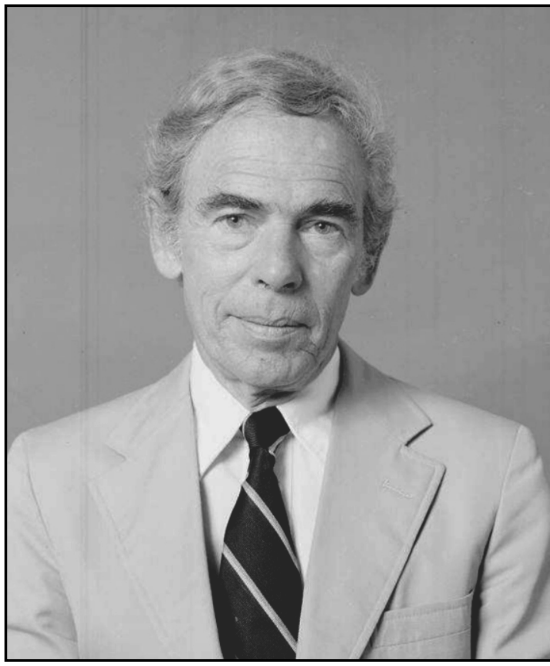
EDWARD A. FRIEMAN



SCRIPPS INSTITUTION OF OCEANOGRAPHY, UC SAN DIEGO

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IT IS HARD TO GET A MEMORIAL RIGHT, especially for someone who was instrumental in your own life. None of us ever sees the whole picture. We form a construct in our minds from a few emotionally salient incidents—the ones salient to us. We connect these dots. The longer we have known the person, the more the salient incidents stand out. The more important the person has been to our lives, the less we want to rethink, for that could be unsettling. The lazy part of our mind is content to let our mental picture rest safely where it is.

I began to get a rounder picture of my teacher and colleague Ed Frieman during his memorial service at the Scripps Institution of Oceanography. There I realized how much I had missed. Different people saw different facets of his life. It was richer than I thought, and I thought I knew him well. The historical Frieman began to emerge in fuzzy outline. Yet his memorial service did not tell me what I now wish I had known. I began to feel that even together we, the ones asked to speak, could never see the whole arc of Ed Frieman's uncommon journey through life. We saw his world as we saw him, and the important thing is to see his world as he saw it, his life as he lived it.

My apologies, but that must be left to the persistent dispassion of the biographer.

What I can do is start by relating three incidents salient to me that, taken together, tell me how uncommon Ed Frieman's journey through life really was. To do that, I will also have to bring up some of my own background, for which I hope I might be forgiven. It would be better if I could keep the focus entirely on Ed Frieman, but I cannot tell my story without burdening the reader with my solipsism.

I have had chalk thrown at me three times in my life. The first was in Newton, Massachusetts, when I messed up a calculus proof in front of my 11th grade math teacher, Mr. Taylor; the third time was in Moscow, when Rashid Sunyaev and I disagreed about some topic in astrophysics. Now, in retrospect, I still feel sheepish about Mr. Taylor and rather proud that someone as accomplished as Sunyaev felt so strongly about a disagreement with me. These were salient incidents, all right, but they did not change my life. The second one did. It was when Ed Frieman threw chalk at me in Princeton, New Jersey, in 1961.

Ed was my thesis advisor. He taught me to calculate. In fact, he was legendary for his calculations. His hand flew over the chalkboard—sometimes two together—as he laid out before us students long, complex equations and reduced them term by term to one whose answer suddenly appeared obvious. These calculations related to the nonlinear statistical mechanics of collisionless plasmas and the instabilities besetting them. Whenever life threatened to be boring in those days, Ed pulled out a yellow pad and calculated away, on trains, on long airplane flights; others remarked on this propensity—one that only another theoretical physicist could really appreciate. Anyhow, one day, trying to be Ed Frieman, I was filling the blackboard in his office with a thicket of equations from which there appeared to be no way out. That was when Ed threw the chalk at me. "Goddammit, Charlie, I thought you were better than the others. You are trying to solve everything at once; pick one thing and focus on it." I was making the typical graduate student mistake; the important thing is that he knew how to make me sit up and take notice. He had a talent for that.

It was around this time that Ed subjected me to another tough examination, but on a different topic. I had taken a year off after my first year in the Department of Astrophysical Sciences to go back to my hometown, Boston; the Department arranged for me to work at the AVCO-Everett Research Laboratory, then one of the pioneers of plasma physics. AVCO did not know exactly what to do with me, so Harry Petschek put me to work reading the experimental and theoretical literature relating to the then newly discovered Van Allen radiation belts. There wasn't very much to know then, so the task was relatively easy, and Harry and I soon began theorizing. Ed knew this, and it appeared he wanted to know what we knew. The general tenor of his questioning was that some day people would learn how to inject energetic electrons directly into the radiation belts: did anyone know how long they would last once put there? I answered that I thought they would not last very long (despite the fact that Alfven's adiabatic invariant theory suggested that once injected they would remain essentially forever) since there were these plasma instabilities that would grow and scatter the particles out of the belts and into the atmosphere. Ed probed round and round this issue, but eventually he relented, apparently satisfied but still skeptical.

Shortly after our Van Allen Belt talk, Ed went away. I didn't pay much attention; he went away frequently, and besides, I was preoccupied with paring down my thesis and, along with it, my ambition. I did notice when he came back, though; he had a suntan—in winter in Princeton, no less. When I asked him where he had been, he said he couldn't tell me. A few more months passed, and I forgot about his suntan. Then on 9 July 1962, the United States exploded a 1.4 Megaton nuclear device, Starfish Prime, at 248 miles altitude over Johnson Island in the Central Pacific. People couldn't miss it—it lit up the night sky above Honolulu and, inevitably, was reported in the newspapers. Space scientists weren't going to miss it either, as an airplane flying at the Starfish magnetic conjugate point in the Southern Hemisphere photographed an aurora; particles had gotten into the radiation belts.

Now I can tell you, Ed said. Remember when the prominent British astrophysicist A. C. B. Lovell had complained that U.S. nuclear explosions might change the earth's radiation belts forever? President Kennedy, who was worried about public fallout, had appointed a special group, with Ed as a 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 the advice I had given him played any role in the Starfish group's deliberations, but I always wondered. And I came to feel guilty about it. For after the initial burst of aurora, the remaining 1.2 MeV Starfish electrons stayed and stayed; eventually they went away but only a decade or so later.

What had gone wrong with my prediction? Eventually, in the course of another investigation with my own graduate student, Richard Thorne, I figured out what had happened. The Starfish device was exploded at low latitudes, and the magnetic field lines on which it occurred did not extend far enough into space to expect natural instabilities to occur; the waves that were generated by instabilities further out could not get to the Starfish lines of force. As the Starfish electron belt slowly went away, so did my guilt.

This is the second of my salient Ed Frieman events. Keep in mind that Ed, at age 35, found himself at the heart of the Cold War scientific establishment, advising a president. He was already a founding member of the JASON group, created by the President's Science Advisor, James R. Killian, in 1960 to advise the government on high technology and national security. He kept his association with the JASONs for the rest of his life.

Now let us fast-forward 40 years to my final salient event. In 1999, Ed Frieman was the one and only chair the National Research Council's Board on Sustainable Development ever had. The Board, whose vice-chair was the distinguished geographer Robert Kates, had just released one of the most influential reports the U.S. National Academy has sponsored in recent times, *Our Common Journey.* The product of three workshops, two summer studies, and much internal debate, this book-length report set forth a research agenda that could enable a start on fulfilling the great vision Gro Harlem Brundtland had expressed in *Our Common Future"* in 1987, wherein the term "sustainability" was given the simple definition in current use today.

The ideas set forth in *Our Common Journey* were an illumination. They guided me throughout my later career. Let me give you an idea of what Ed's panel thought about. I quote and rearrange excerpts from *Our Common Journey's* executive summary:

> We are in the midst of a transition to a world in which human populations are more crowded, more consuming, more connected, and in many parts of the world, more diverse, than at any time in

> history. Current projections envisage population reaching around 9 billion people in 2050 and leveling off at 10 to 11 billion by the end of the next century . . . . Can the transition to a stable human population also be a transition to sustainability, in which the people living on earth over the next half-century meet their needs while nurturing and restoring the planet's life support systems? . . . . A remarkable number of efforts have grown up around the world over the last decade that have succeeded in putting sustainability issues on the global political agenda—and in beginning the actual search for specific pathways toward sustainability in many local contexts. If, at the close of the 20th century, the end of our common voyage toward sustainability has not yet been charted, much less brought into sight, the journey has at least begun.
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> In recent years, the science and technology community has not been a particularly prominent participant on this journey . . . . Major recent innovations have come in the realm of policies and institutions, rather than knowledge and know-how. Relatively little progress has been made in developing a scientific understanding of the obstacles facing any transition to sustainability, the technological opportunities for pursuing this goal, or the use of modern sensing and information systems for providing navigational aids along the way . . . . Thus, we approach the 21st century with less than might be hoped for in the way of a useful strategic appraisal of how the knowledge and know-how most crucial to successfully navigating the transition toward sustainability is to be identified or of how the capacity to create the needed science and technology is to be developed and sustained . . . . (This) report . . . suggest(s) how the science and technology enterprise can increase society's chances of undertaking and achieving our common journey of a transition toward sustainability.

So here is my fundamental question: How did Ed Frieman navigate his uncommon journey from the center of Cold War scientific power to intellectual leadership of the central issue facing global civilization in the 21st century? How did he make a successful transition from the Cold War to a global world when so many of his scientific peers did not? What happened along the way? What intellectual and spiritual journey corresponds to the milestones we see from the outside?

This is where Ed needs his biographer. I can fill in a few details, but they will not be enough. A more complete account of his professional commitments by Dennis Monday is accessible on the internet, but the longer and deeper treatment still awaits. I will continue with my personal impressions.

Ed was a New York City boy, 15 years old when Pearl Harbor occurred—too young to join the armed forces but just 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 assignment took him to the atomic weapons tests at Bikini Atoll in 1946. He was 20 years old. I do not know one scientist who was present 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, he did go into nuclear science, receiving an M.S. (1948) and Ph.D. (1951) in physics from the Polytechnic Institute of Brooklyn under the guidance of Lloyd Motz. His thesis topic was entitled *The Proton-Proton Reaction and Energy Production in the Sun.* He was in peaceful nuclear fusion research from the beginning. The topic *Energy Production* that appears in his thesis title was to dominate the first half of his scientific life.

Ed's thesis must have caught the attention of John Wheeler and Lyman Spitzer at Princeton, who, just as he completed his degree, were in the process of creating Project Matterhorn, whose goal was to achieve thermonuclear fusion for peaceful purposes. Ed left the City for bucolic Princeton, and in 1954, when Ed was 28, Spitzer asked him to head the theory division, a responsibility Ed still had when I joined him as a student. Those were the days 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 fully international effort in fusion research was made at the great Atoms for Peace Conference in Geneva in 1958, and shortly thereafter, Princeton converted Project Matterhorn into a research and teaching program within an expanded Department of Astrophysical Sciences. Ed was now a professor at a leading research university. 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's life took a big turn in 1979 when he left Princeton to become Assistant Secretary and Director of Research at the Department of Energy (DOE). Several people have remarked to me that even if his career had ended at this point, the honors he achieved— such as membership in the American Philosophical Society and the National Academy of Sciences—would have come to him anyhow. He 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—that provided a unified way to ascertain the basic stability of complex magnetic field and plasma configurations. Frieman, using his prodigious skill with long calculations, unified the field of plasma kinetics, using a sequence of reductions from the Liouville equation. This work did not make practical computations any easier, but it enabled people to see how the various techniques to describe Coulomb collisions, most importantly the fully collisionless approximation, emerge from fundamental principles. My impression was that he was seeking to harmonize complexity. My own thesis 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 also expanded his presence on the national science scene as a consultant for numerous government panels, the national laboratories, and industry; of particular relevance to me, Ed developed a relationship with NASA that endured until the end of his life. Ed's advice was wanted because people trusted his low-key rationality.

Frieman's brief 2 years at the DOE seem to me to be the pivotal point in his personal trajectory. He was by then 53 years old, prominent in his field of science and in advisory circles. It was time to seek broader horizons, time to have real authority, time to deploy his talents throughout science and technology. By then he was fully aware of the environmental impacts of energy projects, and he could not avoid being aware of the profound threat to the global environment from the use of fossil fuel energy. In July 1979, the National Academy of Sciences convened an *ad hoc* study group chaired by the MIT climate scientist Jule Charney, whose report "Carbon Dioxide and Climate: A Scientific Assessment" dramatically raised the visibility of the climate change issue in the high policy community. Ed made sure that the DOE developed a strong program in climate change science, one that continues to this day. But in his behind-the-scenes way, he also did something that we see now had large historical ramifications. At that time, the discoverer of the buildup of carbon dioxide in the atmosphere, Charles David Keeling, was having one of his frequent funding crises. Continuing the accumulation of atmospheric CO2 data was boring and seemed unproductive to funding managers whose job performance was measured by new results every 3 years. People who worked with Frieman at that time recall that he could not stop talking about the profound implications of the relentless growth of greenhouse gas concentrations. At least he could do something about Keeling's predicament. Ed commissioned a study by his JASON colleagues, whose authoritative support helped save that day for Keeling's work. The JASONs have played an important role in assessing earth and climate science ever since. Keeling's project continued to lurch from crisis to crisis until he died in 2005; it continues in the hands of his son, Ralph, and the National Oceanic and Atmospheric Administration. Keeling's work as it accumulated year after year changed the world, as Ed saw it would. It fell to me to preside over Keeling's memorial service; in my remarks, I ventured my opinion that there have been only three scientists in history whose *data* changed history: Johannes Kepler, A. A. Michelson, and Charles David Keeling. Keeling is in great historical company.

With the change of administrations in 1981, it was time for Ed to think of what to do next. He made what must have seemed to many to be an unconventional move, but which made sense for him. He joined a private company, the Science Applications International Corporation (SAIC), then a relatively small firm in La Jolla, California. SAIC deployed a cadre of exceptionally talented scientists as consultants on military and civil programs. It was a kind of private industry JASON. While Ed went on later to do other things, he never severed his relationship with SAIC. In his early years (1981–6), Ed, the former Navy diver, was instrumental in growing SAIC's undersea warfare business; when he returned full time to SAIC in 1996, he was Senior Vice President for Science and Technology, with oversight of new technologies and markets, special programs, sustainable development (*nota bene*), maritime business, space and remote sensing business, and international business. SAIC is now a multi-billion dollar corporation, which is a tribute to both the company and Ed Frieman.

A few years after Ed joined SAIC, William A. Nierenberg, a physicist and JASON colleague, announced he was to step down in 1986 as Director of the Scripps Institution of Oceanography in La Jolla. Bill had continued Scripps' involvement with climate science in his own way, insisting that the scientific underpinnings of climate research needed serious strengthening. Bill had also chaired a famous National Academy report on the climate problem in 1983. Now it was time to find a successor for a director who had served for an exceptionally long 21 years. Richard Atkinson, then Chancellor at the University of California San Diego, and soon-to-be President of the University of California, initiated a quiet search. Atkinson recalls that at first he was reluctant to approach Ed Frieman, his first choice, but Ed's friends told him that Ed was an academic at heart and might be persuaded to take a serious paycut. Ed did, and the rest is Scripps history.

It seems to me that all the threads of Ed's life came together in his Scripps directorship—energy, national security, the Navy, environment, climate change. He saw ahead of most others the way in which global events were unfolding. The Cold War was ending, and the problem of global sustainability was looming ahead. The scientists who had prospered in one political era were not necessarily going to do so in the next.

Scripps was, and is, dominated by a bunch of hardnosed experimentalists who enjoy watching ugly facts murder beautiful theories, in T. H. Huxley's words. Ed's job was not easy. How do you convince an institution already at the top of its game that the game is changing? Ed engineered the delicate task of steering Scripps toward sustainability while maintaining Scripps' close relationship with the Navy. He charted the course Scripps is on today. How he did so day by day has to be the topic of a more serious study, but one thing stands out: he brought the atmospheric physicist Veerhabradan Ramanathan to Scripps from The University of Chicago. In the fullness of time, this move led to the association of two Nobel laureates, Paul Crutzen and Mario Molina, with UCSD; to establishing UCSD as a world leader in the study of the interactions among physical and chemical processes in the atmosphere; and lately, to a new way of thinking about air pollution and climate change, which we are beginning to call the San Diego Synthesis.

The Scripps directorship gave Frieman a solid platform from which to work with national science policy at a new level. The record is full of minor and major boards and committees that he led or served on, but two stand out. In 1987, the United States decided to go ahead with the Superconducting Supercollider (SSC), which has proven to be the last great high-energy particle accelerator that the country would consider building. You can only imagine the jealousies besetting the mandarins of physics who were either jockeying to bring this multibillion dollar project to their locales or fighting it because they were afraid it would eat their own physics lunch. Whom do you think the Academy chose to chair the site selection panel? By now, you must know the answer: Ed Frieman. Ed managed to produce a report that the physics community backed well enough to get congressional approval. A few years later, the funding was withdrawn and the project was stopped in the midst of construction. The United States immediately lost international leadership in high-energy physics; U.S. leadership has not been recovered and probably never will be. But Ed helped take the SSC to the verge of success—before it fell off a cliff.

Ed's other great intervention in science policy during the time of his Scripps directorship was connected with climate change. This will get more attention here, in part because it was successful and in part because I was involved.

A devastating heat wave afflicted the United States in 1988. This extreme event raised a now familiar question: Was it due to climate change? The climate scientist James Hansen had said so in congressional testimony. The accompanying furore had a profound impact on the first Bush administration. By 1990, it had created the U.S. Global Change Research Program (USGCRP) and started NASA's Earth Observing System (EOS). EOS, in its first incarnation, was to be the largest science project ever conceived, even larger than the SSC. There were to be two immense shuttle launched spacecraft—EOS-A and EOS-B—each carrying an unprecedented 24 instruments, many of pioneering and untested design. Each spacecraft was designed to last 5 years and would be replaced twice. The total cost of the EOS program was about 17 billion 1990 dollars over 15 years.

In 1992, sanity began to set in. Wasn't there a cheaper way to get the multidisciplinary measurements required to deal with the earth system? Dan Goldin, then the new NASA administrator, and Ed Frieman thought they knew what the problem was. It was the huge spacecraft. They were very expensive to launch; moreover, their two-dozen demanding instruments were bound to create interacting requirements conflicts and, therefore, the virtual certainty of crippling cost over-runs. Could the 24 instruments be placed on a fleet of smaller, less complex spacecraft? To see if this idea was even feasible, Ed took on the chairmanship of NASA's EOS Engineering Review Committee, a kind of task that NASA very often reserves to itself. This committee found that if certain demanding requirements about simultaneity of measurements were relaxed, the earth system science requirements could be met with a fleet of much smaller and cheaper spacecraft. Ed skillfully persuaded the Congress that this was the right mission strategy even though some districts were going to lose large contracts. The next task was to reengineer the existing system design. Somehow, Ed convinced Dan Goldin that I, a physicist not an engineer who had never worked in government or earth science, could do this job. Even more amazing, they convinced me. In the event, we pulled it off. Dan provided me with first class technical and managerial support, and Ed had to come to my rescue only once, with another Academy Panel that he hosted at Scripps. The system that was eventually built was designed then. Its run-out cost was about 6 billion 1996 dollars. The EOS satellites are still flying, slowly aging.

Ed was quiet and unassuming, seeming happy just to be accepted by the big egos that dominate science. Some people have characterized him as "sweet" or "gentle." But Ed worked differently, confidentially, one-on-one, and it was there that the strength of his mind, his clear-eyed realism, and the extraordinary force of his will really came through. No one else of my acquaintance has understood the relation between science and politics better than Ed. He knew where power lived and how to use it. Unlike others who were bedazzled by their proximity to power, Ed was careful never to go beyond what the science community could support; when he walked into congressional offices, he brought with him the results of serious studies. Ed rarely was mistaken and quietly steered his way through controversies and conflicts that confounded many another.

Ed and his wife, Joy, maintained a wonderful apartment on the Ile St. Louis in Paris that they visited whenever they had a chance. When I was working in earth science for NASA, I met with Ed many times over dinner in Washington and Paris. Paris was in many ways the center of global environmental science—the headquarters of the European and French space agencies, the International Geosphere-Biosphere Programme, UNESCO, and many others. I had the distinct impression that the agenda for all of Earth System Science was being composed over many, many dinners in Paris with Ed Frieman, including the ones I shared with him. I now see that he was becoming altruistic, approaching a final synthesis of science, technology, policy, and humanitarianism that led directly to the Board on Sustainable Development.

Ed's was truly an uncommon journey. But after spotting some of the milestones on the path he chose, do I know more about his inner life? What made him happy? What haunted him? Was he elated when physics complexities resolved themselves on his yellow pads? Was his main ambition to be included in the circles of scientific prestige or influence? Knowing and being around leaders? Did he feel inwardly superior because he saw science and politics more clearly than anyone else he knew? Did he enjoy the quiet manipulation of power? Or did he simply enjoy seeing things work out, helping good but more helpless people prosper?

Like so many others, had Ed Frieman, age 20 at Bikini, been overwhelmed by seeing firsthand a nuclear explosion—one that drove deep into his being a desire to create a better world?

Elected 1990

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