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Draft Foreword to "Judging Edward Teller"

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Istvan, you and your editor will have to decide what really to call this and I would welcome from either of you suggestions for modifications or additions. **On 03/07/2011 Garwin changed about 6 words, for clarity.**

Judging Edward Teller is a task that I would avoid, and I am not going to do it now. In fact, I find myself constitutionally reluctant to judge other individuals, except when driven to do so in regard to hiring decisions, promotion, and the like. Perhaps this is a result of years of discipline as a scientist and technologist much involved with national security and technology for public purposes. I have a firm belief in the democratic ideal, which separates the decision making role from the definition and evaluation of options.

Edward Teller was an extraordinary scientist and person, and his personality and activities are extraordinarily well characterized in this book by Istvan Hargittai. I got to know Prof. Hargittai when he and his wife Magdi, both historians of scientists, interviewed me a couple of times. But here is what I recall about Edward Teller that might help the reader to evaluate "Judging Edward Teller" itself.

I first met Edward Teller when I arrived at the University of Chicago around May, 1947, with a fresh Bachelor's Degree in Physics from what is now Case Western Reserve University and was then Case Institute of Technology in Cleveland, Ohio. I had a fellowship to study physics at the University of Chicago, to which I was attracted especially by the presence of Enrico Fermi, then famous for his work in creating the first self-sustaining artificial neutron chain reaction and its consequences -- the nuclear explosive and the nuclear reactors that produced plutonium and ultimately electrical power and naval propulsion. I have told some of the story at the centennial of Fermi's birth.(1),(2) A month past my 19th birthday, my task was to begin graduate studies for the Ph.D. in Physics, which required learning the subjects from graduate courses and eventually finding a Ph.D. topic and a sponsor for my research. Added to these tasks was the difficult one of finding housing for myself and my new wife, at a time when there was a terrific housing shortage in Chicago.

Workers had come during the War and stayed, and as the economy made the transition to peacetime activities, modest apartments and student housing was difficult to impossible to find. As a result, we changed house 13 times in the first 12 months, renting apartments from people who were on vacation or travel for work. Beyond that, there were two exams in the Physics Department-- the Basic Exam and the Qualifying Exam, for admission to the Ph.D. program and for certification of the knowledge required for a Ph.D. at Chicago. The first opportunity to take the exam was in May of 1948, and normally I would have taken the Basic Exam a year or two later. But I found from reference to previous examinations that a Basic Exam was no more difficult for me than the Qualifying Exam, and I negotiated the opportunity to take the Basic Exam and not the Qualifying Exam, with the proviso that if I did not pass then I would have to take the Qualifying Exam in 1949 and the Basic Exam in 1950.

I did pass the Basic Exam, with a score that was either the highest or the second highest among the graduate students, and now looked for my thesis sponsor. Fortunately, a chemistry Ph.D. student with whom we became friends was able to move to veteran's housing on the campus, and we were able to rent the converted one-room basement apartment that he, his wife, and his newborn daughter had occupied some three miles from the university. And the next year we were able to move to an apartment a mile's walk from the university that had been converted from an open porch by a physics graduate student friend, Harold M.Agnew, who had just received his Ph.D. and was returning to work at the Los Alamos Scientific Laboratory in New Mexico.

So while I had seen Edward Teller occasionally at the university, attending seminars, I had not had much contact with him until I began the work for my thesis, which was done in the laboratory of Enrico Fermi, under his supervision. Teller would sometimes drop in to the lab to talk with Fermi, who in addition to being one of the finest theoretical physicists of the 20th century, which work he did in his small office and at home, was very much a hands-on experimenter. Fermi loved to make his own experimental equipment, whether it was a glass Geiger counter for the detection of the positrons for an experiment that he was doing with a research associate, Dr. Leona Woods Marshall, or mechanical equipment such as the "trolley" for the large cyclotron under construction at the university, which, residing within the vacuum chamber and resting on the large iron pole face of the cyclotron, could be commanded to move in precise steps along the periphery, carrying a small copper or carbon target that would serve as the source of mesons from the collisions of the 450-MeV proton beam of the cyclotron.

I completed my Ph.D. in Research in December 1949 and was awarded the degree and, unusually, hired by the Department of Physics to the rank of instructor, which gave me full faculty privileges and responsibilities.

Among these privileges was to attend various seminars, large and small, or occasional ad hoc meetings. A weekly seminar was hosted by William F. (Bill) Libby, Professor of Chemistry, who was to be a member of the Atomic Energy Commission and eventually the second husband of Leona Marshall.

Bill Libby's seminar had a simple pattern. Participants had to be prepared to report on their own work or on something that they had learned that would interest the disparate group. I recall that Fermi presented his newly hatched theory of cosmic ray acceleration at such a seminar, and Bill Libby talked about his invention of radiocarbon (C-14) dating and also of the use of tritium for dating materials of age a few years to decades-- such as wine. Fermi was always in attendance at Libby's seminar, and Teller whenever he was not traveling.

Leo Szilard was also at the university and the source of many ingenious ideas.

Although I had SECRET clearance in order to work at the Argonne National Lab's heavy water reactor to prepare the radioactive sources for my thesis research, I knew nothing formally about nuclear weapons. From my reading of the newspapers, I had made some suggestions to Fermi, who presumably passed them along to the people with whom he was involved as a consultant to Los Alamos and a member of the General Advisory Committee (GAC) of the Atomic Energy Commission.

I realized that my salary from the University of Chicago covered only nine months and that my wife and I continue to eat during the summertime, so I would need a source of income. Fermi thought that I could be useful to the nuclear weapons program at Los Alamos and arranged for me to be hired as a consultant. In fact, my wife and I and eventually our children spent almost every summer of the 1950s at the Los Alamos Scientific Laboratory.

For my first summer, in 1950, Fermi and I shared a small office, with facing desks. His back was to the window, and mine to the door that opened into the corridor. Our desks were against a wall on Fermi's right, and on his left (my right) was a space of about 6-ft width next to the desks, with a couple of chairs for occasional visitors. Stan Ulam, a Los Alamos Mathematician, was one of these visitors, every morning for several weeks, while he worked with Fermi on manual calculations of the burning of a long cylinder of deuterium-- fuel for nuclear fusion.

Whereas the atomic bombs that destroyed Hiroshima and Nagasaki got their energy from the neutron chain reaction in fissionable materials, the concept of a thermonuclear explosive was to use a mass of liquid deuterium that at sufficiently high temperature would undergo a reaction on which two deuterons would fuse to form a helium-3 nucleus and a neutron (or, with about equal probability, a tritium nucleus and a proton). The requisite temperature at one end of the cylinder would be provided, somehow, by the emanations from a fission bomb, and there was dispute as to whether it was the neutrons from the fission bomb or the thermal x-rays (to which most of the fission energy is immediately converted) that would be a better heat transfer agent. I have described these calculations elsewhere.(3)

Actually, Fermi would work and Ulam would comment, but it was valuable to Fermi to explain the details of his work to such an intelligent person. Fermi would start with the fundamental equations of reaction rates and energy loss and the variables of ion temperature, electron temperature, energy in the form of x-rays, neutron numbers, and the like. Different columns on the accountant's spreadsheet would correspond to these variables, and successive rows would correspond to successive time ticks or intervals.

Fermi would move methodically across the spreadsheet, filling in the numbers by evaluating the formulas at the top of each column, using his sliderule for multiplication, division, logarithms, and exponentials, and a Marchant electro-mechanical calculator for additions and substractions. After filling in a few rows, to see that the computation was going in a reasonable fashion, the "computer" would be called in and she would take away the computation and return the next morning with a spreadsheet filled in.

Although I was a close observer of the process, I was not involved in the computations. In fact, I had devised and was building an experiment to measure accurately the reaction rate of deuterium with tritium and deuterium with deuterium at energies of importance if a thermonuclear explosive was to be realized. The project was depending on data obtained ten years previously by Tom Bonner at the University of Texas, which, to my mind, was not a sound basis for such an important program, without judging whether it would succeed or not. At the end of the summer, I had made sufficient progress that, with Fermi's advice, Los Alamos created a group to finish the design, construction, and to carry out the experiments, with results published in 1954.(4)

During the summer of 1950, I worked also on devising diagnostic experiments for the nuclear weapon tests planned for the summer of 1951 at the Pacific nuclear test site. I met excellent people from the Naval Research Laboratory and from the Lawrence Berkeley Radiation Laboratory of the University of California, and I spoke several times with profit with Edward Teller. In particular, I was much concerned with the GEORGE shot scheduled for May 1951, which had the purpose of demonstrating the burning of thermonuclear fuel-- a small amount of deuterium and tritium.

But there was not much effort applied to thermonuclear weapons in 1950, despite President Truman's announcement in January 1950, following the Soviet nuclear fission explosion in August 1949, that the United States would step up its work on nuclear weapons, and work also on the hydrogen bomb.

When I returned to Chicago at summer's end, my contact with nuclear weapons lapsed until I went once again to Los Alamos in May, 1951.

There I quickly reviewed what had changed during my absence, and was told of the March 9, 1951 secret paper by Teller and Ulam.(5)

I spoke with Edward about this, and his particular interest was to use the phenomenon of "radiation implosion" to compress thermonuclear fuel. Teller had shown no interest in such compression previously, even though substantial compression could be obtained by the use of high explosive, because, as he was later to record(6) he had a "theorem" that if you could not burn liquid deuterium at normal density (0.16 grams per cubic centimeter) you couldn't burn it if compressed 100-fold. I think Teller was quite proud of this theorem, which is not at all obvious. At first sight, one would think that deuterium at 100-fold normal density would produce at a given temperature 10,000 times the fusion energy per second per unit volume, but Teller observed that in addition to 100 times as many deuterons, there were 100 times as many electrons, and the rate of energy transfer from the deuterons to the electrons (energy cooling) would also be enhanced 10,000 times. And if one considered further energy loss from the electrons to the x-rays that were present, that would scale also by a factor 10,000. Hence Teller's theorem, which was admirable but wrong.

Ulam had come to Teller with the proposal that one use an "auxiliary" nuclear explosion to prepare a "main charge," and specifically to do this with the shockwave of the auxiliary explosive (now called a "primary"). After all, the Nagasaki bomb used just such shock compression of a plutonium sphere, the spherically converging shock provided by 32 high explosive "lenses" made of slow-propagation and fast-propagation high explosive, each energized by a detonator, all fired precisely simultaneously. Ulam's task would have been much easier had he had 32 primaries or even four primary nuclear explosives at the corners of a tetrahedron, but the task was to do it with only one.

When Teller began to calculate how easy or difficult this might be, he found many practical problems and immediately suggested to to Ulam that in any case, it would be easier to use the energy that is normally emitted from a nuclear explosive-- almost all in the form of a flood of soft x-rays.

Containing these x-rays in a case opaque to x-rays that would also enclose the "main" or "secondary" charge, might be easier to accomplish than directing the outgoing shock in some way to converge on the secondary, hence the title of the joint paper.

Teller's interest, of course, was in using thermonuclear fuel as the secondary, but here his enthusiasm was quenched by his long-standing theorem, until he recognized that although the amount of fusion energy potentially available per cubic centimeter increased linearly as compression, the amount of x-ray energy at a given temperature of the fuel was the same per cc, whatever the compression of the co-existing fusion fuel. So of course extreme compression would help.

The physicists who had rightly been skeptical of the dead-end approach of burning liquid deuterium at normal density all recognized that a practical way had just been found, and there was much discussion of this.

When I asked Teller how I could be of help, he explained the concept of radiation implosion and said that he felt that he had only one chance to demonstrate this, through one experiment. And it had to be an experiment that was absolutely persuasive, because if it failed either in performance or in convincing the scientists and the government, his concept and the thermonuclear weapon program would be dead.

I puzzled over how one would actually make a thermonuclear weapon in order to see what kind of experiment would be persuasive that one could make such a weapon. I soon concluded that to make a full-size weapon would be no more difficult than to make a convincing experiment and hence made estimates from the ideas and techniques current in the laboratory at that time and on July 25, 1951 published a secret 4-page memorandum with a large engineering sketch of the test object.

This was ultimately endorsed by the powers that were at Los Alamos the summer of 1951 and the Director, Norris Bradbury, assigned the project to Marshall Holloway, a reliable and sound engineer to carry out this massive project. Of this, Teller made a public statement in 1981,

> "I want to do so by telling you a story of which I believe no one has heard. In the early 1950's when I had the first crude design of the hydrogen bomb, Dick Garwin came to Los Alamos and asked me how he could help. Actually the design I had in mind was not that of a real bomb but of a model for an experiment. I asked

> Garwin to change this crude design into something approximating a blueprint. He did so in a short time-- a week or two. That experiment was carried out. 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."

From the formation of Los Alamos in March 1943, Teller had resented that it had not provided equal or comparable effort on thermonuclear weapons as on fission weapons. The Berkeley summer study in 1942, chaired by Robert Oppenheimer, had spent the first couple of days in discussion among renowned theorists as to how one would actually make a fission weapon out of the materials that might be produced-- highly enriched uranium or perhaps plutonium if it could be effectively produced in the nuclear reactor that was yet to be demonstrated December 2, 1942 in Fermi's "pile" at the University of Chicago. At the summer study, several days of more interesting discussion ensued on thermonuclear weapons, which would, of course, require a fission weapon to heat the thermonuclear fuel.

I am persuaded that Oppenheimer did exactly the right thing in providing Teller with the services of only a few capable colleagues during the War. Teller then resented Norris Bradbury, at war's end in 1945, not devoting a large fraction of the laboratory's efforts to thermonuclear weapons. Again, the only concept available at that time was the Classical Super, and the apparent need was for more and more portable nuclear weapons than for what was thought to be the essence of thermonuclear weapons-- enormous explosive output.

Teller had long argued that the effort at Los Alamos was inadequate to the urgent task of developing thermonuclear weapons and had attempted to create a second nuclear weapons laboratory with that mission. But in 1951 Teller's behavior was bizarre. Here the key to his dream of thermonuclear weapons was available, a design was approved by the Laboratory, and the test was scheduled to be carried out in the incredibly short interval to November 1, 1952.

I recall being at a meeting at Los Alamos in the early Fall of 1951 at which Teller stated that the level of effort at Los Alamos was inadequate to the task, and that a second nuclear weapons laboratory was needed, as a consequence.

This bizarre behavior led to a rift between Teller and Los Alamos, although he was still involved in the program from his new base at the Lawrence Livermore Laboratory 44 miles east of Berkeley, California.

I recently learned from a long-time friend and colleague at Los Alamos at that time, Harris L. Mayer, of his recollection of a meeting at which Fermi and I had presented concepts for the hydrogen bomb based on radiation implosion. According to Mayer, these were discussed briefly at the meeting, but participants then went back to their rather academic discussions of matters concerned with the thermonuclear program. After leaving the meeting, Fermi said in conversation to Harris and myself, "What we need is a King," Perhaps I took that seriously, and jumped to the end with a nuclear explosive itself, rather than an experiment to convince that a nuclear explosive was possible.

Although it is often said that a thermonuclear explosive based on liquid hydrogen was not a practical weapon, during the summer of 1951 I designed a flyable hydrogen bomb with liquid deuterium, and much later learned that the Atomic Energy Commission fabricated six of these that were available as Emergency Capability Weapons, before MIKE was even tested successfully.

With the formation of Livermore, Teller was no longer present much at the University of Chicago, and in any case, I left Chicago in December 1952 to take up a position as a staff member of the IBM Watson Scientific Laboratory at Columbia University. There I would work on topics I had chosen that were new to me-- liquid and solid helium and superconductors-- chosen so that I could work at my own pace rather than have to request time on a synchrocyclotron or other machine, and join with six or more people in a team to carry out the research. But I continued to consult in the summer at Los Alamos and in fact spent a year working half-time 1953-1954 with a group in Cambridge, MA, on matters of air defense.

Like everyone in the nuclear weapon community, I was most disturbed by the Oppenheimer hearing, at which Teller famously said(7)

> "In a great number of cases I have seen Dr. Oppenheimer act-- I understood that Dr. Oppenheimer acted-- in a way which for me was exceedingly hard to understand. I thoroughly disagreed with him in numerous issues and his actions frankly appeared to me confused and complicated. To this extent I feel I would like to see the vital interests of this country in hands which I understand better, and therefore trust more."

And toward the end of his testimony

> "I believe, and that is merely a question of belief and there is no expertness, no real information behind it, that Dr. Oppenheimer's character is such that he would not knowingly and willingly do anything that is designed to endanger the safety of this country. To the extent, therefore that your question is directed toward intent, I would say I do not see any reason to deny clearance. If it is a question of wisdom and judgment, as demonstrated by actions since 1945, then I would say one would be wiser not to grant clearance. I must say that I am myself a little bit confused on this issue, particularly as it refers to a person of Oppenheimer's prestige and influence. May I limit myself to these comments?"

In his Memoirs, Teller discusses the conferences at Erice(8) and in particular the 1983 conference:

> TELLER: "The 1983 conference included a great deal of discussion of President Reagan's proposal of the Strategic Defense Initiative. In spite of great differences of opinion, the exchanges on the topic were cogent and reasonable. Each of the several Erice conferences that I attended was marked by civility and openness. I cannot say whether that pleasant situation was the result of the delightful setting or of Dr. Zichichi's careful

> organization; in any event, I enjoyed these sessions very much because they reflected the collegial nature of science.
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> "About Velikhov, he says, 'When I asked him about Andrei Sakharov, he made a slighting, even scornful comment, where upon our personal relationship took on a sour note ... Nevertheless, at the Erice meeting the following year, Zichichi, Velikhov, and I signed a joint proposal of cooperation on scientific matters pertaining to the prevention of nuclear war and defense against the effects of nuclear weapons.'"

Also in Memoirs, on p. 337, Teller describes my contribution to the hydrogen bomb as he did in his testament of 1979 and at Erice in 1981, but with different details, that I ascribe to an evolution in his memory. But he goes on with a striking commentary far more important than my role, describing the September 1951 meeting at Los Alamos:

> "So, in the end, Garwin's design remained unchanged. But that was the only bright spot. In September, Norris Bradbury finally decided how he wanted to reorganize the laboratory to accommodate the program on the new Super. Ten months had passed between the time I had realized how to proceed with the more straightforward approach and the time the program based on the new ideas was finally going to begin ... Bradbury made a decision that gave me a less welcome message: the effort to complete the hydrogen bomb should be placed in the hands of Marshall Holloway. Holloway, who as a member of the Family Committee, had created difficulties in connection with the hydrogen bomb at every turn. Somewhat negative in his approach to life in general, Holloway had not cooperated on any project pertaining to the Super. Bradbury could not have appointed anyone who would have slowed the work on the program more effectively, nor anyone with whom I would have found it more frustrating to work. Bradbury had announced, in effect, that he did not care whether I worked on the project or not. ... The uncertainties and lack of commitment to the thermonuclear program at last convinced me that depositing the full responsibility for the development of nuclear weapons in one laboratory was dangerous."

And so Teller returned to the University of Chicago and mounted his campaign to create a second weapons laboratory, November 1951 to July 1952.(9)

This is a remarkable statement in a book written for publication in 2001. Teller knew, by then, that less than 14 months after Holloway was given the responsibility, the full-size MIKE test detonated with an explosive yield of almost 11 megatons. Far from "slowing" the effort to build a hydrogen bomb at that point, Holloway demonstrated super-human capability.

Teller's internal clock ran at a different rate from that of other people. Had he remained involved, it would surely have taken longer to the first test, because Teller could not restrain himself from having additional ideas and putting them forcefully so as to make an engineering program almost impossible to complete.

I found this both distressing and puzzling, because Teller was apparently stating a theorem that only those who did not have personalities difficult to understand should be involved in the national security program. Teller's own personality was at least as complex as that of Oppenheimer. But although Teller's words were carefully chosen, I think they had not been thought through. Teller was not an emperor, and yet he was expressing a personal preference which would have impact only if it were a theorem and not a desire.

Although distressed at Teller's performance and, I believed, duplicity, I saw no benefit in refusing to greet him or to work with him.

My later interactions with Edward Teller were in part through his association with Livermore and in great part through our presence in programs of the Ettore Majorana Centre for Scientific Culture in Erice, Sicily. This Centre had been created in 1963 by my friend and colleague, Antonino Zichichi, with whom I had worked intensively on an experiment at the European nuclear research laboratory, CERN, in Geneva. This had begun in the Fall of 1959, and was completed in 1962, although I was there for only a year of sabbatical from my IBM job.

Zichichi's Centre was initially a venue for NATO summer schools, sponsored by the NATO Science Committee, but by August 1981, the concept had expanded to a week-long International Seminar on Nuclear War.

The volumes reporting on International Seminar on Nuclear War include 1981, "Worldwide Implications," 1982, "How to Avoid a Nuclear War," and 1983 "The Technical Basis for Peace." These are remarkable in that they are not only a record of the presentations, but a near-verbatim transcript of questions, interventions, and responses. For our present purpose, they are extremely valuable in providing Edward Teller's own words, without later review.

For instance, in 1981, in Session 5, "The Future of Arms Control and Developments" I presented a paper, "Future Strategic Forces," (pp. 109-141). This no doubt had slides or transparencies, not reproduced in the report, but the actual words of my presentation are there, as are comments by Teller (pp. 130).

> TELLER: "I will not enumerate objections where I have them, nor even will I talk about the smaller number of ideas of Professor Garwin with which I agree. But in the absence of my good friend Eugene Wigner, I would like to emphasize again, the importance and above all, the practicability of civil defense ..."

And Teller goes on with a very cogent argument that civil defense could employ in the United States the "great reservoir of unskilled, unemployed labor which has to be maintained whether they work or not," and with a history of the 1965 earthquake in Anchorage, Alaska, where the intervention of the United States armed forces, locally, were important to limiting the death toll to 130. And then I go on,

> GARWIN: "I would like to agree 100% with my colleague, Professor Teller, and I have tried also for many years to obtain acceptance in the United States for the principle of sharing resources for the management of natural disasters and wartime emergencies. The

> army and military have not been in favor, for the most part. ... Now, of course, the difference between a natural disaster and a large nuclear attack is that there is an _outside_ in the natural disaster. ... Nature does not intentionally interfere with the means of preservation and restoration ..."

This week-long seminar was attended also by Georges Charpak, who was in this way introduced to questions of nuclear weapons and strategy and has since been much involved in his native France. American scholars included not only myself but Spurgeon M. Keeny and Jack Ruina, as well as Edward Teller and Eugene Wigner, long-time practitioners of the arts of nuclear weapons and their control. Furthermore, Solly Zuckerman, who had planned the allied invasion of Trapani, the coastal city that lies just below the mountain village of Erice, participated. Zuckerman (then Sir and later Lord) spoke on "The Necessity of Arms Control" (pp. 76-84) followed by pages of discussion by myself and Edward Teller, who argues (pp. 89-90) against forbidding "that which is not invented." And then

> "The second point is one where I have found it difficult to contain my anger at what I have heard. But I will attempt to contain that anger. I will really quote as best I can and I hope with dispassion. I refer to the statement that Washington and Moscow would have perfect defenses which will leave Rome and London as hostage to the terrible weapons that have been developed. This is indeed a dreadful picture to paint, and it was painted with the intention to be dreadful ... I spoke again and again with emphasis and conviction about the defense of the free world, including Sir Solly Zuckerman's wine cellar. I am talking of new ideas and Sir Solly Zuckerman agreed that new ideas, that new technologies, are more important than the quantitative race. How then is he to advocate that these new ideas be hamstrung by agreements against new weapons which are not yet even defined. ... Technical development for war and peace go hand-in-hand. Technical developments in war for attack and defense can be distinguished, but it is true that this distinction, while it exists, is not completely easy and therefore I agree at least in part with what Dick Garwin said. A defensive weapon that was really big was tested. It was not my idea about the right kind of defensive weapon, but still these defensive weapons are very much more than instruments with special interest, and very much more advanced than vaporings about the future. I wish that Dick would spend a little more time with these ideas, and that Sir Solly Zuckerman would spend infinitely more time on these ideas. ... I had been talking about secrecy, and I have been talking against secrecy for 36-years. I've done it consistently since that time, not during the War. I have succeeded practically single-handed in the United States in breaking down the secrecy on research, on controlled thermonuclear reactions which was very wrongly classified ... But I advocated the same thing in regard to weapons ... Indeed the abolishing of secrecy ought to be even the first condition that might make honest negotiations about disarmament even imaginable."
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> ZUCKERMAN: "Dr. Teller's implicit assumption that it is possible to discriminate between offensive and defensive weapons has

> always lacked plausibility. The French have a saying: 'This animal is very mean; when you attack it, it defends itself.' Anti-tank guns are designed to defend against attacks by tanks. But to the attacking tanks they are not defensive-- they are offensive. The same is true of every variety of weapon-- including, were they ever to be used, nuclear weapons."

At the session of the International Seminar on Nuclear War in 1982, the American contingent was supplemented by Roger Batzel, Director of the Lawrence Livermore Laboratory that Teller had founded, Eugene Wigner, and also Frederick Seitz, a physicist prominent in U.S. defense policy for many years. Also Professor Richard Wilson of Harvard, and Teller's colleague, Lowell Wood of Livermore. This time there were two Soviet scientists, Pyotr L. Kapitza, Nobelist in Physics for his work in low-temperature physics and Evgenij P. Velikhov, Soviet plasma physicist in charge of their program of thermonuclear fusion research.

Of particular interest is the colloquy on pp. 216-224, where I comment (p. 218) "Well, I think that Professor Teller has given another example of ingenuity and passion and wonderful presentation with most of which I certainly agree but cannot compete. ... so I would like to go back to another public statement Professor Teller made to some tens of millions of Americans over national television during the argument of the late-1960s or maybe 1970 about the deployment of the ballistic missile defense system. At that time Professor Teller said specifically that the Americans had an 'ace in the hole.' It is now at least 13 years later and I wonder what has become of that 'ace in the hole.' Is it still secret? Did it exist? Was it effective? Can we now discuss in some detail this specific example of secrecy which you raised at the time?

After much discussion I repeat, "Just a question of fact. This device or approach existed in 1969, your 'ace in the hole'?

> TELLER: "I wanted to disclaim any credit for any new invention that might have been made. Maybe these inventions exist, maybe not. Maybe these American colleagues exist, maybe not. Maybe they have existed in 1969, and maybe not. But as to you Dick, I think you are in the wrong profession. I congratulate you, Nino, for having invited a lawyer. He's excellent at cross-examination."

Velikhov then gives a prepared presentation (pp. 225-231), in which he remarks on the offensive side of missile defense,

> "Besides, it should be mentioned that when BMD is planned for the first strike it could function better. Chances to use effectively all components of the system to synchronize the work of different elements, etc., appear. Violation of the ABM Treaty would be a really aggressive act. ..."

Velikhov also criticizes the supposed effectiveness of Soviet civil defense in protecting its population against nuclear attack.

There was, however, substantial agreement, especially among Teller and Velikhov for the reduction of secrecy. This resulted in the Erice Statement on Science, Technology, and Peace of August 1982 signed by

Dirac, Eccles, Garwin, Kapitza, Seitz, Teller, Wigner and Zichichi but not by Velikhov, who perhaps had left the seminar by that time.

The 1983 International Seminar on Nuclear War, "The Technical Basis for Peace," was attended by Harold Agnew, former Director of the Los Alamos National Laboratory and then President of the General Atomic Company, Roger Batzel, Georges Charpak, myself, Dixy Lee Ray, former Chair of the U.S. Atomic Energy Commission, Jack Ruina, Fred Seitz, Teller, Velikhov, Wigner, and Lowell Wood, as well as by Soviet scientists, Vladimir Aleksandrov, Computing Centre of the USSR Academy of Sciences, Moscow; Moisey A. Markov, USSR Academy of Sciences, Moscow; Rem P. Soloukhin, Institute of heat and Mass Transfer, Byelorussian Academy of Sciences, Minsk; and Eugenij P. Velikhov, USSR Academy of Sciences, Moscow.

Although Teller at Erice in August 1983 continued to emphasize civil defense and protection of the population, he and the proceedings were energized by the March 23, 1983 national television broadcast by President Ronald R. Reagan-- the "Star Wars" speech. In fact, this scheduled presentation was for the most part a typical plea by the President for public support of his defense budget. At the end, a few short paragraphs replaced a place-holder in advanced copies of the speech, and they were unknown to Secretary of Defense Caspar Weinberger and Secretary of State George P. Shultz until the day of the speech.

In this insert, President Reagan said:

> "Let me share with you a vision of the future which offers hope. It is that we embark on a program to counter the awesome Soviet missile threat with measures that are defensive. Let us turn to the very strength in technology that spawned our great industrial base and that have given us the quality of life we enjoy today.
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> "What if free people could live secure in the knowledge that their security did not rest upon the threat of instant U.S. retaliation to deter a Soviet attack, that we could intercept and destroy strategic ballistic missiles before they reached our own soil or that of our allies?
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> "I know this is a formidable, technical task, one that may not be accomplished before the end of this century.
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> "... I clearly recognize that defensive systems have limitations and raise certain problems and ambiguities. If paired with offensive systems, they can be viewed as fostering an aggressive policy, and no one wants that. But with these considerations firmly in mind, I call upon the scientific community in our country, those 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. ..."

Just three weeks later, the Los Alamos Laboratory was celebrating its 40th anniversary with several days of presentations and discussions. In particular, I had been asked to participate in a panel discussion in the LANL auditorium, which was completely full. The proceedings were covered by CBS-TV and recorded also by LANL.(10) In preparation for my talk, I had prepared a transparency for projection that dealt directly with one of Teller's favorite defensive weapons against Soviet ballistic missiles. As he was later to say at Erice, a satellite with a defensive weapon aboard would be costly to put up and probably much cheaper to shoot down. Therefore that would not be a good approach. He favored, instead "pop up" interceptors that would carry the weapon into space, where it could have a clear look at Soviet missile boosters as they rose above the atmosphere. Teller was very supportive of "shortwave" lasers, and, in this case, of x-ray lasers that would be powered by a nuclear explosive. The x-ray laser would direct its energy in a very narrow cone, and Teller and his Livermore colleagues even argued that a single nuclear explosive could "pump" many sets of x-ray laser rods, directed individually at rockets in a salvo launched simultaneously by the Soviets against the United States.

Of course, the interceptor, assumed to be based on a submarine, would still need to rise not only above the atmosphere, but to an altitude from which the target missile would be above the horizon, and this would take early detection of the launch, rapid communication of the launch and probably its approximate position to the submarine via a control network, and the launch of a very high speed interceptor. In my transparency, I assumed that x-ray lasers could be built and that they would be effective, but I showed that if a relatively fast burning Soviet missile that took 200 seconds to reach full speed instead had its burn time shortened to 100 seconds, then an interceptor that might weigh 50 tons to deliver a half-ton x-ray laser to firing position would need to have not just a shorter rocket burn time but double the speed, so that it would weigh 5000 tons-- a totally impractical launch mass.

The 100-sec burn ICBM would require a sacrifice of perhaps 5% in payload, but as was later established by a study of the American Physical Society based on information from U.S. strategic missile builders, that would be a small sacrifice and no great technological challenge.

I was dismayed to find a Livermore scientist at a meeting in New York two years later who still had not gotten the simple point that a fast-burn strategic missile could not be countered by a fast-burn interceptor.

I discussed also a system that was being advocated by Sidney D. Drell and myself in which the very large 10-warhead 100-ton MX missile that was part of the Reagan defense modernization program would be survivably based two or four to a small submarine. Rather than being oriented vertically as are the submarine launched ballistic missiles, each MX would be in a neutrally buoyant capsule strapped along-side the hull of the submarine. This concept had been fleshed out in a Secret study for the U.S. government and reported in an unclassified article by Drell and myself.(11) We had studied how to provide accuracy at least as good as existing ICBMs by the use of GPS satellite signals and, because in nuclear war GPS might not be available, by use of hundreds of radio beacons ("pseudolites") deployed on U.S. territory. Knowing that the work could be vulnerable to those who asserted knowledge of highly classified Soviet antisubmarine warfare (ASW) capabilities, Drell and I had requested from the Deputy Secretary of Defense at the time the study was initiated, William J. Perry, a statement that our knowledge was adequate to judge the effectiveness of Soviet ASW systems and prospects.

In fact, I had chaired for many years the ASW Panel of the President's Science Advisory Committee (PSAC).

In the panel discussion, we find,

> GARWIN: "I think we ought to work on these military technologies ONLY openly and jointly. And I go farther than Edward, I think, because I think we shouldn't work on them unless we are willing that the Soviet Union had them as well. Had we done that with MIRV we would not be in the present situation where we feel our land-based forces are vulnerable. That's really a test of whether the government regards this as truly stabilizing or just states that it's stabilizing in order to sell the program. If it is truly stabilizing for both sides to have it then let's give it to the Soviet Union ..."
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> TELLER: "... I think it is much more important to emphasize the points of agreement than those of disagreement. And I think through our discussion and also now the phase of agreement is obvious. I would strongly advocate to start this international cooperation with those people with whom we have cooperated and with whom we know we could cooperate and I will not try to discuss in detail or object to anything else except I would like to tell of an experience which Dick and I have shared in Erice last summer.
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> "The Conference was opened by Zichichi saying, 'the politicians have messed everything up. Time for the scientists to take over.' I tried to respond by saying, the scientists would be excellent provided they have information. In a situation where secrecy prevails and where the scientists can't talk to each other about the facts, the scientists are no better-- conceivably to make a crazy statement-- even poorer than the politicians. Remarkably enough there was no objection to that. There were Soviets present. Velikhov was in agreement, everybody was in agreement. We discussed for three days. We could agree about nothing else, but there was a concrete proposal to be signed, let's decrease secrecy. At that point Velikhov stood up and said, if we cannot agree on anything else, to agree on secrecy makes no sense either. (Laughter). It seems to me that there are people with whom it is more easy to collaborate than others."

And apparently that is the reason why Velikhov did not sign the 1982 Erice statement-- in view of his official position with the Soviet government.

Later in the discussion we have,

> TELLER: "... I believe the response to a poor defense is to be prepared to override it. The response to a good defense is to imitate it. I am therefore very anxious, not just for a defense, but for a good defense. And good defense I hope will bring the reasonable response from the Soviet Union-- reasonable from their point of view-- that we also want to be defended. ..."

I described communication with the U.S. submarine retaliatory force, and Edward was asked for his views:

> TELLER: "There is one point I would like to put in because I am afraid that otherwise it would go uncontradicted and it should not. And that is that our submarine forces are invulnerable. They are. You know why? Because the possibility of finding them and destroying are kept so secret that the Navy does not know it itself. For the short time, 7 years, while I was serving on the President's Foreign Intelligence Advisory Board and we got more information (I got more information) about what the Soviets are doing-- Dick, I am contradicting you that all of us have access to everything. It's simply not true. ... But in this period I got thoroughly convinced that there are a number of real possibilities to destroy the submarines. As you see there is a difference between Hans (Bethe) and me which has existed approximately for the last 60 years. And it's not connected with weapons. It is that Hans always was more doubtful about things that one can accomplish in the future like increasing the energy from a cyclotron or whatever else. You see, it's always wonderful to say this cannot be done. Defense cannot be done. Submarines cannot be destroyed. Please be careful and accept all statements it cannot be done with a grain of doubt and restrict them to the statement that the perpetual motion machine cannot be done."

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1 "Working With Fermi at Chicago and Post-war Los Alamos," by R.L. Garwin, presented at University of Chicago, Enrico Fermi Celebration, September 29, 2001, http://fas.org/rlg/010929-fermi.htm.
2 "Enrico Fermi and Ethical Problems in Scientific Research," by R.L. Garwin, presented at Public Lecture in the Centennial Celebration "Enrico Fermi and Modern Physics," Pisa, ITALY, October 19, 2001, http://fas.org/rlg/011019-fermi.htm.
3 "Enrico Fermi and Ethical Problems in Scientific Research," by R.L. Garwin, presented at Public Lecture in the Centennial Celebration "Enrico Fermi and Modern Physics," Pisa, ITALY, October 19, 2001.
4 "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.
5 "On Heterocatalytic Detonations 1: Hydrodynamic Lenses and Radiation Mirrors", E. Teller and S. Ulam joint report LAMS-1225, 09 March 1951.
6 Transcript of taped conversation between Edward Teller and Jay Keyworth concerning an important question in the wartime history of Los Alamos-- "How the idea of the implosion emerged," September 20, 1979.
7 Page 572 "Memoirs."
8 Pp. 345-347 "Memoirs."
9 "Memoirs," Chapter 27, pp. 330, ff.
10 "Issues in Arms Control," 40th Anniversary Colloquium at LANL, panel discussion by H.A. Bethe, R.L. Garwin, D.M. Kerr, and E. Teller, April 13, 1983, from LA-UR-05-6089, DVD 3 of 3.
11 05/00/81 "Basing the MX Missile: A Better Idea," by S.D. Drell and R.L. Garwin, Technology Review, May/June 1981.

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