2020, Emily invited me to a small meeting in Christ's College's Mountbatten Room with Simon Sharpe, the UK government's principal officer responsible for defining the UK position at the next UN Conference of the Parties (COP 26). Simon earlier had been a

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[142] In 2020, Toope announced that Cambridge would remove fossil fuel investments from its endowment portfolio, something my *alma mater*, Harvard, has now agreed to do.

[143] Stephen Briggs and I had earlier worked with Emily on a brief article about climate change indicators: E. Shuckburgh, C. Kennel, C. Rapley, D. Victor, and S. Briggs, "Focus on Temperature Ignores Other Ways to Take the Planet's Pulse, *The Conversation*, https://theconversation.com/focus-on-temperature-ignores-other-ways-to-take-the-planets-pulse-72701.

speaker at one of the Christ's College seminars. COP26 was to be held in Glasgow in November 2020 but was postponed to late 2021 because of the pandemic. The host country, in this case the UK, frames the agenda of a COP meeting on its territory. France had played a very strong role in the COP21 meeting in Paris, and COP26 was to evaluate the implementation of the Paris agreement, so Britain wanted to play a similarly strong role for COP26. At this meeting with Simon Sharpe, it was decided that Cambridge Zero would support Simon's efforts to develop a UK position for Glasgow. A month later, Emily invited me to a Cambridge Zero organizational meeting. I saw the product of the seminar ferment of the preceding years in the diverse approaches that the faculty brought to the task.

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I had played an imperceptible role in nudging Cambridge opinion along, but I was proud nonetheless, and especially proud when I was asked for valedictory comments at the conclusion of Emily's Cambridge Zero meeting. I said I had wished for something like this to happen for a long time.

The first time I tried to promote a university-wide commitment to solving the climate change problem was in 2001. Each of us who sees something wished for finally happen remembers the moment we first nudged the process along. We should not exaggerate the importance of that one moment or imagine that this particular nudge triggered the avalanche. Thousands of such moments precede big social decisions. But it is not wrong to savor the memory.

My memory is of having been the only scientist on the founding board of the California Climate Action Registry, which was created by the State of California in October 2001. The idea was that industries would voluntarily submit their annual greenhouse gas emissions to the Registry for certification; it was thought that state government regulations would eventually be imposed, and the Registry could provide a state-validated emissions baseline for industries to measure their performance against. My nudge to UCSD came at a meeting of its Vice Chancellors' Council, where I proposed that UCSD be the first university to join the California Climate Action Registry. The Council's initial reaction was discouraging—only the Dean of the Rady School of Management was receptive, while the others thought that UCSD was growing so rapidly that our carbon footprint would be embarrassingly large. They feared that if the world learned the size of our footprint, there would be pressure to limit our growth. To its very great credit, UCSD did join the California Climate Action Registry a couple of years later and became the leader in developing the University of California's climate change plan, called Bending the Curve.

# 38

# Risk Convergence, RACC[144], and Knowledge-Action Networks

As of 2022, the STS Forum had sponsored fourteen annual RACC adjunct meetings that convened as many as fifteen invited speakers and attracted up to 170 participants. The RACC International Advisory Committee (RACC/IAC) became particularly aware of the convergence of climate change, ecological, public health, and human security risks during the COVID-19 pandemic that started in 2020. A billion animals fled Australian wildfires: climate change triggering ecosystem disruption. Refugees fleeing floods in Japan crowded into shelters where they risked COVID-19 infection: climate change exacerbating public health risk. In August 2022 the

[144] STS leadership had asked my RACC successor, Gordon McBean, to resign for reasons obscure to me. Dan Goldin had also been alienated by what he saw as Mr. Omi's inflexible direction of STS, and Dan and I both resigned from the RACC international advisory committee (IAC). RACC thrived in our absence. Ismail Serageldin succeeded Gordon McBean as Chair of the RACC International Advisory Committee (RACC/IAC). In a significant innovation, Serageldin initiated RACC satellite meetings whose outcomes would be reviewed at the annual STS/RACC meetings in Kyoto. Serageldin was succeeded as RACC/IAC Chair by Jim Falk, whose specialty is international environmental governance. After 2013, I managed to put Kyoto out of mind until Jim invited me to rejoin the RACC International Advisory Committee in 2019. Jim has worked far more effectively than I had to build RACC's influence within the STS organization.

Mississippi River flooded, leading to the loss of a water treatment plant: a climate change impact compromising water security. It was no consolation to learn the misery of the populations living along the Mississippi River was shared by many more millions of poor whose homes, health, and livelihoods were washed away by the flooding of Pakistan's own Mississippi, the Indus River, on the same day. In 2023, wildfires in Quebec, a province in eastern Canada, caused air pollution that degraded visibility throughout the northeast US and health emergencies in New York and Philadelphia. RACC/IAC feared that the events making the news represented precursors of more troubling convergence events to come. The impacts of climate change may have been primarily affecting local communities, I thought, but the costs of climate change are beginning to draw on significant national and international resources.

In March 2020, as I left Cambridge prematurely because of the pandemic crisis, Cambridge's Centre for Science and Policy recorded an interview that may be the first public exposure of the risk convergence and Anthropocene Crisis themes. I proposed to Jim Falk that risk convergence be the theme of the October 2020 RACC meeting, drawing on the Anthropocene Crisis ideas I had been developing in Cambridge and in California. The RACC-12 international advisory committee took up the convergence theme; its declaration reads, in part:

> ...we are at a potentially pivotal moment in human history. It [2020] is the year when humanity is experiencing global systemic crises resulting from converging impacts of climate change, biodiversity loss, pollution, inadequate global health infrastructures, and stark inequalities. Failure to respond effectively earlier and faster to these is now beginning to appear as worsening systemic challenges to global, regional, and local security.[145]

I later proposed to Jim Falk that the principal speaker for RACC-13 be Partha Dasgupta, who was leading a UK Treasury study

[145] J. Falk, R. Colwell, A. El-Beltagy, P. Gleick, C. Kennel, Y. T. Lee, A. Luers, C. Murray, I. Serageldin, K. Takeuchi, and C. Watanabe, "Beyond 2020: Converging Crises Demand Integrated Responses," *Sustainability Science* 16, no. 2 (2020): 691-93.

seeking to integrate the economic value of nature's services into national accounts and cost-benefit analyses. (Present economic methods do not place adequate value on nature's services or else we would not protect nature as poorly as we do.) The UK Treasury study was the capstone of Partha's life work; he was in great demand and would be hard to get as a speaker. Jim asked whether I would invite Partha personally; here is an excerpt from my e-mail to Partha of June 10, 2021:

> We have designed RACC-13 partly because of your work. We will look at how to prepare for the impending convergent interaction of climate change and ecological disaster risks. We intend to emphasize in our 2021 declaration that population growth is the underlying driver of both. We believe your present work will eventually provide governments and other organizations a methodology with which to decide on action involving biodiversity issues. We hope to help put the climate and ecology issues on a par in international policymaking.

RACC-13's consensus statement followed up by advocating that governments integrate their human security and environmental sustainability agendas. It reiterated that the risks to Earth's climatic, ecological, and social systems are likely to converge to a comprehensive crisis within the lifetimes of children now living.[146] Governments will have to extend their security responsibilities beyond the military and economic, and the international climate change and biodiversity regime complexes[147] will have to find ways to collaborate with those for resource security to carry out the integrated human security and environmental sustainability agenda.

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[146] J. Falk, F. Attig-Bagar, R. Colwell, S. Behera, A. El-Beltagy, J. von Braun, P. Dasgupta, P. Gleich, R. Kaneko, C. Kennel, P. Koundouri, Y. Tseh Lee, A. Luers, C. Murray, C. Lal, I. Serageldin, Y. Sokona, K. Takeuchi, M. Taniguchi, C. Watanabe, and T. Yasunari, "Addressing Our Planetary Crisis: Consensus Statement from the Presenters and International Advisory Committee of the Regional Action on Climate Change (RACC) Symposium Held in Conjunction with the Kyoto-Based Science and Technology in Society (STS) Forum, 1 October 2021," *Sustainability Science* 17, no. 1 (2021): 5-7.

[147] See. R. O. Keohane, and D. G. Victor, "The Regime Complex for Climate Change," *Perspectives on Politics 9*, no. 1 (2021): 7-23.

In 2022, as wildfires swept California and Portugal, the world saw frequent reminders on television of the convergence of health risks from particulate air pollution with climate risk. Likewise, there were the impacts of flooding and drought on water security in America and Pakistan. RACC-14's declaration read in part:

> Stand-alone risk management strategies will become increasingly ineffective. Recent experience suggests that nations need now to work towards a much more comprehensive human security and sustainability action program addressing a complex system of multiple risks simultaneously. To avoid converging catastrophes, we must confront multiple challenges with cross-disciplinary tools, transforming our societies in multiple ways and on multiple scales: locally, regionally, and globally.[148]

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The 2022 United Nations Climate Change Conference (specifically, the Conference of the Parties of the UNFCCC, more commonly referred to as COP27) was held from November 6-18, 2022, in Sharm El Sheikh, Egypt. On November 8, 2022, the Japanese delegation to COP27 sponsored a special presentation on RACC's theme of converging and cascading risks.[149] At RACC's foundation in 2009, RACC-1 had advocated the creation of specialized Knowledge Action Networks that link international experts to regional and local decision-makers to promote coping capacity at the community level. The 2002 RACC/IAC chair, Jim Falk, had been a member with me of several Venice Sustainability Panels, which were, effectively, pilot Knowledge Action Networks. Below is the text of the concluding power-point slide of my COP27 contribution, the first version of which went back to RACC-1 in 2009 (with adjustments made to the images).

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[148] J. Falk, F. Attig-Bahar, R. R. Colwell, S. K. Behera, A. S. El-Beltagy, J. von Braun, P. Dasgupta, P. H. Gleick, R. Kaneko, C. F. Kennel, and P. Koundouri, "Addressing Our Planetary Crisis," *Sustainability Science* 17, no. 1 (2022): 5-7.

[149] http://copjapan.env.go.jp › cop27 › details › sts-forum

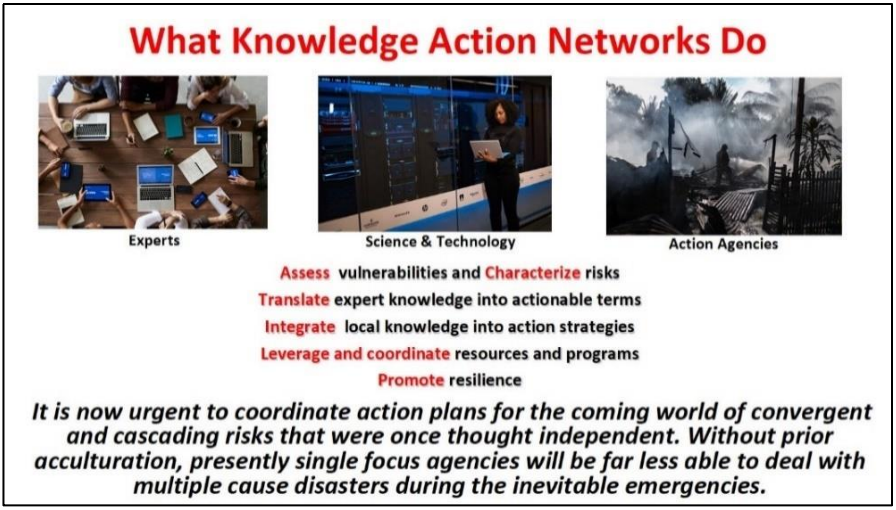


**Figure 38-1: The concluding slide in my zoom talk to COP27 in Egypt, part of the RACC International Advisory Committee's presentation in the Japanese Pavilion.**

The international debate about climate change and sustainability has evolved since RACC-1 in 2009. There is less public doubt about the reality of climate change; 2022's concatenation of disasters—if nothing else—has been persuasive. Responsible public media no longer accompany accounts of climate change with the political rages of denialists. (I sometimes think the denialists were the first outside the science community to see how big climate problems will become.) The principal challenge to the science and technology community is no longer seeking unchallengeable evidence for the reality of climate change but is shifting to the broader and more complex challenge of designing and promoting solutions. It is in RACC's area, adaptation, that the STS Forum will come into its own, for it, virtually unique among global fora, can convene the variety and diversity of leaders with the breadth of cultural background, expertise, influence, and often the formal responsibilities, to carry out the world's sustainability agenda.

# 39

# The Anthropocene Crisis


**Figure 39-1: This is the first slide of the talk I gave at my 50th Harvard reunion in 2009, and to Ellen's 50th at Vassar in 2016. I reminded these audiences of some words and phrases that had entered public discourse since they had been undergraduates, and, to convey the idea that these words pertain to issues affecting everyone on earth, showed a global picture of city lights at night synthesized from images taken from space. The burning of fossil fuels had made our planet a little like a star—it produces its own light. Human civilization has altered the face of the entire planet.**

I was finally doing what I should have done all my life. Freed of worldly ambition, I could at last become the scientist I wanted to be. In the sweet silence of earliest light, I could feel the long, beautiful thoughts reverberating, dancing, shimmering, daring me to catch them in words. Blind John Milton had something thing similar happen to him when writing *Paradise Lost*. Milton composed poetry in the waking hours before dawn and then waited with extreme impatience for the arrival of an amanuensis to "milk him" of the words painfully crowding his mind. I too awoke—in Milton's old undergraduate college in Cambridge no less—with words crowding my head and impatience I imagined to be like his.

**Figure 39-2: A portrait of the young John Milton (1608-1674) in the collection of Christ's College, Cambridge**

Issues that began to haunt me after Hannes Alfvén awakened my social conscience were causing my case of word congestion. My delight in purging the clamoring words into a computer did not mean the result was good. I still was unable to add substance to the views of the protagonists in my internal debate, Partha Dasgupta and Veerabhadran Ramanathan. I could match neither their imagination nor rigor; I could only tell them I was having a hard time framing their views as one unified picture. Should I have asked each to name the largest threat to life on the planet, Partha would have answered species extinctions, and Ram, climate change. The disjunction between these two extraordinarily accomplished scientists' urgencies weighed heavily on me; it should be obvious that civilization will not long endure unless both climate change and species extinctions are stabilized. My worry is that survival may require both be done at the same time.

Ram and Partha's home disciplines, climate science and ecology, have different evidential bases, conceptual structures, social organizations, educational requirements, and primary social actors. They originated in different places, at different times, for different reasons, and follow different paths towards different goals. Climate change presented first as a scientific problem with societal implications, and species conservation as an ethical challenge with scientific requirements. Their solutions are not obviously synergistic: achieving a zero-$CO_2$-emission economy is not sufficient to preserve species from extinction, and saving habitats for endangered species does not persuade industries to avoid emitting $CO_2$. Nonetheless, the two research communities give respectful, if occasionally mystified, lip service to the urgencies of the other.

Martin Rees is the referee of my inner debate. Martin's is an astrophysicist's view of what humanity might make of itself, given an astronomically long future.[150] He can imagine the odd possibilities as well as the arcane risks that could materialize—some created by science and technology themselves. Martin, a polymath with a taste for the big picture, has been warning of the systemic crises about to befall human civilization for at least two decades, having gone on record in 2003 with a book apocalyptically entitled *Our Final Century* in the UK and *Our Final Hour* in the US.[151] Martin and I warmed up our many discussions in Cambridge and America by exchanging US and UK science gossip—always fascinating—but then we devoted a surprising amount of our time together to the implications of science for the future of humanity. I listened as he polished the insights in his 2018 book, *On the Future: Prospects for Humanity*.[152] As Martin thought aloud about the fascinating possibilities of the long future, I remember thinking to myself, *but first...but first...we have to get to your long future.* I did not know what to call Martin's apocalypse at the time, but now I do: *the Anthropocene Crisis; get through that then I am freed to worry about your long future.*

Should anyone ask what I was doing during those early morning hours in Cambridge, tell them I was hearing voices: the

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[150] In co-founding the Centre for the Study of Existential Risk at Cambridge, Martin acted on his perception that society has made itself vulnerable to mortal risks of global scale. Science and technology, while making daily life safer and more prosperous in the short term, are increasing the scale of the catastrophes that can occur given enough time. The Centre for the Study of Existential Risk was founded in the hope that very large—existential—problems could be spotted in gestation before they reach the crisis stage.

[151] M. J. Rees, *Our Final Hour: A Scientist's Warning: How Terror, Error, and Environmental Disaster Threaten Humankind's Future in this Century—on Earth and Beyond* (New York: Basic Books, 2003).

[152] M. Rees, *On the Future: Prospects for Humanity* (Princeton, NJ: Princeton University Press, 2018).

voices of Ram, Partha, and Martin. All three would agree that humanity is on a calamitous path—many of the calamities of humanity's own making—but each of them pictures different calamities. I was not getting from my early morning *seance*s with their specters a picture of what their calamities add up to.

**Figure 39-3: Two heroes in my personal scientific pantheon, Sir Partha Dasgupta (left) and Veerabhadran Ramanathan (right) in conversation at the University of Southern California on the occasion of Partha's award of the 2016 Tyler Prize for environmental achievement. Ellen's and my friend Giri Ramanathan, stands in the center. Photo by Ellen Lehman.**

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Paul Crutzen—Nobel laureate, co-leader of INDOEX with Ram, visiting professor at Scripps—made the word *Anthropocene* famous.[153] To Crutzen, *Anthropocene* meant the human species has

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[153] P. J. Crutzen, "Geology of Mankind," in *Paul J. Crutzen: A Pioneer on Atmospheric Chemistry and Climate Change in the Anthropocene,* eds. P. Crutzen and H. G. Brauch (Cham, Switzerland: Springer, 2016), 211-15; P. J. Crutzen, "The Anthropocene," *Journal de Physique IV* 12, no. 10 (2002): 1-5; P. J. Crutzen, "The Anthropocene," in *Earth System Science in the Anthropocene*, eds. E. Ehlers and T. Kraft (Berlin: Springer, 2006), 13-18.

already transformed the surface of the earth so completely that future geologists will view our time as a new era in earth history. Like the other transitions between geological eras, ours is accompanied by massive extinctions that we humans have been largely helpless to reverse.[154] Exactly when we developed the capacity to alter the face of the earth is lost in scholarly debate about when and where in prehistory humans began to shape the ecologies surrounding their settlements. The name *Holocene* has been given to the past ten thousand years of unusually stable climate when agricultural revolutions started in several parts of the world. The Anthropocene Era, however, is *not* the agricultural revolution globalized. Industrial society has disrupted the balance of bio-geophysical processes that stabilized the Holocene Era's hospitality to primitive agriculture. The Anthropocene Era has a different dynamic, one that makes contemporary human civilization the major agent of global change.

The Anthropocene Era can be said to have started when humans' local ecological transformations merged to achieve global scale. Will Steffen holds that this epochal development occurred around 1950 when "the great acceleration" of energy and natural resource consumption after World War II pushed several natural systems dangerously close to their planetary boundaries.[155] In 2009, the Stockholm Resilience Centre proposed a planetary boundaries framework for gauging how close the planet is to comprehensive crisis. The Centre identified nine boundaries where complex eco-environmental systems cross thresholds, "tipping points," where nonlinear, functionally irreversible changes in biophysical systems can be triggered. Steffen and his coauthors estimated that four of the nine planetary boundaries have been crossed already. By this count we are about halfway through the Anthropocene Crisis.

Learning something immediately useful is not the main reason one should look for antecedents of the Anthropocene Crisis in the anthropological record. That might, however, help us understand whether a planetary crisis necessarily accompanies the evolution of

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154 S. Pimm, "Biological Extinction at the Vatican," *Nature Ecology & Evolution* 1, no. 5 (2017): 136; P. Dasgupta, P. Raven, and A. McIvor, eds., *Biological Extinction: New Perspectives* (New York: Cambridge University Press. 2019).

155 W. Steffen, W. Broadgate, L. Deutsch, O. Gaffney, and C. Ludwig, "The Trajectory of the Anthropocene: The Great Acceleration," *The Anthropocene Review* 2, no. 1 (2015): 81-98.

intelligent life. The Center for Academic Research and Training in Anthropogeny (CARTA), a collaboration of UCSD and La Jolla's Salk Institute to "explore and explain the origins of the human phenomenon," invited me to co-chair seminars in 2015 and 2022. The first was entitled *Human-Climate Interactions and Evolution: Past and Future,* and the second, *Human Origins and Humanity's Future: Past, Present, and Future of the Anthropocene.* 2022's symposium, a tribute to Paul Crutzen, featured a dream list of speakers covering how humans became capable of dominating a geological era from the widest point of view, including the evolution of the human brain, the origins of agriculture, the spread of infectious diseases, ant societies, ocean life, and African wildlife conservation.


**Figure 39-4: Opening the CARTA conference on human-climate interaction, May 15, 2015.**

If the origins of Anthropocene dynamics are fuzzy, it is clear the political issues created by them began their move to the center of international policy debate as the Cold War was winding down. *Our Common Future,* widely considered the foundational document defining sustainability,[156] was published in 1987, the

[156] World Commission on Environment and Development, *Our Common Future*, (New York: Oxford University Press, 1987).

Intergovernmental Panel on Climate Change was established in 1988, and the Berlin Wall came down in 1989. These were pivotal years when global environmental disaster joined global nuclear disaster on the list of mortal risks requiring the attention of international science and diplomacy.

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The term "Anthropocene Crisis" came to me during a 2018 flight from San Diego to London. I found the crisis notion perversely encouraging because crises don't last forever. The present threats to society are immense but finite, not a condition of continued existence. I returned to Cambridge thinking that it might be time for another meeting like the 2014 meeting of the Pontifical Academies of Sciences and Social Sciences, this time to integrate the thinking about the Anthropocene Crisis. I replayed my La Jolla conversations with Ram to Partha and Martin in Cambridge, and we started an email dialogue with Peter Raven, Partha's co-chair of a 2017 Pontifical Academies workshop on extinctions. Eventually six of us, Partha Dasgupta, Veerabhadran Ramanathan, Peter Raven, Martin Rees, David Victor, and I, proposed a book project to the American Academy of Arts and Sciences. I became amanuensis for this group, which proposed that the American Academy devote a multi-author volume of its journal, *Daedalus*, to the coming planetary crisis.

We titled the proposal "The Anthropocene Crisis." We argued that instead of the climate change, biodiversity, resource availability, pandemic, and pollution crises unfolding on parallel paths, global society will face them as a single crisis. It will not be given the luxury of dealing with the crises sequentially. Each, considered separately, is driven by collective human action and each extrapolates to mortal crisis. The fact that all are emerging at the same time suggests but does not prove there may be an overall common causal cofactor. However, starting with Malthus, people would remark with varying degrees of cogency that these trends are paced by a combination of the size of the human population and the intensity of human activity. If so, each threat ought to maximize sometime during the peak population era of the global demographic transition now in progress.

National populations, while growing earlier in many if not most countries, are now in decline in China, Japan, South Korea, and parts of Europe.[157] The planet is in the middle stages of a demographic transition. Vulnerability to climatic, ecological, and environmental (and other) disasters could conceivably grow until the demographic transition has run its course and population declines in nearly all countries. The great ecologist, E.O. Wilson, told me of his fear that the peak population era will be an ecological bottleneck; those ecosystems that survive it set the initial conditions for biological evolution in the post-crisis world. The bottom line is this: when the world is the most crowded with potential victims is when their risks will be largest. Moreover, their risks are most likely to interact at that time; people will have difficulty deciding which risk to deal with first. The international community needs to prepare in advance for risks that interact lest the different risk management communities lose the capability of concerted action. Without prior acculturation among crisis managers, opportunities for cooperative action can be lost in disciplinary territorial squabbles. Even if leaders can find ways to cooperate during a crisis, stressed crisis managers on the front lines can have difficulty responding to the modes of thought of counterparts they have not worked with before.

The American Academy organized planning discussions but decided not to proceed with the proposed volume. In retrospect, my proposal write-up was too vaguely grandiose to be practical; I did not activate little details that spur action in others, and I lacked the persuasiveness to bridge the working styles of natural and social scientists who had achieved prominence by pursuing disciplinary rigor as the one trustworthy route to knowledge. But sometimes you find a useful little nugget on a battlefield strewn with the detritus of defeat. I extracted from the wreckage of the American Academy proposal an essay about the convergence of the risks of climate change, biodiversity, and resource depletion. This essay, *The Gathering Anthropocene Crisis*[158], started in Cambridge and concluded in California during the pandemic, would have set the scene for the other articles in the proposed *Daedalus* volume.

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[157] World Population Review [https://worldpopulationreview.com/country-rankings/countries-with-declining-population] viewed 26 Jan 2023.

[158] C. F. Kennel, "The Gathering Anthropocene Crisis," *The Anthropocene Review* 8, no. 1 (2020): 83-95.

Writing these memoirs reminds me how dependent I am on the intellectual frameworks and emotional attitudes of colleagues. After about three months of helpless dependence on television news about the world during the pandemic, I finally realized that Zoom teleconferencing technology would come to the rescue of people like me, though it could not cure the sick and feed the poor. The American Academy fiasco need not mark the end of my participation in intellectual life. The preparatory work for it need not go to waste; The *Gathering Anthropocene Crisis* was completed in pandemic solitude during sunny California mornings. After that, there were two issues that I could work on during the pandemic. First, what is the evidence that Anthropocene risks are actually converging? This I could pursue in Zoom conversations with RACC's International Advisory Committee. The second issue Martin Rees and I had been circling about for the past fifteen years—how global society could aggregate the motivation to act on what science sees ahead. Below is an excerpt of an e-mail I sent to Martin in early summer 2022:

> I hope to persuade you to co-author this synthesis of the many discussions we have had over the past 15 years on climate change and sustainability. The attached essay contains many ideas of yours that I have internalized so completely that I mistake them for my own.... You have already seen several earlier drafts, whether in La Jolla, Cambridge, or Philadelphia. Each time...you encouraged me...to publish, and each time I did not.... There was something...I was not seeing. Things finally came together just before I left Cambridge [in 2020].... Internalizing the notion of a coming crisis that has not yet materialized...was a key psychological step for me.... At last there was a framework that would enable completion of the orphan article!

Our essay, published by the American science commentary journal *Nautilus*, began with the following sentiments:

> The word, *Anthropocene,* implies a material causal relationship (hazily articulated at present) between what global society does and how planetary eco-environmental systems evolve. Human communities

> have modified their local environments since before the Agricultural Revolution, but ours is the first century in the twenty thousand that the human lineage is recognizable in the fossil record that human actions determine the fate of the entire biosphere. Now our task is to exercise planetwide a familiar form of local agency. Could planetary agency be exercised in time, a tragic outcome would not be inevitable. Even the possibility that human agency has planetary scale means that Anthropocene thought has an ethical dimension—what global society *chooses* to do shapes the eco-environmental systems that sustain later generations.[159]

How can global society exert its planetary agency? When we ask what moves large populations to action or leaves them in inaction, we need to look for what is common in the ways large numbers of people think. Human beings have hearts and minds, ids and egos, unconscious and conscious thoughts. This is not a new idea. Ancient Greek philosophers, Shakespeare, and Freud understood that emotion and rationality compete for human attention. The dual nature of human thought has found institutional expression in Western societies in bicameral legislatures and the separate authorities of church and state.[160] What is new is that modern research in psychology and neuroscience has established a species-wide basis for this ancient insight. *Every* human on earth has two systems of neural circuitry; Daniel Kahneman calls them levels one and two.[161] Level one processes the wants, fears, and perceptions of the daily present, while level two organizes experience into concepts that can guide future action. It takes psychological energy to raise percepts in level one into concepts in level two, and consonance of levels one and two to overcome the inertia to act. People do not

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[159] C. Kennel and M. Rees, "Two Distinguished Scientists on How to Rescue Humanity," *Nautilus*, August 19, 2022, https://nautil.us/two-distinguished-scientists-on-how-to-rescue-humanity-238535/.
[160] George Washington is said to have explained to Thomas Jefferson that the Senate is needed to soften the passions of the popular assembly, much as a saucer is used to cool overheated coffee.
[161] D. Kahneman and P. Egan, *Thinking, Fast and Slow*, (New York: Farrar, Straus, and Giroux, 2011).

always do what is rational;[162] action requires harmony of heart and mind; otherwise level one urgency distracts level two intention. Therein might be found the essence underlying our otherwise inexplicable inertia in facing up to the mortal threat of the Anthropocene Crisis. It is only human to resist risking daily wellbeing to protect unborn strangers of other nationalities and races in a distant and unknowable future.

[162] R. H. Thaler and L. J. Ganser, *Misbehaving: The Making of Behavioral Economics* (New York: W. W. Norton, 2015).

# 40

# From the Berlin Crisis to the Anthropocene Crisis

From the moment I learned to read the newspaper as a toddler, I was aware that World War II made the world I was born into. My two uncles and my father were away from home because of it. When I was six years old, I learned that a handful of nuclear scientists had miraculously brought the fighting to an end not with guns, but with the products of their minds. In my latency, photographs of mushroom clouds made by scientist's hydrogen bombs conveyed what the B-36s, B-47s, and B-52s, tiny specks in the sky above New Hampshire, could do. I first saw the bombers at Camp Tabor in 1948, the year of the Berlin Crisis. Even then I knew that enough of them could destroy civilization. From then on, the hot war that nobody dared start framed my country's outlook.

The Cold War created all my early professional opportunities. My first full-time job was with an industrial laboratory specializing in the aero-physics of missile re-entry, the Avco-Everett Research Laboratory, in 1960-61. My one and only postdoctoral appointment was with the UN's International Centre for Theoretical Physics in Trieste, Italy, a city suspended between sides in the Cold War. A year after returning from Italy, I left Avco for an academic position at UCLA in 1967. I made my name in space plasma research at UCLA and TRW. I was to consult for almost twenty years with TRW Systems, which built satellites for the space program, America's gladiator in the Cold War arena.

Soviet Russia had a mysterious attraction; trips there validated me as a member of the international fraternity of physicists. I got to witness Cold War diplomacy first-hand. I went to Moscow in 1977 at a moment when the US State Department informed my NASA delegation that America had “absolutely no interest” in improving relations with the Soviet Union—that non-progress in our Shuttle-Salyut negotiations would “send a message” to the other side. Later visits to the Soviet Union helped me see the human cost of the Cold War. I saw the Soviet regime impoverish the material, intellectual, and spiritual lives of colleagues who, but for a geographical accident of birth, could have been me. Much as I disagreed with Ronald Reagan’s political philosophy, I saw the truth of his view that Stalin had created an “evil empire”. Many aspects of Stalinism endured; my Moscow colleagues used to joke that the gangsters who once infested government transferred their predations to the private sector after the Berlin Wall came down. Gangsters were the first Russian beneficiaries of Western liberalism.

Scientists on both sides had been working to end the Cold War. Hannes Alfvén had awakened my conscience, but my every day exemplars were the co-chairs of the Trieste plasma workshop, Marshall Rosenbluth and especially Roald Sagdeev, Mikhail Gorbachev’s college classmate. Roald and Marshall were to find occasions for intimate conversations that combined plasma physics and international relations. Roald had mastered colloquial American English without losing his Russian accent—perfect for the mission he was on. He was a master of nonlinear plasma physics, but that was only the beginning. He made jokes in English. His English impressed colleagues, charmed celebrities, and fascinated reporters; besides, he could tame commissars in Russian. It was a thrill to watch him shuttle between the Soviet Union and America during the years of *glasnost* and *perestroika*. Wherever he went, Roald personified hope for a resolution of our suppressed mortal conflict; *Newsweek* and *Time* touted Sagdeev as an avatar of a peaceful future. His marriage to an American princess, Dwight Eisenhower’s granddaughter Susan, seemed to seal the deal. I was infused with an unworldly sense of benign power when in Roald’s presence.

Roald Sagdeev was not the only scientific agent of political change in *perestroika* Russia. I knew another of Soviet Premier Gorbachev’s scientific ambassadors, Evgeny Velikhov, also a plasma physicist. If anything, Velikhov had more influence with the Soviet bureaucracy than Sagdeev. I saw photographs of an alert Velikhov helicoptering through the Chernobyl radiation cloud during the frantic effort to entomb the melted carcasses of Chernobyl's failed reactors. My ordinary friends in Moscow were seeing history change in less dramatic but more significant ways. I too watched the programs obsessing about Stalin's crimes on Moscow TV. Instead of bored passengers on the Moscow subway staring at my Canadian snow boots (Sorels), I saw animated passengers openly reading Solzhenitsyn. My grandiosity told me I was witnessing the transformation of humanity's future on this planet. My never-satisfied New England conscience asked *what had I really done to help besides witness*?

I do not know whether Roald Sagdeev was as surprised as I by how quickly climate change came to rival arms control as an international science issue. The Berlin Wall came down in 1989, the Soviet Union broke up in 1991, and the UN Framework Convention on Climate Change was signed in 1992. By 1994, when Dan Goldin recruited me to NASA to work with the Earth Observing System, a realignment of US security priorities was under way. I was to see educated people who had lived every day with Cold War perils weep in relief that it was over. I attended a celebration in a Virginia hotel where analysts from US and Soviet intelligence agencies shared Cold War reminiscences. Had I chosen to do what my thesis advisor, Ed Frieman, had urged me do in 1964, I could have been on the stage in Virginia reminiscing along with them in 1994. As it was, I was a NASA participant in a CIA group, *Medea*, convened to recommend which old satellite reconnaissance photographs and measurements of sea ice thickness from nuclear submarines be released to unclassified groups seeking to document the changes in climate since the beginning of the satellite era. By bringing post-Soviet Russia into the International Space Station consortium and building the Earth Observing System, my NASA boss, Dan Goldin, was helping to engineer the post-Cold War rebalance in international security priorities. In giving me the opportunity to oversee the redesign of the Earth Observing System, Dan gave me privileged insight into the scientific, engineering, and organizational challenges—and necessity—of maintaining climatic situational awareness.

Cold War experience conditioned how relations between the climate science community and national governments were expected to work out. It seemed natural to seek the solution to the climate problem in the same places the tensions of the Cold War problem had been managed—technically advanced situational awareness,[163] scientific advance, and diplomacy. Governments on both sides had acted promptly on the emergence of new nuclear knowledge during the Cold War, and I thought then that intergovernmental agreements that mandated international action would be sufficient for climate change. Similarly, I imagined governments would act expeditiously on each advance of climate knowledge, even though climate change commanded no arsenals of nuclear weapons to project urgency. *Once ordinary people saw what could happen, their governments would do the right thing;* this naïve presumption conditioned how climate science chose to speak to governmental power. By the time I got to NASA in 1994, the climate policy community had structured this approach to its knowledge-to-action challenge, and governments were trying to follow through.[164] NASA would use its Earth Observing System to anchor an international effort to produce environmental situational awareness, and the United Nations had already created the Intergovernmental Panel on Climate Change (IPCC) to inform diplomatic action. It seemed self-evident that when the risks of climate change became obvious to ordinary citizens everywhere, the world would take action.

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When I first attended the Kyoto Forum on Science and Technology in Society in 2006, the constructive reaction of the California Legislature to SIO's forecasts of major water shortages created by climate change had persuaded me that climate knowledge needs no extra push to inspire climate action. The year 2006 was an interesting time to think about climate change. Its knowledge-to-action proposition was about to change; its research and policy community was beginning to lose faith that greenhouse gas emissions would be reduced fast enough to avoid major impacts of climate change; it was preparing to admit the necessity to adapt. In 2007, a report to the UN Commission on Sustainable Development contained an oft-repeated phrase in its title:

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[163] *viz.,* the Test Ban Treaty observing network and the Earth Observing System

[164] I tried to convey my appreciation of what was done in talks to the Department of Social Anthropology in Cambridge, see: C. F. Kennel, "Afterword: Speaking Scientific Truth to Power," *The Cambridge Journal of Anthropology* 31, no. 1 (2013): 150-55.

*Avoid the unmanageable* (*i.e.*, mitigate), *manage the unavoidable* (*i.e.,* adapt).[165]

Some colleagues saw *manage the unavoidable* as a needless admission of defeat, but I saw it as opening a new frontier. One thing I asked myself was whether the IPCC assessment model would serve as well for adaptation as for mitigation. The impacts of global climate change on regional ecologies and local communities would be the central issue: adaptation's knowledge-to-action proposition would be more complex than mitigation's. Climate action would require not only the large-scale economic and industrial measures central to slowing global warming, but would also introduce ecological and environmental requirements that call for different scientific expertise and touch upon different ethical and social values.

The newly comprehensive formulation of the climate change problem meant that a whole-of-diplomatic-community approach would be needed, not just negotiations among the handful of principal greenhouse gas malefactors. Few climate scientists suspected that a whole-of-society—or even a whole-of-human-psychology—approach would be needed. The founders of IPCC presumed that national governments would reach the basic decisions for mitigation through diplomacy, as with arms control. However, the failure to implement the 1997 Kyoto Protocol made it seem unlikely the world could avoid the need for adaptation. Local communities had thus to look forward to making important adaptation decisions by themselves. They needed highly individualized and regionally specialized knowledge. Not having experienced the anxieties of those who lived every day with cold war threats, they needed to be persuaded to use the knowledge. That there were many more mayors than presidents raised a capacity question in my mind: How can the climate science and policy community, already stretched thin for mitigation, meet the adaptation needs of tens of thousands of communities in hundreds of culturally and environmentally distinct regions around the globe?

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[165] R. Bierbaum, J. P. Holdren, M. MacCracken, R. H. Moss, P. Raven, U. Confalonieri, J. Dubois, A. Ginzburg, P. Gleick, Z. Khatib, J. Lough, A. Mathur, M. Molina, K. Mshigeni, N. Nakicenovic, T. Oki, H. Schellnhuber, D. Ürge-Vorsatz, *Confronting Climate Change: Avoiding the Unmanageable and Managing the Unavoidable*, (United Nations Foundation, 2007).

In 2008, Dan Goldin and I proposed to the Kyoto Forum on Science and Technology what became its annual adjunct meetings on Regional Action on Climate change (RACC). We argued that the assessment procedures adopted by IPCC could not be carried over to adaptation without modification. The IPCC had been established in 1988 and the Berlin Wall came down a year later, 1989, the same year that Tim Berners-Lee invented the World Wide Web. Thus, IPCC's initial operational concepts had been conceived before the globalization of internet culture. At NASA in the mid-1990s, Dan and I had seen that small improvements in internet practice influenced the design of the Earth Observing System, and I wished the same were true for climate change. In particular, good communication between the global science and technology community and regional and local decisionmakers would be required for adaptation.

When RACC started to meet in Kyoto, the World Wide Web was beginning to deliver the volume and variety of expert knowledge required by the thousands of communities faced with serious adaptation challenges. The knowledge production functions of the UN's Global Climate Observing System and the knowledge translation functions of IPCC were ready to be integrated into a web-enabled Knowledge Action Network. RACC suggested as a first step that local communities convene small Knowledge Action Networks comprising international scientific experts and local decision makers[166] to reduce the hesitancy to act when scientifically naïve leaders are confronted with impersonal recommendations.[167]

IPCC assessments were doing what they were designed to do: provide trustworthy knowledge in support of intergovernmental negotiations on targets for reductions in greenhouse gas emissions. IPCC assessments will always be essential but were not regionally specific enough to deal with local adaptation challenges; moreover, they did not appear frequently enough to promote agile responses to disasters and other rapidly emerging threats. Community resilience

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[166] C. Kennel and S. Daultrey, *Knowledge Action Networks: Connecting Regional Climate Change Assessments to Local Action*, (San Diego, CA: UCSD Sustainability Solutions Institute, 2010), retrieved from https://escholarship.org/uc/item/8gd6j0k5.

[167] In serving on a succession of such Knowledge Action Networks for the city of Venice, Italy, I was to learn how important it is for scientists to understand the local decision making culture, something you cannot learn from a distance. Often the decisionmakers themselves have an interest to keep their decision culture obscure to outsiders.

would be strengthened by an always-on knowledge management service that local leaders can rely on when unfamiliar threats arise. Embedding in internet practice the kinds of integration and assessment processes done in person every seven years for IPCC would create a knowledge management service that local and regional decision-makers could consult at times and places of their choosing.

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In 2014, the Pontifical Academies of Science and Social Science convened a study week on the scientific, social, and ethical dimensions of what Paul Crutzen and Eugene Stoermer had called the Anthropocene Era.[168] The workshop's name, *Sustainable Humanity, Sustainable Nature: Our Responsibility,* embraced all three dimensions of Anthropocene thought. It was co-chaired by Scripps climate scientist Veerabhadran Ramanathan and Cambridge economist Sir Partha Dasgupta, my personal climate change heroes. My Vatican presentation proposed working towards a global knowledge action network to support adaptation to climate change and resilience to ecological compromise, as well as greenhouse gas emissions reduction. I was beginning to think about how to go beyond climate change to support the sustainability agenda[169]. I failed to persuade the august personages in attendance to give much thought to the notion of a global Knowledge Action Network for sustainability. Scripps' great oceanographer, Walter Munk, complimented me on the passion with which I delivered my paper, but passion without precision only goes so far with such an audience. I failed to win over the most ambitious, highest level, and most interdisciplinary conference I ever attended. It may also have been the most influential, for when else will a Pope devote an encyclical (*Laudato Si'*) to its ideas?

I had to leave the Vatican meeting a day early to co-chair the first US-China Academies' Workshop on Space Science. On the long

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[168] Crutzen, Paul J., and Eugene F. Stoermer. *The 'Anthropocene'(2000)*. Springer International Publishing, 2021.

[169] C. F. Kennel, "Global Knowledge Action Network," in *The Proceedings of the Joint Workshop on Sustainable Humanity, Sustainable Nature: Our Responsibility, 2-6 May 2014*, eds. P. Dasgupta, V. Ramanathan, and M. Sorondo (Vatican City: The Pontifical Academy of Sciences, 2015), 347-69.

flight from Rome to Beijing, I pondered what I had heard. Humanity's crisis had more, and more intertwined, dimensions than I had thought; climate change's impacts on agriculture and food security had aggravated, but did not originate, the ancient challenges of social inequality and economic poverty, but it was through things like its impact on water and food security that climate change would have its destabilizing social impacts.

**Figure 40-1: Two heroes in the world's struggle to subdue climate change. In front are my friend and mentor, Veerabhadran Ramanathan (left), who was co-chairing the great 2014 Vatican conference with another in my scientific pantheon, Partha Dasgupta, and Pope Francis (right), who composed the encyclical *Laudato Si'* having the discussions at our seminar in mind. This photograph was taken at an informal gathering in front of the Vatican Guest house.**

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I went on finish *The Gathering Anthropocene Crisis* (almost) on my last visit to Cambridge. When I found Heathrow, the busiest airport in Europe, deserted in late February 2020, I realized there was something to the pandemic rumors. I left Cambridge early, staying only to wrap up conversations with Martin and Partha before I left for California. I arrived in San Diego on March 11, 2020, the day the US announced it would close air travel to Europe. Ellen met me and we repaired to our mountain home in Ramona, far from the madding crowd. She and I have been sequestered there, and in La

Jolla, ever since.[170] It took a while to figure out how to spend the hours of enforced professional solitude. My New England Puritan wannabe mother would have said it was too showy, but I opted to write these memoirs. My excuse was that I had been an above average witness to the transformation of international science as the Cold War wound down and climate change warmed up.

[170] Ellen and I did not visit Alaska in October 2020, the first year since 1989 that both of us missed a meeting of the Alaska Federation of Natives. We did return in 2022.

# 41

# Epilogue

I was about thirteen. It was one of those luminous afterschool afternoons. I went upstairs to where I knew I would find my mother on her bed. Our day had opened with the news that the United States completed a series of nuclear tests and an image of a mushroom cloud over the Pacific was flashing on and off in my mind. It was a pretty big series of tests, I said to her; I hope they got what they wanted. Normally that would have been enough distancing from reality and we would go on to gossip about my school day, but today was different.

*You have no idea how those things changed our lives*, my mother said. You cannot imagine what it had been like to feel absolutely free. We Americans thought we could do anything we wanted when I was growing up. America emerged from World War I the world's leading financial power; we could keep the world's troubles an ocean away. The last war taught us otherwise; your Uncles Bill and Jim were caught up in it and your father left us to go to Lockheed in Dayton and California. You and I moved in with my parents in Abington. That was the world you grew up in. Now, those awful nuclear things will finish off the process. We cannot move without jostling Russians. Some stupid mistake could get us all killed. You will never know true freedom.

◆❖◆

I never thought Robert Oppenheimer over-wrought when he quoted from the *Bhagavad Gita* after witnessing the nuclear explosion he helped create:

*Now I am become death, destroyer of worlds.*

Marshall Rosenbluth revealed his own existential despair in the plaintive irony of his voice. I cannot decide whether he was the way he was because he had done the numerical computations that enabled the first test of a hydrogen bomb at Eniwetok, or whether it was because he could not singlehandedly achieve the promise of peaceful thermonuclear fusion power. I shared Roald Sagdeev's effervescent—almost manic—joy, that he, a plasma physicist, could help his college classmate, Mikhail Gorbachev, end the Cold War. Princeton's Jerry Mahlman's forecast of climate change in 2200 exploded like a nuclear bomb inside my head, which is why I was to compare climate change to a hydrogen bomb explosion—slower moving but equally deadly.

My life has not been blighted by existential despair as Marshall's was, but I approach its end with the sense that the big things are undone. No matter how good a mind I had, I could not save my mother from alcoholic dissolution or my first marriage from legal dissolution. I could see the Anthropocene Crisis coming but not how to head it off. What I do see is enough to induce intellectual despair, for the coming crisis has its roots in the very thing that gives us joy as humans. The social cooperation that builds community and culture also transforms habitats and the climate. Our minds have evolved that way; social cooperation is the secret of our success as a species. The disciplined cooperation that gives science its power has now to figure out how to keep less disciplined humans from doing what comes naturally. It is natural as one ages to regret things left undone and friends soon to be left behind, but my generation's particular curse is not to see how our children survive a planetary crisis made in our own image.

Old age will soon return me to the silent safety of the unknowable oblivion whence I came. Right now, I still am afloat with eyes, ears, and nose just above water. I still can breathe, but now there are times when I gasp for breath. I can still hear the voices of the others floating on Homer's wine dark sea. The sea is quiet for the moment, and we in the floating commentariat sense the silent current moving us from the deep below. We have been in distant conversation for some time now; *where is the current is taking us?*

Voices I once heard clearly are now faint, as from over the horizon. I call out and responses from familiar voices are fewer but others have joined in and now there are more. I cannot repress the urge to cry out to my unseen companions: *Stop your chattering for a moment and listen. Can't you hear the surf crashing on the reefs ahead?*

*No man is an island,*
*Entire of itself.*
*Each is a piece of the continent,*
*A part of the main...*
*Therefore, send not to know*
*For whom the bell tolls,*
*It tolls for thee.*

John Donne

# 42

# Acknowledgments

I have learned that wealth is measured by one's debts to others. While those acknowledged below (and countless others) have contributed much to my life, they did not contribute to the errors of fact and perspective that have crept in to this account. Certainly, none are Tim DeBold's fault; Tim, a sensitive and intelligent editor, has among other things prepared the manuscript for publication by University of California e-Scholarship. He reorganized my words into an order more accessible to the general reader. Ryan Hearty, a graduate student in the history of science at Johns Hopkins University, interviewed me for an oral history on behalf of the American Institute of Physics on December 19, 2019; its transcript is available on the AIP website. I undertook these memoirs in part because I regretted not having given the topics covered in Ryan's interview more thorough consideration; I feared that in my spontaneous hurry I had not been fair to people to whom I owe much. A transcript of Sarah Johnson's October 20, 2002, interview on behalf of the NASA History Office is also available on the Internet. The Cambridge Centre for Science and Policy arranged for an interview just before I left Cambridge on March 11, 2020, because of the Covid-19 pandemic. This interview is also available on the internet.

I have had the life privilege of working in five quintessentially great universities, each special in its own way. In the order I encountered them, they were Harvard, Princeton, UCLA, UCSD, and Cambridge. The University of California, of which UCLA and UCSD are branches, has so much been part of my life that it has receded into the background of consciousness. It is time to call it out for proper acknowledgement. It is entirely fitting that the University of California be the e-publisher of these memoirs, though I am only one of hundreds of thousands who make the University what it is. I want to thank Amy Butros and Allegra Swift of the UCSD library who led me to Tim DeBold, my editor, who has guided me through the mysteries of the University of California's e-publication process as well as made many stylistic improvements; they are the last of many from the University who gave me help I could not give myself.

In midcareer, when my principal challenge was harmonizing the work of others, my dithering threatened to impede their progress. Here the situation was frequently saved by Lisa Shaffer, who had been the most effective person on my staff at NASA and came to SIO when I did. I know nobody else who can act on new information more expeditiously and more effectively than Lisa. Her straightforward efficiency created fear in those unsympathetic to what we were trying to do. Finally, as the organizational complexities of my jobs grew beyond my capacity to cope, I came to depend on the people on staff at NASA, UCLA, and SIO, who did cheerfully and competently what I would have done resentfully and badly. I appreciate each one of you. Working with you gave me some of my most pleasurable moments. I only wish a grand reunion were possible. Two of you have become Ellen's and my lifelong friends, Melissa Licker from the UCLA Physics Department, and Kay Marie Moreno from the Scripps Director's Office. Lately, hearing from my former UCLA PhD students has become another joy of this academic's old age.

I have asked colleagues to review parts of these memoirs. Doug Bennett, who also worked like Lisa Shaffer both at NASA Headquarters and at Scripps, and Lisa reviewed Chapters 25-29 where work at the Scripps Institution of Oceanography is taken up. I have shared Chapters 31 and 32 with my mentor and exemplar in climate science, Veerabhadran Ramanathan. Here I also acknowledge Partha Dasgupta, whose perspective on the relationships of ecology and economics, along with Ram's views on climate change, got me to worry about the Anthropocene Crisis. After sharing spacecraft launch anxiety with Heinz Stoewer, it was somewhat easier to share Chapter 33 with him. Heinz, now of the International Council of System Engineering (INCOSE), and his wife, Ingrid, have become lifelong friends of Ellen and me; we see one another virtually yearly at one of our homes in Munich, Holland, or California. David Victor of the USCD School of Global Policy and Strategy gave me special insight to the intricate world of climate diplomacy. Ismail Serageldin, of the Library of Alexandria, and Jim Falk, of the University of Melbourne, reviewed what is written about RACC, a group they were instrumental in founding and are now sustaining. Peter Cowhey, former Dean of the USCD School of Global Policy and Strategy (and my successor as Chair of the Board of the California Council on Science and Technology) and Amber Mace, CCST's Chief Executive Officer, reviewed what is said about CCST. I shared the sections on the Anthropocene Crisis with Jim Falk of the University of Melbourne and Rob Doubleday of the University of Cambridge. I asked UK Astronomer Royal, former Master of Trinity College, Cambridge, former President of the Royal Society of London, and good friend, Lord Martin Rees, to share with me his views on just about every topic in science. We touched on every aspect of the Anthropocene Crisis that we could think of in numerous conversations.

I have been inconsistent in providing birth and death years for those who have passed on. My criterion, inconsistently applied, was to do so for senior scientists whose examples were important to me in one way or another. I hope the friends of the slighted and those who believe my choices not objective will forgive me.

The biggest debts, like mine to Martin, can never be repaid. I hope my thesis advisor, Ed Frieman,[171] is in a place where he can read my life tribute to him and hopefully forgive me for not repeating it here. Dan Goldin's forceful approach to all he does, especially his acute realism, made transitioning from twentieth-century Cold War space research to twenty-first-century global warming research seem the right thing to do. He also made it possible to do. It is now about thirty years since I left space science, and the passage of time, not to mention fading memory, leave me with an uncharacteristic sense of clarity about my earlier career,

[171] C. F. Kennel, "Edward A. Frieman: 19 January 1926 – 11 April 2013," *Proceedings of the American Philosophical Society* 160, no. 3 (2016): 283-93.

the kind of sense that is unburdened by details that once seemed compelling. Space science took up a whole generation of scientists' attention, but there were two who made my world of space plasma physics come alive, Harry Petschek and Roald Sagdeev. It was a joy to explore that world with Ferd Coroniti.

And then there is Ellen. Her presence has given life to the last half of my life, just as her photographs lend color to the last half of this account. Without obvious preparation, she intuitively gives her pictures (and my life) symmetry and balance.

# If the globe hasn't been warming, why are we experiencing so much climate change?


## Charles F. Kennel

**Scripps Institution of Oceanography, University of California San Diego**

**Christ's College and Centre for Science and Policy, University of Cambridge**



The title page of my talk at Charlie Fest 2014. I would not have done it, but Ellen pushed me to it. I had not celebrated my 60th and 70th birthdays in a big way, she said; when, oh when, was I going to do it if not for my 75th? She had a point, so she and I spent the spring and summer planning it. Scripps lent us the use of its wondrous seaside forum for two days of talks in areas of science that I had worked in or with-space science, fusion plasmas, astrophysics, earth observations, oceans, biology and conservation, climate change, energy and sustainability, science and technology policy, and university administration. To my great astonishment and honor, forty-six of my colleagues from the US, UK, and Germany chose to spend Labor Day weekend in La Jolla. It was wonderful to see them again. They gave wonderful talks to an audience of several hundred friends, family, and colleagues. And Ellen outdid herself; dare I say it? She planned four days of lunches and dinners, and kayaking, snorkeling, and visits to the Birch Aquarium at Scripps for those so inclined.

When it was over, I said to Ellen that there would never ever, ever, again be four days like those that had just passed into memory.

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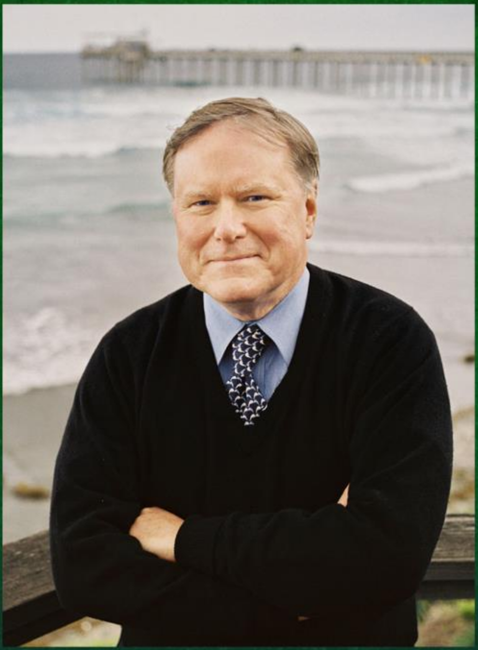

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**Charles F. Kennel was educated in astronomy and astrophysics at Harvard (AB) and Princeton (PhD). After a postdoctoral year at the International Centre for Theoretical Physics, Trieste, he joined the UCLA Physics Department and its Institute for Geophysics and Planetary Physics. There he pursued research and teaching in space plasma physics and astrophysics, eventually chairing the Physics Department. He served as UCLA's Executive Vice Chancellor, its chief academic officer, from 1996 to 1998. While at UCLA, Kennel was a consultant to TRW Systems (now Northrop Grumman Aerospace Systems), where he participated in numerous satellite experiments, most notably the Voyager missions to the outer planets. From 1994 to 1996, Kennel was Associate Administrator of NASA for Mission to Planet Earth. Kennel's experiences at NASA converted him to earth and climate science, and he has devoted the rest of his career to these fields. He became the ninth Director of the Scripps Institution of Oceanography and the Dean and Vice Chancellor of Marine Sciences at the University of California, San Diego, serving from 1998 to 2006. Kennel is presently Director and Distinguished Professor *Emeritus* of the Scripps Institution of Oceanography and Vice Chancellor of Marine Sciences *Emeritus* at the University of California, San Diego, and a Fellow Commoner at Christ's College and at the Centre for Science and Policy at the University of Cambridge, UK**

**Kennel has been a Fulbright Senior Lecturer, a Sloan and Guggenheim Foundation Fellow, a Fairchild Scholar at Caltech, and a visiting professor at the Princeton Plasma Physics Laboratory. In the international arena, he had visiting appointments to the International Centre for Theoretical Physics (Trieste), the University of Trieste, École Polytechnique (Paris), the Space Research Institutes in Moscow (IKI), and São José dos Campos, Brazil (INPE). During winter terms 2007, 2010, 2012, and 2014-2020, he was a Distinguished Visiting Scholar at Christ's College, and the Centre for Science and Policy at the University of Cambridge. He was the 2007 C. P. Snow Lecturer at Christ's College.**

**Kennel served thirteen years on the NASA Advisory Council, chairing it from 2000 through 2005, and was a member of the Presidential ("Augustine") Commission on human space flight in 2009. Kennel chaired the NRC Space Studies Board from 2008 until 2014. Earlier, he chaired the US National Academies' Board on Physics and Astronomy, Committee on Global Change Research, Fusion Science Program Assessment Committee, Committee on Solar and Space Physics, and he co-chaired its Beyond Einstein Program Assessment Committee. For the State of California, he was a member of the founding board of the California Climate Action Registry, the first Chair of the California Ocean Sciences Trust, and Chair of the Board of the California Council on Science and Technology.**

**Kennel is a member of the US National Academy of Sciences, the American Academy of Arts and Sciences, the American Philosophical Society, and the International Academy of Astronautics. He is a fellow of the American Physical Society, the American Geophysical Union, and the American Association for the Advancement of Science. He has been awarded the James Clerk Maxwell Prize (American Physical Society), the first Hannes Alfvén Prize (European Geophysical Union), the Aurelio Peccei Prize (Accademia Lincei, Rome), the NASA Distinguished Service Medal, and two NASA Exceptional Public Service Medals.**