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Citizen Scientist: Frank Von Hippel's Adventures in Nuclear Arms Control

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ABSTRACT
In this part, von Hippel starts by briefly describing his family background, his education and his pursuit of a career in theoretical physics. He then describes the beginning of his engagement with policy issues as a result of the student protests and activism while he was an assistant professor at Stanford University and a fellow at the University of California Berkeley during 1966-70. This resulted in the book, *Advice and Dissent: Scientists in the Political Arena*, co-authored with one of his students. A spin-off article in Science magazine on "*Public-Interest Science*" resulted in a one-year fellowship at the National Academy of Sciences during which he organized the American Physical Society's 1974 summer study on nuclear reactor safety, which facilitated his transition into a career in nuclear policy analysis at Princeton University.

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Overview

Frank von Hippel is a Senior Research Physicist and a Professor emeritus of Public and International Affairs in Princeton University's Program on Science and Global Security. He has a Ph.D. in nuclear physics from Oxford University (1962). He has worked on nuclear policy issues for almost half a century and has been praised by his fellow physicists "for his outstanding work and leadership in using physics to illuminate public policy in the areas of nuclear arms control and nonproliferation, nuclear energy, and energy efficiency" (American Physical Society 2010). The publisher of a collection of his essays, described him as "at the forefront of those scientists grappling with the troubled legacy of our Nuclear Age" and as offering "insights about the choices we must make and how science can help us to make them" (Springer Nature 1991).

The edited and footnoted interviews published here are von Hippel's account of his years of engagement with a series of nuclear policy problems – mostly as an outsider but also as a White House official. He began by trying to strengthen nuclear-safety regulation in the United States. He and his colleagues then fought successfully to stop the US Atomic Energy Commission's planned "plutonium economy." As the chairman of the Federation of American Scientists, he worked with a group of physicists advising Mikhail Gorbachev in his successful efforts to end nuclear testing, reverse the nuclear arms race, and

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dismantle the tank confrontation across the Cold War boundary dividing Germany. After the Soviet Union collapsed, von Hippel worked in the White House to help launch cooperative programs to secure nuclear materials in Russia from theft and to eliminate the stocks of highly enriched uranium and plutonium resulting from the drastic downsizing of the US and Soviet Cold War nuclear arsenals. In 2006, he co-founded the International Panel on Fissile Materials, which seeks to end the production and use of highly-enriched uranium and separated plutonium for both weapons and non-weapons purposes.

In a sense, von Hippel inherited the problem of nuclear weapons from his grandfather, James Franck, who participated in the US World War II nuclear-weapon project, overseeing the development of the process to separate plutonium from irradiated uranium. In the spring of 1945, Franck convened the group that produced the *Franck Report*, which became famous after the end of the war because it had warned of a nuclear arms race with the Soviet Union if the US used the new weapon in a surprise attack on Japan.

Von Hippel initially proceeded down the career path of a theoretical physicist until he was an assistant professor at Stanford. There, because of the Vietnam War and revelations that many consequential government decisions for technology had been based on political calculations rather than considerations of effectiveness or the public interest, he decided to change his career to focus on public policy. He and Joel Primack, a younger activist physicist, co-authored a book on the roles of scientific insiders and outsiders in influencing government policy. Von Hippel then opted for the outsider role for himself, starting in 1974 by organizing a study of reactor safety sponsored by the professional society of American physicists.

The impact of that study resulted in an offer to continue his research at Princeton. There, he learned from a colleague, Harold Feiveson, of the Atomic Energy Commission's (AEC's) proposal to base the world's future energy system on plutonium breeder reactors. India had just tested a bomb using plutonium that had been separated for supposedly peaceful purposes with AEC assistance. Feiveson, von Hippel and a third colleague, Robert Williams, organized a group that critiqued the AEC's proposed "plutonium economy." Their work helped provide the analytical basis for the Carter Administration's 1977 decision to try to shut down the program, which Congress finally agreed to five years later.

In 1979, von Hippel was elected chairman of the Federation of American Scientists (FAS), a group created in 1945 by veterans of the US World War II nuclear-weapons program to try to prevent a nuclear arms race with the Soviet Union. Two years later, the Reagan Administration took office asserting that the Soviet Union believed it could fight and win a nuclear war. The Reagan Administration asserted that the only way for the US to deter nuclear war was to develop a similar posture. The public's reaction was the Nuclear Weapons Freeze movement. Von Hippel joined the movement, focusing his efforts on ending nuclear testing and the production of "fissile" materials for nuclear weapons.

In 1983, in response to public concerns, President Reagan announced a program to develop a system to protect the US from a Soviet ballistic-missile attack – quickly labeled

"Star Wars" by critics. A group of Soviet academicians wrote an open letter to US scientists asking whether they had changed their view that the proposed ballistic missile defenses (BMD) could easily be overcome by countermeasures and would provoke an offense-defense arms race. The FAS leadership responded that it had not changed its views and was invited to Moscow to brainstorm with a small group of the academicians on how to end the nuclear arms race. Two years later, Mikhail Gorbachev became the leader of the Soviet Union and the FAS group learned that the Soviet academicians they had been brainstorming with were advising Gorbachev.

During the ensuing five years, von Hippel found himself and his colleagues swept up in a head-spinning series of Gorbachev initiatives to end the nuclear arms race:

- A unilateral nuclear test moratorium, verified by the US Natural Resources Defense Council (NRDC) because the Reagan Administration refused to reciprocate;
- A decision, after a public debate, that President Reagan's "Star Wars" initiative would collapse under its own weight (which it did), and therefore the Soviet Union could agree with the United States in the INF and START Treaties on the bilateral elimination of their land-based intermediate-range missiles and the withdrawal from deployment of half of their strategic warheads;
- The unilateral withdrawal of 5,000 Soviet tanks from eastern Europe, and an agreement to further Soviet reductions in the 1990 Treaty on Conventional Forces in Europe;
- "Glasnost" ("openness") visits to the Soviet Union's first plutonium-production city, its ballistic missile defense (BMD) test site, and demonstrations of the detectability of the radiation from a Soviet cruise-missile warhead on a Soviet missile cruiser in the Black Sea off Yalta; and
- Just before the Soviet Union's disintegration in 1991, denuclearization of the Soviet Army and withdrawal of nuclear weapons from all Soviet surface ships in parallel to similar actions by the United States.

In 1993, von Hippel was invited in as an advisor on the Clinton Administration's plan to end US nuclear testing. Later that year, he was invited to join the White House Office of Science and Technology Policy where he became involved in launching programs to secure nuclear materials in Russia and to stabilize Russia's nuclear cities. He was also responsible for supervising the process of deciding how to dispose of excess US weapons-grade plutonium, and pushed to expand programs to end the use of weapon-grade uranium as a reactor fuel.

Back in Princeton, von Hippel co-founded Princeton's Program on Science and Global Security and the International Panel on Fissile Materials. His research since has focused primarily on ending the production and use of highly-enriched uranium and plutonium as reactor fuels worldwide and dealing with Iran's nuclear program along with the longer-term goal of total nuclear disarmament. He also has been working with citizens groups to educate the US Congress and the public on opportunities to advance nuclear nonproliferation and disarmament.

PART 1. Looking for a Way to Contribute

Family Background

***Tomoko Kurakawa (*TK**): Let me ask you first about your background and how you became a scientist.

***Frank von Hippel (FvH*)**: I am the third of five children, the first to be born in the United States. My mother was Jewish and my parents left Germany in 1933 after Hitler came into power. They went first to Istanbul, where Kemal Atatürk set up a whole university staffed with refugees from Germany. The idea was that the refugees would be temporary faculty and train Turkish replacements. Phillip Schwartz, himself a Jewish refugee who had relocated to Switzerland, was involved in initiating this project and identifying recruits among the refugees (Grant 2018). My father, Arthur von Hippel, taught there for two years.

TK: He was a scientist?

FvH: He was an experimental physicist who had been a post-doctoral researcher in Göttingen, Germany where my grandfather, James Franck, directed an institute for experimental physics.

James Franck was a good friend of Niels Bohr.[1] They first came to know each other as professionals because the so-called Franck-Hertz experiment in 1914 was the first observation of the atomic energy levels that Bohr had predicted in 1913. Franck and Hertz were awarded the 1925 Nobel Prize in Physics for that experiment.

That same experiment provided the physical basis for the mercury fluorescent lamp. Electrons from one end of the bulb are accelerated with an electric field through a gas of mercury atoms. When the electrons get to a certain energy, they can excite atoms to an energy level where the energy is released as light.

In 1933, all university faculty in Germany were government employees. One of the first things the Nazis did after taking power was to fire all the Jewish professors. An exception was made for people who had been soldiers in World War I. My grandfather used that exemption to resign in protest (Lemmerich 2011; Von Hippel 2010).

Bohr invited Franck to his institute in Copenhagen while my parents were in Istanbul and then, in 1935, Bohr invited my parents to Copenhagen as well. I was not born yet.

My grandfather took a job in the United States in 1935 and began to look for a job for my father and found one. In 1936, my parents and two older brothers, accompanied by my grandfather, emigrated to the United States (Figure 1) and my father took up his job at MIT. I was born at the end of 1937.

Franck left his Nobel Prize medal with Bohr, as did Max von Laue, an anti-Nazi German who remained in Germany during the war. When the Nazis invaded Denmark in 1940, Georg von Hevesy, who would receive the Nobel Prize for chemistry in 1943, dissolved the two medals in aqua regia. The solution sat on a shelf in Bohr's institute for the rest of the war. In 1950, Bohr sent the gold back to Sweden to be remade into medals and returned to Franck and von Laue. My grandfather was not totally clear about what

[1] "Danish physicist … generally regarded as one of the foremost physicists of the 20th century. He was the first to apply the quantum concept, which restricts the energy of a system to certain discrete values, to the problem of atomic and molecular structure. For that work he received the Nobel Prize for Physics in 1922" (Aaserud 2019).

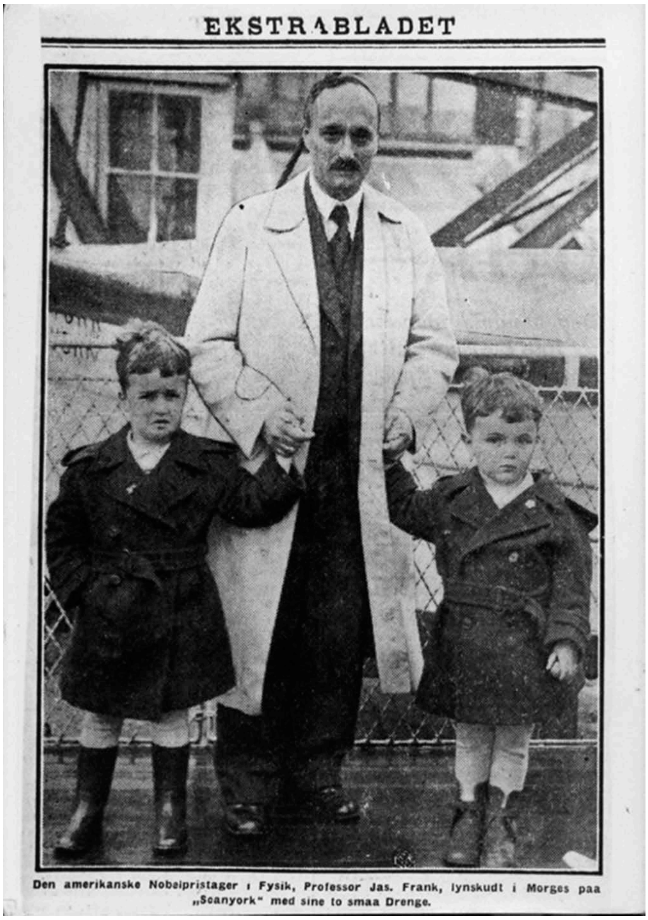

Figure 1. Danish news photo of James Franck and my two older brothers, Peter and Arndt, at the dock in Copenhagen, ready to set off on the sea voyage to New York in 1936.

had happened and thought that Bohr had dissolved his own Nobel Prize medal with his. So he was very happy to have half of Bohr's Nobel Prize medal. But it turned out that Bohr had auctioned off his own Nobel Prize the month before the German invasion to raise funds for Finnish refugees.

TK: What did your grandfather do in the States?

FvH: Initially, he worked at Johns Hopkins University. In Germany, he had a whole institute and was able to pursue many lines of research but, after coming to the United States, he only had one or two people to work with. So he decided to focus on trying to understand how photosynthesis works.

After the US joined in World War II and launched its nuclear-weapon project in 1942, Franck was invited to join the project's research and development effort based at the University of Chicago. He remained with the University of Chicago for the rest of his life.

At MIT, my father founded the field of materials science and engineering, designing materials based on atomic properties (Materials Research Society n.d.) (Figure 2).

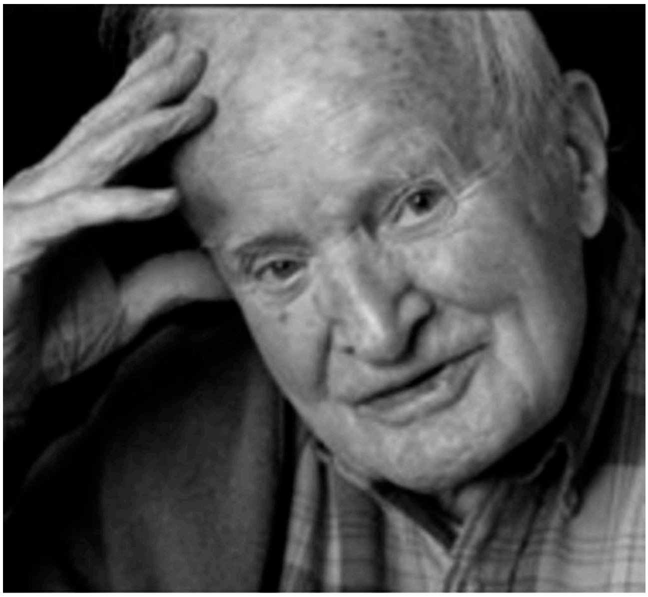

Figure 2. Arthur von Hippel at 100 (in 1998). Picture taken by his grandson, Jonas Kahn.

So here I was with a grandfather who was a physicist, a father who was a physicist, and I was the first son born in the US. So they named me Franck Niels von Hippel. My first and middle names were chosen to honor my parents' heroes, James Franck, and Niels Bohr. I dropped the "c" in Franck. My last name, von Hippel, was complicated enough for Americans.

We lived on a road in a suburb of Boston where there were fields and woods behind the houses. My father loved to walk back there, and would sometimes bring one of us along to talk about what our interests were and advise us about a career. For me, he suggested astrophysics.

TK: How old were you when he made that suggestion?

FvH: I probably was in high school. Scientifically, it was a very good suggestion because we now know how interesting astrophysics became. But I decided on another career.

Education

I graduated from high school in 1955, two years before the shock to the US government from the "backward" Soviet Union launching the first artificial earth satellite, Sputnik.[2]

Niels Bohr happened to be at our house for dinner shortly after. He was visiting MIT to give a series of public lectures and I learned that it had been an open question in his mind whether there would be a deviation in the period of Sputnik's orbit from the

[2]Sputnik 1, weighing 84 kilograms, was launched by the Soviet Union on 4 October 1957. It transmitted radio signals for three weeks until its batteries went dead.

predictions of Einstein's theory of gravitation. Now it would be possible to answer that question by measuring the exact time the satellite took to go around the earth.

After high school, I went to MIT to study physics. Although my father was a physicist, he was in a different department, the electrical engineering department and I didn't see much of him at MIT. Occasionally, when we passed in the corridor, he was deep in thought and I wasn't sure he recognized me.

I was interested in elementary particle physics and my bachelor's thesis was a measurement of the "Panofsky Ratio," a ratio of the probabilities of two reactions of short-lived negative π-mesons produced by MIT's electron synchrotron stopping in a target of liquid hydrogen.

In high school, I had learned that you could clarify your thoughts by writing. At MIT, because I had good scores in humanities, I was invited to join a special seminar for my first two years in which we read great books and wrote down our reactions to them.

There was nothing specific at this point relating to my later interest in policy.

I later learned that both Bohr and my grandfather were concerned about the implications of nuclear weapons for the future of humanity. In fact, during World War II, Bohr thought deeply about those implications and managed to meet with both Churchill and Roosevelt to discuss them. He urged them to discuss the nuclear bomb project with Stalin to lay a basis for international control of nuclear weapons after the war.

At the end of my time at MIT, in 1959, I obtained a Rhodes Scholarship to go to Oxford University in England.

TK: So, going back, you spent your high school days at the '50s. What was going on?

FvH: It was initially a hopeful time – like the first years after the end of the Cold War. The US had helped save Europe from the Nazis. The country was prosperous and governed by able people. But then the Korean War began in June 1950. I heard about it in a summer camp. (I was 12 years old at the time.)

The Soviet Union's first nuclear test in 1949 and the Korean War marked the beginning of the Cold War. But politics were not discussed in our family – at least not with the children – so I was not tuned into the larger world.

The Rhodes Scholarship was originally set up for young men who were believed to have leadership potential. The evidence in my case was my work during five summers in the Appalachian Mountain Club's Trail Crew where I ended up as the leader of the crew, the "trail master," in the summer of 1957 at the age of nineteen (Figure 3).

The Rhodes Scholarship was very prestigious but my oldest brother, Peter, dropped a little cold water on my glory by saying, "You know what they say about Rhodes Scholars? People with a great future behind them."

TK: Like President Clinton.

FvH: Clinton was a Rhodes Scholar who had a great future *before* him. Perhaps my brother was thinking of scientists.

Oxford was my first time living away from home other than for summer jobs. When I was at MIT, I lived at home and commuted to the university because my parents had five children and not much money. Living at home was less costly than paying for room and meals at the university.

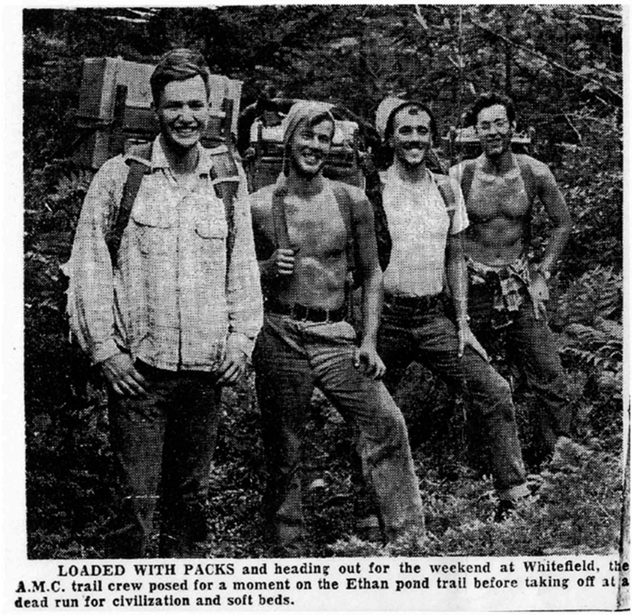

Figure 3. Frank von Hippel (on the left) and Appalachian Mountain Club trail crew colleagues in 1957 on their way out of the woods after five days work building a log shelter. (Manchester Union).

My friends in Oxford were mostly Americans, Australians and a South African. They were interested in politics. One, Steve Breyer, is a justice on the US Supreme Court. This was my first encounter with people focused on politics and economics. I started wondering whether being a physicist would be the best way for me to contribute. I even dropped out of physics for two weeks and studied economics. My idea was that perhaps I could save Africa with an economic theory of development. But I concluded after two weeks that maybe saving Africa would be more complicated than that, so I went back to physics.

TK: What attracted you back to physics?

FvH: At least I had a strong background there. I think my idea was that, like Bohr and Franck, if you become a famous physicist, then people will listen to your opinions on things like nuclear weapons. *[Laughs]*.

I did enjoy the simplicity and elegance of physics. However, at MIT, learning physics was more of a training than an education shaped by pursuing one's interests. I had learned a lot of physics at MIT but had not enjoyed the learning process because there was so much pressure.

TK: What kind of pressure?

FvH: The freshman year shaped my image of MIT. Every week you would have an exam in every subject. I had a weak background in mathematics and physics from my high school. So I struggled.

Most of the other students had stronger backgrounds in those areas. I had gone to a small private high school that was focused more on English, the humanities and foreign languages than on science. In the long run, that helped balance out MIT. The humanities courses that I took at MIT also helped develop my writing skills.

At Oxford, I struggled again, but for a different reason. In educational philosophy, it was at the opposite extreme from MIT: lectures but no coursework. You prepared for a qualifying examination at the end of the first year and, if you passed, you did research for a doctoral thesis.

I sat in my room and read physics books all winter to prepare myself for the general exam. About two weeks before that exam, my advisor gave me a practice test and I did very badly. He said, "Well, maybe you'll have to go back to the United States."

But he told me that you prepare for the general exams by practicing with previous years' exams. So I did that for two weeks and was able to pass.

That was my first year at Oxford. I was then expected to do a thesis in two years, which is faster than in the United States. I was interested in elementary-particle theory but Oxford was very weak in that area at the time. I never found an advisor I was happy with.

TK: What was your thesis topic?

FvH: It was on hypernuclei. It involved a very short-lived relative of the proton and neutron called the Λ-hyperon. It lives less than a billionth of a second. But, during that time, it can become part of nucleus. A billionth of a second is a very long time on the nuclear timescale so you can see how these strange particles interact with ordinary matter. I did a thesis on how that interaction influenced the way the Λ decayed.

I did the calculations in a way that was inadequate, however. Luckily for me, Richard Dalitz, a professor from the University of Chicago, came through. His research focused on hypernuclei and he invited me to be a post-doc with him and do my thesis calculation again using a simpler, more intuitive approach.

Fermi Institute (1962–64).

So my first post-doc was at the Enrico Fermi[3] Institute at the University of Chicago. The month after I arrived, October 1962, was the month of the Cuban Missile Crisis.[4] I was completely oblivious to it. Other people were petrified.

TK: What were you doing?

FvH: I was just settling into my new job and I felt pressure to produce from the moment I stepped through the door of the institute. I went to the office of the director and his secretary told me that the institute's weekly seminar was that afternoon. She asked me to fill out a form so that the director could introduce me at the seminar.

[3]Italian-born American physicist awarded the 1938 Nobel Prize for Physics, who, in 1942, led the project to build the first nuclear reactor as a part of the US World War II nuclear-weapon program.

[4]The Cuban Missile Crisis was perhaps the most dangerous nuclear confrontation between the US and Soviet Union (USSR). It was triggered by the USSR introducing intermediate-range nuclear missiles into Cuba to make up for its inferiority in intercontinental-range missiles. The crisis was resolved by the USSR agreeing to withdraw those missiles and the US secretly agreeing to reciprocate by withdrawing its own intermediate-range missiles from Turkey.

I filled out the form and she looked it over and said, "Oh, you missed this page!" I looked at the page and the heading was "publications." I said I had no publications. She responded, "Oh, you had better start!" *[Laughs]* So even the secretary was pressuring me.

TK: Secretary to Fermi?

FvH: Fermi had died of stomach cancer in 1954 at the age of 53. When I arrived, Herbert Anderson, who had started as a student doing experiments on fission with Fermi at Columbia University, was the director.

TK: You wrote in your prepared chronology that your grandfather had a laboratory in the same building.

FvH: There were two entrances to the building; one said "Enrico Fermi Institute" over it, the other said "James Franck Institute."

TK: But the reason you decided to go to the Chicago was not because of your grandfather.

FvH: It was a complete coincidence. Professor Dalitz, who found me in Oxford, was an Australian. He didn't know my grandfather. Dalitz was a very nice man who saved me temporarily for physics. I continued for ten years in physics until I decided that I was not contributing enough to justify all the effort.

TK: According to your memo, you got some advice from your grandfather.

FvH: My grandfather, who was then in his early eighties – my age today – would spend part of the year at the University of Chicago and part of the year with his second wife, Hertha Sponer, who was a physics professor at Duke University in North Carolina.

I worried about what the secretary had said. And I became more worried as a result of the productivity of another post-doc who had arrived two weeks earlier. He had even written a couple of articles between our arrivals.

So I said to my grandfather, "I'm not sure I'm going to be successful as a physicist. Look at how much more productive this other post-doc is!"

My grandfather tried to reassure me with a story about a conversation between a lion and a rabbit. The rabbit asked the lion, "How often do you have babies? I have nine a year!" And the lion responded, "I only have one baby every other year but, when I have a baby, (roaring) *it's a lion*!"

TK: So, during your two years in Chicago, did you publish anything?

FvH: I did publish five articles, but none of them was a lion.

James Franck

TK: Did your grandfather talk about the Manhattan Project[5] with you?

FvH: For some reason he didn't talk about it. I later figured out that a couple of things he told me related to discussions he had during the Manhattan Project days. Probably they

[5]The code name for the secret US World War II nuclear-weapon development project.

were both with Leo Szilard,[6] who had also been at the University of Chicago during World War II, working with Fermi inventing and designing plutonium-production reactors. Szilard's cleverness was enjoyed by many of his colleagues. One of those was my grandfather.

Among the physicists, Szilard was a pioneer first in nuclear weapons and then in nuclear arms control. There is the famous story from 1933 when Szilard was a Hungarian refugee in England. Ernest Rutherford,[7] the most famous nuclear scientist of his day, was quoted in the newspaper as describing the possibility of controlling the rate of energy coming out of radioactive nuclei as "moonshine!"

TK: Not reality.

FvH: This inspired Szilard to think about how one could accelerate the release of nuclear energy and he came up with the idea of the neutron chain reaction. But this was five years before the discovery of uranium fission. He investigated some possible nuclear chain reactions with other elements but none of them worked.

TK: How did your grandfather get involved in the Manhattan Project?

FvH: He was older than most of the other physicists, 60 years old in 1942, and did not have much depth in nuclear physics. My guess is that he was invited in by Arthur Compton[8] as a wise man. Compton was responsible for the part of the Manhattan Project that was headquartered at the University of Chicago where the first reactor was built and the first US plutonium-production reactors were designed.

Compton gave Franck the responsibility for managing the research on the chemical separation of plutonium from irradiated uranium.

TK: Reprocessing.

FvH: Reprocessing, which I have spent decades trying to end. *[Laughter]*

The scientific work of developing the chemistry of reprocessing was done by Glenn Seaborg.[9] But, on the organization chart, my grandfather was supervising Seaborg. My guess is that this supervision probably amounted to him periodically asking Seaborg how things were going and discussing his progress with him.

After the Hanford plutonium-production reactors[10] were designed – probably in 1943 – Fermi left for Los Alamos, New Mexico where the bombs were being designed. Some of the other Manhattan Project scientists at the University of Chicago started to think about the post-war nuclear problem. Szilard was a leader in those discussions.

[6]Hungarian-American physicist, inventor and political activist who partnered with Fermi in developing the first nuclear reactor.

[7]New Zealand-born British physicist who was the central figure in the study of radioactivity during the first decades of the 20th century, was awarded the Nobel Prize in Chemistry in 1908 and discovered the atomic nucleus in 1911.

[8]American physicist awarded the Nobel Prize for Physics in 1927 for his discovery and explanation of the change in the wavelength of X rays when they bounce off electrons.

[9]American nuclear chemist who shared the 1951 Nobel Prize for Chemistry for discoveries of transuranium elements including plutonium. Chair of the US Atomic Energy Commission, 1960–70.

[10]Hanford was a small town on the Columbia River in south-central Washington State. Because the population density was very low, the site was considered suitable because of justified concerns about the safety of the first high-power reactors, which had no containment buildings.

In 1944, Szilard also invented the plutonium breeder reactor. He was hopeful that there would be a long-term benefit for humanity from the discovery of fission. But he was convinced that uranium was very scarce. He estimated that, if just the energy in chain-reacting U-235 were exploited, there would only be enough uranium obtainable from high-grade uranium deposits to support the equivalent of two 1000-Megawatt-electric power reactors.

In order for nuclear energy to make a major contribution, it would be necessary to invent a much more uranium-efficient reactor. So he invented the breeder reactor, which would get one hundred times more energy out of a kilogram of uranium by turning non-chain-reacting uranium-238 into chain-reacting plutonium-239.

The Army, which managed the Manhattan Project, did not allow the scientists to discuss policy. The young scientists got around that by making simultaneous appointments with senior physicists and then arriving early and having their discussions in the waiting rooms.

TK: Did James Franck worry about nuclear weapons?

FvH: He had been involved in Germany's poison-gas program during World War I. His scientific mentor was Fritz Haber who had become famous by inventing a way to make fertilizer from the nitrogen in the air. Before that farmers had depended on natural deposits of nitrates. In this way, Haber made possible the green revolution and the resulting ability of agriculture to support the world's growing population.

During World War I, however, Haber decided to work on poison gas as a way for Germany to end the stalemate and win the war. He recruited younger scientists to work with him. Franck was one of them. Another was Otto Hahn, who later co-discovered fission (Figure 4). After the war Haber was briefly sought as a war criminal. Then, in 1919, he was awarded the Nobel Prize in chemistry for the fixation of nitrogen.

Figure 4. James Franck (left) and Otto Hahn (center) in front of the shack at Fritz Haber's institute in Berlin where gas masks were tested against poison gases (Dietrich Hahn).

Bohr was very critical of Franck for letting himself be involved in the poison-gas program. That may have made Franck more thoughtful about getting involved in the nuclear-weapons program.

So, when Compton invited him to join the Manhattan Project in 1942, Franck said that he would, out of concern that the Nazis might get nuclear weapons first. But, if it turned out that the US was in a position to use nuclear weapons first, he wanted to be able to have his views on the matter heard at the highest levels of the US government. Compton agreed.

On the basis of that commitment, in the spring of 1945, Franck was allowed to organize a discussion group about the post-war implications of nuclear weapons. The group wrote the "Franck Report" meant to be delivered to the president.

The war against Germany was in its final stages and it was obvious that Germany did not have nuclear bombs. The view of the group Franck assembled was that the war against Japan also would be over soon, independently of whether nuclear bombs were used. They believed, however, that what happened after the war with regard to nuclear weapons *would* depend on what the US did with regard to the use of nuclear weapons during the war. They argued that nuclear weapons should not be used unless it was agreed by the "United Nations," the alliance against Germany and Japan which included the Soviet Union.

One of the stories my grandfather told was about an argument he had with Szilard. Szilard had circulated among the Chicago scientists a petition against using nuclear bombs on Japanese cities. The Manhattan Project was run by the Army and Szilard wanted to bypass the Army's channels to get the petition to the president. Franck insisted, however, that the petition be sent through channels. He recounted that he told Szilard, "Your way may be cleverer, but I think mine is wiser." Szilard's retort was, "I agree with half your statement."[11] Szilard understood that the petition would be futile but he wanted as many of the nuclear scientists as possible to be on record in opposition to the nuclear bombing of the cities of an already defeated country.

TK: Your grandfather's way was more formal, I think.

FvH: Yes, he was more respectful of authority than Szilard.

Roosevelt died before the *Franck Report* was written. But he had already considered this issue as a result of Bohr's separate meetings with him and with Churchill. Churchill wanted to have Bohr put in jail because he thought Bohr would reveal to the Soviets the secret that the US and UK were working on the development of nuclear weapons. (Because of spies, Stalin knew much more than that.) And he talked Roosevelt out of taking Bohr's proposal seriously. So, in fact, the Franck committee's proposal had already been rejected.

When the *Franck Report* arrived in Washington, Truman was president and had just been informed for the first time about the US nuclear-weapon program. The report was discussed by a group called the "Interim Committee" that had been established to consider policy with regard to nuclear weapons and energy.

[11]Szilard tells the same story in his memoir. The editors report in a footnote that the Army did not deliver the petition to the President, explaining a year later that the bomb's use "had already been fully considered and settled by the proper authorities" (Weart and Szilard 1978, 187–88).

One of the recommendations in the report was that the power of a nuclear explosion be demonstrated to representatives of the United Nations and Japan. The Interim Committee had a scientific advisory committee made up of the four senior scientists of the Manhattan Project: Robert Oppenheimer,[12] Arthur Compton, Enrico Fermi and Ernest Lawrence.[13] They were asked to see if they could come up with a demonstration that would have as much impact on Japan's willingness to surrender as bombing a city. They came back and said that they had not been able to think of such a demonstration and that they agreed that the highest priority should be to use the weapon to end the war as quickly as possible. So that was that.

The *Franck Report* became famous, however, for foreseeing the US-Soviet nuclear arms race and for proposing how it might be forestalled. And its proposal to cooperate with the Soviet Union on controlling nuclear weapons became a founding document of the post-war movement to control nuclear weapons.

But my grandfather told me the story about his argument with Szilard without this context.

One of his other stories related to the resentment the scientists felt about the Army compartmentalizing information in the Manhattan Project and thereby preventing discussion of the big issues. He said, "You know, when you look up intelligence in the dictionary you will find in order: human, animal, and military." I would not be surprised if that were one of Szilard's witticisms as well.

TK: So what was the most important thing you learned from your grandfather?

FvH: From my perspective in those days, he was an old man. *[laughs]* I'm now about the age he was then. He actually died at the end of my two years at the University of Chicago. But he was a great man who took some important moral stands. He also was a very nice man and a good grandfather. He would pay attention to us grandchildren when he visited my family.

I also learned one thing as a child talking with him: It is easier to stay awake when you're talking than when you are listening. I can remember talking to my grandfather and I would see his eyelids drooping. Then he would start to talk and my eyelids would begin to droop. *[Laughter]*

TK: As a coincidence, both of you were involved with the issue of plutonium separation.

FvH: Indeed! I once I told me grandnephew, Jimmy, "I'm working on trying to end the separation of plutonium that my grandfather helped start." Jimmy responded, "Oh, I understand, you're fighting with your grandfather!" *[Laughter]*

I suspect my grandfather followed the arguments around nuclear energy enough to know that the breeder reactor was at that time considered essential to the future of nuclear power and that that separation and recycle of plutonium would be essential to breeders.

It has always seemed strange to me, however, that Seaborg, who developed the first reprocessing technology for separating plutonium for nuclear bombs and, later on, as chairman of the US Atomic Energy Commission, promoted plutonium as the fuel of the future, apparently didn't worry about the fact that it was a nuclear-weapon material.

[12]J. Robert Oppenheimer, an American theoretical physicist, was the wartime leader of the Los Alamos nuclear weapons laboratory in New Mexico where the Hiroshima and Nagasaki bombs were designed and produced.

[13]Ernest Lawrence won the Nobel Prize in Physics in 1939 for his invention of the cyclotron and created the Radiation Laboratory at Berkeley where plutonium and other transuranic elements were discovered. During the Manhattan Project, cyclotron-type magnets were used to do some of the enrichment of uranium for the Hiroshima bomb.

During the entire decade of the 1960s, while he was chairman of the Atomic Energy Commission, Seaborg relentlessly promoted what he called a "plutonium economy" in which plutonium would be the fuel for human civilization.

I had the opportunity to give Seaborg my views on breeder reactors in 1982 when I was invited to participate in a symposium at the University of Chicago to mark the 40th anniversary of the first human-devised sustained nuclear chain reaction in Fermi's reactor.[14] I was on a panel chaired by Seaborg. Another person on the panel was Alvin Weinberg, who had been the director of the Oak Ridge National Laboratory where the light-water reactor, which dominates nuclear power today, was first developed.

My talk was a denunciation breeder reactors.[15] Seaborg was very distressed. So he turned to Weinberg and said, "Alvin, tell them how well it's going in France."

At that time the breeder reactor demonstration project in the United States was about to be ended by Congress. I had been involved in educating Congress on the subject. So Seaborg's hopes were pinned on the Super-Phénix breeder reactor, the most powerful breeder reactor ever built (1.2 billion Watts electric), then under construction in France,. It was started up in 1986, three years after the Chicago symposium, but had many technical problems and operated very little before being abandoned in 1998.

TK: What would your grandfather say to you if you had the chance to encounter him now and if he could learn what you have done in the nuclear arena?

FvH: I think he would like what I have been doing because it has been in the tradition of Bohr, worrying about the implications of technology for society. He probably would have been happier if I had also made great discoveries in physics, but would have approved that I had found a way to become engaged with these issues even without becoming a famous physicist.

TK: You *are* a famous physicist.

FvH: Thank you.

Cornell and Hans Bethe (1964–66)

TK: Well, next you moved to Cornell, right? There you worked with Hans Bethe.

FvH: Bethe was a great physicist. He got a Nobel Prize in physics for figuring out how nuclear reactions power the sun. He was the head of the theory group at Cornell, but he was working mostly on nuclear matter while I was there. I would see him at seminars and periodically I would go and talk with him about what I was working on. He would listen. He didn't have any suggestions, but he was encouraging, "Keep at it."

TK: But he had already been critical of nuclear weapons?

FvH: Bethe had been the head of the theory group at Los Alamos during World War II. During the debate in 1950 over whether to proceed with the hydrogen bomb, he wrote

[14] Uranium-235 decays more rapidly than U-238, with a half-life of 0.7 billion years. Billions of years ago, therefore, natural uranium contained a higher percentage of U-235 and could sustain a chain reaction in rich uranium deposits saturated with water. Evidence has been found for chain reactions in such "natural reactors" 1.5 billion years ago in Oklo, Gabon, Africa.

[15] See Sachs (1984, 230). Seaborg's request to Weinberg was apparently edited out.

a remarkable article giving both the strategic and moral arguments against proceeding (Bethe 1950, 18). But, after President Truman made the decision to go ahead, Bethe spent his summers at Los Alamos, working on developing the hydrogen bomb. He wanted to be on the inside. During the years I was at Cornell, he was acting as a government advisor and was not apublic critic.

Bethe didn't surface again as a high-visibility critic of the nuclear arms race until after I had left Cornell, when the debate over ballistic missile defense (BMD) became public. In fact, the debate over ballistic missile defense was the first policy debate I became personally involved in.

In 1967, a year after I left Cornell, Bethe decided to join Richard Garwin[16] and make public their criticisms of the Sentinal BMD system that was being proposed by the Johnson Administration. Their article in *Scientific American* (Garwin and Bethe 1968, 21) had a great impact on the physics community, many of whose younger members became very engaged with the issue. At that point, I was at Stanford.

Later, during the the 1980s debate over President Reagan's "Star Wars" program, several Cornell physics graduate students decided to go into nuclear-weapons policy rather than continuing on with physics.[17] That probably was due to the tradition of social concern that Bethe had established among the Cornell physicists.

While I was at Cornell, US engagment in the Vietnam War was starting to become a public issue. I remember I went to a "teach-in" in a big lecture hall with multiple speakers denouncing the war. I happened to sit next to Bethe. He saw that I was getting excited by one speaker and advised me quietly, "He's a demagogue."

TK: Bethe supported the Vietnam War?

FvH: I don't think he was a supporter, but, at that time, he was not an active critic either. He just didn't like the speaker's style. So my only policy interaction with Bethe was sitting next to him in this anti-Vietnam War teach-in!

I did benefit, however, from a book that I borrowed from his office, which was unlocked. It was *American Scientists and Nuclear Weapons Policy* by Robert Gilpin (1962). It laid out the debate in the US nuclear community between those who advocated arms control and those who emphasized containing the Soviet Union with nuclear deterrence. It was my first reading on science advising.

Stanford and the Vietnam War (1966–69)

TK: Okay. Then the years at Stanford University. Perhaps life changed?

FvH: My next stop was at Stanford as an assistant professor of physics. The Vietnam War was already an issue for the students and became more of an issue during my three years there.

A key concern that drove the student activism was the fact that the US had a "draft," conscription with young people being called on a random basis to join the Army to be sent to

[16]Richard Garwin, a student of Enrico Fermi, a prolific inventor and analyst, worked half time for IBM inventing technologies for civilian use and half time advising the government, especially on nuclear-weapons technology and policy.

[17]Dan Fenstermacher, now in the State Deparament; and Lisbeth Gronlund, Edwin Lyman, and David Wright, now all on the staff of the Union of Concerned Scientists.

Vietnam. Students could delay reporting for service until they graduated but the war was controversial, and most students did not want to go either immediately or later. During my first year at Stanford, the student president, David Harris, decided that he would rather go to jail than participate in the Vietnam War. A few years later, he did go to prison for 15 months for draft refusal.

During academic year 1968–69, my last year at Stanford, the president of the university created a Faculty Senate, a group of elected representatives of the faculty to discuss important policy issues confronting the university. For some reason – perhaps because I had been a Rhodes Scholar – the other assistant professors, without consulting me, decided to pool their votes to elect me to the Senate. So I found myself in the middle of the university's debate over the Vietnam War.

Our first crisis was when the students decided to occupy the Applied Physics Laboratory on campus where there was classified (secret) Army-supported research going on. It was rumored that one of the projects being worked on was to stabilize the terrifying helicopter gunships that were being used in Vietnam. The students were very civilized. They even invited the faculty in for discussion over tea with home-made cookies.

The classified documents had been locked up so there was time for such discussion. The administration asked the Faculty Senate what the University's policy should be. We had a debate and voted that there should be no secret research on campus.

After the vote, I went back to tell the physics faculty what we had decided. Robert Hofstadter, who had shared the 1961 physics Nobel Prize, was scandalized. He declared, "This policy infringes on our academic freedom!" I responded, "Academic freedom is not an absolute. There are other considerations against which it has to be balanced. For example, at the medical school, they are not given complete academic freedom about what kind of experiments they can do with human beings." He retorted, "Well, then we should get rid of the medical school!"*[Laughs]*

So, winning a Nobel Prize does not necessarily mean that you are a sophisticate on policy issues.

The students appeared to have won but, when they learned that the research would continue in the nearby university-owned Stanford Research Institute, they were furious and decided to occupy ("sit in" at) the building where the university president had his office and administrative staff.

The administration took this very much more seriously because all the University's secrets, including the salaries of the president and of the faculty, were there and and the files had not been locked up. After a day or two, at 3:00 AM one morning, I was awoken by a call from the university to tell me that the county sheriffs had been called in to clear the students out. Could I come in and help prevent violence?

I went to the university and found three busloads of county deputy sheriffs in front of the building. They had long clubs and visors and you could see that their adrenaline levels were very high. So the sheriff and I went inside to talk to the students and found a group of them debating, according to *Robert's Rules of Order*,[18] whether or not they should leave. We encouraged them to hurry up and they decided to leave. After they left, the

[18]The most widely used manual in the US for how run a decision-making meeting of a government body or non-governmental organization.

sheriff and I walked around within the building. We found a few more students in sleeping bags and woke them up to tell them what their colleagues had decided.

At that point, the students split on what to do. Some went the next level and started breaking windows. But there was another group that asked themselves whether there was a way they could use their brains to make a difference.

The Politics of Technology

TK: So the activist students at Stanford had a strong impact on you.

FvH: Yes. The same reasons that drove me to change my career into what I call "policy physics" also impacted a number of the physics graduate students who had taken classes with me. Two of them, Joel Primack and Robert Jaffe, joined with an undergraduate, Joyce Kobayaski, and proposed to the university that it allow one or two semester-long student-organized workshops on policy issues with faculty advisors and that students be graded and given credit for their work in these workshops, just as they would for their work in a course.

The administration agreed and the following year the students organized a number of Stanford Workshops on Social and Political Issues, some of which had real impact.[19]

One of the workshops was organized by Joel Primack, a student of Sidney Drell,[20] a physicist who was at the time a member of the President Nixon's Science Advisory Committee and chairman of its Panel on Strategic Nuclear Weapons Policy.

Joel asked Drell, "I'm sure you and your colleagues are giving the President good advice. Why is his policy so stupid?" Drell responded, "I can't tell you what our advice to the president is. It's all secret."

We later realized that the secrecy was the problem. In any case, Joel decided to organize a policy workshop to find out why the science advisory system was not producing better policy results.

The policy workshop began in the fall of 1969. I had left for Berkeley on the other side of San Francisco Bay. Joel recruited as the workshop faculty advisor Martin Perl,[21] a senior physicist who was sympathetic to the students. The workshop stretched into two semesters, however, and, when Perl went on sabbatical in the spring of 1970, Joel asked me whether I could take over as faculty advisor and commute from Berkeley to Stanford once a week for the workshop meetings. My "yes" had a major impact on my subsequent professional career.

The workshop came up with one big idea: that Congress should have science advisors. Specifically it proposed a fellowship program for scientists to spend a year working either in the offices of individual Representatives or Senators or on committee staffs. After that year, they could either stay on in government or move on to other careers.

A couple of years later, at Harvard, Joel was recognized by a well-connected senior colleague as a focused and effective activist and was appointed to policy-making

[19]"Public Interest Science in the University: The Stanford Workshops on Political and Social Issues," in Primack and von Hippel (1974, 196).
[20]A science advisor and physicist who led the theoretical group at the Stanford Linear Accelerator Center.
[21]Perl later shared the 1995 Nobel Prize in physics for his discover of the τ-meson, a much heavier short-lived clone of the electron.

committees of the American Physical Society (APS) and American Association for the Advancement of Science (AAAS). He used those opportunities to promote the workshop's idea. The result was the creation of the first Congressional Science and Technology Fellowships that now place tens of scientists and engineers on Congressional staffs every year. The program has been so successful that the government has funded more than one hundred similar fellowships for scientists and engineers in the executive branch.

The students in the workshop did not, however, produce an answer to Joel's original question about why the science advice to the executive branch was not having more of an impact. He therefore asked whether I could help him research and write up a report on that subject. We spent most of the summer of 1970 doing that.

We found a number of cases in which science advisory reports had later become public and we concluded that science advisory committees were sometimes being used to legitimize political decisions. We wrote up these case studies in a report, *The Politics of Technology*. It was printed up and put in a closet at Stanford. That fall (1970) Joel went to Harvard as a post-doc and I joined the High Energy Physics Division in Argonne National Laboratory outside Chicago. Argonne was the laboratory that had been started by Fermi's reactor-design group after World War II and, in 1970, half of its budget was still coming from the Atomic Energy Commission's breeder-reactor development program.

About six months after I arrived at Argonne, back at Stanford, a journalist from the *San Francisco Examiner* decided to do a story on the Stanford policy workshops. Stanford gave him our report among others. He thought our case studies very interesting and wrote an Associated Press story about them. It was picked up all over the world. There was even a front-page story in the tabloid, *The National Enquirer* (Figure 5). In that more innocent age, our revelations were more shocking than they would be today.

NATIONAL ENQUIRER 15¢

How to Get the Most Out of Life

Stanford Univ. Report on Last 15 Years Charges:

GOVT. SUPPRESSED SCIENTISTS' WARNINGS OF DANGERS TO THE HEALTH AND SAFETY OF PUBLIC

Caroline & Jackie

'Cinderella' Rockefeller to Rewed

By BERNARD H. GOULD

Stanford Univ. Report on Last 15 Years Charges:

Govt. Suppressed Scientists' Warnings of Dangers to the Health and Safety of Public

ALASKAN EARTHQUAKE in 1964 buckled streets and crumpled buildings in Anchorage. U.S. government has suppressed warnings that nuclear tests might trigger quakes and tidal waves in Alaska.

DEPUTY DEFENSE SECRETARY David M. Packard

PHYSICIST Frank von Hippel

Facts & Fallacies About the Mind

By DR. JOHN BRAXTON

SUPERSONIC TRANSPORTS, according to a study that was kept from the public, will generate sonic booms and take-off and landing noises that will be intolerable to the population.

NATIONAL ENQUIRER

Figure 5. Front-page story about the Stanford workshop report, *The Politics of Technology*, 21 February 1971.

So, suddenly, the report from our Stanford workshop was famous.

One of our recommendations was that, if you are a science advisor and feel your advice is being misused, you have a responsibility to go public and correct the record. Joel was told that that recommendation was discussed at a meeting of a panel of the President's Science Advisory Committee (PSAC). Richard Nixon was president and the panel members reportedly were told, "Anyone inclined to follow this report's advice should resign." In fact, some PSAC members – notably Richard Garwin – *did* give their views to Congress as well as the White House and those views were not always consistent with the president's policy preferences. This angered President Nixon and, shortly after his 1973 reelection, he abolished PSAC. Later, it was recreated in an attenuated form as today's President's Council of Advisors on Science and Technology.

We were impressed by the interest in our report and decided to write a book. That is the origin of our 1974 book, *Advice and Dissent* (Primack and von Hippel 1974).

The weakness of providing secret advice to high officials is that they are free to ignore analyses inconsistent with their political priorities. Since the reports are secret, outside critics can't cite or critique them. Eventually, the reports leak out but, by then, it is often too late. In the case of the President's Science Advisory Committee, in 1967, President Johnson decided to ignore its advice on the infeasibility of effective ballistic missile defense (BMD). He decided to deploy the Army's proposed system because then Presidential candidate Richard Nixon was claiming in the runup to the 1968 US presidential campaign that, since the Soviet Union was deploying a BMD system, there was a developing BMD "gap" analogous to the "missile gap" that John Kennedy had incorrectly claimed existed to help defeat then Vice President Nixon in the 1960 presidential election.[22]

Only half of *Advice and Dissent* built on the Stanford report, however, because we looked for alternatives to the confidential science-advisor model – or rather complements. We did not want to eliminate executive branch science advisors; we wanted a more robust and balanced system of technical and analytical input into the policy-making process.

We discovered non-governmental organizations (NGOs) and I finally learned that you don't have to be a famous scientist to get people to listen to you. At NGOs such as Environmental Defense Fund (now Environmental Defense) and the Union of Concerned Scientists, there were scientists coming directly out of their PhDs and having an impact on policy by taking their analyses to the press.

As a result, there were two messages in our book. One was that having scientists advising the president and the executive branch was not enough. The second was that NGOs were beginning to provide a complementary source of information and analysis that was freely available to the public and Congress as well as the Executive branch. When the public can read an analysis, government officials will as well, because they may have to respond to questions about the recommendations in that analysis.[23]

Initiation into Activism at Berkeley

In the fall of 1969, I went to Berkeley on a year-long post-Stanford fellowship. There, I finally became an activist. Charles Schwartz, a professor in the physics department,

[22]"Invoking the Experts: The Antiballistic Missile Debate," in Primack and von Hippel (1974, 59).
[23]"When outsiders can be effective," in Primack and von Hippel (1974, 239).

invited me to join in some of his activism.[24] I also marched through San Francisco as part of a huge anti-Vietnam War protest.

I had decided to spend my year at the Lawrence Berkeley Laboratory (LBL) on the hillside above the campus because that is where the elementary particle physicists were. That year, the student turmoil was at its peak. We saw clouds of tear gas coming up from the campus below.

Schwartz had a summer research office at LBL as well as a teaching office on campus. Somehow he quickly detected my interest in policy and suggested that we team up and start a weekly mid-day seminar on science and social responsibility at LBL. I agreed.

We scheduled Schwartz to give the first talk but, when we came to the auditorium where we had scheduled the seminar, its door was chained shut. We therefore went outside and had the seminar on the grass.

Just at that time there was a whistle-blowing event at LBL's larger sister laboratory, the Lawrence Livermore nuclear-weapons laboratory 40 miles to the east.[25] The whistle-blowing was related to a claim by Ernest Sternglass, a physicist at the University of Pittsburgh, that the radioactive fallout from atmospheric testing had cumulatively caused 400,000 infant deaths in the US alone (Boffey 1969, 195). His estimate was based on the fact that the trend in infant mortality had been down until the 1950s when the US and Soviet Union started to conduct multimegaton tests in the atmosphere.

The Atomic Energy Commission (AEC) asked two biologists at Livermore, John Gofman and Arthur Tamplin, to review the basis for Sternglass' conclusions. They did and they concluded that his claims were exaggerated and that atmospheric testing had caused only perhaps 4,000 infant deaths in the US.

The AEC suggested to Gofman and Tamplin that it would be sufficient to say that Sternglass' estimate was faulty. It was not necessary for them to publish their own estimate. Gofman was enraged and went to the newspapers declaring that the AEC was trying to gag them (Boffey 1970b, 838).

We invited Gofman to give our second Berkeley lunch seminar on science and social responsibility. When we came to the auditorium, it was chained shut again. So we went out to the grass and found that the water sprinklers were on.

TK: Wow!

FvH: So I went to the director of LBL, Edwin McMillan, who had shared with Seaborg the 1951 Nobel Prize for Chemistry for the discovery of neptunium, the element in the periodic table between uranium and plutonium. I asked, "Why are you blocking our seminar?" and he responded, " I don't want what is happening on the campus to happen up here." I said, "Then I can't stay here" and moved down to the campus for the rest of my year. Schwartz was suspended from his position at the laboratory (Boffey 1970a, 743). He was never reinstated.

[24]For a lengthy interview with Schwartz about his own development as an activist, see in the American Institute of Physics Niels Bohr Libary, dated 15 May 1987, https://www.aip.org/history-programs/niels-bohr-library/oral-histories/5053.

[25]The Lawrence Livermore National Laboratory was founded in 1952 as the second US nuclear-weapons design laboratory, largely because of complaints by Edward Teller that the Los Alamos National Laboratory was not putting enough effort into the development of thermonuclear weapons, https://www.llnl.gov/about/history. The Soviet Union followed suit and founded its own second nuclear-weapon-design laboratory, VNIITF, in 1955.

While I was on campus, I became involved in two other Schwartz-related events. One was a demonstration that Schwartz organized against the nuclear-weapons activities at Livermore. We marched down a street between the Livermore and Sandia National Laboratories. Livermore competed with Los Alamos in designing the configurations of the nuclear explosive materials in nuclear weapons and Sandia added the electronics. This was my first confrontation with the bomb.

Schwartz also was creating trouble in another front. He was asking students who took his course to sign a Hippocratic-type oath[26] in which they would commit not to use what he taught them for harmful purposes.

Some of Schwarz's colleagues raised the concern that this infringed on the academic freedom of his students so the physics faculty met to discuss the issue. I attended because I was teaching a course on elementary particle physics, which made me a member of the faculty for that semester. So I witnessed the Socratic dialogue between Schwartz and his colleagues.

Some of the faculty – notably those involved in defense consulting – agreed that Schwartz's Hippocratic Oath requirement was a violation of his students' academic freedom. In response, Schwartz posed a hypothetical, "What if you were teaching a chemistry course and one of your students told you, 'I'm taking this course because I want to be able to learn how to make explosives so that I can blow up the Bay Bridge.' What would you do?"

Once again, as with Hofstadter at Stanford, some of the physicists insisted that academic freedom is an absolute.

The issue was very serious because they were considering voting to recommend that Schwartz be removed from the faculty.

In the end, there was a compromise. Schwartz received a letter of reprimand from the university's chancellor for infringing on the academic freedom of his students, and he stopped requiring the oath.

TK: What did you learn from Professor Schwartz?

FvH: I think I was inspired by his willingness to challenge the legitimacy of the status quo and then use the controversy and the audience that resulted as an opportunity for a consciousness-raising debate.

Later, I tried to do the same when I gave physics colloquia on the findings of our Stanford report on *The Politics of Technology*. But I always had to put down an inner voice telling me, "This is not what one should speak about in a physics colloquium! You should be talking about physics!"

TK: So your experience on the West Coast changed you?

FvH: It happened to me on the West Coast but the Vietnam War had created an environment that stimulated such activism at college and university campuses all over the country.

The students at Stanford had the greatest influence on me. It was the radicals among the students who got us all excited but it was the moderates who found a way forward for

[26]Oath attributed to the great Greek physician Hippocrates (460–370 BC) in which a doctor swears not to do harm with the skills that have been imparted to him by his teachers.

me and some of the students into real careers in what I call "policy physics," using our analytical skills to understand important societal problems and to develop alternative solutions.

Argonne National Laboratory

FvH. I enjoyed teaching but I was not satisfied with the meager results of all the effort I was putting into my physics research. I concluded that elementary-particle theory would progress as well without me. So I thought about how I could make more of a difference. The workshop that Joel had organized and invited me to join provided me a way to make my career transition.

During my year at Berkeley, I looked for my next job. If I continued on the academic track, I would take a tenured professorship in a university physics department. I did look at opportunities at the University of California at Los Angeles and the University of Utah. But, in the end, I decided that I would transition out of elementary particle physics into the new field of "technology assessment." That was the name used at the time for the systematic evaluation of the likely impact of specific new technologies on society.

I heard that there was a group that was doing technology assessment at Argonne National Laboratory. So I decided to join the high energy physics division at Argonne. Because I would not have to teach, I would have plenty of time to spend on my physics research and I could spend the rest of my working hours involving myself with Argonne's technology-assessment group.

Six months after I arrived at Argonne, however, Joel and I decided to write a book on science advising. So, instead of becoming involved in technology assessment, I spent the rest of my working time while at Argonne writing *Advice and Dissent*.

Some other staff members at Argonne became aware of what I was doing – in particular, the editor of *Applied Spectroscopy*, the official journal of the Society for Applied Spectroscopy.[27] He wanted an article on science and society for the upcoming 25^{th} anniversary issue of the journal and asked if I would be willing to write one. I agreed on behalf of Joel and myself and we wrote an article that appeared in the summer of 1971. We used it to summarize the Stanford report (Primack and von Hippel 1971, 403).

Applied Spectroscopy is a specialized technical journal, but someone brought our article to the attention of a high-level official in the Atomic Energy Commission – presumably because it included criticisms of the AEC. In our article, we recounted the AEC's suppression of an advisory report warning of the tsunami hazard from a large nuclear test under the Aleutian island, Amchitka; the AEC's proposal to set off hundreds of nuclear explosions to release trapped natural gas in shale under Colorado; and its coverups of plutonium fires in a facility outside Denver where the plutonium components for US warheads were being made at the time.

TK: Was not the 1971 nuclear test under Amchitka of a warhead for US BMD system?

[27]Spectroscopy is the measurement of the wavelengths of the light emitted by or reflected by a material. It can be used to study the composition, temperature, velocity, etc. of the material.

FvH: Yes, it was the largest ever underground nuclear test, five megatons. It was of the warhead for the long-range nuclear-armed interceptor of the ballistic missile defense system that President Johnson proposed under pressure from Nixon and that President Nixon began to deploy but then had to abandon as a result of public and Congressional opposition.[28] Today, the components of these warheads are still kept in storage. The argument from the nuclear-weapon laboratories is that they might be needed for defense against an incoming asteroid.

TK: Wow!

FvH: The report of the President's Science Advisory Committee expressed concern about whether the explosion would trigger an earthquake and cause a tsunami. The report was kept secret until it was forced out by a lawsuit three days before the test. The AEC went ahead with the test and there was no tsunami.

In any case, someone brought our article to the attention of the Atomic Energy Commission, which funded Argonne. A senior official from the AEC called up the director of the laboratory to complain, and the director of my division came to me and asked, "Frank, are you planning on writing any more articles like this?"

For some reason my reaction was smarter than I would have expected. I answered his question with a question, "Isn't this a matter of freedom of speech?"

In fact, I was not completely correct. I had used laboratory resources to produce and distribute a preprint of the article and the article itself has a footnote, "Work performed partially under the auspices of the U. S. Atomic Energy Commission."

My division leader backed off, however, and never brought the subject up again. But, this exchange started me thinking that perhaps an AEC laboratory was not the best place for me to pursue a new career as a technology policy analyst.

By this time, Joel and I had discovered what we called "public-interest science," the NGO approach to influencing policy. We therefore decided to do an overview article that we titled "Public Interest Science." It was published in 1972 in the magazine, *Science*, a much more visible journal that was a major source for science journalists (Primack and von Hippel 1972).

We were very much influenced at this point by Ralph Nader,[29] who had popularized the idea of "public-interest law" and had shown that young lawyers could make a difference by challenging big corporations and the government in court and as interveners in regulatory proceedures. In our article, and later in our book, we provided examples of scientists similarly making a difference – sometimes partnered with lawyers, sometimes by themselves in the court of public opinion.

Perhaps most famously, Rachel Carson with her 1962 book, *Silent Spring*, brought to public attention the danger to birds from the persistent pesticide, DDT, and helped stimulate the rise of the environmental movement.

[28]"Stopping Sentinel" in Primack and von Hippel (1974, 178).

[29]Ralph Nader first rose to prominence in 1965 as the author of *Unsafe at any Speed*, a critique of the safety of US automobiles and of General Motors' Corvair in particular. General Motors mounted an investigation and tried to compromise Nader, which led to a Congressional hearing and lawsuit by Nader that he won. Nader used the proceeds to establish a number of groups staffed by young lawyers who worked successfully for consumer protection in many areas. He fell out of public favor after he ran as an independent candidate for President in 2000 and was blamed by many for attracting enough votes away from Vice President Gore to contribute to the victory of George W. Bush.

In response to Carson's book, a lawyer on Long Island went to a group of scientists and asked them to look into the dangers of the local use of DDT. They decided that DDT was a threat to a broad spectrum of wildlife and sued to stop its use. Then, after they were successful locally, they established the Environmental Defense Fund (now Environmental Defense) and launched a legal campaign that, ten years after the publication of Carson's book, resulted in a national ban on the use of DDT.[30]

Another case at the time was in the area of reactor safety. An MIT physics professor, Henry Kendall,[31] and a Harvard economics graduate student, Dan Ford, became concerned about the safety of a nuclear power plant that was being proposed for the coast of Massachusetts and decided to participate in the AEC's local public hearing on the proposal. They got deeper and deeper into the subject and finally learned and revealed to the public that the AEC was ignoring technical concerns from its own experts about the effectiveness of the emergency cooling systems in water-cooled reactors. This helped lead to the demise of the AEC. As a result of this experience, Kendall founded the Union of Concerned Scientists whose concerns have broadened out to include climate change and nuclear arms control.[32]

TK: Did you get to know him personally?

FvH: I got to know Kendall after Joel and I published our book. But Joel actually did the interviewing for the book chapter about Kendall's and Ford's engagement with the reactor-safety issue. At the time, Joel was at Harvard, which is near MIT. As a result of working on that chapter, however, I became involved in the reactor-safety debate myself.

After we published our article, "Public Interest Science," in the fall of 1972, I got a call from the National Research Council, which does studies for the government under the umbrella of the National Academies of Science, Engineering and Medicine. The caller invited me to take a fellowship to see at first hand the science-advising process at the National Research Council. He said, "We don't think you understand it properly. It is important that we educate you on the subject."

I accepted the offer and went to Washington for the academic year 1973–4. That year in Washington finally provided me the opportunity to escape from theoretical physics.

Ballistic Missile Defense

TK: Let's move back to Hans Bethe. He also dedicated significant efforts to reducing the danger from nuclear weapons.

FvH: Bethe became a model for me after he published the 1968 article with Garwin, criticizing the Johnson Administration's proposed "Sentinel" system for defending the US against ballistic missiles. That article was authoritative and empowered a lot of

[30] "The Battle over Persistent Pesticides: From Rachel Carson to the Environmental Defense Fund," Primack and von Hippel (1974, 128).

[31] Kendall later shared the 1990 Nobel Prize in physics for experimental studies on the structures of protons and neutrons.

[32] "Challenging the Atomic Energy Commission on Nuclear Reactor Safety: The Union of Concerned Scientists," Primack and von Hippel (1974, 208).

physicists to actively oppose proposed deployments of nuclear-armed interceptor missiles in their areas.

In that connection, I'll go backwards to Stanford briefly. I became excited about the 1968 Garwin-Bethe article and decided it was important to educate the public about it. Someone organized an opportunity for Martin Perl and me to give a talk at a local high school.

It was in the evening and we sat up on the stage of a huge high school auditorium. The auditorium could have accomodated thousands of people but there were only about 20 people in the audience. So we encouraged them to come to the front and gave our talks. Then a woman stood up. It turned out that this was Joan Baez, the most famous folk singer of the time, who lived nearby. Her father was a physicist and a pacifist. She asked, "Why don't you people stop doing this?" by which I believe she meant, why don't physicists stop working nuclear on weapons? Then we had a brief discussion, the session ended and she left the auditorium trailed by a group of entranced students.

TK: She was really famous, wasn't she?

FvH: And an activist. At that time, she was involved in protests against the draft. She met David Harris, the Stanford undergraduate student body president, in jail and they were married for five years.

TK: So your topic was the Ballistic Missile Defense (BMD) system? Why were you interested in the BMD issue?

FvH: Because it was a breakout into the public arena of the secret debates within the government about the nuclear arms race and arms control. Also, this was an area of public policy where my expertise in physics was relevant. Finally, I had found a subject on which I could help educate the public.

Eventually, in 1972, public concerns forced the Nixon Administration to abandon its proposed nationwide BMD system and sign the Anti-Ballistic Missile (ABM) treaty with the Soviet Union that neither side would deploy more than a modest defense against strategic missiles limited to 100 interceptor missiles at a single site. At a cost equivalent to two billion of today's dollars, the Nixon Administration completed the permitted US single site in North Dakota to defend two hundred of the one thousand US Minuteman missile silos in the Great Plains from Soviet attack but Congress had decided it was a waste of money and it was scrapped after being operational for only 24 hours.

Perl and I made only that one public presentation. Other physicists did much more.

TK: But you wrote articles about BMD, right?

FvH: In *Advice and Dissent*, two of our chapters were about the national debate from 1968 to 1972 over ballistic missile defense. One chapter discussed the misrepresentation of the advice given to the White House and the Defense Department by their science advisors. The other was about the effectiveness of independent scientists in educating the public and Congress after the debate became public.[33]

[33]"Invoking the Experts: The Antiballistic missile Debate" and "Stopping Sentinel" in Primack and von Hippel (1974, 59, 178).

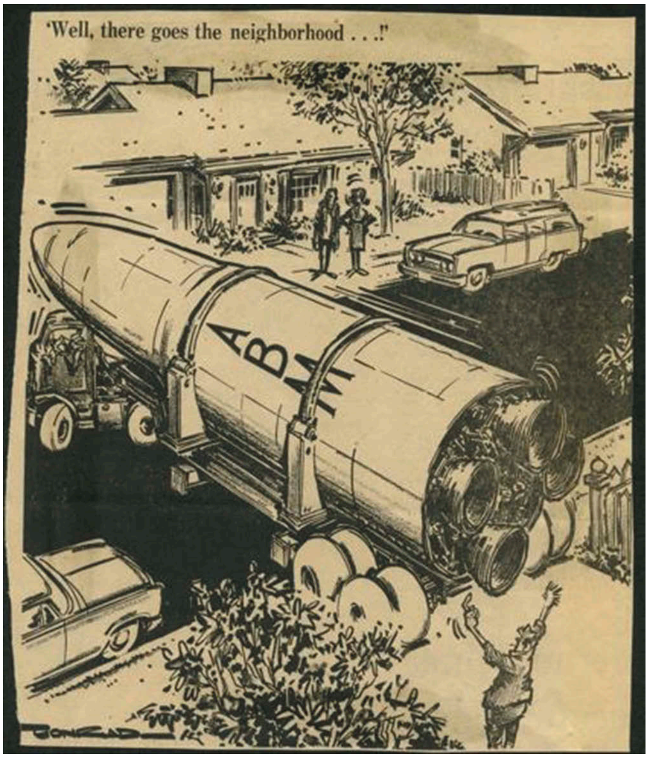

Figure 6. Editorial cartoon about the not-in-my-backyard movement opposition to the Johnson Administration proposed deployment of nuclear-armed ballistic missiles in US suburbs.[34]

The public became involved in large part because the Army decided to site its nuclear-armed interceptors in the suburbs of the big cities. The suburbanites didn't like the idea of 5-megaton warheads in their back yards (Figure 6).

Recently, George Lewis[35] and I have written a couple of articles on the BMD situation today, including arguing that the current US BMD buildup has provoked Russia to develop new types of nuclear delivery systems and China to build up its strategic missile force (Lewis and von Hippel 2018a, 2018b).

Policy Physics

TK: And what is the objective of the policy physicist?

FvH: For me, better informed, more democratic policy. Not just to become a confidential government science advisor but also to help open up the issues to the public.

[34]Attributed on the web to the *New York Times* but *New York Times* permissions department believes not.

[35]George Lewis, now a researcher based at Cornell University, became well known as a BMD expert after he and Prof. Theodore Postol of MIT demonstrated, by studying news videos of supposed intercepts by US Patriot missiles of Iraqi short-range Scud missiles during the 1991 Persian Gulf war, that the explosions the videos were recording were not intercepts but the self-destruction of the Patriot interceptors after they had missed their targets (Lewis and Postol 1993, 1).

TK: At the time you started that activism did not that way of influencing politics already exist?

The Federation of American Scientists and Bulletin of the Atomic Scientists

FvH: Although I did not realize it before we wrote the book, it turned out that small numbers of people had been doing this for decades. Most important for me were the atomic scientists who, in 1945, after the Manhattan Project, founded the Federation of American Scientists (FAS) and tried to influence nuclear policy through Congress.

They had a victory in 1946, when they succeeded in obtaining civilian control of nuclear research and development rather than leaving it under the control of the Department of Defense. Oppenheimer, who was the government's leading science advisor on nuclear weapons policy until he was was ousted later, testified in favor of military control.

In 1954, Oppenheimer was stripped of his security clearance after he opposed the Air Force's demand for ever more multi-megaton thermonuclear bombs. He favored smaller bombs that could be used in densely-populated European battlefields where NATO forces would be outnumbered by Warsaw Pact forces.

The purported reason for stripping Oppenheimer of his clearance, however, was concern that he might be influenced by Communist sympathies. During the same period, the FAS also came under suspicion for favoring arms control. The FAS receded from activism until Jeremy Stone took it over in 1970 and created the FAS that I became involved with.

Eugene Rabinowitch, the principal writer of the *Franck Report* and a long-term collaborator with James Franck in photosynthesis research, became the founding editor of the atomic scientists' movement's newsletter, which became the *Bulletin of the Atomic Scientists*. For a time, I published my policy articles mostly in the *Bulletin*. It was an entry-level journal for activist physicists. It was ideologically sympathetic and a relatively easy place to publish if you had something to say. It still is.

The National Research Council and the American Physical Society

FvH: My fellowship with the National Research Council was for the academic year September 1973 – August 1974. The Council turned out not to be that interesting to me but, again, Joel Primack had a major impact on my trajectory – this time as an activist within the American Physical Society (APS).

Joel suggested that the APS sponsor summer studies on policy issues and that I be charged with organizing a meeting to examine possible topics for their suitability.

I did organize a week-long meeting at Los Alamos National Laboratory during the summer of 1973. I arranged briefings on a number of issues and we came up with topics for possible studies. Two of them became summer studies the following year. The one most relevant to my expertise was on nuclear reactor safety.

In the meantime, at the National Research Council, I also suggested, "Why don't we do a study on reactor safety?" My advisor there responded, "that's too hot for us." In fact, the Council initiates very few of the studies it does. Almost all are requested by different branches of the government. Furthermore, the Atomic Energy Commission had just launched its own study which it hoped would settle the reactor-safety issue.

It was suggested to me that I could be a staffer on one of the ongoing National Research Council studies. I was not interested, however, and my advisor didn't push. Instead, I started studying the reactor safety issue and ended up organizing the APS reactor safety study for the following summer.

The APS Reactor Safety Study

That year, 1974, Wolfgang (Pief) Panofsky,[36] was president of the American Physical Society. He had been one of the younger physicists in the Manhattan Project and was in the observational aircraft that accompanied the bomber that destroyed Nagasaki. He was one of the original members of the President's Science Advisory Committee, which President Eisenhower created after the surprise of Sputnik in 1957 and he was a leading advisor on nuclear arms control for the following half century. My favorite recollection of Panofsky is toward the end of his life when we were standing at adjoining urinals at the National Academy of Sciences and he turned and said to me, "Frank, this is the only place where people know what they are doing!"

Panofsky chose Harold (Hal) Lewis[37] as chairman for this APS study. For some reason, I took an immediate dislike to Lewis. I decided that his interest in the issue was to prepare himself to become a consultant to the nuclear industry. It is very difficult for me to conceal my feelings so the situation quickly escalated into one in which Panofsky felt he had to intervene. Somehow, with the help of Panofsky and Tom Neff, who had taken a class with me at Stanford and was acting as Panofsky's assistant during Panofsky's year as APS President, Lewis and I papered over our mutual dislike sufficiently to proceed.

Again, the summer study was at Los Alamos but this time it was for a month.

While we were there, I met Harold Agnew, the director of of the Los Alamos National Laboratory. He had worked on Fermi's reactor at the University of Chicago in 1942 and had been on the observation plane that had accompanied the bomber that destroyed Hiroshima .

TK: Luis Alvarez[38] was on the same plane. Their mission was to measure the size of the explosion.

FvH: For some reason, Agnew offered me a job at Los Alamos. I said I would come if I could start an arms-control group. He did not mention the subject again.

I still think this is a good idea and not necessarily incompatible with the economic interests of the nuclear-weapons laboratories. Despite the end of the Cold War, the end of nuclear testing, and a ten-fold reduction in US deployed nuclear weapons, funding of the US nuclear-warhead complex is higher than ever (Figure 7).

[36]German-born American physicist who led the construction and operation of the two-mile-long electron accelerator at Stanford. Panofsky also was a leading government advisor and educator on nuclear arms control issues. The "Panofsky Ratio" I had measured as an MIT undergraduate was named after him because he had made the first measurement.

[37]An Oppenheimer student who was a physics professor at the University of California, Santa Barbara.

[38]"American experimental physicist who was awarded the Nobel Prize for Physics in 1968 for work that included the discovery of many resonance particles," https://www.britannica.com/biography/Luis-Alvarez. Alvarez, jointly with his son, Walter, also came up with the asteroid hypothesis for the extinction of the dinosaurs.

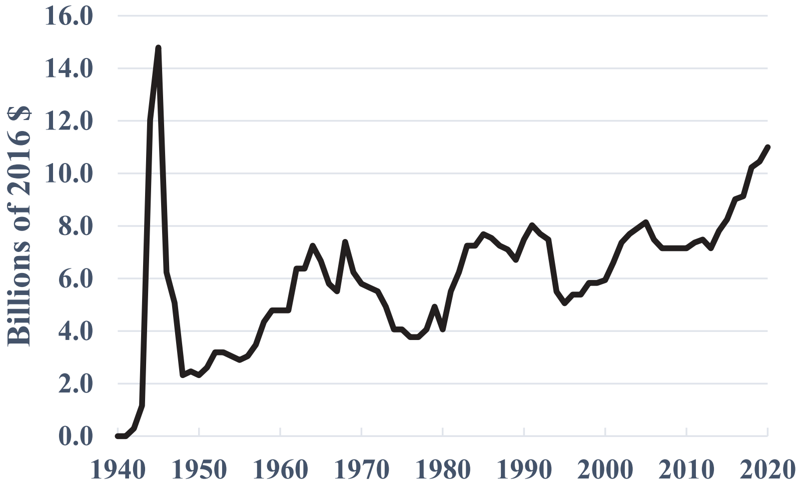

Figure 7. Funding for US nuclear-warhead development and production in constant dollars (author).

Agnew was a great provocateur. Many years later he introduced me to a co-owner of the company, General Atomics: "This is Frank von Hippel. I don't pay any attention to what he says but some people do."

TK: Let's return to the 1974 APS Reactor Safety Study.

FvH: Okay. By coincidence, a few months before our month of study at Los Alamos, the Atomic Energy Commission had released for review its own draft *Reactor Safety Study*. It was the 1970s and power reactors were being sited all over the United States and almost everywhere were being met with "not in my back yard!" opposition. The AEC was trying to deal with this opposition.

Two decades earlier, the AEC had commissioned Brookhaven National Laboratory to do a study that the AEC hoped would prove that the consequences of a reactor accident would not be so bad. But the Brookhaven study, "WASH-740," published in 1956, found that a worst-case accident could be very bad.

In 1965, the AEC had a second study done that it hoped could rule out such a worst-case accident. The consequences were still so bad, however, that the report was released only in 1973 as a result of Congressional pressure.

The AEC's *Reactor Safety Study* took another approach. It estimated the probabilities of severe accidents and then compared their probabilities and consequences with those of other catastrophes such as dam failures or airplane crashes. The study was done by AEC experts under the nominal supervision of Norman Rasmussen, an MIT professor of nuclear-engineering.[39] The AEC's *Reactor Safety Study* was therefore also known as the "Rasmussen Report."

[39]Rasmussen became the head of MIT's nuclear engineering department after the *Reactor Safety Study* was completed. He also chaired a National Academy of Sciences study, *Nuclear Wastes: Technologies for Separation and Transmutation* that concluded that the costs of reprocessing spent fuel in order to fission the plutonium and other transuranic elements it contains far exceed any benefits in reducing the risks from deeply-buried radioactive waste (National Research Council 1996). I have found this report useful in my campaigning against the separation of plutonium from spent power-reactor fuel.

Much of the APS Summer Study was devoted to reviewing the AEC's draft *Reactor Safety Study* (*RSS*). I took responsibility for trying to understand the *RSS* calculations of the consequences of a reactor accidents with large releases of radioactivity. I took a particular large-release scenario for which the authors of the *RSS* had used a computer to average over different wind speeds and directions for different sites to estimate the average summed radiation doses to the surrounding populations. According to the standard "linear hypothesis," which relates radiation dose to the risk of cancer, this "population dose" would determine the number of resulting cancers.

I did a back-of-the-envelope check of the calculation of cancer consequences assuming an average uniform population distribution and an average wind speed and atmospheric conditions and came up with much larger population doses than the AEC study had. So, I tried to understand where the discrepancy was coming from.

Eventually, I found that the AEC team had calculated the population dose only for the first day after the release. It had been assumed that everyone would be evacuated from contaminated areas within a day. But, when I looked at my own calculations, I found that most of the population dose came from small doses to millions of people in areas far from the accident site who incurred individual doses so low that it would be prohibitive to relocate them. When I estimated the long-term doses to the people beyond the relocation zone, I found an average of about 10,000 cancer deaths instead of the 300 estimated in the Rasmussen Report.

I also estimated various other consequences that the Rasmussen Report had failed to estimate, including 20,000 to 300,000 cases of cancerous and non-cancerous thyroid-nodules from inhaling radioactive iodine. A decade later, thyroid cancers – fortunately almost all non-fatal – were the most visible health consequence from the Chernobyl accident. Most were from drinking contaminated milk, however, not from inhalation of radioactive iodine. Both the authors of the Rasmussen Report and I assumed that contaminated milk would be interdicted and that thyroid cancers would be primarily from inhalation of radioactive iodine.

After I reported my findings to the APS study group, our chairman, Hal Lewis, called up the executive director of the Rasmussen study, Saul Levine,[40] and told him what I had found. Levine responded by warning Lewis that the APS would be very embarrassed if our study group included my results in its report. Lewis reported back to our group and suggested that I publish my results separately.

One of the people in our study group was Richard Garwin, the same physicist who had written the article with Hans Bethe about ballistic missile defense. Garwin was a student of Enrico Fermi. Fermi had taken Garwin to Los Alamos over the summer of 1951 when Garwin was 23. Edward Teller and Stanislaw Ulam had just come up with a design concept for a thermonuclear bomb that looked promising. Teller suggested to Garwin that he turn the concept into an actual design. The result, which was tested in the South Pacific the following year was an explosion almost 1000 times more powerful than the Hiroshima bomb. Garwin became instantly famous within nuclear-weapon circles and from then on was a senior government advisor. He is 91 and still active (Figure 8).

[40]Levine earned a Masters of Nuclear Engineering degree at MIT and then had served in the US nuclear navy for 17 years before serving on the staffs of the AEC and NRC, also for a total of 17 years, including as director of the Office of Nuclear Regulatory Research.

Figure 8. Richard Garwin in 2011 (Comprehensive Test Ban Organization).[41]

After hearing Lewis's recommendation that my results not be included in the APS report, Garwin said, "I would be happy to join Frank in publishing his results separately." Lewis' responded, "In that case, we might as well just include them in our report."

In retrospect, Garwin's intervention may have enabled me to use my role in the APS report to launch my new career as a policy physicist.

TK: You knew Garwin before the APS summer study?

FvH: No. Joel Primack had interviewed him for our book. The first time I met Garwin myself was during the APS reactor-safety study. Garwin has been a mentor ever since.

So the APS summer study produced a report critical of the Rasmussen Report. Our report said that the calculations of the probabilities of major accidents were much more uncertain than claimed. It also said that the cancer consequences had been seriously underestimated (Lewis et al. 1975).

Ralph Nader, who was very anti-nuclear and who I met as a result of my new fame, was very happy.

There were some changes in the final version of the *Reactor Safety Study*. These changes mostly related to the errors I had pointed out in the calculations of the accident

[41]https://commons.wikimedia.org/wiki/File:Richard_Garwin_2011.jpg.

cancer consequences. But the changes were largely invisible in the published summary of the report where it made its comparisons of the consequences of reactor accidents with those of other man-made and natural disasters. The comparisons were made on the basis of "early fatalities," i.e. excluding the cancer deaths that dominate the fatalities from a reactor accident – by a factor of hundreds in the case of the Chernobyl accident, for example.

Tom Cochran and I published the first rough estimates of 2,000–40,000 cancer deaths as a result of the Chernobyl accident. These cancers are invisible in the sea of millions of other cancers in Europe due to other causes (Von Hippel and Cochran 1986, 18). The most recent official estimate of which I am aware is 7,000–40,000 (Cardis et al. 2006, 1224). The number of early fatalities from acute radiation syndrome among the reactor operators, firemen and others is still officially 28 (UN Scientific Committee on the Effects of Atomic Radiation 2011, Table C-1, 146).

In the meantime, the first Congressional Science Fellowships that Joel Primack had promoted had been awarded in 1973. I was on the selection committee for the first two fellowships awarded by the American Association for the Advancement of Science. We awarded one to Jessica Tuchman (now Jessica Mathews), a molecular biologist and daughter of Barbara Tuchman, the famous historian. Jessica later became famous in her own right.[42]

Jessica took her year as a congressional fellow with Representative Morris Udall, who had just become the chairman of the new oversight subcommittee for the Nuclear Regulatory Commission (NRC). When Congress abolished the Atomic Energy Commission, it also abolished the AEC's oversight committee, the Joint (House-Senate) Committee on Atomic Energy. The new NRC, which was now responsible for the *Reactor Safety Study*, therefore had separate oversight subcommittees in the House and Senate.

Jessica asked me to brief the committee on nuclear-energy issues. My briefing was reprinted at the beginning of the subcommittee's first hearing in May 1975 (Von Hippel 1975).

When Jessica learned of the APS reactor safety study, she thought it would be interesting for Udall to have a hearing. Saul Levine, Henry Kendall, Wolfgang Panofsky, Norman Rasmussen, I and two others testified.[43] As a result, Udall asked the Nuclear Regulatory Commission (NRC) to reconsider its endorsement of the Rasmussen Report.[44]

Ultimately, the NRC decided it would commission its own review of the Rasmussen report. Hal Lewis was appointed chair and I was invited to be a member of the review committee. Initially, everyone else on the committee had a better opinion of the Rasmussen Report than I had.

The panel had many very long discussions. I was the principal critic and would point to specific calculations and statements in the Rasmussen Report and ask, "What do you think about this … and this?"

[42]After working for Udall, Mathews served in the Carter Administration's National Security Council as Director of Global Issues. She then joined the *Washington Post* as a columnist for two years, followed by 11 years as the research director of the World Resources Institute and 14 years as president of the Carnegie Endowment for Peace.

[43]*Reactor Safety Study (Rasmussen Report)*, Oversight Hearing before the Subcommittee on Energy and Environment of the Committee on Interior and Insular Affairs of the House of Representatives, 11 June 1976. My testimony was published as Von Hippel (1977).

[44]Chairman Udall's request for the review is reprinted in Lewis et al. (1978, 55).

Initially, the other members of the review group would concede that what I pointed out was problematic but asserted that, nevertheless, the report was a great contribution. Finally, however, after many such discussions, one of the other members asked, "Can anybody tell me anything that the Rasmussen report did right?" So the majority did turn around and we wrote a critical report.

The NRC responded by putting out a statement saying most significantly that:[45]

- "The Commission withdraws any explicit or implicit past endorsement of the Executive Summary" of the Rasmussen Report.
- "The Commission accepts the Review Group Report's conclusion that absolute values of the risks presented by WASH-1400 should not be used uncritically either in the regulatory process or for public policy purposes."

The NRC had taken a long time to organize the review, however. We finally produced our report in 1979, three years after the Udall hearing. Nevertheless, Udall was still interested and had a hearing on the "Lewis Report".[46]

Coincidentally, the Three Mile Island accident in Pennsylvania, the first meltdown accident of a light water power reactor, happened a month after that hearing. Fortunately, the containment building did not fail. Jan Beyea[47] and I wrote an article warning, however, that other reactor containments were more vulnerable to overpressure.

We pointed in particular to the small-volume containments on the first generation General Electric boiling water reactors and urged that all power-reactor containments be equipped with filtered vents through which the radioactive gases could be released, if the pressure of the gases from a reactor meltdown approached a containment's failure point (Beyea and von Hippel 1982, 52).

After the Three Mile Island accident, West European regulators did require such filtered vents to be installed. They were not required in the U.S. or Japan, however.

After the Fukushima accident, Japan's new Nuclear Regulation Authority required the installation of filtered vents in all of Japan's power reactors.

In 2012, the US Nuclear Regulatory Commission's (NRC's) Fukushima Lessons Learned review group recommended to the NRC Commissioners that NRC require filtered vents on US Fukushima-type reactors. Before this recommendation, Allison MacFarlane, then NRC chairman, invited me to meet with some of the NRC staffers working on the analysis. They told me that they had Jan's and my 30-year-old article pasted on their wall as a reminder of how long the issue had festered within the NRC.

In March 2013, MacFarlane was outvoted in her support of the staff recommendation to require filtered vents on US Fukushima-type reactor containments. The primary

[45]"NRC Statement on Risk Assessment and the Reactor Safety Study Report (WASH-1400) in Light of the Risk Assessment Review Group Report," 18 January 1979, https://www.nrc.gov/docs/ML1112/ML11129A163.pdf.

[46]*Reactor Safety Study Review*, Oversight Hearing before the Subcommittee on Energy and Environment of the Committee on Interior and Insular Affairs of the House of Representatives, 26 February 1979.

[47]Beyea worked in our program on reactor-safety issues from 1976 to 1980. He then went on to become the chief scientist of the Audubon Society. In 1996, he formed his own NGO, Consulting in the Public Interest. Recently, I collaborated with him on critiquing the doses that the Air Force had estimated to deny compensation to approximately 1600 US servicemen who had participated in the cleanup of plutonium contamination from a 1996 crash in Spain of a US bomber loaded with four thermonuclear bombs (Beyea and von Hippel 2019).

rationale used by the majority Commissioners, who voted three to two against the requirement, was that the probability-weighted costs would exceed the benefits.

Two months earlier, the Republican majority of the NRC's House of Representatives oversight committee had written to the Commissioners quoting Commissioners who had argued publicly that the Fukushima accident could not have happened in the United States and the fact that the staff's probabilistic cost-benefit analysis did not support the installation of filtered vents.

Through Congress, the NRC had suffered regulatory capture by the industry that it was regulating. And the tool that had been been used to rationalize the capture was the same probabilistic risk assessment that the AEC had introduced 40 years earlier to try to convince the US public that nuclear power plants were safe. Unfortunately, as two colleagues and I showed in a recent confrontation with the NRC over the practice of dense-packing spent fuel pools in the US, regulation based on estimates of the probabilities of events that have never happened is so arbitrary and opaque that it can be and has been skewed against safety upgrades that nuclear-power plant operators consider unacceptably costly (Lyman, Schoeppner, and von Hippel 2017).

Disclosure Statement

No potential conflict of interest was reported by the author.

Notes on Contributors

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