The Secret Hans

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I recount[1] some early interactions I had with Hans, beginning in 1951. Hans had led the Theoretical Division at Los Alamos from 1943 to 1945, and despite his antagonism to the hydrogen bomb, was willing to turn his talents to learning whether it could be done or not, which was his role when we interacted in the summer of 1951.

In May of 1951 my wife and I and our infant son went to Los Alamos for the second summer, where I would continue to work mostly on nuclear weapons. I was at that time an Assistant Professor at the University of Chicago and had spent the summer of 1950 at the Los Alamos Laboratory, sharing an office with my colleague and mentor Enrico Fermi—Hans Bethe's mentor in Rome as well. When I returned in 1951, and asked Edward Teller, another University of Chicago colleague, what was new and what I could do, he asked me to devise an experiment to confirm the principle of "radiation implosion," then very secret, that he and Ulam had invented that February.

In May 1951, the young physicists Marshall Rosenbluth and Conrad Longmire were trying to do actual calculations on this method for using the energy from an ordinary fission bomb to compress and heat fusion fuel-- that is, heavy hydrogen (deuterium).

I decided that the most convincing experiment would be a full-scale hydrogen bomb, so I set about designing that.

In my research at the University of Chicago in experimental particle physics, I had built some liquid hydrogen and liquid deuterium targets for the 450-MeV proton beam of the new cyclotron there. So it was easy enough to contrive a large system that used both liquid hydrogen and liquid deuterium in order to implement the radiation implosion. Hans was at Los Alamos for about 2 months in 1950 and again in 1951 (and most of 1952), and in addition to his own analyses and contributions, he chaired the Theoretical Megaton Group (TMG), to which my particular proposal was brought.

I was primarily an experimental physicist, so it was possible to anticipate many of the problems of technology in the design, which was soon accepted by the TMG.

Then ensued the enormous and speedy development effort under Marshall Holloway, to build the test weapon, MIKE, that was fired November 1, 1952 with an explosive yield of about 11 megatons-- almost 1000 times the yield of the bomb that destroyed Hiroshima.

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[1] In the 10-minute presentation, the italicized paragraphs were not included. The italicized paragraphs are included to provide more detail. The manuscript is posted in "The Garwin Archive" at www.fas.org/RLG/

The "wet" version of the hydrogen bomb was in fact weaponized by the Atomic Energy Commission (AEC) and five or six of these "Jughead" bombs were available for delivery before the solid-fuel weapons were tested in 1954.

Hans did not permit his clear preference that the H-bomb be infeasible to cloud his technical judgments on the path to build one.

I worked again with Hans to interpret the information that the United States gathered on the fourth Soviet nuclear explosion-- Joe-4-- in a 1953 paper coauthored also with Enrico Fermi and Luther Nordheim, of which you can find on the web a heavily redacted three of the report's 49 pages. For many years, Hans led this important effort to divine details of Soviet weapons from the fragmentary information provided by the extensive seismic, acoustic, and radiochemical (air sampling) detection systems that the US created for this purpose.

I next encountered the secret Hans in the context of the President's Science Advisory Committee, to which I became a consultant about 1956 and pretty soon a member of the Strategic Military Panel, of which Hans was a stalwart.

PSAC itself had 18 members and met for two days every month, with an almost perfect attendance record. It had on the order of a dozen panels of 10-12 people, typically, many of which also met for two days every month. The Strategic Panel in those days was focused largely on the nascent threat and tool of intercontinental ballistic missile (ICBMs) and submarine-launched ballistic missiles (SLBMs) and the defense against them. It was sometimes difficult to tell which side we were on, since we tried at the same time to help create such weapons for the United States, and also to evaluate defenses against such weapons that might be launched against us by the Soviet Union.

It was our task as well to assess what the Soviet Union might do to thwart our own offensive force, by destroying it before it could be launched, by interfering with its command and control, and by actively intercepting missiles on the way to their targets in the Soviet Union.

This was a highly technical panel, with capable people of every opinion, including Albert Latter, as I recall, and Dan Fink, as well as Murph Goldberger, myself, Hans, and Pief Panofsky.

Most of you have not experienced the circumstances under which we had to work in the Old Executive Office Building across West Executive Avenue from the White House. Typically we had an enormous conference room in this massive building, and there was secretarial staff to type the draft reports as we were writing them. It is difficult now to conceive the drudgery, though, of generating a technical report in the 1959-1960 era. For the early stages of the reports, when there were perhaps only 6-10 of us involved, the text would be typed and that many carbon copies made. We would introduce the symbols-- alpha, gamma, subscripts, superscripts, and the like-- by hand in the spaces left by the typist. And we would need to press very hard in order to have any chance of reading the symbol on the last carbon copy. If there were more of us or later in the process, the typist would create a spirit master, and we would deal with the purple-inked copies that would result, of which we could get up to about 20, as I recall. And many of our group smoked at the meetings.

Beyond that, another retyping and we would resort to the mimeograph.

In that era, in my lab and that of everybody else's, people used "TypIts," with their IBM electric typewriters. A little jig would be added to the typewriter and when a symbol was required, the typist would place a plastic-handled slug in that jig, to be struck by a normal type bar, with the result that a perfectly good looking symbol would be created on the page, carbon copies, or the master. The Office of Science and Technology, staff to PSAC, did not have such non-standard capabilities, so I spent several hundred dollars of my own money on an assortment of TypIts and jigs to facilitate the work of the Strategic Panel, and that of the OST in general.

Each year the Strategic Panel would assess the state of the Army's progress toward defense against nuclear-armed ballistic missiles, and would judge, "Not there yet." So we would write a Top Secret letter to the President from PSAC giving our judgment and analysis, and for the most part adding, "We realize that more than technical matters might be involved in the deployment of a missile defense." This was clearly the case in 1967 when Secretary of Defense Robert S. McNamara announced in his speech in San Francisco that despite all of the cogent arguments in the first 90% of his speech, the Johnson Administration had decided to build a light area defense against ICBMs that might potentially be deployed by China-- a milestone not to be achieved for the next 11 years. Dubbed Sentinel, the proposed deployment had obvious fatal flaws that had been analyzed by the Strategic Panel.

Despite the high-yield nuclear warheads on long-range interceptors that could be used against the Soviet ICBMs, it was easy to imagine countermeasures that could be implemented by the Soviet Union to hide or protect their warheads, so that they would be vulnerable to intercept only after they had begun to reenter the atmosphere. But since China did not need to destroy Washington or New York to achieve strategic influence, short-range interceptors would be needed near every city that might be a target of a few Chinese ICBMs.

The Sentinel program self-destructed because, contrary to the views of the experts, the American public feared nuclear-armed interceptors near their cities more than they feared nuclear-armed missiles attacking them. There is something to be said for that view.

It was clear, however, that electoral politics was being mixed with national security matters, and that as much as possible (while respecting official secrets) it was essential to inform the American public and the Congress, who after all had an important role in approving and funding such deployments. Accordingly, Hans and Murph Goldberger and I accepted when Gerard Piel, publisher of the Scientific American, asked us to participate in a national security panel at the Christmas 1967 AAAS meeting in New York City. And then Hans and I agreed to put in writing what we had said, resulting in our Scientific American article "ABM Systems" of March, 1968.

Hans and I split the task-- one of us taking the initiative for the nuclear weapon aspect, and the other the radar aspect of missile defense, and at the moment I don't recall who did which.

We described the potential threat, especially that from the Soviet Union, including the multiple independently targeted reentry vehicles (MIRVs) and the possibility of maneuvering reentry vehicles (MaRVs), concepts that are still important now. And we described countermeasures and, where appropriate, vulnerabilities so that decision makers and their staffs could

understand that a system that looked good in words or on paper could very well have a fatal flaw if someone (not nature) wanted to defeat it.

Anyone launching nuclear-armed missiles against another country has every motivation to understand and to defeat the defensive system-- lessons that served us well when Hans and I and Kurt Gottfried were involved with the Star Wars program 15 years later.

Hans and I were working together again in 1958 in Geneva, following the first 8-nation Conference of Experts June 1-August 21, of which he was a key participant. The negotiations for the Test Ban Treaty had started October 31, with a moratorium on nuclear testing that was to last a year, and on November 15 I came to Geneva as part of the U.S. delegation to the 10-nation Conference on the Prevention of Surprise Attack.

Jim Fisk, head of Bell Labs, was the delegation leader, and Jerome Wiesner of MIT (later to be President Kennedy's Science Advisor) led the technical staff. We were housed one floor above the test ban delegation, and since not much was going on at our conference,

I helped out where I could with the test ban, even making a presentation at one of the sessions on comparative analyses of the capability of U.S. and Soviet seismometers to detect a distant nuclear explosion.

This was the time when the "big-hole decoupling" concept was introduced by Albert Latter and espoused by Edward Teller as a way of reducing the detectability (apparent explosive yield) of an underground nuclear explosion by detonating in a large cavity. Never mind that it would take a cavity 160 meters in diameter (some 500 feet) to gain the benefits of decoupling for a 70-kt nuclear explosion. Hans's initial reaction was that the concept was unsound in principle and that a large cavity would not reduce the seismic signal, but he soon did his own analysis and accepted the concept in principle. It played a big role in delaying a total test ban long past the 1963 Limited Test Ban Treaty that forbade tests in the atmosphere, in space, or in the waters of the world.

About ten years later on the Strategic Military Panel, we encountered another Latter insight, that the greatest vulnerability of our nuclear missile warheads to nuclear-armed interceptors in space could be the influence of soft x-rays on the skin of the vehicle. Some 80% of the energy of a nuclear explosion in space is emitted in few hundredths of a microsecond in the form of soft x-rays, which are absorbed in a very thin layer of the surface of an object, and if the object is close enough (perhaps 100 km) the surface blows off and, like a rocket, imparts a recoil momentum to the structure that can split a thin layer from the back of the structure and damage the contents.

The momentum isn't much-- about 1000 dyne-s/sq cm-- about the momentum imparted to the underlying surface by a US 5-cent piece resting on a table for a second. But because the momentum is delivered in perhaps 0.01 microseconds, the pressure is on the order of 100,000 atmospheres or 1.5 million pounds per square inch.

To demonstrate the effectiveness of our mitigation proposal, we used variously a blackboard pointer or a stiff blade from a keyhole saw and invited individuals to put their hand under it as it slapped against the table. No takers.

And then with 1 cm of Styrofoam on the palm of my hand I accepted such a blow willingly.

In brief, we found a simple technical solution and these techniques were then deployed to protect our reentry vehicles.

One last "it was fun" episode of discovery aired in April 1983 at an Arms Control session of the Los Alamos National Lab's 40'th anniversary celebration. Hans was invited to a panel with Edward Teller and Don Kerr (LANL director at the time) and insisted that I be on the panel as well. Hans and I had been involved separately in reviews of the Livermore nuclear weapons lab's program on an X-ray laser weapon powered by a nuclear explosive, which Teller asserted would produce many lethal beams to destroy a whole flock of Soviet missiles in their boost phase of flight.

Just before the April meeting, Hans and I recognized the simple but devastating fact that the proposed deployment of the non-existent X-ray laser was as a "pop-up" rocket interceptor that would need to reach an altitude of many hundreds of km in order to see over the curve of the Earth to project its X-ray beams at the Soviet missiles while they were still in their boost phase. The consequence was that the interceptors had to have the speed of ICBMs to counter the existing Soviet missiles with a boost time of 200-300 seconds. But the Soviets could build ICBMs that, at cost increase of about 5% would reach ICBM speed in 100 s. The consequence would be that the US X-ray laser rocket would need to reach its firing point in half the time, and it would need not just "fast burn" of the same amount of rocket fuel but enough fuel to double its SPEED. Instead of a launch weight 50-100 times its payload, the interceptor would have a launch weight of 2500-10,000 times its payload. Hans and I agreed on a chart we presented at the Panel discussion summarizing this analysis and pointing out that the X-ray laser weapon could not overcome the unfortunate fact that "The Earth is round."

In the many days of meetings with the secret Hans, I discovered that he was the same as the everyday Hans—the ideal colleague and friend. He would do his calculation and compare it with those of others, and he would explain clearly the truth as he saw it, independent of where it led.

Among all with such independence and integrity, Hans did it for the longest time, and with unique style.

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