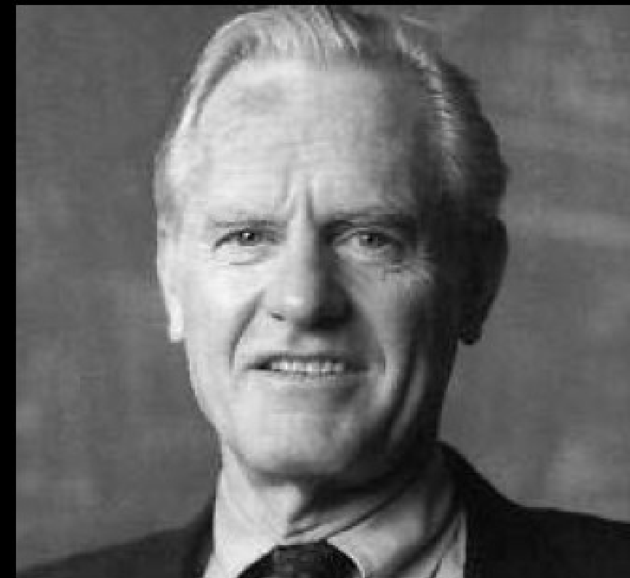
Physics in the Interest of Society

Herman Feshbach



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Physics in the Interest of Society

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Physics in the Interest of Society

Massachusetts Institute of Technology
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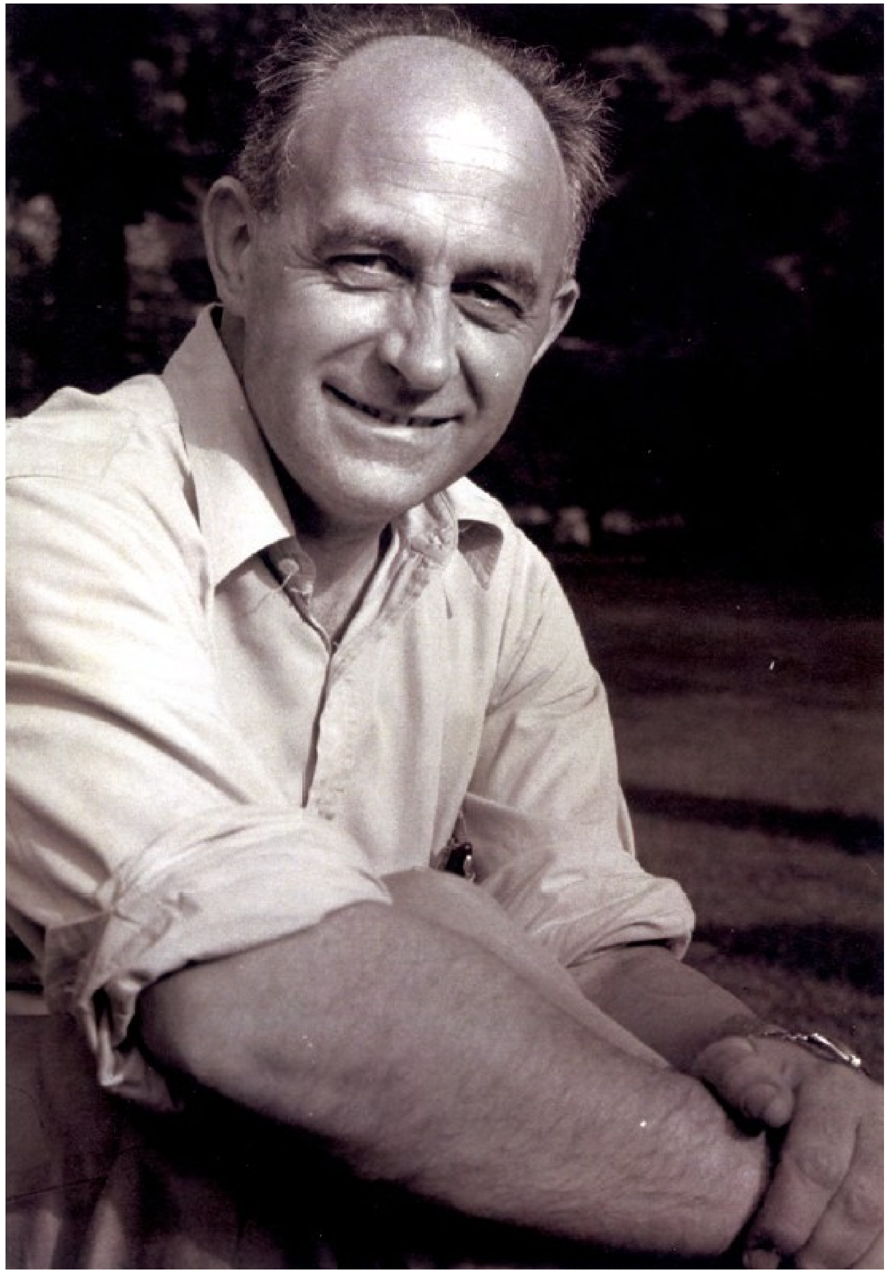
In preparing for this lecture I was pleased to reflect on outstanding role models over the decades. But I felt like the centipede that had no difficulty in walking until it began to think which leg to put first. Some of these things are easier to do than they are to describe, much less to analyze.

Moreover, a lecture in 2011 is totally different from one of 1990, for instance, because of the instant availability of the Web where you can check or supplement anything I say. It really comes down to the comment of one of Elizabeth Taylor later spouses-to-be, when asked whether he was looking forward to his wedding, and replied, "I know what to do, but can I make it interesting?" I'll just say first that I think almost all Physics is in the interest of society, but I take the term here to mean advising and consulting, rather than university, national lab, or contractor research.

I received my B.S. in physics from what is now Case Western Reserve University in Cleveland in 1947 and went to Chicago with my new wife for graduate study in Physics. Uneasy without a laboratory, I asked Enrico Fermi to take me on as an assistant, which he did, and as a Ph.D. candidate, so I received that degree in December 1949 for an experiment on the angular correlation between a gamma ray and the previous beta ray from nuclear decay. In the course of my thesis work, I invented and published a simple coincidence circuit of few-nanosecond resolving time that became the standard in the field for several decades, and also the “adiabatic light pipe” for scintillation counters.

I can’t say that my entry into the world of weapons, healthcare, and national technology was the result of an urge to prevent or solve social and national problems, but perhaps it was. It was at least equally due to my unfamiliarity with the system of obtaining research grants and to the fact that the University of Chicago paid faculty salaries for 9 months, and I and my family ate for 12. So when Enrico Fermi suggested that I accompany him to Los Alamos in the summer of 1950 as a consultant to the Laboratory, I was happy to do so, and to learn about the secret world of nuclear weapons. Remember, in August 1945 WWII was concluded by the explosion of two nuclear weapons against Hiroshima and Nagasaki, and in 1949 the Soviet Union tested its first nuclear weapon, and the U.S. Government and citizenry were concerned about nuclear war with the Soviet Union and about nuclear annihilation.



Enrico Fermi in Los Alamos, 1952. (Photo by Harold M. Agnew)

I spent the first week in the Los Alamos Classified Report Library reading the weekly progress reports back to 1943 of the groups that developed the nuclear weapon.[1] I was fortunate to share an office with Enrico Fermi, our desks facing one another, his back to the window, mine to the door. I tended the safe and maintained my secret laboratory notebook in which Fermi would occasionally write something for his own record or for my edification. For example, here is his calculation of seismic radiation from a fully contained underground nuclear explosion, perhaps motivated by a question of the intensity of seismic waves at Las Vegas, NV, produced by underground detonations at the nuclear test site some 110 km away.

[1] I have reported some of this in my papers, "Working With Fermi at Chicago and Los Alamos" **http://tinyurl.com/3b55eqb**, and "Working With Fermi at Chicago and Post-war Los Alamos" http://fas.org/rlg/010929-fermi.htm.

Explosion in underground cavity

(E. Fermi in R.L. Garwin's notebook LANB 3616)

Total energy $= 5\times10^{21}$ ergs $= W$

Initial radius $R = 33$ m

Initial volume $\frac{4\pi}{3}R^3 = 1.25\times10^5\ m^3$

$$p = \frac{W}{V}(\gamma-1) = \frac{5\times10^{21}}{1.25\times10^{11}}\,\frac{2}{3} = 2.7\times10^{10}$$

From p. 6

Assume equation of state of rock

$$E = \frac{1}{2}k(v_0-v)^2 = \text{en per cc}$$

$$p = (v_0-v)\,k$$

$$c = \sqrt{k v_0^2}$$

$v_0 = .4$ $c = 5\times10^5$ $k = 1.57\times10^{12}$

From 3rd Hugoniot

$$\frac{1}{2}k(v_0-v_1)^2 = \frac{1}{2}p\,(v_0-v_1)$$

$$v_0 - v_1 = \frac{p}{k} = \frac{2.7\times10^{10}}{''} = .0172$$

$v_0 = .4000$

$v_1 = .3828$

"Explosion in underground cavity," seven pages by Enrico Fermi in R.L. Garwin's Los Alamos notebook (LANB 3616), calculating the radiated wave from an explosion in an underground cavity of initial radius 33 m. and total energy 100 kt. (070050.EF)

07/00/50

30

From 2nd Hug

$$U^2 = \frac{v_0^2}{v_0-v_1}\,p = \frac{.4^2}{.0172}\,2.7\times10^{10} = 25.1\times10^{10}$$

From 1st Hug

$$u = \frac{v_0-v_1}{v_0}\,U = \frac{.0172}{.4}\,5\times10^5 = 2.15\times10^4$$

radial expansion $q'-1$ $q =$ new radius

lateral " $\frac{q}{r}-1$

Density of elastic energy

$$\frac{\alpha}{2}\left[(q'-1)^2 + 2\left(\frac{q}{r}-1\right)^2\right] + \beta\left[\left(\frac{q}{r}-1\right)^2 + 2\left(\frac{q}{r}-1\right)(q'-1)\right]$$

Elastic energy =

$$W_{el} = \int 4\pi r^2 dr \left\{ \quad \right\}$$

Minimum problem 31

$$r^2 q'' + 2r q' - 2q = a$$

Solution

$$q = r + \frac{a}{r^2}$$

$$\frac{q}{r} - 1 = \frac{a}{r^3} \qquad q' - 1 = -\frac{2a}{r^3}$$

$$W_{el} = 4\pi(\alpha-\beta)\frac{a^2}{r_0^3}$$

$$p_{el} = 2(\alpha-\beta)\frac{a}{r_0^3} = 2(\alpha-\beta)\frac{q_0 - r_0}{r_0}$$

$$\frac{\alpha}{2} + \beta = \frac{3}{2} k v_0^2$$

$$\sigma = \frac{\beta}{\alpha+\beta} = \text{Poisson ratio} = .3$$

$$.7\beta = .3\alpha \qquad \alpha = \frac{7}{3}\beta$$

$$\left(\frac{7}{6}+1\right)\beta = \frac{3}{2}\, 1.57\times 10^{12} \times .16 = .378\times 10^{12}$$

$$\beta = 1.74\times 10^{11} \qquad \alpha = 4.06\times 10^{11} \qquad \alpha - \beta = 2.32\times 10^{11}$$

32 Adiabatic gas expansion

$$p\left(r^3\right)^{5/3} = \text{const}$$

$$p r^5 = 2.7\times 10^{10} \times 3300^5 = 1.1\times 10^{28}$$

$$2(\alpha-\beta)\frac{q_0 - r_0}{r_0} = \frac{p_0 r_0^5}{q_0^5} \qquad \frac{q_0}{r_0} = x$$

$$x^5(x-1) = \frac{p_0}{2(\alpha-\beta)} = \frac{2.7\times 10^{10}}{2\times 2.32\times 10^{11}} = .058$$

$$x = 1.046$$

$$.046 \times 3300 = 152 \text{ cm}$$

$$\frac{W}{x^2} = \text{en. in gas}$$

$$W_{el} = 4\pi(\alpha-\beta) r_0 (q_0 - r_0)^2 = 4\pi(\alpha-\beta) r_0^3 (x-1)^2$$

$$= 4\pi r_0^3 \cancel{(\alpha-\beta)} \frac{p_0^2}{4(\alpha-\beta)^{\cancel{2}} x^{10}}$$

$$W = \frac{p_0 V_0}{\gamma - 1} = \frac{3}{2} p_0 V_0$$

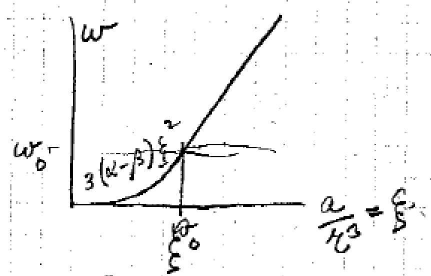
5% of energy is elastic radiation

period of order .040 sec

33

Assume plastic flow for energy density $> w_0$

En density



$$w = \begin{cases} 3(\alpha-\beta)\xi^2 & \text{when} < w_0 \\ w_0 + 6\xi_0(\alpha-\beta)(\xi - \xi_0) \end{cases} \qquad \xi < \xi_0 = \sqrt{\frac{w_0}{3(\alpha-\beta)}}$$

$$W = 4\pi\int_{r_0}^{\infty} w\, r^2\, dr = 4\pi\int_{r_0}^{r_1}\left[w_0 + 6\xi_0(\alpha-\beta)\left(\frac{a}{r^3}-\xi_0\right)\right]r^2 dr +$$

$$+ 4\pi\int_{r_1}^{\infty} 3(\alpha-\beta)\frac{a^2}{r^6}r^2 dr \qquad \frac{a}{r_1^3} = \xi_0$$

$$\frac{W}{4\pi} = (\alpha-\beta)\frac{a^2}{r_1^3} + 3(\alpha-\beta)(\xi_0^2 - 2\xi_0^2)\frac{r_1^3 - r_0^3}{3} + 6(\alpha-\beta)\xi_0 a \ln\frac{r_1}{r_0}$$

$$\frac{W}{4\pi} = (\alpha-\beta)\xi_0 a + (\alpha-\beta)(\xi_0^2 - 2\xi_0^2)\left(\frac{a}{\xi_0} - r_0^3\right) + 2(\alpha-\beta)\xi_0 a \ln\frac{a}{\xi_0 r_0^3}$$

34

$$\frac{\partial V}{\partial a} = 4\pi \qquad \frac{\partial W}{\partial V} = p = \frac{1}{4\pi}\frac{\partial W}{\partial a}$$

$$\frac{p}{\alpha-\beta} = \xi_0 + \xi_0 - 2\xi_0 + 2\xi_0 \ln\frac{a}{\xi_0 r_0^3} + 2\xi_0$$

$$= 2\xi_0 + 2\xi_0 \ln\frac{a}{\xi_0 r_0^3}$$

$$p = 2(\alpha-\beta)\xi_0 + 2(\alpha-\beta)\xi_0 \ln\frac{a}{\xi_0 r_0^3} \qquad \frac{r_1^3}{r_0^3} = 1.03$$

Assume $\xi_0 = .01$

$$\frac{20}{400} = .05 = .01 + .04$$

$$P = p_{rr} \qquad Q = p_{\perp\perp}$$

$$\frac{\partial}{\partial r}\left\{r^2 P(r)\right\} = 2rQ \qquad \text{(for sphere)}$$

$$\frac{d}{dr}\left\{r P(r)\right\} = Q \qquad \text{(for cylinder)}$$

In elastic case

$$P = \alpha(q'-1) + 2\beta\left(\frac{q}{r}-1\right)$$

$$Q = (\alpha+\beta)\left(\frac{q}{r}-1\right) + \beta(q'-1)$$

$$r^2\left[\alpha q'' + 2\beta\frac{q'}{r} - 2\beta\frac{q}{r^2}\right] + 2r\left[\alpha(q'-1) + 2\beta\left(\frac{q}{r}-1\right)\right] =$$

$$= 2r(\alpha+\beta)\left(\frac{q}{r}-1\right) + 2r\beta(q'-1)$$

$$q''\left[\alpha r^2\right] + q'\left[~~2\beta r~~ + 2r\alpha\right] + q\left[~~-2\beta + 4\beta~~ - 2\alpha\right]$$

$$q'' + \frac{2q'}{r} - \frac{2q}{r^2} = 0$$

~~In plastic case~~

Assume $P - Q < A$

In plastic flow case $P - Q = A$

$$\frac{d}{dr}\left\{r^2 P(r)\right\} = 2r\,(P - A)$$

$$P = P_0 - 2A \ln\frac{q}{q_0}$$

(for cylinder without factor 2)

That first summer I was attached to the Physics Division at the Lab, headed by Jerry Kellogg, who was later to advise his friends at Columbia University and IBM in 1952, when they proposed to offer me a job, “If you hire him, you will regret it, but you will regret it more if you don’t.”

At Los Alamos in June 1950, I began to build an apparatus for the measurement of d-t and d-d cross sections down to 10 keV or so (useful for the thermonuclear weapon work), and although I couldn’t complete the experiment, by far, before the end of summer, the Lab decided that this was a necessary and reasonable approach and created a team to carry out the measurements. Fermi recalled an ingenious British physicist, Jim Tuck, who had been at Los Alamos during the war, and brought him first to Chicago while his clearance was obtained, and then to LASL to head the team, which published in 1954[2].

At the same time, I was interested in applying my new-found knowledge of shock waves and the phenomena of nuclear weaponry and devised several approaches for obtaining detailed information in nuclear weapons tests, publishing documents, including the proposal to use stable isotopes at particular points in a 1951 nuclear

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[2] "Cross Sections for the Reactions D(d,p)T, D(d,n)He**3, T(d,n)He**4, and He**3(d,p)He**4 below 120 keV," by W.R. Arnold, J.A. Phillips, G.A. Sawyer, E.J. Stovall, Jr., and J.L. Tuck in Physical Review Vol. 93, No. 3, pp. 483-497, Feb. 1, 1954.

explosion test in order to determine position, neutron spectrum, and fluence (time integrated flux) by the amount of the resulting radionuclide.[3] This became a standard technique.

I also identified a long-range, long-time interaction between nuclear explosions, which was verified in one of the early weapon tests in Nevada. But in August or September I returned to Chicago to take up my regular duties of research and instruction, working with the 100-MeV betatron and preparing experiments for the 450-MeV synchrocyclotron being built at Chicago. Here I provided unwelcome suggestions and advice to my colleagues, while I worked to build external targets containing liquid hydrogen or liquid deuterium for the meson beams from the cyclotron.

But I should talk about more important matters on which I have some knowledge and not necessarily those to which I contributed. As an aside, over many decades I have tried to persuade individuals and organizations that innovation and not invention should be rewarded. Too often, one finds a person so wrapped up in creating something new that he or she ignores what has been done outside. Further, there is

[3] The proposal was published August 11, 1950, and a status report October 12, 1950.

little reward for bringing into an organization best practices or for licensing a patent that would solve the problem at affordable cost. Early on, I felt, perhaps naturally, that getting credit for one's ideas was important. Only in 1953 did I learn from Jerome B. Wiesner, who in 1961 was to be President John F. Kennedy's Science Advisor and head of the President's Science Advisory Committee, "You can either get credit for something or get it done, but not both."

During the summer of 1950, from a distance of about two meters, I watched Enrico Fermi and mathematician Stan Ulam, morning after morning, do their best to calculate the performance of a propagating fusion burn in an infinite cylinder of liquid deuterium. This, naturally, required attention to the local temperature, with the cylinder zoned axially and radially, the resulting d-d (and d-t) reaction rate, the loss of energy from ions to the electrons, and from the electrons by Bremsstrahlung. There was also the deposition of energy locally from alpha particles and, at a distance, from the neutrons from the reaction, with resultant energy density contributing pressure gradients that led to the hydrodynamic disassembly of the burning stick of deuterium.

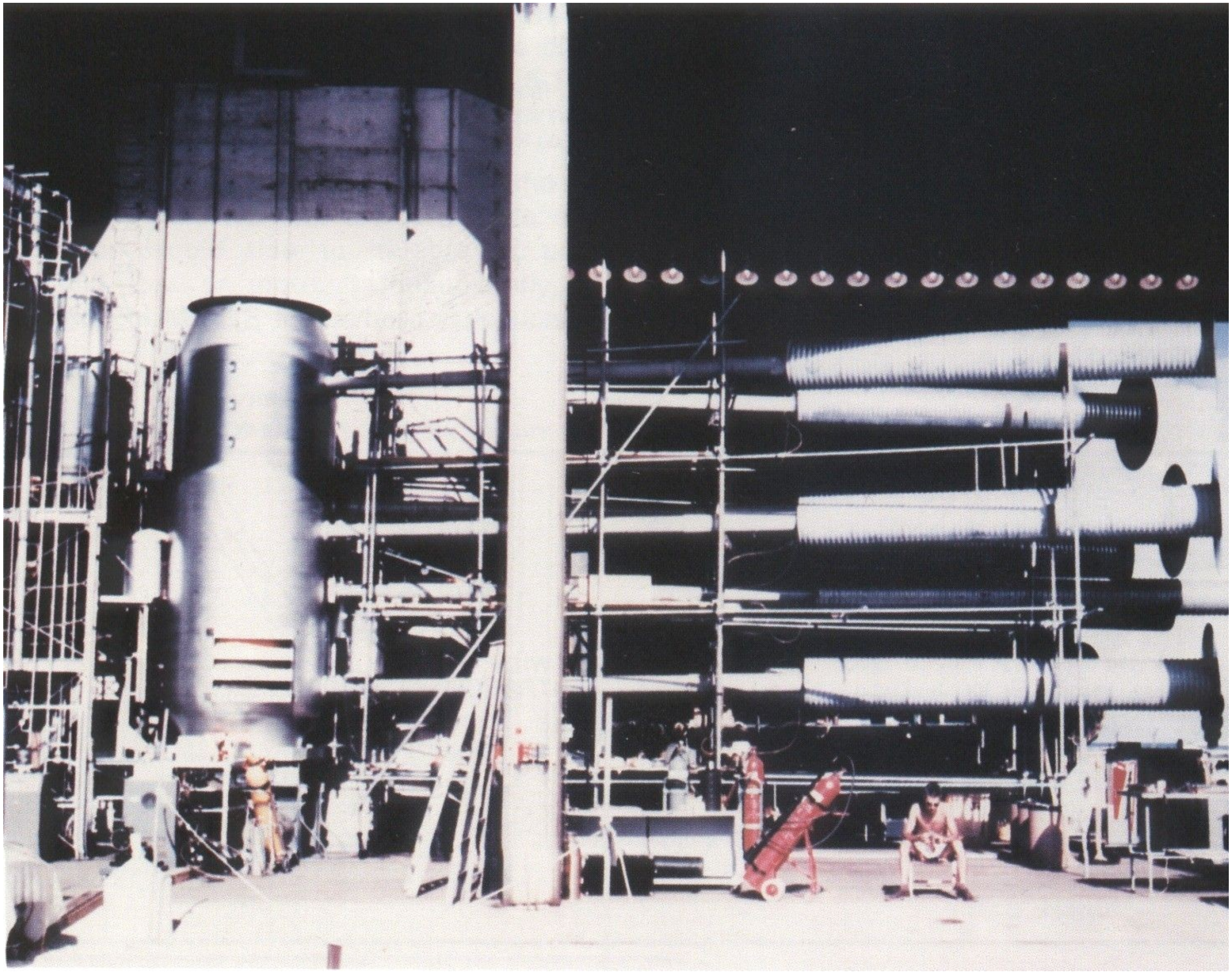
Except that it would not burn without being enriched with more tritium than would ever be possible. Ulam would sit next to Fermi's desk, while Fermi would fill out "cells" on a paper spreadsheet, with the time going down the page. He had transformed the differential equations to first-order equations, so that from one time step to the next there would be the calculation of quantities and the addition of an increment. Multiplications and exponentials Fermi did on his slide rule, and additions and subtractions on a motor-driven Marchant desk calculator. After filling in six rows or so on the spreadsheet, Fermi and Ulam would call in their computer, Miriam Caldwell, who would day after day take away the spreadsheet with its equations and bring it back the next morning filled in for the two scientists to plot, diagnose and to prescribe different parameters.

Fermi was rightly famous. He was very modest about his accomplishments, except for his physical stamina. He had received the Nobel Prize December 1938 for his work with slow neutrons, including the discovery of transuranic elements, most of which turned out a few weeks later to be fission products from the irradiation of uranium with neutrons. Fermi was also known also for his four-field theory of beta decay, a manuscript rejected by *Nature* magazine in 1933.

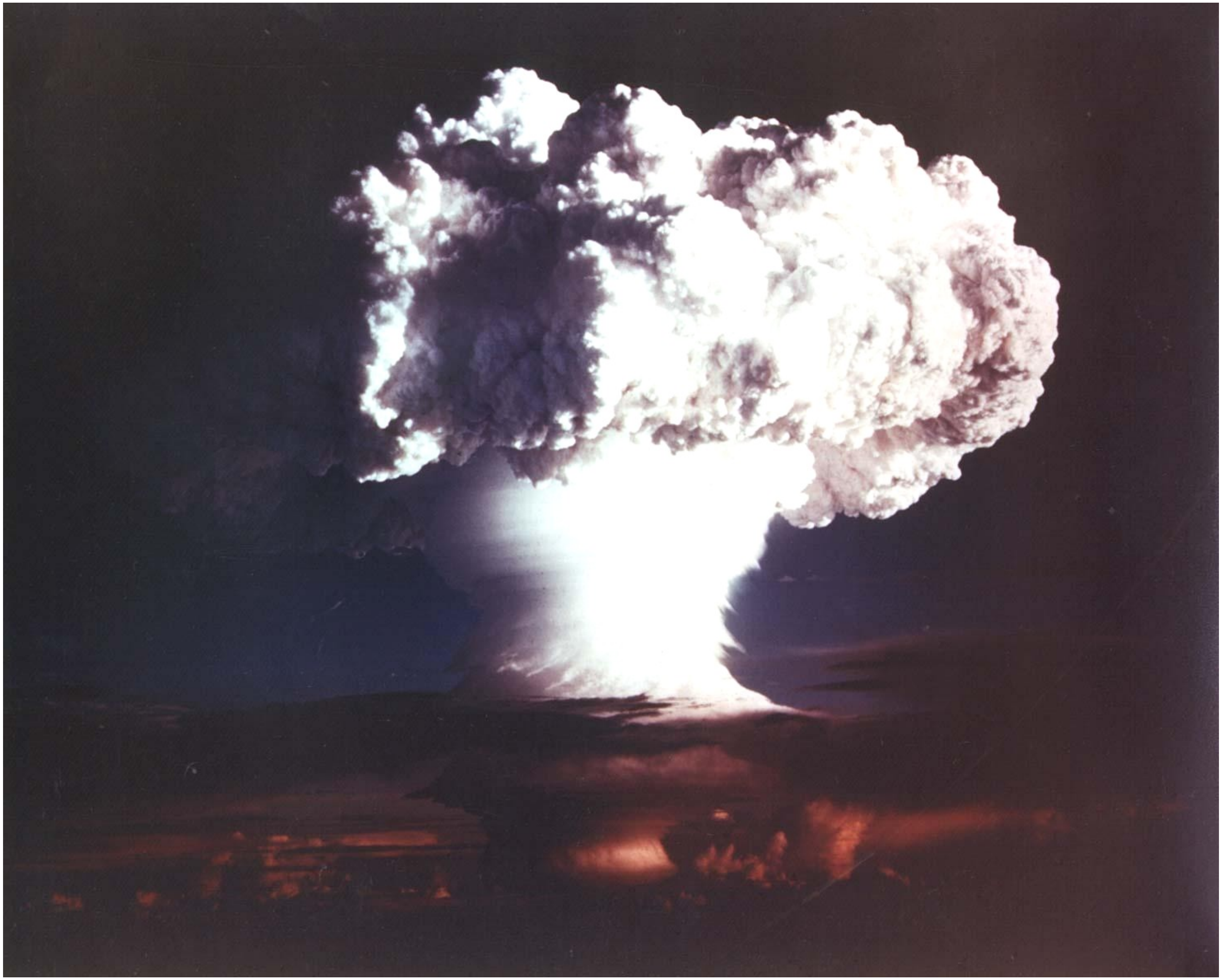
Fermi had used the opportunity at the Nobel Prize Award to leave Italy with his wife and two children, with the secret plan not to return but to take a position at Columbia University. Within weeks after the Nobel Prize, Lise Meitner and Otto Frisch conceived and published the concept of nuclear fission. When the news reached Fermi at Columbia he naturally wanted to study fission as a physical phenomenon, but was persuaded by Leo Szilard that fission had the potential to lead to nuclear weapons and that it was important for Fermi to study this instead. His work on exponential piles of uranium (or uranium oxide) lumps in a graphite lattice led to the first self-sustaining nuclear reactor at the University of Chicago December 2, 1942. Los Alamos was founded in March 1943 in order to build bombs of the enriched uranium to come from Oak Ridge and the reactor-produced plutonium to come from Hanford, Washington. Fermi, with Wigner and others remaining at Chicago, was designing the 200 megawatt (thermal) production reactor at Hanford, on the basis of data from the 2-watt Chicago pile. When Fermi did go to Los Alamos in 1944, staying until December 1945, he was referred to as "The Pope," for the infallibility of his predictions and the accuracy of his estimates. Rather than answer a question directly, Fermi would often ask, "Have you considered the influence of X?" But, if pushed, he would give the answer and an estimate of the magnitude of the quantity involved.

When I returned to LASL in May 1951, Edward Teller told me what had been going on at the Lab, particularly the Teller-Ulam invention of radiation implosion. He asked me to provide the design of an experiment that would be absolutely persuasive of the correctness of this principle. It seemed to me easier to achieve and much more persuasive to demonstrate this principle at full scale. After I had sketched the design of MIKE, published at LASL July 25, 1951, I still had a month or so that summer at the Lab, and so designed flyable versions of the liquid-deuterium thermonuclear weapon, which I learned much later were actually built and deployed, under the name, JUGHEAD.

MIKE was tested November 1, 1952 with a yield of 11 megatons, almost 1000 times the yield of the Hiroshima bomb. As Teller later wrote, "Garwin's blueprint had been criticized by many people, including Hans Bethe. In the end the shot was fired almost precisely according to Garwin's design, and it worked as expected."



Ivy Mike in preparation



Ivy Mike mushroom cloud, 11 megatons

When I moved to New York December 1952 to join the IBM Watson Scientific Laboratory at Columbia University, It was with a clause in my employment contract that permitted me to spend one-third of my IBM time working with the U.S. government on matters of national security and technology, and IBM would have no access to the substance of my involvement. This was to hold until my retirement from IBM more than 40 years later.

I moved from Chicago because I was more interested in conducting my own research than in working in particle physics as a member with a group of six, with the necessity to say what one was going to do six weeks in advance. Little did I know that it would become groups of 600 and a time horizon of six years. So I began to work in low-temperature physics (condensed matter) with thin-film superconductors and superfluids and other aspects of He-3 and He-4.

Not long after I arrived in New York, and at about the same time as the birth of our second son in March 1953, I was asked by the top echelon of IBM to work for a year with the Project LAMP LIGHT study group of MIT, which was a collection of physicists and electrical engineers exploring the extension of the air defense system of the United States and Canada to the sea lines of approach of Soviet bombers. This is not what I had signed up for at IBM, but I had intended to involve myself in the technology of information storage and transmission, so I was, in fact, quite interested in the subject. I managed to negotiate spending half-time for the year, so I believe that I was at LAMP LIGHT three days a week, while my long-suffering wife managed with two young sons at home at our apartment in Riverdale, NY.

This did introduce me to Jerome B. Wiesner, in 1961 to become President John F. Kennedy's Science Advisor and PSAC chair, and Jerrold Zacharias, Professor of Physics at MIT. It was from Zacharias that I learned, "Don't get it right, get it written!" And from the other members of the study group the science and engineering of radar and communication systems, including "meteor-scatter radio communications" and the like. I was introduced to a whole additional semi-secret world of technology but also to the overall weapon delivery systems and contest of offense vs. defense of strategic nuclear weapons.

Jerry Wiesner Jerrold Zacharias

Although I did not know it at the time, Wiesner and Zacharias were members of the Science Advisory Committee of the Office of Defense Mobilization, nominally advising the President of the United States, although he had never communicated with it. That is, until September 1954, when President Dwight D. Eisenhower met with the SAC to formally initiate the Technological Capabilities Panel (TCP) in response to the first Soviet test of a nuclear weapon containing fusion fuel, and the U.S. test, CASTLE BRAVO of a 15-megaton deliverable (i.e. solid-fuel-- lithium hydride) nuclear weapon.

The exceptional performance of the TCP under James Killian led to the highest priority being given by President Eisenhower to the Air Force ICBM program and then to the mid-range ballistic missile program, to the first flight of a fleet of Top-

Secret U-2 reconnaissance aircraft[4] over the Soviet Union in 1956, to the development of the Top Secret CIA OXCART A-12 (later dubbed the SR-71) Mach-3 reconnaissance aircraft, and to the CORONA film-return satellite system that with its first operational flight in August 1960 dispelled the myth of the missile gap in favor of the Soviet Union—one of the elements of the Kennedy presidential campaign to which Wiesner was a technical advisor.

The SR-71 OXCART Mach-3 Recon Aircraft

Not until 1957 were the SAC and the TCP elevated to the Eisenhower White House 18-person President's Science Advisory Committee (PSAC) of which Wiesner and

[4] https://www.cia.gov/library/center-for-the-study-of-intelligence/csi-publications/books-and-monographs/the-cia-and-the-u-2-program-1954-1974/u2.pdf

Zacharias became members, and to which I was a consultant. I then had two 4-year terms on PSAC under Presidents Kennedy and Johnson, and Nixon.

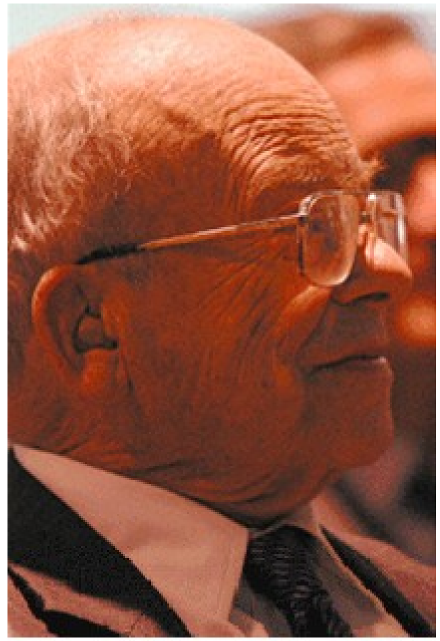
An Eisenhower passion was a ban on explosive testing of nuclear weapons; in his farewell statement to the American people he characterized the failure to obtain a universal ban on nuclear weapon tests as the greatest failure of his or of any other administration.

For PSAC I chaired several of the military-oriented panels, which like the parent committee, typically met for two days a month and took their tasks very seriously. I was a member also throughout the PSAC era of the Strategic Military Panel, SMP, together with Hans Bethe of Cornell, "Pief" Panofsky of Stanford, and others, where my background in nuclear weapons, radar, and intelligence was helpful. I was impressed by the seriousness and dedication of these people, and their commitment to informing the President and his staff, and to providing not arguments in favor of a preconceived program, but potential solutions with their positive and negative aspects.

I mention my copying and distributing the Rachel Carson *New Yorker* articles, later published in book form as "Silent Spring," that led to the creation of the PSAC Panel on Insecticides and Pesticides, under the chairmanship of John Tukey, statistician and extraordinary contributor from Bell Labs and Princeton University. I used to sit next to John at PSAC meetings, in part to eat some of his supply of dried prunes.

Panofsky, in particular, was a phenomenon of energy and words. He would use his flight from Stanford (San Francisco Airport) to write draft position papers, and his flight home to do more. He recounts that after his return from a two-day PSAC (or Panel) meeting in Washington, he and his wife Adele would take the laundry from their home with five children to a local laundromat, then proceed to the university where she would help him set up physics demonstration equipment for the early-morning lecture the next day. Then back to the laundromat to pick up the dried clothes. And, of course, Panofsky's secretary would then be busy transcribing his in-person dictation into lucid and technically competent prose for circulation to his PSAC colleagues or to those on the Strategic Military Panel.

This was complicated by the very secret nature of much of the work, but fortunately Pief and Sid Drell had an authorized security-cleared facility for defense secret and Atomic Energy Commission Restricted Data ("RD") documents.



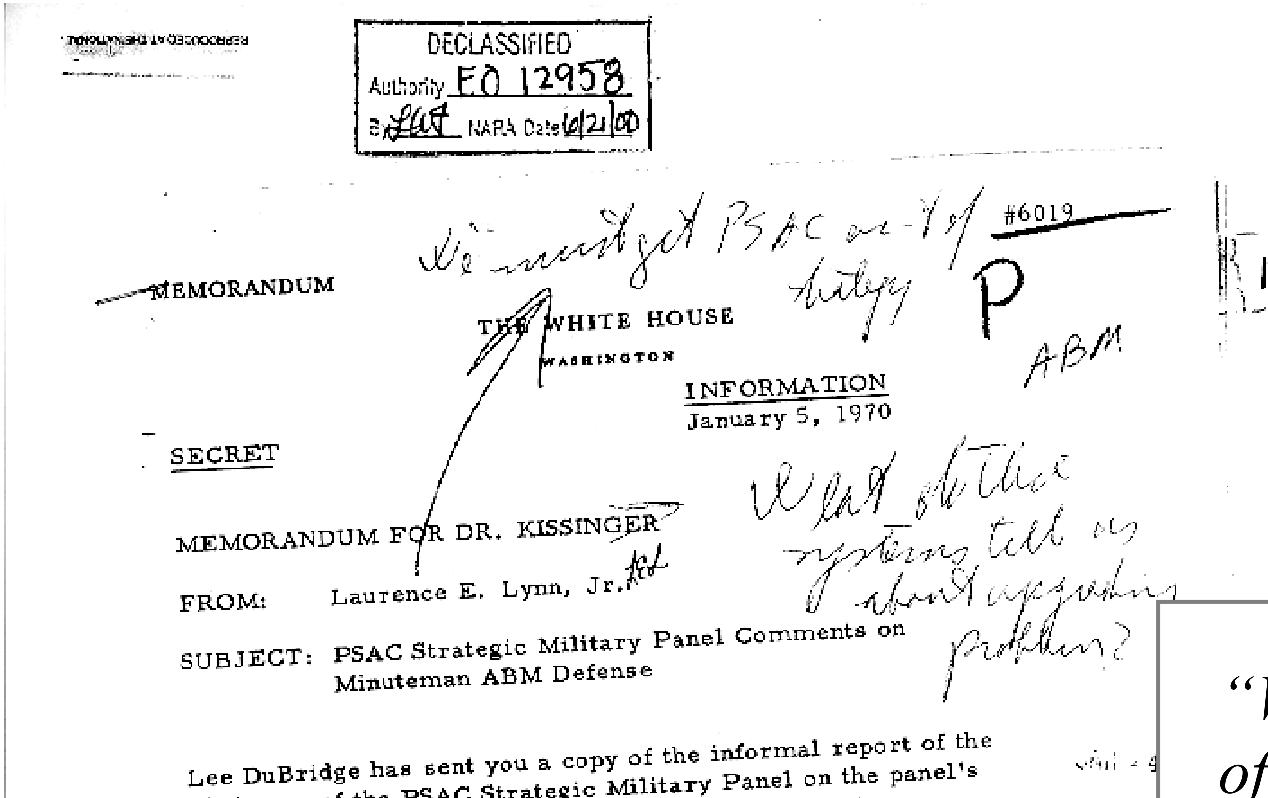
“Pief” Panofsky

The PSAC SMP was concerned with both U.S. offensive and defensive strategic weapons, although the long-range bombers were handled in the PSAC Military Aircraft Panel (MAP) which I chaired. We dealt also with Soviet ballistic missiles and bombers and with the creation of a Ballistic Missile defense (BMD) system in the United States for countering the missiles. And with the Soviet BMD system, particularly sites deployed around Moscow, which were revealed to be equipped with SA-2 interceptor rockets for defense against U.S. bombers, and later with the nuclear-armed exo-atmospheric interceptors (GALOSH) to counter U.S. ballistic missile warheads. On May 1, 1960, an SA-2 rocket shot down Francis Gary Powers's U-2 near Sverdlovsk, scuttling a planned 4-power summit meeting in Paris.

Each year, in preparation for the budget decisions, the SMP would provide the President an assessment of the current proposal of the U.S. army for ballistic missile defense of the country or, in some cases, of the strategic offensive retaliatory missile force. As befits elements of the government, the Army had a program every year ready for deployment. It had excellent contractors for the radar and interceptor in the AT&T Bell Telephone Laboratories, and for phenomenology of reentry physics, the MIT Lincoln Laboratory. The SMP assessed much work by Lincoln and Bell Labs on measurements of reentry phenomenology, both optical and radar, which might be used in discriminating real ballistic missile warheads from decoys.

Every year we would write the President in a Top Secret memo that the system would have this or that performance, but that it could be nullified with technical countermeasures, with tactics, or it could be overwhelmed by numbers of incoming reentry vehicles. Or destroyed by a small fraction of the warheads.

The enthusiasm with which the President's National Security Advisor (Henry Kissinger for President Nixon) received these substantive highly classified reports is clear from the note on this declassified memo[5] from Kissinger's aide:



DECLASSIFIED
Authority EO 12958
NARA Date 6/21/00

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MEMORANDUM

THE WHITE HOUSE
WASHINGTON

INFORMATION
January 5, 1970

SECRET

MEMORANDUM FOR DR. KISSINGER

FROM: Laurence E. Lynn, Jr.

SUBJECT: PSAC Strategic Military Panel Comments on Minuteman ABM Defense

Lee DuBridge has sent you a copy of the informal report of the ... of the PSAC Strategic Military Panel on the panel's

"We must get PSAC out of strategy."

Conflict between supporters of the BMD systems and the objective analysis of PSAC probably contributed to Nixon's eliminating PSAC in early 1972, at the end of my second four-year term. PSAC members, including Jerry Wiesner, then a consultant-

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[5] "Missile Defense Thirty Years Ago: Déjà Vu All Over Again?" Edited by William Burr, December 18, 2000. http://www.gwu.edu/~nsarchiv/NSAEBB/NSAEBB36/index.html

at-large, had carefully asked the PSAC chair, President's Science Advisor Lee Dubridge, whether they should resign from PSAC in order to provide their own personal testimony to Congressional hearings on the antiballistic missile (ABM or BMD) system. Dr. Dubridge asked the President and reported that PSAC members should not resign—that it was important for the Congress to have the personal views of the members. But other White House staff were undoubtedly unhappy with such testimony, and with my own on the commercial Supersonic Transport (SST) program that I had long studied both in and out of government.

This BMD story, of course, extends to the present day, with the evolution of technology and the proclamation by President Ronald Regan, March 23, 1983, of the Strategic Defense Initiation ("Star Wars") that would use orbiting directed energy weapons (laser or neutral particle beams--NPB) to destroy Soviet ballistic missiles in their boost phase. The intent was to provide an impenetrable shield so that not one of the 6000 Soviet reentry vehicles armed with nuclear warheads could strike the territory of the United States. This fantasy is not beyond the laws of physics, but taking into account not only costs and technology and time on the United States side but also the vulnerability of a system to being overwhelmed, under flown, deceived, or destroyed, meant that it was a waste of funds that it could otherwise have been employed in the economy or in other military programs.

Some have it otherwise, asserting that the recognition by Mikhail Gorbachev that the Soviet Union could not compete in a Star Wars race led to the dissolution of the Soviet, but I disagree. Even administrations recognizing the futility of BMD to protect the country against the deterrent/retaliatory nuclear force of the Soviet Union (now Russia) nevertheless were moved to deploy such a system in order to counter political criticism. However, the Nixon Administration, to the credit of Richard Nixon and his National Security Advisor and later Secretary of State, Henry A. Kissinger, negotiated the Moscow ABM Treaty of 1972 that limited each side to 100 interceptors, presumably nuclear armed, but deployed in such a fashion that it they could not form the basis for the defense of the national territory.

At present, the Obama Administration has, with the agreement of the Department of Defense, switched the deployment plans of the George W. Bush Administration for very large ground-based interceptors (GBI) in Poland and elsewhere, to the deployment of hundreds of smaller interceptors on naval ships and on land, in a phased adaptive approach (PAA) to BMD.

This far more feasible task is not to destroy 6000 warheads in a raid by the Soviet Union or even a few thousand warheads in a nuclear attack by Russia, but to counter a few or a few tens of ballistic missiles from Iran or North Korea. However, as I and my colleagues, particularly Dr. Theodore A. Postol, have emphasized repeatedly over the years, this mid-course defense system is fundamentally useless because of the feasibility of mid-course countermeasures—particularly the surrounding of the missile warhead by a large enclosing aluminized balloon that is opaque to radar, visible, and infrared sensors, and that would have no effectiveness against a nuclear-armed interceptor, but defeats handily a hit-to-kill interceptor of the current and recent past proposals for BMD.

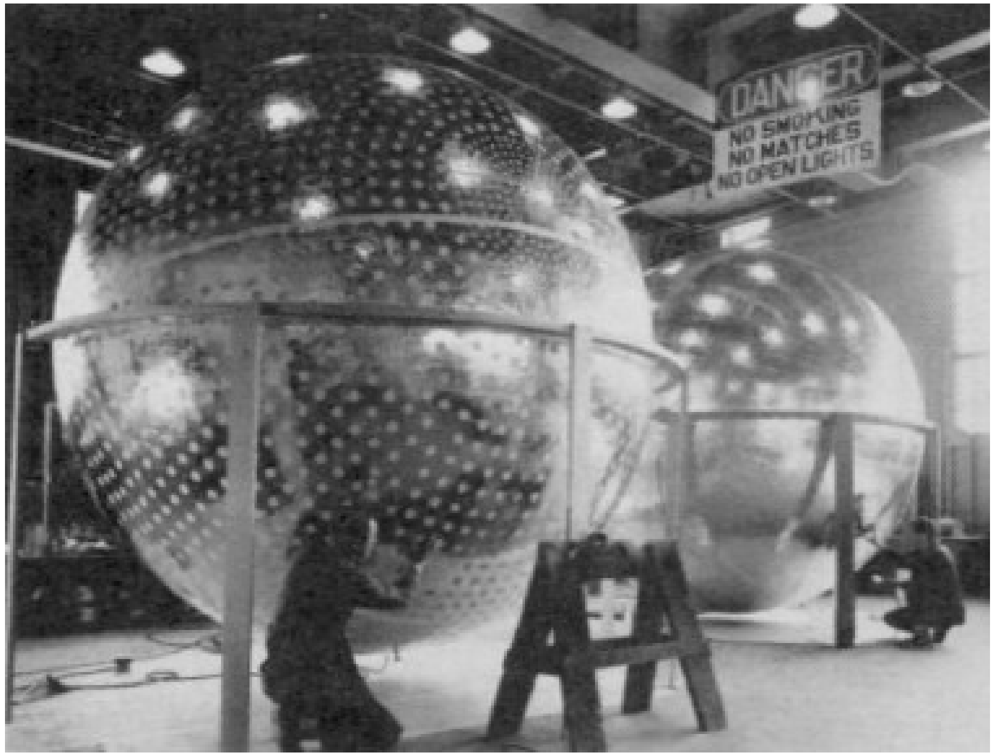
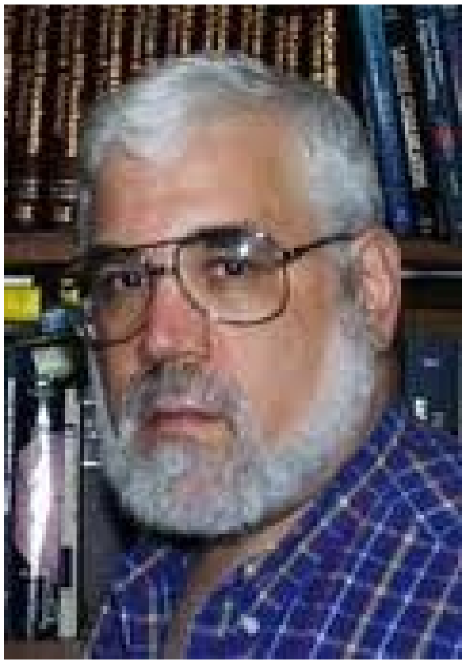


Figure 8-1 of "Countermeasures," photograph of NASA Air DensityExplorer balloons, first launched in 1961

Ted Postol has provided incisive, technically competent testimony and papers and has been a major force for rationalizing military programs. He should be listened to more attentively. I first encountered Ted in 1980 when Sid Drell and I helped to put together a team of three young outside physicists for the Office of Technology

Assessment study of the basing options for the ill-fated MX missile—a 95-ton replacement for the Minuteman missile, twice its size, that would contain 10 independently targeted reentry vehicles (MIRVs) in comparison with the three warheads of the Minuteman-III. Ted Postol then joined the government after this fine and exhaustive study, in the role of Advisor to the Chief of Naval Operations (particularly in regard to the Navy strategic ballistic missile force—Polaris/Poseidon/ and now Trident).



Ted Postol

Ashton B. Carter

Ashton Carter has been professor in the Kennedy School at Harvard and has served in the Clinton administration in the Department of Defense, where is now the Under Secretary of Defense for Acquisition, Technology, and Logistics and has been nominated to be Deputy Secretary of Defense to Leon Panetta.

At a time when the massive reductions in strategic nuclear forces of the United States and Russia are indeed in progress, an ineffective BMD can derail this improvement in U.S. security. Indeed, it is widely recognized that having more nuclear weapons than is needed (and how many are "needed"?) is a great threat to society and, in particular, to U.S. society, in the form of nuclear terrorism.

Nuclear terrorism has replaced nuclear attack by a state as a principal threat to the United States, even though it would involve, probably, only a few nuclear explosions instead of hundreds or thousands. Much effort is going into countering nuclear terrorism, especially the threat of stolen or otherwise acquired nuclear weapons, or the creation of improvised nuclear explosive devices (IND) from the hundreds of tons of highly enriched uranium (HEU) or plutonium (military and "civil") now available in the world. But that is another story.

The TCP had an intelligence Panel under Edwin H. Land, the inventor of polarizing film and of the Polaroid process for instant photography. The Land Panel, evolved from its TCP era as a group advising the President's science advisor, the director of the CIA and the deputy secretary of defense, was much involved with the details of "overhead reconnaissance," a euphemism for satellite imagery, although other satellites perform invaluable roles in the acquisition of radio signals from communication systems, radars, and telemetry. The film reconnaissance satellite flew in August 1960, the first of 145 film capsules returned to Earth in the CORONA program, which terminated in 1972. The CORONA program and its imagery, at best about 2 m ground resolution, was completely declassified in 1995. It had been created as a CIA "black program" response to the technical and programmatic recommendations of the Land Panel and the intelligence panel of the TCP before it. The Land Panel was involved with optical imagery, but other information from space was important, and I worked as an independent consultant to the relevant U.S. agencies that were developing and deploying such systems.

The Land Panel was a key element in the definition and selection of two CORONA follow-on film-return systems, HEXAGON and GAMBIT, which were much more capable and which were partially declassified on September 17, 2011. The enormous HEXAGON spacecraft was on public exhibit at the Air and Space Museum of the

Smithsonian—a vehicle the size of a large school bus, 60 feet long and 10 feet in diameter. It had four large film-return buckets. HEXAGON had a resolution on the order of 0.7 m, and GAMBIT, not a panoramic camera like CORONA or HEXAGON, a resolution measured in centimeters, but, of course, over a small field of view.

The first HEXAGON flight was in 1971; the last in 1986. The vehicle was on orbit for as long as 129 days. Each image of the stereo +/- 60-deg scan captured a "bow tie" 300 nautical miles cross-track and 17 nm along track, repeated as desired to have continuous along-track coverage[6].

In all this, physicist Edward M. Purcell was a key player, as evidenced by this footnote in an official history of the HEXAGON program, declassified September 17, 2011. In 2000, he was named one of ten Founders of National Reconnaissance, along with Edwin Land, Sidney D. Drell, myself, Wm. J. Perry, Wm. O. Baker, Merton E. Davies, Amron H. Katz, James R. Killian, and Frank W. Lehan.

[6] http://www.nro.mil/foia/declass/GAMBHEX%20Histories.html

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The "Purcell Panel," headed by E. M. Purcell, included A. F. Donovan, E. G. Fubini, R. L. Garwin, E. H. Land, D. P. Ling, A. C. Lundahl, J. G. Baker, and H. C. Yutzy--perhaps the most distinguished group of authorities on reconnaissance, space, and photography ever to be collected in one study group. Many of the

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~~TOP SECRET~~

The HEXAGON photographic satellite vehicle. Length 60 ft; diameter 10 ft

Aerial Recovery By C-130

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As was indicated in the 1995 speeches and documents accompanying the declassification of CORONA, the film-return systems have been replaced by a "near-real-time electro-optical system" which came on line in January, 1977.

I have discussed under the rubric, "Physics in the Interest of Society" mostly the role of advisors and consultants to the Executive branch. In fact, there are many, many advisory committees not only to the government but also to the National Academy of Sciences—boards and commissions and individual *ad hoc* and standing committees. For instance, there is the Board on Effects of Ionizing Radiation, which in 2007 published BEIR VII-Phase 2, the latest estimate of health effects of ionizing radiation—primarily latent cancers, amounting to about 0.1 latent cancer per sievert (Sv) of weighted whole-body energy deposition. One gray of minimum ionizing radiation contributes 1 Sv, and, in physical units amounts to about 1 J/kg of tissue. With the present state of medical science and treatment, about half the cancers are fatal, so that an overall dose response coefficient for minimum ionizing radiation is 0.05 lethal cancers per Sv.

In 1981, the National Academies of Science created the Committee on International Security and Arms Control, primarily for semi-annual bilateral sessions with a corresponding group of Soviet scientists. Of course, there is very little similarity between the Soviet Academy of Sciences (now the Russian Academy of Sciences (RAS)) and the U.S. National Academies of Science (NAS). The RAS now has 130,000 employees, whom 65,000 are researchers. It has 165 Institutes under the RAS; the NAS has none (despite the name of the Institute of Medicine, which is a sister of the NAS).

The RAS is at times very influential in decisions of the Russian government, and our emphasis on nuclear weapons and military technology have, at times, been highly beneficial, as in providing technical views of the prospects for the Strategic Defense Initiative.

Now a bit of philosophy, although I am much better at problem solving by the use of elementary physics or, as former Secretary of Defense William J. Perry put it, "Just plain good sense." Why is it that outside advisory groups or individual consultants can have a disproportionately beneficial influence on U.S. government programs, in particular. Well, it is not always that easy, and not always of that sign, but let that pass for the moment. Sometimes, as is the case with determining the position of the three radar-sensing aircraft by means of three tiny pseudo-radar generators (beacons) rather than precision tracking radars deployed in the theater of conflict, it is just a matter of seeing the problem overall, and of applying the principle that if something can be eliminated, one doesn't need to worry about its schedule, cost, or reliability.

Or it could be phrased as, if you assume that the system you are working on actually works, what else can it do (in this case determine with extreme accuracy the position of the sensing aircraft themselves). How is the utility of outsiders consistent with the discovery by Herb Simon (Carnegie Mellon University) that it takes about "40,000 facts" to make an expert. That is, until one has about that many facts and concepts at one's disposal—at the tip of tongue, so to speak, it is not easy to correlate anything new with everything that is already known. Similar conclusions are recounted, for instance by Malcolm Gladwell, that 10,000 hours of practice are required whether one is a musician, a public speaker, or a Professor of Physics. How, then, can an occasional consultant help?

As a member of a high-function group such as PSAC in its glory days, the individuals are chosen because of their demonstrated record of accomplishment, scientific integrity, and constructiveness. In general, they not only get along, they even like one another and are bound in a common mission.

Given the option of constituting an advisory committee of recognized experts, or an advisory committee of recognized accomplishment in their own fields, the second is by far preferable. The first is too likely to encounter inherent conflicts of interest, and to limit itself to the conventional wisdom.

Then there is the well known and frequently experienced concept of "hybrid vigor," which in this context comes about because a person accomplished in one field is intimately familiar with concepts and tools in that field, and can immediately see the mapping of problems in a new field onto the tools of the old field.

Still, someone needs to know enough about the field (the current problems of the government or its agencies and departments) to understand them and transform them into questions and goals. In the late 1950s, it was apparent that the scientists who had worked so effectively in the wartime laboratories if the United States under the National Defense Research Council (NRC) aegis and developed radar, the proximity fuse, underwater sound (antisubmarine warfare) and the nuclear weapons, were dying off, and that it would be good to involve researchers of the highest caliber in their advisory work with these important government programs.

Charles Townes as Vice President of the Institute of Defense Analyses (on leave from his position as Professor of Physics at Columbia University), was key in creating the JASON group of consultants, attached at first to IDA and then administratively housed at SRI International and now the MITRE Corporation. Initially in large part physicists with a sprinkling of chemists and mathematicians, JASON is now about 60 scientists and works at a seven-week intensive Summer Study in La Jolla, CA, supplemented by a 10-day Winter Study there and a Spring and Fall three-day session in Washington, DC, two days of which are devoted to classified and unclassified talks to JASON and to many of the sponsoring agencies. Study topics are negotiated between the sponsors and the JASON leadership, and are completed during the Summer Study in La Jolla, by early August, with finished reports provided to the sponsoring agency by October of that same year.

About half of the reports are unclassified, and of these most are made available by the sponsoring agency, but some are not. JASON has worked on climate change, underwater sound, deployment of large 10-warhead missiles horizontally on small strategic submarines, and in recent decades especially on maintaining the health and safety of the U.S. nuclear weapon stockpile without nuclear explosive testing.

JASON also has done a good deal of work on intelligence matters—analysis as well as technology.

It had been hoped that bringing young scientists on to PSAC panels would provide this needed expertise and familiarity, but the panels themselves did not do enough concentrated work to generate new knowledge, although in many cases the pooled knowledge and expertise of the members was extremely valuable, as in the case of the Military Aircraft Panel's strong support for what was to become the Global Positioning System, GPS. Among the experts of the MAP was Luis W. Alvarez who, beyond his Nobel-Prize winning feats in physics, had invented and developed Ground-Controlled Approach (GCA) a technique perfected for U.S. and British aircraft in Britain during WW II, that allowed landing under all weather conditions on British airfields, without adding any equipment and very little training to the aircraft and its pilot.

Every talk must end, and this one ends here. Except for "one more thing."

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