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The Historian and the Nuclear Temptation

April 12,1945 marked a critical turning point in the history of nuclear warfare. On that day Franklin D. Roosevelt, President of the United States, died. Some two weeks earlier, on March 25, Leo Szilard, the Hungarian born physicist who had been one of the initiators of the atom bomb project, wrote a letter to Roosevelt using the same services of Albert Einstein to reach the President as had been used to stimulate bomb construction. This time, however, Szilard's goal was to block the use of the bomb. We know that the letter followed an exhausted President to his retreat at Warm Springs, Georgia where he was resting after the rigors of the big power conference at Yalta. No attention was given to Szilard's plea by the 29th of March and Szilard arranged an appointment with the President's wife in an effort to enlist her in the campaign to stop the rush toward testing, and ultimately using, the atom bomb. Roosevelt was dead before the appointment ever took place, and on the evening of April 12 the newly inaugurated Vice President, Harry S. Truman, was taken aside by his predecessor, Henry A. Wallace, and by Secretary of War Henry L. Stimson and told for the first time about the atom bomb project.

By late spring Germany's defeat was certain and the war in Europe was over. The secret ALSOS mission sent into Germany with the front line troops to round up those scientists capable of building an atomic bomb rapidly concluded that there was no German bomb. But since great secrecy surrounded the bomb project in the United States, there were no policy debates to guide the decisions about the next steps for the atom bomb. Alternatives to the ultimate use of the bomb over two Japanese cities, Hiroshima and Nagasaki on August 6 and 9 were never explored. The atomic era opened with no prior public examination of the consequences that would follow actual use of the atomic bomb.

Were a large-scale nuclear war ever fought, there would be no opportunity for historians to pore over the documents and declarations and to write the critical analyses of steps taken and alternatives missed that have marked the study of previous wars. Historians may begin the task now, however, recognizing that their examinations of assumptions (including the moral dimensions), processes and choices among possible alternatives might serve to influence policy and action in the making rather than merely examining it in retrospect. Although my own bookshelf contains several linear feet of volumes about »the bomb,« historians have done little of the hard probing they are trained to do. Journalists have led the way with studies of variable quality, often serving particular interests or parties. Historians of science, my own colleagues, have been noticeably silent on the history of nuclear weapons. The official histories remain just that: much material but largely uncritical. In addition, they were written too early to permit them to make use of documents that have since been declassified or forced into the open through the use of the Freedom of Information Act. An early critical study - Alice Kimball Smith's *A Peril and a Hope* (1965), an examination of the scientists' political movements in the immediate post-war years - still stands alone in providing some of the social and political history of the atomic weapons projects. The other group who has re-examined the steps taken in the development of nuclear weapons is that of the scientists who were themselves participants in weapons design and construction. Several of these volumes like Herbert York's *Race to Oblivion* are excellent; others have a quality of self-justification. Indeed, there is something in the tone of many of the *current* arguments and actions by former participants that reminds me of the line from Tom Stoppard's play, *Rosencrantz and Guildensterra Are Dead:*

> Our names shouted in a certain dawn ... a message ... a summons ...
> There must have been a moment, at the beginning, where we could have said »no.«
> But somehow we missed it.

My purpose in this paper is to examine historically several of these possible turning points. By scrutinizing the underlying background knowledge and the structures of decision making, and by especially focusing on the emerging role of experts - the scientists, engineers, and so-called »defense intellectuals« largely drawn from the ranks of economists and political scientists - we may be able to identify structural weaknesses in the decision process and not merely recount the »accidents« of history.

»Nuclear fear« refers to the sense of foreboding or outright fright that accompanied almost every stage of development of our knowledge about radioactivity. Parts of this underside of the history of nuclear physics has been explored in an essay by the historian of science Spencer Weart. His study, and those that I have undertaken alongside it, show a consistent pattern of realisation on the part of scientists and commentators upon science, that the release of radiation from heavy elements represented a process of significant potential danger. Pierre Curie, who together with his wife Marie was responsible for the earliest sustained work on radioactive elements, travelled to Stockholm to receive the Nobel Prize for 1903. In closing his address he gave direct recognition to upcoming problems:

> It is conceivable that radium in criminal hands may become very dangerous, and here one may ask whether it is advantageous for man to uncover natural secrets, whether he is ready to profit from it or whether this knowledge will not be detrimental to him. The example of Nobel's discoveries is characteristic; explosives of great power have allowed men to do some admirable works. They are also a terrible means of destruction in the hands of great criminals who lead nations to war. I am among those who believe, with Nobel, that mankind will derive more good than evil from new discoveries.

While he expressed the kind of optimistic faith so often demonstrated by scientists when their work raises problematic issues, Curie began a pattern of fearful estimates of the consequences of radioactive release. The literature abounds with similar contemporary assessments by those close to the scientific work such as Frederick Soddy, Ernest Rutherford, and Cecil Dampier-Whetham.

Detailed science fiction accounts such as H.G. Wells's *The World Set Free*, written on the eve of the First World War, also raise these dangers. In this book about POWER (Wells always capitalized the word), he was the first to call the new weapon an »atomic bomb.« Its use in warfare (primitive by Hiroshima and Nagasaki scales) would occur about 1933 in Wells's predictions, and because the outcome was so catastrophic to the European nations, a successful if troubled move to world government was the response.

In the interwar years, perhaps because they saw the application of science to warfare, additional numbers of scientists expressed fear of the development of atom weapons. Both J.B.S. Haldane and Bertrand Russell, in the course of a debate about science in the human future, expressed worry about weapons derived from radioactive sources. Pierre Joliot, who together with his wife Irene Curie won the Nobel Prize in 1935, took his father-in-law's fear one step further, realizing that the transformation of matter may form a chain reaction capable of the »enormous liberation of useable energy.« He worried out loud about the »contagion« of such a reaction spreading to all the elements of the planet and unloosing a cataclysm, likening it to what astronomers believed to occur in the formation of a Nova, a new hot star.

It was about this time, Leo Szilard has recalled, that he also began serious speculation about a nuclear chain reaction. In 1932 he had read H.G. Wells's »fantasy« and clearly was influenced to try out the possibility that in fission some element or another might release a second neutron and thus increase the probability of an explosive chain. He went so far as to patent the idea, but unfortunately (for him) he had chosen the element, beryllium.

Those physicists who thought of themselves as more responsible, not given to speculation, derided, as did Robert Millikan, their colleagues »who have pictured the diabolical scientist tinkering needlessly, like a bad small boy, with those enormous stores of sub-atomic energy and some sad day touching off the fuse and blowing our comfortable little globe to smithereens.« Such explosions, he wrote in 1930, were impossible. Ernest Rutherford, the dean of British nuclear physicists, recalled Szilard, reacted in a similar fashion in his 1933 address to the British Association for the Advancement of Science, commenting that anyone who talks about liberating atomic energy on a large scale is talking »moonshine«.

The underlying implications of the release of large amounts of energy through nuclear fission were widely recognized, if not wholly agreed upon, within the scientific community by the late 1930's as the sampling of remarks above indicates. But upon careful examination it becomes apparent that while science had the organized institutional means for continued pursuit of power from the atom, there were no structures in the scientific community (or for that matter in the body politic) for dealing with the increasingly negative potential consequences of these new discoveries in science. The scientist was alone with his / her observations and fears. This became quite noticeable in late 1938 and early 1939.

It was in December, 1938 that Otto Hahn and Fritz Strassmann, working in Berlin, discovered the fission of uranium when it was bombarded by a neutron. Within a few months three groups, Joliot and colleagues in Paris, Enrico Fermi and his associates in New York, and Leo Szilard and a colleague also in New York, discovered the pattern of neutron emission in the break-up of uranium and realized the potential for an explosive chain reaction. Others very rapidly heard of the discovery and did their own calculations of the energy that might be released. J.R. Oppenheimer in a letter of February 5,1939 relates his response: »So I think it really not too improbable that a ten cm cube of uranium deuteride ... might very well blow itself to hell.«

While Szilard and some others initially proposed the idea of keeping the fission research results secret and there was some discussion of such a research publication moratorium among the three groups most actively involved, they were unable to achieve it. There were no effective procedures or structures in the scientific community for analyzing, suggesting, or carrying out such a moratorium. As Spencer Weart reported in his essay, the research went public and the public was given a largely optimistic picture, the fears somewhat suppressed. The *New York Times* (1939) reported hopes for »Wellsian utopias where whole cities are illuminated by the energy in a little matter.« Other journals reflected the growing nervousness in the scientific community. But on the eve of launching, in secret, the project to build an atomic bomb, there was no formal discussion in public of the implications of the plan. The war was getting under way in Europe and, one might argue, representative decision making was therefore not possible. Even more important, secrecy was such that among scientists only those directly involved knew that a project was launched to make a bomb.

The assumption of those in the know about the knowledge process was that there could / should be no interfering with new developments in science. So at the first critical turning point, knowledge *was* available, potential consequences even widely speculated on, but procedures for discussing the implications and developing a decision-making framework seemed precluded by context (war and secrecy) and impeded by the unavailability of procedures and mechanisms for conscious decision-making involving significant amounts of technical knowledge, large numbers of scientists, and the public interest. Was this a point at which they »could have said no,« but neither individual scientists nor the scientific community knew how to do so?

Albert Einstein, when he heard in 1950 of the decision on the part of the United States to take another giant step in the nuclear arms race by building the »Super« or Hydrogen-Bomb, complained of the seeming »ghostlike character of this development [which] lies in its apparently compulsory trend.« Every step, he sensed, appeared as the unavoidable consequence of the preceding one. »In the end,« he pessimistically noted, »there beckons more and more clearly general annihilation.« The sense of a technological determinism and a developmental momentum marked the earliest important decisions in the atomic program. One lingering question must be asked, but often remains only implicit: Why did we not stop the A-Bomb project when the Germans surrendered? The bomb was born, initially, in fear of Germany; yet by late 1944 it had become clear that the German uranium program was not far advanced (the reasons for this German failure are complex and interesting, but I will leave them for another discussion). There was no threat of a German atomic bomb, and there was no anticipation, whatsoever, that the Japanese were trying to construct one. The initial argument for the U.S. atom bomb program was gone; was it possible then to stop? Or at least to pause? Some of the scientists involved believed a halt was possible, indeed desirable. Joseph Rotblat actually left the project early, seeing no necessity for continuing. An important group at the Metallurgical Laboratory in Chicago, including Szilard, James Franck, and others urged that some sort of non-lethal demonstration of the bomb's power be conducted as a means of hastening Japan's surrender. The moral implications of the A-bomb effort were not examined as a part of any of the official inquiries that went forward in the spring and summer of 1945. But, then, the context must be recalled.

Strategic bombing which had become a part of the allied strategy in response to German bombing of largely civilian city targets - Rotterdam, Coventry, et al. - had significantly escalated the attacks on civilian targets first in Europe and then in Japan, which incidentally had not engaged in strategic bombing of its own. It was clear from early in its development that the atom bomb was a weapon which would have its greatest impact upon noncombatant civilians. However, this aspect of its use was no longer an issue after the barrier against destroying civilian targets had been passed in the strategic bombing decisions.

Although several internal memoranda (Jeffries Report) and informal discussions (James Franck, et al.) addressed the longer-term consequences of use of the atom bomb in warfare, there was again no operational mechanism or forum for evaluation of these issues. In particular, no mechanism existed which would have been forceful enough to have made itself felt at the policy-making level. This is illustrated by the fact that the proposals for alternative use, such as a non-lethal demonstration against dummy targets at sea, received no serious consideration. The bomb project and the weapons laboratories were geared up for making an atomic weapon. There was production momentum, and there was a strong and unquestioned commitment to build the bomb. J. Robert Oppenheimer, the scientific director of the bomb project at Los Alamos (the focal construction center), made this point clear in his letter of October 6, 1944 to General Leslie Groves, the military commander of the project: »The Laboratory is acting under a directive to produce weapons; this directive has been, and will be, vigorously adhered to.« A controlled test or experimental demonstration would not suffice. A »techno-strategic momentum« (a concept to which I will return later) drove the program forward.

Although by 1947 Oppenheimer referred to the physicists' involvement in planning and building the atom bomb as »knowing sin,« his sense of the scientists' role in the project reflected another attitude well known in the scientific community:

> But when you come right down to it, the reason that we did this job is because it was an organic necessity. If you are a scientist you cannot stop such a thing. If you are a scientist you believe that it is good to find out how the world works; that it is good to find out what the realities are; that it is good to turn over to mankind at large the greatest possible power to control the world and to deal with it according to its lights and its values.

These were his words before the scientists of the Los Alamos labs as he was about to take leave of them on November 2, 1945. But he did not back away from this image of how the scientist conceived of himself even when working in the midst of a morally problematic situation. During the quasi judicial loyalty hearings of 1954 in which he was stripped of his security clearance, he reviewed once again the motivations of the atom bomb scientists:

> However, it is my judgment in these things that when you see something that is technically sweet, you go ahead and do it and you argue about what to do about it only after you have had your technical success. That is the way with the atomic bomb. I do not think anybody opposed making it.

Technical »sweetness«, when linked to the wartime conditions of secrecy made almost certain that hard questions would not be asked; that the moral considerations and long-term social / political concerns would give way to the techno-strategic momentum, the urge to end quickly the war in Japan, and the not too hidden desire to keep the Soviets in their place. It seems clear that although alternative paths were open, there would have to have been an explicit decision *not* to use the bomb to stop the new weapons from running their full course and being used. But another question remains unanswered. Why was the second bomb, the Nagasaki bomb of August 9, used, only three short days after the devastating, and not at once understood, destruction of Hiroshima on August 6, 1945? How many points were passed when there could have been a decision to say no?

The decision announced by President Harry S. Truman on March 10,1950 to move ahead in an all-out program to build the Hydrogen bomb represents another of those critical points where alternatives did exist, but remained unexplored. Instead, a new phase in the nuclear arms race became linked to the increased tensions of the rapidly emerging cold war between the United States and its allies and the Soviet Union. In producing the first atom bombs, the U.S. had gained a monopoly on these new and exceedingly powerful weapons. As the revisionist historians led by Martin Sherwin and Gregg Herken have convincingly demonstrated through their use of newly released documents, the United States had every intention of maintaining that monopoly and using it to advantage in its dealings with the Soviets. United States political leaders believed that the Soviet bomb was at least a generation away. What a sense of shock they must have faced when the Soviets »prematurely« exploded their atom bomb in September, 1949!

The arguments over the H-bomb decision have been examined in several books and articles, Herbert York's *The Advisers* being the fullest. McGeorge Bundy, National Security Adviser to Presidents Kennedy and Johnson, in a recent article makes the strongest case for a possible alternative. The question he leaves with us is central: was this a lost opportunity to gain control of the nuclear arms race? In part an answer would have to rest on a prior question: did the U.S. previously want to see the achievement of international control, or, as Sherwin and the others claim, was U.S. policy actually set against international controls and more interested in maintaining U.S. superiority? We know that, in the immediate post-war years, many of the key figures in the A-bomb project fought vigorously both for civilian domination of future atomic development (rather than the military) and for international agreements. These positions put the scientific advisers squarely in conflict with political leaders acting within the Cold War dynamic.

Once again secrecy shrouded the decision-making process and the public learned only later about the several points of view advanced by responsible and knowledgeable people. On nuclear issues, a »habit of secrecy« had gained permanence in government.

Another element emerged at this time which was to become a permanent part of all future discussion of sophisticated or high technology weaponry. A group of»scientific enthusiasts« for weapons development made important alliances with political figures in Washington and built pressure for Hydrogen Bomb construction. E.O. Lawrence, Luis Alvarez, and Edward Teller shared strong (anti-communist) political views and significant entrepreneurial skills. Lewis Strauss, who became head of the Atomic Energy Commission, and Senator Brian McMahon from the congressional Atomic Energy Committee were among the major political supporters of weapons development. On the other side, the General Advisory Committee of the AEC, chaired by J. Robert Oppenheimer and having as members several of the key scientific builders of the first bomb, took a much harder and more negative look. They were aware of the prior enthusiasm for a hydrogen bomb shown as far back as 1943 by Edward Teller, but were not convinced of the necessity for the new weapon. At the end of a three-day meeting in October 1949, they expressed their unanimous »hope that by one means or another, the development of these weapons can be avoided.« Their report raised the normal technical considerations, but also advanced moral concerns:

> It is clear that the use of this weapon would bring about the destruction of innumerable human lives; it is not a weapon which can be used exclusively for the destruction of material installations of military or semi-military purposes. Its use, therefore, carries much further than the atomic bomb itself the policy of exterminating civilian populations.

In addition to their unanimous recommendation against building the bomb based on their judgment that any military advantage would be significantly outweighed by the extreme dangers to humankind, two of the committee members (Fermi and Rabi) presented an additional argument. First, that a political announcement be made by the President outlining that such a weapon is wrong on fundamental ethical principles and, second, that other nations be urged »to join us in a solemn pledge not to proceed in the development or construction« of these weapons. Such a pledge, they contended, could be made even without control machinery (they were obviously well aware of the failed efforts at international agreements) since a violation »could be detected by available physical means.« In addition, they noted that the U.S. had an adequate supply of atomic bombs to offset any gain from cheating. Their proposal was, in fact, a call for a non-negotiated, yet mutual, freeze on Hydrogen bomb development. Here was an alternative that involved both informing the public and consulting the Soviets.

The Atomic Energy Commission was evenly split in their response to the G.A.C. report. David Lilienthal, the chairman, joined the opposition to the H-bomb giving it a majority but not authority. President Truman, wanting a rapid decision to ward off the political pressures that were being generated by insider lobbying and fearful that the issue would go public, appointed a three-member committee (Secretaries of State and Defense, Acheson and Johnson and AEC Chairman Lilienthal) to resolve the dispute for him Was he not disingenuous, knowing that there was two to one support for the bomb on the committee?

Oppenheimer, in his presentation to the new three-man committee, claimed that atomic (or fission) bombs were adequate for the military task and that hydrogen (or fusion) bombs were not militarily necessary. He went one step further, responding to the fear that the Strategic Air Command might conceive of using new, very powerful hydrogen weapons for obliteration bombing, and proposed the development of small »tactical« atomic weapons for possible fighting in areas such as Europe. Here was a classic »technical fix.« Solve a deep political-strategic disagreement with a new piece of hardware. In the end, of course, both the hydrogen bomb *and* tactical nuclear bombs were built and deployed.

Senator Brian McMahon argued that the Hydrogen bomb, because it greatly increased explosive power, could compensate for the lack of accuracy of other weapons. He further asked whether H-bombs were any less moral than A-bombs, which had been enlarged through the addition of boosters, or for that matter less moral even than tactical weapons.

Harry Truman met with his three-man committee on January 31,1950 for a total of seven minutes and probably did not read their supporting analysis. Instead he asked one pointed question: »Can the Russians do it?« In response to the now obvious positive answer, he decided to go ahead with an H-bomb project and tell the public about it only after the decision. By March 10, Truman had abandoned all constraints and endorsed an »all out program.« Was the wartime requirement for decision in secrecy and speed present? Was a unilateral move to build a major new weapon militarily justifiable? Was the failure to attempt a program of mutual restraint with the Soviet Union so obviously unworkable as never to be tried?

Another procedural block to full consideration of the issues was invoked on this occasion. The General Advisory Committee was pledged to secrecy, even after the presidential decision. No further discussion was possible. As Lilienthal commented, they were »gagged.« They could not move to the public arena even though Oppenheimer and J.B. Conant, a member of the GAC, believed that the public could have been convinced that the H-bomb was both unnecessary and too dangerous; nor could they bring their positions to the Congress. But on this occasion, judged by the participants to be an extremely critical turning point and one which would almost certainly lead to an accelerated arms race, no one resigned; no one made a move to public protest.

The much accelerated bomb building project moved ahead, and on November 1,1952 a hydrogen »device« was exploded at the Eniwetok atoll in the Southern Pacific Ocean (a deliverable bomb came in 1955). Its strength, approximately 10.4 megatons, made it equal to almost 1000 Hiroshima-size bombs. The small island at Elugelab was erased from the Pacific charts.

How would the public have responded to the debate had they been allowed to enter it? Some clues are available in polls taken at the time. It is important to remember that the public still had relatively little information about the issues. During January and February 1950, public sentiment favored building the H-bomb 73 % to 18 %, *but* it also favored negotiations for international *control* with the Soviets before building a bomb by 48 % to 45 %. Pessimistically, the public believed such negotiations would probably fail, 70% to 11%. President Truman opposed negotiations on hydrogen weapons with the Soviet Union. By March of the same year after the public learned of the decision to proceed with H-bomb building, support for negotiations for international control increased significantly to 60 % favorable to 23 % opposed. (Failure was still expected by 60% to 17%.) It was this mixed public response that had led several of the members of the GAC to believe that had they not been »gagged,« public opinion could have been turned around to support alternatives to bomb building. On their side, neither President Truman, nor Secretary of State Acheson, nor other members of their administration examined the opportunity of utilizing the strong public support for negotiation to delay or slow down H-bomb construction and to seek agreement or understanding with the Soviet Union to avoid H-bomb building, testing, and deployment. That the arms race was now in full throttle was confirmed by the Soviet's testing of a hydrogen device just two years after the initial U.S. success.

There are a number of subsequent moments in the nuclear arms race when viable alternatives were available that were neither tried nor adopted. Each could have significantly altered the striking upward trajectory of the arms race. Each needs a full examination to pinpoint the mix of factors, forces and circumstances that led to the decisions as they were made and the actual outcomes as they occurred. For example, the decision to limit the scope of the nuclear weapons test ban to atmospheric tests was followed by an accelerated rate of underground bomb testing. Had the goal of a comprehensive test ban been achieved, the arms race might well have been »smothered in the laboratory.« The decision to construct and deploy intercontinental missiles was made prior to significant attempts to ban them through negotiated or other forms of agreement. This move markedly heightened the dangers of nuclear conflict by dramatically shortening flight (and thus warning) times and putting weapons incapable of recall in the arsenals of the two major contenders. Technical, strategic, and industrial interests formed a potent counterforce to any efforts at real control. The technically guided decision to arm intercontinental missiles with multiple warheads (now widely recognized by U.S. strategists to have been a mistake) provided a clear example of the role that institutionalized technical innovation played in driving the arms race. In this case there were highly visible alternatives that were probably fully acceptable to the Soviet Union.

There was one alternative path that was taken after conscious debate involving both experts and an educated segment of the public: the decision to ban anti-ballistic missile systems (ABM's). While this also reflected technical uncertainties, even among ABM advocates, it was probably the political arguments against its potential destabilizing and threat-provoking attributes that led to ABM's rejection. It is just this same system, now dressed up in much fancier technologies, that is re-emerging in President Reagan's »star-wars« proposals for construction of a shield against intercontinental ballistic missiles.

Our analysis here has one important purpose in addition to retelling of incidents in the nuclear arms race. It is to force the search for, and full examination of, alternatives to the threatening addition of new systems in the methods of nuclear warfare. The elements of the history of »star-wars« are already visible: a »technically sweet« problem with a strong group of advocates in the centers and laboratories now committed to nuclear weapons expansion; the political allies who see the technical achievement as a means of gaining an upper hand over the enemy, the Soviet Union; the technical critics, many former insiders, now operating outside the corridors of power who have alternate visions of how to manage the nuclear arms race; the public, partially informed and mightily patronized by the experts on all sides; and a congress with a potentially significant role to play in arms decisions, itself indecisive, still treating nuclear weapons in its own time-honored fashion of political trade-offs. To the extent that in the midst of this welter of interests the historical vision can be made clear and potential alternatives be identified, then the historians' imperative demands corrective action.