Recollections of HEDS Involvement with the U.S. Nuclear Weapon Labs

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NATIONAL SECURITY & HIGH ENERGY DENSITY SCIENCE
Technical, Policy, and International Perspectives and Implications

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Richard L. Garwin was born in Cleveland, OH in 1928 and grew up there, attending public schools in Cleveland and then in Cleveland Heights, OH, graduating April 1944 from Cleveland Heights High School. He graduated in Physics from Case Institute of Technology in Cleveland in March 1947, and began graduate school in physics at the University of Chicago, from which he received an MS and then a Ph.D. in Physics in December, 1949.

Before leaving for Chicago, He married Lois E. Levy. They had three children. Lois Garwin died in February 2018 after a marriage of more than 70 years.

He served on the Physics faculty at Chicago until November 1952, when he joined the IBM Watson Scientific Laboratory at Columbia University, changing his field from particle physics to condensed matter physics and technology. He worked at the Watson Laboratory until 1970, when he moved to the IBM T.J. Watson Research Center, Yorktown Heights, from which he retired in June 1993, but where he is still IBM Fellow Emeritus.

In addition to his pure physics work on liquid and solid He^3 and He^4, in particle and nuclear physics at the University of Chicago and Columbia University, he did some of the first work in the early 1970s, showing that gravitational radiation had not been detected.

As a result of a unique employment contract with IBM, he was able to continue his consulting with the U.S. government, after the first three summers at Los Alamos while he was on the faculty of the University of Chicago, spending many additional summers at Los Alamos with his family.

At the Los Alamos Scientific Laboratory (now LANL) he worked primarily on nuclear weapons and nuclear weapon testing, where he introduced several innovations, including new technologies for diagnosing thermonuclear burn in nuclear explosion tests. He also designed the first large-scale thermonuclear explosive-- the Mike test of November 1, 1952, with a four-page Secret memo at Los Alamos July 25, 1951, with a large fold-out drawing. He was responsible for other innovations in nuclear weapons as well and then expanded his work in national security for the U.S. government as a consultant to and panel member of the President's Science Advisory Committee-- PSAC-- in the Administrations of Presidents Eisenhower, Kennedy, Johnson, and Nixon.

In addition to his work at Los Alamos on High Energy Density Science and its applications, he was a member of various visiting committees to Sandia National Laboratory, especially the Z machine.

In work for the National Academy of Sciences and for the JASON group of consultants to the U.S. government, he reviewed the work on the National Ignition Facility at LLNL-- NIF. His JASON work included participation in reports on Science-Based Stockpile Stewardship, impact fusion, and the like.

Most of his scientific work is accessible via Google Scholar at https://scholar.google.com/, and hundreds of his publications and speeches of more general interest can be found at the Garwin Archive, www.fas.org/RLG/.

His 50 patents can be read or downloaded conveniently from Google Patents, https://patents.google.com/?q=Garwin&oq=Garwin

Search the Garwin Archive, for instance, by entering into any Google search box, e.g., site:fas.org/RLG/ Fermi Teller

NATIONAL SECURITY & HIGH ENERGY DENSITY SCIENCE
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This workshop offers an introductory exploration of the national security dimensions of High Energy Density Science (HEDS) from technical, policy, and international perspectives. Scientists from the national laboratories, current and former policy experts from the federal agencies, and university faculty will provide a diverse array of views on the role and importance of HEDS to national security. Key issues that will be addressed in the two-day event include the stockpile stewardship program and HED science, comparing how HEDS research and development is taking place in the U.S. with China, HEDS and pulse power, and export control issues and HEDS. The main goals of this workshop are for scientists to gain an understanding of the policy and national security implications of their research and to encourage research from social scientists to examine the national security impact of HEDS.

In thinking how to provide best value for the audience's time spent in listening to me, I decided on a historical retrospective of my own involvement in HEDS (where "S" could as well stand for "systems" as for "science"). I do have some slides, if only to add value to the permanent record of this talk, and to attract your comments or questions.

Soon after I received my Ph.D. for experimental work in nuclear physics at the University of Chicago—the first experimental look at beta-gamma angular correlation following beta decay--and was hired as an instructor in the Physics Department there, my thesis sponsor, Enrico Fermi, suggested that I might be interested in summer consulting at the Los Alamos Scientific Laboratory, to which he had returned each summer after he left the nuclear weapon program there in December 1945. So in June, 1950, I drove from Chicago with my wife, Lois, and our 7-month-old son, Jeffrey, to take up residence in the Zia-Company-managed housing in the "Chapel Apartments" in Los Alamos.

A few months after I arrived in Chicago for graduate work in the Spring of 1947, and had had a lecture course by Fermi, I realized how much I was missing lab work in which I had been engaged for my Bachelor's degree and thesis at Case Institute of Technology (as it was known when I graduated)—the same institution from which Sig Hecker received his B.S., M.S. and Ph.D. in 1965-8. Fermi agreed to let me help with his experiments and I soon eagerly accepted his suggestion that I work on β-γ angular correlation for my PhD thesis. It wasn't HEDS, but I had invented new fast coincidence circuits that reduced the coincidence resolving time by about 100-fold from the vacuum-tube Rossi circuits that had been used up to then (by simply adding a capacitor and a semiconductor diode), and I published and popularized the coaxially-mounted mercury-wetted reed relay for providing nanosecond pulses for testing such systems. These were very useful in allowing increases in counting rate by a factor 100 or more.

I also saw Fermi and his colleague Leona Woods Marshall scooped in their work on the lifetime of positronium (for which they filled their own Geiger counters, that included a string wet with a dilute solution of Na-22) by Martin Deutsch of MIT, who used the developmental RCA two-inch diameter end-window photomultiplier tubes as his detectors of positron emission and the two 0.51 MeV annihilation photons. Fermi soon obtained them from RCA and and put them to good use in his experiments, as did I in my thesis, and they became standard in the cyclotron work at Chicago and elsewhere.

Arriving in Los Alamos in June, 1950, I acquired two desks—one in the Physics building in P-Division headed by Jerry Kellogg, and the other in an office shared with Fermi, near Ashley Pond. For the first week, in the classified report library, I read the weekly progress reports from the beginning of LASL in March 1943 through June, 1950, learning what I could at that time of the physics and mathematics of hydrodynamic shocks. I duly took notes in my lab notebook, classified SECRET, which went into the safe in our office at the end of the day. Occasionally Fermi would write a few lines or pages in my notebook, I think in large part because he didn't want the burden of keeping his own classified notebook, so I am able to show[1] you his explanation and estimate of the earthquake strength in Las Vegas from an assumed 100-kiloton fully contained explosion at the Nevada Test Site (pp. 1 and 7 exhibited here).

[1] https://fas.org/rlg/070050..EF%20Explosion%20in%20underground%20cavity.pdf

Explosion in underground cavity

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

Total energy $= 5 \times 10^{21}$ ergs $= W$

Initial radius $R = 33$ m

Initial volume $\frac{4\pi}{3} R^3 = 1.25 \times 10^5$ m^3

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

From p. 6

Assume equation of state of rock

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

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

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

$v_0 = .4 \quad c = 5 \times 10^5 \quad k = 1.57 \times 10^{12}$

From 3rd Hugoniot

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

$$v_0 - v_1 = \frac{p}{k} = \frac{2.7 \times 10^{10}}{15.7 \times 10^{11}} = .0172$$

$v_0 = .4000$

$v_1 = .3828$

"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)

In elastic case

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

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

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

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

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

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

~~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)

But there was also a lot of discussion of fission and fusion cross-sections, and I decided that the cross-sections that had been measured in 1939 for D-D and D-T reactions were not adequate to support a U.S. program to build thermonuclear weapons, as had been announced by President Truman on January 31, 1950.

I had enough flexibility in those informal days in the Laboratory to go to the little cyclotron and set up a photomultiplier tube to look at the fluorescence of air excited by the external beam of the cyclotron, with its rf structure, in order to see what kind of signal would be obtained as a diagnostic in the neighborhood of an atmospheric nuclear explosion. I also received authorization to scrounge materials and equipment and to build a 100-kV accelerator and ion source for a beam of D or T in order to measure the reaction cross-section down to perhaps 10 keV beam energy.

The innovation was to use a gas cell for the target (at ground potential) with both an entrance and an exit thin window of Zapon or SiO, and to analyze the beam exiting the cell by means of a retarding potential and Faraday cup, so that I could see the energy lost in the individual front and rear windows, and also in the gas, when the target cell was filled at an appropriate pressure with D_2 gas. I got far enough that summer to have the Laboratory take over the experiment, with Fermi recruiting from England Jim Tuck, who had been with the British team at Los Alamos during the war. Their experimental results were published in 1954 in the Physical Review[2].

With my new-found-knowledge of the orders of magnitude of nuclear energy release and energy density in nuclear explosions, I was drawn into analyzing and proposing diagnostic

[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, *Physical Review*, Vol. 93, No. 3, pp. 483-497, Feb. 1, 1954.

experiments for nuclear weapon tests and tests of thermonuclear burn, such as those that were scheduled to be performed in the Pacific GREENHOUSE series in 1951—especially GREENHOUSE ITEM and GREENHOUSE GEORGE—tests of the first "boosted" fission weapon and of pure thermonuclear burn. Most of these were successful, but they were not regarded by those who performed them or analyzed them as advancing significantly the program to build large-scale thermonuclear weapons. But I did meet many interesting people—e.g., from the University of California Radiation Laboratory at Berkeley-- who were much involved in the diagnostic experiments, particularly for GEORGE. Among the couple of papers I published at that time in the Report Library was one titled "(U)Arsenic and Nickel in George," which I believe was the first significant proposal to use stable isotopes, carefully chosen to diagnose details of 14-MeV neutron production. Of course, it had long been proposed to use external detectors of the 14-MeV neutrons, and even to image with "pinhole cameras" the spatial and even temporal characteristics of the source, but the use of these two "threshold" internal activation detectors would allow the radiochemists, working with the remotely sampled debris cloud from the atmospheric explosion, to determine not only the absolute number of 14-MeV neutrons produced, but also the distance of one of the detectors from the source—a question of great interest in the rapidly changing geometry of thermonuclear burn driven by the high-energy density output from a fission bomb.

When I returned to Los Alamos for the second summer in May 1951, I asked Edward Teller whether there had been any developments since I had left the previous Fall, and he indicated that there been—specifically that he and Stan Ulam had published a SECRET paper March 9, 1951, titled (U)"On Heterocatalytic Detonations I: Hydrodynamic Lenses and Radiation Mirrors". Knowing of my experimental work at the University of Chicago, Teller urged me to design an experiment that would prove the concept of (now it can officially be stated: “radiation implosion”) in which the energy output from an auxiliary bomb (the “primary”) could be used to prepare and ignite a “secondary” charge of thermonuclear fuel. While catching up on other matters and providing support for diagnostic experiments for the GREENHOUSE series, I strove to define such an experiment and finally published my proposal in a four-page SECRET memo of July 25, 1951, with a large fold-out drawing of what was tested November 1, 1952 at Eniwetok as the (IVY) MIKE shot, at a yield of almost 11 megatons (11 MT)-- almost 1,000 times the energy release of the Hiroshima bomb. The test device weighed some 74 metric tonnes.

I had been unable to find a small-scale experiment that would be, as Edward requested, totally persuasive and decided that the best and quickest way to go forward was to test at full scale. Because my work with the synchrocyclotron at Chicago involved external particle beams interacting in targets of liquid hydrogen or liquid deuterium, I was familiar with the practicalities of dealing with liquid hydrogen and liquid D_2, so it was no problem-- and simpler and surer theoretically-- to use liquid D_2 fuel. Ferdinand Brickwedde of the National Bureau of Standards (now NIST), assumed responsibility to design a 500 liter per hour hydrogen liquefier in Boulder, CO, and a 250 LPH deuterium liquefier.

There were meetings of the Theoretical Megaton Group at Los Alamos and other organizations to review the status of the work on the hydrogen bomb, and soon my proposal was largely adopted by the laboratory management, led by Norris Bradbury since the departure of J. Robert Oppenheimer in 1945. An informed but also somewhat impressionistic narrative of that momentous TMG session has been published by long-time Los Alamos theorist, Harris L. Mayer who was present at those meetings, and also worked closely with Edwart Teller at LASL and at the Pacific test sites.

After providing that design, I decided that I would design also a flyable version of the liquid-fueled bomb and did so. I did not learn until much later, from Herb York's book[3] that the Atomic Energy Commission had built six of these Emergency Capability Weapons (ECW) and had them ready before the solid-fuel hydrogen bombs were tested in 1954. In the summer of 1952, during my stay in Los Alamos, I worked the night shift for almost two weeks with Marshall Rosenbluth, who, with Conrad Longmire, had carried out the computer calculations on MIKE) at the National Bureau of Standards in Washington, using its mercury-delay-line-memory digital computer to study solid fuel or other versions of the two-stage radiation-implosion weapon.

My role in MIKE was not widely known after the summer of 1951. Norris Bradbury had appointed Marshall Holloway as project engineer on MIKE, and Holloway did a bang-up job in moving this program from rough sketch end-July 1951 to fabrication, transportation, and detonation on November 1, 1952. I did a few other things once MIKE was partially defined and that was to study the equation of state of materials driven now with the pure black-body pressure of a Hohlraum, or shocked by materials (plasma) itself driven by the radiation pressure. For these experiments, the highly capable physicists and engineers of the test division built (as I recall) a helium-filled (plastic-film-lined) plywood tunnel to carry the diagnostic light from the surface of the shocked materials, as well as neutrons and gamma rays, through the helium to imaging photodetectors almost 4 km away.

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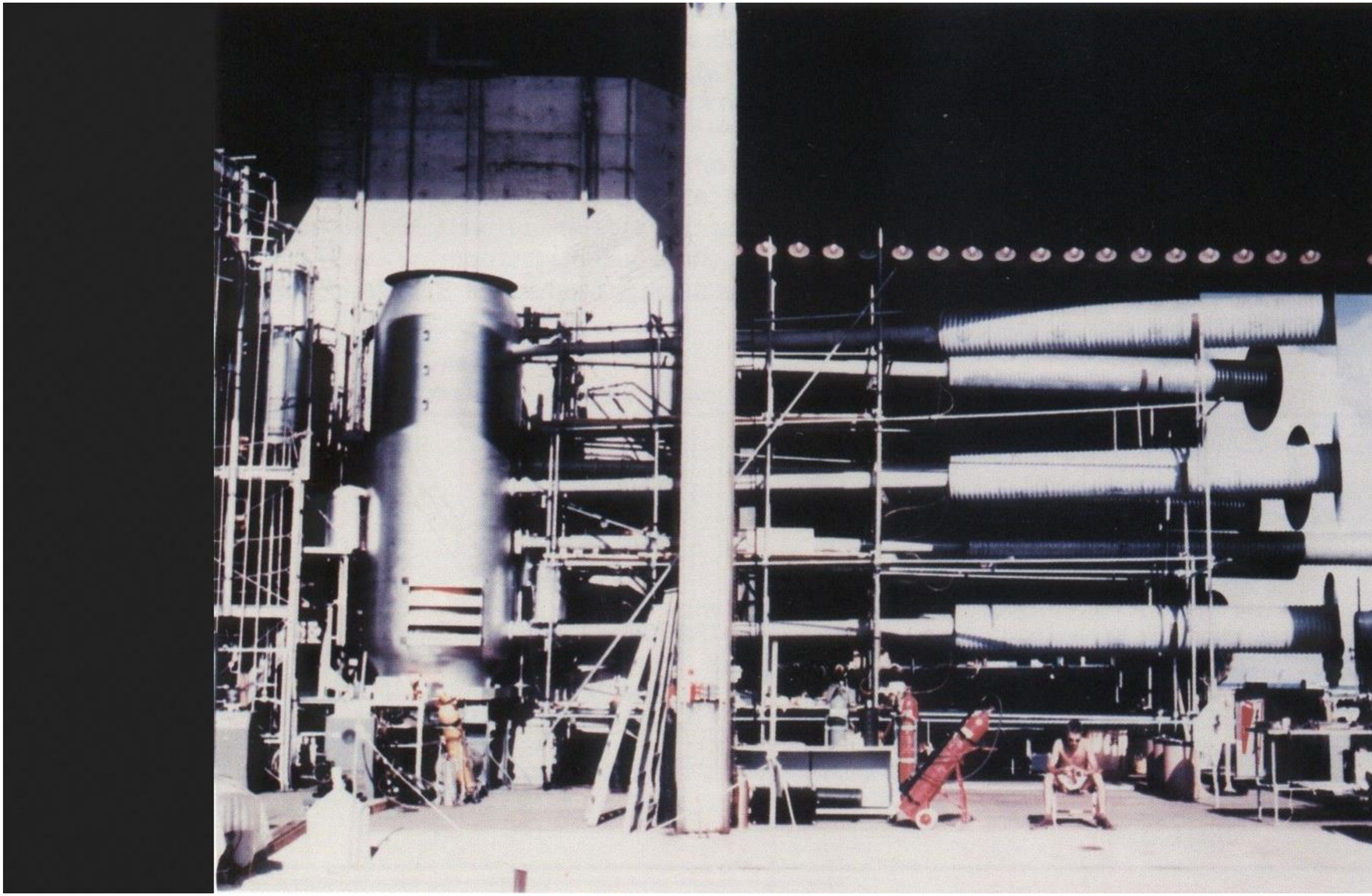
[3] The Advisors: Oppenheimer, Teller, and the Superbomb



(IVY) MIKE thermonuclear test device that would be detonated 11/01/1952 at Enewetak

Here is a photo of some of my colleagues a few years later-- a Space advisory committee to General Dynamics (San Diego), 09/21/1956.

My further involvement with HEDS was quite secondary (no pun intended) and included participation on the visiting committee for Z-machine at Sandia and chairing that committee for a couple of years. One of my first activities in Los Alamos in 1950 had been to work a bit with Marshall and Arianna Rosenbluth on a paper on the theory of the Z-pinch, experiments later taken up by Jim Tuck, again at Los Alamos after he had completed the leadership of the D-D and D-T cross-section measurements. I was also involved, via Los Alamos a bit, but more heavily via the JASON group of consultants to the U.S. government and to the Department of Energy and nuclear weapons establishment in particular, with the x-ray laser in the Star Wars era following President Ronald Reagan's speech of March 23, 1983 in which he asked the scientists "who gave us nuclear weapons, to turn their great talents now to the cause of mankind and world peace: to give us the means of rendering these nuclear weapons impotent and obsolete."

By then, three decades had passed since my work on MIKE, over which time I had been heavily involved with the President's Science Advisory Committee with arms control efforts in the White House and State Department, and with other activities of the U.S. government. At the time of serious consideration of the Comprehensive (nuclear) Test Ban Treaty, on which I had worked in multiple roles, including a Conference of Experts in Geneva in November-January 1958-59, I was involved once again via the National Academies' study committees, the CISAC[4], and the JASON group of consultants to the U.S. Government, on technical matters of the value of nuclear explosion testing (to the United States and to others), with the detectability of nuclear explosions, and the like.

[4] Committee on International Security and Arms Contro (of the NAS)

At this point, I want to raise some non-physical questions. What we do and what we say in HEDS does matter, and I have a bad feeling about some of it, which I expressed vigorously in public discussions of NIF.

Here is what I wrote[5] the leadership of the HASC in January, 2016, excerpted here:

In support of the National Ignition Facility, in the years following the 1992 moratorium on nuclear testing initiated by the Administration of President George H.W. Bush, it was argued that without continued nuclear explosion testing, evident the world over by seismic records of the underground nuclear explosions at the U.S. national test site, nuclear deterrence could be maintained only by the achievement of "ignition" at NIF. I took the other side in this discussion, arguing that such a proposal was self-serving and that the argument itself contributed to the weakening of deterrence, because the United States clearly had many nuclear weapons which had been tested and could be maintained indefinitely in the future by what is known now as a LEP (Life Extension Program); More particularly, the suggestion that such a force-in-being of tested nuclear weapons suffered in deterrent value because ignition could not be achieved in a charge of fusion fuel a million times smaller than that in a weapon was both logically deficient and both politically and technically wrong-headed. Unforeseen difficulties, either in principle or in practice might prevent the achievement of ignition at NIF, without in any way impairing the continued ability of the United States to produce two-stage thermonuclear weapons.

And that is how it turned out—failure to achieve ignition, but who had believed the hype?

And now for comments and questions. THANK YOU!

[5] https://fas.org/rlg/hasc-01112016.pdf