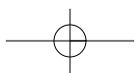


PART I

Successful Odyssey with
the Anti-Ballistic Missile



CHAPTER I

First Thoughts on
Anti-Ballistic Missile Systems

The most important arms control agreement of the Cold War was the Anti-Ballistic Missile (ABM) Treaty of 1972. The titanic battle to create this treaty, which banned anti-ballistic missile systems (except for a single site), took a full decade from conception to ratification and involved an army of arms controllers and government officials working in many stages.

Scouting activities for this army, over the same decade, are described in these first three chapters. The first chapter describes initial activity, in 1962–1964, in which an original approach to dealing with ABM systems (“no-first-procurement”) is conceived. A related paper on ABM limitations of various kinds is presented in 1963 to the Russians through an international conference.

A follow-up paper is deemed sufficiently important that it is sent to the highest level of the Defense Department and presented, by special invitation, at the most important private meeting of U.S. and Soviet scientists being held at the time.

My decade-long odyssey in search of a U.S.-Soviet treaty on anti-ballistic missile systems began in 1963 with an electric thought whose arrival in the attic of my home in Elmsford, New York, I can still vividly recall.

If the Russians—who were then said to be experimenting with a missile defense system around Tallinn in Estonia, then part of the

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USSR—could be persuaded, one way or another, not to build such a system, then America might be persuaded to desist from building its own system. Both sides would then avoid the waste of expensive, ineffective systems that would, still worse, accelerate each side’s interest in buying offsetting offensive missile systems.

The idea appealed to me a great deal more than the work on which I had been engaged. In fact, my arrival at the Hudson Institute, in March 1962, had been traumatic. Its director and founder, the extraordinary Herman Kahn, had welcomed me as follows: “Jeremy, we have a problem with a twelve-month contract. Nine months have passed since we got the contract, and not a word has been set to paper. Now that you are here, we want you to be the one to fulfill it. It does not have to be great, just good.” To my further horror, the subject of the contract, funded by the Defense Department’s Office of Civil Defense (OCD), turned out to be a pet Kahn idea: the strategic evacuation of American cities.

It was a key belief of Herman’s (called “not-incredible first-strike”) that the Soviets should be persuaded that the United States just might launch a nuclear first strike against them if they invaded Western Europe. The strategic evacuation would, he reasoned, give such a threat credibility and commitment by emptying U.S. cities—thus arguably putting American citizens largely out of reach of the immediate effects of Soviet nuclear retaliation.

For a liberal like me, who then opposed even the prudential program of designating fallout shelters, this project was a creation of the devil. I seriously considered returning to the Stanford Research Institute (SRI) and asking for my job back as a research mathematician. I can still see myself with my head in my hands, sitting forlornly on my stoop in Elmsford, considering just that.

Persisting, however, I produced a study in which, on paper at least, the entire northeastern United States would be evacuated by car and rail in three days. Later when I briefed OCD in the Pentagon, its director asked whether I thought this plan would work.

“Thanks so much for asking,” I said. “No, I don’t think it would

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work at all!" I was intensely relieved to realize that OCD was far more pedestrian in its thinking than Herman, and that my study would go nowhere and do no harm.

How had a twenty-six-year-old, freshly minted Ph.D. found himself in this predicament? My college roommate—and the best man at my wedding—the distinguished economist Sidney G. Winter, had been working at RAND, an Air Force think tank set up after World War II. He recommended me to Herman, then planning to leave RAND and start his own institute. Sid then persuaded me to visit RAND and meet Herman, who, Sid said, was an influential person who could be influenced by me and for good purposes.

It was 1961, and storm clouds were forming in the Cold War with a Berlin blockade that led, in due course, to the Cuban missile crisis. Saving the world, even through Herman Kahn, seemed more important than whatever my meager mathematical talents would produce at SRI. I agreed to move as soon as my wife, B.J., finished her own Stanford University Ph.D. in mathematics. My decision was made more difficult by the fact that most of my friends and many relatives considered Herman Kahn an evil person.

Kahn had become famous for three-day lectures on the subject of his tome *On Thermonuclear War*.¹ He, almost alone, considered nuclear war something to which strategy should, and could, be applied. He alone considered it even winnable. Startled liberal commentators asked, "Is there really a Herman Kahn?"

But Herman was, at least, smart. Sid called him the smartest person "to his right" that he had ever met. Herman was said to have received the highest intelligence rating on the Alpha test given to World War II recruits up to the time he took it in 1942. He was very incisive but seemed, up close, much like a super used-car salesman. His friends spoke of the "wit and wisdom of Herman Kahn." Short and very fat, about 350 pounds, Kahn held people to him with a kind of intellectual sex appeal. He was always "on," spouting anecdotes and running through set-piece minilectures.

Believing that, as God had turned mud into men, he could turn

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ordinary mortals into strategists, he felt he did not need, or really want, to hire established experts. He therefore dominated the staff even more than would otherwise have been the case; although he welcomed debate, few could maintain opinions in opposition to him.

Soon after I joined the staff, the issue arose of my getting a security clearance, something that had been pending for four years. In 1958, after our first year of graduate work in mathematics, B.J. and I had been offered summer work at the prestigious RAND Corporation in Santa Monica, California, as consultants. The jobs offered the splendid salary of twenty-five dollars per day for each of us, and RAND had good working conditions. We obtained the necessary "interim" clearances for dealing with secret material that were required to work in the RAND building.

Our work was satisfactory. I had, indeed, published two RAND papers in the summer, one on a new method of doing linear programming and another on economic arbitrage.² We were invited to return for another summer, but on May 12, 1959, we learned that the interim clearances provided by RAND had been withdrawn by the Air Force.

The Air Force Office of Special Investigations (OSI) interviewed me under oath and asked questions about my relationship with my father, I. F. Stone, a well-known journalist. These interviews require vigilance. Sometimes, the all-important printed transcript turned out to be less than faithful to my obvious verbal intention. For example, by sticking a period into an oral statement that needed a comma instead, they changed the entire meaning:

Q. Does your father exert any influence over your thinking or over you?

A. As much as any man can say.*[sic]* My political views are my own.

The hearing, and later information provided me in 1977 under a Freedom of Information Act (FOIA) request, strongly suggested that OSI had no derogatory information whatsoever about me per-

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sonally—I was young and apolitical. But the withdrawn security clearance prevented me from returning to RAND.^[3]

Herman's hiring me and, perhaps, the Hudson Institute's influence reopened the security-clearance matter, and eventually I found myself sitting in the bowels of the Pentagon, armed with a pocket handkerchief, being interrogated by a lieutenant colonel, a captain, and a civilian employee of the Air Force.

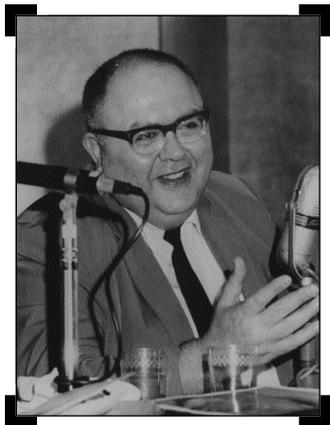
My interrogators were polite, junior, and seemed politically naive. They wanted an explanation of why my father had opposed such things as the House Committee on Un-American Activities and new laws on wiretapping. I mentioned that the White House had recently used my father's credibility as an honest journalist in its White Paper on Cuba by quoting him as saying, after a visit to Cuba, that "Cuba has the smell of an Eastern European satellite state."

In any case, I was cleared two months later and, eventually, cleared for "top secret." (Two years later, after leaving the Hudson Institute, I decided to let these clearances lapse. This would make it easier to travel to the Soviet Union and write freely on arms control because no question would arise of my misusing classified information.)

The most dangerous episode of the Cold War, the Cuban missile crisis, occurred in October 1962, six months after my arrival at the Hudson Institute. To everyone who worked in these nuclear-war think tanks, this incident precisely mirrored in reality the "escalation ladders" and scenarios then in wide use. So when President Kennedy gave his October 22 speech saying: "It shall be the policy of this nation to regard any nuclear missile launched from Cuba against any nation in the Western Hemisphere as an attack by the Soviet Union on the United States, requiring a full retaliatory response upon the Soviet Union," I drove immediately to Princeton, where B.J. was at a conference, to be with her in case the world was in its final moments.^[4]

At about the same time of the Cuban missile crisis, Donald G. Brennan, then an arms controller, became Hudson's president, with Herman remaining the director. Brennan was tall, with a stiff Pruss-

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Herman Kahn, founder and director of the Hudson Institute



Donald G. Brennan, president of the Hudson Institute, and the author (rt.).

ian presence but with an ideology then much more congenial to my own. Herman and Don could not have been a more unlikely couple, not only politically but also physically. Hudson’s symbol at that time could have been that of the 1935 New York World’s Fair: obelisk (Don) and sphere (Herman). They instantly agreed on a Hudson Institute rule that any employees who were heavier than 300 pounds or taller than six feet four inches could travel first class.

Under Herman’s leadership, the Hudson Institute produced voluminous studies that often said little but were saved by some “kicker” that could justify the contract. For example, the Army had asked for a study on the anti-ballistic missile, which it certainly favored. The Hudson study’s main usable conclusion was to invent a “new” argument for the ABM that, indeed, might make the entire study worth the money to the Army.

What was this new argument? Following a nuclear attack, an anti-ballistic missile protecting a steel plant or other key resource could, it was argued in the study, make the difference between successful postwar recovery and disastrous collapse. This was a perfect example of the kind of mind-stretching argumentation, hypothetical and extreme, characteristic of Herman. I complained that I opposed the anti-ballistic missile and was not eager to work on

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studies like the one for the Army. Herman's response: "No problem, we can do a study for the Arms Control and Disarmament Agency" (ACDA, which was unsympathetic to the ABM). To someone running a policy institute and trying to raise money to keep it going, it was all clear. But, for me, the approach seemed too mercantile, although I was happy to do one ACDA study.⁵

After the electric moment in Elmsford, stopping an ABM race became my real preoccupation. I quickly prepared a paper for a conference in Michigan entitled "Should the Soviet Union Build an Anti-Ballistic Missile System?" It argued that the United States would desist from deploying an ABM system if, but only if, the Soviet Union did not move forward with its own ABM system. Donald Brennan's reaction was gratifying. Since he had edited a book then called the "bible" of arms control,⁶ he was well placed to judge how important and original this was or was not.⁷ To my thrilled satisfaction, Brennan tried to bring the idea before the secretary of defense. Now this was what I had come to Hudson for!

In a letter of December 3, 1963, to Adam Yarmolinsky, then "The Special Assistant" to Secretary of Defense McNamara, Brennan said the paper was "important and interesting enough to be brought to the Secretary's attention." The paper argued, he said, "with high persuasiveness" and "careful historical documentation" that the Soviets should not proceed. Did Yarmolinsky want to suggest any changes or to show it to Mr. McNamara before it was communicated to the Russians?

Herman Kahn's reaction was more skeptical. He supported ABM deployment and believed, as many did at that time, that all new technologies were bound to be implemented. Herman sent me to Princeton to talk to the astrophysicist Freeman Dyson, then on the Hudson board.

Hat in hand, I presented myself before the famous Professor Dyson, who proceeded to explain that the Russians liked defense, for historical reasons; why not let them waste their money if they wished? But the problem was, I responded, that the United States

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would feel obliged, politically, to respond with its own ABM. I was startled by the candor and simplicity of Professor Dyson's response: "I never thought of that."

Returning to Hudson, I prepared, by December 12, 1963, a sixty-page paper, "Anti-Ballistic Missiles and Arms Control."^[8] I urged consideration of a "no-first-procurement policy for ABM systems" based on "more-or-less tacit understandings" and encouraged the secretary of defense to testify, in open session, on the likely costs of a Soviet ABM system. Furthermore, he should declassify information in the Department of Defense that bore on the waste and inefficiency of past Soviet efforts to build defenses.

The paper further argued that "certain decoys will easily saturate the Soviet system" and mentioned non-ballistic missiles as a possibility. It also suggested that we should threaten to build an ABM of our own that would "degrade the Soviet offensive missiles or require additional Soviet resources to keep up." (This is, of course, what some of President Reagan's staffers later considered his Star Wars program to be!)

The Pugwash Conference, begun by scientists in 1955 as part of an international peace movement and awarded the Nobel Peace Prize in 1997, had been deemed by Brennan, some years before, too multinational for really private discussions. Instead, he pressed for the establishment of a quiet U.S.-Soviet bilateral Pugwash. It came to be chaired by the Harvard biochemist Paul Doty. In early 1964 Brennan advised me that the Doty group would pay me a thousand dollars (about five thousand in today's dollars) for the use of my paper in a forthcoming meeting in Boston with the Soviets in late spring. Would I agree? Yes! But only if I could present the paper myself. Doty reluctantly agreed. 

About this time, I decided to call the thirty-five-member Hudson staff together and give a lecture entitled "The Incredibility of Not-Incredible First Strike." ("Not-Incredible First Strike" was, as mentioned, Herman's favorite force posture—a credible threat to attack the Soviet Union with strategic weapons in response to some

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provocation in Europe. Recognizing how hard it would be to make this “credible,” he advanced the notion that we should keep it, at least, “not incredible.” I felt this was not possible.)

This talk was an unprecedented success. I had learned the Kahn technique of jokes, slides, and quotations and had prepared carefully. I had answers for all of Herman’s interventions and it was the first time I ever felt I had defeated him in debate. But somehow this success just deepened my impatience with the Hudson Institute and with the entire profession. I resigned,  without a new position, and began writing a book on my experiences, after which I would return to mathematics.

Working at home, I quickly prepared a first chapter on “bomber disarmament,” which I mailed widely to strategists, government officials, and the like.⁹ How pleased I was to get a polite letter from the assistant secretary of defense for systems analysis, Alain Enthoven, even though he noted the enormous political obstacles that bomber disarmament would face. At least someone was listening.

More important, both the Princeton Center for International Studies, led by Klaus Knorr, and the Harvard Center for International Affairs, in the person of Thomas C. Schelling, said I could join for a year as a research associate. I chose Harvard. 

Late in the spring of 1964, the Doty study group met with the Russians. On the American side, Doty was joined by Henry Kissinger, then a Harvard professor and strategist; Marshall Shulman, later the senior adviser on the Soviet Union to Secretary of State Cyrus Vance; and Jerome B. Wiesner, who had been a science adviser to Presidents Kennedy and Johnson.

On the Soviet side there was N. Talensky, a Soviet general (but really a specialist in Soviet military history); M. D. Millionshchikov, an academician and vice president of the Soviet Academy of Sciences; Vasily Emelyanov, a metallurgist who had been a chief designer of tank armor during World War II and, later, the Soviet ambassador to the International Agency for Atomic Energy (IAEA); and I. Sedov,

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reputed to be a major figure in the Soviet military-industrial complex.

On hearing my view that the Soviet Union should not build an ABM, General Talensky said that it constituted an ultimatum and that, if this continued, he would walk out. I was stunned. Marshall Shulman, ever the diplomat, explained that my statement was just a warning (not a threat) that the United States would inevitably take action in the face of a Soviet ABM system.

Undaunted, I approached Millionshchikov after the session and said, "Well, at least one Soviet academician agrees with me; look at this quote from Artsimovich, who says that someday defensive systems might be worse than offensive ones."

Millionshchikov said, "Artsimovich always disagrees with everyone else."

Later Kissinger told the Russians a relevant joke, which they enjoyed hugely: As a Texas sheriff was beating a group of communists, one of them shouted, "Don't beat me, I am *anticommunist*." The Texas sheriff continued to beat him, exclaiming, "I don't care what kind of communist you are."

The next morning, at breakfast, Emelyanov repeated this joke and said to me in friendly fashion, "That is the way you are, Stone, missiles or anti-missiles, you don't care, you beat them all." He had gotten the point. (And he turned out to be a great ally.)

Emelyanov also proved important in another way. He had advised me and my wife that he had learned English in his sixties in the back of a chauffeur-driven car in Moscow and that I could therefore surely learn Russian. I was not about to undertake such an onerous task, but I was not above asking my wife to do it. And this she did, beginning at once with summer school at Harvard in 1964 and at intensive summer school courses in 1965 and 1966 at Wyncam University in Putney, Vermont. In the ensuing five years, B.J.'s competence in Russian became a key element in my efforts to promote an ABM treaty.