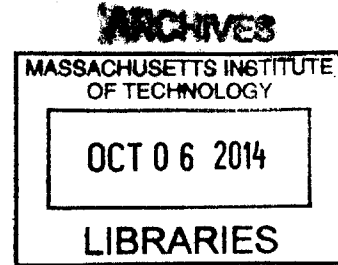


Insiders and Outsiders: Nuclear Arms Control Experts in Cold War America

by

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Submitted to the Program in Science, Technology, and Society in Partial Fulfillment of the Requirements for the Degree of

Doctor of Philosophy in History, Anthropology, and Science, Technology and Society at the Massachusetts Institute of Technology

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Abstract

This dissertation presents a history of the community of nuclear arms control experts in the United States during the middle and later years of the Cold War, the age of thermonuclear ballistic missiles. Arms control experts were, in many interesting ways, both insiders and outsiders to the American “nuclear state.” The dissertation begins by exploring the formation of strategic arms control in the years leading up to 1960, showing how arms control emerged from the mixing of local communities of disarmament advocates and theorists of nuclear deterrence. Rather than inevitable doctrinal unity, early arms control was highly local and contingent. In particular, the crucial concept of “stability” was open to multiple interpretations. In the 1960s, arms control problems motivated groundbreaking scientific research. Elite contract consultants to the government contemplated the use of lasers as weapons against ballistic missiles. As consultants performed calculations and experiments in the context of classified discussions and studies, they founded a new field of physics called nonlinear optics. In the late 1960s, strategic arms control became a public issue during a complex political dispute over missile defense. Arms control experts mediated and fueled this controversy by participating in a surprising range of activity, rallying alongside local residents whose neighborhoods would be impacted by missile defense installations, and criticizing defense policy in Congressional testimony—even as they worked their connections to the White House. In the 1960s and 1970s arms controllers shaped a changing institutional landscape for the support of arms control expertise. They built arms control into a new government agency, and later drew on the resources of philanthropic foundations to create major university arms control centers. By the 1980s, arms control reached peak public visibility amid controversy over the Reagan administration’s Strategic Defense Initiative. This dissertation uses the private papers and correspondence of numerous experts, a wide range of arms control publications, and government records to explore the diverse practices of arms control. It engages a wider discussion among historians about the status of Cold War elites, the relationship between experts and the American state, and the character of scientific knowledge during the Cold War.

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What they say is true: the lonely meeting between the writer and the blank page needn't be so lonely, after all. It is satisfying to reach the end of a long writing project and have the chance to thank those who made it possible.

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In writing this dissertation, I didn't always choose the right arguments or the right words. But I did choose the right dissertation committee. Christopher Capozzola is a wealth of knowledge and insight into the history of American society and politics, a keen guide to the historical profession in general, and a humane soul. Concerning Michael Gordin, I always had the suspicion that he already knew everything contained in my dissertation (and more about my topic besides), but he was just too encouraging, too funny, and too busy writing his own acclaimed books to let on. Thank you, Chris and Michael, for your wise counsel and challenging criticisms and questions.

Many teachers helped me grow as a scholar and navigate what was, for me, a long, difficult, and thrilling journey from life as a physics grad student to life as a historian. At MIT, I extend sincere admiration and thanks to Michael Fischer, Stefan Helmreich, Meg Jacobs, Leo Marx, Anne McCants, Merritt Roe Smith, and Rosalind Williams. They showed me so much—not least what the life of the mind, and the life of a supportive scholarly community, should look like. As an errant physics student at Yale, I was given my first, enticing introduction to the history of science in a reading course directed by Ole Molvig. I was charmed, began packing my bags for the humanities, and Ole is in no small part to blame. An especially encouraging conversation with Daniel Kevles was more than a little help, too. Not long after, Michel Janssen conveyed to me the intellectual excitement of the history of physics, Don Howard did much the same (with a little philosophy added), and I set sail. I am pleased to count these amazing scholars now as colleagues. At the University of Toronto I worked under the skillful supervision of John Sipe, an exceptional physicist and teacher, who introduced me to nonlinear optics. Master John Fraser at the University of Toronto's Massey College was a learned and kind shepherd to the flock of junior fellows, and he indulged my love of classical music. At the University of Saskatchewan, Rainer Dick and Tom Steele were the first to lift my nose out of my textbooks on circuits and control systems and show me the many-colored beauty of theoretical physics.

For countless laughs and great conversations, I'd like to thank my fellow students in the HASTS doctoral program. What good luck to have been able to steer through the program alongside a convivial, collegial group of people. It's a pleasure to thank my friends and cohort-mates Nate

Deshmukh Towery, Teasel Muir-Harmony, Ellan Spero, and Michaela Thompson. For making HASTS a stimulating and supportive place to spend six years of one's life, among students past and present I thank Marie Burks, Kieran Downes, Amah Edoh, Xaq Frohlich, Chihyung Jeon, Shreeharsh Kelkar, Lisa Messeri, Lucas Müller, Canay Özden-Schilling, Tom Özden-Schilling, Rebecca Perry, Sophia Roosth, Michael Rossi, Ryan Shapiro, Shira Shmueli, David Singerman, Alma Steingart, Emily Wanderer, and Rebecca Woods. My Harvard co-laborers in the field of nuclear history were Dan Volmar and Alex Wellerstein, from whom I continue to learn constantly, and who've become good friends.

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I presented a version of Chapter One in the CISAC Social Science Seminar, for which Chip Blacker gave helpful and astute comments. Material for Chapter Two was first presented at the 2011 History of Science Society meeting in a panel organized by Joe Martin, work on which I've since received (and still haven't lived up to) excellent feedback from Michel Janssen. I presented a later version at Leiden University's Lorentz Center in 2013, and I am grateful to Jeroen van Dongen for the invitation. An early draft of Chapter Five benefited from the wisdom of members of the 2011–12 seminar at Harvard's Charles Warren Center for Studies in American History, especially Andy Jewett, Julie Reuben, Mark Solovey, and Jessica Wang. Rebecca Slayton generously shared archival materials that I drew on in writing the draft. Long ago, my first proposal for this project was improved by the feedback of the Phunday gang, overseen by the presidium of Peter Galison, Michael Gordin, and Dave Kaiser. I received a boost of confidence presenting an overview of my project at the 2013 "Nuclear Boot Camp," put on by the Wilson Center's Nuclear Proliferation International History Project, where Leopoldo Nuti, Christian Ostermann, David Holloway, Joseph Pilat, and Martin Sherwin had words of insight and encouragement.

Friends near and far kept me grounded when I floated away into my thoughts, lifted me up when I was discouraged, and reminded me that there is a wide world beyond my desk and keyboard. In the far-off days of undergrad, Mike Barnett, Nate Olfert, and Matthew Townley-Smith were my buddies and my teammates on an intramural curling team. For more recent incarnations of generous friendship, deepest thanks to Heather Blank, Helen Kongsgaard, and Carl and Jamie Schwendinger-Schreck. Jon Lee and I first compared notes in a high school calculus class; then he went a little further in mathematics, getting his Ph.D. at Stanford. It's been great seeing more of him again this past year.

It comes as a shock to realize that it has been a decade since Nathan Ballantyne and I met, at Massey College in Toronto, and began talking about everything together. Nathan is a model scholar, teacher, and writer, a master of philosophical distinctions, and a fine editor of prose. But above all he is a top-shelf person, wonderfully funny, a dear friend, and one of the few people with whom I can reminisce about the music of Glenn Gould and the unstoppable lineup of the

1992 Toronto Blue Jays. He has been my guide in many things, academic and non-. Knowing him, and his wife Jenna, and now their little daughter Clara, has been one of the greatest gifts the last ten years have given me.

I am blessed with a large family, based for the most part in the near-mythical Canadian province of Saskatchewan. These people remind me where I come from, wherever I am. I will forever be thankful for the bottomless generosity and love of my grandmother, Gretchen Roesler. I never had the chance to meet Audrey Wilson, but her good influence has reached me in other ways. My grandfathers, William Roesler and Oren Wilson, would have been proud to see me complete this dissertation, and I would be proud to show it to them. Within the Wilson household, I had the undeserved good fortune to grow up with a kind, funny, smart, supportive sister, Toby. She put up with me when we were kids and she puts up with me still. She has decided to marry a terrific young chap named Lance, and I'll be delighted to have him as a brother-in-law.

When it comes to my parents, Robert and Rebecca Wilson, words fail. I'm not sure how to express my gratitude for all they have given me, for making me who I am. I begin by dedicating this dissertation to them.

Abbreviations

Archival Collections:

- APS-DEW* American Physical Society Records of the Directed Energy Weapons Study, 1983–1988, Niels Bohr Library and Archives, American Institute of Physics, College Park, MD.
- BBS* Brian B. Schwartz Papers (AR 1999–5), American Institute of Physics, Niels Bohr Library and Archives, College Park, MD.
- BTF* Bernard Taub Feld Papers (MC167), Institute Archives and Special Collections, Massachusetts Institute of Technology, Cambridge, MA.
- CHT* Charles H. Townes Papers (unprocessed collection, MSS 84413), Manuscript Division, Library of Congress, Washington, DC.
- DNSA* Digital National Security Archive, <http://nsarchive.chadwyck.com>.
- FFG* Ford Foundation Grants, Ford Foundation Archives, The Rockefeller Archive Center, Sleepy Hollow, NY.
- FFR* Ford Foundation Reports, Ford Foundation Archives, The Rockefeller Archive Center, Sleepy Hollow, NY.
- HAB* Hans Bethe Papers (14–22–976), Division of Rare and Manuscript Collections, Cornell University Library, Ithaca, NY.
- HFY* Herbert F. York Papers 1958-1999 (MSS 107), Mandeville Special Collections Library, University of California, San Diego, CA.
- HSP* Herbert Scoville, Jr. Papers (MC224), Institute Archives and Special Collections, Massachusetts Institute of Technology, Cambridge, MA.
- IIR* I.I. Rabi Papers, 1899–1989 (MSS76467), Manuscript Division, Library of Congress, Washington, DC.
- JBW* Jerome B. Wiesner Papers (MC420), Institute Archives and Special Collections, Massachusetts Institute of Technology, Cambridge, MA.
- JES* International Affairs, Office Files of Joseph E. Slater, 1957–1967 (FA619), Ford Foundation Archives, The Rockefeller Archive Center, Sleepy Hollow, NY.
- JFK-MGB* McGeorge Bundy Personal Papers, John F. Kennedy Presidential Library and Museum, Boston, MA.

- JFK-NSF* National Security Files, John F. Kennedy Presidential Library and Museum, Boston, MA.
- JFK-POF* President's Office Files, John F. Kennedy Presidential Library and Museum, Boston, MA.
- JFK-PPP* Pre-Presidential Papers, John F. Kennedy Presidential Library and Museum, Boston, MA.
- KAB* Keith A. Brueckner Papers, 1949–1983 (MSS 94), Mandeville Special Collections Library, University of California, San Diego, CA.
- KTP* Kosta Tsipis Papers (MC527), Institute Archives and Special Collections, Massachusetts Institute of Technology, Cambridge, MA.
- LPB* Lincoln P. Bloomfield Papers (MC326), Institute Archives and Special Collections, Massachusetts Institute of Technology, Cambridge, MA.
- MGM* Murray Gell-Mann Papers, 1931–2001 (10219–MS), Caltech Archives, California Institute of Technology, Pasadena, CA.
- MIT-CIS* Records of the Massachusetts Institute of Technology Center for International Studies, (AC236), Institute Archives and Special Collections, Massachusetts Institute of Technology, Cambridge, MA.
- NB* Nicolaas Bloembergen Personal Archive, 1940–2000 (unprocessed collection, Accession 12840), Harvard University Archives, Harvard University, Cambridge, MA.
- PHN* Paul H. Nitze Papers (MSS80281), Manuscript Division, Library of Congress, Washington, DC.
- RCP* Records of the Carnegie Program and the California Seminar on Arms Control and Foreign Policy (10065–MS), Caltech Archives, California Institute of Technology, Pasadena, CA.
- RF* Rockefeller Foundation Archives, 1912–2000, The Rockefeller Archive Center, Sleepy Hollow, NY.
- RFB* Robert F. Bacher Papers, 1924–1994 (10105–MS), California Institute of Technology Archives, Pasadena, CA.
- RLG* Finn Aaserud Collection on Richard Garwin (AR 2008–866), American Institute of Physics, Niels Bohr Library and Archives, College Park, MD.

Other Abbreviations:

<i>ABM</i>	Antiballistic Missile
<i>ACDA</i>	Arms Control and Disarmament Agency
<i>APS-DEW</i>	American Physical Society Study Group on the Science and Technology of Directed Energy Weapons
<i>ARPA</i>	Advanced Research Projects Agency
<i>DOD</i>	Department of Defense
<i>IDA</i>	Institute for Defense Analyses
<i>MIRV</i>	Multiple Independently-targeted Reentry Vehicle
<i>PRL</i>	<i>Physical Review Letters</i>
<i>PSAC</i>	President's Science Advisory Committee
<i>SALT</i>	Strategic Arms Limitation Talks
<i>SDI</i>	Strategic Defense Initiative

INTRODUCTION

The Arms Controllers

Cocktails were served at 6:15pm sharp, followed by dinner. They'd assembled in the Harvard faculty club, sixteen of them in total. Fourteen were regulars, mostly professors at Harvard and MIT. A couple of guests were present; one was an analyst from the RAND Corporation, an expert in Soviet politics. Ordinarily discussion would have started at 7:30. The plan for the evening had been to talk about a paper by Fred Iklé, a political scientist at MIT. He'd written it on a consulting contract with the Defense Department, but wanted to run the results by his colleagues. Their discussions were always bracing and thorough. No doubt "Alternative Approaches to the International Organization of Disarmament" would have engaged the Harvard-MIT Arms Control Seminar in heated conversation late into the crisp autumn night.

But it had just been announced that the President was to make a special address to the nation. The papers that morning, a Monday in mid-October 1962, described an "air of crisis" in Washington. Secretary of State Dean Rusk had apparently stayed at his office all weekend, working unusually long hours. Defense Secretary Robert McNamara was crashing at the Pentagon. President Kennedy had come back to town early from a campaign trip, his handlers telling the press he was suffering from a cold. Yet he'd "looked fit" attending church the previous day, according to the *New York Times*. The country stood by for his words. At the Harvard faculty club, the arms control seminar members agreed, unanimously, that Iklé's paper

could wait. A television set was wheeled into the room. At 7:00 they turned it on and, like everyone else, watched.¹

Kennedy's black and white image came crackling over the airwaves. His message concerned Soviet missiles in Cuba. U.S. surveillance had spotted medium-range nuclear ballistic missiles capable of hitting Washington, and intermediate-range missiles that could reach as far as Hudson Bay. Kennedy sounded grim and worried, but determined. "This nation is opposed to war," he said. "We are also true to our word." The U.S. government would not tolerate Soviet missiles in the Western Hemisphere. He would blockade Cuba with U.S. naval forces, and stop more missiles from going ashore. He called it a "quarantine," rather than a blockade: it sounded a little less bellicose.²

The arms control seminar participants took it all in. First to speak was Thomas Schelling of Harvard's Center for International Affairs, the seminar's unofficial captain. "The most important aspect of the crisis is that it is a direct confrontation between the U.S. and the Soviet Union," he said. It was a contest of wills at nuclear gunpoint. Schelling, who had been working out theories of nuclear coercion and bargaining since the late 1950s, was riveted by the severity of the showdown. Roger Fisher, a scholar of international law at Harvard Law School, spoke second, his voice carrying a less confident timbre. "One needs to know what can be done in such a confrontation that will not lead to general war," where general war meant thousands of nuclear

Notes

¹ "Top aides confer: U.S. forces maneuver off Puerto Rico—link is denied," *New York Times* (22 October 1962): 1.

² "Text of Kennedy's address on moves to meet the Soviet build-up in Cuba," *New York Times* (23 October 1962): 18.

warheads falling like rain. “How do you have a *controlled* confrontation of this kind? The measures used must be no more than what is needed for the specific purpose.”³

This was no club of idle speculators gathered at Harvard; to call them well informed and well connected would put it much too mildly. On an ordinary night the attendance sheet would have listed several elite physicists involved in extensive contract consulting on highly classified defense matters for the government. Charles Townes, Murray Gell-Mann (based at the California Institute of Technology, but a frequent visitor to the East Coast) and, in later years, Steven Weinberg all participated in the seminar. But even of the sixteen in the room on that October night, probably half a dozen held high-level security clearances from the U.S. government. Five had served, or would later serve, in official government positions. All knew friends and associates involved in similar work.

One of their old MIT colleagues was Jerome Wiesner, now working in the Kennedy administration as the President’s science advisor. Recently departed from Harvard was McGeorge Bundy, who could have been spotted occasionally in the arms control seminar during its first semester in the autumn of 1960. In 1961 Bundy had become Kennedy’s national security advisor; and during those tense days, he was huddled with Kennedy in the “Executive Committee,” the special, super-secret team of advisors helping the President consider options in Cuba. Schelling could have had a post somewhere in the Kennedy administration if he had wanted it. Not ten days before (just four days before the start of the crisis), he’d delivered a top-secret report on “strategic developments over the next decade” to Assistant Secretary of Defense Paul Nitze. The report mused on the “prospects for deterrence” in the 1960s, the effects of new strategic technologies on the horizon, and related matters. Schelling was to have discussed it over

³ “Joint Arms Control Seminar, Minutes of the Second Session, October 22, 1962,” Box 3, Folder 3 “Joint Harvard-MIT Arms Control Seminar 1962–63 Minutes,” *LPB*.

a two-day meeting with various top officials at Camp David beginning October 19th. More pressing matters had intervened.⁴

They were, all things considered, remarkably calm for a group of people who'd just heard the speech that made the Cuban Missile Crisis public, now an iconic moment of the Cold War. But there was much that these insiders—for the time being, outside the closed loop of emergency advising and decision-making—did not know. They had difficulty accepting that there could really be Soviet nuclear warheads in Cuba, even if the reconnaissance had revealed the missiles being stored and built on the island. Schelling could not believe that the Soviets “had the intention of putting nuclear warheads on the missiles in Cuba,” he said. “The intention might have been to provoke us,” offered Nathan Leites, the interloper from RAND. “Perhaps,” replied Schelling, “but it does seem foolish.” One member even floated the remarkable suggestion that the missiles might, in fact, be decoys.⁵

Kennedy's closest advisors certainly believed no such thing. They had no direct photographic evidence of Soviet warheads—only the missiles—but they were scared enough to prepare, from the start of the crisis a week earlier, to launch an airstrike on the installations. The day before the speech, Kennedy had met in the Oval Office with McNamara, CIA director John McCone, and high-level military officials, who told him how an airstrike would work. Kennedy ordered them to be ready to launch the next morning, “or any time thereafter during the remainder of the week.” Timed to begin at the start of the television address the following day, the President ordered global U.S. nuclear forces to “Defense Condition 3” (or “DEFCON 3”), a

⁴ “Strategic Developments Over the Next Decade,” Draft, 12 October 1962; and W.W. Rostow to Carl Kaysen, 2 October 1962, both in Box 376, Folder “Nuclear Energy Matters, Inter-Agency and Schelling Study Group Report, 10/62,” *JFK-NSF*. Nitze had offered Schelling a job as Deputy Assistant Secretary of Defense, but Schelling preferred to remain a consultant. See Fred Kaplan, *The Wizards of Armageddon* (Stanford, CA: Stanford University Press, 1991 [1983]), 332–333.

⁵ “Joint Arms Control Seminar, Minutes of the Second Session, October 22, 1962,” Box 3, Folder 3 “Joint Harvard-MIT Arms Control Seminar 1962–63 Minutes,” *LPB*.

state of high readiness. Two days later the Strategic Air Command, the backbone of the American nuclear striking force, would reach DEFCON 2, the highest alert level it would ever attain, one notch below preparations for imminent nuclear war.⁶

No one at the seminar on October 22nd had concluded that at that moment, they were on the cusp of the riskiest episode of the superpower nuclear competition, arguably the closest the Cold War would ever come to the abyss.⁷ That night only Robert Bowie, the venerable founder and director of Harvard's Center for International Affairs, guessed correctly that the Soviets had put missiles on the Caribbean island, so close to American shores, because the U.S. had already done something similar in Europe, placing its Jupiter missiles in Italy and Turkey. The Soviet missiles had gone in, he said, to "dramatize the fact that nuclear war is real." Neither he, nor the seminar, would have known a few days later when it was precisely this issue—withdrawal of the U.S. Jupiter missiles—that would break the deadlock in secret negotiations between Kennedy's counselors and Soviet diplomats.⁸

So knowledgeable and so connected, and yet so in the dark in that dark hour. Who were these people? These thinkers and doers, scientists and analysts, physicists and statesmen, agitators and stewards of the status quo. I find them fascinating. They were a small group of unusually knowledgeable Americans who were exercised endlessly by the question of what to do about nuclear weapons: to manage their risks and, maybe, one day, to get rid of them. Not just

⁶ Robert McNamara, "Notes on October 21, 1962 Meeting With the President," Document CC00738, *DNSA*; USAF Historical Division Liaison Office, "The Air Force Response to the Cuban Crisis," January 1963, Document CC02811, *DNSA*.

⁷ This in spite of Arthur Schlesinger, Jr.'s later praise for JFK's "combination of toughness and restraint, of will, nerve and wisdom, so brilliantly controlled, so matchlessly calibrated..." Schlesinger quoted in Michael Dobbs, *One Minute to Midnight: Kennedy, Khrushchev, and Castro on the Brink of Nuclear War* (New York: Alfred A. Knopf, 2008), 342. Schlesinger, Kennedy's aide and speechwriter, was not part of the Executive Committee. But his reflections accord well with those of Robert McNamara, who said that the Cuban Missile Crisis was "the most dangerous period in my seven years as Secretary of Defense, but I think it was also the most expertly handled." See WGBH Media Library & Archives, "Interview with Robert McNamara, 1986 [1]," 20 February 1986, available online at <http://openvault.wgbh.org/catalog/wpna-27c3ba-interview-with-robert-mcnamara-1986-1>.

⁸ "Joint Arms Control Seminar, Minutes of the Second Session, October 22, 1962," Box 3, Folder 3 "Joint Harvard-MIT Arms Control Seminar 1962-63 Minutes," *LPB*.

the people gathered in Cambridge at that particular seminar, but also those who ordinarily would have joined them, and those of a similar cast of mind in other places around the country. Theirs was the community of experts in Cold War America that argued and thought and wrote with the most sustained intensity about the most dangerous technology. They were the nuclear arms controllers.

This dissertation explores the history of the nuclear arms control community in the United States. It is concerned with the ideas, debates, and political career of this community as it lived through the middle-to-late years of the Cold War. The dissertation asks about the diverse meanings and dimensions of arms control: as a domain of knowledge and object of expertise, a motivator of research in other fields, a subject of high-level policy, and an issue for public politics and debate. Here, arms control is framed as a social web of experts, the field they explored, and the political activities they undertook. These experts were concerned, sometimes coolly, sometimes to the point of anguish, to put nuclear weapons and the risk of nuclear war under control. Control could mean different things, and the arms control community was not free of its internal divisions, sharp disagreements, contradictions, and flaws. But as a community, coalescing in the years after 1960, the arms controllers played an important role in the science, technology, and politics of America's nuclear age.

The project seeks to shift the terms of a longstanding discussion among historians about the character of Cold War political and intellectual life. Many of the arms control experts studied here migrated across the categories that have organized our thinking about this period. Open and secret. Pure and applied. Public and private. Assent and dissent. The state and civil society. The arms controllers flowed between these headings in a way that asks us to revise our understanding of how such categories should be applied to the modern United States. As elite experts, they

were neither the state's servants nor its masters. They opened a middle space between the cloistered realms of defense and foreign policy and the forums of public politics. They were both insiders and outsiders. In the 1960s and 1970s, the arms controllers navigated and actively shaped a changing institutional landscape for arms control, building arms control into the government and, when government support receded, drawing on the resources of philanthropic foundations. By creating the field of nuclear arms control, pushing the nuclear question more and more into public view, and crafting a new institutional structure for nuclear expertise, they changed America's relationship with its most powerful weapons. But their community, and their experiences and work, will not resolve into the primary colors of our received picture of Cold War America.

Expertise and Community

In 1962 the United States had more than 26,000 nuclear warheads in its possession, just a few thousand short of the peak, reached in 1967. Most of these weapons were many times more powerful than the two that devastated Japanese cities in 1945. The first Minuteman intercontinental ballistic missile went on alert in October 1962, the month of the Cuban Missile Crisis. Within five years there would be 1,000 of these remarkable machines in silos across the country. Hundreds of bomber planes, and dozens of submarines holding sixteen missiles each, could deliver thousands more weapons to their targets around the globe. In the early 1960s, the U.S. still had a vast "superiority" in nuclear weapons over the Soviet Union, by a ratio of perhaps twenty to one. But everyone knew that the Soviets still had enough to threaten American cities. They certainly had enough to prey on the minds of U.S. decision-makers; and they were building more with great haste. By the time of the Cuban crisis, Robert McNamara had concluded that the

notion of superiority had become meaningless in the age of thermonuclear missiles. And yet the weapons were still there, ready to fire, multiplying in numbers and growing in sophistication on each side, year by year. As Cuba had shown, the nuclear situation was brittle—even a small disturbance could set off an international crisis. How many more crises before a nuclear war?⁹

We might say, with Hugh Gusterson, that modern nuclear weapons had come to represent “not so much a problem with a solution as a predicament.”¹⁰ In the late 1950s this predicament seemed to demand a specialized response. Cold War America harbored thinkers who were eager to apply rational knowledge to the nuclear condition. Many of them came to see such efforts as marking off a new domain of thought and policy, known as nuclear arms control. Before roughly 1960, there was no “arms control.” “Disarmament” was the more common term, and goal. There was an occasional mention of the words “arms control,” but never in their post-1960, “strategic” sense. In the years leading up to 1960, “arms control” became an idiom, a distinct approach to nuclear questions.¹¹ A group of people, mostly academics, collected around this new way of seeing matters nuclear. They supposed that a new “field” had been founded, its enticing potential untapped. The founders gathered in conferences and studies—the first in the summer of 1960, in

⁹ Such was this condition, often referred to as a “stalemate,” that the basing of the weapons no longer affected their strategic implications, in a strict sense: they could reach anything from anywhere. In an early meeting of the Executive Committee, as the group debated the strategic impact of the Soviet missiles so close to the North American continent, McNamara (now famously) remarked that “a missile is still a missile. It makes no great difference whether you are killed by a missile fired from the Soviet Union or from Cuba.” McNamara quoted in Barton J. Bernstein, “The week we almost went to war,” *Bulletin of the Atomic Scientists* 32, no. 2 (1976): 12–21, on 16. Information on the U.S. nuclear arsenal is found in Thomas B. Cochran, William M. Arkin, and Milton M. Hoenig, *Nuclear Weapons Databook, Volume I: U.S. Nuclear Forces and Capabilities* (Cambridge, MA: Ballinger Publishing Company, 1984), 15 and 118. A classic historical account of the relationship between nuclear planning/targeting policies and the size of the U.S. arsenal is David Alan Rosenberg, “The origins of overkill: Nuclear weapons and American strategy, 1945-1960,” *International Security* 7, no. 4 (1983): 3–71.

¹⁰ Hugh Gusterson, *Nuclear Rites: A Weapons Laboratory at the End of the Cold War* (Berkeley, CA: University of California Press, 1996), 4.

¹¹ In this dissertation I occasionally refer to arms control as “strategic arms control,” to get at that specific connotation. Of course many commentators and sometimes the arms controllers themselves often meant “disarmament” when they said “arms control,” or meant disarmament to be included as part of arms control. The point is that arms control, as a piece of terminology and a way of approaching nuclear issues, is peculiar to the period after about 1960.

Boston—and wrote reports and papers probing the meaning of arms control. They came from all corners of the academy, across the disciplines. Large parts of their time were spent in Washington, DC, where they advised and consulted. Arms control was said to be their field of expertise.

This dissertation concerns itself with the relationship between experts and the state. For the social theorist Max Weber, the “professional expert” was a type endemic to modern states and societies. The expert fit the modern bureaucratic state hand-in-glove; expertise was both the state’s product and its nourishment. Expertise has had an important place in the history of modern America, whose last century featured extraordinary blooms of administrative capacity and state power. Experts have been studied as informers and explainers, managers and administrators, planners and fixers, experimenters and calculators—solvers, and creators, of social problems. On the social and political map of the United States, experts have been pinpointed at the borderline of the professions and the state. Experts were crucial participants in the co-creation of each.¹²

What role did the experts have in modern America? What agency, what authority? For the sociologist C. Wright Mills, the answers to these questions were complicated. When he looked out at the midcentury vista, he saw institutions. Large, powerful, expanding institutions reached into every crease and crevice of social and political experience; they were comprised chiefly of the corporations, the military, and the government. The magnetic field lines of the

¹² Max Weber, *Economy and Society: An Outline of Interpretive Sociology*, eds. Guenther Roth and Claus Wittich (Berkeley, CA: University of California Press, 1968 [1922]), 998. Some prominent examples of scholarship on expertise in U.S. history would include Samuel P. Hays, *Conservation and the Gospel of Efficiency: The Progressive Conservation Movement, 1890–1920* (Cambridge, MA: Harvard University Press, 1959); Daniel Kevles, *The Physicists: The History of a Scientific Community in Modern America* (Cambridge, MA: Harvard University Press, 1995 [1978]); Brian Balogh, *Chain Reaction: Expert Debate and Public Participation in American Commercial Nuclear Power, 1945–1975* (New York: Cambridge University Press, 1991); Theodore M. Porter, *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life* (Princeton, NJ: Princeton University Press, 1995); David C. Engerman, *Know Your Enemy: The Rise and Fall of America’s Soviet Experts* (New York: Oxford University Press, 2009).

whole society bent around them, and the experts felt their tug more strongly than most. When it came to war, the kind of expertise the state thirsted after was technical in character. “Since World War II,” Mills wrote, “the general direction of pure scientific research has been set by military considerations, its major finances are from military funds, and very few of those engaged in basic scientific research are not working under military direction.” The Manhattan Project veteran Ralph Lapp echoed Mills in 1962. The scientists “are merely the servants,” he had decided, “most of them unwillingly, of the arms race generated by the Cold War, the terrifying new weapons, and the demands of national defense. The weapons and military technology are now so complex that they call for enlistment of the major part of a nation’s scientific and engineering manpower.”¹³

But Mills was more delicately attuned to stratification. He could see a special caste, an elite, among those fleets of scientists and technicians (he might have counted Lapp one of its members). “It is these senior circles that have become deeply involved in the politics of military decisions, and the militarization of political life,” Mills wrote. “In the last fifteen years,” Mills observed from the vantage point of 1956, “they have moved into the vacuum of theoretical military studies, in which strategy and policy become virtually one.” The experts had become advisors; the advisors strategists. Public policy had become military policy, secretive and opaque. The postwar era, he said, had seen the disappearance of any “free and wide debate of military policy or of policies of military relevance.”¹⁴

“‘Democracy’ takes an ambivalent attitude also towards the system of...expertise,” wrote Max Weber, long before the day of nuclear consultants and strategists—“as it does towards all the phenomena of the bureaucratization which, nevertheless, it promotes.” That was a paradox.

¹³ C. Wright Mills, *The Power Elite* (New York: Oxford University Press, 1956), 216. Ralph E. Lapp, *Kill and Overkill: The Strategy of Annihilation* (New York: Basic Books, 1962), 16.

¹⁴ Mills, *The Power Elite*, on 217–218 and 220, respectively.

The relationship between modern America's democracy and its experts has always been always a tense one, and during the Cold War this called forth an acute anxiety. Dwight Eisenhower famously warned of the power of a "military-industrial complex" in his 1961 farewell address. He added a cautionary note about the experts, the wielders and shapers of this power. Science and technology were transforming American life, he said. "Yet in holding scientific research and discovery in respect, as we should, we must also be alert to the equal and opposite danger that public policy could itself become the captive of a scientific-technological elite."¹⁵

The arms controllers lived and breathed this anxiety. Herbert York was a nuclear physicist, a defense technocrat and a longtime arms control advocate—a curious and not uncommon combination of traits for a first generation arms controller. In 1970 his government service was (for the most part) behind him. The dust was still settling from a tremendous political struggle the previous year. It had been over a key arms control issue: whether to build a massive defensive system in the United States that would protect against ballistic missile attack. York and the arms controllers didn't want it; they believed it was hopeless, for technical reasons, and could only weaken nuclear deterrence and worsen the nuclear arms race. He returned to his desk at the University of California, San Diego believing that the struggle had been lost. The system would be built. He wrote that year about how the complexity of new nuclear weapons systems "is creating new and serious problems of the general kind that Eisenhower warned us about." It wasn't just the warheads but all the terrible embroidery around them—the missiles, the means of basing and launching them, of making them more lethal, of defending against them. To allow these technologies to twist from the grasp of reasoned political choice "would be to accept an absurdity: the transfer of control over our destinies from ourselves and the statesmen and

¹⁵ Weber, *Economy and Society*, 999; Dwight D. Eisenhower, Farewell Address (January 17, 1961), The Miller Center, available online at <http://millercenter.org/president/speeches/speech-3361>.

politicians we select into the eager hands of strategic analysts, technologists, and other experts.”¹⁶

Physicists, like York, had always had a special, fraught relationship with nuclear weapons. Perhaps no life illustrated more starkly the tensions between the authority and subservience of expertise in postwar America than Robert Oppenheimer’s. As head of the Los Alamos bomb-design efforts of the Manhattan Project, he was elite expertise personified. As a postwar ally of the atomic scientists’ movement for civilian and international control of atomic energy, and as a high-level technical advisor to the Atomic Energy Commission, he bore witness to the compromises of the nuclear age. “But as the scientists’ movement atrophied,” writes Charles Thorpe, and “as the atomic scientists increasingly divided into those thoroughly incorporated into technical and policy functions within the state and those who remained outsiders,” Oppenheimer lost his autonomy. He made errors, advocated policies distasteful to the hierarchy. He was, Thorpe argues, finally purged by the state. Dramatized by a humiliating interrogation in 1954 and the loss of his security clearance, he was “subject to the institutional constraints of bureaucratic office and to the state’s disciplinary apparatus.”¹⁷

¹⁶ Herbert York, *Race to Oblivion: A Participant’s View of the Arms Race* (New York: Simon and Schuster, 1970), 13. Also see Garry Wills, *Bomb Power: The Modern Presidency and the National Security State* (New York: Penguin Press, 2010), and Elaine Scarry, *Thermonuclear Monarchy: Choosing Between Democracy and Doom* (New York: W.W. Norton, 2014), more recent explorations of the idea that nuclear weapons, the executive powers reserved to manage them, and the secretive, expert bureaucracies that have grown around them, have represented a basic corruption of democratic ideals.

¹⁷ Charles Thorpe, *Oppenheimer: The Tragic Intellect* (Chicago: University of Chicago Press, 2006), 201. Also see Charles Thorpe, “Disciplining experts: Scientific authority and liberal democracy in the Oppenheimer case,” *Social Studies of Science* 32, no. 4 (2002): 525–562. Oppenheimer’s security hearing “exposed dilemmas and tensions endemic to the role of expert authority in liberal democratic politics,” writes Thorpe. “It was an event which crystallized tensions between competing understandings of the legitimate place of scientists and scientific expertise in the operations of the state and in civil society” (on 528). On the political experiences of the postwar atomic scientists’ movement, see Jessica Wang, *American Science in an Age of Anxiety: Scientists, Anticommunism, and the Cold War* (Chapel Hill, NC: University of North Carolina Press, 1999). Wang studies a slightly earlier period of government-sanctioned anticommunism in the late 1940s, but concludes, in a manner similar to Thorpe, that experts could either accommodate themselves to the state’s demands or face censure and banishment. “The very structure of modern bureaucratic government,” she writes, “combined with the specific political conditions of postwar America, restrained scientists’ inclination to challenge the dominant order.... The political culture of Cold War liberalism celebrated the potential of the bureaucratic state, privileging procedural reform over principle, and

The relationship between knowledge and power in the Cold War United States has inspired a deep and varied literature in the last few decades.¹⁸ A related subset of this literature has lavished special attention on scientists in their roles as technical experts. It is a fascinating literature. It is also a literature rendered in sharp contrasts. The contrast is between that which is inside, and that which is outside—that which is of the state, and that which is independent of (or opposed to) the state. When scholars have fixed their sights on the Cold War expert, they have seen two kinds of political actor. There are experts who have been drafted by the state, even created by it; and those who have stood outside, or been alienated from, the state's goals, projects, and resources. Some, like Oppenheimer, were in until they were out. This dichotomy embraces related ones: the distinction between secret and open, between public and private.¹⁹

process over fundamental questions of value" (on 8). It is important to note that each of these studies, deeply preoccupied with the power of the state, trains its gaze on the 1940s and 1950s, the heyday of anticommunist fervor in the U.S. In a later period, as this dissertation shows, the relationship between technical experts and the American state was far less rigid, far more porous and fluid.

¹⁸ This literature eludes easy summary. Among the most prominent studies are Paul Forman, "Behind quantum electronics: National security as basis for physical research in the United States, 1940–1960," *Historical Studies in the Physical and Biological Sciences* 18 (1987): 149–229; Peter Galison, "Physics between war and peace," in Everett Mendelsohn, Merritt Roe Smith and Peter Weingart, eds., *Science, Technology, and the Military* (Boston: Kluwer Academic Publishers, 1988); Daniel J. Kevles, "Cold war and hot physics: Science, security, and the American State, 1945–56," *Historical Studies in the Physical and Biological Sciences* 20 (1990): 239–264; Peter Galison and Bruce W. Hevly, eds., *Big Science: The Growth of Large Scale Research* (Stanford, CA: Stanford University Press, 1993); Stuart W. Leslie, *The Cold War and American Science: The Military-Industrial-Academic Complex at MIT and Stanford* (New York: Columbia University Press, 1993); Peter Galison, *Image and Logic: A Material History of Microphysics* (Chicago: University of Chicago Press, 1997), esp. Chapter 4, "Laboratory War: Radar Philosophy and the Los Alamos Man," 239–312; David Kaiser, "Cold War requisitions, scientific manpower, and the production of American physicists after World War II," *Historical Studies in the Physical and Biological Sciences* 33 (2002): 131–159; Philip Mirowski, *Machine Dreams: Economics Becomes a Cyborg Science* (New York: Cambridge University Press, 2002); Naomi Oreskes, "A context of motivation: U.S. Navy oceanographic research and the discovery of sea-floor hydrothermal vents," *Social Studies of Science* 33, no. 5 (2003): 697–742; Jacob Darwin Hamblin, *Oceanographers and the Cold War: Disciples of Marine Science* (Seattle: University of Washington Press, 2005); Steven Shapin, *The Scientific Life: A Moral History of a Late Modern Vocation* (Chicago: University of Chicago Press, 2008); Angela N.H. Creager, *Life Atomic: A History of Radioisotopes in Science and Medicine* (Chicago: University of Chicago Press, 2013); Mark Solovey, *Shaky Foundations: The Politics-Patronage-Social Science Nexus in Cold War America* (New Brunswick, NJ: Rutgers University Press, 2013); David Kaiser, *American Physics and the Cold War Bubble* (Chicago: University of Chicago Press, forthcoming).

¹⁹ Important representatives of the literature drawn especially clearly along these lines would include Gregg Herken, *Cardinal Choices: Presidential Science Advising from the Atomic Bomb to SDI* (Stanford, CA: Stanford University Press, 1992); Jessica Wang, *American Science in an Age of Anxiety: Scientists, Anticommunism, and the Cold War* (Chapel Hill, NC: University of North Carolina Press, 1999); Charles Thorpe, *Oppenheimer: The Tragic Intellect* (Chicago: University of Chicago Press, 2006); Kelly Moore, *Disrupting Science: Social Movements, American Scientists, and the Politics of the Military, 1945–1975* (Princeton, NJ: Princeton University Press, 2008);

Were there exceptions? One could, as the political scientist Don K. Price did in the mid-1960s, detect the presence of an unusual breed of scientist-critics, or “established dissenters,” as he called them. But they followed the same basic rule. For Price, as for more recent scholars of the Cold War U.S., if you were a dissenter you were on the outside; and if you bore the state’s imprimatur, true dissent was a barricaded avenue for you. Price’s study was populated by two species. “Insiders” were the scientists “who hold important positions of influence in government, and in the institutional structure by which government and science are now so closely connected...” These insiders, elite and blessed with authority, were nevertheless “likely to accept the subordination of science to the value systems established by the nation’s political tradition and interpreted by the authority of its government.” Paired against them, naturally, were the “Outsiders.” These were the scientists “who prefer to appear as independent critics of present policy.”²⁰

The work and experiences of the arms controllers cannot be sorted so easily, and such hard-walled containers will not fit them. This is true in several senses, as I argue in this study. There is, first, the question of how one became an “arms control expert.” In these pages we will not find anyone sitting for an examination to become an accredited arms controller. Rather, beginning around 1960, we will see people participating in an intellectual, technical, and political community. Almost every member had experience working on defense matters for the U.S. government. Some had been on the Manhattan Project. Some were members of top-level government groups like the President’s Science Advisory Committee; others were strategic analysts whose name badges featured the insignia of the RAND Corporation. Some were “Jasons,” members of the ultra-elite band of defense consultants created in 1959 as part of a

Zuoyue Wang, *In Sputnik’s Shadow: The President’s Science Advisory Committee and Cold War America* (Piscataway, NJ: Rutgers University Press, 2008).

²⁰ Don K. Price, *The Scientific Estate* (Cambridge, MA: Belknap Press of Harvard University, 1965), 83.

Washington think tank known as the Institute for Defense Analyses. Many were consultants to defense-industrial firms; nearly all of them had been paid at some point by a military contract, perhaps from the Atomic Energy Commission or the Defense Department's Advanced Research Projects Agency. They had helped build bombs, designed missiles and antiballistic missile technologies; they'd designed the strategies for using them—or, more accurately, strategies for preventing their use. Major participants in the world of nuclear arms control were also government nuclear experts of one stripe or another. This hands-on, “insider” experience was what made them nuclear experts in the first place, giving heft and authority to their arguments. And it gave shape to their social world, putting them in contact with one another in various studies and committees. To call them “outside critics” is to miss the point—even when they aired critiques of government policy from non-government positions, and even when that was what they sometimes called themselves.

There is the question of the kinds of knowledge people involved in such work were able to produce. “Servants,” Ralph Lapp seemed to suggest, were ill equipped for the creative production of pure scientific knowledge. But I find this to be a poor description of many of the elite consultants discussed in this dissertation. They did important (even Nobel Prize-worthy) scientific research—not in spite of the demands of their government patrons, but in part *because* of the opportunities and resources offered by their consulting duties. Such work provided not just money, but the motivation of specific, urgent problems, along with a tight circle of people to discuss them with. As I explain, the problem of missile defense—and the dream of using high-

tech gadgets to shoot down ballistic missiles—motivated some groundbreaking physics research in the 1960s.²¹

There is the question of the state's relationship to the project of arms control itself. The relationship was, in a word, ambivalent. Many officials perceived arms control as inimical to national security, a dangerous pursuit for a superpower locked in twilight struggle with a nuclear-armed rival. But arms control was not a project hostile to the “nuclear state” (as the arms controllers were often at extraordinary pains to argue). It was always meant to be compatible with—to strengthen—nuclear deterrence, the core of U.S. nuclear weapons policy during the Cold War. The government did not “create” experts with this view, however. The expert arms control community sprang initially from the university, aided by the spotty funding of private foundations. It was the arms controllers who built arms control expertise into the state by promoting its insights among policymakers and, soon enough, by helping to create a dedicated federal agency for arms control. The state hadn't raised and harnessed arms control expertise; arms control experts raised and harnessed themselves.²²

And there is the question of criticism and opposition. Arms controllers rarely made clear political or professional distinctions between their work for the government and their roles as critics questioning the wisdom of U.S. nuclear strategies, policies, and technologies. If they did, such distinctions were for rhetorical effect. They wrote classified reports and think tank studies; they wrote scientific and scholarly articles in the professional literature; they wrote opinion pieces and essays in major periodicals. They wrote and wrote, constantly and sometimes desperately, in a huge range of venues open and secret, public and private, for elites and non-

²¹ The most important example of a later scholarly interpretation of Cold War science in a spirit similar to Ralph Lapp's is Paul Forman, “Behind quantum electronics: National security as basis for physical research in the United States, 1940–1960,” *Historical Studies in the Physical and Biological Sciences* 18 (1987): 149–229.

²² I borrow the term “nuclear state” here from Charles Thorpe, *Oppenheimer*, xiv. It comes originally from Robert Jungk, *The Nuclear State*, trans. Eric Mosbacher (London: J. Calder, 1979).

elites, for the government and not, friendly to its policies and energetically opposed. None of this was contradictory to them.

A key moment divides the history of U.S. arms control, in which the arms controllers' activities took on complex new dimensions. The fight over missile defense reached top intensity and rancor in 1969. It was coupled to a less publicly visible (but for the arms controllers, no less important) controversy over a new technology that put multiple nuclear warheads on a single missile. During this adventure arms controllers began to interact with and perform for multiple audiences and venues. Less and less were their debates about the nitty-gritty of arms control confined to classified settings, or privileged spaces like the Cambridge arms control seminar. Their discussions and disagreements about whether or not missile defense would work, and about its implications for nuclear strategy and the arms race, were drawn out into public view. Suddenly they could be found providing adversarial testimony before Congress, debating the size and capability of American and Soviet nuclear weapons in the national opinion pages, speaking at protest gatherings organized by concerned local residents whose neighborhoods would host radars and antimissile missiles. These instruments were meant, the U.S. government said, to maintain the security of the homeland. The arms controllers objected.²³

At a basic level the dispute was about knowledge and its relationship to status, to access. The arms controllers supported their claims by positioning themselves as insiders and outsiders—gadflies with security clearances, “independent” critics who had worked on the very technologies being debated. As the sociologist Robert K. Merton argued in a lecture that same

²³ I view the arms controllers, in this moment, as the mediators of a collision between the state's nuclear policies and citizens, whose lives and homes were to be impacted by the realization of those policies. In this I am inspired by recent work in U.S. social and political history that has examined the role of war and national security in the developing relationship between citizens and the modern American government. For example: Christopher Capozzola, *Uncle Sam Wants You: World War I and the Making of the Modern American Citizen* (New York: Oxford University Press, 2008); James T. Sparrow, *Warfare State: World War II Americans and the Age of Big Government* (New York: Oxford University Press, 2011); Gretchen Heefner, *The Missile Next Door: The Cold War Minuteman and the Arming of the Heartland* (Cambridge, MA: Harvard University Press, 2012).

year titled “Insiders and Outsiders,” group identity “becomes intensified under specifiable conditions of acute social conflict.” That was surely true of the missile defense critics, who were identified in the late 1960s more and more by a collective label: “arms controllers.” Theirs was an increasingly distinctive voice.²⁴

Were they dissidents, then? Were they cut down by the state’s hand? Hardly. For the arms controllers, there was no need to choose between the austere roles of the functionary and the dissident. Multiple roles and commitments could be mixed, simultaneously and over the course of a career. The situation was revealingly different in the Soviet Union, America’s Cold War nuclear counterpart, where nuclear research, design, and policymaking were kept behind the heavy doors of the state system. There, when the celebrated nuclear physicist and pioneering weapons designer Andrei Sakharov took the lonely step of openly criticizing Soviet nuclear policies in the late 1960s, he was rewarded with prompt expulsion from military research, and ultimately exile from Moscow. In the United States, by contrast, some of the most forceful critics of the nuclear state were also its contractors and advisors, who comprised a group that was at

²⁴ Robert K. Merton, “Insiders and outsiders: A chapter in the sociology of knowledge,” *American Journal of Sociology* 78, no. 1 (1972): 9–47, on 18. The quotation here is drawn from the essay Merton published in 1972 based on the 1969 lecture. In the quotation, Merton refers specifically to “ethnocentrism,” but the essay in general discusses the phenomenon of “insiderism,” as Merton calls it. Insiderism was the notion that only group membership confers the ability to have social knowledge of that group. Merton argued that the discipline of sociology ca. 1970 was increasingly riven by such insiderism, exacerbated by increasingly isolated scholarly enclaves defined by gender, race, and ethnicity. In Merton’s view, social knowledge was built from the stuff of social structure: hierarchy, power, resources, etc. “Insider” and “Outsider” were themselves, for him, categories of social structure; and structural knowledge was accessible to the careful analyst whether the analyst was an Insider or not. The essay contains a suggestive passage. In it, Merton makes clear how his structuralism meshes with a traditional mid-century view of the science-state relationship: “Chauvinism finds its fullest ideological expression when groups are subject to the stress of acute conflict,” he wrote. “Under the stress of war, for example, scientists have been known to violate the values and norms of universalism in which they were socialized, allowing their status as nationals to dominate over their status as scientists” (on 18). Here Merton pairs “status as nationals” (which is perhaps the same thing as citizenship, and is intrinsically of the state) against “status as scientists” (science being, for Merton, intrinsically *not* of the state). In this dissertation, I propose to reject such dualism.

once firmly established, flexible and mobile. Their activities and arguments uncorked the nuclear bottle, forcing debate and information out into places it had never been before.²⁵

What began to happen to the status and support of U.S. arms control in the early 1970s was subtler than censure, or a display of the state's naked power. But it was no less important. International arms control negotiations settled into predictable rhythms, and the Soviet Union appeared, finally, to reach a parity of nuclear forces with the United States. The Nixon administration's long-simmering suspicion of arms control finally boiled over. The government's support structures for specialized arms control negotiation and research—particularly those lodged in the Arms Control and Disarmament Agency, established in 1961—were defunded and undermined from within the executive. This did not mean that arms control experts were expunged from the state and defrocked of their status. Many of them continued as well connected advisors and consultants, just as before. But it meant that arms control became, increasingly, a matter for non-government investment and activity. In a matter of months, the Ford Foundation—headed by McGeorge Bundy, an arms controller, former government official, and leading light of the establishment—became the most important supporter of arms control research in the United States. With Ford money, arms controllers built academic arms control

²⁵ On Sakharov, see Richard Lourie, *Sakharov: A Biography* (Hanover, NH: Brandeis University Press/University Press of New England, 2002); and Gennady Gorelik, *The World of Andrei Sakharov: A Russian Physicist's Path to Freedom* (New York: Oxford University Press, 2005). Historical scholarship on the Soviet nuclear program and nuclear policymaking has been sparse. The foundational works are David Holloway, *The Soviet Union and the Arms Race* (New Haven, CT: Yale University Press, 1983); and David Holloway, *Stalin and the Bomb: The Soviet Union and Atomic Energy, 1939–1956* (New Haven, CT: Yale University Press, 1994). An important recent contribution, drawing on both U.S. and Soviet perspectives and sources, is Michael D. Gordin, *Red Cloud at Dawn: Truman, Stalin, and the End of the Atomic Monopoly* (New York: Farrar, Straus and Giroux, 2009). Matthew Evangelista argues for the importance of transnational contact between U.S. and Soviet scientists in bringing arms control concerns to the attention and agenda of the Soviet government, even at the height of the Cold War. The actual impact of these exchanges and discussions on Soviet policy is debatable. But Evangelista's work remains indispensable for understanding the history of “backchannel” (or “track-two”) diplomacy in Cold War policymaking and the formation of social movements. See Matthew Evangelista, *Unarmed Forces: The Transnational Movement to End the Cold War* (Ithaca, NY: Cornell University Press, 1999). While this dissertation refers to arguments and ideas presented in the course of international scientific exchanges (especially meetings of the Pugwash movement), it is not focused on such efforts.

institutes at several prominent research universities. Where public support had receded, private support would take its place.

The arms control debate had been permanently loosed into uncontrolled spaces of argument and contest in the 1970s and 80s, exposed to the harsh environment of adversarial politics, battered by claims and counterclaims of “technical objectivity.” The state and the arms controllers, both, had lost their always-tenuous grip on the nuclear predicament as the Cold War came to an end.

A Chapter in Nuclear History

The Cuban Missile Crisis offered nuclear experts an indelible lesson in what starting a nuclear war might actually look like. Imagine, for a moment, that the United States had exercised its option to strike the missiles on their launch pads in Cuba. That surely would have invited some sort of Soviet response, perhaps in Berlin. And what if one of the Cuban missiles, under fire from the Americans, was launched, either by the Kremlin or by some desperate field commander, or even by accident? An American city turned to ash and rubble: and then what?

Arms controllers and other nuclear thinkers thought hard about such possibilities. Only a month after the crisis, in another evening session of the Harvard-MIT arms control seminar, the participants could be heard discussing the “rational” conduct of a nuclear war, if nuclear war came. They bantered about a plan sometimes called the “no-cities” strategy in the early 1960s, but more often “counterforce” throughout the Cold War. The “no-cities” doctrine said that it was better—it was more rational—for each opponent to strike at the other’s weapons, rather than its cities, because that would keep the damage of a nuclear war to a minimum. Robert McNamara had publicly announced the no-cities doctrine in a speech the previous May. One of the seminar

members, MIT professor William Kaufmann, had not only written the speech (and an earlier classified version) but had done the studies at RAND that showed how counterforce strikes would work. More traditional nuclear deterrence, as analysts had usually pictured it, rested on the idea of a massive retaliatory assault on the enemy's cities, delivered by a "secure second strike" force. Since both sides feared this second strike, neither would launch the first strike. Increasingly in the 1960s, analysts and policymakers would talk of this as the promise of "assured destruction."

Most arms controllers favored deterrence by assured destruction, and distrusted the idea of counterforce. But there were very different ways of expressing distrust. Thomas Schelling, sharp and precise, had one way. He liked the idea of reducing war's damage, and of controlling it—of enhancing the "stability of deterrence" by extending the definition of this concept to cover not just the prevention of war, but the deterrence of certain destructive acts *within* a nuclear war. Whatever enhanced the stability of deterrence in Schelling's eyes, he favored. But, as he told the seminar that evening, since "any major war would become a 'cities' war" even if it started out as a "no-cities" affair, there was "no good logical reason to prefer a 'no-cities' to a 'cities' doctrine." The *threat* of striking cities would always be a part of the bargaining process in a crisis, even if cities weren't struck at the outset. And even if they were moved down the list of targets, cities would inevitably be the victims of great violence in any real nuclear war, if only because so many counterforce targets sat in or near urban concentrations. A "no-cities" strategy was therefore a "cities" strategy in disguise. It came short of Schelling's logical expectations; and who could fault his reasoning?

But Bernard Feld—an original member of the atomic scientists' movement, an MIT nuclear physicist, and a perennial arms control figure in Cambridge—raised a very different

objection. "The side that achieves a counterforce capability must also obtain superiority," he said, for otherwise counterforce would have no purpose. What point was there in developing a force that could take out the other side's nuclear weapons if you couldn't also threaten its cities, which it valued much more than its weapons? Counterforce demanded that the side employing it acquire many more weapons than the other side, to destroy the other side's weapons *and* threaten its cities. Now, suppose both sides chased this superiority at the same time: that was an arms race. And the arms race was Feld's nightmare. Feld and likeminded thinkers were concerned with stability, too, but much more with stabilizing and reversing the arms race than with the stability of deterrence. For Feld, unlike Schelling, the arms race was the central malady of the nuclear age, and the point of arms control was to cure it.²⁶

Right there was an important fracture running through arms control from the very beginning. Is the purpose of arms control to reduce the destructiveness of nuclear war, or to eliminate nuclear war's possibility? To what ends, if any, can nuclear weapons be used? Is there a best, most rational way to fight a nuclear war, or is nuclear war so terrible that the only possible purpose of strategy and policy is to prevent it? At what targets should nuclear weapons be aimed? And what will bring them to their targets? Can they be defended against? If they can, should the attempt be made? They are threatening; but what, precisely, should they threaten? What is an arms race? As we will see, of all arms control problems, the issues raised by the "delivery" of nuclear weapons in the ballistic missile age plagued the arms controllers and arms control negotiations to no end. These questions were, at bottom, both technical and political. Their very complexity and hybridity brought thinkers from a great range of disciplinary

²⁶ "Joint Arms Control Seminar, Minutes of the Fourth Session, November 19, 1962," Box 3, Folder 3 "Joint Harvard-MIT Arms Control Seminar 1962-63 Minutes," *LPB*.

backgrounds, across the natural and social sciences, together to the same seminar rooms and conference tables.

This dissertation is a history of arms control thought, as well as a social and political history. Along with the structure of arms control, I am interested in arms control's substance.²⁷ I ask how arms controllers arrived at their alliance with nuclear deterrence, their nervousness about arms races, and their anguished relationship with technology. The arms controllers, like many Cold War thinkers, prized rationality.²⁸ But if they valued one thing above all, it was stability. Time and time again they paid tribute to it, dissected it, and measured policies and technologies against it. They applied it to nuclear war, to the prevention of nuclear war, and to the arms race. Where did stability come from? For strategic analysts and arms controllers, stability quickly became a natural category. It was a property of something called the "strategic balance," or a property of the structure of international politics, and it was produced (or jeopardized) by the nature of nuclear technology. (Analysts of nuclear policy and international relations continue to talk, easily and volubly, of stability.) Stability came, somehow, from the weapons themselves. So it has been for many histories of nuclear ideas, which view nuclear thought and policy as born out of the encounter between disembodied minds and technological

²⁷ Kelly Moore writes: "The debates that scientists had about...their relationship to the government and other parts of society were thus not only about substantive issues. They were also about the status of science as an ideological system to serve as a basis for organizing political life." (Moore, *Disrupting Science*, 132.) This is an important point. But for the arms controllers, it should also be said that the "substantive issues" were much more than the wrapping paper on ideological or political claims, or ideology reduced to practice. For the arms controllers the specific issues were the only means to participation in the community. The substance animated arms control as a distinct sensibility, and it was the vehicle by which arms control experts moved between their various worlds of activity. In describing the history of this particular group of experts and thinkers, we cannot do without the substance.

²⁸ S.M. Amadae, *Rationalizing Capitalist Democracy: The Cold War Origins of Rational Choice Liberalism* (Chicago: University of Chicago Press, 2003); Paul Erickson, Judy L. Klein, Lorraine Daston, Rebecca Lemov, Thomas Sturm, and Michael D. Gordin, *How Reason Almost Lost Its Mind: The Strange Career of Cold War Rationality* (Chicago: University of Chicago Press, 2013).

and political facts in the raw. The weapons presented the problems; it remained for the “defense intellectuals,” the “Wizards of Armageddon,” and the policymakers, to think about them.²⁹

This study attempts to ground nuclear ideas and debates by locating them in a social world. The weapons are inhuman, but nuclear thinkers were not: they take on a more human size and shape when they and their ideas are placed in richer context. A long subsequent history has bestowed a highly developed and inevitable-seeming jargon—a mannered speech—on analysts and policymakers debating nuclear weapons today. But nuclear ideas were never purely nuclear, as careful study of the arms control field in its early days demonstrates. Arms control issues did not make themselves; arms control thinkers made them. The issues should therefore be observed and understood in their institutional and intellectual settings, where ideas are developed and altered in the flow of social traffic. Arms controllers worked in an environment of changing institutions, moving between universities, the government, and private foundations. And increasingly from the late 1960s onward, they could be found at the crossroads of private discussion and public debate. In that moment, secret issues and disagreements became open, open controversies affected national security policies, and the hairline intellectual fractures in the world of strategy and arms control were split wide.³⁰

²⁹ Lawrence Freedman, *The Evolution of Nuclear Strategy, Third Edition* (New York: Palgrave Macmillan, 2003 [1981]); Fred Kaplan, *The Wizards of Armageddon* (Stanford, CA: Stanford University Press, 1991 [1983]); Gregg Herken, *Counsels of War* (New York: Alfred A. Knopf, 1985); Marc Trachtenberg, “Strategic thought in America, 1952-1966,” *Political Science Quarterly* 104, no. 2 (1989): 301-344, reprinted in Marc Trachtenberg, *History and Strategy* (Princeton, NJ: Princeton University Press, 1991). Of these four classic studies, Kaplan’s, centered on the RAND Corporation, is the most implicitly sensitive to the role of institutions and social communities in the history of nuclear ideas. Still, for Kaplan as for the others, strategic thought is essentially a self-contained field. Defense intellectuals and strategists on non-nuclear issues have merited focused historical study, as well. For a highly critical take, see Bruce Kuklick, *Blind Oracles: Intellectuals and War from Kennan to Kissinger* (Princeton, NJ: Princeton University Press, 2006). For an interpretation more sensitive to some of the transnational sources of intellectuals’ construction of the “Cold War,” see Udi Greenberg, *The Weimar Century: German Émigrés and the Ideological Foundations of the Cold War* (Princeton, NJ: Princeton University Press, forthcoming).

³⁰ My interest, in other words, is in the *practice* of nuclear arms control—that is, arms control ideas grounded in a community of arms control practitioners. In writing about arms control as a community of practice, my primary guides are Peter Galison, *Image and Logic: A Material History of Microphysics* (Chicago: University of Chicago Press, 1997); and David Kaiser, *Drawing Theories Apart: The Dispersion of Feynman Diagrams in*

“They may live in many hotels and houses,” wrote C. Wright Mills of the unfettered power elite, “but they are bound by no one community.” The elite were no longer obligated to a larger conception of civic life or the public good, he thought. They were beholden only to the mighty institutions, which expressed their power. The notion of “community” was archaic, the relic of a pre-modern time. He described the “advisers and consultants” who populated “the immediate scene in which the drama of the elite is enacted” as, simply, “more or less unattached.”³¹ But I describe the arms controllers as a community. They were advisors and consultants, preeminently so; they were self-consciously, unashamedly elite. Members of the establishment intelligentsia, many of them were tenured professors. They were white, and mostly male. A few women began to join the ranks in the 1970s, but they, too, traveled in unusual echelons of privilege. The arms controllers would have been excellent candidates for inclusion in Mills’ “higher circles.” They were what Norman Mailer had in mind when he wrote of “liberal technologists” and the magistrates of “technology land.”³²

Postwar Physics (Chicago: University of Chicago Press, 2005). Within the vast field of “nuclear history,” numerous scholars have unpacked nuclear knowledge, technology, and politics using insights and techniques from the fields of the history and sociology of science and technology. I see my project as an effort in the same vein. Notable examples include: Donald MacKenzie, *Inventing Accuracy: A Historical Sociology of Nuclear Missile Guidance* (Cambridge, MA: The MIT Press, 1990); David Holloway, *Stalin and the Bomb: The Soviet Union and Atomic Energy, 1939–1956* (New Haven, CT: Yale University Press, 1994); Paul N. Edwards, *The Closed World: Computers and the Politics of Discourse in Cold War America* (Cambridge, MA: The MIT Press, 1996); Hugh Gusterson, *Nuclear Rites: A Weapons Laboratory at the End of the Cold War* (Berkeley, CA: University of California Press, 1996); Lynn Eden, *Whole World on Fire: Organizations, Knowledge, and Nuclear Weapons Devastation* (Ithaca, NY: Cornell University Press, 2004); Sharon Ghamari-Tabrizi, *The Worlds of Herman Kahn: The Intuitive Science of Thermonuclear War* (Cambridge, MA: Harvard University Press, 2005); David Kaiser, “The atomic secret in red hands? American suspicions of theoretical physicists in the early Cold War,” *Representations* 90 (Spring 2005): 28–60; Scott Kirsch, *Proving Grounds: Project Plowshare and the Unrealized Dream of Nuclear Earthmoving* (New Brunswick, NJ: Rutgers University Press, 2005); Michael D. Gordin, *Five Days in August: How World War II Became a Nuclear War* (Princeton, NJ: Princeton University Press, 2007); Michael D. Gordin, *Red Cloud at Dawn: Truman, Stalin, and the End of the Atomic Monopoly* (New York: Farrar, Straus and Giroux, 2009); Gabrielle Hecht, *Being Nuclear: Africans and the Global Uranium Trade* (Cambridge, MA: The MIT Press, 2012); Rebecca Slayton, *Arguments That Count: Physics, Computing, and Missile Defense, 1949–2011* (Cambridge, MA: The MIT Press, 2013).

³¹ Mills, *The Power Elite*, 4.

³² Norman Mailer, *The Armies of the Night: History as a Novel, the Novel as History* (New York: Plume, 1994 [1968]), 15.

Still, the arms controllers belonged to an intellectual and political community, and they knew it. They enjoyed the benefits of status, but they also felt its burdens. Community confirmed their status as experts, gave shape and impetus to their ideas and proposals. Community silhouetted the arms controllers against the political backdrop of the middle and late Cold War, distinguishing theirs from rival approaches and commitments. When we dig down through arms control ideas and the institutions in which they developed and circulated, we find personal relationships between specific people with a shared history. In one venue after another, study after committee after seminar, they worked and wrote and talked together. They were not unattached.

Insiders and Outsiders

As Chapter One reveals, it was around 1960 that arms control first emerged as a distinct field with a distinctive cast of characters. And yet arms control as it had developed by the beginning of the decade seemed to bear only a faint resemblance to the disarmament tradition that had preceded it in the 1950s. In this chapter I show how this shift in thought and practice developed by situating it in its social and institutional context—in particular, by describing the institutional and intellectual mixing of a group of Boston-area disarmament advocates, Washington policymakers, and RAND social scientists. Strategic arms control emerged quickly alongside a new theory of nuclear deterrence, within significantly overlapping groups of thinkers.

The intellectual history of nuclear arms control has largely been written as a history of ideas, untethered from a deeper social context. This chapter reinterprets the early history of arms control thought by placing it within a community of disarmament advocates, located mainly in

the Boston area, during the late 1950s. Arms control ideas were neither a simple functional response to the events of the Cold War, nor did they spring from the properties of nuclear weapons technology. Local and contingent factors, too, shaped their history. In particular, the all-important idea of stability was contested within the early arms control community. Strategic analysts like Thomas Schelling preferred to think of stability as a more static phenomenon, applicable primarily to nuclear deterrence. But others, especially Jerome Wiesner—a control systems engineer and cyberneticist by background, and a participant in the Boston disarmament group—proposed to stabilize and correct the arms race through comprehensive arms control systems and processes of long-term dynamic feedback. These arguments and discussions took shape within a developing arms control milieu. In the summer of 1960 the American Academy of Arts and Sciences sponsored the first major unofficial conferences in the new field. This culminated in the Academy’s “summer study on arms control,” fashioned in the style of the many Cold War summer studies that military technical consultants had participated in for years.

Chapter Two examines the movement of technical expertise between the university and the defense bureaucracy in the early-to-mid 1960s, and its implications for the creation of scientific knowledge. New institutional arrangements were forged in an effort to tackle a range of specialized nuclear age problems. Unique government institutions were raised for the purpose, including the Defense Department’s Advanced Research Projects Agency. And new not-for-profit organizations—occupying a niche between the government, the universities, and private corporations—channeled expertise from the civilian academic world to the realm of classified defense projects. This chapter centers on an elite group of advisors working for the Institute for Defense Analyses (IDA), a not-for-profit outfit consulting on a wide spectrum of missile age issues.

After the laser was invented in 1960, officials hoped the new gadget might become a revolutionary weapon for military applications, including ballistic missile defense. Expert consultants working for IDA began to study this possibility intensively. Closely connected through elite circles of national security consulting, several of the physicists (including the Harvard University professor Nicolaas Bloembergen) set to work on the problem of generating powerful laser pulses and propagating them through the atmosphere. Along the way, in an admixture of classified discussions and reports, and a series of important publications in the unclassified literature, the consultants shaped the foundations of a new field: nonlinear optics. Nonlinear optics is the science of the interaction between matter and intense light, and it became a major branch of quantum electronics in the 1960s, bridging optics and solid-state physics. This chapter attempts to look beyond Paul Forman's famed argument that postwar American physics (as exemplified by quantum electronics) had been captured, even corrupted, by military patronage. The origins of nonlinear optics paint a more complicated picture than Forman's thesis suggests. Nonlinear optics was no simple product of government funding or innovations in technology. Rather, it developed within an intimate community of defense consultants preoccupied with a tricky Cold War problem.

Public debate concerning nuclear weapons in the late 1950s and early 1960s had revolved around the issue of nuclear testing and the detrimental effects of radioactive fallout on human health. But in the late 1960s, more arcane arms control issues bearing on missile design and missile defense began to intersect with public concerns and public politics. In Chapter Three I ask how and why these nuclear issues came up for such remarkable scrutiny and contestation in a relatively short time. I recount two key arms control issues of the late 1960s: the "Antiballistic Missile" (ABM), and the "Multiple Independently-targeted Reentry Vehicle" (MIRV). ABM was

a system designed to shoot down incoming nuclear warheads with nuclear missiles, or “antimissile missiles.” MIRV technology allowed one missile to carry several nuclear warheads, each to an independent target. To many arms controllers, ABM and MIRV were noxious for numerous reasons, but especially because these technologies offended their deeply held commitment to deterrence and stability. The arguments they offered against ABM usually stressed both its technical fallibility and its tendency to undermine stability by appearing to give the side with missile defenses an incentive to strike first. Arms controllers leapt into the political fray with these issues, providing testimony before Congressional committees, writing position papers, speaking at local rallies, and joining forces with groups of suburban homeowners who decried the military’s appropriation of public land to put hydrogen weapons in their backyards. Yet other experts—especially the longtime foreign policy advisor Paul Nitze and his conservative allies—felt that missile defense was essential to American security and nuclear strength. This chapter shows how such arcane controversies within the arms control and nuclear strategy and policy worlds were suddenly propelled outward into a very divisive and very public controversy.

Chapter Four charts the changing institutional structure of support for arms control expertise in the 1970s. I consider the impact of these changes on the arms control debates of the era following the conclusion of the first round of Strategic Arms Limitation Talks between the U.S. and the Soviet Union in 1972. Discussion among program officers at the Ford Foundation early in the decade described a growing need to replenish the stock of ideas and personnel in arms control. Among many, it was thought that arms control had suffered an intellectual decline in the decade or so since the field’s founding. A new program of support had the enthusiastic backing of the foundation’s president McGeorge Bundy, who had left his position as national

security advisor a few years earlier. Initially the Ford Foundation began by supporting “regional” arms control seminars, the most active and successful of which was based at the California Institute of Technology, featuring many experts from the nearby RAND Corporation. In the early 1970s, a few well-placed participants in the so-called California Seminar on Arms Control and Foreign Policy formulated an influential critique of nuclear deterrence and some of the precepts of strategic arms control. Their ideas would soon find a place in the policies of the Nixon administration.

In 1973 Nixon slashed the payrolls and the research budget of the most important voice for arms control (and home for arms control expertise) in the executive branch: the Arms Control and Disarmament Agency. Arms controllers were deeply troubled. The Ford Foundation’s trustees decided that year to pursue a long-term, multi-million-dollar program of support for arms control research, filling the gap left by the government’s retreat. The money was directed at the universities, where arms control had first flourished, and arms controllers soon built substantial academic institutions with Ford Foundation support. At Harvard in 1973, Paul Doty established the Program for Science and International Affairs, and similar ventures emerged at Stanford, MIT, and Cornell. Important arms control work flowed from these new programs—including (at MIT) some of the earliest and most vigorous criticisms of improved missile accuracy—and the legacy of these Ford-funded programs and centers remains with us today. The case of arms control, and its complicated patronage relationships with the state and with the foundations, requires putting aside some of our stock narratives about the “Cold War University.” U.S. arms control was a complicated affair: a Cold War project, yet always pursued waveringly by the state; a child of the academy but raised specifically for application to public policy; and supported mainly by private money in the 1970s, the decade in which arms control’s

pursuit by the government at the international negotiating table was needed more desperately than ever.

As American politics splintered into a complex pattern during the late Cold War, the meaning of nuclear arms control, and the community that had once cohered around it, shifted in response. In the 1970s and 80s arms control was caught up in fractious debates, as issues that would have at one time been confined within elite circles continued to spill out into the public. Chapter Five explores the controversy surrounding Ronald Reagan's announcement of the Strategic Defense Initiative (SDI) in 1983. Soon after SDI was made public, expert panels were formed within the government and by non-government organizations to assess the feasibility and wisdom of SDI. The focus here is on the work, internal politics, and controversial wider reception of a major critical study of SDI by the American Physical Society.

The APS study assembled the most thorough and pointed critique of the “directed energy” weapons technology under consideration and development by the Strategic Defense Initiative Organization (SDIO) and the federal weapons laboratories. Formed in 1984 and comprised of elite scientists with insider experience in missile defense—overseen by a review panel including several prominent first-generation arms controllers—all of the study group's members were cleared to view secret military information. But because the study was conducted chiefly for public release, this access came with complications and compromises. At the root was unease over perceptions of the study's objectivity: the study group's members were at pains to protect it; opponents were eager to attack. The Defense Department claimed the study group hadn't seen the most up-to-date information and was therefore unqualified to judge its programs authoritatively. The leadership of the American Physical Society, meanwhile, issued a controversial statement condemning SDI—a statement the study group itself had not approved.

As a result, the report was mired in a complex dispute from the very beginning. Its fate was quick dismissal by officials in power, though it had performed the increasingly non-governmental function of arms control: to fan the flames of public criticism of the government's most sophisticated nuclear weapons systems.

Before we can make sense of the arms control concerns and debates of the late Cold War, however, we must return to the high Cold War of the late 1950s. It was then that a small group of thinkers began meeting and talking, in the privileged space of the academy, about how to structure a specialized response to the problem of nuclear weapons.

CHAPTER 1

Disarmament, Arms Control, and Stability: Creating the Community of Nuclear Arms Controllers, 1957– 1960

“Arms control” as a synonym for “arms regulation” is an American innovation which has come into usage only in the last decade and a half. While “arms regulation” points in the direction of agreement to regulate, “arms control” connotes power to control. In different languages, this meaning of “control” comes more or less to the foreground; among the various translations given in dictionaries, words like *beherrschen* in German and *autorité* in French are prominent. In the *Oxford English Dictionary*, even “overmaster” is given as one of the meanings of “control.” An overtone of some body having interest or power to control is quite audible in many of the United States pronouncements on “arms control.”

— Alva Myrdal (1977)¹

Introduction

In 1960 the economist and strategy intellectual Thomas Schelling announced the arrival of a new field of thought and policy. Schelling wrote to his fellow participants in a “summer study on arms control” conducted that year near Boston:

I am impressed...with how far we are, and everybody else is, from having a reasonably mature and sophisticated conception of what arms control is all about, what genus of activity it belongs to, how to characterize it, what it depends on, what it aims to do, and how it relates to more general areas of policy like diplomacy and military affairs.²

Notes

¹ Alva Myrdal, *The Game of Disarmament: How the United States and Russia Run the Arms Race* (Manchester: Manchester University Press, 1977), xvi.

² T.C. Schelling to B. Feld (undated), Box 3, Folder 19 “AAAS, Committee on Technical Problems of Arms Limitation, Summer Study on Arms Control, Summer Study on Arms Control 1960, 1/2,” *BTF*.

In an essay from the same period Schelling argued that although arms control was “still listed anachronistically under ‘disarmament,’ it is differently oriented. It assumes deterrence as the keystone of our security policy, and tries to improve it. It accepts a retaliatory capability as something to be enhanced, not degraded.” Most important of all, arms control “rather shakes the disarmament tradition” by its indifference to the mere numbers of nuclear weapons in a country’s possession. If disarmament sought to reduce, even eliminate, the stockpiles and deployments of nuclear weapons, arms control wanted to tailor the weapons to make deterrence more robust. A new term—an unexplored new field of study—was called for.³

When scholars have looked back at the origins and development of arms control thought in the United States, they have often sketched a picture similar to Schelling’s. Almost everyone agrees that arms control was a natural outgrowth of the theory of stable nuclear deterrence that analysts (notably Schelling) had developed in the late 1950s. Suddenly, writes Lawrence Freedman, those who would impose “control” on nuclear weapons in the service of stable deterrence had reached “conclusions that seemed quite bizarre to the military mind. The argument was to abstain from the most advanced militarily useful weapons, while *encouraging* the enemy to improve his defences.” Thus arms control represented a clear “shift in emphasis towards the notion of managing rather than eliminating the arms race.”⁴

More than other scholars, Jennifer Sims stresses long-term evolution, rather than sudden change, in arms control thought during the postwar period. Her intellectual history of arms control surveys a trove of published writings from 1945 to 1960. She labels strategic arms control the “Cambridge Approach,” after two influential Boston-area conferences held during the

³ Thomas C. Schelling, “Reciprocal measures for arms stabilization,” *Daedalus* 89, no. 4, *Arms Control* (Fall 1960): 892-914, on 892.

⁴ Lawrence Freedman, *The Evolution of Nuclear Strategy, Third Edition* (New York: Palgrave Macmillan, 2003 [1981]), 181, 183-184, emphasis in original.

summer of 1960. In later years, these meetings would be widely regarded as inaugural events in the history of U.S. arms control. “The general literature maintains that the major theoretical foundation for modern nuclear arms management was laid in the late 1950s and early 1960s after disarmament efforts proved unproductive.” But Sims disagrees, discovering important precursors for some of the main tenets of the Cambridge Approach in diverse sources going all the way back to the 1940s. Eminent physicist-statesmen like Robert Oppenheimer and Leo Szilard, and the dean of civilian strategists, Bernard Brodie, all displayed a far more sophisticated understanding of the strategic implications of nuclear weapons than has often been admitted.⁵

These interpretations (and others), though differing in periodization and emphasis, share important traits. One is the suggestion that Schelling-esque arms control—arms control in the service of deterrence and indifferent to “literal disarmament”—triumphed in the United States. Sims’s interpretation is, in this sense, a prehistory. It searches for ideas and arguments throughout the postwar era that would come to resemble post-1960 strategic arms control (as codified, for example, in Schelling and Halperin’s 1961 classic *Strategy and Arms Control*).⁶

Another shared trait is a view of arms control thought as a functional response to changes in the geopolitical and technological environment. Emmanuel Adler writes that early arms

⁵ Jennifer Sims, *Icarus Restrained: An Intellectual History of Nuclear Arms Control, 1945-1960* (Boulder, CO: Westview Press, 1990), quotation on 9. Also see Jennifer E. Sims, “The American approach to nuclear arms control: A retrospective,” *Daedalus* 120, no. 1 (1991): 251-272; Emanuel Adler, “The emergence of cooperation: National epistemic communities and the international evolution of the idea of nuclear arms control,” *International Organization* 46, no. 1 (1992): 101-145; Steven E. Miller and Scott D. Sagan, “Nuclear power without nuclear proliferation?,” *Daedalus* 138, no. 4 (2009): 7-18. Adler remarks that “The [American Academy of Arts and Sciences] convened a summer session to deal with arms control, and this session resulted in the participation of more than fifty individuals and the publication of several books that became landmarks in the intellectual history of arms control” (on 123). Adler does not distinguish here between the two separate conferences (one brief, one an extended summer study) in 1960.

⁶ Thomas C. Schelling and Morton H. Halperin, *Strategy and Arms Control* (New York: The Twentieth Century Fund, 1961). In 1959 Schelling recommended against understanding “disarmament in its traditional, literal sense of reducing the aggregate level of armaments...but rather as a generic term covering measures to reduce the likelihood of war.” See T.C. Schelling, “Toward a theory of strategy for international conflict,” RAND P-1648 (19 March 1959); reprinted in part as T.C. Schelling, “The retarded science of international strategy,” *Midwest Journal of Political Science* 4, no. 2 (1960): 107-137, on 129-130.

control ideas “were a response to changes in technology and weapons systems, the balance of power between the superpowers, and American domestic politics.”⁷ Marc Trachtenberg locates arms control more broadly within an “extraordinary period of intellectual productivity” in nuclear strategy between the mid-1950s and the late 1960s, whose ultimate source was the “thermonuclear revolution...dominated and symbolized by the huge thermonuclear explosives first tested in 1952.”⁸ And here is Colin Gray (in a more skeptical mood): “The classical arms-control literature of the early 1960s was the product of a chain of ‘expert’ responses to a series of public policy initiatives of differing degrees of implausibility,” mainly in reaction to the advent of the intercontinental ballistic missile and the threat of surprise attack.⁹

The collective picture of arms control thought provided in these accounts is one rich in arguments. But it is also sparse in more immediate context and social texture. The ideas live in an abstract space—responsive to shifts in technology and geopolitics, but untethered from the more mundane details of personal background and training, and the web of connections that characterized the social life of the early U.S. arms control community.

Here I argue for the importance of local and contingent factors shaping the intellectual history of early arms control. Drawing from the personal archives and correspondence of some of the participants, I try to locate arms control and its concepts within a community, to see where ideas meet people and institutions. Arms control thought was not delivered by external

⁷ Adler, “The emergence of cooperation,” on 119. Adler follows the interpretation of Bernard Bechhoefer, who wrote a widely read contemporary history of arms control in 1961. He stated that the Surprise Attack Conference had been a “catalyst for much of the serious rethinking of arms control and stabilized deterrence which took place in the U.S. between 1959 and 1961.” Bernard G. Bechhoefer, *Postwar Negotiations for Arms Control* (Washington, DC: Brookings Institution, 1961), 475.

⁸ Marc Trachtenberg, “Strategic thought in America, 1952-1966,” *Political Science Quarterly* 104, no. 2 (1989): 301-344, on 303.

⁹ Colin Gray, *Strategic Studies and Public Policy* (Lexington, KY: The University Press of Kentucky, 1982), 72.

developments in the Cold War or the nuclear arms race, nor was it plucked out of the intellectual ether.

Arms control in the United States was local in the sense that it was lodged in a social community, steeped in what Schelling referred to as the “disarmament tradition.” In 1957 a professor of nuclear physics at MIT named Bernard Feld became the impresario of a small group of Boston area disarmament advocates. Its members were all professional scientists, and most had been affiliated with the postwar atomic scientists’ movement. Most of the participants in this group had also been Manhattan Project veterans; and most believed that serious nuclear disarmament was the only sensible answer to the conundrum of nuclear weapons. They began to meet regularly and discuss technical approaches to the problems of the nuclear arms race—at first under the auspices of the Federation of American Scientists, later with the support of the American Academy of Arts and Sciences.

Over the course of 1958 and 1959, new ideas about nuclear deterrence, and the relationship between deterrence and nuclear disarmament, began to percolate throughout informal and official advising communities in the United States. Framed by the widely recognized problem of surprise attack, this new cluster of concepts and proposals were increasingly gathered under a new name: arms control. Arms control indicated some sort of cooperative effort between the United States and the Soviet Union to stabilize nuclear deterrence and put reins on the nuclear arms race (or some proportion of the two). Personal contact between analysts at the RAND Corporation, government science advisors, and members of the Boston community put these new ideas and terms into circulation among the experts. When arms control was launched at two major conferences in Boston in 1960, these events and the community they

supported crystallized around the original Boston disarmament committee and the work Bernard Feld and his colleagues had been engaged in for years.

Components of arms control's intellectual structure were also, I argue, contingent. Contrary to Schelling's portrait, the disarmament tradition was not cleanly superseded by arms control; the early arms control community included many who professed commitment to disarmament. And at least one of the key concepts of modern arms control—the idea of *stability*—had a contested status and definition in the early arms control community. While one notion of stability gained traction among some strategic analysts (the stability and strength of deterrence under short-term strains), another concept (the stability of arms limitation and the arms race) was developed and promoted by some experts who continued to put stock in disarmament. Jerome Wiesner, in particular, recommended comprehensive arms control systems that would reduce and stabilize arms levels through processes of long-term dynamic feedback. Wiesner, a control systems engineer and cyberneticist by background, relied on a particular mental image of stability in crafting his view of how arms control should work.

In this sense there was no inexorable evolution toward modern arms control in the postwar period, and no coherent “Cambridge Approach” to arms control. Among the arms controllers gathered in Boston were those who held to a commitment to some form of nuclear disarmament. And where fundamental disagreements persisted over the meaning and ends of stability—whether the purpose of arms control was to stabilize deterrence or disarmament—no consensus would develop.

The Boston Committee and the Disarmament Tradition

Disarmament negotiations between the United States and the Soviet Union left a dismal record in the 1950s. The issue of inspection and verification (the U.S. always wanted more of it, the Soviets always less) was a perennial sticking point. The question whether robust technical solutions to the problems of inspection could be designed grew more urgent for the Americans as initial talks on a possible nuclear test ban stalled in London in 1956 and 1957.¹⁰ In a meeting of the National Security Council in January 1958, James Killian, chairman of the new President's Science Advisory Committee (PSAC), argued that the U.S. needed more "up-to-date technical appraisals" before it could support a test ban, or any other disarmament measure.¹¹

The deadlock led some U.S. scientists to conclude that perhaps only non-official channels offered a way forward. Foremost among such 'track-two' diplomatic efforts was the international "Pugwash movement," which first brought together twenty-two scientists representing ten nations (including the U.S. and the Soviet Union) in the summer of 1957 to debate the problems of the nuclear arms race at the financier Cyrus Eaton's Nova Scotia estate. But the shared sense of crisis also encouraged new domestic efforts, challenging some members of the American intellectual community to organize themselves for the intensive study of nuclear disarmament.¹²

Bernard Feld, a physicist at MIT and the chairman of the Greater Boston Branch of the Federation of American Scientists, decided to take up this challenge. In 1957 he spearheaded a

¹⁰ On the history of the U.S.-Soviet nuclear test ban negotiations, see Robert A. Divine, *Blowing on the Wind: The Nuclear Test Ban Debate, 1954-1960* (New York: Oxford University Press, 1978); and Benjamin P. Greene, *Eisenhower, Science Advice, and the Nuclear Test-Ban Debate, 1945-1963* (Stanford, CA: Stanford University Press, 2006). Killian is quoted in Zuoyue Wang, *In Sputnik's Shadow: The President's Science Advisory Committee and Cold War America* (New Brunswick, NJ: Rutgers University Press, 2008), 121.

¹¹ On the U.S. government's technical approach to disarmament, see, e.g., Charles A. Appleby, Jr., "Eisenhower and Arms Control, 1953-1961: A Balance of Risks (Volume I)" (PhD dissertation, The Johns Hopkins University, 1987); and Jeremi Suri, "America's search for a technological solution to the arms race: The Surprise Attack Conference of 1958 and a challenge for 'Eisenhower revisionists'," *Diplomatic History* 21, no. 3 (1997): 417-451.

¹² On the Pugwash movement see, e.g., Matthew Evangelista, *Unarmed Forces: The Transnational Movement to End the Cold War* (Ithaca, NY: Cornell University Press, 1999), 3-44; Joseph Rotblat, *Scientists in the Quest for Peace: A History of the Pugwash Conferences* (Cambridge, MA: The MIT Press, 1972).

new disarmament study group comprised of scientists in the Boston area. “I was involved in the original sin,” Feld would tell an audience in 1981, “and I have spent a large part of the rest of my life atoning.”¹³ Feld received his Ph.D. from Columbia University in 1939. He was working as a teaching assistant for Enrico Fermi when the legendary physicist asked him to help out with a new project in Fermi’s Columbia laboratory—some of the first experiments on self-sustaining nuclear fission. By 1941 Feld had taken a leave from graduate school and was on his way to work with Fermi and Hungarian nuclear pioneer Leo Szilard on the first nuclear reactor, housed at the Metallurgical Laboratory at the University of Chicago. In 1943 he was off to the new installation at Oak Ridge, Tennessee, to apply himself to reactor design and isotope separation. And by 1944 he had been appointed Assistant Group Leader of Critical Assemblies at Los Alamos, thus completing a whirlwind tour through the Manhattan Project. Not long after the plutonium implosion bomb he had helped engineer detonated over Nagasaki, he returned to Columbia, wrapped up his PhD, and was appointed an instructor of physics at MIT in 1946.¹⁴

Following the war, Feld’s “atonement” took the form of participation in the atomic scientists’ movement, pressing for the civilian and international control of atomic energy. He even gave up six months of his promising young physics career for a stint in Washington, DC, lobbying and testifying on behalf of the FAS in 1946. In Cambridge he organized the Greater Boston Branch of the FAS (one day he would be vice president at the national level, and editor of the organization’s mouthpiece, the *Bulletin of the Atomic Scientists*). Feld’s feelings about

¹³ Clifford J. Levy, “Bernard Feld, who led scientists in fighting arms race, dies at 73,” *New York Times* (20 February 1993).

¹⁴ “Preliminary Inventory to the Papers of Bernard Taub Feld,” Massachusetts Institute of Technology, Institute Archives and Special Collections, available at <http://libraries.mit.edu/archives/research/collections/collections-mc/mc167.html>; “Professor Bernard Feld dies at 73,” MIT News, available at <http://web.mit.edu/newsoffice/1993/feld-0224.html>.

nuclear weapons, and the nuclear arms race, were unambiguous: the arms race was a moral disaster, unthinkable dangerous, and in need of drastic correction through disarmament.¹⁵

In 1957 Feld and much of the intellectual community associated with the Federation were in a bleak mood; a feeling of exhaustion with the failed negotiations prevailed. As Harvey Brooks (dean of the engineering school at Harvard and a member of the FAS executive committee) wrote to Feld, “I think we must face the fact that the chance of reaching any disarmament agreement with the Russians probably disappeared for some time to come with the advent of Sputnik and the demonstration of Russian power.”¹⁶ Similarly David Cavers of the Harvard Law School, a regular commentator on disarmament issues in the 1950s, told Feld that “after ten years of keeping a low flame of optimism alive I have grown very pessimistic as to the possibility of getting support for arms control measures in either this country or the USSR in the next few years.”¹⁷ Feld’s former boss on the Manhattan Project, Robert Oppenheimer, was perhaps the most pessimistic of all: “I am convinced that on the assumption of irreconcilable hostility between major powers, of a prevailing secrecy on military, technical, and strategic matters, and in the face of rapidly changing military technology, meaningful disarmament is not attainable by international agreement.”¹⁸

In mid-August 1957 the members of the FAS Boston Branch met at Endicott House, a mansion in the leafy countryside outside the city. The group talked disarmament. David Frisch, another MIT nuclear physicist and veteran of wartime bomb work at Los Alamos, pointed out

¹⁵ On the atomic scientists’ movement see Alice Kimball Smith, *A Peril and a Hope: The Scientists’ Movement in America, 1945-47* (Chicago: University of Chicago Press, 1965); Jessica Wang, *American Science in an Age of Anxiety: Scientists, Anticommunism, and the Cold War* (Chapel Hill, NC: University of North Carolina Press, 1999).

¹⁶ Harvey Brooks to Bernard T. Feld, 5 December 1957, Box 11, Folder 97 “Federation of American Scientists, Disarmament Study Committee, 1956-1959 (1 of 3),” *BTF*.

¹⁷ David F. Cavers to Bernard T. Feld, 12 December 1957, Box 11, Folder 97 “Federation of American Scientists, Disarmament Study Committee, 1956-1959 (1 of 3),” *BTF*.

¹⁸ Robert Oppenheimer to Bernard T. Feld, 2 December 1957, Box 11, Folder 97 “Federation of American Scientists, Disarmament Study Committee, 1956-1959 (1 of 3),” *BTF*.

that apart from the office of Harold Stassen, Eisenhower's special assistant for disarmament affairs, no group in or outside of government was seriously studying the question of disarmament. He argued that the group should carry out research for disarmament in the same way that MIT had often sponsored work for the Defense Department: by conducting a summer study. The FAS was "an appropriate group to organize such a study," he felt, bringing "some technical competence to bear on those problems that required such competence."¹⁹

So they set up a committee to plan the effort. Paul Doty was the newly elected chairman of the FAS and a mid-career Harvard biochemist. As a graduate student in chemistry at Columbia in the early 1940s, he had participated in early Manhattan Project experiments on heavy water, and studied with the physical chemist Harold Urey and the physicists Edward Teller and Enrico Fermi.²⁰ John Edsall was Doty's biochemist colleague at Harvard and an old friend of J. Robert Oppenheimer from college days. (They had co-edited the Harvard Liberal Club's short-lived publication, *The Gad-Fly*.)²¹ Another chemist named Charles Coryell, on the faculty at MIT, joined up, too. He'd been head of the Fission Products Section for the Manhattan Project at the Met Lab and Oak Ridge.²² Along with Feld and Frisch, the MIT physicists M. Stanley Livingston (co-inventor of the cyclotron with Ernest Lawrence in the 1930s, and veteran of wartime operations research with the Navy) and Martin Deutsch (during the war a member of

¹⁹ D.G. Brennan to Members of the Special Committee on the Disarmament Study, 3 October 1957, Box 11, Folder 97 "Federation of American Scientists, Disarmament Study Committee, 1956-1959 (1 of 3)," *BTF*; "Tentative Outline for Summer Study Project on the Problems of Disarmament," 8 October 1957, Box 11, Folder 97 "Federation of American Scientists, Disarmament Study Committee, 1956-1959 (1 of 3)," *BTF*. On Endicott House, see Elizabeth Durant, "A 'rare and unusual gem'," *MIT Technology Review*, available at <http://www.technologyreview.com/article/405060/a-rare-and-unusual-gem/>.

²⁰ "Paul Doty," Harvard Belfer Center for Science and International Affairs, available at http://belfercenter.ksg.harvard.edu/experts/134/paul_doty.html; Richard L. Garwin and E. Peter Geiduschek, "Paul M. Doty, 1920–2011," *Biographical Memoirs* (National Academy of Sciences, 2013), available at <http://www.nasonline.org/publications/biographical-memoirs/memoir-pdfs/doty-paul.pdf>.

²¹ Paul Doty, "John T. Edsall," *Proceedings of the American Philosophical Society* 149, no. 1 (March 2005): 89-92.

²² "Guide to the Charles D. Coryell Papers 1945-1959," The University of Chicago Library, available at <http://www.lib.uchicago.edu/e/scrc/findingaids/view.php?eadid=ICU.SPCL.CORYELL&q=charles%20coryell>.

Emilio Segre's fission physics group at Los Alamos, and later the discoverer of positronium) made the list, too.²³ Only two non-veterans of wartime projects joined the committee: Louis Osborne (another MIT nuclear physicist), and Donald Brennan, the part-time secretary of the FAS Boston Branch who was working on a mathematics dissertation at MIT, all while holding down a position at the Air Force-funded MIT Lincoln Laboratory.

A few weeks later, on the eve of the launch of the Soviet Sputnik satellite in early October 1957, the group drafted and circulated a proposal for a summer study on disarmament. They said they hoped the study would "provide a basis for some progress in subsequent international negotiations."²⁴ The Boston group's proposal struck a chord when Feld sent it off to the FAS executive committee. Respondents saw a need for sophisticated technical studies of the disarmament problem—both by the government and by outsider groups. Jerome Wiesner, an MIT professor of electrical engineering as well as a government advisor recently appointed to PSAC, told Feld that he was "heartily in favor" of a disarmament study. "As a matter of fact, I have been urging that such a group be set up officially by the President's Office or the State Department," he added. Wiesner's enthusiasm for disarmament studies may have seemed curious for someone who, only the previous month, had completed work on the high-level Gaither Panel, which called for a huge buildup of U.S. intercontinental and intermediate-range nuclear ballistic missiles.²⁵ Hubert Humphrey, champion of the disarmament cause on Capitol Hill, wrote to Feld that a study of the technical issues goes "right to the heart of the problem." Humphrey believed that his own disarmament subcommittee of the Senate Foreign Relations Committee lacked key

²³ Ernest D. Courant, "Milton Stanley Livingston," *Biographical Memoirs, Volume 72* (Washington, DC: National Academy Press, 1997), 264-287; "Martin Deutsch, MIT physicist who discovered positronium, dies at 85," MIT News, available at <http://web.mit.edu/newsoffice/2002/deutsch.html>.

²⁴ D.G. Brennan to Members of the Special Committee on the Disarmament Study, 3 October 1957, Box 11, Folder 97 "Federation of American Scientists, Disarmament Study Committee, 1956-1959 (1 of 3)," *BTF*.

²⁵ See U.S. Office of Defense Mobilization, Science Advisory Committee, Security Resources Panel, "Deterrence and Survival in the Nuclear Age," 7 November 1957 ["Gaither Report"], 8-9. Available online from the National Security Archive, <http://www2.gwu.edu/~nsarchiv/NSAEBB/NSAEBB139/nitze02.pdf>.

information about “the various forms of inspection.” And Henry Kissinger at the Center for International Affairs reassured Feld that disarmament was “a topic in which I am personally very interested and I think it would be a useful thing to do.”²⁶

Still, other FAS members worried that by focusing so heavily on the technical issues, Feld and his group risked degrading the impact of their work. The Boston disarmament committee, composed exclusively of scientists, believed that technical characteristics preceded and conditioned politics: science and technology showed what lay within the range of feasible political choice. But other disarmament advocates saw things nearly the other way around. As Robert Oppenheimer (himself no stranger to the politics of nuclear weapons) wrote to Feld, “I have always been disturbed by the separation between political and technical considerations which you contemplate. The requirements on a system for regulating armaments are determined by a political and strategic context, and the extent to which they can be fulfilled by technical means must be judged against the background of these requirements.”²⁷

In a similar key, Samuel Huntington, a young assistant professor in Harvard’s Department of Government and an expert on military-civilian relations, told Feld that “any analysis of the technical problems of disarmament will have to rest upon certain assumptions as to the political conditions which will prevail.” Huntington recommended that Feld make his

²⁶ Hubert H. Humphrey to Bernard T. Feld, 26 December 1957, Box 11, Folder 97 “Federation of American Scientists, Disarmament Study Committee, 1956-1959 (1 of 3),” *BTF*; Henry Kissinger to Bernard T. Feld, 17 December 1957, Box 11, Folder 97 “Federation of American Scientists, Disarmament Study Committee, 1956-1959 (1 of 3),” *BTF*. Kissinger’s comment was also curious, when paired with his newly published book *Nuclear Weapons and Foreign Policy*. There he contended that “the horrors of nuclear war are not likely to be avoided by a reduction of nuclear armaments.” See Henry Kissinger, *Nuclear Weapons and Foreign Policy* (New York: Harper, 1957), 211.

²⁷ Robert Oppenheimer to Bernard T. Feld, 2 December 1957, Box 11, Folder 97 “Federation of American Scientists, Disarmament Study Committee, 1956-1959 (1 of 3),” *BTF*.

“political assumptions explicit... Otherwise there is the danger of getting off into technical analyses of only marginal relevance or significance.”²⁸

And yet with the right balance between the technical and the political, Feld and his Boston committee might fulfill what some of his correspondents regarded as a clear need for critiques from outside the official channels. Huntington believed it was “quite obvious from the recent history of our disarmament policy that very little can be expected along this line from the agencies of the government,” so that responsibility for such policy formulation “should be assumed by specially qualified nonofficial groups.”²⁹ And Francis Bator, then teaching in MIT’s Department of Economics and Social Science, argued that it was “vital, on this of all problems, that there be a growing body of outsider opinion sufficiently literate in the technicalia effectively to take on this role, as it were, of a ‘loyal opposition’.” Bator expanded on the relationship between outsiders and insiders, admitting that “the insider-outsider distinction is, of course, overly sharp.” The U.S. had gotten itself “into the uncomfortable box of having to accept (reject) insider views largely on trust (mistrust),” and even when the official view “has embedded in it highly debatable value- and risk-preference judgments (concealed by vague allusion to classified technicalia), there [has not] existed sufficient outsider competence for an effective challenge to ‘Daddy-knows-best’.” Bator wanted more muscular expertise from those not in government positions but endowed with insider knowledge—people who had “near-insider competence on the essentials” but “are not now involved in the machinery”—to mount more robust critiques of government positions.³⁰

²⁸ Samuel P. Huntington to Bernard T. Feld, 18 December 1957, Box 11, Folder 97 “Federation of American Scientists, Disarmament Study Committee, 1956-1959 (1 of 3),” *BTF*.

²⁹ Samuel P. Huntington to Bernard T. Feld, 18 December 1957, Box 11, Folder 97 “Federation of American Scientists, Disarmament Study Committee, 1956-1959 (1 of 3),” *BTF*.

³⁰ Francis Bator to Bernard T. Feld, 20 December 1957, Box 11, Folder 97 “Federation of American Scientists, Disarmament Study Committee, 1956-1959 (1 of 3),” *BTF*. And this was from a man who would, in 1964, become a deputy national security advisor to Lyndon Johnson.

For the time being, Feld was committed to exploiting his group's specific competence in science and technology. Only the criticism of potential funders of the committee's work—especially the Ford and Rockefeller Foundations, which in the postwar era had become the dominant supporters of social science research in the United States—would eventually drive Feld and his committee to incorporate expertise from a much wider range of disciplines.³¹

Feld's committee began regular Saturday morning meetings in January 1958. Debate focused on what the group should study first. Initially the group considered suggestions made by Richard Leghorn, a Boston-area defense consultant who was leading his own study of the disarmament problem for the nonprofit National Planning Association.³² Joining Leghorn on his study (among roughly 25 others) were Boston disarmament committee participants David Cavers and David Frisch.³³ Leghorn (an MIT-trained engineer) and his committee of physical and social scientists argued for the implementation of a vast reconnaissance program in order to cooperatively monitor both American and Soviet nuclear deployments.³⁴ The emphasis on surveillance wasn't accidental. Leghorn had recently created a company in Boston called ITEK, publicly billed as an "information management" firm but actually the prime contractor for the photographic reconnaissance equipment to be launched on the CIA's new spy satellites. In the late 1950s Leghorn frequently urged (in the *Bulletin of the Atomic Scientists* and elsewhere) that

³¹ The federal government remained the dominant source of support for the natural sciences during the Cold War. On the patronage of the social sciences in the U.S., see Theodore Porter and Dorothy Ross, eds., *The Modern Social Sciences* (New York: Cambridge University Press, 2003); Hunter Crowther-Heyck, "Patrons of the revolution: Ideals and institutions in postwar behavioral science," *Isis* 97 (2006): 420-446; and Mark Solovey, *Shaky Foundations: The Politics-Patronage-Social Science Nexus in Cold War America* (New Brunswick, NJ: Rutgers University Press, 2013).

³² The National Planning Association was created in the 1930s to join business and labor interests in the pursuit of private economic planning, in place of New Deal-style ambitions for a "planned economy." Like its world-federalist cousins, the NPA had international security preoccupations, too, and now and again issued a "planning pamphlet" on foreign policy and world affairs. The National Planning Association's board of trustees ran the gamut from William Paley, Chairman of CBS, to Walter Reuther, head of the United Auto Workers union.

³³ NPA Special Project Committee on Security Through Arms Control, *1970 Without Arms Control: Implications of Modern Weapons Technology*, Planning Pamphlet No. 104 (Washington, DC: The National Planning Association, 1958).

³⁴ NPA Special Project Committee on Security Through Arms Control, *1970 Without Arms Control*.

the U.S. and the Soviet Union should pursue a joint “rational world security system.” He saw a ban on missile tests as particularly crucial—even more so than the nuclear test ban—since neither side had yet deployed intercontinental missiles in its arsenals.³⁵

Thus Leghorn commended Feld and his disarmament committee to a study of the missile test and inspection problem. Over the course of the next several months, they took him up on his proposal.³⁶ At that first meeting, the Boston disarmament committee heard a presentation on the topic by Walter J. Levison, one of Leghorn’s ITEK colleagues, who worried that launch-ready missiles could easily be concealed from overhead photography. David Z. Robinson, assistant director of research at the defense contractor Baird-Atomic, told the group about infrared techniques for detecting the heat from a ballistic missile in the boost phase of flight, during the firing of its rocket engine. And the committee’s junior partner, Donald Brennan, began work on calculations that showed how missile tests could be detected with a global network of radar stations.³⁷

³⁵ As a member of a special military task unit in 1946, Leghorn had photographed the Operation Crossroads atomic tests on Bikini Atoll. On his way to the South Pacific, reading a copy of the U.S. Strategic Bombing Survey, Leghorn experienced what he later described as a personal epiphany. The purpose of reconnaissance in the atomic age would have to undergo a dramatic revolution, from “traditional targeting and damage assessment” to “one focused on warning indicators...and an enemy’s capability to launch an attack.” He had been a consultant to Harold Stassen, Eisenhower’s disarmament attaché, and advocated for the uses of reconnaissance in disarmament inspection. “Richard S. Leghorn, Biography,” available at http://richardleghorn.com/military_history.html; also see Philip Taubman, *Secret Empire: Eisenhower, the CIA, and the Hidden Story of America’s Space Espionage* (New York: Simon & Schuster, 2003); Jonathan E. Lewis, *Spy Capitalism: Itek and the CIA* (New Haven, CT: Yale University Press, 2002).

³⁶ See Richard S. Leghorn, “The problem of accidental war,” *Bulletin of the Atomic Scientists* 14, no. 6 (June 1958): 205-209. Leghorn’s recommendations to the disarmament study group are recorded in D.G. Brennan, Memo to Disarmament Study Committee members, 18 January 1958, Box 12, Folder 99 “Federation of American Scientists, Disarmament Study Committee, 1956-1959 (3 of 3),” *BTF*.

³⁷ Robinson actually wanted to have his name removed from the study, unsure of how his disarmament work might look to his employer. He told Feld, “I...feel that direct attribution of these statements to me, even though they are unclassified, might cause some repercussions which I would like to avoid.” See D.G. Brennan, “Memo to Disarmament Study Committee members, and special guests invited to the Committee meeting on Saturday, January 18,” 14 January 1958; Bernard T. Feld, “Summary of meeting, Saturday, January 18, 1958”; and David Z. Robinson to Bernard T. Feld, 17 February 1958, Box 12, Folder 99 “Federation of American Scientists, Disarmament Study Committee, 1956-1959 (3 of 3),” *BTF*; D.G. Brennan, “The Detection of High-Altitude Missile Tests,” Box 11, Folder 98 “Federation of American Scientists, Disarmament Study Committee, 1956-1959 (2 of 3)”. Also see Lewis, *Spy Capitalism*, 75-76. The proposal for a global network of observing posts was similar to the recent ideas of an

As the group's work progressed over the coming months, Feld began to reach out to other thinkers interested in the disarmament problem. When he caught wind of a study at Columbia University's Institute of War and Peace Studies directed by the economist Seymour Melman, Feld asked Melman to travel up to Cambridge to talk to the Boston committee. Melman gladly obliged and, after hearing Brennan's and Levison's presentations, invited them to contribute papers to his forthcoming collection *Inspection for Disarmament*. The book would become essential reading for anyone concerned about the nuclear arms race in the late 1950s.³⁸

The coming year presented a mix of opportunities and struggles. An exciting chance for new work came when the FAS's Eugene Rabinowitch asked the Boston committee to prepare a report on "technical problems of disarmament controls" for the third Pugwash Conference in Vienna in September 1958. Since the institutional backing for American participation in Pugwash would be provided by the American Academy of Arts and Sciences, conveniently headquartered in the Boston area, Feld approached contacts at the Academy for logistical support for a summer study to prepare the report. In March 1958 the Council of the AAAS voted to sponsor Feld's group and its work on the "technical problems of arms limitation." In the course of preparations for Pugwash the group produced the first published study of the "Nth country

assistant professor of physics at Columbia University named Jay Orear. Orear had written a paper in recent months claiming that a nuclear test ban agreement could be monitored by a network of control stations, roughly twenty-five in the Soviet Union and seven in the U.S. (a number remarkably close to the result that a PSAC panel, chaired by Hans Bethe, would come up with in March), spaced no more than 300 miles apart. Each was to be equipped with sensors to detect seismic vibrations from underground and underwater tests, acoustic waves from atmospheric explosions, and electromagnetic radiation from high-altitude and outer space tests. These sensors would supplement the traditional technique of air sampling at high altitudes to detect radioactive material. Orear's paper was published as "The Detection of Nuclear Weapons Testing" in Seymour Melman, ed., *Inspection for Disarmament* (New York: Columbia University Press, 1958), 85-99.

³⁸ Bernard T. Feld to Seymour Melman, 6 February 1958; and Seymour Melman to Bernard T. Feld, 17 February 1958, Box 12, Folder 99 "Federation of American Scientists, Disarmament Study Committee, 1956-1959 (3 of 3)," *BTF*; Seymour Melman, ed., *Inspection for Disarmament* (New York: Columbia University Press, 1958). Brennan also published a version of his paper in the *Bulletin*: D. G. Brennan, "Why outer space control?," *Bulletin of the Atomic Scientists* 15, no. 5 (1959): 198-202.

problem” (today it would be called nonproliferation), which argued for a link between the development of civilian nuclear power and the development of nuclear weapons programs.³⁹

But the group’s struggle to secure funding endured. The committee’s hopes for a major summer study would meet with frustration when its bid for a grant from each of the Ford, Carnegie, and Rockefeller Foundations all fell flat in 1958. Their negotiations with the foundations—especially with Rockefeller—are revealing, however, suggesting a deep tension between the technical and the political in possible approaches to the disarmament problem. It was this issue that would ultimately drive Feld’s group (and the larger collection of thinkers that would ultimately develop, in 1960, from Feld’s disarmament committee) to widen its disciplinary embrace, incorporating both the physical and the social sciences.

* * * *

In May of 1958, Feld and colleagues received the bad news that the Ford Foundation had turned down their proposal. Feld promptly began working his contacts, cabling Hubert Humphrey to ask the Senator to impress upon the Rockefeller Foundation the worthiness of the Academy project. Humphrey obliged, writing to Foundation president Dean Rusk that such studies were essential “as we prepare to negotiate an agreement on a possible control and reduction of armaments.”⁴⁰ Warren Weaver, Vice President of Natural and Medical Sciences at the Rockefeller Foundation, telephoned David Frisch, asking him whom the study would be directed at, and how the Cambridge group would make sure that the technical questions

³⁹ Howard Simons, “World-wide capabilities for production and control of nuclear weapons,” *Daedalus* 88, no. 3 (Summer 1959): 385-409; William C. Davidon, Christoph Hohenemser, and Marvin I. Kalkstein, *The Nth Country Problem: A World Wide Survey of Nuclear Weapons Capability*, Planning Pamphlet No. 108 (Washington, DC: The National Planning Association, 1959).

⁴⁰ Hubert H. Humphrey to Dean Rusk, 16 May 1958, Box 2, Folder 12 “AAAS, Committee on Technical Problems of Arms Limitation 1958-1960, 1/3,” *BTF*.

considered would be placed within the proper political context.⁴¹ Responding by letter, Feld and Frisch boasted of the group's "close contact with most members of the President's Scientific Advisory Committee...Among these H.A. Bethe and J.B. Wiesner have been among our most active advisors." And he felt the need to reassure Weaver that the study's membership had been widened beyond the natural sciences to incorporate jurists, economists, and political scientists.⁴² Deciding that the proposal should be adjudicated as part of the Foundation's new program in international relations, Weaver bumped it over to the Social Science Division where Kenneth W. Thompson would oversee the file.⁴³

Thompson drew up a series of tough and pessimistic questions. To start with, "Does this group know about the immense literature and thought that has gone in to the whole disarmament question?" It was a literature intimately known to Thompson, along with most everything written within the new mid-century "science" of politics. Back in 1955, he had organized a Rockefeller Foundation-funded conference on international relations—an event now central to the creation story of international relations theory—at which most of the U.S. founders of the theory of geopolitical realism (including Hans Morgenthau, Reinhold Niebuhr, and William T.R. Fox) delivered important papers.⁴⁴ In his office in New York, staring at the proposal by Feld and Frisch, Thompson again asked: "Is there any evidence that this study is, or could be, geared to an approach to political problems? For example, could it be linked in any way with various disarmament suggestions?" And "how can one assure that this effort will not repeat the

⁴¹ Inter-Office Correspondence from Warren Weaver to Dean Rusk, 13 May 1958, Record Group 1.2, Series 1.0002, Box 476, Folder 4070, "American Academy of Arts and Sciences, Arms Limitation (Conference), 1958," *RF*.

⁴² Bernard T. Feld and David H. Frisch to Warren Weaver, 8 May 1958, Box 2, Folder 12 "AAAS, Committee on Technical Problems of Arms Limitation 1958-1960, 1/3," *BTF*.

⁴³ "WW Diary, May 6, 1958," Record Group 1.2, Series 1.0002, Box 476, Folder 4070, "American Academy of Arts and Sciences, Arms Limitation (Conference), 1958," *RF*.

⁴⁴ On Thompson's coordinating and funding activities in the new field of international relations theory, see Nicolas Guilhot, ed., *The Invention of International Relations Theory: Realism, the Rockefeller Foundation, and the 1954 Conference on Theory* (New York: Columbia University Press, 2011).

melancholy story of unattainable proposals which have appeared so frequently in the *BAS* and elsewhere?”⁴⁵

The proposal, and the opposition between the technical and the political, was soon debated at the top of the Foundation. Dean Rusk telephoned former Secretary of Defense Robert Lovett, who told Rusk that “because of very serious clashes between personalities in Washington a really good independent study on technical factors bearing on disarmament could be very useful, providing it is really free of political taint.” But his warning was firm: “If scientists get political issues tangled up in their technical inquiry or concern themselves with political issues...their work will lose its usefulness.” Rusk responded favorably and recommended that Rockefeller fund a suitably modified version of the proposal—as long as “we can fully satisfy ourselves that political considerations will not influence the work.”⁴⁶

Thompson would have none of it. He remained wedded to the idea that disarmament was fundamentally a political subject. A few days later he traveled up to Cambridge and grilled Feld and Frisch at MIT over the course of a five-hour meeting. He asked them how they proposed to conduct their study without access to classified information; they responded that sufficient information existed in the public domain and, even better, Hans Bethe, James Killian, and Jerome Wiesner would be “looking over [their] shoulders as they go along.” He asked them about redundant overlap with the Melman study; they replied that the Columbia project had been

⁴⁵ Inter-Office Correspondence from Kenneth W. Thompson to LCD, 15 May 1958, Record Group 1.2, Series 1.0002, Box 476, Folder 4070, “American Academy of Arts and Sciences, Arms Limitation (Conference), 1958,” *RF*.

⁴⁶ LCD to KWT, 23 May 1958, Record Group 1.2, Series 1.0002, Box 476, Folder 4070, “American Academy of Arts and Sciences, Arms Limitation (Conference), 1958,” *RF*.

“a rather diffuse collection of writings”—a study of treaty evasion techniques. The Boston group didn’t want to study evasion, but positive techniques of arms control.⁴⁷

“The group,” they promised, “would aim to discover what was technically reasonable and feasible. They would then explore whether this was feasible politically and join with social scientists to make such exploration.” When Thompson pushed them on the “question of commitment,” the political leanings of the committee, Feld and Frisch nearly tripped over themselves trying to put distance between their group and a recently circulated public letter by Linus Pauling calling for an end to nuclear testing. Though Feld and Frisch had added their signatures (along with those of more than 2,000 U.S. scientists), they said the Pauling letter was “messily formulated and not very persuasive.” “Most good scientists,” they assured Thompson, “are not as fuzzy minded as Pauling, as overconfident as Teller, and read the papers better than Kissinger.” Their group was young, sharp, “objective and hard headed”: “None of the romance of the thirties has wiped off on them.”⁴⁸

Thompson called Feld and Frisch the next day from New York and informed them that under their current proposal, the answer from Rockefeller was “no.” He was, however, willing to consider a grant-in-aid for a much smaller consultation conference, which Feld and Frisch could use to reorient their goals and carve out two or three focused studies for later in the summer.⁴⁹

That night the two MIT physicists met with their committee to talk over Thompson’s counterproposal, and agreed to hold a preliminary planning meeting first, in early June.

⁴⁷ Interviews: KWT of Professor B.T. Feld and Professor D.H. Frisch, 27 May 1958, Cambridge, Massachusetts, Record Group 1.2, Series 1.0002, Box 476, Folder 4070, “American Academy of Arts and Sciences, Arms Limitation (Conference), 1958,” *RF*.

⁴⁸ Interviews: KWT of Professor B.T. Feld and Professor D.H. Frisch, 27 May 1958, Cambridge, Massachusetts, Record Group 1.2, Series 1.0002, Box 476, Folder 4070, “American Academy of Arts and Sciences, Arms Limitation (Conference), 1958,” *RF*. On Pauling’s anti-testing campaign see, e.g., Benjamin P. Greene, *Eisenhower, Science Advice, and the Nuclear Test-Ban Debate, 1945-1963* (Stanford: Stanford University Press, 2007), esp. 122-127.

⁴⁹ Interviews: KWT of Professors Frisch and Feld, 28 May 1958, Record Group 1.2, Series 1.0002, Box 476, Folder 4070, “American Academy of Arts and Sciences, Arms Limitation (Conference), 1958,” *RF*.

Thompson agreed, funding it modestly at \$1,200 (compared with the nearly \$50,000 the group had asked for initially). The committee and several guests met over a weekend that month at the Academy headquarters at Brandegee House, adjacent to the bucolic Arnold Arboretum in Brookline, near Boston. Several of the PSAC people joined, including Hans Bethe, Jerome Wiesner, and Edward Purcell. So did a handful of social scientists, including the MIT economists Francis Bator and S.S. Alexander, and the newly hired Harvard economist Carl Kaysen.⁵⁰

A few days after the planning study, Feld and Frisch wrote to Dean Rusk to express their confidence in the project. “We were no more successful than previously in classifying this proposed study as political or physical science,” they wrote, “but it became clear that the largest part of the work would be done by physical scientists, and to a lesser extent, economists, with advice from and final discussions with political scientists.” They were sticking to their guns on the disciplinary question, but the move was miscalculated; the reaction from Rockefeller was swift and final. In a telegram two days later, Dean Rusk told the Boston group that the Foundation would not give them a dollar more.⁵¹ They were crushed. Donald Brennan managed to scrounge up a few personal donations later that summer—from the Boston Brahmin Henry B. Cabot, the Harvard chemist Albert Sprague Coolidge, and Josephine Pomerance, a disarmament activist affiliated with the Women’s International League for Peace and Freedom. It was just enough to keep the group running on a small-scale basis—an individual study here and there—

⁵⁰ “Council Minutes of 13 May 1959, Report of the Committee on Technical Problems of Arms Limitation,” Box 11, Folder 98 “Federation of American Scientists, Disarmament Study Committee, 1956-1959 (2 of 3),” *BTF*; “Tentative List of Participants, Conference on Technical Problems of Arms Limitation, June 7-8, 1958,” Box 12, Folder 99 “Federation of American Scientists, Disarmament Study Committee, 1956-1959 (3 of 3),” *BTF*; “List of Participants, Conference on Technical Problems of Arms Limitation, June 7-8, 1958,” Record Group 1.2, Series 1.0002, Box 476, Folder 4070, “American Academy of Arts and Sciences, Arms Limitation (Conference), 1958,” *RF*.

⁵¹ Bernard T. Feld and David H. Frisch to Dean Rusk, 17 June 1958; and Dean Rusk to Bernard Feld and David Frisch, 23 June 1958, Box 2, Folder 12 “AAAS, Committee on Technical Problems of Arms Limitation 1958-1960, 1/3,” *BTF*.

but nowhere near sufficient to mount the large-scale effort that Feld, Frisch, Brennan and their colleagues had been hoping for.⁵²

They would wait for more than another year before a chance at conducting a full summer study would present itself again.⁵³

Deterrence, Disarmament, and the Question of Stability

In 1955 a panel headed by MIT president James Killian had been specially appointed by Dwight Eisenhower to study the problem of U.S. vulnerability to surprise attack. The report of the so-called “Technological Capabilities Panel,” delivered to the President early that year, stressed the disturbing fact that “for the first time in history, a striking force could have such power that the first battle could be the final battle, the first punch a knockout.” In July that year, during the first summit meeting between American and Soviet leaders in the postwar era (in Geneva), Eisenhower dramatically proposed that each country open its territory to aerial overflight and inspection for the prevention of surprise attack. Though the “Open Skies” plan was celebrated by some as a relaxation of Cold War tensions, the Soviet leadership heaped scorn upon the proposal, denouncing it as propaganda.⁵⁴

In the second half of 1958 the two superpowers (each flanked by a small group of allies) convened semi-formal disarmament talks in Geneva, in the form of two “conferences of experts.” The first, during July and August, concerned the issue of the nuclear test ban. Then, over the course of six weeks from November to December, talks returned to the problem of

⁵² Donald Brennan to Albert Sprague Coolidge, 29 July 1958; Donald Brennan to Henry B. Cabot, 13 August 1958; Donald Brennan to Josephine Pomerance, 19 August 1958, Box 2, Folder 12 “AAAS, Committee on Technical Problems of Arms Limitation 1958-1960, 1/3,” *BTF*.

⁵³ See the materials in Record Group 1.2, Series 1.0002, Box 476, Folder 4070, “American Academy of Arts and Sciences, Arms Limitation (Conference), 1958,” *RF*.

⁵⁴ On “Open Skies” see David Tal, “From the Open Skies Proposal of 1955 to the Norstad Plan of 1960: A plan too far,” *Journal of Cold War Studies* 10, no. 4 (2008): 66-93; and Appleby, “Eisenhower and Arms Control,” 99-106.

surprise attack. Numerous observers at the time (as well as later historians) criticized the U.S. delegation to the surprise attack conference for its narrow focus on technical problems—a worrying trend identified by correspondents of Bernard Feld, long before the meeting itself had taken place.⁵⁵ But the conference did serve an important purpose. It assembled a group of people from diverse backgrounds (some of whom had first met on the Gaither Panel in 1957) and kept them in conversation with one another about a fairly well defined problem. The surprise attack issue focused discussion, in a newly intense way, on the relationship between nuclear deterrence and nuclear disarmament.⁵⁶

Among several of the experts it also prompted a focus on *stability* as a key concept and goal of U.S. policy. For the emerging corps of civilian strategic analysts, stability was a sought-after quality of nuclear deterrence. This idea was especially pronounced in the work of RAND's Albert Wohlstetter, who served as an advisor to the U.S. delegation to the surprise attack conference. Wohlstetter produced his own definitive take on the problem in a report circulated

⁵⁵ Jeremi Suri, "America's search for a technological solution to the arms race: The Surprise Attack Conference of 1958 and a challenge for 'Eisenhower revisionists'," *Diplomatic History* 21, no. 3 (1997): 417-451; Bechhoefer, *Postwar Negotiations for Arms Control*, 464-487. Initial government planning for the conference stressed the importance of disarmament to schemes that would mitigate the threat of surprise attack. As early as July 1958, Presidential science advisor James Killian had argued that arms limitations were essential. Writing to Secretary of State Dulles, Killian claimed that "no reliable system can be devised to provide dependable advance warning of a surprise attack except in conjunction with agreed limitations on weapons numbers or deployment." In August an Interagency Working Group on Surprise Attack, chaired by the Harvard chemist and PSAC member George Kistiakowsky, produced a planning report arguing that the U.S. should couple some limited form of disarmament to an inspection scheme that would provide advanced warning of surprise attack. But as Jeremi Suri has argued, the Defense Department's perpetual hostility to any discussion of disarmament, and Eisenhower's lack of firm leadership, left the U.S. without a strong and coherent negotiating position at the beginning of the conference. The representatives (led by William C. Foster and Kistiakowsky) were unprepared to wade into diplomatic discussions with the Soviets. Instead the Americans, in a recurring theme, focused exclusively on the technical characteristics of an inspection system. The Western and Eastern delegations never got beyond their contradictory terms of reference (the Soviets wanted to negotiate the disarming of Central Europe; the Americans were prohibited from discussing arms limitations), and the conference ended in failure.

⁵⁶ Among the technical and political advisors to the delegation were a contingent from the RAND Corporation (including Albert Wohlstetter, Henry Rowen, and Andrew Marshall), Jerome Wiesner of PSAC, Dalimil Kybal (chief scientist of Lockheed's Missile Systems Division), Richard Garwin of IBM, James Goodby of the Atomic Energy Commission, and the State Department's Julius Holmes and Lawrence Weiler. An even larger group of approximately 100 experts participated in preparatory activities for the surprise attack conference. See Appleby, "Eisenhower and Arms Control," 294; and various correspondence in Box 5, Folder 147 "Safeguards Against Surprise Attack," *JBW*; Suri, "America's search," 434.

just as the conference of experts began, titled “The Delicate Balance of Terror.” Slightly revised for the January 1959 issue of *Foreign Affairs*, the piece was a summation of Wohlstetter’s views on deterrence. He took aim at the assumption that the “atomic stalemate” (equivalent first-strike arsenals of thermonuclear weapons possessed by both the U.S. and the Soviet Union) naturally gave rise to a balance of deterrence. He found this idea not only presumptuous but incredibly dangerous. Wohlstetter clarified an idea that had been bubbling through discussions at RAND for several years: that the source of deterrence in the nuclear age was not a force that would land the first nuclear blow in a war with the Soviet Union, but an invulnerable retaliatory force. Such a secure “strike second” force would be hard to hit—mobile, dispersed, “hardened” by protective sheltering, and on constant alert.⁵⁷

Thomas Schelling, Wohlstetter’s colleague at RAND during the year 1958-59, had also participated in planning activities for the conference. An economist by training, he had spent five years after the war working on the Marshall Plan as part of the Economic Cooperation Administration, followed by another five as an economics professor at Yale. Schelling would walk away from his time at RAND one of the foremost theorists of deterrence, expressed in his preferred framework of coercive bargaining.⁵⁸

In a RAND research paper titled “Surprise Attack and Disarmament,” Schelling echoed Wohlstetter in distinguishing between mere numerical balance and the stability of the balance.

⁵⁷ Albert Wohlstetter, “The Delicate Balance of Terror,” RAND Paper P-1472 (6 November 1958; Revised December 1958). Later published as Albert Wohlstetter, “The Delicate Balance of Terror,” *Foreign Affairs* 37, no. 2 (January 1959): 211-234. In many ways the essay was a distillation of Wohlstetter’s work at RAND during the 1950s—studies that had sprung from an almost fanatical obsession with the vulnerability of U.S. strategic forces. In the years since his famed RAND study of the Strategic Air Command’s overseas bomber bases, he had returned time and again to the issue of vulnerability, making his case tirelessly in detailed reports and countless Pentagon briefings. Now he had updated his message for the missile age. On Wohlstetter’s obsession with vulnerability and his efforts promoting his views in elite defense circles, see Fred Kaplan, *The Wizards of Armageddon* (Stanford, CA: Stanford University Press, 1991 [1983]), esp. 86-110 and 111-123.

⁵⁸ Robert Ayson, *Thomas Schelling and the Nuclear Age: Strategy as Social Science* (New York: Frank Cass, 2004). It seems clear that the second of Schelling’s surprise attack papers from 1958 was deeply influenced by Wohlstetter. Ayson points out that Schelling had arrived at RAND in September 1958 with the first of the papers, “The Reciprocal Fear of Surprise Attack,” already in hand. See Ayson, 62.

Schelling relied heavily on the distinction between first- and second-strike forces in this definition of stability. “The situation is symmetrical but not stable when either side, by striking first, can destroy the other’s power to strike back; the situation is stable when either side can destroy the other whether it strikes first or second.” Thus stability attached to a scenario where neither side could derive advantage from launching a surprise attack (for “fear of being a poor second for not going first”) under any circumstances.⁵⁹

Schelling found the various disarmament proposals of the 1950s a decidedly mixed bag, “some ingenious and some sentimental.” For him, the disarmers had mistaken disarmament as an end in itself; they had assumed that more weapons led to greater risk of war. As Schelling was quick to point out, in many conceivable scenarios, having more nuclear weapons led to *less* risk of war. The crucial point was that nuclear forces were much harder for the enemy to hit than were cities. If an attacker hoped to disarm a defender in a preemptive strike, the size of the necessary first-strike force would have to increase *exponentially* as the size of the defender’s arsenal. The more missiles the defender had at the start of the conflict, the larger was the proportion of missiles the attacker would have to fire to destroy them. As Schelling put it, “a limitation on the number of missiles would appear to be more stabilizing, the larger the number

⁵⁹ Thomas C. Schelling, “Surprise Attack and Disarmament,” RAND P-1574 (10 December 1958). A condensed version was reprinted as “Surprise attack and disarmament,” *Bulletin of the Atomic Scientists* 15, no. 10 (December 1959): 413-418, as well as in Thomas C. Schelling, *The Strategy of Conflict* (Cambridge, MA: Harvard University Press, 1960), 230-254. Schelling in 1958 was beginning to formulate a concept that analysts would later generalize as “crisis stability.” See, for example, Charles Glaser, “Why even good defenses may be bad,” *International Security* 9, no. 2 (1984): 92-123. Robert Ayson has also made the compelling claim that much of Schelling’s strategic thought relies heavily on a concept of stability derived from his earlier experiences with international negotiation and bargaining (as part of the postwar Marshall Plan), as well as his subsequent intensive studies of bargaining behavior. For Schelling, deterrence was a kind of bargain in which the price of a breach of contract was nuclear war. He thought of stability in much the same way that he imagined the robustness (or fragility) of a bargaining agreement. Ayson also endeavors to put Schelling in conversation with trends in mid-century social and systems sciences. See Ayson, *Thomas Schelling and the Nuclear Age*, esp. 52-86 and 160-196. For a more sustained analysis of Schelling’s thought within the context of rational choice theory, see S.M. Amadae, *Rationalizing Capitalist Democracy: The Cold War Origins of Rational Choice Liberalism* (Chicago: University of Chicago Press, 2003), esp. Chapter 2, “Kenneth J. Arrow’s *Social Choice and Individual Values*,” 83–132; and Paul Erickson, Judy L. Klein, Lorraine Daston, Rebecca Lemov, Thomas Sturm, and Michael D. Gordin, *How Reason Almost Lost Its Mind: The Strange Career of Cold War Rationality* (Chicago: University of Chicago Press, 2013).

permitted.” This conclusion ran against the grain of traditional disarmament, for it suggested that possessing larger arsenals (of a “good,” invulnerable sort of weapon) could enhance the stability of deterrence, not degrade it.⁶⁰

Other experts, however, had drawn very different conclusions from the experience of thinking about surprise attack. Jerome Wiesner, who directed the expert staff at the surprise attack conference, was perhaps most important among them. Wiesner admitted in his post-conference report that nuclear deterrence was an unavoidable necessity. As he put it, the strengthening of mutual deterrence was “implicit in any proposal to reduce the danger of surprise attack by the establishment of an inspection and observation system, whether it involves limitations upon weapons systems or not.”⁶¹ Indeed if retaliatory forces were made secure enough, mutual deterrence would “arise naturally” from the weapons themselves. But this was an incomplete, even repugnant, guarantee of security in Wiesner’s eyes. “It would almost certainly lead to a tremendous and costly production race and this, in turn, would result in a heightening of tensions and so increase the danger of an accidental war.”⁶² Wiesner, a member of the FAS and an occasional participant in Bernard Feld’s disarmament committee back at MIT, had strong disarmament convictions despite his high-level role in planning for nuclear war in the missile age.

⁶⁰ Schelling, “Surprise Attack and Disarmament,” 13-14. The risk of inadvertent or accidental nuclear war (another pet subject that Schelling touched on in this paper and would develop in the future) was also understood by some to offer a good reason for the elimination of nuclear weapons. But for Schelling such arguments did not persuade; the question for him was not one of quantity but of capability and purpose. Such themes would dominate Schelling’s writings about disarmament and the arms race in this period. In a 1960 article on the relationship between accidental war and arms control, for example, Schelling would enjoin readers of the *Bulletin of the Atomic Scientists* to “get away from the notion that arms control means simply the elimination of weapons, and search instead for cooperative arrangements that may reduce the likelihood of war.” See Thomas C. Schelling, “Meteors, mischief, and war,” *Bulletin of the Atomic Scientists* 16, no. 7 (1960): 292–296, 300, on 295.

⁶¹ He even referred to deterrence as a “delicate balance of terror,” suggesting he had recently read Wohlstetter’s report. See Jerome Wiesner, “A Report on the 1958 Conference Convened to Study Means of Preventing Surprise Attack,” in Jerome B. Wiesner, *Where Science and Politics Meet* (New York: McGraw-Hill, 1965), 181-208 (quotations on 189 and 190).

⁶² Wiesner, “Report on the 1958 Conference,” 191.

Wiesner and many of his colleagues on the technical staff were not thinking about the surprise attack problem in an abstract or strategic sense. They were thinking about the design of a control system—a literal, engineering control system—that would provide advance tactical warning of a surprise attack (potentially to launch forces quickly in reprisal). Surprise attack posed an extraordinarily complex problem of command, control, and communications.⁶³ In Wiesner's judgment the task was intractably messy—all the more so without arms limitations, given the speed of the new weapons systems. The only sensible alternative, he judged, was to rely on a limited form of mutual deterrence (chiefly by land- and sea-based ballistic missiles) for security, and then to limit the total permissible numbers by formal agreement. This agreement could then be monitored by an inspection and control system whose tolerance was significantly relaxed compared to a hair-trigger surprise-attack warning system.⁶⁴

The idea of a long-term inspection system to monitor arms levels and deployments soon captured Wiesner's imagination, even as he wrote about the thorny tactical warning problem. As he noted to James Killian when submitting his post-conference report to PSAC, "I finished the paper with a strong suspicion that not many years hence an inspection system would only be required to monitor long-term strategic information rather than to provide tactical warning." Wiesner would soon fixate on the idea of stability, as Schelling had, but in a rather different frame of mind.⁶⁵

⁶³ A physicist working at IBM (and former weapons designer) named Richard Garwin, for example, wrote to Wiesner proposing that the U.S. use relay satellites as a "prime communication path" for a surprise-attack warning system. See R.L. Garwin, Memorandum for J.B. Wiesner, 28 November 1958, Box 5, Folder 145 "Safeguards Against Surprise Attack," *JBW*.

⁶⁴ Wiesner repeated the consensus that aerial reconnaissance could provide little useful information about the alert status (or even the location) of land-based missiles. Submarine-launched missiles would be entirely undetectable. And it would never be possible to tell for certain whether a manned bomber fleet was on a training mission or readying for a sudden strike. See Wiesner, "Report on the 1958 Conference," 191-195.

⁶⁵ Jerome B. Wiesner to J. R. Killian, 11 April 1959, Box 5, Folder 147 "Safeguards Against Surprise Attack," *JBW*; Wiesner, "Report on the 1958 Conference," 197-198.

In his report Wiesner frequently used a new term—“arms control”—to describe the purpose and character of such a system. (Neither Schelling nor Wohlstetter had used these words, opting for the standard “disarmament” instead.) As he described it, arms control promised the use of both technical and political instruments (inspection/control systems, joined with formal agreements on limitations) to achieve its ends. And its object—the pathology for which arms control was the cure—was the arms race. Disarmament—now underwritten by minimum nuclear deterrence but still disarmament in an old-fashioned sense—was the purpose of arms control as he envisioned it. Arms control was disarmament short of *total* disarmament.⁶⁶ In the coming months, with colleagues from PSAC and the Boston disarmament committee, he would begin to articulate the requirements for a very different kind of stability than that imagined by Wohlstetter and Schelling: the stability of arms limitations and the arms race.⁶⁷

Jerome Wiesner and the Cybernetic Theory of Arms Control

Debate about the meaning of stability within the context of deterrence and arms control soon reached the President’s Science Advisory Committee. During PSAC’s annual audience with Eisenhower in the spring of 1959, James Killian had described a desperate need to address “anew” the problem of nuclear weapons, backed up with robust technical studies. In the coming weeks Killian reorganized and re-staffed the Disarmament Panel within PSAC, which had

⁶⁶ For another expression of this view, see Gerald Holton to Robert R. Bowie, 27 May 1960, Box 8, 255 “Daedalus Meeting May 20, 1960,” *JBW*.

⁶⁷ The term “arms control” was not original to Wiesner, nor was it original to the surprise attack conference. It was used on occasion during the period 1955-57, for example, in disarmament discussions between the White House and Departments of State and Defense. A keyword search for “arms control” in the volume of *Foreign Relations of the United States, 1955-1957* dedicated to arms regulation and atomic energy, for example, reveals the occurrence of the term in nine documents (always connected to the traditional goal of disarmament). The term does not become truly prevalent until the Kennedy administration. Thus the use of “arms control” to describe arms limitation and disarmament policy and thinking during the Eisenhower years is something of an anachronism. Keyword search performed in U.S. Department of State, Office of the Historian, *Foreign Relations of the United States, 1955-1957, Regulation of Armaments; Atomic Energy, Volume XX*, available at <http://history.state.gov/historicaldocuments/frus1955-57v20>.

previously focused on recommendations related to the nuclear test ban. To reflect the panel's new mission, Killian even gave it a new name: the "Panel on Arms Limitation and Control." The PSAC Arms Limitation and Control (ALC) panel would become, in the second half of 1959, the main forum in PSAC (and thus one of the closest groups to the President) in which the ideas of deterrence, arms control, and stability would be discussed and developed.⁶⁸

The group's official task was to bring to the "[Committee of] Principals a better understanding of various levels of deterrence" coupled with a "possible understanding of a [quota]" on nuclear weapons—in other words, to explore the relationship between disarmament and deterrence.⁶⁹ Joining Killian on the panel as it took shape in the coming weeks were Jerome Wiesner, Richard Leghorn, the nuclear physicist Robert Bacher, Ralph Johnson of the Ramo-Woolridge Corporation (the firm doing much of the research and engineering in support of the U.S. ballistic missile program), Oskar Morgenstern (an economist at Princeton, and one of the co-founders of game theory with John Von Neumann), Spurgeon Keeny, Harold Brown of Livermore National Laboratory, the Princeton statistician John W. Tukey, and the CIA's Herbert Scoville. Rounding out the new group was Paul Doty. Doty, known to Wiesner through Feld's Boston committee and the Pugwash conferences, was invited by Wiesner to serve as a special consultant.⁷⁰

⁶⁸ On the PSAC Disarmament Panel, see Wang, *In Sputnik's Shadow*, 135-138. Wang does not pursue the work of this PSAC subcommittee further than its May 1959 meeting with Eisenhower, and he does not mention its renaming.

⁶⁹ Handwritten notes by Robert F. Bacher, "JRK 6/19/59," Box 27, Folder 9 "PSAC, Panel on Arms Limitation and Control, 1959, undated," *RFB*. On Richard Bissell, see Lewis, *Spy Capitalism*, 171. The Committee of Principals had been established by Eisenhower in 1958 as a special group to review disarmament policy. Chaired by the Secretary of State, it included the Secretary of Defense, the chairman of the Atomic Energy Commission, the director of the CIA, and the President's science advisor.

⁷⁰ Early meetings of the panel reflected the influence of Leghorn. In September they discussed "stabilized deterrence based on determination of missile locations"—what Bacher termed "strategic inspection." See the list of correspondents in Oskar Morgenstern to Panel on Arms Control and Limitation, 28 December 1959, Box 27, Folder 9 "PSAC, Panel on Arms Limitation and Control, 1959, undated," *RFB*.

Killian arranged for the new theory of deterrence to be imported, by personal contact, from the West Coast back to PSAC in Washington. First he put Robert Bacher in charge of a special West Coast subcommittee of the ALC panel. Then Bacher scheduled a series of meetings between the ALC panel members and experts in the newly prominent social science of nuclear war (mainly at RAND), including Wohlstetter, Herman Kahn, Hans Speier and Charles Hitch, as well as Dalimil Kybal, the Chief Scientist at Lockheed's Missiles and Space Division. In mid-October Bacher held an official meeting of the PSAC Arms Limitation and Control Panel at the RAND headquarters, and then conducted separate interviews with each of the RAND experts.⁷¹

In frequent discussions that fall, the ALC panel debated the tradeoffs between deterrence and arms limitations. The statistician John Tukey argued that deterrence ought to be stable in three specific senses: use (neither side was tempted to launch under any circumstances), numbers (neither side was tempted to break out of a limitation agreement and seek numerical supremacy), and technological change (neither side would be tempted to design better warheads or more sophisticated delivery systems when adequate deterrence was already guaranteed). In other words, Tukey's definition of stability included not just the deterrence of war, as in Wohlstetter's formulation, but also the stability of weapons levels and sophistication. As it happened, Tukey had recently been thinking about the meaning of stability in the context of disarmament with great care. Partnering with Jerome Wiesner and Paul Doty, he was just then at work on a detailed study of the meaning of stability for arms control.⁷²

The report was to be a summary of Wiesner's view of arms control, under development since the surprise attack conference. "A Study of Comprehensive Arms Control Systems" was

⁷¹ See the handwritten notes by Robert F. Bacher, "9/21/59, PSAC Panel ALC"; and Alice W. Horne [Bacher's secretary] to Harold Brown, 7 October 1959," Box 27, Folder 9 "PSAC, Panel on Arms Limitation and Control, 1959, undated," *RFB*.

⁷² Handwritten notes by Robert F. Bacher, "10/28/59, Leghorn - Security Concept," Box 27, Folder 9 "PSAC, Panel on Arms Limitation and Control, 1959, undated," *RFB*.

the most fully elaborated attempt to date to harness nuclear deterrence to the project of nuclear disarmament. Rooted deeply in Wiesner's experience with the surprise attack problem, the proposal was framed in a very specific language—that of *systems* and *dynamic feedback*, a way of thinking that came naturally to Wiesner in light of his background as a communications engineer and cyberneticist.

Two aspects of the report stood out. The first was its commitment to a broad form of disarmament or arms limitation. As the report put it: “By comprehensive arms control systems are meant those systems designed to limit or regulate all major instruments of war. Such systems may be designed so as to eventually eliminate all military weapons except those required for internal police action, or merely to reduce their numbers to a less dangerous level.” The second was the report's development of the concept of stability as applied to the nuclear arsenals themselves, not simply to deterrence. Deterrence was essential to the goal of arms control, but it was not arms control's purpose.

Several comprehensive arms limitation schemes had been proposed in the early years of the atomic age, the authors argued, but none had “provided adequate means to insure stability of the system once the agreed upon force and weapon level had been reached.” There was no way to guarantee that the other side would not break out of the agreement whenever it chose to, rebuilding its arsenal and reigniting an arms race. The disarmament effort had been confined to “partial measures,” such as the prevention of surprise attack and the nuclear test ban. But these had so far foundered on disagreements about inspection and verification.⁷³

The reason was simple: the absence of deterrence. A “comprehensive system” would implement deterrence along with arms reductions, whereas limited agreements were open to

⁷³ P.M. Doty, J. Tuckey [sic], and J.B. Wiesner, “First Draft, A Study of Comprehensive Arms Control Systems,” 26 October 1959, Box 5, Folder 172 “Doty, Tukey, Wiesner Paper, October 26, 1959,” *JBW*.

violation and arms build-ups. New technological and political developments suggested the possibility of a comprehensive arms control system. These included the arrival of ballistic missiles “which make possible the creation of a highly secure deterrent force”; improvements in warhead designs that gave each side a range of options for inflicting nuclear destruction, from small-scale tactical strikes to massive assaults; and a new openness in the Soviet negotiating stance, witnessed in the Geneva experts’ conferences, to the idea of an inspection system coupled with partial reductions in weapons levels.⁷⁴

The authors asserted that a prerequisite for arms control was the removal “from each nation...the power to attack another major nation successfully and without unbearable reprisal.” In other words, nuclear deterrence was a component of nuclear arms control. Fortunately the trend of technological development seemed “to lead more or less naturally to the condition desired”: the impetus for an arms race would disappear, since “the ability to achieve relatively secure retaliatory systems makes it appear feasible to control the size of the force using only strategic inspection techniques.” (In fact they attributed the concept of mutual deterrence not to Wohlstetter, or anyone else at RAND, but to Richard Leghorn, the Boston disarmament committee member and PSAC affiliate who had originally turned Feld’s group onto the problem of missile inspection.)

The three authors wanted disarmament but not *total* disarmament. “Complete disarmament would create a vacuum in which adventure would be attractive, for there would be no way to discipline a violator of the agreements. Such a situation would be unstable, and could easily result in a new arms race.” Thus either an international force would have to be created to police fully disarmed states, or disarmament would be partial. “The growth in potency and

⁷⁴ P.M. Doty, J. Tuckey [sic], and J.B. Wiesner, “First Draft, A Study of Comprehensive Arms Control Systems,” 26 October 1959, Box 5, Folder 172 “Doty, Tukey, Wiesner Paper, October 26, 1959,” *JBW*.

fearfulness of weapons has made total disarmament impossible,” the authors wrote. “Today we must see arms control.”⁷⁵

In November 1959, with the draft report still incomplete, Tukey left the trio to work on other problems in PSAC. Wiesner (reaching out to another Boston connection) decided to bring in Donald Brennan to beef up the mathematical analysis in the report. He even had PSAC chairman George Kistiakowsky write to the director of Lincoln Lab, Carl Overhage, for permission to steal Brennan from his day job. “As you are well aware, disarmament has become an important issue, and Dr. Brennan is one of the few people in the country with a good background in the field”—a background obtained working with the Boston disarmament committee.⁷⁶

In their completed draft of January 1960, the authors had now identified a critical index of stability. It was something called the “exchange ratio.” Originating in operations research, the exchange ratio was a measure of the effectiveness vs. the cost of an attack.⁷⁷ (Wiesner had very likely first encountered this concept in conversations with Dalimil Kybal, the ballistic missile expert who had worked with Wiesner on the Gaither Panel and on the surprise attack conference delegation.⁷⁸) As the three authors defined it, the exchange ratio was the number of weapons spent by an attacker to destroy a single defending weapon. If you could destroy a single enemy weapon—by exploding a warhead near an enemy bomber or missile silo, for example—using

⁷⁵ P.M. Doty, J. Tuckey [sic], and J.B. Wiesner, “First Draft, A Study of Comprehensive Arms Control Systems,” 26 October 1959, Box 5, Folder 172 “Doty, Tukey, Wiesner Paper, October 26, 1959,” *JBW*.

⁷⁶ G.B. Kistiakowsky to Carl Overhage, 21 November 1959, Box 7, Folder 227 “Arms Limitation and Control Panel, 1960,” *JBW*.

⁷⁷ See, for example, Philip M. Morse and George E. Kimball, *Methods of Operations Research* (New York: John Wiley & Sons, 1951), 45-48. There, the “exchange rate” is defined specifically as the ratio of own losses to enemy losses in a given engagement. Morse and Kimball drew their illustrations from actual World War II cases: battles between U.S. and Japanese aircraft in the Pacific, and Allied convoys vs. U-Boats in the North Atlantic, for example.

⁷⁸ See, e.g., Dalimil Kybal, “The role of stabilized deterrence,” in David H. Frisch, ed., *Arms Reductions: Program and Issues* (New York: The Twentieth Century Fund, 1961), 135-156.

only one nuclear weapon in your own arsenal, the exchange ratio was 1. If it took two weapons—because of uncertainty in the accuracy of the delivery mechanism, or because the enemy had dispersed, hardened, or mobile forces—then the exchange ratio was 2. And so on.⁷⁹

The idea, the authors believed, was that the U.S. and the Soviet Union should cooperate in making the exchange ratio as large as possible. That is, they should make the retaliatory arsenals in both the U.S. and the Soviet Union extremely resistant to a disarming assault. The reason was not simply deterrence stability, but the stability of arms levels. An exchange ratio hovering around 1 would give rise to instability, a situation susceptible to arms racing. “If the number of weapons permitted each ‘side’ in a bipolar arms control situation was the same and if the exchange ratio were strictly unity, then one ‘side’ could gain an overwhelming upper hand by secreting a few clandestine weapons...”⁸⁰ That was where inspection entered the picture. The larger the exchange ratio, the more strenuous effort required to build up a force adequate for a surprise attack. And the harder it would be to conceal so much illegal construction.

With data from the Atomic Energy Commission book “The Effects of Nuclear Weapons” and using a hand-held RAND Corporation Bomb Damage Effect Computer, Brennan got to work drawing “specimen curves”—numbers of missile bases destroyed as a function of numbers of attacking missiles, yield and resistance to blast damage (hardness, as measured in pounds-per-square-inch overpressure), and so on. Each curve corresponded to a different value of the missile accuracy; less accurate attacking missiles would destroy fewer bases than an equivalent number

⁷⁹ For R weapons spent destroying one enemy weapon, the exchange ratio was R . Thus R was the ratio of asymmetry in arsenal size required to deliver a fatal blow without fear of reprisal: if R were 10, for example, and the enemy had 200 weapons, then you would need at least 2,000 to leave the enemy toothless. P.M. Doty, J. Tuckey [sic], and J.B. Wiesner, “First Draft, A Study of Comprehensive Arms Control Systems,” 26 October 1959, Box 5, Folder 172 “Doty, Tukey, Wiesner Paper, October 26, 1959,” *JBW*.

⁸⁰ See the draft paper in Box 8, Folder 260 “Doty, Brennan, Wiesner Paper, January 18, 1960,” *JBW*.

of higher accuracy missiles. From these curves Brennan could read off a sample of representative exchange ratios. The authors found the numbers encouraging.⁸¹

They believed an arms control system could be conceived and implemented in timed, gradual stages. At the beginning, a very modest inspection system (the easier to negotiate with the Soviets) would be coupled with independent efforts at hardening (the larger to make the exchange ratio). As the Soviets, too, came to appreciate the system's stability-making features, they would be more amenable to slightly improved inspection capabilities. The territory and installations made visible to aerial reconnaissance could be expanded, and ground inspection teams could be implemented gradually. With a moderately improved inspection system, a new agreement reducing numbers slightly could be reached. This would provide yet more incentive to improve the inspection system—and so on, until some stable minimum was reached at which the size of each arsenal would come to rest. Thus the two sides could begin “arms limitations with only a modest amount of inspection effort and a corresponding modest reduction in force...then allowing the system to evolve as confidence is built up.”⁸²

The system would stand up under crisis and resist the temptation to a new arms race because deterrence had long since been guaranteed. Thus Wiesner and his coauthors spoke of mutual deterrence as “the device which will enable us to cope with the problem of the illegal nuclear weapons and/or missiles,” the factor that would give the system tolerance and flexibility as it gradually tended toward its steady state. It wouldn't matter if the other side had secreted a

⁸¹ Assuming one wanted at least 10 bases left after an attack to fire a retaliatory assault, Brennan found an exchange ratio of about 20 for the most accurate attacking missiles. For less accurate missiles, the exchange ratio was much higher. P.M. Doty, J. Tuckey [sic], and J.B. Wiesner, “First Draft, A Study of Comprehensive Arms Control Systems,” 26 October 1959, Box 5, Folder 172 “Doty, Tukey, Wiesner Paper, October 26, 1959,” *JBW*.

⁸² P.M. Doty, J. Tuckey [sic], and J.B. Wiesner, “First Draft, A Study of Comprehensive Arms Control Systems,” 26 October 1959, Box 5, Folder 172 “Doty, Tukey, Wiesner Paper, October 26, 1959,” *JBW*. The proposal advanced by Doty, Tukey, and Wiesner here bears some resemblance to the scheme of GRIT (graduated and reciprocated initiatives in tension reduction) promoted by the psychologist Charles Osgood in the early 1960s. On Osgood and his ideas, see Erickson et al., *How Reason Almost Lost Its Mind*, esp. 88–106.

few extra warheads away, since hardened retaliatory forces would always make a first strike irrationally costly. And if the other side tried to conceal a huge number of illegal warheads, they would invariably be found out, especially during the latter stages of the system's operation, when arsenals were smaller and inspection more intrusive.⁸³

The comprehensive arms control system Wiesner envisioned resembled nothing so much as a dynamic feedback loop. It fed information gleaned from inspection techniques back into itself, incentivizing gradual political agreement on arms limitations and slowly, stably, reducing the numbers of nuclear weapons, even as the amount of information fed back into the system continued to increase.

⁸³ Given the exchange ratio data Brennan had computed, he and Wiesner and Doty tentatively prescribed a sufficient strategic force for which they believed both the U.S. and the Soviets should strive. Their ideal force structure was decidedly missile-heavy. It would include a total of 200 "large weapons," including 100 fixed land-based ICBMs in silos hardened to more than 300 psi; 5 missiles in each of 12 Polaris submarines; and 40 weapons available for long-range bombers or cruise missiles. Non-nuclear air defense was allowed. But there were three strict prohibitions: active missile defense; submarine detection systems; and improvements in the accuracy of land-based missiles. P.M. Doty, J. Tuckey [sic], and J.B. Wiesner, "First Draft, A Study of Comprehensive Arms Control Systems," 26 October 1959, Box 5, Folder 172 "Doty, Tukey, Wiesner Paper, October 26, 1959," *JBW*.

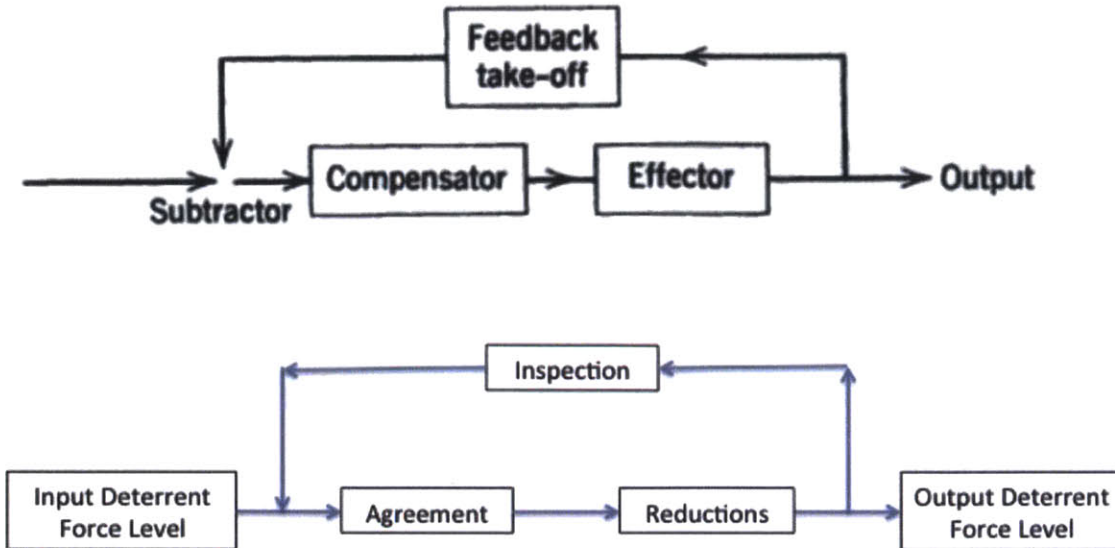


Figure 1.1: Arms control as a cybernetic feedback loop. The top image is taken from Norbert Wiener, *Cybernetics: Or Control and Communication in the Animal and the Machine* (Cambridge, MA: The MIT Press, 1948), 112.

The likeness was more than coincidence. Wiesner had cybernetics in his blood. At virtually every step of his career before becoming a full-time government advisor, he was deep in the engineering work of feedback, communications, and control. Trained as an electrical engineer in the 1930s at the University of Michigan, he became a specialist in communications engineering, designing electronics for the Speech Correction Department (Claude Shannon, the founder of information theory, was an undergraduate classmate). During the war, Wiesner joined the staff of the MIT Radiation Laboratory, the hub of U.S. radar design and development. At the “Rad Lab” he climbed quickly: starting out on the research staff, he was appointed associate leader of the Radio Frequency Development Group before leading his own, called Project Cadillac, in charge of designing an airborne radar system. Following the war Wiesner was briefly hired as leader of a group at Los Alamos laboratory in charge of developing electronics for the Operation Crossroads test in 1946. In 1947 he returned to MIT as an assistant professor of

electrical engineering, and joined the Research Laboratory of Electronics (RLE). The RLE was postwar successor to the Rad Lab, absorbing much of its equipment as well as its high-level personnel. It would also become a congenial home for feedback systems engineering and cybernetic thinking at MIT. By 1952 Wiesner had been named director of the laboratory.⁸⁴

The invention of cybernetics in the United States (the theory, as well as its name) is credited to Norbert Wiener, the legendary and eccentric MIT mathematician. Wiener, in his classic 1948 monograph *Cybernetics*, announced a “universal science” of communications, feedback, and control in complex systems. His theory embraced a wide array of concepts from physiology, psychology, communication engineering, and thermodynamics. And its range of application was staggeringly ambitious, from purely mechanical to purely physiological systems, systems of human-machine interaction, to politics and society itself. The notion of feedback—the process by which a system could send information concerning its output back into its input, so as to adjust and stabilize the output—was central to cybernetics’ concept of control. Wiener’s book included an entire chapter on feedback and oscillation, in which he discussed the general properties of feedback control and listed representative examples ranging from steam engines to thermostats to the physiology of human proprioception.⁸⁵

As David Mindell has argued, cybernetics actually grew out of ideas and techniques deeply entrenched in American “control systems” engineering projects that could be dated back to the decades prior to World War II. He shows how several distinct engineering cultures

⁸⁴ Walter A. Rosenblith, ed., *Jerry Wiesner: Scientist, Statesman, Humanist: Memories and Memoirs* (Cambridge, MA: The MIT Press, 2003); Eric Pace, “Jerome B. Wiesner, President of MIT, is dead at 79,” *New York Times* (23 October 1994); IEEE Global History Network, “Jerome B. Wiesner,” available at http://www.ieeeahn.org/wiki/index.php/Jerome_B._Wiesner. On the history of the Radiation Laboratory and the RLE, see Stuart W. Leslie, *The Cold War and American Science: The Military-Industrial-Academic Complex at MIT and Stanford* (New York: Columbia University Press, 1993), esp. 20-27; and Deborah Douglas, “MIT and War,” in David Kaiser, ed., *Becoming MIT: Moments of Decision* (Cambridge, MA: MIT Press, 2010), 81-102.

⁸⁵ Norbert Wiener, *Cybernetics: Or Control and Communication in the Animal and the Machine* (Cambridge, MA: MIT Press, 1948). Wiener’s cybernetic view of political and social systems is more fully elaborated in Norbert Wiener, *The Human Use of Human Beings: Cybernetics and Society* (Boston: Houghton Mifflin, 1954).

developed around problems of feedback, control, communications, and computing in the interwar period—from the Navy Bureau of Ordnance and the problem of fire control (accurately aiming artillery to hit moving targets), to the Bell Telephone Laboratories and its struggle to achieve stability in long-distance signals transmission in the 1920s. These techniques and concepts converged on MIT (in government-sponsored work on radar fire control) in the 1940s. Wiener’s “cybernetic synthesis” at the end of the decade is virtually unimaginable without these much earlier traditions.⁸⁶

Jerome Wiesner was caught up in the development of control engineering and cybernetics both in practice and by close personal association. His early-career work in communications and signal processing involved him in topics close to the heart of cybernetics.⁸⁷ As an expert in microwave electronics, Wiesner had spent three years immersed in the Rad Lab’s work on radar. In the late 1940s he even became a close personal colleague of Norbert Wiener, coming to regard him as a mentor, and regularly attending the famed cybernetics dinner club that Wiener first convened in 1948. (Wiesner later described these rambling conversations as “a tower of Babel, as engineers, psychologists, acousticians, doctors, mathematicians, neurophysiologists, philosophers, and other interested people tried to have their say.... As time went on, we came to understand each other’s lingo and to understand, and even believe, in Wiener’s view of the

⁸⁶ These strands of practice, and many of the people associated with them, coalesced during the Second World War. The National Defense Research Committee (NDRC) established a special division devoted to control systems, headed by Warren Weaver of the Rockefeller Foundation. One of its contracts was let to Norbert Wiener, who produced a widely read report on feedback systems. According to Mindell, “Wiener’s frequently cited paper formed the basis for postwar work of optimal estimation, smoothing, and signal processing, much of it intimately tied to military applications”—a full six years before the publication of his famous book. See David Mindell, *Between Human and Machine: Feedback, Computing, and Control Before Cybernetics* (Baltimore, MD: Johns Hopkins University Press, 2002), on 280. Also see Peter Galison, “The ontology of the enemy: Norbert Wiener and the cybernetic vision,” *Critical Inquiry* 21, no. 1 (1994): 228-266.

⁸⁷ See, for example, Y.W. Lee, T.P. Cheatham, Jr., and J.B. Wiesner, “The application of correlation functions in the detection of small signals in noise,” MIT Research Laboratory of Electronics Technical Report No. 141 (13 October 1949).

universal role of communications.”⁸⁸) In 1949 Wiesner, with colleagues at the RLE, constructed a machine they called the “autocorrelator,” which used feedback techniques to analyze communications signals for predictability. And that year he coauthored a paper on human-machine interaction with Wiener and another MIT researcher. In short, Wiesner’s cybernetic credentials were impeccable and deep; cybernetics shaped the way he saw the world.⁸⁹

Wiesner was, of course, not the first to introduce the term “control” into the discussion of nuclear arms limitation. The earliest postwar proposals, after all, had discussed schemes for the control (especially the international control) of atomic materials, technologies, and weapons.⁹⁰ But “control” had very specific connotations for Wiesner in light of his extensive background in systems engineering and cybernetics. Initially spurred by the engineering problem of tactical warning and surprise attack, and motivated by the larger political goal of disarmament, he reached into a conceptual toolkit he had carried since the 1940s. It allowed him to imagine stabilizing a technical and social system of baffling complexity and danger. Long-term dynamic feedback, control, and stability were the terms he used to grapple with the problem of nuclear weapons.⁹¹

⁸⁸ Rosenblith, ed., *Jerry Wiesner: Scientist, Statesman, Humanist*, 217.

⁸⁹ J. Wiesner, N. Wiener, and L. Levine, “Some problems in sensory prosynthesis,” *Science* 110 (1949): 512.

⁹⁰ Bechhoefer, *Postwar Negotiations for Arms Control*, 27-82. As David Mindell points out, the concept of “control,” more broadly, traces its ancestry at least back to the 19th century, in fields as diverse as economics, science, politics, and engineering. See James R. Beninger, *The Control Revolution: Technological and Economic Origins of the Information Society* (Cambridge, MA: Harvard University Press, 1986); Miriam Levin, “Contexts of control,” in Miriam Levin, ed., *Cultures of Control* (Amsterdam: Harwood, 2000).

⁹¹ Wiesner’s expertise in cybernetics would impact his government advising career in other ways. In the Soviet Union cybernetics had been suppressed and denounced as ideologically suspect during the 1950s, but by 1961 the Soviet Academy of Sciences had acquired a Council on Cybernetics. The Council published a report titled “Cybernetics in the Service of Communism” promising society-wide benefits of cybernetic techniques, including a “single automated system of control of the national economy.” The Twenty-Second Party Congress that same year included cybernetics in its list of the sciences that would vault the Soviet system to a state of modernity. Back in the U.S. the CIA was watching these developments closely, setting up a “task force” on Soviet cybernetics. In 1962 Attorney General Robert F. Kennedy was briefed by the CIA on “the serious threat to the United States and Western Society posed by increasing Soviet commitment to a fundamentally cybernetic strategy in the construction of communism.” When Arthur Schlesinger, Jr. brought the matter up with President Kennedy, Kennedy asked his science advisor—Jerome Wiesner—to set up a “cybernetics panel” to study the matter. Wiesner arranged for his fellow MIT electrical engineer (and former Boston disarmament committee member) Walter Rosenblith to chair the

Wiesner first distributed his semi-complete report to the PSAC Arms Limitation and Control panel at the end of 1959. The panel, which held multiple sessions to discuss the so-called “Wiesner Systems Study,” enthusiastically endorsed its recommendations. Thus even as PSAC came to accept the growing role of stabilized nuclear deterrence as theorized at RAND, it continued to stress arms limitation and disarmament as arms control’s purpose.⁹² Along the way, Wiesner had injected a very particular concept of stability—stability of weapons levels through dynamic feedback—into the nuclear weapons debate of the late Eisenhower administration.⁹³

The Daedalus Conference and the Summer Study on Arms Control

Back at MIT, in the autumn of 1959 Bernard Feld and David Frisch won a small grant from a foundation known as the Institute for International Order. Still hoping to run a summer study, Feld and Frisch used the money to host a planning conference, including the usual cast of Boston disarmament advocates, plus a few members of PSAC (including Wiesner) and a couple of analysts from RAND.⁹⁴

panel. And he asked John Tukey, Wiesner’s coauthor on the first draft of the arms control systems paper in 1959, to help out as well. See Slava Gerovitch, *From Newspeak to Cyberspeak: A History of Soviet Cybernetics* (Cambridge, MA: The MIT Press, 2002); the quotation is found in Slava Gerovitch, “The cybernetics scare and the origins of the Internet,” *Baltic Worlds* 2, no. 1 (2009): 32-38. Also see David Holloway, “Innovation in science—the case of cybernetics in the Soviet Union,” *Science Studies* 4 (1974): 299-337.

⁹² Handwritten notes by Robert F. Bacher, “12/6/59, Wiesner Systems Study,” Box 27, Folder 9 “PSAC, Panel on Arms Limitation and Control, 1959, undated,” *RFB*; Handwritten notes by Robert F. Bacher, “For PSAC Meeting December 14, 1959, Stable Deterrent – Some Conclusions,” Box 27, Folder 9 “PSAC, Panel on Arms Limitation and Control, 1959, undated,” *RFB*. See also the handwritten notes by Robert F. Bacher, “12/14 ALC – PSAC,” Box 27, Folder 9 “PSAC, Panel on Arms Limitation and Control, 1959, undated,” *RFB*.

⁹³ Richard Leghorn, in particular, carried the new lessons of arms control into the highest levels of policymaking. In 1959 he had been asked to consult on a Joint State and Defense Department disarmament study, headed by the lawyer (and friend of Secretary of State Christian Herter) Charles A. Coolidge. A meeting between Coolidge and the NSC in early December 1959 reveals the clear influence of Leghorn and the idea that arms control and deterrence were intimately related. The Coolidge Report was delivered to the President in 1960 just before the start of the Ten Nation Disarmament Conference (where the nuclear test ban negotiations assumed center stage). See Document 236, “Memorandum of Discussion at the 426th Meeting of the National Security Council,” 1 December 1959, U.S. Department of State, *Foreign Relations of the United States, 1958-1960, Volume III, National Security Policy, Arms Control and Disarmament*.

⁹⁴ B.T. Feld to the Committee on the Technical Problems of Arms Limitation, 25 September 1959, Box 11, Folder 98 “Federation of American Scientists, Disarmament Study Committee, 1956-1959 (2 of 3),” *BTF*.

The draft proposal for a summer study that emerged from the December meeting marked a shift in the thinking of the Boston committee. The influence of Jerome Wiesner was palpable, even revealed in the title of the proposal—“Arms Control Systems and the Development of New Inspection Techniques of Wide Applicability.” The group’s disciplinary representation had also been considerably widened beyond the natural sciences, including local economists like Francis Bator and Carl Kaysen. The committee was now contemplating a multidisciplinary attack on the arms race, a “systems approach” to the problem of nuclear weapons.⁹⁵ They argued that arms control “requires an approach in which both the technical and the political aspects of the problem are simultaneously taken into account.” On the table was a large, multidimensional study whose aim was to formulate “overall control systems” adapted to both geopolitical and technological possibilities—“the intimate intermingling of the technical and political requirements of the system as a whole.”⁹⁶

By January 1960 the committee had arrived at a final draft of the summer study proposal.⁹⁷ But who to approach for the money? The major foundations had mostly rejected the group’s efforts in the past. Feld turned to his old Manhattan Project boss, Robert Oppenheimer, for advice. Oppenheimer sat on the board of trustees of The Twentieth Century Fund, based in New York, along with David Lilienthal, the first chairman of the Atomic Energy Commission. As it happened, these veterans of the early atomic program had recently been urging their fellow trustees to support work on the nuclear question. In early March Feld submitted tentative budgets

⁹⁵ “First Rough Draft (12-29-59), Proposal for a Summer Study Program, Arms Control and the Development of New Inspection Techniques of Wide Applicability,” Box 11, Folder 98 “Federation of American Scientists, Disarmament Study Committee, 1956-1959 (2 of 3),” *BTF*.

⁹⁶ “First Rough Draft (12-29-59), Proposal for a Summer Study Program, Arms Control and the Development of New Inspection Techniques of Wide Applicability,” Box 11, Folder 98 “Federation of American Scientists, Disarmament Study Committee, 1956-1959 (2 of 3),” *BTF*.

⁹⁷ “Second Draft (1-18-60), Proposal for a Summer Study Program, Arms Control Systems and the Development of New Inspection Techniques of Wide Applicability,” Box 7, Folder 227 “Arms Limitation and Control Panel, 1960,” *JBW*.

while Oppenheimer smoothed things over behind the scenes. By April officers at the Fund had approved the summer study Feld had been dreaming of since 1957, to the tune of about \$100,000. Feld would serve as the study's director, joined by co-directors Donald Brennan and David Frisch; Paul Doty and Jerome Wiesner were to round out the steering committee.⁹⁸ In the next few weeks Feld, through Brennan, also approached Thomas Schelling, who was now ensconced nearby in the economics department at Harvard. Schelling would join the steering committee and initially served as the study's bibliographer, preparing a pre-summer reading list.⁹⁹

Meanwhile in early 1960 the editor of the *American Academy*, Gerald Holton, began to make plans for a special *Daedalus* volume dedicated to arms control. Holton had been involved with the Boston disarmament committee since 1958, helping to publish its early study of the Nth country problem. An Austrian émigré who had fled fascism in Austria, Holton had trained for his Ph.D. in physics at Harvard under the famed experimentalist and philosopher of science Percy Bridgman. (He would also develop deep interests in the history of science, becoming a renowned expert on the life and career of Albert Einstein.) With a grant from the Johnson Foundation in

⁹⁸ John T. Edsall and Bernard T. Feld to Ben T. Moore, 2 May 1960, Box 7, Folder 227 "Arms Limitation and Control Panel, 1960," *JBW*; B.T. Feld to the Committee on Technical Problems of Arms Limitation (American Academy of Arts and Sciences) and associates, 18 April 1960, Box 2, Folder 18 "AAAS, Committee on Technical Problems of Arms Limitation, Summer Study on Arms Control, 1960: Preparation correspondence, 2/2," *BTF*.

⁹⁹ Feld spread invitations for the summer study widely, including government figures like Charles Coolidge (still in charge of the State Department disarmament study) and Hubert Humphrey. He also had interest from various members of the military analysis community. Charles Townes, director of research at the Institute for Defense Analyses, called Feld and asked if members of its new "Jason" division might participate. Thus Edwin Salpeter (a Cornell physicist and new Jason) and his wife Miriam, a psychologist, attended the study. So did Leon Lederman from Columbia University, who explained to Feld that "one of my main objectives in joining [Jason] was to work actively in this direction [i.e., on arms control]." See Feld handwritten notes, 5/6/60, Box 2, Folder 18 "AAAS, Committee on Technical Problems of Arms Limitation, Summer Study on Arms Control, 1960: Preparation correspondence, 2/2," *BTF*; Leon Lederman to Bernard T. Feld, 9 May 1960, Box 2, Folder 17 "AAAS, Committee on Technical Problems of Arms Limitation, Summer Study on Arms Control, Preparation correspondence 1960, 1/2," *BTF*. Also see Bernard T. Feld to T. Schelling, 26 May 1960, Box 2, Folder 13 "AAAS, Committee on Technical Problems of Arms Limitation 1958-1960, 2/3," *BTF*. Schelling's summer reading list can be found in Box 3, Folder 19 "AAAS, Committee on Technical Problems of Arms Limitation, Summer Study on Arms Control, Summer Study on Arms Control 1960, 1/2," *BTF*.

Wisconsin, he organized a short two-day conference to be held at the headquarters of the American Academy in May. Holton, who had wide-ranging interests himself, was ecumenical in his pursuit of participants. The list of people invited to speak spanned the whole methodological and political range of thought on the disarmament and arms control issue as it had developed in the late 1950s. The papers were bound together in a special issue of the American Academy's journal *Daedalus* in the fall of 1960. As Holton wrote in his introduction to the volume, it seemed that a sea change had occurred in the field: "As recently as a year ago a coordinated group of papers of this range and quality could not have been assembled."¹⁰⁰ Donald Brennan also edited (with the assistance of Wiesner) a separate volume of slightly revised papers, published in 1961 as *Arms Control, Disarmament, and National Security*.¹⁰¹ In his preface to the volume Brennan reported with undisguised pride that many were referring to the book as the "Bible of arms control."¹⁰²

Wiesner submitted a modified version of his comprehensive arms control report for publication in the volume.¹⁰³ Schelling presented his very different perspective in a paper titled "Reciprocal Measures for Arms Stabilization," in which he expanded upon his view that arms

¹⁰⁰ To Jerome Wiesner, Holton wrote that he had "finally chased down Hans Bethe at his Washington hotel" to convince Bethe to attend the conference. Gerald Holton to Donald Brennan and Jerome Wiesner, 27 April 1960; and Jerome Wiesner to Gerald Holton, 3 May 1960, Box 8, Folder 255 "Daedalus Meeting May 20, 1960," *JBW*. Among the attendees at the May *Daedalus* meeting were the full cast of the Boston arms limitation committee, plus forty others, including people from PSAC (Wiesner, Tukey), from RAND (Herman Kahn, Albert Wohlstetter, Henry Rowen, Lewis Bohn, Fred Iklé), from Harvard's Center for International Affairs (Henry Kissinger, Robert Bowie, Thomas Schelling, Morton Halperin), from various posts in Washington (Herbert Scoville, Lester Van Atta, Richard Leghorn, Ronald Spiers, Ernest Lefever), and other prominent figures (Paul Nitze, J. David Singer, David Riesman, William T.R. Fox). See Gerald Holton, "Editor's Prefatory Note," *Daedalus* 89, No. 4, *Arms Control* (Fall 1960): 674-676, quotation on 675.

¹⁰¹ Donald G. Brennan, ed., *Arms Control, Disarmament, and National Security* (New York: George Braziller, 1961).

¹⁰² Donald Brennan, "Editor's Preface," in Brennan, ed., *Arms Control*, 9-12. Brennan also wrote the first paper in the collection, very much adopting a perspective on arms control much closer to that of Thomas Schelling than to that of Bernard Feld and the Boston disarmament committee. See Donald G. Brennan, "Setting and Goals of Arms Control," in Brennan, ed., *Arms Control*, 19-42. On Gerald Holton, see Daniel M. Siegel and David C. Cassidy, "Gerald Holton wins Pais Prize," *History of Physics Newsletter* 10, no. 4 (2008): 1, 5.

¹⁰³ Jerome Wiesner, "Comprehensive arms limitation systems," *Daedalus* 89, no. 4, *Arms Control* (Fall 1960): 915-950.

control meant the tailoring of forces to remove incentives for preemption and prevent failures of deterrence.¹⁰⁴ Feld, in his own contribution to the *Daedalus* issue and Brennan volume, summarized physical techniques of inspection. “It would be less than candid of me not to admit to the conviction that the possible solutions that have the greatest interest...lie more in the direction of disarmament than in deterrence through mutual terror,” he wrote. Wiesner’s plan for stabilized disarmament looked most promising to him. Feld had absorbed the terminology of systems and stability in the past few months—language he had never used before 1960—but remained pledged to disarmament.¹⁰⁵

The following month Feld kicked off his summer study. The study was carried out almost every day for the duration of three months, from June to September. The group convened mostly at MIT’s Endicott House, where Feld’s disarmament committee had first gathered three years earlier. A total of about fifty people participated, with around twenty-five active at any given time.¹⁰⁶ The summer began with a series of seminars to orient the group, including a debate between Schelling and Wiesner on the merits of comprehensive vs. limited arms control measures. The group also received a visit from Philip Noel-Baker, the British disarmament activist and winner of the 1959 Nobel Peace Prize, who conversed with Schelling on the merits of complete disarmament.¹⁰⁷

The precise meaning of deterrence was a regular theme of discussion throughout the coming weeks. Schelling gave a presentation around the end of June on “Deterrent Force Composition,” telling the group that stabilizing deterrence was the single best purpose of arms

¹⁰⁴ Thomas C. Schelling, “Reciprocal measures for arms stabilization,” *Daedalus* 89, no. 4, *Arms Control* (Fall 1960): 892-914.

¹⁰⁵ Bernard T. Feld, “Inspection techniques of arms control,” *Daedalus* 89, no. 4, *Arms Control* (Fall 1960): 860-878.

¹⁰⁶ See August Heckscher’s Foreword in Thomas C. Schelling and Morton H. Halperin, *Strategy and Arms Control* (New York: The Twentieth Century Fund, 1961).

¹⁰⁷ See, e.g., Philip Noel-Baker, *The Arms Race: A Programme for World Disarmament* (London: Stevens & Sons, 1958).

control. At least ten separate sessions were devoted to the topic of “stabilized deterrence,” and the group heard a focused presentation on exchange ratios from John Mullen of the Air Force Research Division. Hans Bethe visited the group twice over the summer, taking a break from his work with the ongoing negotiations with the Soviet Union. “He was profoundly impressed by the idea of a stable invulnerable deterrent force limited by treaty,” according to the minutes taken during his second visit. It struck Bethe that “there seemed to be a great deal of group consensus in favor of this idea”—stabilized deterrence might well be “the best of all visible worlds,” as far as he could tell.¹⁰⁸

The discussion during the entire summer centered almost exclusively on the bilateral U.S.-Soviet relationship, and proliferation was never taken up as a specific topic. A deep undercurrent of worry about China surfaced frequently, however. One session on “The Problem of China,” led by Feld, concluded that China was perhaps within six or seven years of a nuclear capability (in fact they were within four).¹⁰⁹

Since the *Daedalus* conference, Donald Brennan had grown increasingly attracted to the style of strategic analysis developed at RAND, and increasingly drawn to RAND personalities. With Herman Kahn, Brennan co-directed a series of “peace games” toward the end of the summer, in which a group of about twenty participants acted out RAND-style war game scenarios.¹¹⁰ One scenario began with “the explosion of a U.S. nuclear weapon in U.S. hands at

¹⁰⁸ *Summer Study on Arms Control: Collected Papers* (Boston, MA: American Academy of Arts and Sciences, 1961). Schelling’s remarks on deterrent force composition begin on 57. The quotation from Bethe is found on 109.

¹⁰⁹ Morton Halperin also directed a separate session on Chinese military policy. See “Seminar: The Problem of China,” in *Collected Papers*, 353-360; and “Chinese Military Policy,” in *Collected Papers*, 361-374. As Michael Gordin notes in *Red Cloud at Dawn: Truman, Stalin, and the End of the Atomic Monopoly* (New York: Farrar, Straus and Giroux, 2009), when it came to guessing nuclear proliferation times during the Cold War, overestimation was common (beginning with U.S. estimates of the time it would take the Soviet Union to acquire their first fission bomb). See the Epilogue, 285–306.

¹¹⁰ Brennan would go on to join the Hudson Institute Kahn founded in 1961 after leaving RAND. See D.G. Brennan to Herman Kahn, 8 August 1960, Box 3, Folder 20 “AAAS, Committee on Technical Problems of Arms

the SAC base near Mobile, Alabama, which kills tens of thousands of United States citizens.” Intelligence sources would leave open the possibility that the explosion had happened because of pure accident, Soviet sabotage, or “unauthorized (including insane) conduct.” Through two iterations of this scenario, Kahn and Brennan decided that the best U.S. move was to fire a warning shot across the Soviet bow, sending one missile to take out a Soviet gaseous diffusion plant.¹¹¹

The study of disarmament had come a long way since 1957.

The “Cambridge Approach”: Consensus or Conflict?

The occasion of the *Daedalus* conference and Summer Study on Arms Control, and the collected volumes that resulted from them, leave the first impression of coherence and consensus within the early arms control community. It is tempting to think of these events and publications as constitutive of a new school of thought. Jennifer Sims, while mindful of the diversity of viewpoints represented in these meetings and publications, identifies a “Cambridge Approach” to arms control, a “dominant orientation and a cohesive set of concepts.” The new arms control, in

Limitation, Summer Study on Arms Control, Summer Study on Arms Control 1960, 2/2,” *BTF*. On RAND war gaming, see M.G. Weiner, “War Gaming Methodology,” RAND RM-2413 (10 July 1959); and Sharon Ghamari-Tabrizi, *The Worlds of Herman Kahn: The Intuitive Science of Thermonuclear War* (Cambridge, MA: Harvard University Press, 2005), esp. Chapter 6, “Faith and Insight in War-Gaming,” 149–180.

¹¹¹ Max Singer, another co-founder of the Hudson Institute with Kahn, described the various “peace game” scenarios in *Summer Study on Arms Control*, 165-182. “War games and peace games are not sports, nor are they played for fun,” Singer wrote. “In our games at Endicott House, some of the roles played were those of the United States, the British Labour Party, the Catholic Church, the Asian-African bloc, the Secretary-General of the United Nations, and the ‘Stalinist faction’ in Russia. One player plays ‘nature.’ He describes the situation at the beginning of the game, determines whether proposed actions by the players are possible or impossible, and presents any events not under the control of the players.” Even Feld apparently warmed to the gaming method during the summer study, drawing an analogy between political games and “using a computer to obtain enough solutions to non-linear differential equations in order that mathematicians can get a feeling for the types of solutions that exist, and can then make a more effective use of their intuition with other examples of such equations.” A second scenario began with Chinese criticism of an imagined late-1960s U.S.-Soviet arms control agreement as “white man’s arms control”—only permitting northern industrial nations to possess nuclear weapons—and ended in pure silliness: “A Chinese missile delivers multimegaton explosions high over Chicago and Moscow... This breaks many windows, etc., but does no great damage. The Chinese announce that the missiles were a mistake... They offer to pay for the windows. The Chinese and the United States’ troops parade together down Fifth Avenue.”

her view, had a few salient features. It offered “a new sphere for the pursuit of strategic ends.” It was “primarily addressed to the problem of containing the threat which nuclear weapons pose to strategic stability,” a goal Sims identifies as the “weapons-stability nexus.” And arms control emphasized a robust “firebreak” between conventional and nuclear weapons so as not to “compromise American capability to use force in support of its interests.”¹¹² Arms control as a component of military and strategic policy; an emphasis on strategic stability; and the nuclear firebreak: these were the keystones of the new arms control perspective according to Sims.¹¹³

I find these characteristics congenial to Schelling’s picture of arms control. But they sit awkwardly next to the views and backgrounds of so many of the 1960 participants—especially those that came out of an earlier disarmament tradition and retained a deep-seated aversion to the nuclear arms race, and to the possession and possible of nuclear weapons. The idea of a “Cambridge school” is therefore called into doubt by considering the full range of participants, their separate backgrounds and views, and the specific institutional history out of which the 1960 meetings emerged.

A fundamental difference of sensibility separated two groups. One camp (represented most forcefully by Schelling, and by the end of the summer also including Donald Brennan and a Harvard postdoctoral researcher named Morton Halperin) was committed to the stability of strategic deterrence and viewed arms control as an enlargement of strategic policy. The other (led by Wiesner, with Doty, Bernard Feld and David Frisch, as well as a Harvard expert on

¹¹² Sims, *Icarus Restrained*, 225-232.

¹¹³ Also see Miller and Sagan, “Nuclear power without nuclear proliferation?,” which follows Sims in identifying a “Cambridge school.” Miller and Sagan list the core insights of this group somewhat differently, however. Like Sims they highlight the firebreak and strategic stability through secure second-strike forces, but they add the prevention of the spread of nuclear weapons. The emphasis on nonproliferation seems slightly overstressed, however, since it scarcely came up during the entire three-month summer study (which was far more preoccupied with deterrence and disarmament in a bipolar world). Only Paul Doty’s contribution to the *Daedalus* volume dealt centrally with the question of nuclear weapons in the possession of “Nth countries.” See Paul Doty, “The role of the smaller powers,” *Daedalus* 89, no. 4, *Arms Control* (1960): 818-830.

international law named Louis Sohn) was committed fundamentally to the abatement of the nuclear arms race and, since the developments of 1958-59, had come to view deterrence (with various degrees of regret) as an asset to the pursuit of disarmament.

As the study went along, a few participants had considered writing a consensus statement on arms control. Morton Halperin felt confident that “beneath all the disagreements (and with the exception perhaps of a few right and left wing deviants) there is broad agreement in the group as to the aims of arms control [and] its relation to deterrence...”¹¹⁴ Thomas Schelling enthusiastically agreed. “If there were some way to distill out of our minds, our minutes, and any papers that we write, some common essence of our separate understandings about arms control, and organize it in the form of a monograph, it ought to be damned well worth our time, or the Twentieth Century Fund’s money, and a real contribution to public policy,” he wrote to the group.¹¹⁵

The collective primer on arms control would never materialize. In October Bernard Feld, now on sabbatical leave in Europe, took up the task of writing a conference summary report for the Twentieth Century Fund. Feld tried to describe what he thought represented group agreement about the basic problems and aims of arms control. He warned of a “full-fledged technological arms race” and worried (more than the strategists had) about “a very wide diffusion of nuclear weapons among the smaller nations in the next five years or so.” Predictably, a few of Feld’s colleagues rejected the document as a consensus statement. Schelling said he had “strong disagreements”: “It does not seem to me to be closely related to the ground we covered this

¹¹⁴ M.H. Halperin to Summary Study Participants, 15 July 1960, Box 3, Folder 19 “AAAS, Committee on Technical Problems of Arms Limitation, Summer Study on Arms Control, Summer Study on Arms Control 1960, 1/2,” *BTF*.

¹¹⁵ Schelling wanted the group to assemble a guide to the new field, “as serious and technical as we can make it,” expressing “the wisdom and sophistication that may have become the common but inarticulate knowledge of a few hundred people in and out of government in the last few years.” T.C. Schelling to B. Feld (undated memorandum), “Summer Program,” Box 3, Folder 19 “AAAS, Committee on Technical Problems of Arms Limitation, Summer Study on Arms Control, Summer Study on Arms Control 1960, 1/2,” *BTF*.

summer.” In fact “we do not even agree completely on those areas we explored fairly thoroughly.”¹¹⁶ Donald Brennan read the report in draft form and marked it up heavily in pencil, abusing almost every one of Feld’s main points. Where Feld referred to the creation of an invulnerable second-strike force as a prerequisite for arms control, Brennan countered in the margins: “It is in fact somewhat controversial if we want to persuade the Soviets to harden their forces,” apparently gesturing to the argument for “counterforce” nuclear capabilities made by some RAND analysts. Brennan made especially heavy weather over Feld’s section on “secured disarmament,” where Feld proposed a “cutback (but not complete elimination) of nuclear weapons stockpiles and their means of delivery.”¹¹⁷

So when the summer study concluded, the two camps each ventured forth on their own to publish books on arms control. Schelling and Halperin coauthored a paper called “The Functions of Arms Control”—a condensation of the views Schelling had been developing since 1958—and work-shopped it in a new joint Harvard-MIT arms control seminar. (The seminar, which began biweekly meetings in October 1960, was co-convened by Robert Bowie of Harvard’s Center for International Affairs and Max Millikan of MIT’s Center for International Studies. Bowie and Millikan, with the backing of Harvard dean McGeorge Bundy, Jerome Wiesner, Schelling, and a few other major names in the Cambridge area, had won a \$50,000 grant from the Rockefeller Foundation to start the new seminar.¹¹⁸) Over the next few months Schelling and Halperin

¹¹⁶ Schelling also cordially reassured Feld that “the statement is very well written, sober, well balanced, intelligent, and all of that.” In handwriting below the typed note, Schelling added: “Let me emphasize that I could not write a better paper than yours. I just think that we probably do not have a strong statement to make that arises out of our summer’s work.” Thomas C. Schelling to B.T. Feld, 6 October 1960, Box 2, Folder 15 “AAAS, Committee on Technical Problems of Arms Limitation, Summer Study on Arms Control, Negotiation correspondence 1959-1960, 1/2,” *BTF*.

¹¹⁷ Brennan’s annotated copy of “Summary of Report by the Summer Study on Arms Control,” Box 2, Folder 15 “AAAS, Committee on Technical Problems of Arms Limitation, Summer Study on Arms Control, Negotiation correspondence 1959-1960, 1/2,” *BTF*.

¹¹⁸ See “Harvard University and/or Massachusetts Institute of Technology – Disarmament Studies,” the first document in Box 511, Folder 4368 “Harvard – Disarmament Studies (joint program with M.I.T.), 1960-1965,” *RF*.

expanded the paper into a book, *Strategy and Arms Control*, which spoke of arms control as “a promising, but still only dimly perceived, enlargement of the scope of our military strategy.”¹¹⁹

Meanwhile David Frisch collected papers by participants who were more sympathetic to the traditional disarmament perspective, publishing them as *Arms Reduction: Program and Issues*, including an introduction by Bernard Feld.¹²⁰

Consider the second of Sims’s unifying themes: strategic stability. As we have seen in detail, contrasting images of “stability” competed within the early arms control community gathered in Boston. Wiesner’s long-term dynamic feedback concept was meant to stabilize nuclear arms levels at some minimum value while maintaining deterrence. Schelling’s more static concept, inspired by Wohlstetter’s notion of “balance,” was meant to stabilize deterrence so as to remove the temptation to preempt—so that even in the event of an unsettling crisis, a limited conflict, or an accident unfolding in a compressed timeframe, the opponents would not trip headlong into an unlimited conflict. Both of these concepts were represented in the *Daedalus* volume and in the summer study.

Indeed, even the idea that stable deterrence rested in a secure second-strike capability (apparently the least common denominator between Wiesner and Schelling) was not free of controversy in Cambridge that year. Though the published record of the conferences does not reveal it, some participants supported very different views about the requirements of deterrence.¹²¹ Henry Rowen provides one clear example. Rowen had been a RAND analyst for

¹¹⁹ Thomas C. Schelling and Morton H. Halperin, *Strategy and Arms Control* (New York: The Twentieth Century Fund, 1961), 1.

¹²⁰ David H. Frisch, ed., *Arms Reduction: Program and Issues* (New York: The Twentieth Century Fund, 1961). The Schelling-Halperin book was the much better remembered among the strategic arms control community. See, e.g., Michael Krepon (guest editor), “Has arms control worked?,” *Bulletin of the Atomic Scientists* 45, no. 4 (1989): 26-45.

¹²¹ The lone exception was Herman Kahn’s *Daedalus* paper, which included his soon-to-be famous typology of various species of deterrence. Herman Kahn, “The arms race and some of its hazards,” *Daedalus* 89, no. 4, “Arms Control,” (1960): 744-780.

much of the 1950s, and participated in both the *Daedalus* conference and the summer study, before taking up residence at the Harvard Center for International Affairs in 1960. He became a regular interlocutor in the Harvard-MIT arms control seminar that year (before joining the Pentagon in 1961). He was therefore very much part of the early Boston arms control discussion.

Influenced by a group at RAND that had pioneered studies of “counterforce” or “war-fighting” nuclear strategies during the 1950s, Rowen had become convinced that a model of nuclear deterrence in which retaliatory forces would execute unspecified (or purely civilian or economic) “damage” was simplistic, and might contain hidden instabilities.¹²² The idea of secure second-strike forces for the destruction of cities didn’t answer to the way a nuclear war could actually be fought. As Rowen wrote to Wiesner in a long letter from 1960, disagreements over military posture and disarmament boiled down to fundamentally different views about the “character of possible nuclear war.” He thought nuclear war would be a military contest, with forces opposing forces, not “a pure duel of missiles, one salvo to a side.” U.S. strategy should be “to keep forces alive, to continue to pose threats, to execute some threats, and to reduce enemy nuclear power, including his power to attrite our forces and attack our civil society.” Wiesner’s assertion that the U.S. would soon possess a more-than-adequate deterrent composed chiefly of land- and sea-based ballistic missiles made Rowen “uncomfortable” (as did the bulk of Wiesner’s arguments).¹²³

¹²² On the development of counterforce ideas at RAND, see Kaplan, *The Wizards of Armageddon*, 201-219.

¹²³ More specifically, Wiesner’s exchange ratio analysis struck Rowen as “incomplete and somewhat misleading,” since it did not discuss variations in the yield or the reliability of attacking warheads. Modest improvements in both would greatly reduce the exchange ratios that Brennan had computed, reducing confidence in the argument for disarmament Wiesner had presented. So would improved accuracies and multiple warheads. “One booster,” Rowen said (foreshadowing the MIRV developments of the 1960s), “might be able to send many warheads against a constellation of fixed points.” The exchange ratio was a useful index “if generalized to include the relative effort (e.g. in defense budgets) needed to accomplish certain objectives”—i.e., as a cost-exchange ratio (a RAND systems analysis concept that would resurface in later debates over missile defense). It was less helpful, Rowen felt, as a ratio of pure missile numbers. Put bluntly, Rowen thought Wiesner had done the wrong calculations. A much smaller number of weapons could land a decisive counterforce blow than Wiesner had supposed; and as far as

Rowen was far more sympathetic to Schelling's view, but still found the reigning model of deterrence worryingly underspecified. In the Harvard-MIT arms control seminar that fall, presenting a written response to the Schelling-Halperin paper on arms control, Rowen said he shared the authors' desire to eliminate incentives for a preemptive strike. But they hadn't adequately extended their model to cover purely military contests. "If [a] situation...were to be attained with both sides having 'stable' postures, a war might still occur in which it was clearly understood...that civil targets were to be avoided." The prospect of military victory (not just civil destruction) had to be removed before any scheme of deterrence could be considered "stable."¹²⁴

Finally, consider the idea of the "firebreak." By 1960 the notion of putting up a high threshold between conventional war and escalation to nuclear attack had gained popularity among many thinkers. Henry Kissinger, in his article for the *Daedalus* volume, recanted his earlier opinion that nuclear weapons might be employed in limited war, arguing now that it was better that limited conflicts should remain conventional.¹²⁵ In their paper on the test ban, Donald Brennan and Morton Halperin made a long list of supporters of the firebreak idea, including such RAND figures as Bernard Brodie, Herman Kahn, William Kaufmann, Albert Wohlstetter, and Rowen. In 1959 Thomas Schelling had also argued that a bright line separating conventional and

Rowen could tell, this deflated Wiesner's argument for deterrence "as a way-station to total disarmament even though we might all feel better off with such a situation." Henry Rowen to Jerome B. Wiesner, 17 June 1960, Box 8, Folder 255 "Daedalus Meeting May 20, 1960," *JBW*. For a more complete expression of Rowen's views in this period, see Henry Rowen, "National security and the American economy in the 1960s," Study Paper No. 18, prepared for the Joint Economic Committee, Congress of the United States (Washington, DC: Government Printing Office, 1960).

¹²⁴ Henry Rowen, "Some Suggestions on Reducing the Incentive to Pre-empt," part of "Joint Arms Control Seminar, Schedule Memo No. 2A," Box 3, Folder 1 "Joint Harvard-MIT Arms Control Seminar - Papers Given 1960-62," *LPB*.

¹²⁵ See Henry Kissinger, "Limited war: Conventional or nuclear? A reappraisal," *Daedalus* 89, no. 4, "Arms Control" (Fall 1960): 800-817; Henry Kissinger, *Nuclear Weapons and Foreign Policy* (New York: Harper, 1957); and Freedman, *The Evolution of Nuclear Strategy*, 101-105.

nuclear force, while strictly non-logical, might be useful in controlling escalation and limiting the damage of war in the event it occurred.

It is true that virtually everyone at the Cambridge meetings in 1960 would have subscribed to some version of the firebreak. Yet such agreement was superficial, covering deep underlying disagreements about how one would achieve a firebreak, and what a firebreak was for. Schelling and the RAND group wanted a firebreak for reasons of crisis and conflict stability, placing a well-defined ceiling on the level of violence and restraining the escalation from limited conflict to general nuclear war. And Schelling had argued that the firebreak would be maintained not through any modification to the arsenal but through tradition, “precedent, convention, and the force of suggestion.”¹²⁶ The firebreak was illogical, perhaps, but it had practical utility. Disarmament played no part in this framework, nor was disarmament the reason for policing a high nuclear threshold.

But for other members of the community, not only did the firebreak comport intuitively with idea of ultimately eliminating nuclear weapons, but disarmament was imagined as *central* to the initial achievement of a firebreak. One example appears in an unattributed paper in Jerome Wiesner’s file on the PSAC Panel on Arms Limitation and Control, dated February 1960 and titled “Fire-Break: An Approach Toward Some Nuclear Disarmament.” The paper recommended cutting a large gap in the arsenal between multi-megaton thermonuclear weapons and conventional arms, completely eliminating “middle-grade” nuclear weapons. The large weapons would maintain deterrence; the absence of smaller weapons would prohibit the escalation of a smaller war into a full-scale strategic exchange.¹²⁷ Bernard Feld, too, outlined a similar scheme in a private letter sent to participants of the 1960 summer study, advocating for what he called

¹²⁶ Schelling, *The Strategy of Conflict*, 260.

¹²⁷ “Fire-Break: An Approach toward Some Nuclear Disarmament,” 24 February 1960, Box 7, Folder 227 “Arms Limitation and Control Panel, 1960,” *JBW*.

“secured disarmament.” By this he meant “an agreement to reduce the level of all armaments with the retention of an agreed stock of nuclear weapons and their delivery systems for the sole purpose of insuring that no nuclear weapons will be used in any possible conflict on any level.”

For Feld and many of his colleagues, tradition and convention alone were not enough to police a firebreak. Some form of disarmament was necessary.¹²⁸

The intellectual rifts within the nascent arms control community were manifest not only as differences of opinion, but also as differences in attitude toward the process of arms control discussion and negotiation. The growing divisions were exposed during and after the Pugwash meeting in Moscow in December 1960, for example. Paul Doty summarized the disagreements in his speech at the conference. For the benefit of his Soviet counterparts, Doty distinguished between arms control and disarmament as these concepts had evolved in studies and debates in the United States. “Arms control tends to imply a primary concern with specific measures that will diminish the present danger,” he said. “Disarmament studies imply a commitment to find ways of reaching a state of substantial disarmament on a definite schedule. Thus the long range aim can be the same but the emphasis is different.”¹²⁹

Doty attributed these contrasts to differences in risk preference. At one end of the spectrum “we find persons who require certain gain at no risk as their criterion for acceptable arms control measures.” These conservatives “emphasize that every such [limited] agreement on

¹²⁸ B.T. Feld, “Where Do We Go From Here?,” Memorandum to Summer Study Participants, 4 July 1960, Box 3, Folder 19 “AAAS, Committee on Technical Problems of Arms Limitation, Summer Study on Arms Control, Summer Study on Arms Control 1960, 1/2,” *BTF*. Underlining in original.

¹²⁹ Paul Doty, “Current Attitudes on Disarmament in America,” Pugwash Conference, Moscow, 1960 (undated draft in Doty’s personal collection). I am grateful to archivists at the Harvard University Archives, especially Timothy Driscoll and Robin McElheny, for arranging for me to view some of materials in Doty’s personal archive, even while the collection was still being processed. See the Paul M. Doty Personal Archive, 1940–2011 (Accession 18511), Harvard University Archives, Cambridge, MA. Doty shared this particular document with Alex Wellerstein before Doty’s death. See Alex Wellerstein, “R.I.P.: Paul Doty and Lawrence H. Johnston,” *Restricted Data: The Nuclear Secrecy Blog* (6 December 2011), <http://blog.nuclearsecrecy.com/2011/12/06/r-i-p-paul-doty-and-lawrence-h-johnston/>.

arms control can either increase or decrease the probability of war.” But closer to the disarmament advocates’ end of the spectrum, where Doty located himself, were those who sought military security “so that arms control can proceed to the turn-around point” (a phase during which “the automatic escalation of the arms race would stop; the time when new weapons systems would no longer be introduced”). Doty found “stable deterrence” to be an “unfortunate” label, because it implied a static state of affairs. Doty did not want that kind of stability; like Wiesner, he wanted to see “a very large reduction in nuclear armaments.”¹³⁰

When the Western and Soviet delegations to the meeting issued a joint statement affirming “disarmament under effective control,” Thomas Schelling declined to sign it.¹³¹ Instead he wrote a letter published in the *New York Times* after returning to the United States, criticizing the Soviets for scoring propaganda points with glib talk of disarmament. “There was no evidence that the Soviets had given serious thought to the problems raised by their own disarmament proposals. Disarmament is complicated business, as difficult as any major innovation in military strategy, or more so.”¹³²

In the Harvard-MIT arms control seminar that month the stage was set for a heated debate about Pugwash and disarmament, featuring a verbal spat between Schelling and Wiesner. Schelling again dismissed the proceedings as bluster. “The Russians present were on average fifteen years or more older than the Americans and had passed the years of greatest activity and intellectual capability,” he said. They “were almost wholly preoccupied by the notion of a quick, total and complete disarmament.” Wiesner countered: “The behavior of the Russians at the

¹³⁰ Paul Doty, “Current Attitudes on Disarmament in America,” Pugwash Conference, Moscow, 1960 (undated draft in Doty’s personal collection).

¹³¹ “Disarmament and Arms Control: Statement from the Sixth Pugwash Conference,” *Bulletin of the Atomic Scientists* 17, no. 2 (1961): 74-75.

¹³² Thomas C. Schelling, “Discussing arms control: Strong Soviet motivation toward disarmament doubted,” *New York Times* (30 December 1960): 18.

conference could confirm any view that one came with.” Wiesner had gone to the conference hopeful for serious discussion of arms control; Schelling had been immovably pessimistic. “The Soviets really want disarmament,” Wiesner declared. The past months had seen “a tremendous shift in the willingness of the Russians to talk about international security forces and controls.” Without trust—and a willingness to take the other side seriously—what hope did arms control have?¹³³

Conclusion

In the spring of 1958 Willy Higinbotham penned a note to his old friend Bernard Feld. “Last summer I got sucked into a study of defense in Washington,” he wrote, speaking of his work on the Gaither Panel. “There I learned the horrid details of this frightening age. I don’t feel very good about the committee’s work or about my part in it.” Higinbotham, like Feld a veteran of the Manhattan Project and the postwar atomic scientists’ movement, sensed that a troubling shift was afoot.

Everyone is properly scared but the almost universal reaction is to seek security through ever greater destruction. It is discouraging to find that few technical people hold out any hope for disarmament...One finds criticism of disarmament in classified defense studies which have nothing to do with disarmament. The young generation seems to consider nuclear arsenals a normal condition.¹³⁴

There was nothing “normal” about nuclear arsenals for the bomb project veterans and disarmament advocates. But by 1959 and 1960 a large proportion of nuclear experts, even from

¹³³ “Joint Arms Control Seminar, Abstract of Discussion, Meeting of December 12, 1960,” Box 2, Folder 48 “Joint Harvard-MIT Arms Control Seminar - Minutes, 1960-61,” *LPB*.

¹³⁴ Willy Higinbotham to Bernard Feld, 17 March 1958, Box 2, Folder 16 “AAAS, Committee on Technical Problems of Arms Limitation, Summer Study on Arms Control, Negotiation correspondence 1959-1960, 2/2,” *BTF*.

the older generation, were resigned to the idea that there was no getting rid of nuclear weapons, no prospect for total disarmament. The question was, as Leo Szilard put it, how to live with the bomb and survive.¹³⁵ For strategic analysts like Thomas Schelling the answer lay in mutual deterrence and an abandonment of the old disarmament shibboleths. For thinkers of a different sensibility it remained in reduction, if not elimination, of nuclear arsenals. For everyone, it came to depend in some way upon the virtues of stability.

Marc Trachtenberg writes: "The stability doctrine...developed in a fairly natural way out of the body of thought that had been concerned primarily with strategic vulnerability." The idea that "one might be able to achieve a world where each side's strategic forces were secure" had, by 1960, come "to provide the conceptual basis for American thinking on arms control."¹³⁶ This has been the dominant view of historians and international relations scholars. Arms control thought evolved "naturally" from a theory of deterrence articulated at the end of the 1950s, a response to the evolving technology of nuclear weapons and protracted Cold War hostility.

The argument of this chapter has been that the history of arms control thought had a much more local and contingent character. A community of interest in arms control was built, piece-by-piece through personal connections and the endless search for grant money, out of the raw material of an earlier group of nuclear disarmament advocates in the Boston area. Through contacts with government advisors and strategic analysts, many of these disarmament advocates adjusted to the idea of deterrence. But many also remained committed to disarmament as chief among arms control's purposes.

Conceptually, the idea of stability did not spring naturally from the theory of deterrence, nor was it decreed by the objective realities of international politics or the nature of technology.

¹³⁵ Leo Szilard, "How to live with the bomb and survive," *Bulletin of the Atomic Scientists* 16, no. 2 (1960): 59-73.

¹³⁶ Trachtenberg, "Strategic thought in America," 317.

Stability could be taken from an intellectual toolkit much closer to hand. Jerome Wiesner—a control systems engineer and cyberneticist by background, and a participant in the original Boston disarmament group—proposed to stabilize and correct the arms race through comprehensive arms control systems and processes of long-term dynamic feedback. There was no natural, fixed category of stability; there were different flavors for different tastes.

Schelling, who announced an arms control revolution in 1960, emphasized a more static concept of stability—stability as resistance to shock, stress, and miscalculation. But Wiesner, who wanted arms control “to halt the frightening arms race and to provide, by rule of law, the security now sought so futilely from nuclear armaments and ballistic missiles,” marshaled stability to different ends. Each view was designed to meet a crisis; but the crisis each imagined was of a different kind.¹³⁷

¹³⁷ Wiesner, “Comprehensive arms-limitation systems,” 915.

CHAPTER 2

The Consultants:

Ballistic Missile Defense, Lasers, and the Origins of Nonlinear Optics

Laser technology contains a core of foreboding and myth. It is a clean sort of lethal package we are dealing with, a well-behaved beam of photons, an engineered coherence, but we approach the weapon with our minds full of ancient warnings and fears. (There ought to be a term for this ironic condition: primitive fear of the weapons we are advanced enough to design and produce.)

— Don DeLillo, “Human Moments in World War III” (1983)¹

Introduction

On June 21st, 1985, researchers at the Air Force Maui Optical Station (AMOS), a facility sitting atop the summit of Mt. Haleakala 10,000 feet above the ocean waves, fired a beam from an argon-ion gas laser. Climbing upward some 220 miles, through the atmosphere and into outer space, the blue-green laser light struck an eight-inch-wide mirror fixed to the side of the space shuttle Discovery, which orbited overhead at roughly 17,000 miles per hour. The astronauts aboard Discovery recorded images of the laser light as they viewed it from the spacecraft, pulsing periodically as scientists on the ground at AMOS adjusted beam-correcting optics to guide the light to its target. The test was only the latest in a series of hundreds performed over previous months. But this one had been carried out for the purposes of a public relations spectacle. “This is an important step in a series of steps that will prove we can effectively shoot lasers from the ground into space without suffering unacceptable atmospheric losses,” said James

Notes

¹ Don DeLillo, “Human Moments in World War III,” in *The Angel Esmeralda* (New York: Scribner, 2011 [1983]), 25-44, on 35.

A. Abrahamson, Jr., the head of the Strategic Defense Initiative (SDI) Organization, the new Pentagon office tasked with realizing President Reagan's ambitions for ultrahigh-technology ballistic missile defense.²

The mountaintop facility on Maui had always been called AMOS, but for most of its life the acronym had stood for something slightly different: "ARPA Midcourse Optical Station," so named after its original sponsor, the Advanced Research Projects Agency. Conceived in 1961, AMOS had served the Defense Department for more than twenty years as a specialized observatory, using its infrared telescopes to track the radiation signatures of ballistic missile reentry vehicles—the special bullet-shaped, heat-proof capsules designed to deliver thermonuclear warheads successfully through reentry, from outer space through the atmosphere to their targets below. Researchers at AMOS observed the missiles in the "midcourse phase" of flight, at the peak of their intercontinental arc hundreds of miles above the ocean, fired from the coast of California to splash-down at Kwajalein Atoll in the balmy South Pacific. As ARPA turned over management of the facility to the Air Force in the 1970s, high-power lasers were built at the station as part of the "compensated imaging system," a technology designed to correct automatically for the distortions of atmospheric turbulence. The system would allow laser beams to find and lock onto—perhaps someday to blast apart—objects in outer space like satellites and ballistic missile reentry vehicles.³

² William J. Broad, "Laser beam hits 8-inch target in space," *New York Times* (22 June 1985): 11. Mt. Haleakala had also been the location of a Baker-Nunn satellite camera installed by Harvard's Smithsonian Astrophysical Observatory as part of the International Geophysical Year in 1957 (one of only three in the United States). See W. Patrick McCray, *Keep Watching the Skies!: The Story of Operation Moonwatch and the Dawn of the Space Age* (Princeton, NJ: Princeton University Press, 2008), 79.

³ Institute for Defense Analyses, "DARPA Technical Accomplishments: An Historical Review of Selected DARPA Projects," Volume 1, IDA Paper P-2193, February 1990, Chapter X, "AMOS: ARPA Midcourse Optical Station," 10.1–10.15; Federation of American Scientists, "Air Force Maui Optical Station (AMOS)," <http://www.fas.org/spp/military/program/track/amos.htm>.

Sitting in his office about a year after the space shuttle laser test, the physicist Norman Kroll was asked about a report he had written back in 1968 for an organization called the Institute for Defense Analyses (IDA).⁴ IDA had been under contract to ARPA to provide expert advice on its missile defense programs. Titled “Transient Effects in Stimulated Rayleigh Scattering,” Kroll could only generalize about the document’s contents. “In fact there were a whole series of papers,” he remarked, “some of which are classified, on the propagation of high powered laser beams through the atmosphere, which is the context of that work.” “For what purpose?” he was asked.

Well, why do people want to propagate high intensity laser beams through the atmosphere? It’s the reason they do now. A very old problem this modern SDI stuff.... That 1968 paper is probably an outgrowth of the 1967 laser summer study. It’s just the published output. The fact is, there’s a *Physical Review* paper on that subject also.⁵

Kroll’s recollections compress a complex piece of history—a story of dense entanglements between American ideas and institutions in the Cold War. The connections between laser weapons and ballistic missile defense, military consulting and summer studies, *Physical Review* articles and the birth of an important field of physics called nonlinear optics, which Kroll and his colleagues helped to create in the 1960s, are the subject of this paper.

Almost thirty years ago Paul Forman ignited debate among historians about the relationship between science and the Cold War. In his view, support of basic physics research in the United States by the Defense Department and the Atomic Energy Commission had altered

⁴ IDA is usually pronounced “Ida,” as in the name.

⁵ Interview of Dr. Norman Kroll by Finn Aaserud, 28 June 1986, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD, <http://www.aip.org/history/ohilist/28394.html> (hereafter cited as Kroll interview, 1986).

the discipline, its practices, and its intellectual content in profound and insidious ways. Behind quantum electronics, Forman said, lurked the distorting interests and priorities of the national security state. Remarkable devices like lasers and atomic clocks were built with the help of military agencies' ballooning budgets. But with what consequences for the intellectual character of physics? "Certainly not new understanding," Forman lamented. Experts in quantum electronics evolved a unique new American style—"a kind of instrumentalist physics of virtuoso manipulations"—better suited to weapons engineering than to posing deep questions about natural reality.⁶

Historians have been ambivalent about the distortion thesis and its moral narrative of pure science corrupted by military and mercenary interests. Some have rejected the idea of distortion altogether. Daniel Kevles summed up his dismissal in a pithy phrase: "Physics is what physicists do—or have done."⁷ Yet if Forman's interpretation is too severe, there is something equally unsatisfying about Kevles's relativism. It fails to capture the sense that something profound *did* happen during the Cold War to U.S. science, its patronage relationships, and its social and institutional structure. It misses the peculiarity and richness that make Cold War science a category worth interrogating historically. Some scholars have therefore turned to more careful examinations of the social, intellectual, and material connections between the mobilized Cold War state and the development of American science. They have even sometimes inverted Forman's argument, showing how military interests and military dollars were often productive of

⁶ Paul Forman, "Behind quantum electronics: National security as basis for physical research in the United States, 1940–1960," *Historical Studies in the Physical and Biological Sciences* 18 (1987): 149–229, on 216.

⁷ Daniel Kevles, "Cold war and hot physics: Science, security, and the American State, 1945–56," *Historical Studies in the Physical and Biological Sciences* 20 (1990): 239–264, quotation on 263.

fundamental research—knowledge bought with military money and materiel, but knowledge all the same.⁸

Virtually no one, however, has wrestled with the specific inspiration for the distortion thesis: the history of postwar quantum electronics.⁹ Here, I dig into a case from the history of quantum electronics after the invention of the laser in 1960. The origins and early development of a field known as “nonlinear optics”—the science of the interaction between intense light and matter—were inseparable from defense efforts in the post-Sputnik period. In its early days the laser was touted as a technological wonder-device with a head-spinning array of applications. At the beginning of the missile age, military planners, building on the optimism of their scientific advisors, hoped that the new invention might become a revolutionary military tool—even a means to destroy enemy ballistic missiles in flight. Powerful lasers seemed poised to solve one of the Cold War’s most challenging strategic and technological problems: ballistic missile defense.

But generating a beam of sufficient power to knock out a nuclear warhead streaking downward from outer space, and propagating the beam through the earth’s atmosphere, presented major technical roadblocks. It was in thinking about some of these problems that an elite group of military consultants working for the Institute for Defense Analyses (prominent among them the physicists Nicolaas Bloembergen, Charles Townes, Keith Brueckner, and Norman Kroll) turned their expertise in quantum electronics toward the physics of light-matter

⁸ See, for example, Peter Galison, “Physics between war and peace,” in E. Mendelsohn, M.R. Smith and P. Weingart, eds., *Science, Technology, and the Military, Volume 13* (Amsterdam: Kluwer Academic Publishers, 1988), 47–86; Spencer Weart, “Global warming, Cold War, and the evolution of research plans,” *Historical Studies in the Physical Sciences* 27, no. 2 (1997): 319–356; Edward Jones-Imhotep, “Disciplining technology: Electronic reliability, cold-war military culture, and the topside ionogram,” *History and Technology* 17 (2000): 125–175; Naomi Oreskes, “A Context of Motivation: US Navy Oceanographic Research and the Discovery of Sea-Floor Hydrothermal Vents,” *Social Studies of Science* 33 (2003): 697–732; Benjamin Wilson and David Kaiser, “Calculating times: Radar, ballistic missiles, and Einstein’s relativity,” in *Nation and Knowledge in the Cold War*, ed. Naomi Oreskes and John Krige (Cambridge, MA: The MIT Press, forthcoming).

⁹ One exception is Joan Lisa Bromberg, “Device physics vis-à-vis fundamental physics in Cold War America: The case of quantum optics,” *Isis* 97, no. 2 (2006): 237–259.

interaction, helping to forge the new field of nonlinear optics. Thus even within quantum electronics—the field Forman presented as an object lesson in science warped by the military-industrial complex—new knowledge could take root and grow. The dream of missile defense, and the advent of novel technology, had encouraged the growth of an important new field of physics.¹⁰

Yet injections of government money and technological innovations were not enough. Nonlinear optics emerged in the writing and research of a small group of contract consultants to the federal government, working through a private not-for-profit advisory corporation.¹¹ The field's history invites a more careful consideration of the specific institutions designed to harness scientific expertise to the U.S. Cold War mission, and the kinds of hybrid activities fostered by such institutional arrangements. The consultants typed up classified reports detailing the implications of their work for missile defense while, all in one motion, publishing major articles that mapped the limits of the new field. They mixed their defense advising and their academic research in ways that dissolve a categorical separation between civilian and military, or pure and applied science.

ARPA and Project Defender

A shiny aluminum sphere whirled in orbit around the earth in October 1957 and seemed to announce, with regular electronic chirps, that the Soviet Union had taken the lead in the Cold

¹⁰ Norman Kroll, in his interview, put it more directly: “All these nonlinear optics questions have come up in the question of laser beam weapons.” Kroll interview, 1986.

¹¹ Nonlinear optics also, it should be said, emerged from the laboratories of several major industrial corporations (as all laser-based sciences and the laser itself had). Researchers in huge, well-equipped and well-funded corporate labs made some of the most important early advances in nonlinear optics and related fields. This point has been appropriately stressed, for example, in Joan Lisa Bromberg, *The Laser in America* (Cambridge, MA: The MIT Press, 1991). My claim is that a key ingredient—one I find absolutely indispensable in describing the history of U.S. nonlinear optics—is missing from a picture populated only by the laser, opulent industrial laboratories, and federal grants.

War contest for technological superiority. Newspapers splashed dramatic headlines across their front pages; ambitious politicians capitalized on the media frenzy, sounding the alarm that America lagged in science and military technology, now more vulnerable than ever to Soviet aggression. The Sputnik launch may have barely registered on Dwight Eisenhower's personal radar (at first), confident as he was in the health of longstanding U.S. rocketry and ballistic missile programs. But soon the president, too, reacted to the clamor by authorizing dramatic new programs and sweeping legislation designed to put the U.S. back in the scientific and technological vanguard.¹²

The Sputnik surprise would become a flashpoint in Cold War ideology, helping to tighten the embrace between science and defense. "Big Science" got bigger in every conceivable way, from the size of experimental machinery to the rosters of professional researchers and classroom students.¹³ Scientific expertise cozied up to the highest levels of power in new organizations like the President's Science Advisory Committee, which graduated from a more peripheral bureau in the Office of Defense Mobilization (ODM) to an independent executive office with direct access to the White House.¹⁴ Sputnik was a catalyzing moment in the nuclear competition—"the crucial psychological landmark in the course of postwar arms development," according to Herbert York, the first Director of Defense Research and Engineering in the Department of Defense.¹⁵

To a degree unprecedented since the Second World War, American officials worked to expand the institutional links between scientific expertise, technical innovation, and military

¹² Yanek Mieczkowski, *Eisenhower's Sputnik Moment: The Race for Space and World Prestige* (Ithaca, NY: Cornell University Press, 2013); James R. Killian, Jr., *Sputnik, Scientists, and Eisenhower: A Memoir of the First Special Assistant to the President for Science and Technology* (Cambridge, MA: The MIT Press, 1977), esp. Chapter 1, "Sputnik and its Shock Waves," 1–17.

¹³ David Kaiser, "Cold War requisitions, scientific manpower, and the production of American physicists after World War II," *Historical Studies in the Physical and Biological Sciences* 33 (2002): 131–159.

¹⁴ Zuoyue Wang, *In Sputnik's Shadow: The President's Science Advisory Committee and Cold War America* (Piscataway, NJ: Rutgers University Press, 2008).

¹⁵ Herbert F. York, *Race to Oblivion: A Participant's View of the Arms Race* (New York: Simon & Schuster, 1970), 20, 106.

power, especially in the area of strategic nuclear delivery vehicles. Top advisers had called for an intensification of U.S. ballistic missile development since at least the mid-1950s. In early 1955 the Technological Capabilities Panel of the ODM—or the “Killian Panel,” so nicknamed for its chairman, MIT president (and soon the Eisenhower’s first Special Assistant for Science and Technology) James R. Killian, Jr.—had urged that “the National Security Council recognize the present Air Force program” for ICBM development “as a nationally supported effort of highest priority.”¹⁶ Eisenhower himself attached “enormous psychological and political significance” to the pursuit of strategic missiles, reacting to the panel’s recommendations by authorizing accelerated ICBM and intermediate-range ballistic missile (IRBM) programs in each of the three armed services.¹⁷ But after Sputnik had flaunted the Soviet Union’s own technological capabilities two years later, the need to streamline and centralize U.S. efforts in strategic nuclear weaponry seemed desperate. When the ODM’s Security Resources Panel—better remembered as the Gaither Panel—filed its landmark top-secret report *Deterrence and Survival in the Nuclear Age* in November of 1957, it demanded not just more missiles but better institutional coordination for strategic defense. “The new weapons systems,” it explained, “in cutting across traditional Service lines, have caused management problems which have been difficult to resolve... We have lost ability to concentrate resources...and to change direction or emphasis

¹⁶ Report by the Technological Capabilities Panel of the Science Advisory Committee, 14 February 1955, in Department of State, S/S-RD Files: Lot 71 D 171, U.S. Department of State, *Foreign Relations of the United States, 1955–1957, National Security Policy*, Volume XIX, Document 9. On the history of the Technological Capabilities Panel and the position of elite science advising in the Eisenhower administration, see Richard V. Damms, “James Killian, the Technological Capabilities Panel, and the emergence of President Eisenhower’s ‘Scientific-Technological Elite’,” *Diplomatic History* 24, no. 1 (Winter 2000): 57–78.

¹⁷ Eisenhower to Secretary of Defense Charles Wilson, 15 December 1955, quoted in Damms, “James Killian,” 69.

with the speed that a rapidly developing international situation and rapidly developing science and technology make necessary.”¹⁸

The alarmists got their coordinating agency. By late 1957 Neil McElroy, the former Proctor & Gamble executive who had been sworn in as Secretary of Defense only days after the Sputnik launch, testified in Congress that the DOD planned “to establish...a special agency”—what he sometimes called a “fourth service,” distinct from the Air Force, Army, and Navy—“to handle our satellite and space research and development projects.” The new agency would also tackle “the Department of Defense program in the antiballistic missile field.”¹⁹ In January of 1958 Eisenhower requested \$10 million from Congress for the creation of an Advanced Research Projects Agency (ARPA). Later that month, Eisenhower consecrated the new “fourth service” in his State of the Union address: “Some of the important new weapons which technology has produced do not fit into any existing service pattern. They cut across all services, involve all services, and transcend all services....In recognition of the need for single control in some of our most advanced development projects, the Secretary of Defense has already decided to concentrate into one organization all the antimissile and satellite technology undertaken within the Department of Defense.”²⁰ By February 1958, officially established under a DOD Directive with a mandate to pursue the most cutting-edge military technology, ARPA was up and running in new Pentagon offices under the directorship of another former business executive, Roy W.

¹⁸ U.S. Office of Defense Mobilization, Science Advisory Committee, Security Resources Panel, “Deterrence and Survival in the Nuclear Age,” 7 November 1957 [“Gaither Report”], 8–9. Available online from the National Security Archive, <http://www2.gwu.edu/~nsarchiv/NSAEBB/NSAEBB139/nitze02.pdf> (accessed 5 September 2013).

¹⁹ McElroy quoted in Richard J. Barber Associates, Inc., *The Advanced Research Projects Agency, 1958–1974*, report prepared for the Advanced Research Projects Agency, Dec. 1975, DTIC no. ADA154363 (Washington, DC), on II–5.

²⁰ Eisenhower quoted in Barber Associates, *Advanced Research Projects Agency*, on II–14.

Johnson. Herbert York, the first man to head up the nation's thermonuclear weapon design lab at Livermore, became ARPA's chief scientist.²¹

ARPA absorbed the DOD's work on the detection and concealment of warhead reentry vehicles. It undertook a massive program, codenamed VELA, on the detection of clandestine nuclear tests in every environment. And it became the parent agency for ballistic missile defense. Missile defense was arguably one of the Cold War's most challenging and tantalizing physics and engineering problem. Back in 1955 the Killian Panel had cautioned that "although the technical problems that must be solved in attaining a defense against intercontinental ballistic missiles are extremely complex, there are sufficiently promising leads to justify an expanded and accelerated research effort on a broad front." So the group had recommended a full-scale "investigation of the basic problems of detection, interception and destruction"—seeing, hitting, and killing rockets and warheads.²² The Gaither Panel in 1957 had made an even more urgent and concrete plea for missile defense work. A section on "active defenses" (i.e., defense by the active destruction of attacking forces, rather than "passively" protecting against their explosive violence), overseen by MIT electrical engineer Jerome Wiesner, identified two general classes of missile-intercepting systems to be pursued in succession. The first was to be jury-rigged from existing technology like the Nike Hercules anti-aircraft missile; the second was more exploratory in nature, designed "to intercept the incoming warheads at much higher altitudes" and thus protect larger areas of U.S. real estate. The decoys and chaff that would accompany a sophisticated ballistic missile attack (the better to hide real warheads amid a clutter of metallic

²¹ Barber Associates, *Advanced Research Projects Agency*, II-25-II-26.

²² Report by the Technological Capabilities Panel of the Science Advisory Committee, 14 February 1955, in Department of State, S/S-RD Files: Lot 71 D 171, U.S. Department of State, *Foreign Relations of the United States, 1955-1957, National Security Policy*, Volume XIX, Document 9.

junk, which would produce confusing radar returns) demanded a “high-priority research and test program.”²³

Work proceeded more or less along the lines drawn by the Gaither Report. The Army and the Air Force had long fought jurisdictional battles over who had the right to design, build and launch strategic missiles, and their dispute extended naturally to missile defense. Each department had had its own experimental program for several years: the Air Force its Project Wizard, the Army its Project Nike, each building on anti-aircraft weapons platforms. Engineers at Bell Laboratories designed the Nike system, upgrading the Nike Hercules missile to the more powerful Nike Zeus for the antimissile job. In January 1958 McElroy broke the stalemate by giving the Army and its Nike program funding priority and permission to go ahead with development; the Air Force program took a backseat.²⁴

Missile defense work within ARPA was to be collected under an umbrella program called Project Defender, soon the agency’s biggest and cash-hungriest enterprise.²⁵ By November of 1958 ARPA officials had initiated a Guide Line Identification Program for Antimissile Research, or GLIPAR, enlisting the help of private industry to determine the long-term technical challenges of missile defense. Early the following year, ARPA Director Roy Johnson invited representatives from 48 separate organizations, including universities and industrial firms, to bid on contracts for various missile defense system components. Thirty firms submitted bids and a committee of ARPA staff and consultants adjudicated the various proposals, ultimately awarding twelve

²³ U.S. Office of Defense Mobilization, “Deterrence and Survival in the Nuclear Age,” 28–29.

²⁴ Donald R. Baucom, *The Origins of SDI, 1944–1983* (Lawrence: University Press of Kansas, 1992), 11–13.

²⁵ Barber Associates, *Advanced Research Projects Agency*, III–54; Baucom, *The Origins of SDI*, 15–17.

exploratory contracts worth roughly \$135,000 each to firms like Ramo-Woolridge, RCA, and Hughes Aircraft Company.²⁶

As Robert Seidel and Joan Lisa Bromberg have demonstrated, the military services had an abiding enthusiasm for technologies of coherent radiation. The maser (“microwave amplification by the stimulated emission of radiation”) was invented and developed largely with the support of the Joint Services Electronics Program (JSEP), initially by importing techniques (and leftover gadgetry) from the wartime radar projects. Later in the 1950s, as proposals emerged to push the maser concept to smaller electromagnetic wavelengths in the infrared and optical regions, the Defense Department’s interest only increased.²⁷

ARPA got into the game early, beginning its support for laser research in 1958. A representative from a small defense contractor called TRG (Technical Research Group) approached ARPA officials with a request for a laser research and development contract. TRG had recently employed Gordon Gould, a former Columbia University graduate student who had independently arrived at some of the crucial laser design insights that physicists Charles Townes (then on the faculty at Columbia) and Arthur Schawlow (at Bell Laboratories) had discussed in their classic 1958 paper on the principles of an “optical maser.” TRG was seeking federal defense dollars to kick-start its laser program, promising applications in laser radar, communications and power transmission.²⁸

There was clearly no shortage of money for the endeavor. When TRG made an initial request for \$300,000 from ARPA in 1958, the agency responded by awarding TRG close to \$1

²⁶ Barber Associates, *Advanced Research Projects Agency*, III–57.

²⁷ Robert W. Seidel, “From glow to flow: A history of military laser research and development,” *Historical Studies in the Physical and Biological Sciences* 18, no. 1 (1987): 111–147; Bromberg, *The Laser in America*.

²⁸ Gould dubbed his concept the “LASER,” replacing “microwave” with “light.” Bromberg, *The Laser in America*, 69–73. The classic paper by Schawlow and Townes was published as A.L. Schawlow and C.H. Townes, “Infrared and optical masers,” *Physical Review* 112, no. 6 (1958): 1940–1949.

million instead.²⁹ And in the summer of 1960, after representatives of Hughes Aircraft announced that a scientist named Theodore Maiman had constructed a working laser at its Research Laboratories in Malibu earlier that spring, ARPA padded TRG's contract with another \$750,000.³⁰

IDA and Academic Defense Consulting

To understand how nonlinear optics crystallized in the mid-1960s, it must be situated in the institutions created to support the many-pronged U.S. Cold War effort, and the unique social world such institutions nurtured. As much as the new field was the child of expensive gadgets, special materials, and big government grants, U.S. nonlinear optics was a product of the not-for-profit corporation and elite contract expertise. The Institute for Defense Analyses, as a private adjunct to the Defense Department (including ARPA), was a significant channel through which academic expertise made contact with the defense-industrial community.

By the late 1950s, after years of attempts by scientists and the government alike to bring the brightest minds in touch with the most urgent problems, not-for-profit corporations like IDA presented a middle course—an American solution—by which university researchers would hold on to campus careers while the government benefited from their specialized and (it was claimed) disinterested knowledge. Whether or not the arrangement succeeded in preserving either academic freedom or fortifying national security, it did serve to reinforce and expand a community of highly placed experts.³¹

²⁹ Bromberg, *The Laser in America*, 82. Gould was denied clearance to work on TRG's ARPA-funded laser project because of his ties to a Marxist study group during the Second World War. He remained at TRG, and continued work on unclassified elements of the laser study, in any case.

³⁰ Bromberg, *The Laser in America*, 102.

³¹ I use the term "not-for-profit" rather than "nonprofit" to indicate that such organizations did seek lucrative contracts, but not from a profit-making motive. In this usage I follow Bruce L.R. Smith, "The future of the not-for-profit corporations," Rand Paper P-3366 (May 1966).

The not-for-profit corporation dedicated to defense consulting was a new and peculiar Cold War species, “an organization that serves the government without being a part of it,” in the words of one report on IDA activities from the 1960s.³² Like the more famous RAND Corporation, IDA’s goal was to match the best technical expertise with the most challenging defense problems; like RAND, it serviced specific defense constituencies with research reports and analysis. According to the Bell Report, an extensive study of government contracting undertaken by a committee chaired by Director of the Bureau of the Budget David E. Bell in 1962, “the principal advantages [that not-for-profit corporations] have to offer are the detached quality and objectivity of their work.”³³ But the Bell Report also issued a sharp warning about the dangerous prospect of the government delegating too many of its decision-making responsibilities to private contractors.³⁴

Similar in many ways, IDA and RAND were also different animals. IDA was more rooted in the tradition of operations research, a defense management and planning technique born of the Second World War. Operations research started with the resources and equipment on hand and proceeded, from information about the hardware’s technical specifications, to craft policies and plans for their use. The RAND experts, on the other hand, were noted for their more adventurous strategizing; in heady pursuit of a “science of war,” they flipped operations research on its head and called it “systems analysis.” RAND’s analysts *started* with strategy—confined by

³² Institute for Defense Analyses, *Activities of the Institute for Defense Analyses, 1961–1964* (Washington, DC, 1964), 10.

³³ The Bell Report quoted in Richard M. Bissell, Jr. to Director of Defense Research & Engineering, “The Relationship of the Institute for Defense Analyses to the Department of Defense,” 11 June 1962, Accession 22019, Box 6 of 42, Folder “Institute for Defense Analyses,” CHT.

³⁴ United States Bureau of the Budget, *Government Contracting for Research and Development* (Washington, DC: Government Printing Office, 1962).

the limits of cost-effectiveness, imagination and logic—and then worked out the technological requirements to put strategy into action.³⁵

IDA had nothing like the sunny, ocean-side campus or the freewheeling intellectual atmosphere that RAND became legendary for. In 1961 the physicist Edward Gerjuoy turned down IDA's offer of a staff scientist position because he worried that the Institute was too compartmentalized and too narrow, wanting for a broad "research division...something like the Theoretical Division of Los Alamos which provides advice and guidance to all of the Los Alamos divisions on research problems."³⁶ An internal report on IDA's interview procedures listed among numerous "disadvantages of working for IDA" its subpar office facilities, its lack of a clear organizational structure within divisions and the "relatively impermeable membrane between divisions," insufficient support staff, and limited library facilities.³⁷ In its early years IDA's Washington, DC-area offices were distributed among four separate addresses (two on H Street, one on Connecticut Ave., and one in the Pentagon); only in 1964 was IDA's entire staff consolidated into a single unit in Arlington, Virginia. Both RAND and IDA benefited from a post-Sputnik burst of funding, but IDA's contract volume was less than half of RAND's in the early 1960s (by 1962 it was pulling in \$20.5 million compared to IDA's \$7.4 million). And whereas RAND had 1100 employees in 1962, IDA's payroll was smaller (but by no means

³⁵ On RAND's "science of war" see David R. Jardini, "Out of the Blue Yonder: The RAND Corporation's Diversification into Social Welfare Research, 1946–1968" (PhD dissertation, Carnegie Mellon University, 1996), esp. Chapter 2, "Early RAND and the Emergence of Systems Analysis," 28–113; Fred Kaplan, *The Wizards of Armageddon* (Stanford, CA: Stanford University Press, 1991 [1983]), esp. 52–57 and 86–88; and Sharon Ghamari-Tabrizi, *The Worlds of Herman Kahn: The Intuitive Science of Thermonuclear War* (Cambridge, MA: Harvard University Press, 2005).

³⁶ Keith Brueckner to Charles Townes, 19 June 1961, Box 23, Folder 12 "Institute for Defense Analyses—Correspondence, 1961," *KAB*.

³⁷ "Memorandum for William E. Bradley and John F. Kincaid, Subject: Report of ad hoc panel on interview procedures," 31 May 1961, in Box 23, Folder 12 "Institute for Defense Analyses—Correspondence, 1961," *KAB*. The report referred specifically to the Research and Engineering Support Division (RESA) of IDA. To be fair, the same document listed several "advantages of working for IDA/RESA" alongside the disadvantages mentioned here, including "significant opportunity for creative work," excellent salaries and fringe benefits, IDA's small size, and the "opportunity to be fully informed on critical national problems and to participate in their resolution."

insignificant), growing from 221 professional and 159 support staff in 1962 to 285 professional and 239 support staff by 1964.³⁸

IDA had started out in 1956 as a consulting body for the Weapons Systems Evaluation Group (WSEG), an operations research branch of the Defense Department created in 1949. By 1958 IDA's board of trustees had set up an Advanced Research Projects Division specifically devoted to ARPA tasks. In 1960 this group was renamed the Research Engineering and Support Division, and began carrying out work for each of the various offices beneath the Office of the Director of Defense Research and Engineering (ODDRE), ARPA among them.³⁹

The story of how IDA got its Jason Division at the end of 1959—the crème-de-la-crème academic wing of IDA, staffed mostly by theoretical physicists (and more than a few Nobel Prize-winners), collectively dubbed “the Jasons”—has been well told elsewhere.⁴⁰ On one hand the creation of Jason was a story of political failure—the inability of the physicist John Wheeler and his collaborators to realize their grand vision of a federally sponsored, academically-staffed “National Security Research Laboratory” where basic science research and the solution of defense problems would feed productively from each other.⁴¹ But more important, Jason's placement within IDA reflected a growing preference (among both researchers and patrons) for contractual and entrepreneurial—rather than centralized—arrangements for defense research. Jason allowed an elite group of scientists to spend part of the workweek consulting on military problems, making an occasional trip to IDA's Washington offices for a meeting or briefing.

³⁸ Jardini, “Out of the Blue Yonder,” 115; Institute for Defense Analyses, *Activities of the Institute for Defense Analyses, 1961–1964*, 31.

³⁹ U.S. Congress, Office of Technology Assessment, *A History of the Department of Defense Federally Funded Research and Development Centers*, OTA–BP–ISS–157 (Washington, DC: U.S. Government Printing Office, 1995), 27.

⁴⁰ Finn Aaserud, “Sputnik and the ‘Princeton Three’: The national security laboratory that was not to be,” *Historical Studies in the Physical and Biological Sciences* 25, no. 2 (1995): 185–239; Anne Finkbeiner, *The Jasons: The Secret History of Science's Postwar Elite* (New York: Viking, 2006).

⁴¹ Aaserud, “Sputnik and the ‘Princeton Three’.” The “National Security Research Laboratory” in particular, one of many iterations in the proposed title for Wheeler's project, is mentioned on 208.

Much of the part-time consultants' work was accomplished during a yearly six-week summer study. Always held in pleasant surroundings (usually on Cape Cod or in La Jolla, California), the event combined an intensive Cold War defense study with a family vacation at the seashore.

One scientist who ferried back and forth between the academy and IDA with enormous success was the quantum electronics pioneer Charles Townes. Townes had first gotten involved with military consulting in 1957, working on an Air Force summer study at Woods Hole, Massachusetts. Townes was among the first to recommend to the Air Force, months before Sputnik, that it invest heavily in space and satellite technology.⁴² In 1959 Garrison Norton, then president of IDA, made an unannounced visit to Townes's Columbia University office and pleaded with him to join IDA as its Vice President and Director of Research. Townes mulled over the offer. Unlike some others, Townes actually thought of IDA as having an attractively academic feel. "It was primarily run by universities and university people. I wouldn't have done it, I think, otherwise, if it had been a government organization or an industrial organization, because I would have felt there was not...enough openness and freedom and support for unpopular opinions if necessary." He also regarded IDA as uniquely influential. "IDA at that time was in an unusually powerful position.... It was sort of the chief advisor to the government in national defense issues, and some broader issues connected with national defense such as arms control.... We had to make a lot of decisions and the country was in trouble, and so I felt really a

⁴² And befitting someone on the cusp of inventing the laser principle, Townes pressed the Air Force to fund research on pushing the maser to shorter wavelengths. Interview of Charles H. Townes by Finn Aaserud, 20–21 May 1987, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD, USA, <http://www.aip.org/history/ohilist/4918.html> (cited hereafter as Townes interview, 1987). The Air Force study Townes had consulted for was a so-called "second von Kármán study," after the original 1945 air power technology forecast led by the Hungarian émigré physicist Theodore von Kármán in 1945. See William Leavitt, "Toward new horizons: Theodore von Kármán, in memoriam," *Air Force Magazine* 46, no. 6 (June 1963).

pretty strong sense of duty that somebody ought to be doing this. I had been tapped and asked to do it, and I was reluctant to turn it down.” So he took the job.⁴³

Even the service-minded Townes clung jealously to his personal research career in the midst of full-time defense management. He accepted the position at IDA on the condition that IDA pay for his weekly transportation between Washington and New York City, allowing him to spend Saturdays advising his graduate students and tending to his lab at Columbia University.⁴⁴ Townes, who helped set up an institutional home for the Jasons within IDA, also forged special publishing arrangements between Jason and IDA that would permit the group’s members to publish the results of their research in the usual public professional venues—even when the research in question had originated in classified contexts. In April 1960 he wrote to Brigadier General Austin W. Betts, the new Director of ARPA, suggesting that “there is one matter covered by Contract SD-50 between the Institute for Defense Analyses and [ARPA]”—the contract that had established the formal consulting relationship between IDA and ARPA—“which may prove inapplicable to most of the individuals who form the [Jason Division].” The original agreement required that the ARPA Director personally approve the public release of any findings obtained in the course of fulfilling the contract. Townes ceded that this stipulation rightly applied to full-time IDA staff who wanted to keep in touch with their fields and their universities, as Townes himself had as IDA’s Director of Research. But “members of the Jason Group do not fall into this category,” he argued: “They are employees of universities who conduct basic (and largely theoretical) research in the sciences independently of, as well as for, the Jason Project. It would place an unusual and difficult restriction on their activities... to

⁴³ Townes interview, 1987.

⁴⁴ Interview of Dr. Charles Townes by Joan Bromberg on 28 January 1984, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD, USA, http://www.aip.org/history/ohilist/4917_1.html (cited hereafter as Townes interview, 1984).

require that the [Jason-related] basic research of an unclassified nature...be cleared before it is published in scientific and technical journals...”⁴⁵

Townes asked Betts for an exemption from the offending clause—and he got it. Sharing the happy news with the Jasons days later, he cautioned them to “minimize the possibility of mistakenly revealing classified material” by sending the Jason management “a copy of any unclassified paper you write which stems from Jason work,” just to double-check. And he informed the Jasons that they were not obligated to acknowledge IDA’s support in published papers. In fact IDA “in many cases does not desire it, because of the military emphasis implied by Institute interest.” No need to muddy the clear waters of pure science by suggesting a connection to national security consulting.⁴⁶

The IDA Laser Advisory Committee

Townes and IDA enlisted the Jasons in missile defense work for ARPA right out of the gate. At its very first meeting at IDA offices in Washington, DC in February 1960, the Jasons heard briefings on “missiles and their comparison with aircraft” and “defense against ballistic missiles” from representatives of ODDRE. (The group also listened to Hans Bethe give a briefing on “detection of nuclear explosions” and RAND’s Herman Kahn on “hardening and dispersal of military and civilian targets.”)⁴⁷ The Jason physicists Sidney Drell, Sam Treiman, Donald Glaser, and Malvin Ruderman presented a review briefing on ARPA’s GLIPAR program.⁴⁸ At the end of the first Jason summer study at the Cape Cod town of Woods Hole later

⁴⁵ Charles H. Townes to Austin W. Betts, 7 April 1960, Box 35, Folder 1 “JASON Project 1960,” *MGM*.

⁴⁶ Charles H. Townes, Memorandum for Members of the Jason Group, 25 April 1960, Box 35, Folder 1 “JASON Project 1960,” *MGM*.

⁴⁷ Charles Townes to Keith Brueckner, 19 January 1960, Box 23, Folder 10 “Institute for Defense Analyses – Correspondence, 1960 January – July,” *KAB*.

⁴⁸ “IDA: JASON,” 5 February 1960, Box 23, Folder 10 “Institute for Defense Analyses—Correspondence, 1960 January – July,” *KAB*.

that year, over the course of two weeks in September 1960, the group undertook a comprehensive review of the U.S. ballistic missile defense program, summarizing the state of the art from various key documents made available by IDA management in the makeshift summer study library (including a copy of an “ARPA BMD [ballistic missile defense] Technology Program Review” from 1959, the first volume of the “GLIPAR Summary Report,” and the General Dynamics Convair division’s two-volume “Ballistic Missile Defense Manual”).⁴⁹

By 1961 Project Defender—ARPA’s exploratory ballistic missile defense program—had become a massive effort with mixed results. It cost ARPA \$128.5 million in the first year; its appropriation maintained at least that level through most of the following decade, soaking up more than half of ARPA’s entire yearly budget.⁵⁰ But skepticism prevailed, even among the leadership. Jack Ruina, an electrical engineer on leave from the University of Illinois, assumed the directorship of ARPA in 1961. In February of that year, only a couple of weeks after taking the helm of ARPA, in testimony reported by the *New York Times* Ruina expressed to the House Science and Astronautical Committee a “great deal of pessimism about ever developing a complete and adequate umbrella against ICBM attack”—and this despite the fact that Ruina’s own agency was shuffling around some \$170 million dollars in the coming year for its overall missile defense effort. Herbert York, who as Director of Defense Research and Engineering had tapped Ruina for the ARPA job, was equally skeptical about the prospects for the Army’s Nike Zeus missile defense system, which he suggested would be incapable of defending large geographical areas of the U.S.⁵¹ For these technocrats at the top of the defense research hierarchy, ARPA and its Project Defender were mainly valuable as a buffer against the military

⁴⁹ H.N. Beveridge to Keith Brueckner, 17 August 1960, Box 23, Folder 11 “Institute for Defense Analyses – Correspondence, 1960 August–December,” *KAB*; H.N. Beveridge to Murray Gell-Mann, 18 August 1960, Box 35, Folder 1 “JASON Project 1960,” *MGM*.

⁵⁰ Barber Associates, *Advanced Research Projects Agency*, IV–21.

⁵¹ “U.S. expert doubts full ICBM defense,” *New York Times* (17 February 1961): 9.

services' more outlandish plans and propagandizing. Years later Ruina would recall that "all the other programs—Air Force or the Army—would have done the whole thing a tremendous injustice. The Air Force was still in its Buck Rogers period, and the stuff they were pushing in ballistic missile defense was absolute garbage."⁵²

In 1961 officials at ARPA asked Charles Townes and IDA to formally assess the feasibility of the various radiation weapon proposals swirling around under Project Defender, grasping for some balance between optimism and prudence. "If the project cannot be justified on a weapon or weapon technology basis," Albert Weinstein (of ODDRE) wrote to IDA, he wondered if Defender might still usefully serve "as a means of supporting basic research, i.e. high caliber research...which contributes importantly to the total fund of knowledge in a specialized area."⁵³

Keith Brueckner, who had helped Townes put together the Jason Division in 1959 and remained on its Steering Committee, became IDA's Vice President and Director of Research when Townes's tenure expired in the summer of 1961.⁵⁴ Brueckner was a jack-of-all-trades physicist. Trained as a nuclear theorist and an early adopter of the Feynman diagram technique in many-body physics, he would make important contributions to multiple fields from the theory of the solid-state to laser-initiated thermonuclear fusion.⁵⁵ He was also a defense consultant extraordinaire, bouncing restlessly between a staggering array of nuclear-age military problems in the 1950s and 60s. He juggled an academic appointment at the new La Jolla campus of the

⁵² Richard J. Barber Associates, *The Advanced Research Projects Agency*, V-23.

⁵³ Weinstein quoted in Robert W. Seidel, "How the military responded to the laser," *Physics Today* (October 1988): 36-43, on 41.

⁵⁴ Brueckner to Marvin L. Goldberger, 4 January 1960, Box 23, Folder 10 "Institute for Defense Analyses—Correspondence, 1960 January–July," *KAB*. Brueckner had proposed calling the group "Project Logos" but the name "Jason" was preferred by the first chairman of the Jason steering committee, Princeton physicist Marvin Goldberger.

⁵⁵ On Brueckner's use of Feynman diagrams and his early work in many-body physics, see David Kaiser, *Drawing Theories Apart: The Dispersion of Feynman Diagrams in Postwar Physics* (Chicago: University of Chicago Press, 2005), 98-99 and 238-242.

University of California with consulting gigs in multiple Defense Department bureaus and a handful of industrial firms. In a letter for Jason management from May of 1960 outlining his industrial consulting obligations alone, Brueckner listed work on “atomic physics and radiation opacities” for RAND; “the atomic physics connected with high altitude nuclear explosions” for Lockheed Aircraft Corporation; “the high altitude effects of nuclear weapons” for General Electric’s Project TEMPO; “high altitude nuclear explosions, RF [radio frequency] plasma interaction, anti-ICBM defense” for the Convair division of General Dynamics; and “theoretical solid state physics” for Bell Laboratories.⁵⁶ He was intense, tough, and allergic to work he considered substandard.⁵⁷

Now installed at IDA, Brueckner responded to Weinstein’s request for IDA’s advice on laser weapons by setting up a special Laser Advisory Committee of experts in solid-state physics and optics. The group would be charged with evaluating the feasibility of high-power lasers for ballistic missile defense and other military applications. The first meeting of the committee—which included Townes, Nicolaas Bloembergen of Harvard, Norman Kroll of Columbia

⁵⁶ IDA required such statements to forestall potential conflicts of interest with private contractors. Brueckner to Marvin L. Goldberger, 20 May 1960, Box 23, Folder 10 “Institute for Defense Analyses—Correspondence, 1960 January–July,” *KAB*. On IDA’s worries about possible conflicts of interest, and special concerns associated with the Jason Division (which had been accorded an “unusual and immensely broad need-to-know certification” by the Director of Defense Research and Engineering), see Charles Townes, Memorandum for the Jason Group (Subject: Outside Interests, II), 5 April 1961, Box 23, Folder 12 “Institute for Defense Analyses—Correspondence, 1961,” *KAB*.

⁵⁷ Examples of Brueckner’s forthrightness (and occasional abrasiveness) abound in his correspondence. Here, for example, is Brueckner writing to the IDA’s S.S. Penner, evaluating the work of a younger IDA scientist in 1963 (I have removed the younger scientist’s name): “I have thought a little more about the problem of the shock overpressure from nuclear explosions at high altitude. As you know, [name removed] claims to have a theory which predicts correctly the observed experimental results. Although it may turn out that he has correctly found an empirical relationship I think that his theory is without content. I am, therefore, disturbed to find both him and [name removed] defending this as gospel.... My own view is that he has put himself in an indefensible position by his somewhat irrational approach to this problem. You perhaps also know that I recommended some time ago before you became Division Director, that he be fired.” Brueckner to S.S. Penner, 8 October 1963, Box 24, Folder 4 “Institute for Defense Analyses—Correspondence, 1963 September–December,” *KAB*. Even a physicist of Nicolaas Bloembergen’s stature could feel the sting of Brueckner’s judgment: “Keith Brueckner thinks not too much about all my other work, but what he remembers of me, is that I suggested that the military required a million joules of light as a ballistic missile system.” Interview of Nicolaas Bloembergen by Joan Bromberg and Paul L. Kelley, 27 June 1983, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD, USA, <http://www.aip.org/history/ohilist/4511.html> (cited hereafter as Bloembergen interview, 1983).

University, Robert Kingston of the MIT Lincoln Laboratory, Anthony Siegman of Stanford University, Max Weiss of Aerospace Corporation, and Stirling Colgate of the Lawrence Livermore National Laboratory—took place in December of 1961.⁵⁸

At the meeting Bloembergen offered, as a rough estimate, that a workable laser-based antimissile system would have to deliver on the order of one million joules of energy to a ballistic missile in order to destroy it. Bloembergen carried out a quick calculation showing that a cubic meter of neodymium-glass, pumped to such energetic extremes that it would risk melting, might provide a laser pulse of sufficient power. Norman Kroll remembered years later that Bloembergen's calculation was the first demonstration "that the energy scales were such in laser devices that you could imagine producing enormous output powers." The experts soon dubbed the concept the "Bloembergen Cube."⁵⁹

In short order William Culver, a physicist brought to IDA from RAND by Charles Townes to direct IDA's high-power laser work, reported back to ARPA. "Current Optical Maser developments have led a number of people in government and industry to believe that it may be possible to generate and direct enough coherent optical power to make a useful radiation weapon," he wrote. Ten different types of lasers had been constructed since 1960. Ruby lasers were capable of emitting pulses of 10 megawatts peak power, each containing 50 joules of energy. The only limit to the power of the laser seemed to be the volume of the gain (or "lasing") material. And based on Bloembergen's suggestion, neodymium-glass lasers, invented earlier that year at American Optical Company, had roughly fifty times more lasing ions per unit volume

⁵⁸ "Report of the IDA Committee on Optical Maser Radiation Weapons," TN 62-3 on Contract SD50, vol. 1 (January 1962), AD 346 862, cited in Robert W. Seidel, "From glow to flow," 120.

⁵⁹ Bloembergen interview, 1983; Interview of Dr. Norman Kroll by Joan Bromberg, 29 March 1987, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD, <http://www.aip.org/history/ohilist/31407.html>.

than pink ruby (the material Theodore Maiman had used to build the first laser), and could likely provide proportionately more peak power.⁶⁰

Enthusiastic about Bloembergen's suggestion, the group recommended that ARPA set up a special high-power laser program, soon labeled Project Seaside. Seaside's official goal was to build a scale-model high-power laser producing between 1,000 and 10,000 joules of energy per pulse. Responsibility for the project was shared among ODDRE, ARPA, IDA, and the Office of Naval Research (ONR). ARPA and ONR soon began dispensing Seaside-affiliated contracts to industrial and university researchers. Project Seaside's mission was ambitious to say the least, but when it came to the laser, technological optimism was in ample supply.⁶¹

Early Nonlinear Optics: The Laser and Optical Harmonic Generation

Nonlinear optics is the science of the interactions between matter and very high intensity light. When super-intense light—light of intensities only achievable with lasers—passes through matter, the electric fields carried by the light can alter the atomic energy levels, the electronic configuration, and even the physical structure of the matter it travels through. The coupling between powerful light and the propagation medium (the atoms or molecules of a gas or liquid, or the crystal structure of a solid) produces conditions in the material that act *back* on the light itself, altering its wavelength, phase, and other propagation characteristics. If the light in question is monochromatic and coherent (as laser light is), nonlinear optical effects can be studied and quantified with great precision.

⁶⁰ Townes interview, 1984; William Culver to Albert Weinstein, "Optical maser radiation weapons," 15 December 1961, quoted in Seidel, "From glow to flow," 120. See also F.J. McClung and R.W. Hellwarth, "Giant optical pulsations from ruby," *Applied Optics* 1, no. S1 (1962): 103–105.

⁶¹ William Culver to Albert Weinstein, "Optical maser radiation weapons," 15 December 1961, cited in Seidel, "How the military responded to the laser," 41.

Nonlinear optics took shape in the early and mid-1960s alongside, and in direct contact with, military-sponsored research on high-power lasers for ballistic missile defense. Connections between ballistic missile defense and the formation of nonlinear optics, however, were not uncomplicated or unidirectional. They varied over time and according to several factors, including access to lasers, available funding sources, and personal and intellectual connections through consulting activities of the kind that Charles Townes and Nicolaas Bloembergen were getting more deeply involved in. The earliest theoretical and experimental developments in nonlinear optics, involving the demonstration and explanation of so-called optical harmonics, arguably bore a less immediate relationship to specific military goals. But subsequent developments in the new field—especially researchers' turn to stimulated scattering phenomena by the mid-1960s—were directly indebted to the elite defense consultants' interest in the operation of high-power solid-state lasers, and the propagation of high-power beams through the atmosphere.

As the University of Michigan physicist Peter Franken sat in a meeting of the Optical Society of America in Pittsburgh in early 1961, he made a back-of-the-envelope calculation of how much energy could be delivered to a solid by a laser beam. For a five-kilowatt laser pulse confined to a spot of ten micrometers diameter, Franken found a power density on the order of megawatts per square centimeter, and an electric field density of roughly 100,000 volts per centimeter. In other words, the laser would create electric fields within the crystal structure that were just a few orders of magnitude smaller than the electric fields within the atoms themselves. There was little question in Franken's mind that the intense laser light would distort the electronic structure of the crystal atoms around it—the fields created by the intense light would

stretch and pull the very stuff through which it traveled, modifying the optical properties of the propagation medium.⁶²

Though Franken was without a laser, he knew how to get one. One of Franken's former students, Lloyd Cross, had just founded a company called Trion Instruments in Ann Arbor, virtually on the doorstep of Franken's University of Michigan home. Trion was among the very first commercial manufacturers of the ruby laser after Hughes Aircraft. Though lacking the immediate funds to purchase a laser, Franken worked his personal connection to Cross and was able to rent one of Trion's early commercial models. For a target material, Franken and his colleague Gabriel Weinreich obtained samples of crystalline quartz left over from the University's wartime research on piezoelectric materials. They arranged a small spectrograph and a camera to record the output light, and proceeded to fire three-joule, one-millisecond pulses at the quartz, each containing roughly 10^{19} photons.⁶³

In a phenomenon known as "second harmonic generation"—long familiar to radio and microwave engineers at longer electromagnetic wavelengths—when a beam of radiation passes through certain kinds of crystals, a fraction of the beam is shifted to *twice* the original frequency.⁶⁴ In other words, a component of the second harmonic of the incident beam is excited through the beam's interaction with the crystal. Under certain conditions, potentially all of the input beam can undergo conversion to the second harmonic frequency. In the case of the ruby

⁶² Interview of Dr. Peter Franken by Joan Bromberg, 8 March 1985, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD, USA, <http://www.aip.org/history/ohilist/4612.html> (hereafter cited as Franken interview, 1985).

⁶³ Franken interview, 1985; P.A. Franken, A.E. Hill, C.W. Peters, and G. Weinreich, "Generation of optical harmonics," *Physical Review Letters* 7 (1961): 118–119.

⁶⁴ Second-harmonic generation of the kind demonstrated in Franken's experiment could only be performed with non-centrosymmetric crystals—that is, crystal lattices lacking symmetry about the center of the "unit cell." Researchers at Bell Laboratories would soon demonstrate second-harmonic generation in symmetric crystals as well, which they explained as a consequence of the absorption of photons by individual atoms in the crystal. In that case, for every two photons of the incident beam absorbed by a crystal atom, one would be re-emitted, only at twice the frequency (to conserve energy). Researchers called this process "two-photon excitation." See W. Kaiser and C.G.B. Garrett, "Two-photon excitation in $\text{CaF}_2:\text{Eu}^{2+}$," *Physical Review Letters* 229 (1961): 229–231.

laser, whose natural output wavelength is in the red portion of the visible spectrum, when its beam is passed through a nonlinear crystal it is converted to blue light (see Fig. 2.1).

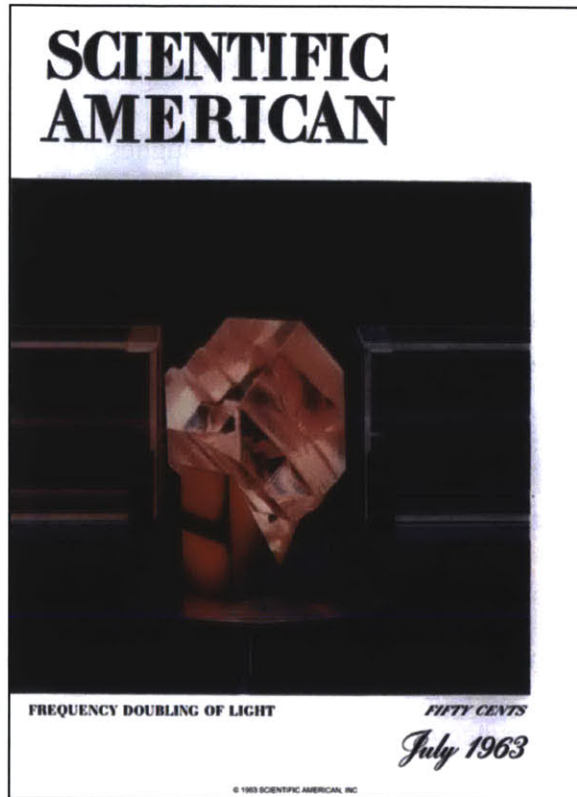


Figure 2.1: The cover of the July 1963 edition of *Scientific American* shows optical second harmonic generation in a crystal of ammonium dihydrogen phosphate, produced in the laboratory of Robert Terhune at Ford Motor Company. Taken from *Scientific American* 209, no. 1 (July 1963).

Sure enough, as Franken had suspected, a weak second-harmonic beam was detected at the camera. Though the second-harmonic signal was just barely measurable (containing roughly one billion times fewer photons than in the original pulse), Franken published his groundbreaking result in *Physical Review Letters*.⁶⁵

⁶⁵ Franken, et al., "Generation of optical harmonics."

Nicolaas Bloembergen and the Theory of Nonlinear Optics

At Harvard University's Cruft Laboratory, Nicolaas Bloembergen read a pre-print of Franken's paper, and was fascinated. In 1961 Bloembergen had already become a major player in the world of solid-state physics and optics. He had studied at the University of Utrecht, obtaining a master's degree in 1943, when the occupying German army shut down the university. He taught himself quantum mechanics during 1943–44, reading Hendrik Kramers's *Quantentheorie des Elektrons und der Strahlung* ("Quantum theory of electrons and radiation")—even by the light of an oil lamp during the Dutch "hunger winter" of 1944, when the electricity was cut off and food was scarce. He went to Harvard in 1946, finished doctoral work under Robert Pound and Edward Purcell on nuclear magnetic resonance (Bloembergen did much of his research at Harvard, though his degree was awarded formally by the University of Leiden). After spending two years at the Harvard Society of Fellows beginning in 1949, he was hired as a Professor of Applied Physics at Harvard.⁶⁶ In 1956 Bloembergen came up with a novel pumping scheme for the maser using a solid-state gain medium instead of a gas (Charles Townes's original maser had used an ammonia gas). This innovation—the use of a solid-state gain medium with a pumping scheme comprised of three separate energy levels rather than two (as with ammonia)—brought researchers a huge step closer to the invention of the first laser, which used a three-level scheme and a solid crystal as the gain medium. It also made Bloembergen a marquee name in the young and exciting field of quantum electronics.⁶⁷

Bloembergen had anticipated diving into laser research as early as 1960, soon after the laser's invention. He put one of his former graduate students (recently hired into a junior faculty

⁶⁶ Bloembergen interview, 1983; Nobelprize.org, "Nicolaas Bloembergen – Biographical," http://www.nobelprize.org/nobel_prizes/physics/laureates/1981/bloembergen-facts.html.

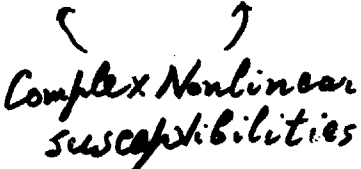
⁶⁷ Bromberg, *The Laser in America*, 34–38; William L. Laurence, "Science in review: Discoveries on the frontiers of physics are explained at a Harvard conference," *New York Times* (30 March 1958): E9.

position at Harvard), Peter Pershan, in charge of building a ruby laser for their own laboratory according to Maiman's original design. But it soon proved difficult to reconstruct Maiman's laser model. "I asked Peter Pershan to build us a duplicate ruby laser, just copying what Maiman did, and we had a hell of a time, you know," Bloembergen later remembered. "If you have no optical experience, it just isn't that easy, in the first year."⁶⁸ In the meantime, lacking a working laser, Bloembergen's group dove into a general theoretical formalism for nonlinear light-matter interactions. Bloembergen hoped to explain harmonic generation microscopically, by calculating the nonlinear polarization—that is, the response of the material to the electric fields introduced by the intense light—in quantum-mechanical perturbation theory.

There were two ways to think about the effect of high-energy light on a nonlinear crystal. In the semi-classical picture, an electric field applied to a crystal induces a new electric field—a response called the "polarization"—within the crystal. In ordinary classical optics, the polarization is proportional to (in other words, linear in) the electric field. That is, $P = \chi E$, where P is the polarization, E is the applied (i.e., laser) electric field, and χ is a quantity called the "susceptibility," which depends on the material properties of the crystal. In nonlinear optics, the intensity of the incident field actually alters the properties of the crystal sufficiently that the equation for P is generalized to a nonlinear expansion in powers of E . The proportionality constants are called "nonlinear susceptibilities." In Bloembergen's own handwriting:

⁶⁸ Bloembergen interview, 1983. The reconstruction of a working laser figured in Harry Collins's classic argument for the role of tacit knowledge and close social contact in the transmission of science between isolated communities. His original case involved a TEA gas laser, rather than the original ruby design. See Harry Collins, "The TEA set: Tacit knowledge and scientific networks," *Science Studies* 4, no. 2 (1974): 165–185.

$$P_i = \chi_{ij} E_j + \chi_{ijk}^{(2)} E_j E_k + \chi_{ijkl}^{(3)} E_j E_k E_l + \dots$$



 Complex Nonlinear
 susceptibilities

Figure 2.2: Nicolaas Bloembergen’s rendering of the basic equation of nonlinear optics. P is the polarization, χ the susceptibility, and E the electric field components of the laser light. Taken from lecture slides, Nicolaas Bloembergen, “The birth of nonlinear optics,” paper delivered at the conference Nonlinear Optics: Materials, Fundamentals and Applications, Kauai, Hawaii, 17–22 July 2011.

If the applied field is a radiation field (i.e., if E is oscillating at a particular frequency) then the polarization will also oscillate—that is, the crystal will radiate as indicated by its oscillating polarization. In the case of a linear crystal, P oscillates at the same frequency as E : the crystal radiates light of the same wavelength as the laser. But if the laser light is strong enough to induce a nonlinear response from the crystal—if the nonlinear equation for P applies—then the crystal radiates at all the frequencies represented in the formula for P , which happen to be integer multiples of the original frequency. The second term in the equation above, $\chi_{ijk}^{(2)} E_j E_k$, represents second harmonic generation—radiation at twice the frequency of the incident light.⁶⁹

The second picture of harmonic generation is informed by quantum mechanics. Nonlinear optics was boiled down to microscopic processes at the atomic scale. Harmonic generation, in particular, was a process in which individual atoms within the crystal absorb multiple photons and in turn coherently (i.e., in phase with other crystal atoms) emit fewer numbers of photons. So

⁶⁹ Bloembergen here uses the “Einstein summation convention,” a shorthand notation for sums. The j and k are summing indices, denoting the three dimensions of space, and running from 1 to 3. In each term, the repeated indices are implicitly summed over, so that the equation describes the i^{th} component of the polarization P . In more conventional “sigma notation,” for example, the first term on the right-hand-side of the equation would be $\sum_{j=1}^3 \chi_{ij} E_j$.

in second harmonic generation, for example, an individual crystal atom absorbs two photons of the incident laser light, emitting only one. Because energy is conserved in the process, and the energy of each photon is proportional to its frequency, the emitted photon has *twice* the frequency of the two that were originally absorbed: red light becomes blue. In principle, multi-photon absorption events of all kinds are possible—two, three, four, and on up the ladder—but each additional number of absorbed photons (corresponding to a higher order in perturbation theory) becomes less and less likely. Linear optics applies in ordinary circumstances, involving the scattering of single photons by crystal atoms; one needs a very powerful source of light to see nonlinear effects.⁷⁰

Bloembergen and company made the first theoretical connection between these two pictures, the semi-classical and the quantum, by showing how the semi-classical result could be built up from shopworn techniques in quantum mechanics. They also aimed their calculations at more general nonlinear optical processes involving the “frequency mixing” of multiple beams. With Pershan, post-doctoral associate John Armstrong, and graduate student Jacques Ducuing, Bloembergen worked out a general theory describing harmonic generation of light in nonlinear materials, including higher (third and fourth) harmonics, as well as the generation of coherent laser light at pre-selected wavelengths by the addition or subtraction of the frequencies of two input beams which interact inside a nonlinear crystal. Between July 1961 and early 1962, during Bloembergen’s sabbatical semester that fall, Bloembergen met with his group each day at Harvard for several hours, going over the developing results as the research hit top speed.⁷¹

⁷⁰ In particular, the ratio of strengths between adjacent orders of scattering processes is the square of the ratio between the electric field of the incident light and the average atomic electric field felt by valence electrons in the crystal. See J.A. Armstrong, N. Bloembergen, J. Ducuing, and P.S. Pershan, “Interactions between light waves in a nonlinear dielectric,” *Physical Review* 127 (1962): 1918–1939, on 1918.

⁷¹ Bloembergen interview, 1983.

The laborious calculations were divided among the group members to exploit their individual skills. Armstrong, who knew the most about perturbation theory, calculated the material response to the intense laser light quantum-mechanically. Ducuing, who had studied at the École Polytechnique in Paris and was no stranger to dense mathematics, tackled all of the nonlinear differential equations. Pershan handled the tricky calculation of the “local” electric field produced by the polarization of the medium in response to the intense laser light. And Bloembergen calculated the mixing of light waves as a result of that response. By April 1962 Bloembergen and company had hammered out a 22-page manuscript and shipped it off to the *Physical Review*, where it was published that September.⁷² It would become a landmark paper, one of the founding documents of the field.⁷³

Nonlinear optics in the early 1960s was a relatively small-scale affair, involving tabletop experiments that could be carried out by small groups of researchers in modestly sized laboratories. But because the laser was still such a novel instrument, built with a tremendous input of federal capital, nonlinear optics was also an expensive enterprise. For researchers outside of major industrial laboratories who were interested in laser applications, government support was a *sine qua non*. Bloembergen’s group was no exception. Their work in nonlinear optics was funded for the first year or so with a longstanding contract Bloembergen had through JSEP, dating back to the early 1950s. The group’s experimental program started slowly when

⁷² J.A. Armstrong, N. Bloembergen, J. Ducuing, and P.S. Pershan, “Interactions between light waves in a nonlinear dielectric,” *Physical Review* 127 (1962): 1918–1939. Also see the Bloembergen interview, 1983, for a description of the division of labor in writing this paper.

⁷³ Jeff Hecht reports Robert Boyd of the University of Rochester’s Institute of Optics as recently saying, “To this day, every time I make a discovery in nonlinear optics, I look at [Bloembergen’s] paper and he’s done it. He put the whole field together in 18 months.” Hecht, “How the laser launched nonlinear optics,” *Optics and Photonics News* (November 2010): 34–40, on 40. To date the paper has garnered more than 2700 citations in the scientific literature, according to the *Science Citation Index* (Philadelphia: Thomson ISI, 1961 –). Roughly 400 citations have come from within the *Physical Review*. Data from the Physical Review Online Archive, <http://prola.aps.org/> (accessed 22 September 2013). This citation record compares favorably with some of the most-cited articles in the *Physical Review*’s history. See Sidney Redner, “Citation statistics from 110 years of *Physical Review*,” *Physics Today* 58 (June 2005): 49–54.

Bloembergen, eager to begin testing some of his group's theoretical calculations, purchased one of Trion's early ruby laser models in 1962.⁷⁴ By 1963 Bloembergen's research was also funded through unclassified contracts with both the Office of Naval Research and ARPA. That year Bloembergen used his Project Defender grant from ARPA and the Office of Naval Research to spend roughly \$12,500 to purchase a Maser Optics model 3020 laser with two ruby crystals, reporting to his project manager that "this project is specifically for the behavior of materials at high optical power levels." Bloembergen also purchased a simpler Trion ruby laser, model LS-2, for around \$4,500.⁷⁵

Of course not *all* of Bloembergen's thinking and toiling on nonlinear laser-matter interactions was ensconced within the ivied walls of Harvard. Throughout the early-to-mid-1960s he spent a growing fraction of his time traveling between Cambridge and Washington, a corridor well trod by many academic advisors to the government. In March of 1963 the Office of the Secretary of Defense, through IDA, issued Bloembergen an "Interim Top Secret" clearance.⁷⁶ Later that summer, Bloembergen requested that IDA forward his new security clearance to the American Optical Company (one of the contractors building a prototype high-power laser for Project Seaside) so that he could discuss the classified work going on at their plant in

⁷⁴ Bloembergen interview, 1983.

⁷⁵ "Equipment for GMK Laboratory," 30 October 1963, Box 1D, Folder "October–November 1963," *NB*; "Renewal of Contract Nonr 1866(28) for One Year, Starting August 1, 1964, at \$40,000," Box 1D, Folder "Jan–March 1964," *NB*; and memorandum from N. Bloembergen to Harriet Johnson, 19 March 1964, Box 1D, Folder "Jan–March 1964," *NB*. Also see a report of Bloembergen's group's work on nonlinear optics in "Final Technical Report, Covering Period June 1, 1963 – June 1, 1965," Office of Naval Research Contract Nonr-1866(49), NR-015-807, ARPA Order No. 455, available at www.dtic.mil/dtic/tr/fulltext/u2/630452.pdf (accessed 22 September 2013). All contracts for on-campus research at Harvard were necessarily non-classified, given a prohibition on secret research at Harvard dating back to the University presidency of James Conant. Bloembergen's ARPA money came by way of Harvard, since the University had been designated an ARPA Interdisciplinary Research Laboratory in 1961. Several such laboratories were funded around the United States beginning in 1960 to support scientific and engineering research and training in materials science. See the Bloembergen oral history with Bromberg and Kelley, 27 June 1983, and Bernadette Bensaude-Vincent, "The construction of a discipline: Materials science in the United States," *Historical Studies in the Physical and Biological Sciences* 31, no. 2 (2001): 223–248.

⁷⁶ Pierre Dowd to Bloembergen, 14 March 1963, Box 1D, Folder "May 1963," *NB* (the letter is currently misplaced in these files).

Southbridge, Massachusetts.⁷⁷ His work as a contract consultant was hitting full stride just as he made his career-defining foray into nonlinear optics.

Missile Defense Work Intensifies

As scientists began to explore the intricacies of laser-matter interaction, the missile defense research and development effort continued to intensify. Early in 1962 ARPA directed almost \$2.9 million to the Office of Naval Research for the development of high-power lasers. Ruby and neodymium-glass were the gain materials of choice. Three industrial contractors—Hughes Aircraft, American Optical, and Westinghouse—made successful bids and produced four weapons prototypes within roughly a year. But in the process it would also become clear to the consultants that solid-state lasers would present terrific obstacles before they could ever be counted on for a practical ground-based ballistic missile defense system. Hughes had difficulty scaling up its ruby laser design to more than 30 joules; American Optical developed an “avalanche pulse generator” with neodymium-glass rods, but the rods shattered during testing.⁷⁸ And the projected cost was enormous: one IDA estimate of complete system cost was pegged at more than \$10 billion.⁷⁹

By October, amid a recently broken moratorium on nuclear testing and a continued stalemate in negotiations for a nuclear test ban treaty, mere days before the start of the Cuban Missile Crisis, Project Defender catapulted to the top of the defense agenda. In National Security Action Memorandum No. 191, dated October 1, 1962 and written by Deputy Special Assistant for National Security Affairs Carl Kaysen, President Kennedy “in response to a recommendation

⁷⁷ Bloembergen to Security Officer, Institute for Defense Analyses, 29 May 1963, Box 1D, Folder “May 1963,” *NB*.

⁷⁸ Barry Miller, “U.S. begins laser weapons programs,” *Aviation Week & Space Technology* (March 1962): 41–45, cited in Seidel, “From glow to flow,” 121.

⁷⁹ Seidel, “From glow to flow,” 123.

by the Department of Defense...today established [Project Defender] as being in the highest national priority category for research and development.”⁸⁰ Struggling to manage the sprawling missile defense effort, in January 1963 ARPA Director Jack Ruina announced the creation of the ARPA Advisory Committee on Ballistic Missile Defense, chaired by nuclear physicist (and Jason Steering Committee member) Kenneth Watson.⁸¹

At the same time, IDA continued to step up its laser work, especially on laser ballistic missile defense. A classified review conference on laser research sponsored by IDA’s Research and Engineering Support Division was held in February 1963 for a group of about ten IDA managers and consultants, including Keith Brueckner. Laser power sources were discussed; Charles Townes told the group about the state of the art in laser device design and cost. And a young atomic and plasma physics expert recruited to IDA from Convair named Sinai (“Si”) Rand gave a briefing on “the coupling of laser energy to solid bodies.”⁸² Rand soon issued a series of

⁸⁰ National Security Action Memorandum No. 191, National Security Action Memoranda, Meetings and Memoranda Series, National Security Files, The Papers of John F. Kennedy, John F. Kennedy Presidential Library and Museum, Boston, MA. Kennedy heard mention of the high-power laser program in Project Defender at least once, during a meeting of the National Security Council on 12 September 1963. At the meeting Air Force General Leon W. Johnson made a case for the importance of an early antiballistic missile capability, continued laser R&D and something known as Project Casaba-Howitzer, which would have directed the energy from a nuclear explosion in a kind of beam—to destroy ballistic missiles, for example. See Summary Record of the 517th Meeting of the National Security Council, Washington, September 12, 1963, United States Department of State, *Foreign Relations of the United States, 1961–1963, National Security Policy*, Volume 19, Document 141. Casaba-Howitzer had been conceived by scientists working at Livermore Laboratory. See George Dyson, *Project Orion: The True Story of the Atomic Spaceship* (New York: Henry Holt and Company, 2002). IDA consultants, including Keith Brueckner, assisted with studies on Casaba-Howitzer. See “Announcement of Casaba-Howitzer Study Effort,” 26 November 1965, Box 25, Folder 1 “Institute for Defense Analyses—Correspondence, 1965 May–December,” *KAB*.

⁸¹ J.P. Ruina to Keith Brueckner, 17 January 1963, Box 21, Folder 8 “Advanced Research Projects Agency—Ballistic Missile Defense Committee, 1963–1969,” *KAB*. For a couple of years Brueckner had also been attending the meetings of the Antimissile Research Advisory Council of the Army Rocket and Guided Missile Agency based at the Redstone Arsenal in Huntsville, AL. AMRAC, as it was usually abbreviated, provided technical oversight and guidance on the Army’s Nike missile project. Nils L. Muench to Keith A. Brueckner, 3 March 1961, Box 23, Folder 12 “Institute for Defense Analyses—Correspondence, 1961,” *KAB*.

⁸² Even without seeing a transcript of the classified briefing, it almost certainly concerned the mechanisms by which a laser beam might destroy a hard target, such as a ballistic missile reentry vehicle. Memorandum from S.S. Penner, 6 February 1963, Box 24, Folder 2 “Institute for Defense Analyses—Correspondence, 1963 February–April,” *KAB*. Brueckner had had his eye on Rand back in 1961, writing to Townes that Rand “stands out in the Convair physics group of approximately 80 people at the Ph.D. level as one of the two or three most outstanding....I think that he is the sort of young talented person who can grow enormously under the stimulating atmosphere of IDA and that his quick and sharp intelligence will be a real help on the technical problems encountered.” Keith

IDA Research Papers on the interaction between light and hard surfaces, including the interaction between high-power laser light and the shock wave surrounding an object undergoing atmospheric reentry, such as a ballistic missile reentry vehicle.⁸³

IDA hoped to improve the circulation of missile defense knowledge among its experts by setting up a classified *Journal of Defense Research*, establishing an economy of academic credit within the classified community. The new journal promised “proper credit and authority to authors of important research papers who do not choose to seek declassification,” at least according to the 1963 proposal by the journal’s first editor, Stanford S. Penner (then the head of IDA’s Research and Engineering Support Division). The new journal would nurture a competitive academic culture within classified circles, offering “quality control for classified papers of the same type that has been found effective in...unclassified scientific and engineering progress” as well as a “permanent record of achievement on defense problems...” Like an academic publication, it would be peer reviewed and guided by an editorial board; but unlike any ordinary journal it would be classified Secret and published by IDA on behalf of the Department of Defense. Reflecting IDA’s dominant focus on missile defense work in the early 1960s, Penner

Brueckner to Charles Townes, 11 May 1961, Box 23, Folder 12 “Institute for Defense Analyses—Correspondence, 1961,” *KAB*. When Brueckner departed IDA for UCSD in 1963, he recruited Rand to join him there. In 1970, amid the Vietnam era campus turmoil, Rand underwent a crisis, deciding that his work had been “irrelevant to the human condition” (in the words of two of Rand’s disappointed colleagues). Persuaded not to resign from the university, he remained at UCSD until his retirement in 1992. See David R. Miller, Daniel B. Olfe, and Stanford S. Penner, “Sinai Rand, Aerospace and Mechanical Engineering Sciences: San Diego,” in Micki Conklin, ed., *University of California: In Memoriam, 2000* (Oakland, CA: Academic Senate, University of California, 2000).

⁸³ S. Rand, “A mechanism for the efficient coupling of high-flux optical and infrared radiation to surfaces,” IDA Research Paper P-11, March 1963; S. Rand, “Breakdown of the shock about a slender body by laser radiation,” IDA Research Paper P-36, July 1963. See also S. Rand, “Effect of the bow shock on the coupling of laser radiation to surfaces,” IDA Research Paper P-28, June 1963, and S. Rand, “Shock breakaway under intense laser irradiation,” IDA Research Paper P-34, July 1963. These publications (all classified Secret) are listed in “IDA/RESD Publications Issued: January–December, 1963,” Box 35, Folder 5 “JASON Project 1964,” *MGM*.

announced that “publication will be confined to quarterly issues dealing with Ballistic Missile Defense.”⁸⁴ By April it had been rebaptized as the *Journal of Missile Defense Research*.⁸⁵

From Classified Discussion to Scientific Mainstream: Stimulated Scattering

A core topic in classical optics is the scattering of light from matter. At the microscopic scale, light is imagined as scattering from individual molecules, or collections of molecules, of the propagation medium; and there are various types of scattering, distinguished by the nature of the interaction. In “Raman scattering” (named for the Indian physicist Chandrasekhar V. Raman), for example, a photon interacts with an internal state of a single atom or molecule and either loses or gains some energy in the process, changing its original frequency. In “Brillouin scattering” (named for the French physicist Léon Brillouin), the light scatters from collective density fluctuations within the medium and, similarly, undergoes some change in energy and frequency.⁸⁶

In nonlinear optics such scattering processes are said to be “stimulated” because they are induced by the laser light itself. That is, the laser light produces excitations or fluctuations in the medium, and then scatters from them.⁸⁷ Stimulated scattering (of the Raman variety) had been

⁸⁴ S.S. Penner to Keith Brueckner, 7 February 1963, Box 24, Folder 2 “Institute for Defense Analyses—Correspondence, 1963 February–April,” *KAB*.

⁸⁵ S.S. Penner to Murray Gell-Mann, 15 April 1963, Box 35, Folder 5 “JASON Project 1963,” *MGM*. Peter Westwick has written about the creation of classified peer-reviewed science and technology journals within the Atomic Energy Commission’s National Laboratory system. See Peter J. Westwick, “Secret science: A classified community in the National Laboratories,” *Minerva* 38 (2000): 363–391. Hugh Gusterson has written on similar themes, focusing on U.S. nuclear weapons designers: Hugh Gusterson, “The death of the authors of death,” in Mario Biagioli and Peter Galison, eds., *Scientific Authorship: Credit and Intellectual Property in Science* (New York: Routledge, 2003), 281–308.

⁸⁶ See, e.g., Robert Boyd, *Nonlinear Optics* (Burlington, MA: Academic Press, 2008 [1992]), esp. 391–428.

⁸⁷ Boyd, *Nonlinear Optics*, 429–471. For example, Boyd remarks (on 429): “A light-scattering process is said to be *spontaneous* if the fluctuations (typically in the dielectric constant) that cause the light-scattering are excited by thermal or by quantum-mechanical zero-point effects. In contrast, a light-scattering process is said to be *stimulated* if the fluctuations are induced by the presence of the light field.”

observed as early as 1960, in experiments at Hughes Research Laboratories.⁸⁸ And yet it did not become a mainstay in nonlinear optics research until a particular group of early nonlinear opticians—the IDA consultants—began to recognize the huge importance of stimulated scattering in military applications of the laser, especially in missile defense.

It wasn't the case that stimulated scattering effects were simply discovered, pristine and pure, in academic or industrial laboratories and then transplanted to the classified discussions. Rather, the chronological and causal arrows often ran in the other direction. Nicolaas Bloembergen, asked in 1983 by an interviewer about the science of laser weapons, offered this suggestive comment: "Well, that's what the newspapers and clearly some military types wanted [i.e., laser weapons], and they obviously still want it again. ...But that was very heady stuff, also scientifically, because then very soon came all these propagation characteristics, through the atmosphere, stimulated Brillouin, stimulated Raman in the atmosphere."⁸⁹ Without the stringent technical requirements demanded by laser weapons applications, it is arguable that research on such extreme conditions and processes would not have proceeded—certainly not in the same way, at the same time, and not by the same intimate circle of highly placed experts. As major

⁸⁸ At Hughes a researcher named Robert Hellwarth came up with a clever technique for producing large, short pulses of laser light in 1960. The technique was known as 'Q-switching.' Hellwarth realized that if the efficiency of the laser—its 'Q factor'—could be switched abruptly from a very low value to a very high value, it would fire a giant pulse of energy just after the switch was thrown. To build a Q-switched device Hellwarth and his coworker Fred McClung fitted a Kerr cell to a ruby laser. The Kerr cell exploited the electro-optical properties of nitrobenzene, an organic liquid, to change the polarization of light that passes through it (thus potentially acting as an optical filter) when a voltage was applied across the cell. During some 1962 experiments with the new Q-switching gadget, two other Hughes scientists made a curious observation. They found that not only were the laser pulses enormous—far bigger than expected—but they also contained light at frequencies somewhat less than that of the laser light, down in the infrared region. The explanation Hellwarth and colleagues would soon suggest was stimulated Raman scattering. The culprit, they believed, was the nitrobenzene in the Kerr cell. The high electric fields of the intense laser light actually excited vibrational or rotational states within the individual molecules of the Q-switch material (the nitrobenzene). The laser light in turn scattered off of those excitations, shifting the frequency of part of the incoming beam. See Robert W. Hellwarth, "Theory of the pulsation of fluorescent light from ruby," *Physical Review Letters* 6 (1961): 9–12; F.J. McClung and R.W. Hellwarth, "Giant optical pulsations from ruby," *Journal of Applied Physics* 33 (1962): 828–829; Gisela Eckhardt, R.W. Hellwarth, F.J. McClung, S.E. Schwarz, D. Weiner, E.J. Woodbury, "Stimulated Raman scattering from organic liquids," *Physical Review Letters* 9 (1962): 455–457; R.W. Hellwarth, "Theory of stimulated Raman scattering," *Physical Review* 130 (1963): 1850–1852; Bromberg, *The Laser in America*, 131–134; Hecht, "How the laser launched nonlinear optics," 37–38.

⁸⁹ Bloembergen interview, 1983.

academic figures who had the military's ear, physicists like Bloembergen and Charles Townes were in a special position. Not only could they bring their expertise to bear on problems of national security: they could carry knowledge created in the context of national security consulting and convey it to the academic mainstream. Under the flexible publication arrangements Townes had engineered at IDA right when the Jason Division was born, that was precisely the point.

Two main problems captured the attention of the consultants by the mid-1960s. One was a problem of propagation—how to send a very intense laser beam through the atmosphere free of significant deflection or degradation. The other was a problem of generation—how to produce a high-power beam in a solid-state system without damaging the gain medium or the internal optical materials of the laser. The expert community had come to understand stimulated scattering processes as central to these problems, encouraging their focused work on stimulated scattering and, through a series of prominent articles in the open literature, making the topic a centerpiece of nonlinear optics by the middle of the decade.

IDA and Jason first began to intensify work on laser-matter interactions in 1963. That year Keith Brueckner had started to worry that the heating of the air by a powerful laser beam, and the coupling of the beam's energy to the molecules of the atmosphere, would distort and destabilize the beam itself.⁹⁰ Very quickly in classified circles, it was recognized that stimulated Raman scattering might not only be involved in the beam's interaction with the atmosphere, but might be used *within* the laser system to the beam's advantage. Brueckner, along with IDA

⁹⁰ K.A. Brueckner, "Penetration of an intense optical beam through the atmosphere" (IDA HQ 63-1421), mentioned in David A. Katcher to Jason Division, 11 April 1963, Box 35, Folder 4 "JASON Project 1963," *MGM*. This document is also cited as K.A. Brueckner, "Penetration of an intense optical beam through the atmosphere," IDA Internal Note N-23, February 1963, in "IDA/RESA Publications Issued: January-December, 1963," Box 35, Folder 5 "JASON Project 1964," *MGM*. While this classified document is mentioned in the consultants' correspondence, I am indebted to Robert Seidel's reading of it, cited in "From glow to flow," 131.

Research and Engineering Support Division scientist William Culver and the ONR's Fred Quelle, initiated a series of studies on the possibility that intentionally shifting the frequency of laser light would enhance beam propagation characteristics through the atmosphere by avoiding frequencies where absorption and heating occurred.⁹¹ As Alexander Glass (another IDA laser scientist) recalled, the desire was for “beam clean up”—the use of a Raman-scattering medium to eliminate the frequencies that proved troublesome for atmospheric propagation. Culver, always IDA's greatest enthusiast for the military applications of the laser, repeatedly stressed the need for more studies of atmospheric laser propagation in coming months.⁹²

The issues of high-power laser beam generation and propagation soon vaulted to the top of the agenda at the 1963 Jason summer study. In April, Marvin Goldberger, the Jason chairman, had started canvassing options for the study. (Potential topics included the physics of ballistic missile reentry into the atmosphere, “large” nuclear weapons, the radar “blackout” produced by the ionized aftermath of a high-altitude nuclear detonation, and a question supplied by the strategist Thomas Schelling: “How do you determine that the war is over?”)⁹³ IDA management pushed the laser work. In the end, its Research and Engineering Support Division would

⁹¹ Interview of Keith Brueckner by Finn Aaserud, 2 July 1986, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD, <http://www.aip.org/history/ohilist/4542.html>; and Seidel, “From glow to flow,” 132. Brueckner ramped up laser research even as his appointment as IDA's Director of Research ended in early 1963 following a dispute with IDA's new president, Richard Bissell, over the management of IDA's technical programs. Brueckner signed a new contract as an IDA consultant in February (continuing also as an advisor to the Jason group), just as the first results on stimulated scattering began to emerge. “Agreement for Consulting Services,” Dr. Keith A. Brueckner, 1 February 1963, Box 24, Folder 2 “Institute for Defense Analyses—Correspondence, 1963 February–April,” *KAB*.

⁹² See, e.g., W.H. Culver to Thomas F. Rogers, “Proposal for dissemination of information on atmospheric propagation for laser applications,” 16 April 1965, Box 24, Folder 9 “Institute for Defense Analyses—Correspondence, 1965 January–April”; and Eugene G. Fubini memorandum on “IDA study of atmospheric propagation,” 9 June 1965, Box 25, Folder 1 “Institute for Defense Analyses – Correspondence, 1965, May – December,” *KAB*. Alexander Glass, a fellow staff scientist working on lasers for the IDA, remembered that “[Culver] is an enthusiast, period. Anything Bill's involved with, he's an enthusiast for.” See the Interview of Dr. Alexander J. Glass by Joan Bromberg, 13 October 1986, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD, <http://www.aip.org/history/ohilist/31410.html> (hereafter cited as Glass interview, 1986).

⁹³ Marvin L. Goldberger to Keith Brueckner, 24 April 1963, Box 24, Folder 2 “Institute for Defense Analyses—Correspondence, 1963 February–April,” *KAB*.

cosponsor the study with Jason, bringing in several outside consultants. These included such notables as Peter Franken (the discoverer of optical harmonic generation), William R. Bennett, Jr. (co-inventor of the first gas helium-neon laser at Bell Laboratories), Ray Kidder (head of Livermore Laboratory's laser fusion program), and John Atwood (a laser specialist working for Perkin-Elmer, an optical company and defense contractor).⁹⁴

The IDA and Jason experts met at Woods Hole from June to August 1963, chipping away at the issues of beam propagation, stability, and stimulated scattering. Building on Brueckner's earlier work on optical beam stability, several members of the group tackled the problem independently. Goldberger and Kenneth Watson, for example, wrote a paper on beam stability, later combining it with some of Brueckner's calculations in a Jason Research Paper. IDA's Andrew Sessler, along with Brueckner, Goldberger, Norman Kroll, Watson, and the young Jason theorist Steven Weinberg prepared a similar paper for publication in the classified *Journal of Missile Defense Research*.⁹⁵ Additional classified IDA research reports on beam quality and stability were produced from the summer work, including Brueckner's "Instability of an Intense Optical Beam" and "Cloud Penetration by a Laser Beam," while the summer study as a whole was summarized in a two-volume classified report.⁹⁶ (Among the study's major

⁹⁴ Robert J. Collins, Memorandum for the Files, Subject: Summer Study Participants, 29 April 1963, Box 24, Folder 2 "Institute for Defense Analyses—Correspondence, 1963 February–April," *KAB*. On William Bennett see Jeremy Pearce, "William R. Bennett, 78, pioneer in gas lasers, dies," *New York Times* (7 July 2008): A15. On Kidder, see Bromberg, *The Laser in America*, 214–216.

⁹⁵ Brueckner, tireless as ever, spent 28 days during the month of July supervising the summer project (he had rented a house in Falmouth, Massachusetts for his family, commuting a few miles down the road each day to the study at the Whitney Estate in Woods Hole). David A. Katcher to Brueckner, Goldberger, Watson, Weinberg, and Sessler, 18 October 1963; and Brueckner to M.L. Goldberger, 28 October 1963, Box 24, Folder 4 "Institute for Defense Analyses—Correspondence, 1963 September–December," *KAB*. The larger paper was published as K.A. Brueckner, G.F. Carrier, M.L. Goldberger, N.M. Kroll, A.M. Sessler, K.M. Watson, and S. Weinberg, *Journal of Missile Defense Research* 2 (1964): 11; Keith Brueckner, "Jason/RESO Summer Study, July Consulting Statement," 30 July 1963, Box 24, Folder 3 "Institute for Defense Analyses—Correspondence, 1963 May–August," *KAB*.

⁹⁶ K.A. Brueckner, "Instability of an intense optical beam," Jason Research Paper P-42, December 1963, mentioned in "Jason Accession List No. 9," 3 June 1964, Box 35, Folder 5 "JASON Project 1964," *MGM*; the summer study report is mentioned in "Classified Reports by Keith A. Brueckner," Box 21, Folder 7 "Consulting Summary, 1953–1973," *KAB*. The 1963 Jason/IDA laser summer study would be remembered by Alexander Glass,

recommendations to ARPA was the creation of a special laboratory for laser research, though several months later Brueckner would report to the Jasons that there had been “no hearty response” to the idea.⁹⁷)

As the consultants mulled over the atmospheric propagation characteristics of high-power lasers, they took their concerns back to their university chalkboards and lab benches. Charles Townes (now commuting from an administrative position as MIT’s Provost) had confronted the beam quality and stability issue for the first time in IDA’s Laser Advisory Committee. By no accident, right in the middle of the 1963 Jason summer study on lasers, Townes (with graduate student Elsa Garmire and another visiting scientist), submitted one of the earliest theoretical treatments of stimulated Raman scattering to *Physical Review Letters (PRL)*, where it was published at the end of the summer.⁹⁸

Nicolaas Bloembergen, too, set out on his first investigations of stimulated Raman scattering in the same period. By April of 1964 Bloembergen and his student Ron Shen had submitted their group’s first paper on the topic to *PRL* as well. Bloembergen and Shen took issue with Townes’s calculations and strove to put the whole topic on a firmer mathematical footing. The coauthors billed their effort as “a straightforward extension of the theory of coupled light

newly hired out of graduate school as a staff scientist at IDA working on the laser project (and William Culver’s successor as head of laser work at RESD), as “seminal...the point of departure for a lot of our discussions.” See Glass interview, 1986.

⁹⁷ Brueckner mentioned a proposal for the “reassignment of certain areas of responsibility to a centralized laboratory” in a letter written in his capacity as Chairman of the Jason laser summer study, asking Charles Townes to serve on a committee to review the summer study’s findings. The proposal for a dedicated laser research laboratory was still alive as late as January 1965, though it appears ARPA never acted on it. See Brueckner to Townes, 24 June 1963, Accession 22019, Box 6 of 42, Folder “Institute for Defense Analyses,” *CHT*. Also see David A. Katcher to Jason, 15 May 1964, Box 24, Folder 7 “Institute for Defense Analyses—Correspondence, 1964 May–June,” *KAB*, where the laser laboratory proposal is mentioned as well. The final mention of the laser research laboratory is Keith A. Brueckner to W.H. Culver, 10 January 1965, Box 24, Folder 9 “Institute for Defense Analyses—Correspondence, 1965 January–April,” *KAB*.

⁹⁸ E. Garmire, F. Pandarese, and C.H. Townes, “Coherently driven molecular vibrations and light modulation,” *Physical Review Letters* 11, no. 4 (1963): 160–163. Townes and his coauthors calculated the effects semi-classically, treating stimulated Raman emission as the result of coupled molecular dipoles oscillating in response to the electric field of the laser. The paper did not reference any of the earlier results in nonlinear optics, including Bloembergen’s work on optical harmonics and his detailed calculations of nonlinear susceptibilities.

waves”—i.e., techniques debuted in Bloembergen’s original 1962 paper—“to include the effects of the imaginary part of the nonlinear susceptibilities.” Townes later said that “Bloembergen took exception to [Townes’s earlier calculation] and felt that it wasn’t right, so he published an alternative way of describing it. ...Bloembergen put it in a more abstract mathematical framework...but it was essentially the same thing. There was more apparent disagreement than real disagreement.”⁹⁹ Disagreement real or apparent, it was more than mere coincidence that both Townes and Bloembergen had started earnest theoretical investigations of stimulated Raman scattering in mid-to-late 1963, when Brueckner’s directive and the IDA/Jason summer study had jumpstarted efforts in this area.

The cases of stimulated Brillouin scattering and the self-trapping of laser light provide even clearer instances of academic research following defense discussions and priorities. At a meeting of the IDA Laser Advisory Committee organized by William Culver in IDA’s Washington offices in February of 1964, an agenda was set for “achieving beam directivity of high powered lasers.” A few items were familiar from the research already initiated by Brueckner, including the destabilization of the beam from “self heating of the atmosphere.” But now a new topic had been floated: “limitations on laser beam coherence due to electro-strictive coupling of high intensity light waves to sound waves within the laser.”¹⁰⁰

This was something altogether different. Norman Kroll, a charter member of the IDA Laser Advisory Committee and one of the original Jasons, had initiated investigation of this new kind of interaction between laser light and matter, circulating a Jason Research Paper that same month on “Excitation of Hypersonic Vibrations by Means of Electrostrictive Coupling of High

⁹⁹ N. Bloembergen and Y.R. Shen, “Coupling between vibrations and light waves in Raman laser media,” *Physical Review Letters* 12 (1964): 504–507. For Townes’s quotation, see the Townes interview, 1984.

¹⁰⁰ W.H. Culver to Keith A. Brueckner, 5 February 1964, Box 24, Folder 5 “Institute for Defense Analyses – Correspondence, 1964 January – February,” *KAB*.

Intensity Light Waves to Sound Waves.”¹⁰¹ Kroll later published an unclassified version of the paper, in which he acknowledged the stimulus behind the work: “That some material damage induced by laser illumination might be due to developed photoelastic instability was suggested to the author by C.H. Townes. This suggestion was the starting point of this investigation.” In other words, the context for his study of the interaction between light and the structural properties of matter—“electrostrictive” or “photoelastic” coupling—was IDA’s Laser Advisory Committee.¹⁰²

Townes had gotten his hands on a copy of the classified appendix to Kroll’s paper on photoelastic coupling from the Jason secretary as early January of 1964 (a month before the Kroll report was officially circulated).¹⁰³ Then, back at MIT, with the visiting Canadian experimenter Boris Stoicheff, Townes began to look for scattering of light from stimulated elastic vibrations with a new Trion laser he had purchased for his university laboratory. By the spring of 1964 Townes’s group had made the first definitive observation of the effect in quartz and sapphire; he was apparently the first to christen the process “stimulated Brillouin scattering.” As he described it, high-intensity sound waves in the propagation medium were created through a process called “electrostriction” (an effect in which an applied electric field—in this case, the electric field of the intense light—strains and distorts the physical structure of the medium and the arrangement of its atoms). Some of the incident laser radiation would then scatter off of these vibrational modes and undergo a frequency shift, as with stimulated Raman scattering.¹⁰⁴

¹⁰¹ “Jason Accession List No. 10,” 2 September 1964, Box 35, Folder 5 “JASON Project 1964,” *MGM*.

¹⁰² Norman M. Kroll, “Excitation of hypersonic vibrations by means of photoelastic coupling of high-intensity light waves to elastic waves,” *Journal of Applied Physics* 36 (1965): 34–43, quotation on 42.

¹⁰³ David A. Katcher to Charles Townes, 27 January 1964, Accession 22019, Box 6 of 42, Folder “Institute for Defense Analyses (2),” *CHT*.

¹⁰⁴ R. Y. Chiao, C.H. Townes, and B.P. Stoicheff, “Stimulated Brillouin scattering and coherent generation of intense hypersonic waves,” *Physical Review Letters* 12 (1964): 592–596. Elsa Garmire remembered the work on stimulated Brillouin scattering as growing out of discussions between Townes and Norman Kroll—though she was aware only of Kroll’s status as a professor at UCSD, and not of the classified context of their collaboration. See the Interview of Dr. Elsa Garmire by Joan Bromberg, 4 February 1985, Niels Bohr Library & Archives, American

The study of photoelastic coupling and stimulated Brillouin scattering prompted, in turn, Townes's careful thinking about a problem he had recently been made aware of in the course of his consulting activities: the creation of thin, filament-like streaks of damage in the glass in which laser light was amplified and propagated. His student Elsa Garmire recalled that Townes had been particularly impressed by glass damage shown to him by John Atwood at the defense contractor Perkin-Elmer. (Atwood, another member of the elite consulting community, had been present at the 1963 IDA/Jason laser summer study.)¹⁰⁵ Solid-state lasers might never be scalable to power levels required for antimissile duty—the power levels Bloembergen had enthusiastically imagined in the IDA laser meeting in 1961—if the damage was a limitation not simply of materials or design but of fundamental physics.

Townes suspected that the damage arose from a nonlinear optical effect closely related to the electrostriction he had been studying: the self-trapping of the beam. Expressed in terms of the nonlinear susceptibility, Townes knew that the optical susceptibility of a material is related simply to its index of refraction (which indicates how light will propagate through the material). A nonlinear optical susceptibility makes for a nonlinear index of refraction—an index of refraction said to be “intensity dependent.” The intensity of a laser beam, however, isn't uniform throughout a cross-section of the beam; it follows something more like a bell-curve shape, more intense in the center and dwindling in intensity further out. Townes realized that the edge of the beam would tend to focus inward as it passed through the material. Balanced by the ordinary optical dispersion of the beam, which would push it outward, the beam would self-trap, creating

Institute of Physics, College Park, MD, USA, <http://www.aip.org/history/ohilist/4621.html> (hereafter cited as Garmire interview, 1985).

¹⁰⁵ The subsequent paper cited observations of glass damage by Michael Hercher at the University of Rochester's Institute of Optics, and did not mention Atwood or Perkin-Elmer.

its own waveguide and punching a huge amount of energy through a narrow tunnel in the glass.¹⁰⁶

Townes set his graduate students Elsa Garmire and Raymond Chiao to work on a few of the calculations as the group prepared a paper at the end of the summer. On August 31 Townes urgently wrote to David Katcher, the Jason secretary, asking for “the latest version of Norman Kroll’s work on acoustic excitations by laser beams. Even if I have already had one copy of his latest, please send me another.”¹⁰⁷ At the beginning of September his group submitted their results to *PRL* under the title “Self-trapping of optical beams.”¹⁰⁸ Townes also wrote up a version of the self-focusing study as Jason Internal Note N-184, “Comments on Self-Trapping in the Generation and Transmission of High-Power Laser Beams.” Classified Secret, it was circulated among the Jasons that same month.¹⁰⁹ Paul Kelley, a physicist at MIT’s Lincoln Laboratory and another consultant to IDA, as well as a frequent interlocutor with Townes’s group, soon considered the case of self-focusing in a Raman-scattering medium, where the dispersion of the beam would be overcompensated by the focusing effect and the beam would completely collapse inward—the case of true self-focusing, rather than self-trapping. His results, too, rapidly made their way into the pages of *PRL*.¹¹⁰

It was Nicolaas Bloembergen who would tie everything together—in the open literature and in the classified community at the same time. In the second half of 1964 Bloembergen (again

¹⁰⁶ For a summary of self-focusing see, e.g., Y. Ron Shen, “Self-focusing and filaments of light: Past and present,” in Robert W. Boyd, Svetlana G. Lukishova, and Y.R. Shen, eds., *Self-Focusing: Past and Present* (New York: Springer, 2009), 3–19; and R.Y. Chiao, T.K. Gustafson, and P.L. Kelley, “Self-focusing of optical beams,” in Boyd et al., eds., *Self-Focusing*, 129–143.

¹⁰⁷ Charles Townes to David Katcher, 31 August 1964, Accession 22019, Box 6 of 42, Folder “Institute for Defense Analyses (2),” *CHT*.

¹⁰⁸ R.Y. Chiao, E. Garmire, and C.H. Townes, “Self-trapping of optical beams,” *Physical Review Letters* 13 (1964): 479–482.

¹⁰⁹ David A. Katcher to the Jason Division, 19 November 1964, Box 35, Folder 5 “JASON Project 1964,” *MGM*.

¹¹⁰ P.L. Kelley, “Self-focusing of optical beams,” *Physical Review Letters* 15 (1965): 1005–1008.

with his student Shen, who had been hired as a postdoctoral researcher at the University of California, Berkeley, where Bloembergen spent a sabbatical term) wrote a *Physical Review* article that incorporated the various types of stimulated scattering into a general, unified framework.¹¹¹ Then, befitting the life of a busy defense consultant, he promptly crossed the country to attend a two-day January 1965 meeting of the IDA Laser Advisory Committee, held to discuss “problems of generating and propagating coherent beams.”¹¹² Bloembergen briefed the group on the “fundamentals of stimulated Raman and Brillouin scattering,” while a discussion of “self-trapping of optical beams and material damage”—presumably led by Townes—occupied much of the second day. The meeting concluded with a presentation of program recommendations to ARPA Director Robert Sproull.¹¹³

Notes in Bloembergen’s files from the weeks following document his thinking about the role of stimulated Brillouin scattering as a mechanism of damage-production (within the laser gain medium itself). The pages are filled with numerous calculations of stimulated scattering effects in high-power beam production, jotted down in Bloembergen’s elegant scrawl.¹¹⁴ In the same period, spanning just a few weeks, Bloembergen was encouraging John Atwood at the Perkin-Elmer Corporation (Bloembergen had been a contract consultant with Perkin-Elmer since 1961 and, like Townes, had been in regular contact with Atwood) to publish Atwood’s results on “the damage characteristics by a single mode laser beam.” “I shall have to think some more about the theoretical explanation of the damage channel,” Bloembergen added; perhaps it had

¹¹¹ N. Bloembergen and Y.R. Shen, “Theory of stimulated Brillouin and Raman scattering,” *Physical Review* 137, no. 6A (1965): A1787–A1805. Whereas the stimulated Raman effect had involved scattering from induced optical phonons, Bloembergen and Shen said that stimulated Brillouin effect involved light scattering from induced *acoustic* phonons. Also see the Bloembergen interview, 1983, for a discussion of the significance of this paper.

¹¹² W.H. Culver to Nicolaas Bloembergen, 13 January 1965, Box 1D, Folder “January 1965,” *NB*.

¹¹³ W.H. Culver to Keith Brueckner, 13 January 1965, Box 24, Folder 9 “Institute for Defense Analyses—Correspondence, 1965 January–April,” *KAB*.

¹¹⁴ Undated notes on “Momentum and Energy Transfer from a Laser Beam, Mechanisms of Laser Damage,” Box 1D, Folder “1965 February–May,” in *NB*.

something to do “with my suggestion last summer that hypersonic shock waves are involved.” He ended by encouraging Atwood to use one of the company’s high-power diffraction-limited lasers to run experiments on stimulated Raman scattering.¹¹⁵

As the weeks and months ticked by, Bloembergen continued to refine and expand his studies of stimulated scattering, working both with his university colleagues and in consultation with the IDA committee. He and Shen now ventured into the topic of self-focusing, independently making the first laboratory observations in the United States of the pure self-focusing effect. It was yet another result fit for the pages of *PRL*.¹¹⁶ Bloembergen pushed the general field in new directions, including the nonlinear optics of plasmas and the coupling of light to spin waves in solids.¹¹⁷ Townes and his students observed (and even photographed) the self-focusing of laser light in a sample of liquid carbon disulfide in early 1966.¹¹⁸ Norman Kroll, who was now heading up high-power laser work for the Jason group, made further headway on the problem of atmospheric propagation, publishing a classified Jason Research Paper in 1965 on stimulated Raman scattering in the atmosphere by multi-frequency laser light.¹¹⁹ Keith Brueckner, who had kicked off the frantic study of beam propagation from his post in IDA in

¹¹⁵ Bloembergen to Mr. John G. Atwood, 26 January 1965, Box 1D, Folder “January 1965,” *NB*.

¹¹⁶ Y.R. Shen and Y.J. Shaham, “Beam deterioration and stimulated Raman effect,” *Physical Review Letters* 15 (1965): 1008–1010; P. Lallemand and N. Bloembergen, “Self-focusing of laser beams and stimulated Raman gain in liquids,” *Physical Review Letters* 15 (1965): 1010–1012. Garmire suggests that Shen and Bloembergen narrowly beat out Townes’s group in making the first self-focusing observation in the Garmire interview, 1985.

¹¹⁷ Major publications by Bloembergen and collaborators from this period include N. Bloembergen and Y.R. Shen, “Optical nonlinearities of a plasma,” *Physical Review* 141 (1966): 298–305; N. Bloembergen and P. Lallemand, “Complex intensity-dependent index of refraction, frequency broadening of stimulated Raman lines, and stimulated Rayleigh scattering,” *Physical Review Letters* 16 (1966): 81–84; Y.R. Shen and N. Bloembergen, “Interaction between light waves and spin waves,” *Physical Review* 143 (1966): 372–384.

¹¹⁸ E. Garmire, R.Y. Chiao, and C.H. Townes, “Dynamics and characteristics of the self-trapping of intense light beams,” *Physical Review Letters* 16 (1966): 347–349.

¹¹⁹ N. Kroll, “The Stimulated Raman Effect in the Atmosphere for a Multi-frequency High Intensity Beam,” Jason Research Paper P-216, July 1965. See attachment to “ARPA Project Assignment No. 17, 1 January 1960,” Box 36, Folder 4 “JASON Project May–Jun. 1967,” *MGM*.

1963, summed up his work in a general paper on instability and energy transfer in a laser beam propagating through a fluid, writing it up for *PRL* in 1966.¹²⁰

And so the brickwork of nonlinear optics in the United States was laid by defense consultants simultaneously preoccupied with the physics of high-power lasers for military applications, especially missile defense. It is difficult to overstress how thickly and richly entangled—topically and temporally and institutionally—defense work and basic research were for these elite consultants. Indeed these two activities were, in essence, indistinguishable; the traditional categories of basic and applied, civilian and military, fail to capture the blended character of their work.

What is especially striking about the later inclusion of stimulated scattering in the core of nonlinear optics by 1964–65 was its convergence with earlier work on harmonic generation. Retrospectively it seems clear that the two topics should be included within the same field: each involved the creation, by laser light, of environmental conditions that acted back on and modified the light. But the earliest literature treating harmonic generation, including the Bloembergen group's seminal paper in 1962, did not mention the possibility of stimulated scattering. Nor did the earliest literature on stimulated scattering and self-focusing (from authors other than Bloembergen) refer to earlier work on optical harmonic generation, or appeal to higher-order

¹²⁰ K.A. Brueckner and S. Jorna, "Linear instability theory of laser propagation in fluids," *Physical Review Letters* 17 (1966): 78–81. In turn Kroll and Paul Kelley each produced important work in the open literature on the 'thermal blooming' of laser beams propagating through an absorptive medium like the atmosphere. Thermal blooming, a tricky problem over which Brueckner and Townes had argued bitterly back in 1963, is the opposite of self-focusing. In a gas that absorbs laser energy thermally, the larger average intensity at the center of the beam causes the gas to heat and expand in the beam's path, reducing the index of refraction there. The beam quickly spreads outward rather than focusing inward, dissipating its power. R.L. Carman and P.L. Kelley, "Time dependence in the thermal blooming of laser beams," *Applied Physics Letters* 12 (1968): 241–244; N.M. Kroll and P.L. Kelley, "Thermal blooming and instability of light beams due to absorption," *IEEE Journal of Quantum Electronics* 4, no. 5 (1968): 328. Townes recalled his battles with Keith Brueckner over the molecular physics of thermal blooming in the Townes interview, 1984. His memory is corroborated by correspondence in his files: Townes to Brueckner, 16 April 1963, and Brueckner to Townes, 29 April 1963, Accession 22019, Box 6 of 42, Folder "Institute for Defense Analyses (2)," *CHT*.

nonlinearities in the susceptibility of the propagation medium.¹²¹ It would take Nicolaas Bloembergen—an expert in harmonic generation who was concurrently involved in the IDA Laser Advisory Committee’s deliberations over the importance of stimulated scattering—to unite the two phenomena in an overarching theoretical framework in the period 1964–66.

The Eclipse of the Solid-State Laser Weapon

Just as the number of live research topics was expanding in nonlinear optics, however, intractable problems began to chasten and redirect the Pentagon’s dreams for laser ballistic missile defense. The IDA advisors, who had registered some skepticism even as early as the 1962 summer study, remained wary of endorsing the feasibility of a working system. Even Nicolaas Bloembergen, who had enthusiastically promoted solid-state lasers to IDA in 1961 (so enthusiastically that Charles Townes remembered it years hence as “quite a pep talk”), was apparently less sanguine about the future of high-power solid-state lasers by the middle of the decade.¹²² In early 1964 Bloembergen was irked when he came across a *Harvard Crimson* article on laser weapons, which ascribed to lasers “many of the properties of science-fiction ‘death-rays,’” and to Bloembergen himself the view that laser research “may lead to the most significant military development since the ballistic missile.” (Charles Townes saw the article, too, and sent a copy to William Culver at IDA, anxiously asking “whether there are any security restrictions on

¹²¹ Bloembergen’s 1966 paper with Lallemand suggests, in its opening sentence, that the original 1962 article on nonlinear optics had “recognized...the intensity-dependent index of refraction...as a generalization of the quadratic Kerr effect.” Whether or not Bloembergen had actually recognized the intensity-dependent index of refraction in 1962 (the term does not appear in the original paper, though the Kerr effect is discussed), he did not predict self-trapping or self-focusing (the most notable consequence of the intensity-dependent index of refraction), nor discuss stimulated scattering effects of any kind in 1962. Charles Townes, who discussed the intensity-dependent index of refraction and predicted self-trapping in 1964, apparently did so in response to conversations with John Atwood at Perkin-Elmer about laser material damage, and with Norman Kroll and colleagues at the IDA about electrostriction and stimulated scattering. Townes, however, did not situate these effects within the general theory of nonlinear susceptibilities (as Bloembergen later would).

¹²² Townes interview, 1984.

the material discussed?”) A few days later Bloembergen chastised the student journalist: “The realization of a laser weapon system is an enormous engineering problem with an uncertain outcome,” he wrote. “The United States Government cannot afford to take chances and, therefore, continues to support laser work as long as the impossibility of a weapon realization is not entirely ruled out.”¹²³

In fact the Pentagon was significantly reconfiguring its investment in lasers in early 1966. ARPA was in the process of offloading its research effort onto programs managed by the Air Force Weapons Laboratory and the Naval Research Laboratory.¹²⁴ By February IDA was shifting its attention from solid-state lasers to new technologies, especially the highly classified gas-dynamic laser being developed by researchers at the industrial firms AVCO, Boeing, United Aircraft Company, and others.¹²⁵ In the gas-dynamic laser, a mixture of carbon dioxide and

¹²³ A clipping of the article is attached to Charles Townes to William Culver, 21 January 1964, Accession 22019, Box 6 of 42, Folder “Institute for Defense Analyses (2),” CHT. The clipping does not reveal the author or exact date of publication, but indicates that the article was found on the first page of a recent issue of the *Harvard Crimson*. Also see Bloembergen to Stephen Bello, 21 January 1964, Box 1D, Folder “Jan–March 1964,” NB, in which Bloembergen indicates that Bello was the author of the article. On the early fascination for lasers as “death-rays,” see Rebecca Slayton, “From death rays to light sabers: Making laser weapons surgically precise,” *Technology and Culture* 52 (2011): 45–74. Slayton points out that military managers often expressed more caution than enthusiasm in public about the technological possibilities of lasers, even in the heady early days of the new device, in order to retain tighter control over their development programs. Bloembergen’s caution, however, seems more a product of his intimate familiarity with the difficulties involved in scaling up solid-state lasers for high-power applications.

¹²⁴ Charles M. Herzfeld, “Comments on IDA Laser Advisory Committee Meeting,” 20 April 1966, Box 25, Folder 2 “Institute for Defense Analyses—Correspondence, 1966 January–June,” KAB.

¹²⁵ J.L. Walsh to Charles Townes, 11 February 1966, Accession 22019, Box 6 of 42, Folder “Institute for Defense Analyses (2),” CHT; J.L. Walsh to Keith Brueckner, 7 November 1966; and “IDA High Power Laser Advisory Committee Meeting, December 9, 1966,” Box 25, Folder 3 “Institute for Defense Analyses—Correspondence, 1966 July–December,” KAB. Work on the gas-dynamic laser was originally considered so secret that an additional layer of classification was draped over it. John Martin of Jason told the consultants that gas-dynamic work “is considered by DOD to be particularly sensitive. The Department is in fact generating just now new rules to remind you of the sensitivity of this material.” John Martin, “For Information Only,” 5 June 1968, Accession 22019, Box 6 of 42, Folder “Institute for Defense Analyses (2),” CHT. The U.S. government’s original classification policy on lasers dated from 1964, after a special ad hoc advisory group to the Atomic Energy Commission and Department of Defense (including the nonlinear optics expert Peter Franken) agreed that lasers should be classified on the basis of power output. Specifically, R&D on lasers and laser systems was to be classified “if the maximum power output is 10^{11} watts or greater and the total energy output is 10^3 joules or greater.” In addition, any use of lasers for the purposes of thermonuclear fusion, or “toward a classified goal (e.g., pure fusion weapons or anti-ICBM devices) will be conducted on a classified basis.” See “Classification Policy for Lasers,” 16

nitrogen gas was heated to thousands of degrees, and the gas rapidly cooled and expanded by ejection at supersonic speeds through a thin nozzle. The process achieved significant pumping of the molecular energy states of the carbon dioxide and a power output several orders of magnitude larger than the best solid-state lasers. The DOD began pouring money into the new program: whereas one estimate pegged a typical Pentagon laser contract at roughly \$100,000 per year, AVCO was pulling in around \$2 million for its gas-dynamic laser work in 1966.¹²⁶

By 1967 solid-state lasers had been ruled out permanently by ARPA, its IDA consultants, and industrial contractors. The Jasons took over the Falmouth Intermediate School on Cape Cod for their summer study that year, devoting three weeks to laser weapons under the leadership of Norman Kroll. The usual cast of IDA characters paid visits to the study, including Bloembergen, Brueckner, Kelley, Kidder, and Penner; the group from Jason included Townes, Freeman Dyson, Richard Garwin, and Steven Weinberg.¹²⁷ Collectively they admitted that the gas-dynamic laser, successfully demonstrated the previous year by researchers at AVCO-Everett, AVCO's research facility in Massachusetts, would be the military's new go-to technology for a laser-based missile defense system. By 1968, the heat and light surrounding solid-state laser weapons had dimmed completely.¹²⁸

Nonlinear optics, however, had taken off through the mid-1960s, rising to the top of the agenda at major international conferences.¹²⁹ Bloembergen penned the first standard American

July 1964, Box 1428, Folder 6 "Sec 2-1, Classification, Vol. 4," Record Group AEC 326, U.S. Department of Energy Archives. My thanks to Alex Wellerstein for sharing this document with me.

¹²⁶ J.L. Walsh to Distribution, 30 November 1966, Box 25, Folder 3 "Institute for Defense Analyses—Correspondence, 1966 July–December," *KAB*.

¹²⁷ "Jason Summer Study, Laser Weapon, N. Kroll, June 26 – July 14," Box 36, Folder 4 "JASON Project May–Jun. 1967," *MGM*.

¹²⁸ Seidel, "From glow to flow," 135–141.

¹²⁹ A major international venue for the dissemination of laser research in this period was the series of International Conferences on Quantum Electronics. The first was held in New York City in September 1959; the second and third were held in Berkeley, California and Paris in 1961 and 1963, respectively. See the Interview of Dr. N.G. Basov by Arthur Guenther, 14 September 1984, Niels Bohr Library & Archives, American Institute of

textbook on nonlinear optics for the publisher W.A. Benjamin in 1965, quickly bound together from notes written for a course on quantum electronics he had given at Harvard in 1963. The book (which would merit four editions, the most recent in 1996) presented the classical and quantum theories of nonlinear susceptibilities followed by assorted topics in stimulated scattering. The combined presentation of harmonics and stimulated scattering would become a canonical way of framing the field, as borne out by subsequent standard textbooks, including one written by Bloembergen's student Y.R. Shen.¹³⁰

A Study in Contrasts: Soviet Nonlinear Optics

Soviet scientists were heavily involved in their own government's antiballistic missile effort; and laser research, including nonlinear optics, flourished in the Soviet Union in the same years as its spectacular growth in the United States. But unlike in the American context, the Soviet advisors to the antimissile program and the scientists building up Soviet nonlinear optics worked, for the most part, within segregated communities. Teams headed by Nikolai Basov and O.N. Krokhin in the Lebedev Physics Institute of the U.S.S.R. Academy of Sciences advised the military bureaucracy at the Vympel Design Bureau on laser radar and high-power laser weapons.¹³¹ Soviet nonlinear optics, meanwhile, blossomed independently at the Moscow State University, where Rem Khokhlov and Sergei Akhmanov opened the first Soviet nonlinear optics

Physics, College Park, MD, <http://www.aip.org/history/ohilist/4495.html>; Bromberg, *The Laser in America*, 97. Likewise, the first International Conference on Coherent and Nonlinear Optics was organized by Soviet scientist Rem Khokhlov in 1965 at the Moscow State University; Bloembergen and numerous other leading lights were in attendance. See Sergei Bagayev, Oleg Krokhin, and Alexander Manenkov, "Pages from the history of quantum electronics research in the Soviet Union," *Journal of Modern Optics* 52, no. 12 (August 2005): 1657–1669.

¹³⁰ Nicolaas Bloembergen, *Nonlinear Optics* (New York: W.A. Benjamin, Inc., 1965); Nicolaas Bloembergen, *Nonlinear Optics, 4th Ed.* (River Edge, NJ: World Scientific, 1996); Y.R. Shen, *The Principles of Nonlinear Optics* (New York: John Wiley & Sons, 2002 [1984]); Robert Boyd, *Nonlinear Optics* (Burlington, MA: Academic Press, 2008 [1992]).

¹³¹ P.V. Zarubin, "Academician Basov, high-power lasers and the antimissile defence problem," *Quantum Electronics* 32, no. 12 (2002): 1048–1064.

laboratory and soon became known for their pioneering experiments in optical harmonic generation, coauthoring the first standard Russian text on nonlinear optics in 1964.¹³²

Positioned within institutional frameworks for laser research and military advising that were markedly different than those in the U.S., Soviet researchers did not as readily blend particular problems and techniques in the same way, or in the same order. Whereas harmonic generation research was concentrated in the group of Khokhlov and Akhmanov at Moscow State University, researchers at the U.S.S.R. Academy of Sciences were the first to investigate stimulated Brillouin scattering in the Soviet Union (where the effect was known as Mandel'shtam-Brillouin scattering).¹³³ Alexander Prokhorov (co-founder, with Basov, of Soviet maser and laser research) led the earliest Soviet work on electrical breakdown and molecular dissociation of the propagation medium (a problem that Kroll, Brueckner, and Townes had long worried about in the U.S.).¹³⁴ Self-focusing was pioneered by Vladimir Talanov, a researcher born, trained, and for his entire career employed in the military-industrial city of Gork'ii. Building on the prediction of optical self-trapping of light in a plasma made by the Soviet theorist and military scientist G.A. Askaryan in 1962, Talanov predicted the effect in nonlinear optical materials in 1963, even slightly ahead of Charles Townes and his group.¹³⁵

But stimulated Brillouin scattering and self-focusing—topics that had gestated in the context of research on high-power laser applications—did not permeate Soviet nonlinear optics in quite the way they had in the United States. Prokhorov and Zel'dovich and their colleagues at

¹³² A.S. Chirkin and K.N. Drabovich, "Sergei Aleksandrovich Akhmanov (on the anniversary of 70th birthday)," *Laser Physics* 9, no. 5 (1999); S.A. Akhmanov and R.V. Khokhlov, *Problemy Nelineinoi Optiki* (Moscow, 1964), translated as *Problems of Nonlinear Optics* (London: Routledge, 1972).

¹³³ D.I. Mash, V.V. Morozov, V.S. Starunov, E.V. Tiganov, and I.L. Fabelinskii, "Induced Mandel'shtam-Brillouin scattering in solid amorphous bodies and in liquids," *JETP Letters* 2, no. 5 (1965): 246–249.

¹³⁴ S.L. Mandel'shtam, P.P. Pashinin, A.V. Prokhindeev, A.M. Prokhorov, and N.K. Sukhodrev, "Study of the 'spark' produced by in air by focused laser radiation," *Journal of Experimental and Theoretical Physics* 20, no. 5 (1965): 1344–1346; F.V. Bunkin, R.V. Karapetyan, and A.M. Prokhorov, "Dissociation of molecules in a strong radiation field," *Journal of Experimental and Theoretical Physics* 20, no. 1 (1965): 145–148.

¹³⁵ "Vladimir Il'ich Talanov (on his seventieth birthday)," *Physics-Uspokhi* 46, no. 6 (2003): 665–666.

the Academy, and Talanov, cloistered away in Gork'ii's secret research facilities, had little or no contact with the main nonlinear optics school at Moscow State University. (Akhmanov and Khokhlov also soon began research on self-focusing, but their first articles on the subject did not come out until 1966.¹³⁶) Unlike their Soviet counterparts, the American founders of nonlinear optics had been socially entwined and engaged in virtually every facet of nonlinear optics as it emerged. The circulation of elite defense consultants had encouraged a peculiarly broad topical complexion to the field in the United States.

Conclusion

Was nonlinear optics a fundamental science, or was it an applied field, perhaps a branch of engineering? The history of nonlinear optics, like that of so many interdisciplinary Cold War sciences, suggests that such categories in the postwar era were mirage-like, shimmering in and out of focus. Nicolaas Bloembergen was sufficiently well aware of the dual character of nonlinear optics to take out a patent on nonlinear optical processes not long after the publication of his foundational 1962 *Physical Review* paper.¹³⁷

Yet he also argued in correspondence with other physicists that nonlinear optics was a basic field, comfortably rubbing shoulders with the purest of the traditionally pure. In the fall of 1962, for example, just as work in nonlinear optics was beginning to coalesce in several laboratories in the U.S., University of North Carolina physicist Cecile DeWitt wrote to Bloembergen seeking his advice about potential themes for an upcoming eight-week session of the Les Houches summer school in the French Alps. Les Houches was an esteemed yearly event,

¹³⁶ S.A. Akhmanov, A.P. Sukhorukov, and R.V. Khokhlov, "Self-focusing and self-trapping of intense light beams in a nonlinear medium," *Journal of Experimental and Theoretical Physics* 23, no. 6 (1966): 1025–1033.

¹³⁷ Nicolaas Bloembergen, "Apparatus for converting light energy from one frequency to another," patent no. 3,384,433 (patented 21 May 1968), United States Patent Office. The patent application was filed on 9 July 1962, just months after Bloembergen's group had submitted their groundbreaking study.

renowned for tackling foundational problems on the leading edge of physics (recent meetings had studied the topics of “dispersion relations and elementary particles,” “low temperature physics,” and “relativity and topology,” for example). She wondered whether it might be possible to organize a session on quantum electronics, but to somehow emphasize the basic theoretical questions that underlay the field.¹³⁸

Bloembergen answered that the fundamental theoretical questions beneath quantum electronics were identical with the problems of nonlinear optics. He dilated upon several basic issues raised by the new advances in quantum electronics since the invention of the laser, including “coherence and noise fluctuations in the electromagnetic field, with their relation to the uncertainty principle,” “information theory and noise,” and “two-photon absorption and nonlinear scattering processes, leading to a generalization of dispersion formulae, [and a] theory of interaction between light waves in nonlinear dielectrics” (in other words, the core of Bloembergen’s own path-breaking work in nonlinear optics). Bloembergen then swung for the fences, suggesting that DeWitt title the summer school “Quantum Electronics—Interaction of Electromagnetic Waves with Matter.” “It is true,” he qualified this suggestion somewhat, “that the theoretical problems are perhaps not as fundamental as the problems associated with elementary particles.” Yet nonlinear optics still had a legitimate claim to basicness, in Bloembergen’s opinion. It asked fundamental questions about the behavior of light and the structure of matter. It used intense radiation to probe previously unimagined physical processes, including optical harmonic generation, stimulated Raman and Brillouin scattering, and self-

¹³⁸ See Cecile DeWitt to Bloembergen, 17 October 1962, Box 1D, Folder “Misc. Correspondence, Pre-1963,” *NB*.

focusing.¹³⁹ With the aid of a crystal ball Bloembergen might have added that nonlinear optics was worthy of a Nobel Prize in Physics—his own, awarded in 1981.¹⁴⁰

During its formative years nonlinear optics was a deeply hybrid enterprise—perhaps more akin to a blend of unstable components. It drew heavily on optics and solid-state physics; it was a mix of classical electromagnetism, materials science, and quantum mechanics. Its experts, like Bloembergen, were often knee-deep in both the theoretical and experimental aspects of the labor. Pure *and* applied, nonlinear optics interlaced high technology with high science. Civilian *and* military, it grew and developed when and how it did with the encouragement of government money, corporate and academic laboratories, and an intimate circle of elite Cold War defense consultants with clearances to the world of classified problems and discussions.

For Paul Forman, the laser had been “the centerpiece and symbol of the largest, farthest-reaching scientific-technical initiative since the space program”—the culmination of twenty years of head-over-heels federal investment in physics and electronics.¹⁴¹ The origins of nonlinear optics confirm the laser’s status as a powerful scientific instrument, inspiring deep questions about the nature of light and the structure of matter. This is certainly how many commentators have sketched the history of the laser and the research fields that this technology helped to inspire.¹⁴² Bloembergen himself, in a lecture celebrating the fiftieth anniversary of nonlinear optics, put it even more bluntly: the “Maiman ruby laser *is* nonlinear optics.”¹⁴³

¹³⁹ Bloembergen to Cecile DeWitt, 22 October 1962, Box 1D, Folder “Misc. Correspondence, Pre-1963,” NB.

¹⁴⁰ Nobelprize.org, “The Nobel Prize in Physics 1981,” http://www.nobelprize.org/nobel_prizes/physics/laureates/1981/ (accessed 22 September 2013).

¹⁴¹ Forman, “Behind quantum electronics,” 149.

¹⁴² Hecht, “How the laser launched nonlinear optics”; Robert W. Boyd, “History of research in nonlinear optics at the Institute of Optics,” in Carlos R. Stroud, ed., *The Jewel in the Crown* (Rochester, NY: Meliora Press, 2004), 293–300, esp. 294.

¹⁴³ Taken from lecture slides, Nicolaas Bloembergen, “The birth of nonlinear optics,” paper delivered at the conference Nonlinear Optics: Materials, Fundamentals and Applications, Kauai, Hawaii, 17–22 July 2011. (The emphasis is mine.)

But it took much more than just a new gadget. The histories of interdisciplinary Cold War sciences like nonlinear optics call for careful attention to the unique social and institutional matrices in which these fields grew and evolved. In 1963 a handful of physicists, closely connected through their defense consulting activities, began studying the physics of stimulated scattering in an attempt to gain a firmer grasp on the terrifically complicated problem of making high-power laser beams for ballistic missile defense.

Nicolaas Bloembergen was perhaps in a unique position to bring together the diverse pieces of what is now regarded as the core of nonlinear optics. With his group at Harvard he performed the earliest quantum-mechanical calculations of harmonic generation in 1962. And through his numerous, interlocking connections to the advising community sponsored by ARPA and the Institute for Defense Analyses, as well as their defense-industrial partners, Bloembergen and his consultant colleagues were motivated to work intensely on stimulated scattering. By 1965 several major experimental results, and Bloembergen's unifying theoretical efforts, had laid the foundations of a new field. Nonlinear optics had not sprung merely from a new technology, but from new institutions and a unique social world—one concentrated on the most ambitious technical efforts to manage the problems of the nuclear age.

CHAPTER 3

Spiral to Oblivion: Deterrence, Defense, and the Arms Control Community in Private and Public

Dear Mr. Alderman, Must the Army have its Nike missile site on the Point? Perfectly futile, I believe, obsolete, and taking up space. Plenty of other sites in the city. Why not move this useless junk to some blighted area?

— Saul Bellow, *Herzog* (1964)¹

Introduction

In May of 1969 the physicist, government advisor, and nuclear arms control advocate Richard Garwin distributed a remarkable proposal to dozens of his colleagues. From his office at the IBM Watson Laboratory on Columbia University's Manhattan campus, Garwin described a system of what he called "emplaced weapons for assured destruction." "In the context of an agreement to limit strategic offensive and defensive forces," Garwin wrote, "a perhaps useful 'assured destruction' posture would have the US emplace the equivalent of Minuteman beneath 15 to 100 Soviet cities, while the Soviet Union does the same in the United States." He went on:

This Minuteman equivalent would consist of a 1 to 100 megaton nuclear weapon at the bottom of a small shaft perhaps 1000 feet deep, and provided with two small capsules at a similar depth, each containing two US military personnel. A one-megaton nuclear

Notes

¹ Saul Bellow, *Herzog* (New York: Penguin Books, 2003 [1964]), 75 (italics in the original).

explosion at this depth... would insure destruction of ordinary buildings and the death of their occupants.²

In other words: Bury beneath Moscow a multi-megaton H-bomb and arrange for an American military crew to live next to this weapon a mile underground. Should the crew receive an encrypted go-code from Washington, their assignment would be to turn the key, vaporizing themselves and causing the city above them to plunge into a Moscow-sized sinkhole. A similar arrangement would be made for Washington, and many other cities in each country. Garwin admitted there might be complications. An outside party could tamper with the communication system, sending false messages to the underground crew. And of course “the question may be asked whether crews can be found who will destroy themselves on command.” Garwin was confident they could be found, especially since the payoff was considerable: “The death of the crew,” he wrote, “would be accompanied by the destruction of 10^5 or 10^6 enemy personnel.” One assumes these “enemy personnel” would have been mostly civilians.

In a cover letter Garwin guessed that the inspiration behind his idea was “probably originally due to Leo Szilard.” His hunch was right. In 1961 Szilard had published a short story in the *Bulletin of the Atomic Scientists* titled “The Mined Cities,” in which a cancer patient wakes up in 1980 after eighteen years of medically induced sleep, and the doctor at his bedside recounts some astounding developments in international relations. All nuclear weapons have been destroyed, except those buried beneath a total of fifteen cities in each of the United States and the Soviet Union. “This mining of each other’s cities,” the patient asks, “does it provide us now with the foolproof second strike, which had been the dream of our arms control boys before I fell ill?” “Foolproof I would not say,” replies the doctor, “but almost foolproof, yes.” Like Garwin’s

² Richard L. Garwin, “Emplaced Weapons for Assured Destruction,” 20 May 1969, attached to Richard L. Garwin to Distribution, 26 May 1969, Box 45, Folder 1 “Antiballistic Missile (ABM), Testimony 1969; 1984-1987, 1 of 2,” *HFY*.

scheme, Szilard's had foreign personnel housed in "a little fortress" below each city, where, in peaceful isolation, they could safely blow everything up.³

Garwin's and Szilard's plans bore more than a passing resemblance to Herman Kahn's notorious Doomsday Machine, an imaginary device designed so that, upon sensing a few nuclear detonations, it destroyed the whole world. The idea was that by making the consequences utterly cataclysmic, even those few explosions would never go off. Kahn presented his Doomsday Machine in legendary rapid-fire briefings to military officials, in marathon lectures, and in his classic book *On Thermonuclear War*, published in 1960. The schemes of Kahn and Szilard were meant as didactic fables, showing the real meaning of nuclear deterrence in the thermonuclear age by reducing it to its madcap essence. The fictional form suited Szilard's combined purposes of lampooning deterrence and endorsing it as the only means of survival—permitting him, the *Bulletin* editor Eugene Rabinowitch wrote, "to be more enlightening by being more entertaining."⁴ Kahn, on the other hand, meant his Machine to demonstrate the inherent impracticability of what he termed "Type I" deterrence, deterrence of the all-out variety, in which there was a single gigantic step to full-scale nuclear use. "Except by some scientists and engineers who have overemphasized the single objective of maximizing the effectiveness of deterrence, the device is universally rejected," he wrote. This was a good thing as far as he was concerned. The problem with a Doomsday Machine was that it was insufficiently "*controllable*":

³ Leo Szilard, "The mined cities," *Bulletin of the Atomic Scientists* 17, no. 10 (1961): 407-412. The story is reprinted in Leo Szilard, *The Voice of the Dolphins: And Other Stories by Leo Szilard* (Stanford, CA: Stanford University Press, 1992 [1961]), 153-167. Barton Bernstein's introductory essay in that volume describes the provenance of Szilard's science fiction story. Barton J. Bernstein, "Introduction," in Szilard, *The Voice of the Dolphins*, 3-43.

⁴ Szilard, "The mined cities," 408. As Bernstein comments, "Szilard's scheme could fulfill a dual function: It could ridicule the deterrence system and, if implemented, shore it up." Bernstein, "Introduction," 42.

“Even though it maximizes the probability that deterrence will work...it is totally unsatisfactory.... There is no chance of human intervention, control, and final decision.”⁵

And that was where Richard Garwin parted company with Herman Kahn and Leo Szilard: Garwin was dead serious. None of the command and control problems presented by the system of “emplaced weapons” were significantly different from those affecting the actual force of Minuteman ICBMs, Garwin believed. Whether American weapons sat in silos in the American heartland, or at the bottom of shafts below Soviet cities, there was always some unavoidable risk of accidental or unauthorized detonation. Garwin sent his plan to just about everyone he could think of in the defense advising world, including all members of PSAC, the Jasons, the Defense Science Board, and a few higher-ups in the DOD research bureaucracy. “Depending upon the comments I receive from you,” he told them, “I will bring it formally to the attention of the government.”⁶ Not everyone was ready to sign up for the plan. Charles Townes waited until Garwin had distributed a second draft before replying, curtly, “The system is, of course, fail-unsafe...I doubt that populations of either country would voluntarily bare their throats and hand the other a knife in this way.” Townes had to admit that “we already have a

⁵ Herman Kahn, *On Thermonuclear War* (Princeton, NJ: Princeton University Press, 1960), 148 and 147, respectively (emphasis in original). On the recent 50th anniversary of Stanley Kubrick’s *Dr. Strangelove*, the *New Yorker* magazine film critic David Denby echoed a common view that Herman Kahn—one of several possible inspirations for the film character—believed his Doomsday Machine “would end the dangerous brinkmanship displayed by the Soviet Union and the United States in the Cuban missile crisis. He thought that it was a reasonable idea, even a clever one.” It is often assumed that Kahn invented the Doomsday scheme in order to recommend it. He did not. The Doomsday Machine was a critique of nuclear deterrence and the criterion of assured destruction. Even Kahn’s biographer Sharon Ghamari-Tabrizi mischaracterizes somewhat Kahn’s intentions with the Doomsday device. She writes that he “was astonished by the disgust it excited”—another example of Kahn’s fabled willingness to think the unthinkable. But the relevant passage in *On Thermonuclear War*, combined with Kahn’s strong preference for strategies of controlled escalation and damage limitation (including missile defense) and his strong dislike of “Type I” deterrence, suggest that he found disgust to be the appropriate reaction to the Doomsday Machine; it was probably his own. See David Denby, “The half-century anniversary of ‘Dr. Strangelove’,” *The New Yorker* (Culture Desk Blog), available online at <http://www.newyorker.com/online/blogs/culture/2014/05/kubrick-dr-strangelove-half-century-anniversary.html>; and Sharon Ghamari-Tabrizi, *The Worlds of Herman Kahn: The Intuitive Science of Thermonuclear War* (Cambridge, MA: Harvard University Press, 2005), 212.

⁶ Richard L. Garwin to Distribution, 26 May 1969, Box 45, Folder 1 “Antiballistic Missile (ABM), Testimony 1969; 1984-1987, 1 of 2,” *HFY*.

system which is almost as hazardous, and much more expensive.” And yet “I suspect most individuals would still choose it.”⁷

Garwin’s “emplaced weapons” were meant as an arms control measure. It is impossible to understand the proposal—or to comprehend how Garwin could have taken it so seriously—without viewing it as part of the impassioned arms control debates of the late 1960s. “As I see it,” Garwin explained, “this scheme of emplaced weapons performs the assured destruction role very well and at very low cost. It is not affected by the possibility of ABM nor of MIRVs, and is therefore quite stable.”⁸ ABM (Antiballistic Missile) and MIRV (Multiple Independently-targeted Reentry Vehicle) were the preeminent arms control issues of the day—two technological developments in the world of ballistic missiles, one defensive and the other offensive, that threatened to upend the apple cart of strategic deterrence. The arms control crowd was almost invariably opposed to them. When Garwin shopped his proposal around the first time, his less dismissive respondents urged him to frame the plan as a case for a treaty banning ABM and MIRV. The idea was that you wouldn’t have to *deliver* your weapons if they were already underneath your opponent’s cities. No need of missiles meant no need of the high-technological elaborations of them that had come along in the 1960s: missiles that could shoot down other missiles, missiles that could cleave and multiply mid-flight into yet more missiles. “Missile madness,” the government expert-turned-tireless arms controller Herbert Scoville called it in 1970.⁹

⁷ Charles Townes to Richard L. Garwin, 8 September 1969, Box 1, Folder 3 “Richard Garwin Information (1968-1976),” *RLG*.

⁸ Richard L. Garwin, “Emplaced Weapons for Assured Destruction,” 20 May 1969, attached to Richard L. Garwin to Distribution, 26 May 1969, Box 45, Folder 1 “Antiballistic Missile (ABM), Testimony 1969; 1984-1987, 1 of 2,” *HFY*.

⁹ In the second draft Garwin reduced the upper bound on the yield of the emplaced weapon by a factor of 10, from 100 to 10 megatons. He also pointed out that an ABM/MIRV ban would not only be an outcome but a prerequisite for the emplaced weapons scheme. In the absence of an ABM/MIRV ban, “one side might in fact build a very large ABM and a very large MIRV force while the other did neither.” Then the side with ABM and MIRV

Developed alongside an agreement prohibiting ABM and MIRV, Garwin said his system “might facilitate the difficult process of reducing...the present very high level of potential destruction”—the titanic arsenals each superpower had built up during the decade since Kahn and Szilard had hatched their bizarre plots. That was the point of Garwin’s vision of subterranean kamikazes with their fingers on the button: not to satirize the sick joke of nuclear deterrence, but to solve the nuclear arms race. It was a plan fit for its moment. Quixotic, manic, half-crazy and perversely reasonable, its aim was to decouple deterrence from the arms race, to make missiles obsolete. The physicist who wrote it was in the midst of a career shift—a longtime government scientist (Garwin had helped design the first thermonuclear bomb), he was building a reputation as a gadfly, now more likely to write a critique of U.S. nuclear weapons policy than a physics research article. So greatly had things changed that even though he was still a Presidential science advisor with high-level security clearances, Garwin hinted to his correspondents that he hoped to publish the emplaced weapons idea in the magazine *Foreign Affairs* before making the proposal formally within the government.¹⁰

* * * *

Among arms control issues, ABM has received considerable scholarly attention. For the first commentators, the missile defense debates and decisions of the late 1960s offered a tutorial on the irrationalities of the national security enterprise. The “rational actor” perspective assumed that the state was a coherent, unitary actor in the field of defense and foreign policy. But ABM—announced in a perplexing speech by Defense Secretary Robert McNamara in 1967, reformatted

could immediately hold the other at gunpoint, so to speak, and demand the removal of the emplaced weapons. The result would be total first-strike advantage to one side, maximum deterrence instability. Richard L. Garwin, “Emplaced Weapons for Strategic Deterrence,” Second Draft, 28 August 1969, Box 1, Folder 3 “Richard Garwin Information (1968-1976),” *RLG*. And for Herbert Scoville’s book, see Herbert Scoville and Robert Osborn, *Missile Madness* (New York: Houghton Mifflin, 1970).

¹⁰ Richard L. Garwin to Distribution, 26 May 1969, Box 45, Folder 1 “Antiballistic Missile (ABM), Testimony 1969; 1984-1987, 1 of 2,” *HFY*.

by the Nixon administration in 1969, deployed in only a single installation in North Dakota in 1975, operational for just a few months before being permanently shuttered—told a different story. Morton Halperin and Graham Allison said that ABM had been an exemplary case of “bureaucratic politics,” born not of the state’s maximizing its security, but of intergovernmental struggles between officeholders heeding selfish interests, and contractors heeding the bottom line.¹¹

For other scholars the ABM debate was an allegory about the changing compact between science and the U.S. government. Anne Cahn’s early study of the fraught relationship between “eggheads and warheads” sketched the borders dividing various classes and camps of scientists on the subject of ABM. Without naming many names, or digging too deeply into the arguments, Cahn conducted numerous interviews, counted how many times scientists testified before Congressional committees, documented how various professional societies handled the ABM issue, and distinguished between the more elite, better connected scientists—she called them the “inners”—from the less genteel “outers,” who had little or no experience with government work.¹²

Other narratives have cast the scientists in a firmly adversarial role. In Gregg Herken’s and Zuoyue Wang’s histories of the President’s Science Advisory Committee (PSAC), ABM is a moment of crisis in high-stakes science advising, one of two or three critical disputes that culminated in Nixon’s abolishing PSAC. For Herken, “the camaraderie and collegiality that

¹¹ Morton H. Halperin, “The decision to deploy the ABM: Bureaucratic and domestic politics in the Johnson administration,” *World Politics* 25, no. 1 (1972): 62-95; Graham Allison and Frederic Morris, “Armaments and arms control: Exploring the determinants of military weapons,” *Daedalus* 104, no. 3 (1975): 99-129. On the school of bureaucratic politics, see Graham T. Allison, *Essence of Decision: Explaining the Cuban Missile Crisis* (New York: Little Brown, 1971); Graham T. Allison and Morton H. Halperin, “Bureaucratic politics: A paradigm and some policy implications,” *World Politics* 24 (Spring 1972): 40-79.

¹² Anne Hessing Cahn, “Eggheads and Warheads: Scientists and the ABM,” Massachusetts Institute of Technology, Center for International Studies report no. 71-7 (1971); originally Anne Hessing Cahn, “Scientists and the ABM” (PhD dissertation, Massachusetts Institute of Technology, 1971).

distinguished early relations between PSAC and the Pentagon had largely disappeared” thanks to PSAC’s opposition to ABM. For Wang, “PSAC scientists’ technological skepticism [was] viewed as a form of dissent by the Nixon administration,” and so they were banished, an event that symbolized “a deterioration in the science-state relationship.”¹³ Much the same is true for Kelly Moore, who argues that the Vietnam War and the ABM issue produced a rupture between two different styles of political engagement by scientists: one elitist, liberal, and founded on principles of “information provision” to lay citizens; the other more radical and participatory, in the vein of New Left-style critiques of established power structures. Moore concurs that physicists “and other scientists were especially angered by the failure of the Congress to reject on technical grounds the antiballistic missile (ABM) system. The issue created a breach between scientists and the government,” and one could deal with that breach as a liberal or a radical.¹⁴

Indeed for many of the disputants at the time, it wasn’t just scientists as a faction but *science itself* that was opposed to missile defense—the cool light of reason and evidence pitted against the parochialisms of election-cycle politics. But it has been possible to see the technical claims in deeper context and with greater subtlety. Rebecca Slayton frames missile defense as a site where experts worked out disciplinary identities and wielded public authority. As they tried various arguments, physical scientists and computer experts applied unique “disciplinary repertoires” to the problems of missile defense. The more politically established physicists could criticize missile defense systems as transgressing basic physical laws; computer experts had to

¹³ Gregg Herken, *Cardinal Choices: Presidential Science Advising from the Atomic Bomb to SDI* (Stanford, CA: Stanford University Press, 2000), Zuoyue Wang, *In Sputnik’s Shadow: The President’s Science Advisory Committee and Cold War America* (New Brunswick, NJ: Rutgers University Press, 2008), 287. Both Herken and Wang agree that the complexity of today’s issues demand more Presidential science advising, a return to “the kind of informed, competent, and independent technological advice and skepticism that PSAC gave [Eisenhower] and his successors” (Wang’s words, on 318).

¹⁴ Kelly Moore, *Disrupting Science: Social Movements, American Scientists, and the Politics of the Military, 1945-1975* (Princeton, NJ: Princeton University Press, 2008), 132.

work harder, and wait longer, to gain a hearing for their arguments that missile defense involved information management problems of near impossible complexity.¹⁵

Of course, ABM opponents' arguments could also be remarkably eclectic, opportunistic, and highly dependent on the setting and the audience. Many did consider the pro-ABM case inadequate by scientific standards, and organized in groups claiming to speak with the authority of science. But critics were often heard making arguments in styles that did not emanate from any traditional discipline: strategic arguments, arguments based on arms race dynamics, arguments from the superiority of offense over defense, arguments about the risk of nuclear accident, and especially arguments drawn from practical work as contractors and consultants for the government. These were arguments tethered to personal careers and specific institutions, reinforced within the community of arms control experts, and measured and performed for specific audiences. Perhaps the best example is that of the best-remembered critique of ABM, a 1968 *Scientific American* article written by the physicists Hans Bethe and Richard Garwin. Their iconic article is often remembered as representing a sudden clash between "scientists" and "the state" in the late 1960s. But as I show in this chapter, Bethe and Garwin's criticism grew out of their years-long involvement as contract consultants on related problems—especially with the AVCO Corporation, a defense contractor working on the technologies of ballistic missile reentry and missile defense. It was that specific experience, rather than a simple desire to shine the light of pure physics on a murky defense issue, that led to their article.

At the deepest level, many of ABM's critics were troubled most by its perceived impact on the stability of deterrence and its role as an arms race accelerant. Lawrence Freedman summarizes the argument against ballistic missile defense in "two propositions: (a) it would not

¹⁵ Rebecca Slayton, *Arguments that Count: Physics, Computing, and Missile Defense, 1949-2012* (Cambridge, MA: The MIT Press, 2013).

work; and (b) if one side was so misguided as to try to build one, this effort would stimulate an arms race.”¹⁶ The idea that ABM wouldn’t work as advertised was a fine reason not to fund and deploy it, and when this argument suited the moment, it was made. But it bears emphasizing that for many of the critics, proposition (b) was the profounder and more disturbing of the two. In this sense, the possibility that missile defense, particularly the defense of urban populations, *would* work could be more troubling than the idea that it *would not*. An effective defense would upset the structure of deterrence that strategic analysts had settled on in the late 1950s, and pour fuel on the nuclear arms competition.

This was certainly the view of the arms controllers, the expert group most consistently and prominently arrayed against ABM. In this chapter I study these issues through the experiences of this particular community and its peculiar sensibility, rather than through the abstractions of scientific discipline or science writ large. Many of the arms controllers were scientists of one stripe or another. But the arms control debates of the late 1960s illustrate the degree to which the arms controllers had become a coherent social, intellectual, and political body in their own right.¹⁷ I try to show what ABM meant for the arms controllers as a community, revealed not only by their publications but at the level of their personal connections, their jobs and institutional involvements, their meetings and debates, and their conversations and correspondence.

¹⁶ Lawrence Freedman, *The Evolution of Nuclear Strategy, Third Edition* (New York: Palgrave MacMillan, 2003 [1981]), 239.

¹⁷ These debates also illustrate the inappropriateness of equating the arms control perspective with that of “the scientists.” It has been tempting to identify the arms controllers *en masse* as scientists and, often (given the elite status of mid-twentieth-century American physics), as physicists. The elite science advisors and arms controllers Jerome Wiesner and Jack Ruina, for example, were trained as electrical engineers. George Rathjens was trained as a chemist (he wrote his 1951 doctoral dissertation at Berkeley on the properties of cyclobutane) and, for the government, worked as an operations analyst. Paul Doty was a chemist, too, but he maintained a dual career, heading a large biochemistry laboratory at Harvard while jetting around the world on various arms control junkets. Jeremy Stone was a mathematician *manqué*, as was Donald Brennan, who started out as a fervent arms control advocate but decamped in the 1960s over the issue of ballistic missile defense. Others were jurists and economists and political scientists. And, of course, some impeccably accomplished physicists were *supporters* of ABM: Freeman Dyson, John Wheeler, Eugene Wigner, Edward Teller, and the inimitable Herman Kahn among them.

Watching this smaller group of people, we are led out onto a wider canvas. For the arms controllers missile defense was inseparable from the larger process of the arms race. The fear of an offense-defense interaction producing an uptick in the numbers and sophistication of nuclear weapons—the arms race “spiral”—had roots in some arms controllers’ long experience within the defense bureaucracy. As they worked for the government on offensive missiles and missile defense technology, many of them absorbed the lesson of offense dominance: the idea that the offense could always be designed to beat the defense, and more cheaply. These fears were made urgent by the Defense Department’s decisions in the late 1960s to deploy a missile defense system (ABM), and multiple warheads (MIRV) on its new Poseidon and Minuteman III missiles. Political expediency demanded that these issues be dealt with separately. Because the planned missile defense batteries were massive projects, slated (initially) for the outskirts of major American cities, ABM earned more attention and faced a more complex political opposition than did MIRV. But both issues came to a head almost simultaneously in 1969. Intellectually, the arms controllers came to understand ABM and MIRV as linked together by the same arms race machinery. “ABM and MIRV are thus inseparable,” the arms controller Herbert York said in 1970. “Each one requires and inspires the other.”¹⁸

But more than a quarrel over an esoteric defense policy issue, ABM and MIRV were at the center of a much broader transformation in the politics of arms control and national security expertise during the Cold War. In the early 1960s arms control debates had been mostly a polite and private affair, conducted inside the government, in privileged spaces like the Harvard-MIT arms control seminar, and in journals read by a handful of sophisticates. Disagreements over the purpose of arms control and the meaning of deterrence and defense had always been present. But

¹⁸ “Statement by Herbert F. York before the Subcommittee on Arms Control, International Law and Organization of the Senate Foreign Relations Committee,” 8 April 1970, Box 19, Folder “ABM #3,” *JBW*.

speeding ahead by a decade or so, it becomes clear that something had happened. The debates had grown fiercer, the differences more exaggerated. The forums for dispute had proliferated; arms control experts were drawn out into exposed settings and aggressive debate on the arcana of strategy and policy. They were routinely seen giving adversarial testimony before high-profile Congressional committees, writing reports for Congressional patrons, lecturing local residents in suburban churches and high school auditoriums, appearing in front of television cameras on the nightly news, and explaining the dynamics of the arms race in mass-circulation periodicals.

ABM and MIRV were therefore at least three things at once. They were arenas for expert disagreement over the nature of the nuclear arms race, the requirements of nuclear deterrence, and the prospects of defense. Two, they marked the moment when arms control became politically polarized—when camps divided over the issues and, to an unprecedented extent, along the line separating Republicans from Democrats. And three, ABM and MIRV were the portals through which nuclear security expertise went public. Arms control experts took on a unique intermediary role between the government and the wider public in the late 1960s, blending “the state” and “civil society.” They cut figures more complicated and contradictory than experts had in any previous episode in the nuclear age, identifying as both insiders and outsiders.¹⁹ They sat at an intersection between the government’s most secretive programs and the concerns and demands of citizens who struggled to make sense of ABM, MIRV, and the nightmarish complexities of nuclear policy.

¹⁹ The nuclear test ban debate of the late 1950s and early 60s, for example, is symbolized by the staged fights between the unrepentant nuclear enthusiast Edward Teller and fierce opponents like Linus Pauling and Barry Commoner. But in that episode there was no question about who was on the inside and who on the outside. And the particular issues at stake concerned the distribution and health risks of nuclear fallout; they did not call into question the legitimacy of the state’s policies for its own defense—the basic premises of nuclear deterrence—in the way the skirmishes over ABM and MIRV did in 1969. There is no question that the test ban debate was an enormous, complex, transformative episode. But the arms control fights of the late 1960s brought the state’s own experts into direct conflict with it, and refracted their disagreements out into public visibility, to a degree unseen during the test ban debate. On the earlier test ban debate, see, for example, Benjamin P. Greene, *Eisenhower, Science Advice, and the Nuclear Test Ban Debate, 1945-1963* (Stanford, CA: Stanford University Press, 2007).

I. Arms Control in Private, 1957–1965

Offense Dominance

Many of the U.S. experts who in the 1960s would come to criticize missile defense as an arms control issue approached the problem from a particular direction: the superiority of offensive over defensive nuclear forces. Since the late 1950s technical advisors had worried about how to improve U.S. defenses, and how to overcome a Soviet missile defense system with an offensive strike. For years they continued to study the physical and strategic dimensions of the interaction between offensive and defensive forces. Most of the prominent members of the arms control crowd in the 1960s had learned about the relationship between offensive and defensive weapons by working on both for the government. The lesson they took was clear: defensive systems would “work,” but only to a point; they could be overwhelmed and confused, and offensive missiles would invariably find their targets.

The Defense Department began studying these problems at the start of the missile age. In 1957 it created a “Reentry Body Identification Group” (RBIG) under its Guided Missiles Office, comprised of technicians from IDA, RAND, and the ballistic missile contractors TRW and Raytheon. The RBIG’s job was to advise the Air Force and Navy on how to deceive, destroy, and otherwise triumph over enemy missile defenses. Its 1958 report recommended techniques for confusing or blinding enemy radar systems. Termed “penetration aids,” these devices of misdirection included decoys mimicking the size and shape of a warhead reentry vehicle (but lacking the warhead); thin metallic wires that would reflect radar signals, obscuring the reentry vehicles from the radar’s sight; and high-altitude nuclear detonations that would strip the electrons from atoms in the upper atmosphere, blanketing huge regions of the sky with a layer of ionized gas that was impermeable to radar (a phenomenon called “radar blackout”). The group

also recommended the offensive strategy of saturation: by clustering several warheads together, but spread far enough apart that a single defense warhead could not kill them all in one blow, it was more likely that some of the warheads would find their way through.²⁰

Such techniques were meant to be effective countermeasures against both “midcourse” and “terminal” defense, referring to the phase of ballistic missile flight in which the missile was to be intercepted and destroyed. An ABM (Antiballistic Missile) system was designed to work by detecting and tracking incoming missiles with radar, then firing an interceptor missile and guiding it to the neighborhood of the offensive weapon. There, the interceptor would detonate a nuclear warhead, destroying the offensive reentry vehicle in one of several ways. The reentry vehicle might be incinerated in the fireball, if the interception were close. If interception occurred in the atmosphere, the reentry vehicle could be blown apart by the blast wave produced by the detonation. If it happened out in space, where there was no air to carry a blast wave, the detonation might “melt” the warhead’s interior with neutron or X-ray radiation. Terminal defense in the atmosphere, especially, was a last-ditch and dicey affair, entailing thermonuclear explosions perhaps thousands of feet above the base or city being defended.²¹

The upshot of the RBIG study, repeated time and again in future government reports, was that a sophisticated offensive missile attack would always beat a missile defense system. As Jerome Wiesner wrote in a 1959 PSAC memorandum on the Nike-Zeus program (the Army’s proposed missile defense system), “our own ICBM offensive plans presently include the use of

²⁰ The idea of decoys to defeat ballistic missile defense was actually proposed in the open literature as early as 1956. See T.R. Walkowicz, *Air Force* 39 (June 1956): 39-41. On the RBIG, see Fred Kaplan, *The Wizards of Armageddon* (Stanford, CA: Stanford University Press, 1991 [1983]), 343-345.

²¹ A declassified Livermore Laboratory history of the MIRV program dates the first proposal for penetration aids, “in particular decoys, and the advantage of fragmenting the last stage of the booster before reentry,” to 1955. It also suggests that engineers at Convair, the first industrial contractor for U.S. ballistic missiles, had looked into the possibility of outfitting the Atlas ICBM with several warheads and decoys as early as 1957. Daniel Buchonnet, “MIRV: A Brief History of Minuteman and Multiple Reentry Vehicles” (Lawrence Livermore Laboratory, February 1976), 28. See Document NH00840, *DNSA*.

much more sophisticated measures of confusion, multiple warheads and decoy than the Nike-Zeus can cope with in its present concept. We must reasonably expect that the Soviets will employ similar offensive tactics.”²² Wiesner’s hunches were confirmed when he received a study of the Nike-Zeus system from the Pentagon’s Weapons Systems Evaluation Group that autumn, report WSEG-45. WSEG-45 concluded that the goal of preserving the land-based ICBM force from Soviet attack was better served, money- and strategy-wise, by simply adding more ICBMs to the force, rather than using Nike-Zeus to protect them. It also illustrated, “in awesome quantitative detail” (according to the PSAC staffer who summarized the report for Wiesner), that Nike-Zeus would provide even less protection of city populations from radioactive fallout than an elaborate shelter program.²³

On a more hopeful note, Wiesner reported to President Eisenhower that year that research was forging ahead on methods to distinguish the real warheads from the decoys. Accomplishing that task—the problem of “discrimination”—would greatly simplify the defensive conundrum: you wouldn’t have to waste defensive warheads on the decoys if you could know with certainty where the real warheads were. Wiesner told Eisenhower that government scientists hoped to solve the discrimination problem by “examining the characteristics of the ionization cloud produced when the objects enter the atmosphere,” so that the warheads and decoys could be distinguished not by what they carried but by what they trailed behind them.²⁴ It would prove to

²² J.B. Wiesner, “Warning and Defense in the Missile Age,” 3 June 1959, Document NH01361, *DNISA*.

²³ H.J. Watters to Jerome B. Wiesner, 6 October 1959, Document NH01367, *DNISA*. The original report is WSEG Report No. 45, “Potential Contributions of Nike-Zeus to Defense of the U.S. Population and Its Industrial Base, and the U.S. Retaliatory System,” 23 September 1959, available online at the Office of the Secretary of Defense and Joint Staff FOIA Requester Service Center, http://www.dod.mil/pubs/foi/Science_and_Technology/WSEG/. Among the WSEG staff who added to this “awesome quantitative detail” was the young physicist Hugh Everett, III, famous for inventing the “many-worlds” interpretation of quantum mechanics while a graduate student at Princeton University a few years earlier.

²⁴ J.B. Wiesner, “Warning and Defense in the Missile Age,” 3 June 1959, Document NH01361, *DNISA*, on 8–9.

be one of the stickiest technical complications of missile defense; several future arms control critics first got their hands dirty working on this specific problem.

When a ballistic missile reentry vehicle plummets down from outer space, transitioning from midcourse to terminal phase around 200,000 feet above the ground, it begins to slice through earth's atmosphere. Extreme conditions are created by this encounter between the reentry vehicle and the atmosphere. Still on a ballistic trajectory in the vacuum of space, the reentry vehicle travels at speeds of several miles per second relative to the earth below. As the vehicle begins to scythe into the atmosphere, a thin "shock layer" forms at its surface, gliding over the reentry vehicle and mediating the contact (more like a collision) between the body and the air around it. In the shock layer the kinetic energy of the vehicle is converted to intense heat as the vehicle begins to slow down. The temperature of the air is elevated almost instantaneously to thousands of degrees, and a wake of superheated air expands conically behind the reentry vehicle. The molecules of oxygen and nitrogen making up the air are literally ripped apart under these conditions, losing some of their electrons as they dissociate, leaving a trail of ionized plasma. As the plasma begins to cool over a fraction of a second, its constituents recombine in a variety of chemical reactions, spontaneously reassembling into the ordinary molecules of air. These reactions are "chemiluminescent"—they emit ultraviolet, visible, and infrared light—so that the wake glows and buzzes with a distinctive electromagnetic signature. Even without seeing the missile or reentry vehicle itself, a tuned sensor could detect the telltale light signatures. And radar signals could be reflected from the wake plasma, where the ionized electrons re-radiate the radar signal back to its source, even without bouncing a radar pulse directly off of the reentry

vehicle. The characteristics of the wake (its shape, size, and composition) say something about the properties of the reentry vehicle that created it.²⁵

The physicist Hans Bethe knew all of this, and not by chance. Since 1955 Bethe had been consulting part-time with the AVCO Corporation, specifically with the AVCO-Everett Research Laboratories (AERL) established in the Boston suburbs that year. The founding director of AERL was Arthur Kantrowitz, who had been a professor of aeronautical engineering at Cornell, where Bethe also taught. Soon Kantrowitz had recruited Bethe to apply his legendary calculating prowess to AVCO's peculiar tasks: the firm was one of a handful doing R&D on ballistic missile reentry vehicles. At AERL in the mid-1950s Kantrowitz pioneered the use of "shock tubes" to mimic the extreme conditions of atmospheric reentry. One of Bethe's first major jobs for the company was a calculation of how quickly the quartz (which AVCO was testing as a protective material for missile nose cones) would evaporate from the surface during reentry. Another study, in 1957, focused on the "structure of magnetohydrodynamic shock waves"—a project funded by the Western Development Division of the Air Force, the people in charge of building the first American ballistic missile. This resulted in a paper on the spectrum of nitric oxide (a gas typically found in reentry wakes) titled, perfectly, "Radiation from Hot Air."²⁶

²⁵ I have drawn some of the technical details of ballistic missile reentry from Herschel Weil, "Radar Echo from Re-Entry Vehicles," RAND Memorandum RM-3251-PR (May 1963); R.O. Hundley, "Air Radiation from Nonequilibrium Wakes of Blunt Hypersonic Reentry Vehicles," RAND Memorandum RM-4071-ARPA (June 1964); and S.S. Penner, *Radiation and Reentry* (New York: Academic Press, 1968). The "Lewis-Rayleigh nitrogen afterglow," for example, is the name physical chemists give to the distinctive band-pattern in the visible and infrared region produced by the recombination of nitrogen atoms to form molecular nitrogen.

²⁶ See Interview of Arthur Kantrowitz by Stuart Leslie, 12 June 2006, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD, <http://www.aip.org/history/ohilist/31816.html>. Kantrowitz described the design and operation of a shock tube as follows: "[I]t's just a long, straight tube. You put a diaphragm in the middle, and high-pressure, say, hydrogen, on one side. You build up [the] pressure of the hydrogen until it bursts the diaphragm, and then a shockwave goes down. Of course, it's very turbulent for a while, but the shockwave outraces the turbulence and leaves a very homogeneous gas sample whose conditions are completely determined by the conditions that you had in the tube to start with, and the velocity of the shockwave, which is easy enough to measure.... You could go to temperatures up to maybe as much as a million degrees, depending on how you drove that shockwave. To get to high temperatures, you would drive it with an electromagnetic field. But temperatures in the range of tens of thousands [of degrees] you could reach with just hydrogen on one side and air

AVCO liked Bethe's work so much that they wanted to put him on retainer during the winter of 1958, and even agreed to fund a visiting professorship in Cornell's physics department during Bethe's extended absence.²⁷ Month after month during the late 1950s and early 1960s Bethe traveled around the Northeast visiting various AVCO facilities—sometimes ferried in an AVCO company aircraft that would make a special trip to Ithaca to pick him up.²⁸ He became a talent scout of sorts for the company, helping to recruit top-shelf scientists, including the physicist Richard Garwin, whom Bethe had known from various government advisory groups. Garwin added work on missile reentry for AVCO to his long list of defense consulting gigs (in addition to his permanent position as a staff scientist at IBM).²⁹

Research on the problems of offense and defense went hand in hand and, before long, AVCO was doing overlapping studies of missile reentry and missile defense. Edwin Salpeter—one of Bethe's Cornell physics colleagues and another of Kantrowitz's AVCO recruits—explained that warhead/decoy discrimination—"the unmasking of the decoy problem"—was AVCO's going concern in this period. "For long time that was really the major thing I was involved in," he said.³⁰ (And of course figuring out how to unmask a decoy might suggest ways to make one's own decoys more convincing.) The trick, the consultants hoped, was that even though decoys and warheads looked almost identical to radar, their upper-atmospheric wakes would not. In 1962, when IDA's Jason Division devoted its summer study to "topics related to

on the other." The paper was refereed for the journal *Annals of Physics* by none other than Bernard Feld. Bennett Kivel to H.A. Bethe, 18 April 1957, Box 38, Folder 26 "AVCO, misc. correspondence (1958-1960)," *HAB*. The paper was published as B. Kivel, H. Mayer, and H. Bethe, "Radiation from hot air: Part I, theory of nitric oxide absorption," *Annals of Physics* 2, no. 1 (1957): 57-80.

²⁷ K.R. Wilson, Jr. to Dale R. Corson, 9 December 1957, Box 38, Folder 26 "AVCO, misc. correspondence (1958-1960)," *HAB*.

²⁸ See, for example, Clara Nardi to Hans A. Bethe, 24 March 1958, Box 38, Folder 26 "AVCO, misc. correspondence (1958-1960)," *HAB*.

²⁹ Interview of Richard Garwin by Finn Aaserud, 4 February 1987, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD, http://www.aip.org/history/ohilist/4622_2.html (hereafter cited as Garwin interview, 1987).

³⁰ Interview of Dr. Edwin E. Salpeter by Spencer R. Weart, 30 March 1978, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD, <http://www.aip.org/history/ohilist/4854.html>.

missile penetration,” Salpeter and Garwin (both of whom had also become Jason members) brought their AVCO experience to bear on the problem.³¹ That summer Salpeter wrote two important papers on radar backscatter from turbulent wakes with the Princeton theorist Sam Treiman. Bethe picked up where this work left off in writing his own quick study of radar discrimination in 1962—a paper deemed “important enough” to warrant rapid publication as a classified AVCO Research Note, widely read and discussed at the lab.³² By 1963 AVCO had created a new “Re-entry Experiments Operation,” “primarily motivated by the national need in the area of ballistic missile offensive and defensive strategy,” as an internal bulletin put it. “The specific problems are largely involved with luminous and ionized trails in the upper atmosphere.”³³

Wake discrimination, it turned out, was harder than Jerome Wiesner had hinted to Eisenhower back in 1959. Discrimination would be a job for the atmosphere itself, not for radar. During reentry, the decoys, which were much lighter, would get slowed down (or burnt up) and effectively “filtered out,” while the real warheads would continue to streak down at thousands of miles per hour.³⁴ All of this was yet more bad news for the defense, because the real warheads after filtering were now that much closer to their targets at interception. The Army’s new missile

³¹ See Attachment 4, Inventory – Keith A. Brueckner – Classified File, Box 21, Folder 7 “Consulting Summary, 1953-1973,” *KAB*.

³² G.B. Mayfield to Hans Bethe, 16 August 1962, Box 38, Folder 26 “AVCO, misc. correspondence (1958-1960),” *HAB*. For a later draft of one of Salpeter’s papers, see E.E. Salpeter, “Radar Backscatter from a Model Wake,” January 1963, in Box 67, Folder 16 “AVCO-Wilmington - EM Scatter from Wake (1962-63),” *HAB*. For Bethe’s extensive calculations, also see Box 67, Folder 17, “AVCO-Wilmington - EM Scatter from Wake (1962-63),” *HAB*.

³³ Arnold Goldburg to H.A. Bethe, 21 May 1963, Box 38, Folder 26 “AVCO, misc. correspondence (1958-1960),” *HAB*.

³⁴ The decoys could be made heavier, to mimic the reentry characteristics of a real warhead. But decoys weren’t solely meant to act as fake warheads. They were intended to confuse the defensive radars while also lightening the payload of the offensive missile that carried them. If the weight of the decoy were increased beyond a certain point, it no longer paid (in terms of the tradeoff between payload weight and damage inflicted) for the decoy to be a decoy—it might just as well carry an actual nuclear warhead. Thus decoys would, inevitably, be lighter than the real warheads—and so their flight characteristics during reentry would be distinguishable from them. On this point see George W. Rathjens, “The dynamics of the arms race,” *Scientific American* 220, no. 4 (1969): 15-25, on 18.

defense concept, dubbed Nike-X, proposed to use two kinds of interceptor missiles (instead of Nike-Zeus's one). The first, eventually called the "Spartan," would destroy warheads while they were still above the atmosphere; the second, a high-acceleration missile known as the "Sprint," would hit warheads that had sneaked through and already started reentry. When representatives from Bell Telephone Laboratories (the main contractor for the Nike system) visited AVCO to talk about their plans, AVCO's management encouraged Bethe to meet with them and explain his "new radar theory." He gladly obliged, and was even moved to give the visitors from Bell Labs a special presentation on radar blackout, telling them how large regions of the sky would be left opaque to radar by high-altitude nuclear detonations, which would temporarily drape a sheet of electrons above the atmosphere. Blackout was a nifty technique for the offense but another quandary for the defense.³⁵

The emerging message was consistent: defense might work, but never perfectly; a reentry vehicle would always get through. Studies poured forth on the topic. The President's Science Advisory Committee (PSAC), under its new chairman Jerome Wiesner, formed an Ad Hoc Panel on Warhead Vulnerability and issued a report in June 1961. In August the Office of the Director of Defense Research and Engineering (DDR&E) distributed a "Missile Penetration Study," which explored the question of how to design offensive forces to execute a variety of attack scenarios in an environment involving active missile defense.³⁶ ARPA and IDA worked continuously on these issues as well. By May of 1964 the ARPA Ballistic Missile Defense Advisory Committee, directed by the Jason physicist Kenneth Watson, had started its so-called

³⁵ Blackout is especially problematic for the intercept of warheads above the atmosphere (if you can't see the warhead in space, you can't fire an interceptor at it). Margaret Kaepfle to Hans Bethe, 18 October 1963, Box 38, Folder 26 "AVCO, misc. correspondence (1958-1960)," *HAB*; Hans A. Bethe to Bennett Kivel, 7 November 1963, Box 38, Folder 26 "AVCO, misc. correspondence (1958-1960)," *HAB*.

³⁶ Daniel Buchonnet, "MIRV: A Brief History of Minuteman and Multiple Reentry Vehicles" (Lawrence Livermore Laboratory, February 1976), Document NH00840, *DNSA*, on 33.

“Pen-X” study—a “mammoth” effort (according to a declassified history of ARPA), which “examined the whole penetration problem in detail and proved to be one of the most influential studies conducted under the Defender program,” ARPA’s large missile defense R&D project. The punchline of the Pen-X study, released more than a year later, was that multiple-warhead systems (with smaller explosive yield per warhead), even without decoys, were more efficient at besting the enemy’s defenses and destroying targets than were single-warhead systems aided by decoys.³⁷

And so another emerging concept began to gather support as it was discussed and studied throughout the defense research bureaucracy: the MIRV (Multiple Independently-targetable Reentry Vehicle). A few of the strategy intellectuals had recognized the benefits of MIRV as early as the *Daedalus* arms control conference in 1960. During group discussion of Jerome Wiesner’s paper on comprehensive arms control systems, both Henry Rowen and Albert Wohlstetter argued that multiple warheads would significantly adjust the exchange ratio in the attacker’s favor (whereas Wiesner at that time had calculated it in favor of the defender).³⁸ According to Ted Greenwood’s authoritative study of the MIRV, the idea for multiple warhead systems in which each warhead would be *separately* targeted (rather than clustering around a single aim-point, as with an earlier technology known as the “Multiple Reentry Vehicle,” or MRV) was developed “almost simultaneously” by at least five different parties in 1962 and 1963. These included scientists at the Aerospace Corporation, the Office of the DDR&E, and RAND. As it would develop over the next couple of years, the critical design concept was that of

³⁷ Richard J. Barber Associates, Inc., *The Advanced Research Projects Agency, 1958-1974*, report prepared for the Advanced Research Projects Agency, Dec. 1975, DTIC no. ADA154363 (Washington, DC), VI-15. In the words of Charles Herzfeld, ARPA’s director at the time: “Pen-X proved that, for all practical purposes, multiple warheads are better than decoys [and] that’s why MIRVs are around, fellas.” Barber Associates, *Advanced Research Projects Agency*, VII-9.

³⁸ Rowen also mentioned the idea of multiple warheads in correspondence with Wiesner following the conference.

a maneuverable “bus” (also known as the “post-boost control vehicle”) carrying several reentry vehicles. In midcourse, the bus would release a vehicle, adjust its course slightly before releasing another, and so on, so that each warhead would make its way to a new target.³⁹

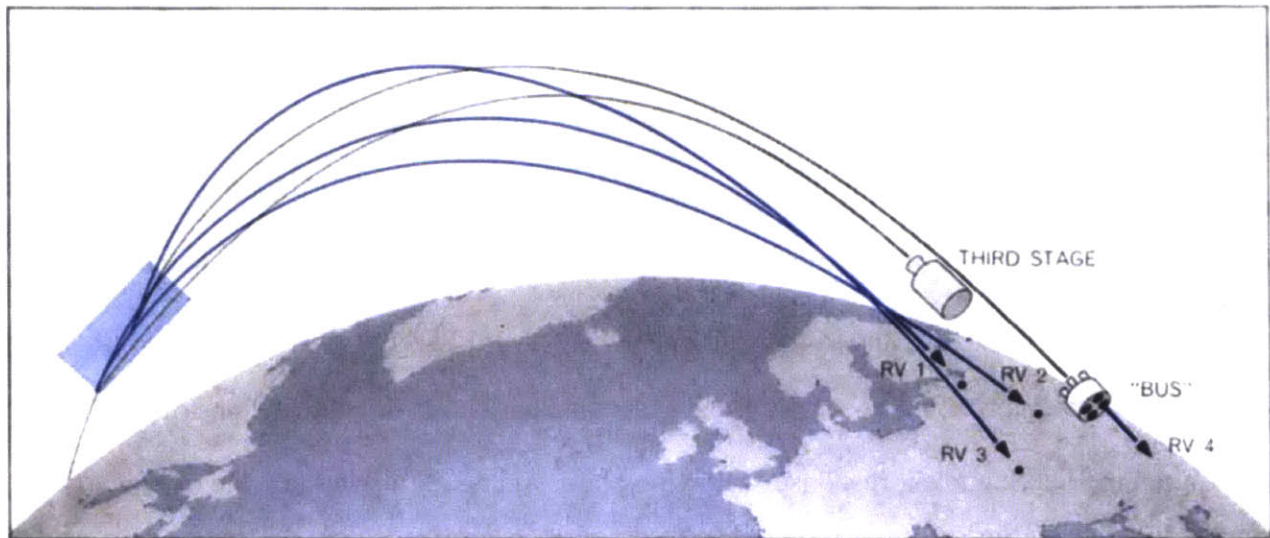


Figure 3.1: The MIRV concept. Multiple reentry vehicles (RVs) separate in midcourse from the “bus” and proceed to independent targets. Taken from George W. Rathjens, “The dynamics of the arms race,” *Scientific American* 220, no. 4 (1969): 15-25.

The justifications for MIRV were many. And they changed over time. The conventional wisdom was that MIRV had been necessary to overcome Soviet missile defense, which intelligence sources in the early said was being built around Moscow and might be extended nationwide. Defense Secretary Robert McNamara said this repeatedly himself. In the eyes of the Air Force, MIRV also came to serve the salutary purpose of increasing the number of targets (the “target coverage”) that a given number of missiles could hit—an important feature at a time when some had started to demand that a leash be put on the size of U.S. missile forces. If the numbers of missiles would be restricted (without a specific restriction on numbers of warheads),

³⁹ Ted Greenwood, *Making the MIRV: A Study of Defense Decision Making* (Cambridge, MA: Ballinger, 1975), 28. Also see Herbert York, “The Origins of MIRV,” SIPRI Research Report No. 9 (August 1973).

the best way to guarantee target coverage was to add more warheads to each missile.⁴⁰ The plasticity of MIRV's mission only helped to dissipate early criticisms; and in any case, for the arms control-minded, MIRV was one more fine demonstration of the technological supremacy of offensive power.

* * * *

Between the Eisenhower and Johnson administrations the U.S. arsenal of nuclear weapons underwent a startling expansion. From 1955 to 1967 roughly 30,000 warheads were added to the U.S. stockpile, 11,000 of which were produced in a two-year window alone between 1958 and 1960.⁴¹ In 1960 the first unified U.S. nuclear war plan—the so-called “Single Integrated Operational Plan,” or SIOP-62—revealed the astounding dimensions of the programmed U.S. nuclear response. Allowing almost no flexibility in the scale or type of nuclear assault, the war plan prescribed laying a total of 3,500 nuclear weapons on 1,050 “designated ground zeros” throughout the entire “Sino-Soviet bloc,” among them 151 separate urban-industrial targets. As work on the first SIOP proceeded at the headquarters of the Strategic Air Command in Omaha, George Kistiakowsky of PSAC made a visit to report on the proceedings to President Eisenhower. He described the emerging plans as “overkill.” Two members of Kistiakowsky's staff who joined him on the visit, the analysts George Rathjens and Herbert Scoville, Jr., were equally horrified. Rathjens later said he was “taken aback” by the grim labor being done at SAC.⁴²

⁴⁰ See, for example, Greenwood, *Making the MIRV*, 77 and 102; Gregg Herken, *Counsels of War* (New York: Alfred A. Knopf, 1985), 200-203.

⁴¹ Thomas B. Cochran, William M. Arkin, and Milton M. Hoenig, *Nuclear Weapons Databook, Volume I: U.S. Nuclear Forces and Capabilities* (Cambridge, MA: Ballinger, 1984), 12.

⁴² David Alan Rosenberg, “The origins of overkill: Nuclear weapons and American strategy, 1945-1960,” *International Security* 7, no. 4 (1983): 3-71; Scott D. Sagan, “SIOP-62: The nuclear war plan briefing to President Kennedy,” *International Security* 12, no. 1 (1986): 22-51. In an interview in 1986, Rathjens recounted one particularly grotesque example of planned overkill: “In a sense it was a rational operation. They specified the levels of damage that had to be inflicted on each target to the extent they could. And then they would, through a computer

Warheads alone do not tell the whole story, for warheads must be delivered to their targets. When Kennedy entered office, only a dozen intercontinental ballistic missiles were operational; but this was about to change. The Minuteman, soon to become the primary land-based ICBM in the U.S. arsenal, had already been in development for several years and would soon come online. But how many would the United States need? The 1961 Minuteman procurement decision would become a flash point of disagreement over the requirements of deterrence and the real meaning of offense dominance. At the middle of the debate was a Cambridge arms controller who had just joined the White House as Kennedy's new science advisor: Jerome Wiesner.

In early 1960, Wiesner's work with Donald Brennan and Paul Doty had concluded that a deployment of 200 "large weapons" would be sufficient for deterrence, including 100 fixed, hardened, in-ground ICBMs. In a late 1960 meeting of the Harvard-MIT arms control seminar, after Henry Rowen had presented a case for a deep retaliatory arsenal to carry out both counter-city and counterforce missions, Wiesner brushed this idea aside. "We have a mighty deterrent now," he said, which "can be maintained by fairly straightforward technological efforts during the next few years."⁴³ Jump ahead a few months, when the Air Force submitted its strategic forces proposal to Defense Secretary Robert McNamara, asking for more than 3,000 Air Force ICBMs, of which 2,500 would be Minuteman missiles. McNamara's own proposal was slightly

code, allocate weapons to these targets to achieve a desired expectation of destruction. Now the thing that has stuck in my mind—two things stuck in my mind most readily about that whole exercise. When I was through...I had some time left over. And so I decided...to satisfy my own curiosity, it had seemed to me that they were attacking these targets very heavily. And so I asked for an atlas. And I looked through the atlas and I picked a city in the Soviet Union that I thought most resembled Hiroshima. Just to see how heavily they would attack that target compared with what we had done in 1945. And I must say, I was a bit taken aback. I had discovered that particular city had four weapons allocated to it. One of them, as I recollect, was four and a half megatons. Three hundred times the size of the Hiroshima weapon. And I believe it had three back-up weapons, each of over a megaton." WGBH Open Vault, "Interview with George Rathjens, 1986," available at <http://openvault.wgbh.org/catalog/wpna-dc15a2-interview-with-george-rathjens-1986>.

⁴³ "Joint Arms Control Seminar, Third Meeting, Abstract of Discussion," 7 November 1960, Box 2, Folder 48 "Joint Harvard-MIT Arms Control Seminar - Minutes 1960-61," *LPB*.

more modest, in the neighborhood of 1,000 Minutemen, a number arrived at by RAND-trained civilians—the “Whiz Kids”—in the Pentagon’s new Office of Systems Analysis.⁴⁴

Wiesner, disturbed by this proposed buildup, prepared for a sparring match. His main ally in the White House was Carl Kaysen, a fellow Cambridge arms control expert who had answered the call to the Kennedy administration, becoming deputy to national security advisor McGeorge Bundy. Wiesner commissioned an independent study in his own office, led by Spurgeon Keeny and the operations researcher Vincent McRae. The study produced a graph of “strategic effectiveness” vs. missile numbers (not unlike the graph Brennan had produced for Wiesner the year before, but incorporating a broader conception of damage than simple numbers of missiles killed in their silos). The question was where this graph flattened off—where it no longer paid in terms of damage inflicted to add missiles to the arsenal. Wiesner’s team decided that the “flat of the curve” lay somewhere around 450 missiles, nowhere near McNamara’s golden mean of 1,000. Wiesner and Kaysen wrote a memorandum for Kennedy based on the graph, proposing a Minuteman force of 600 missiles. Then they locked horns with the Pentagon systems analysts, including the DOD comptroller Charles Hitch, Alain Enthoven, and Henry Rowen, who had left Cambridge like Wiesner and Kaysen. Rowen was now deputy to the storied Cold War hawk Paul Nitze—principal author of such fundamental Cold War policy documents as NSC-68 in 1950 and most of the Gaither Panel report in 1957—who joined the Kennedy administration as the new Assistant Secretary of Defense for International Security Affairs. Wiesner discussed his revised figure in at least two meetings with McNamara and the President. But McNamara, who felt that proposing a smaller number to Congress would be political suicide, held fast to the

⁴⁴ The Air Force request for Minutemen apparently went as high as 10,000 before 1964. See Desmond Ball, *Politics and Force Levels: The Strategic Missile Program of the Kennedy Administration* (Berkeley, CA: The University of California Press, 1980), 69-70.

original number. By 1967 exactly 1,000 Minuteman missiles had been deployed in sprawling missile fields from Montana to Missouri.⁴⁵

Wiesner's wrestling with the ICBM was complicated. Large patches of his career as a government advisor make for a strange pairing with his arms control commitment. He was, in a word, one of the "missilemen."⁴⁶ He had been part of all the important high-level groups that had recommended rapid and large-scale missile development in the 1950s, from the 1954 Strategic Missiles Evaluation Committee (the famous "von Neumann committee") to the Gaither Panel in 1957, to a special Air Force group reevaluating ballistic missile program management in 1959.⁴⁷ This last job led, in 1960, to Wiesner's joining the board of trustees of the new Aerospace Corporation, a nonprofit outfit created "to aid the United States Air Force in applying the full resources of modern science and technology to the problem of achieving...continuing advances in ballistic missiles..."⁴⁸ The board included familiar faces among the power elite of defense

⁴⁵ Ball, *Politics and Force Levels*, esp. 85-87; also see Gretchen Heefner, *The Missile Next Door: The Minuteman in the American Heartland* (Cambridge, MA: Harvard University Press, 2012). A parallel effort to Wiesner's PSAC study was the so-called Foster Panel, chaired by William C. Foster (who had led the U.S. delegation to the 1958 Surprise Attack Conference and, later in 1961, would become the first director of the Arms Control and Disarmament Agency). Assuming a limit of 500 total delivery vehicles, the Foster Panel determined an optimum mix of 200 Polaris submarines, 150 Minuteman missiles, and 150 bombers. But it also guessed that a limit of 1,000 delivery vehicles was a more reasonable starting point for an arms limitation agreement (quite a contrast with the 2,400 vehicles under discussion during the 1970s during the SALT negotiations, 1,300 of which were to be MIRVed with multiple warheads). See Betty Goetz Lall, "Mutual deterrence: The need for a new definition," *Bulletin of the Atomic Scientists* 33, no. 10 (1977): 10-11. On Nitze, see Nicholas Thompson, *The Hawk and the Dove: Paul Nitze, George Kennan, and the History of the Cold War* (New York: Henry Holt and Co., 2009).

⁴⁶ The "missilemen," a new breed of aerospace engineer-bureaucrat, were identified in a *Time* magazine article from 1957. "The bird and the watcher," *Time* 69, no. 13 (April 1957): 18. Also see Heefner, *The Missile Next Door*, 22.

⁴⁷ James Douglas to Jerome Wiesner, 3 September 1959, Box 5, Folder 158 "Ballistic Missile Panel, 1959," *JBW*.

⁴⁸ "Mission of Aerospace Corporation," Box 7, Folder 213 "Aerospace Corp.," *JBW*. Aerospace was created to absorb some contractual responsibilities from Space Technology Laboratories, Inc. (STL), a division of the ballistic missile contractor Thompson Ramo Woolridge (TRW). STL had originally performed the systems engineering work for the Air Force ballistic missile program. The House Committee on Government Operations mandated the creation of the not-for-profit Aerospace Corporation in 1960, when several of TRW's competitors complained that STL's position gave TRW an unfair advantage in contract competition. At Aerospace's first board meeting in New York, the topic of conflict of interest came up early on. According to the minutes, "Wiesner, McCormack and Gardner concluded that they could not pledge or assure that the individuals in enterprises with which they are associated would not 'presently expect to seek business' with 'the Ballistic Missile Division complex.'" For Wiesner, there was no washing his hands completely of ballistic missiles. See "Air Force Ballistic

advising and management: Roswell Gilpatric, Trevor Gardner, Chalmers W. Sherwin, Charles Lauritsen, and a few others. Among Aerospace's first jobs was a comprehensive technical review of the Minuteman project. After the 1960 election, when Wiesner served on a task force advising Kennedy's transition team, his report on the U.S. space program went out of its way to advance the Air Force/Aerospace line. "The nation's ballistic missile program is lagging. The development of missiles and of the associated control systems, the base construction and missile procurement must all be accelerated if we are to have the secure missile deterrent force soon that the country has been led to expect."⁴⁹ Wiesner embraced and battled ICBMs with an ambivalence bordering on double consciousness. "I was a piece of the machine," he reflected many years later.⁵⁰

Like Wiesner, the physicist Herbert York embodied simultaneous allegiances to U.S. nuclear power and to arms control. York was a pioneering defense research manager: the first director of Lawrence Livermore Laboratory, the first chief scientist of ARPA, and the first Director of Defense Research and Engineering. He had served with Wiesner on the von Neumann Committee in 1954, and the Minuteman missile program was shepherded, in part, under York's watch as DDR&E. He was no dove—something he was quick to point out to conservative critics during skirmishes in later years. In 1959 and 1960 he had "personally prepared and conducted extensive arguments and briefings...to advance the deployment of

Missile Management (Formation of Aerospace Corporation)," Third Report by the Committee on Government Operations (Washington, DC: U.S. Government Printing Office, 1961).

⁴⁹ Joseph V. Charyk to Roswell Gilpatric, 24 May 1960, Box 7, Folder 213 "Aerospace Corp.," *JBW*; "Exhibit B, Statement of Work," Box 7, Folder 213 "Aerospace Corp.," *JBW*; *New Frontiers of the Kennedy Administration: The Texts of the Task Force Reports Prepared for the President* (Washington, DC: Public Affairs Press, 1961), 3.

⁵⁰ Wiesner described for an interviewer how it was he had come to accept the inflated intelligence estimates of Soviet military capabilities in the late 1950s: "I couldn't understand how the Soviet Union had been so thoroughly devastated in World War II and could be turning out hundreds of bombers when it took such a big job for us to do it. But somehow it never quite crystallized and I was in no position to ask the questions to get the answers. I was a piece of the machine and I was, ah, a pretty good technical expert...so I was invited in on all these new things and...I was very deeply involved initially in the technology." See WGBH Open Vault, "Interview with Jerome Wiesner, 1986 [1]," available at <http://openvault.wgbh.org/catalog/wpna-9e6621-interview-with-jerome-wiesner-1986-1>.

Polaris,” speeding up the submarine-launched ballistic missile program. When the Kennedy administration proposed to cancel two squadrons of Titan ICBMs in 1961, York forcefully opposed the cutback “on the grounds that penetration aids and multiple warheads were new ideas just then being seriously considered for the first time and,” he said, “I believed that the greater weight-carrying capacity of the Titan could be of great importance in that connection.”⁵¹

As York and Wiesner each departed full-time government work, they began to cultivate a new public image. In 1964 the two coauthored a widely noted *Scientific American* article, arguably the first major public statement on the strategic arms race by current or recent government officials. They made a special point of playing up their insider status, their long acquaintance with the technical details of nuclear forces. The stimulus for the article were demands by some military officials for renewed atmospheric nuclear testing, as the Senate debated ratification of the Limited Test Ban Treaty (which had been signed by the U.S., Soviet Union, and Great Britain in August 1963). Wiesner and York argued that there was little that continued nuclear tests could add to improved offenses. Improvements in missile accuracy and increases in the numbers fired during an attack were far more important, they said. The authors broke new ground by building their arguments from the stuff of ICBM ballistics and weapons effects: yield-to-weight ratio, circular error probable (a figure that had become the standard measure of a missile’s accuracy), reliability, single-shot kill probability, and so forth. Such details were familiar to the classified community and the arms control sophisticates; but it was an

⁵¹ York brought up these points in a dispute with Defense Secretary Melvin Laird in 1969. See Herbert York to Melvin Laird, 25 March 1969, Box 44, Folder 7 “Antiballistic Missile (ABM), Correspondence, 1969,” *HFY*. Also see, for example, Herbert F. York, “Memorandum for the Chairman, Air Force Ballistic Missile Committee, Subject: The Minuteman Weapon System Program,” Document NH00680, *DNSA*. When York left the Pentagon in 1961 he became the first chancellor of the University of California at San Diego, but continued to serve as a frequent government advisor over the next two decades. William Grimes, “Herbert York, 87, top nuclear physicist who was arms control advocate, dies,” *New York Times* (24 May 2009): A12; Interview of Dr. Herbert York by A.B. Christman, 24 September 1980, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD, <http://www.aip.org/history/ohilist/4989.html>.

unprecedented discussion for a periodical like *Scientific American*. On the question of antimissile defenses, the authors employed an argument familiar from the RBIG and the Pen-X study: offense would always win out over defense. And yet missile defense research had to be done, they claimed. “It not only serves the forlorn hope of developing an active antimissile defense but also promotes the continued development of offensive weapons. The practical fact is that work on defensive systems turns out to be the best way to promote invention of penetration aids that nullify them.”⁵²

The article had begun as a fairly technical rundown of the case against the need for renewed nuclear testing. But toward the end it presented, in strong moral terms, something like a philosophy of the nuclear arms race. York and Wiesner cautioned against the use of science and technology in place of politics to “solve” the problem of national security, the logic of which “leads to a diverging series of ever more grotesque measures.” Their warning was severe: “The clearly predictable course of the arms race is a steady open spiral downward into oblivion.”⁵³ This was a formulation with legs. *Scientific American* editor Gerard Piel planted a *New York Times* article about the piece, in which it was pointed out that Wiesner and York “speak from a position of authority” as government advisors “intimately involved in weapons development programs.” A separate editorial appeared in the *Times* the next day under the splashy title “Spiral to Oblivion.”⁵⁴ Piel sent Wiesner a message in November congratulating him on the article’s wide notice. In light of the arms controllers’ hard-won belief in offense dominance, Piel’s choice

⁵² Jerome Wiesner and Herbert York, “National security and the nuclear-test ban,” *Scientific American* 211, no. 4 (1964): 27-35. Quotation on 34.

⁵³ Wiesner and York, “National security and the nuclear-test ban,” 35.

⁵⁴ “2 scientists call disarmament answer to weapon ‘stalemate’,” *New York Times* (25 September 1964): 16; the op-ed appeared as “Spiral to oblivion,” *New York Times* (26 September 1964): 22.

of metaphor seems more than mere wordplay: “I repeat that we were delighted,” he wrote, “to be the vehicle for delivery of this warhead.”⁵⁵

ABM, MIRV, and Stability

As defense researchers and arms control sympathizers continued to puzzle over the problems of offense and defense, they began to worry. It wasn't just that some system or other wouldn't work; the trouble was in the nature of offensive and defensive technologies and in their strategic interaction. The trouble was stability. The Jason physicist and Caltech professor Murray Gell-Mann was among the very first to recognize the problem and develop a strategic critique of ABM and MIRV. He got there by ignoring his homework.

In early 1962 the Jason secretary, David Katcher, urged Gell-Mann to begin a project on the physics of missile reentry, including “decoy and other penetration aid problems.” This was, of course, standard fare for IDA and Jason. Gell-Mann was supposed to team up on the project with his Caltech colleagues Fredrik Zachariassen and Matthew Sands, as well as the Princeton physicist Malvin Ruderman.⁵⁶ But Gell-Mann was less interested in the complex physics of reentry and more interested in questions of strategy. When Jason chairman Marvin Goldberger learned that Gell-Mann had whiled away several weeks dodging his assigned problem, he wrote a terse letter: “I am a little irritated (perhaps even more than a little) to be informed...of your

⁵⁵ Gerard Piel to Jerome Wiesner, 11 November 1964, Box 100, Folder “Scientific American,” *JBW*. Rebecca Slayton quotes this letter in *Arguments that Count*, 90.

⁵⁶ David A. Katcher to Murray Gell-Mann, 29 January 1962, Box 35, Folder 3 “JASON Project 1962,” *MGM*.

reluctance to undertake the job I had asked you to do. I'm willing to honor any decision of this variety, but I think you ought to have let me know."⁵⁷

Unflustered, Gell-Mann searched for a more exciting problem. In 1962 the Jason summer study was held at the Lawrence National Laboratory up in the Berkeley Hills, where the consultants gathered in Building T50, a long, wooden structure perched on stilts, affectionately known by local physicists as "The Motel."⁵⁸ The Jason group, as it often had, took up questions related to missile defense. Gell-Mann later reflected:

By that time JASON had gotten importantly involved in looking at projects for anti-ballistic missile defense. That was sponsored mostly by ARPA, and many of the problems that people were interested in were connected with technical aspects of ABM. I quickly zeroed in, though, on the fundamental question, for my own interest, of why one should have an ABM defense, and whether one should have an ABM defense. I spent the summer interviewing a great number of people—military officers, civilian scientists, officers or officials of the Department of Defense and so on—about U.S. strategy, especially connected with the use of strategic nuclear weapons, and the relevance to all of that of the project to try to develop a partial anti-ballistic missile defense.⁵⁹

Toward the end of the summer, Gell-Mann gave a talk to the Jasons, "outlining what I thought were just about all the principal issues relevant to the development and potential deployment of an ABM system." His little fact-finding project on ABM had left him disturbed. What was unusual and striking about Gell-Mann's analysis was his use of strategic concepts that would

⁵⁷ M.L. Goldberger to Murray Gell-Mann, 13 February 1962, Box 8, Folder 16 "Goldberger, Marvin L.," *MGM*.

⁵⁸ David A. Katcher, "Reassuring Note on the Existence of a JASON Summer Study," 22 June 1962, Box 35, Folder 3 "JASON Project 1962," *MGM*.

⁵⁹ Interview of Dr. Murray Gell-Mann by Finn Aaserud, 23 April 1987, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD, <http://www.aip.org/history/ohilist/28394.html> (hereafter cited as Gell-Mann interview, 1987).

have been more at home in the RAND social sciences division than among IDA's Jason physicists. (Gell-Mann was plenty familiar with the economic-style strategic analysis done at RAND, as he'd consulted for the organization since 1956.)⁶⁰

At the center of Gell-Mann's analysis was a familiar device: the exchange ratio. "During the 1962 JASON summer study," according to an untitled document in Gell-Mann's files, he had "identified the exchange ratio, that is the number of an opponent's missiles that are destroyed by each missile fired, as an important parameter of strategic stability, suggesting that accurate multiple warheads might lead to first strike instabilities."⁶¹ Gell-Mann's biggest worry wasn't MIRV alone, but MIRV in combination with ABM. "If a first strike could be launched with a high exchange ratio, the attacker's ABM system would have many fewer missiles to destroy in the anticipated retaliation, and the attacker would still have a reserve of missiles." That is, an accurate, MIRVed attacking force could do serious damage to the retaliatory force of the enemy. With no ABM, the enemy could still deliver a handful of strikes against the attacker in retaliation. In the thermonuclear age, a handful was more than enough for the purposes of deterrence. But *with* ABM, there was the risk that the enemy's retaliation—much reduced in number and sophistication by the opening strike—could be thwarted by the defense. In this case, a clear advantage accrued to the side firing a first, massive assault, and deterrence was at risk.

⁶⁰ Gell-Mann interview, 1987.

⁶¹ This single undated, untitled, typed sheet of paper resides in Gell-Mann's archival files, tucked away in a Jason project folder holding documents from 1964. It is unclear why the document was written in the third-person; Gell-Mann seems to have composed it for posterity. It can be found in Box 35, Folder 5 "JASON Project 1964," MGM. Note that Gell-Mann inverts the definition of the exchange ratio used by Jerome Wiesner, Paul Doty, John Tukey, and Donald Brennan in 1959 (see Chapter 1). Those authors had defined the exchange ratio as the number of missiles required to destroy a single defending missile. Gell-Mann defines it here as the number of missiles killed by a single attacking missile. Whereas a large exchange ratio was the hallmark of strategic stability for Wiesner et al., Gell-Mann thinks of stability primarily as a function of exchange ratio *and* defensive capability—for him, even a situation of high exchange ratio (with MIRV) might be relatively stable (if each side possesses MIRVed offensive missiles, e.g.), provided there are no active defenses.

The risk was instability of the kind Thomas Schelling had warned about years earlier: first-strike instability, the temptation to preempt.⁶²

Gell-Mann's presentation was a hit. In September, after the summer study had concluded, Katcher told him that "there is an exceptional amount of interest in the paper you have promised to write. You may not think that the intensity is justified, and perhaps it isn't, but you may also not be in a good position to know how welcome is a clear argument on the banks of the Potomac."⁶³ And yet Gell-Mann never produced the report. "I should have then written a report that fall, outlining all of these things. The high officials—the highest officials in the Department of Defense—like Harold Brown [York's successor as DDR&E], were becoming aware of all these arguments, and McNamara as well, and it would have been useful to add to their arsenal of reports this one."⁶⁴

At the Jason summer study in 1963, when most of the group was preoccupied with lasers, Gell-Mann chaired a special committee on the strategic and arms control implications of ABM and MIRV. The Jason group, originally the preserve of elite theoretical physicists, was warming to the social science perspective on nuclear issues.⁶⁵ Thomas Schelling made the trip down to Cape Cod from Boston to serve on the panel. Herbert Scoville took time from his day job in Washington—he had moved from the CIA to join the bureau of science and technology in the new Arms Control and Disarmament Agency (ACDA)—and helped with the study, too. The topic of choice was the strategic stability of multiple warheads and active defense, now "in light of some very optimistic accuracy projections that were being suggested at the time," as Gell-

⁶² See the untitled, undated document in Box 35, Folder 5 "JASON Project 1964," *MGM*.

⁶³ David A. Katcher to Murray Gell-Mann, 17 September 1962, Box 35, Folder 3 "JASON Project 1962," *MGM*.

⁶⁴ Gell-Mann interview, 1987.

⁶⁵ In the mid-1960s Gell-Mann had even supported a proposal to create a "social science Jason," the brainchild of the Stanford psychologist Jesse Orlansky. See the correspondence between Gell-Mann and Orlansky in Box 35, Folder 35.6 "JASON Project 1965," *MGM*.

Mann remembered. Once again he and his committee-mates argued that ABM was strategically pernicious. MIRV by itself was probably okay, but “if an ABM system could handle the residual retaliatory force...a high-exchange ratio MIRV [would] lead to instability.”⁶⁶

Thomas Schelling, who had become a kind of honorary Jason member for the summer of 1963, produced an independent report titled “City Defenses and the Dynamics of Warfare.” Unencumbered by numbers or equations, Schelling mused about the consequences of population defense for the process of escalation from crisis to conflict. Missile defense threatened the deterrence compact that Schelling had described in the late 1950s, which depended upon making cities more vulnerable (holding populations “hostage”) to a nuclear strike, and missiles less vulnerable. But lately he had gotten more interested in the concept of limited war and “damage limitation”: of controlling escalation so that even if deterrence failed, it would fail intelligently, slowly, rationally, and under control. Schelling was willing to think of arms control and deterrence as things that extended into nuclear war itself. In his Jason report he argued that the defense of cities might well temper the war—causing the attacker to pause between successive salvos to check whether its missiles had gotten through the defenses, for example—giving each side more opportunities to stop shooting and negotiate peace.⁶⁷

Most arms controllers in later years resisted this kind of speculation about controlled escalation and “intra-war deterrence”; for them, deterrence functioned if it made nuclear war all but impossible. It was in that spirit that Gell-Mann had given a strategic rationale for rejecting

⁶⁶ Untitled, undated document in Box 35, Folder 5 “JASON Project 1964,” *MGM*. The group also coauthored a larger classified report. Gell-Mann later recalled that the committee made the mistake of trying to estimate the numbers of nuclear warheads that would be available in future years for both offensive and defensive missions, “to get some idea of the absolute scale.” The Defense Department, enormously protective of information about the size of the arsenal in the early 1960s, stamped the report top-secret, making it “unavailable to almost anyone, which greatly reduced its utility” when it was completed in the fall of 1963. See Gell-Mann interview, 1987.

⁶⁷ Thomas C. Schelling, “City defenses and the dynamics of warfare,” Jason Research Paper P-78 (1963).

ABM (and MIRV along with it), built with the language of deterrence and stability, first and second strikes, and the exchange ratio.

* * * *

An early opportunity for a wider airing of the ABM-MIRV instability idea came at a Pugwash meeting in Udaipur, India in early 1964, the only Pugwash meeting Gell-Mann would ever attend. There he met Jack Ruina, an electrical engineer by training who had been the sole academic member of the Reentry Body Identification Group in 1957. That experience had first taught him about the physical limitations of any missile defense system. Ruina had come up during the Second World War working on radar tracking, communications, and signal analysis. Chalmers Sherwin, a physicist and fellow Rad Lab veteran, recruited him to the faculty of the University of Illinois after the war. After Ruina's service on the RBIG, Sherwin (who had become the Air Force's chief scientist) again scooped him up in 1959 and put him in charge of an electronics office in the Air Force. Ruina was on his way up the ladder. A year later Herbert York, as DDR&E, named him his Assistant Director for Air Defense (a post that put Ruina in charge of missile defense research in the DOD). When he met Gell-Mann in India, Ruina had just concluded two years as the director of ARPA. In 1963 his personal persuasion in a meeting with President Kennedy (which had been personally arranged by Jerome Wiesner) had helped convince Kennedy not to pursue the Nike-Zeus missile defense system. By 1964 Ruina had left the government and become a professor at MIT.⁶⁸

⁶⁸ Interview of Dr. Jack P. Ruina by Finn Aaserud, 8 August 1991, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD, <http://www.aip.org/history/ohilist/35154.html> (hereafter cited as Ruina interview, 1991). In Congressional testimony in 1960 Ruina explained that the Army's Nike-Zeus missile defense system should not be deployed in light of "the probability that the enemy can, without prohibitive cost to himself, provide for nearly simultaneous arrival of multiple targets, either decoys or perhaps even true warheads. Then it is clear that in its present design the Nike-Zeus firepower can be rather easily saturated." See United States Congress, Subcommittee of the House Committee on Government Operations, "Organization and Management of Missile Programs" (Washington, DC: Government Printing Office, 1960). On the meeting with Kennedy, see Walter

Together Gell-Mann and Ruina wrote a brief paper at the conference, with an assist from Carl Kaysen, focusing more on ABM than on MIRV and laying out an argument for a cooperative U.S.-Soviet ban on ABM systems. But there had been a shift in the focus—no longer only on the possibility of a first strike, but on the implications of ABM for the nuclear arms race. They imagined a sequence of competitive escalation: Side A deploys a missile defense system for population protection; side B reacts by increasing the number of its offensive forces, overcompensating because it does not know how effective side A's ABM might be; side B also deploys an ABM; side A also increases its nuclear forces. "The net result would be that the strategic balance would still be maintained but," with all those extra missiles and the defenses they now had to contend with, "in a much less obvious manner than originally." Thus Ruina and Gell-Mann concluded that ABM would "accelerate the pace of the arms race" and "increase the dangers of escalation from crisis to nuclear war."⁶⁹

This was something new. In adding the arms race component, the two authors had gone beyond Gell-Mann's original worry about ABM and MIRV. And they joined a longer tradition of arms race analysis that had developed by the mid-1960s. For years analysts had been thinking about arms races as interaction processes: an acquisition of arms by one state precipitates a reaction in kind by its adversary. The action-reaction picture of arms races went all the way back to the work of Lewis F. Richardson, an English Quaker and Cambridge-trained mathematician known early in his career as an expert in numerical weather prediction. After serving as an ambulance driver during World War I, Richardson turned his mathematical acumen to the study of war. He modeled two-country conflict in terms of interacting quantities—for example, each

A. Rosenblith, ed., *Jerry Wiesner: Scientist, Statesman, Humanist: Memories and Memoirs* (Cambridge, MA: The MIT Press, 2003), 296.

⁶⁹ J.P. Ruina and M. Gell-Mann, "Ballistic Missile Defence and the Arms Race," *Proceedings of the Twelfth Pugwash Conference on Science and World Affairs*, Udaipur, India, January 27 – February 1, 1964 (London: Pugwash Central Office, 1964), 232-235, on 234.

nation's "vigor to war" or its "warlike activity." Richardson put such things into time-dependent variables and derived a simple model of arms races in the form of linear, coupled differential equations.⁷⁰

In 1957 Richardson's ideas underwent an American revival. Their champion was Anatol Rapoport, a mathematical biologist and peace researcher writing from the Mental Health Research Institute at the University of Michigan. Rapoport distilled Richardson's work in a long article published in the new *Journal of Conflict Resolution*, showing how the arms levels x and y of two hostile countries could be related in Richardson-style mathematics. The system could be solved for the "steady state"—the case when the rate of change of the size of each arsenal was zero.⁷¹ Under specific conditions (when the factors tending to encourage arms buildups were initially "less than" factors tending to accelerate them), the system of mutual arms levels would evolve naturally toward a stable equilibrium: x and y would tend in time toward fixed numbers and stay there, no matter how big they had originally been. But under different circumstances, the system would, as Rapoport put it, hit "a certain 'ignition point' such that, if for some reason armaments have increased beyond it, a runaway race will begin." Rapoport—no stranger to

⁷⁰ See Lewis F. Richardson, "Mathematical psychology of war," in Ian Sutherland, ed., *Collected Papers of Lewis F. Richardson, Volume 2: Quantitative Psychology and Studies of Conflict* (Cambridge: Cambridge University Press, 1991), 61-100; Lewis F. Richardson, *Generalized Foreign Politics* (British Journal of Psychology Monograph Suppl. No. 23, 1939). Richardson's arms race theories were summarized posthumously in Lewis F. Richardson, *Arms and Insecurity* (Pittsburgh, PA: Boxwood Press, 1960). On Richardson's life, see T.W. Körner, *The Pleasures of Counting* (Cambridge: Cambridge University Press, 1996), esp. chapter 8, "A Quaker mathematician," 159-193.

⁷¹ The central proposition was that the rate of change of one country's arms stockpile (call this stockpile x) depends in a simple way on the present value of x , on the arms stockpile of an opposing side (call it y), and perhaps on an additional factor, which Rapoport called a "grievance." The "grievance" was meant to add another ingredient to the mix—a degree of political hostility (or friendliness) independent of arms levels. The actual differential equations ran as follows: (1) $dx/dt = ky - ax + g$; (2) $dy/dt = lx - by + h$, where x and y are the respective stockpiles of the two sides, k and l are arms race "stimulant" coefficients representing respective incentives to accumulate more arms based on the level of the opponent's arms, a and b are "restraint" coefficients representing "cost consciousness" of one's own strength (so that the larger one's own arsenal is, the less inclined one is to spend more to increase it), and g and h are respective political "grievances" independent of arms levels. The arms levels x and y are each functions of time t and of the various coefficients; the various coefficients can in principle be time-dependent, as well.

systems theory and the idea of dynamic feedback—was searching for a definition of arms race stability.⁷²

Implicit in his argument was the notion that arms races (in some sense) “cause” war. Others fretted more scrupulously over the link between arms races and armed conflict, however. In 1958 Samuel Huntington wrote an article in which he tabulated data on historical arms races and conflicts, finding that quick arms buildups tended to end in war, but long-term arms races could actually help stabilize international politics. For Huntington, an arms race was the outworking of attempts by states to balance power in the international system; and it was perfectly possible for an arms race to achieve exactly that—a balance of power, and a reduced likelihood of war. He drew a distinction between quantitative arms races (of the kind Rapoport focused on)—races in numbers of weapons—and qualitative arms races—technological competitions in weapons sophistication. Huntington concluded that quantitative races were more likely to result in imbalances of power between states, whereas the qualitative kind tended to equalize power.⁷³

By the early 1960s the basic picture of how arms races work invited generalization beyond the mechanical model of Lewis Richardson to something better suited to a Cold War-era taste for systems theory. In 1962 the economist Kenneth Boulding framed arms races in systems language in his book *Conflict and Defense*. Like a good economist, he thought of arms levels not in Richardson’s mechanical terms—like masses connected on a spring—but by drawing reaction curves familiar from the theory of price wars and inflation. Boulding was the doyen of peace

⁷² In terms of the equations above, the stability condition is given by $ab > kl$ (i.e., the product of the restraint coefficients should be larger than the product of the stimulant coefficients). Anatol Rapoport, “Lewis F. Richardson’s mathematical theory of war,” *Conflict Resolution* 1, no. 3 (1957): 249-299, quotation on 278. Rapoport presented a longer treatment of arms races in Anatol Rapoport, *Fights, Games, and Debates* (Ann Arbor, MI: University of Michigan Press, 1960).

⁷³ Samuel P. Huntington, “Arms races: Prerequisites and results,” *Public Policy* 8, no. 1 (1958): 41-88.

research at the University of Michigan; he'd published Rapoport's original article on the Richardson model as editor of the *Journal of Conflict Resolution*.⁷⁴ Well known within the early arms control community, at the 1960 *Daedalus* arms control conference, Boulding had given a paper asking whether the U.S. economy could safely reduce its national security budget without a crippling depression.⁷⁵ Since the early 1950s he had also promoted something he called "general systems theory," an ambitious project to unify knowledge across the isolated disciplines, leading, he said, "ultimately to something like a general field theory of the dynamics of action and interaction." For him, all social phenomena were characterized by general processes of action and interaction; and arms races were especially interesting social phenomena. "Arms control," he told a 1962 conference at the University of Michigan, "is a problem of social systems rather than of physical systems, even though physical systems such as weapons, satellites, and inspection devices may be deeply involved in it. A weapon, however, is not a physical thing—it is a physical thing in a social situation.... It is to social systems, therefore, that we must look if we are to understand the arms race, and if we are to find out how to stop it and to reverse it."⁷⁶

⁷⁴ One colleague later remembered him as "half Milton Friedman, half Mahatma Gandhi." Sylvia Nasar, "Kenneth Boulding, an economist, philosopher and poet, dies at 83," *New York Times* (20 March 1993).

⁷⁵ Boulding pointed out that in 1959, in terms of gross national product, the Pentagon by itself had the world's third largest nonmarket economy. Boulding recognized the impact of massive defense spending on science, too, but was disturbed by the implications. National security spending "organizes research on a scale of expense unknown to the civilian world," he wrote. "The Pentagon and Hollywood seem to be the only two places in our society where extravagance is cultivated as a virtue. Therefore, when research is hitched to the military rocket, it proceeds at a pace far beyond that of the civilian and merely peripatetic philosopher. I am quite willing to deplore this fact, but I am forced to acknowledge it. Perhaps the biggest social invention of the mid-twentieth century was the RAND Corporation, which perpetually makes obsolete the institution that fathered it." Kenneth E. Boulding, "Economic implications of arms control," in Donald G. Brennan, ed., *Arms Control, Disarmament, and National Security* (New York: George Braziller, 1961), 153-164, on 160-161. For more on Richardson, Rapoport, and Boulding, see Paul Erickson, *The Politics of Game Theory: Mathematics and Cold War Culture* (PhD dissertation, University of Wisconsin, 2006).

⁷⁶ Kenneth E. Boulding, "General systems theory: The skeleton of science," *Management Science* 2, no. 3 (1956): 197-208, on 202; Kenneth E. Boulding, "The university, society, and arms control," *Journal of Conflict Resolution* 7, no. 3, Weapons Management in World Politics: Proceedings of the International Arms Control Symposium, December, 1962 (1963): 458-463, on 458; and Kenneth E. Boulding, *Conflict and Defense: A General Theory* (New York: Harper & Brothers, 1962). The air in these years was thick with the terminology and concepts of

By 1964 the theory of arms races was rich and developed enough that a young MIT-trained economist and newly hired analyst at RAND named Michael Intrilligator could sum up the whole genealogy of arms race thought, from Richardson to Boulding, in a report written that year. Intrilligator drew the same kinds of reaction curves that Boulding had drawn in elaborate detail. But he found that subtler refinements could be made if one considered the effects of different nuclear strategies on the shapes of the reaction curves, and their points of equilibrium where the arms curves intersected. He considered two strategies. One was a “deterrence strategy.” The other was an “arms depriving” strategy in which one side tried to take away the other’s weapons by striking them in a counterforce attack. Intrilligator found a point of stable arms race equilibrium in the case when the two sides each opted for a deterrence strategy. But when one side opted for deterrence and the other for counterforce, the equilibrium point was unstable; the arms race was perched like a ball atop a hill, guaranteed to roll off at the slightest nudge. This was precisely the claim that conservative analysts would begin to make as the 1960s proceeded—that while the U.S. had settled for a deterrence capability, the Soviet Union was building toward counterforce.⁷⁷

That same year, the action-reaction model of arms races came naturally enough to Gell-Mann and Ruina that they applied it to the new technologies of MIRV and ABM (without citing any of the arms race literature directly). But unlike the Richardson/Rapoport picture, which hadn’t distinguished between offensive and defensive weapons; and unlike Huntington’s, which

the systems sciences. Schelling sensed the diffusion of interdisciplinary ideas in 1960: “There is something here that looks like a mixture of game theory, organization theory, communication theory, theory of evidence, theory of choice, and theory of collective decision.” Thomas C. Schelling, *The Strategy of Conflict* (Cambridge, MA: Harvard University Press, 1960), 14-15. For a classic attempt at synthesizing systems ideas, see Herbert A. Simon, “The architecture of complexity,” *Proceedings of the American Philosophical Society* 106, no. 6 (1962): 467-482. On the pervasiveness of the systems sciences in this period, see Hunter Crowther-Heyck, *Herbert A. Simon: The Bounds of Reason in Modern America* (Baltimore, MD: Johns Hopkins University Press, 2005).

⁷⁷ In the case of two counterforce strategists facing each other, there was no hope at all. Intrilligator argued that an unmitigated arms race, limited only by budget constraints, would result. See Michael D. Intrilligator, “Some Simple Models of Arms Races,” RAND Memorandum RM-3903-PR (April 1964), quotation on 10.

said that arms races could either be quantitative or qualitative, Ruina and Gell-Mann mixed everything together. They argued that the qualitative developments, offensive and defensive, would lead to a quantitative race and an increased risk of war.

This argument jibed with the models of strategy and arms races that had been in development in the United States since the late 1950s, but the Soviet participants at the Pugwash meeting weren't necessarily ready to get on board. At the conference in India in 1964, after writing up their proposal for an ABM ban, the Americans presented the paper to the Soviets. The two delegations agreed to discuss it privately at a breakfast meeting the next day. The meeting proceeded in awkward silence until Mikhail D. Millionshchikov, a fluid dynamicist and member of the Soviet Academy of Sciences, leapt to his feet and declared Ruina and Gell-Mann's paper "crazy." "Why, with your exchange ratio arguments from the RAND Corporation," came Millionshchikov's denunciation (as dramatized later by Gell-Mann), "you have produced a total absurdity. You are asking the Soviet Union to renounce attempts to defend its population."⁷⁸

Gell-Mann and Ruina both resisted making a wider broadcast of the ABM ban idea. Ruth Adams, managing editor at the *Bulletin of the Atomic Scientists*, caught wind of the Gell-Mann/Ruina paper after the Pugwash meeting and the next month asked Ruina if he would consider publishing it in the *Bulletin*. But neither Ruina nor Gell-Mann felt ready to join the ABM battle so publicly. "I don't think the timing is right for it to become a public issue," Ruina replied, "although I have no qualms about having the matter discussed 'semi-publicly.' I think foul use can be made of our arguments by irresponsible people and this can do irreparable harm."⁷⁹ The two had nonetheless planted an idea in the sheltered realm of the "semi-public." The lesson that several arms controllers had started to internalize in the 1963 and 1964 studies

⁷⁸ Gell-Mann interview, 1987.

⁷⁹ Ruth Adams to Jack Ruina, 14 March 1964; Jack Ruina to Ruth Adams, 20 March 1964, in Box 17, Folder 32 "Ruina, Jonathan P.," *MGM*.

was that ABM and MIRV interacted, with implications for the stability of deterrence and the arms race. Building one would induce, with an almost mechanical necessity, the other side to build the other. During a political and military crisis, the possession of both MIRV and ABM would introduce a dangerous temptation to strike first. And the strategic arms race might well hit the “ignition point” Anatol Rapoport had warned about.

Containing the Arms Race

By 1964, missile defense had risen high enough in the consciousness of the expert community to be considered a genuine arms control problem. In May, the Harvard-MIT arms control seminar devoted for the first time an entire session, its last of the academic year, to the topic of ABM. During the heated colloquy, a new visitor to the seminar, Jeremy Stone, presented what he thought were the chief troubles with ABM. Stone was in between jobs, about to start a temporary research position at Harvard’s Center for International Affairs (CfIA). As a doctoral student in mathematics at Stanford in the late 1950s, he had spent summers at RAND working on linear programming; after finishing a dissertation on algebraic functions and group theory in 1960, he was hired at the Stanford Research Institute as an applied mathematician. Through the introduction of a friend, Stone soon found himself in the RAND office of Herman Kahn. Kahn informed Stone that he was leaving RAND to create his own strategy think-tank near New York, called the Hudson Institute. By 1962 the twenty-seven year old Stone was part of Kahn’s cadre of strategists-in-training. It was quite a turn of events—not least because Stone’s father was the leftist muckraking journalist I.F. Stone. (The government knew this piece of the younger Stone’s biography well enough, tying up his application for a top-secret security clearance.)⁸⁰

⁸⁰ Jeremy J. Stone, *Every Man Should Try: Adventures of a Public Interest Activist* (New York: PublicAffairs, 1999), 3-12.

Stone had thought a lot about the arms control implications of defensive measures. His first job at Hudson had been on an ACDA-funded study directed by Donald Brennan (who had left MIT's Lincoln Laboratory to follow Kahn, and now served as Hudson Institute's president). The project was on the strategic implications of civil defense. Stone—whose political views resembled his father's, despite his new status as a Cold War strategy intellectual—was disturbed to find that the main report would support one of Kahn's "pet ideas": the evacuation of cities prior to a U.S. nuclear attack on the Soviet Union, part of Kahn's notion of a "credible first-strike" capability.⁸¹ Stone wrote an appendix to the report and argued that while crisis evacuation would make the "extended deterrence" of Soviet aggression against U.S. allies more credible, "preparations for the evacuation may tend to accelerate the arms race and make arms control more difficult." The Soviets, naturally, would respond by getting more missiles to hit more targets, probably with larger warheads, inducing further moves along the arms race spiral.⁸²

In 1963, sitting in the attic of his home in Elmsford, New York, Stone was jolted by what he called "an electric thought." Since 1962 U.S. intelligence had been aware of construction on a potential Soviet ABM system. He decided that if the Americans could convince the Soviets *not* to build up their ABM capability, pressure in the United States for an ABM would be reduced; defensive systems might beget defensive systems in a defensive arms race. Stone knew he would have a tough time getting anyone at Hudson on board with this idea. The Institute had just done a

⁸¹ Later Kahn spun this as the "not-incredible first-strike." The basic idea was that if the Soviets moved on Western Europe, the evacuation of American cities would send a clear signal that the U.S. was prepared to unleash nuclear fire. On Kahn's "credible first-strike" capability, see *On Thermonuclear War*, 27-36. On the "not-incredible first-strike" capability, see Herman Kahn, *Thinking About the Unthinkable* (New York: Horizon Press, 1962), 65-66.

⁸² Stone even went as far as proposing a "no-first-evacuation" policy, akin to a "no-first-strike" policy. The purchase of crisis evacuation capabilities "would represent a new round in the arms race—a shift from purchasing more and more offensive weapons to accepting the inevitability of some retaliation and attempting to protect against it in an effective way.... As a result, it seems to us that arms control interests are quite clearly arrayed against a crisis evacuation program." Jeremy J. Stone, "The Question of Crisis Evacuation," Annex I to *Arms Control and Civil Defense*, Hudson Institute Report HI-216-RR (20 August 1963). He referred to Kahn's "not-incredible first strike" as one of his "pet ideas" in *Every Man Should Try*, 4.

missile defense study on the Army's dime, arguing in Kohnesque fashion that an ABM could improve the country's prospects for post-attack recovery by protecting an industrial plant or two while the rest of the country lay in a smoldering heap. This made Stone uncomfortable; he told Kahn that he was reluctant to work on ABM studies like the one done for the Army. No problem, Kahn replied: Stone could simply do his own Hudson study of ABM for ACDA instead. So Stone sharpened his knives. He quickly wrote a paper for a conference at the University of Michigan, arguing that the U.S. should do everything possible to prevent the Soviet Union from acquiring missile defenses.⁸³ And he got to work on a new Hudson Institute report, "Anti-Ballistic Missiles and Arms Control," which followed the same line as his earlier report on civil defense. He announced ABM's pernicious arms race effects, pointing out that "certain decoys will easily saturate the Soviet system," and argued for a "no-first-procurement" policy and a freeze agreement on ABM.⁸⁴

Donald Brennan was sufficiently impressed to send some of Stone's work to Adam Yarmolinsky, one of Robert McNamara's Whiz Kids at the Pentagon. Brennan even offered that perhaps the Secretary would be interested in Stone's argument. Herman Kahn was less enthused; he supported ABM as a component of U.S. capability for fighting and winning a nuclear war.⁸⁵ Stone gave a lecture at Hudson on the topic of ABM, presenting his arguments for an ABM freeze. The formidable Kahn peppered him with questions, but Stone considered the presentation

⁸³ This paper was published as Jeremy J. Stone, "Should the Soviet Union Procure an Urban Anti-Ballistic Missile System?," Hudson Institute Discussion Paper HI-301-DP (15 November 1963).

⁸⁴ Jeremy J. Stone, "Anti-Ballistic Missiles and Arms Control," Hudson Institute Report (1963). The freeze idea (promoted independently by other experts, including Murray Gell-Mann and Jack Ruina in 1963) was gaining traction within ACDA. In January 1964 the United States proposed "a verified freeze of strategic nuclear offensive and defensive vehicles" to the Eighteen Nation Disarmament Committee. See "Message from President Johnson to Eighteen Nation Disarmament Committee, January 21, 1964," United States Arms Control and Disarmament Agency, *Documents on Disarmament 1964* (Washington, DC: Government Printing Office, 1964), 145-146. On the diplomatic history of the Johnson administration's arms control efforts, see Hal Brands, "Progress unseen: U.S. arms control policy and the origins of détente, 1963-1968," *Diplomatic History* 30, no. 2 (April 2006): 253-285.

⁸⁵ Kahn presented his support for a "thin" ABM system (defending against a light, rather than massive, ICBM attack) in Herman Kahn, *On Escalation: Metaphors and Scenarios* (New York: Frederick A. Praeger, 1965), 157-159.

a triumph—the one time he had matched wits with Kahn in verbal combat. Satisfied that he had gotten his point across, he promptly resigned from the Institute. In the spring of 1964, with an invitation from Paul Doty to present his ABM work in the Soviet-American Disarmament Study Group (a smaller, bilateral offshoot of Pugwash sponsored by the American Academy of Arts and Sciences), and an invitation from Thomas Schelling to temporarily join the CfIA at Harvard, Stone found himself in Cambridge.⁸⁶ Now in front of the arms control seminar, he tried out his arguments against missile defense. Jack Ruina—back in Cambridge and a regular seminar attendee—agreed with Stone, and offered that there was a “logical sequence of reaction to the installation of a Soviet ABM” on the U.S. side, from building up U.S. missile forces to purchasing an American ABM system. Even Thomas Schelling seemed receptive to the argument against ABM, opining that since missile defense “may be the next dimension of the arms race, a lack of parity”—one side’s having ABM, but not the other—“could make arms control totally infeasible.”⁸⁷

Murray Gell-Mann had continued to advertise the unique strategic instabilities raised by ABM within closed circles during the summer of 1964. At a classified ACDA summer study in Aspen, Colorado, he renewed his suggestion of an ABM ban.⁸⁸ No one had forgotten about MIRV, either. During the 1964 Jason summer study, ARPA assistant director Charles Herzfeld asked the Jasons to consider (as Jack Ruina remembered it) “what technology is on the horizon that will change the strategic picture in the world.” Ruina, who had just joined the Jason group

⁸⁶ The Soviet-American Disarmament Study Group originated as the Committee on International Studies of Arms Control, sponsored by AAAS, in 1961. See Matthew Evangelista, *Unarmed Forces: The Transnational Movement to End the Cold War* (Ithaca, NY: Cornell University Press, 1999), 36-37; and John Wilson Lewis, “Report: Meeting on US-Soviet Arms Control Discussions, New York City, November 5, 1976,” Box 45, Folder 10 “Arms Control, 1976,” *HFY*.

⁸⁷ “Joint Arms Control Seminar, Minutes of the Eleventh Session, 11 May 1964,” Box 3, Folder 4 “Joint Harvard-MIT Arms Control Seminar 1963-64, Minutes,” *LPB*.

⁸⁸ Leonard S. Rodberg to Murray Gell-Mann, 2 April 1964, Box 56, Folder 1 “Arms Control and Disarmament Agency, United States (USACDA), 1964-1965,” *MGM*.

himself, took charge of this study and brought along his trusted co-author Gell-Mann, as well as the esteemed physicist Freeman Dyson of the Institute for Advanced Study in Princeton, and Robert LeLevier of RAND, to help him with the report. Ruina wrote a summary letter addressed to Herzfeld and George Rathjens (who had since moved from the PSAC staff to a post in ACDA's science and technology bureau), in which Ruina suggested that of all new strategic technologies, MIRV was the most dangerous. "A systematic study of the implications of MIRV under various limiting conditions for the U.S. and [Soviet] arsenals ought to be undertaken. Perhaps this should be part of a more general study which tries to assess the various constraints in the form of treaty prohibitions, inspection requirements, etc., have on the arms race and what constraints tend to have the arms race become more or less stable."⁸⁹ This was Rathjens's first acquaintance with the anti-MIRV argument. Just a few months earlier, he had chaired a special ACDA study of possible improvements to ABM and ABM-defeating systems. The committee recommended in favor of MIRV precisely because it was thought to be so effective against ABM.⁹⁰

In 1965, in another intense and revealing session of the arms control seminar, the group batted around various arguments. Again Jeremy Stone gave his well-honed pitch for a freeze on both defensive systems and offensive delivery vehicles ("to keep the motives for arms expansion in check," he said). For Stone, who wanted to cauterize the arms escalation at its next stage (the procurement of ABM), the right way to deal with the nuclear conundrum was not "managing the arms race" (an approach he associated with Thomas Schelling and Morton Halperin) but "*containing* the arms race." Stone had put a new twist on classic terminology—containment

⁸⁹ J.P. Ruina, "A Comment on Future Weapons Systems," Jason Internal Note N-172 (3 September 1964), Document NH00790, *DNSA*. Also see Ruina interview, 1991. This letter is quoted in Greenwood, *Making the MIRV*, 110-111.

⁹⁰ Greenwood, *Making the MIRV*, 109.

policy turned against the weapons themselves, rather than an expansionist enemy state. The arms race wasn't something to be coaxed into a direction more favorable to U.S. national security. It was something to be tranquilized and reversed.⁹¹

The arms control seminar Stone visited had developed into a lively salon with an interesting mix of opinions and intellectual styles. Among its regular participants were the Sovietologists Marshal Shulman and Alexander Korol; the international law scholars Louis Sohn and Roger Fisher; the communications theorists Ithiel de Sola Pool and Daniel Lerner; the political scientists Eugene Skolnikoff, Lincoln Bloomfield, and Kenneth Waltz; and the old hands Schelling, Feld, and Kaysen. Kissinger and Doty still frequently popped in, too, and now that Wiesner had returned from Washington he could often be found in the seminar. Some of the most regular attendees were recent arrivals from RAND. There was the sociologist Fred Iklé, who had written a blockbuster study on the risk of an accidental detonation of nuclear weapons (which he thought a very serious risk indeed), had led RAND's first full-fledged study of nuclear proliferation (which he found far less worrisome), and had now taken a job in MIT's political science department. Joining him was Ciro Zoppo, who had worked on nonproliferation at RAND as he finished his dissertation in political science at Columbia, and was now a fellow at the CfIA. And there was the jocular William Kaufmann, who had become one of the seminar's leading lights. After heading studies of counterforce and no-cities strategies at RAND in the 1950s, Kaufmann had joined MIT as a professor of political science in the early 1960s—initially half-time, to make room for his frequent consulting trips to the Pentagon.⁹²

⁹¹ See Jeremy J. Stone, *Containing the Arms Race* (Cambridge, MA: The MIT Press, 1966). Stone used this phrase in the arms control seminar meeting. The emphasis is mine.

⁹² For more background on Iklé, see Chapter 4, and Eric Schlosser, *Command and Control: Nuclear Weapons, the Damascus Incident, and the Illusion of Safety* (New York: The Penguin Press, 2013), 190-196; Fred Charles Iklé, Hans Speier, Bernard Brodie, Alexander L. George, Alice Langley Hsieh, and Arnold Kramish, "The diffusion of nuclear weapons to additional countries: The 'Nth country' problem," RAND RM-2484-RC (1959); Fred Charles Iklé, "Nth countries and disarmament," *Bulletin of the Atomic Scientists* 16, no. 10 (1960): 391-394.

For the strategic analyst types, Stone's arguments gave off a familiar odor. This was old-fashioned disarmament talk dressed up in new terminology. And besides, ABM had other benefits. Kaufmann's counterforce thinking gave him an easy affinity for the idea of "damage limitation"—the notion that if a nuclear war will happen, it makes no sense for the combatants to immediately hit each other's cities in an orgy of world-ending violence. It seemed more rational to pick off your opponent's nuclear forces, defending your own cities and forces to the extent possible. For those who favored it (and many at RAND had since the 1950s), ABM was seen as consistent with this picture. In the seminar, Kaufmann argued that Stone was probably underestimating how effective ABM could be in the future. What if, in the age of MIRV, "ABM might possibly neutralize the effects of multiple warheads"? George Quester, a doctoral student at Harvard writing his dissertation on the distinction between counterforce and "countervalue" (or counter-city) targeting in the history of strategic air power, agreed and said it was MIRV, rather than ABM, that might "raise the stability problem again." If ABM could protect missile sites—securing the fabled "secure second-strike" force—then Stone's ABM ban "would prevent this insurance against instability." Iklé agreed and beat up on Stone's proposal for similar reasons. Apparently Stone "doesn't seem to be interested in reducing destructive potential, since ABM is ruled out," he charged. And when MIRV came along, Iklé pointed out, freezing missile numbers would only invite each side to pile more warheads into each missile.⁹³

On Zoppo, see "University of California: In Memoriam, 2000," available at <http://texts.cdlib.org/view?docId=hb1r29n709>. Kaufmann had given Robert McNamara his first taste of counterforce logic in a famous 1961 briefing, a few weeks into McNamara's tenure as the new Secretary of Defense. And he wrote McNamara's famous "no cities" speech, announcing the counterforce doctrine at the University of Michigan's commencement ceremonies in 1962. On Kaufmann's ideas and his role in the McNamara Pentagon, see Kaplan, *The Wizards of Armageddon*, esp. 248-262; and William W. Kaufmann, *The McNamara Strategy* (New York: Harper & Row, 1964).

⁹³ Iklé's comments were an eerie forecast of what would actually happen under the SALT I Interim Agreement of 1972, which restricted numbers of launchers without restricting MIRV or the numbers of deployed warheads. "Joint Arms Control Seminar, Minutes of the Twelfth Session, May 10, 1965," Box 3, Folder 5 "Joint MIT-Harvard Arms Control Seminar - Minutes, 1964-65," *LPB*. Also see George Herman Quester, "Counterforce,

MIRV had ominous implications, even to some of the RAND vets, but ABM would be the first to take pride of place on the arms control agenda in the second half of the 1960s. Not by logical necessity, but because missile defense would find itself searchlights of public scrutiny and controversy. Many of the arms controllers joined it there. MIRV was, by nature, small, hidden, and relatively cheap; but there was no hiding the cost and physical sprawl of an ABM system. In 1965, still ensconced in the well-appointed Harvard Faculty Club, William Kaufmann mused to his seminar colleagues that “it was not clear how ABM would turn out, but...it was likely to become a political problem within the next two years.” An offhand remark, but a forecast truer than Kaufmann could have known.⁹⁴

II. Arms Control in Public, 1967–1970

The ABM Debate Takes Shape

In a speech before editors and journalists at United Press International in San Francisco on September 18th, 1967, Robert McNamara announced the administration’s intention to deploy an ABM system. Oddly, most of the speech was devoted to explaining why missile defense was a bad idea. Here was McNamara preaching the gospel of offense dominance: “Any such system can rather obviously be defeated by an enemy simply sending more offensive warheads, or dummy warheads, than there are defensive missiles capable of disposing of them.” Right there, he said, in the language of one schooled in the theory of arms races, “was the whole crux of the nuclear action-reaction phenomenon.” No matter how much the U.S. spent on missile defense,

Countervalue: The Early History of a Distinction in Air Strategy, 1900-1945” (PhD dissertation, Harvard University, 1965).

⁹⁴ “Joint Arms Control Seminar, Minutes of the Twelfth Session, May 10, 1965,” Box 3, Folder 5 “Joint MIT-Harvard Arms Control Seminar – Minutes, 1964-65,” *LPB*.

the Soviets would build more missiles and cancel out any gains. Among McNamara's Pentagon analysts this idea had been codified in a number called the "cost-exchange ratio," a version of the exchange ratio expressed in dollar figures rather than numbers of missiles. The cost-exchange ratio measured how many dollars the offense would have to spend to offset every dollar spent by the other side on missile defense (where the level of "assured destruction" inflicted by the offense was held constant). In the 1960s most of the Whiz Kids felt that the cost-exchange ratio was too low to warrant ABM deployment—it would be too cheap for the offense to outwit and overwhelm the defense.⁹⁵

But suddenly, near the end of the speech, McNamara turned on his heels and proposed going forward with an ABM program. It was to be a "thin" system, based on the Nike-X concept of long- and short-range interceptors guided by phased-array radar, defending large areas of the country (including both cities and strategic forces) from a "light" ballistic missile attack. But the system was not meant to defend against the Soviet Union: it was meant for China, which McNamara said would soon be able to deliver a "light" ICBM attack, without sophisticated countermeasures.⁹⁶

The speech was a strange brew. McNamara pitched it at a remarkable degree of abstraction for a public statement by a high official. Wearied and ponderous, it wouldn't have been entirely out of place in a meeting of the arms control seminar. It endorsed an ABM system

⁹⁵ The term "assured destruction," much used in the middle and later years of McNamara's tenure at the Pentagon, indicated the firepower required to inflict an "unacceptable" level of damage on Soviet society and military forces. In numerical terms, assured destruction meant the ability to destroy somewhere in the range of 20-33 percent of Soviet population and 50-75 percent of its industrial capacity (often as measured in terms of "industrial floorspace"). See Freedman, *The Evolution of Nuclear Strategy*, 32-34.

⁹⁶ The idea of a "thin" system protecting against a "light" attack by China was afoot as early as April 1967. See J.I. Coffey, "The anti-ballistic missile debate," *Foreign Affairs* 45, no. 3 (April 1967): 403-413. In *The New Yorker* Calvin Trillin poked fun at the DOD's description of the "primitive" attack China could muster, "conjuring up visions of thousands of Chinese peasants laboriously carting the mud of the Yangtze to crude molds, creating out of the baked earth something that roughly resembles an intercontinental ballistic missile, straining together to pull it back on some enormous catapult, and launching it seven thousand miles over the Pole in an attempt to obliterate Chicago." Calvin Trillin, "U.S. journal: Lake County, Ill.," *The New Yorker* (15 February 1969): 100-106, on 101.

that was noxious to arms controllers (a “light” area defense against China) without being the *most* noxious (a “heavy” area defense against the Soviet Union). “There is a kind of mad momentum intrinsic to the development of all new nuclear weaponry,” McNamara told the journalists. “The danger in deploying this relatively light and reliable Chinese-oriented ABM system is going to be that pressures will develop to expand it into a heavy Soviet-oriented ABM system.” The sense of whiplash one gets from the speech can be attributed in part to McNamara’s own embattled position: never a supporter of missile defense, he had been forced into the decision by domestic politics more than considerations of security or strategy. He was also on his way out of the Pentagon. A more immediate culprit was Morton Halperin, the early Cambridge arms controller who had joined the Pentagon as Deputy Assistant Secretary of Defense for Arms Control and Policy Planning. In 1960 with Thomas Schelling, Halperin had written that the arms race was an “interaction between two or more adversaries’ military programs [and] a tendency for each side’s program to respond to what the other is doing,” reciting the basic premise of arms race theory. Now in 1967, using the same language of action and reaction, Halperin had written every word of McNamara’s ABM announcement speech.⁹⁷

The arms controllers had been preparing anti-ABM arguments for years, but the news of its deployment began to pull them out into greater visibility. A new shrillness crept into the debates. Jerome Wiesner found McNamara’s speech “a very curious presentation,” especially since, not two weeks before, he and every other current and past Presidential science advisor and DDR&E had recommended against deployment in a special meeting with Lyndon Johnson,

⁹⁷ Thomas C. Schelling and Morton H. Halperin, *Strategy and Arms Control* (New York: The Twentieth Century Fund, 1961), 34; Halperin, “The decision to deploy the ABM”; Kaplan, *The Wizards of Armageddon*, 346-347. “In announcing this decision in September 1967,” says Lawrence Freedman, “McNamara provided an eloquent statement of his views on the dynamics of the arms race, in a manner which was to influence the defence debate for some years to come.” Freedman, *The Evolution of Nuclear Strategy*, 240. Freedman mentions only in a footnote that the speech had actually been written by the noted arms controller Morton Halperin (see *The Evolution of Nuclear Strategy*, 487, n. 17). In other words he attributes this view of the arms race to McNamara himself, rather than to the expert community that had developed it since the late 1950s.

convened by McNamara. Wiesner, of course, had decided against missile defense while still Kennedy's science advisor, and during the intervening years he'd soaked up the arms race argument in drawn-out discussions of the arms control seminar, among other places. "As I thought about the situation," he later wrote, "I felt that I could not remain silent about what I believed to be a most egregious action on the part of the administration." In the past he had felt that the public airing of dirty government laundry was not "proper"; he agonized over whether to go public with his criticism, possibly opening a breach that would reduce his future influence. In the end he decided that to remain silent was to acquiesce—and so he went public, in a big way. His views hit escape velocity from the Cambridge-Washington orbit as he hastily wrote and placed an article in the popular biweekly *Look* magazine in November 1967, instantly reaching the largest readership of his career.⁹⁸ It was a quick and dirty summary of the basic anti-ABM considerations, but he laid heaviest stress on the claim that ABM was an arms race stimulant. He spoke of "a powerful incentive...for either side to increase its offensive-missile forces the moment the other starts to build an ABM system." Wiesner doubted that the U.S. could protect itself against China "without starting a new Russian-American arms spiral." Just as the U.S. and the Soviet Union had learned to do, the U.S. and China would now have to coexist under a "deterrent balance," a "nightmarish peace insured by only a balance of terror"—at least until significant disarmament was achieved. "There is just no way to avoid this; there is no magical or technical escape from the dilemmas of the nuclear age through defense." The article caught wide notice inside the government and out, even warranting insertion into the record of hearings on ABM of the Joint Committee on Atomic Energy.⁹⁹

⁹⁸ *Look* had circulation figures of several million readers in the 1960s. N.R. Kleinfield, "Looking back at *Look*," *New York Times* (6 February 1983): F6.

⁹⁹ Rosenblith, ed., *Jerry Wiesner*, 300-304; Jerome B. Wiesner, "The case against an antiballistic missile system," *Look* (28 November 1967): 25-27, quotations on 26 and 27.

More than any other issue, ABM began to exaggerate long-present differences within the arms control and strategy community concerning the nature and requirements in nuclear deterrence. In a 1967 session of the arms control seminar, Donald Brennan, visiting from the Hudson Institute, illuminated the terrain of debate in the following way. Supposing the cost-exchange ratio for a given level of assured destruction was very small, say 1:100 (that is, it would cost one hundred times as much to implement a missile defense system as it would for the offense to “nullify” it), then there would be no debate: no missile defense would be deployed by a rational policymaker, because its cost far outstripped its benefit. That was where things had stood in the late 1950s, he said. Likewise, if the cost-exchange ratio were reversed at 100:1, then missile defense was the undisputed winner. As far as Brennan was concerned, defensive technology had improved since the 1950s to the point where the cost-exchange ratio had slid into the ambiguous region around 1:1, where Brennan said the same dollar might just as well be spent saving American lives as threatening Soviet ones. “Today deterrence is something like the model of two scorpions in the bottle,” he said, adopting Robert Oppenheimer’s iconic metaphor from an earlier era. “But this is due to technical history. And the new exchange ratios may change the basis of assured destruction reasoning.” The cost-exchange ratios derived from McNamara’s own figures now militated against what Brennan had become fond of calling the “McNamara freeze”—banning ABM but leaving offensive forces unrestrained—and instead favored the “Soviet freeze”—holding offensive forces down while allowing both sides to develop ABM to their hearts’ content.¹⁰⁰

Brennan spoke and wrote with the passion of a convert. Back in 1960 he had sided with the partisans of deterrence—first with Wiesner’s variety of minimum deterrence, later with

¹⁰⁰ The proposal to allow ABM while limiting offense was more or less the official Soviet position at the time. “Joint Arms Control Seminar, Minutes of the Eighth Session, March 20, 1967,” Box 3, Folder 7 “Joint Harvard-MIT Arms Control Seminar – Minutes, 1966-67,” *LPB*.

Schelling-style deterrence stability. Back then he had regarded the defensive technology as dubious. So strong was his skepticism that he had sublimated it into comedy, mocking the ambitious designs of ARPA's Project Defender by proposing his own ridiculous "Project Turnabout." (His flair for the outrageous would make him a good fit for Herman Kahn's think-tank in later years.) In a document from 1958 jokingly labeled "ULTRA SECRET!" Brennan described a "large array of rigidly fixed rocket engines uniformly distributed in a band about the earth's equator, all pointed tangent to the earth's surface." When one side launched an ICBM toward the other, the equatorial rockets would fire, causing the planet to turn more rapidly on its axis. "By suitable control of the rocket thrust, the earth can be rotated 180° between the time of detection and the time of impact. The missile would, therefore, land on the enemy's own territory, and contribute to his own destruction."¹⁰¹

Brennan then dove giddily into a series of absurd calculations, finding that a mere 10¹⁹ (ten billion-billion) rocket engines could accomplish the job of spinning the globe a half-revolution during the time-span of an ICBM flight, each rocket providing two million pounds of thrust. Combined, they would require an amount of fuel weighing three times the mass of the earth; naturally "some new type of super-exotic fuel will be required." And the centrifugal forces during the turnabout would be so great that some valuable items—"vehicles, personnel, buildings, the oceans, the top several hundred miles of the earth's crust, the atmosphere, etc."—would be flung permanently into the void of space. Brennan's recommendation: a preliminary R&D program funded at ten times the gross national product of the United States. Whether the management at Lincoln Laboratory (Brennan's employer at the time, which had a major ARPA

¹⁰¹ D.G. Brennan, "Project Turnabout—An Effective ICBM Defense," 15 October 1958, Box 8, Folder 256 "Daedalus Meeting May 20, 1960," *JBW*.

contract to design radars and characterize reentry phenomena for the U.S. missile defense program) ever caught wind of the hoax is unclear.¹⁰²

And yet here he was in the late 1960s extolling the virtues of missile defense. What had happened? Brennan himself dated his conversion to the middle of 1964, when he had come by an “improved understanding of Soviet perceptions of these matters.”¹⁰³ (Jeremy Stone guessed that Brennan had left the flock in 1965 after making a trip to the Soviet Union, whereupon he was seduced by Soviet defensive ideas and—not coincidentally, according to Stone—by an attractive female Soviet strategic analyst.¹⁰⁴) Perhaps over several years he had finally crumbled beneath the accumulated weight of Herman Kahn’s arguments, or the sheer force of Kahn’s personality. In any event, Brennan by 1967 had shed his youthful technological skepticism. “Many of us ‘grew up’ in the business of strategic analysis in the late 1950s assimilating the proposition ‘Since we [the United States and the Soviet Union] cannot defend, we must deter,’” Brennan wrote in 1967, “a partially true dictum imposed by the technology of the era. But this seems to have become solidified in some minds in the distorted form ‘Since we must deter, we cannot defend’. This is plainly false.” By the late 1960s Brennan had come to feel a profound aversion to the traditional idea of nuclear deterrence—Schelling’s mutual hostage scheme—especially now that the Soviet deterrent force was growing at a speed to match that of the United States.¹⁰⁵

¹⁰² D.G. Brennan, “Project Turnabout—An Effective ICBM Defense,” 15 October 1958, Box 8, Folder 256 “Daedalus Meeting May 20, 1960,” *JBW*.

¹⁰³ Donald G. Brennan, “The case for population defense,” in Johan J. Holst and William Schneider, Jr., eds., *Why ABM?: Policy Issues in the Missile Defense Controversy* (New York: Pergamon Press, 1969), 91-117, on 93; and “Statement by Donald G. Brennan before the Subcommittee on International Organization and Disarmament Affairs of the Committee on Foreign Relations, U.S. Senate,” 28 March 1969, Box 45, Folder 8 “Antiballistic Missile (ABM), Testimony 1969; 1984-1987, Testimony and Statements,” *HFY*.

¹⁰⁴ Stone, *Every Man Should Try*, 15.

¹⁰⁵ D.G. Brennan, “Post-deployment policy issues in ballistic missile defense,” *The Adelphi Papers* 7, no. 43 (1967): 1-23, quotation on 11. Also see D.G. Brennan, “New thoughts on missile defense,” *Bulletin of the Atomic Scientists* 23, no. 6 (June 1967): 10-15. Brennan had started to lay out his objection to assured destruction in Hudson Institute reports as early as 1966. See D.G. Brennan, “Whither Assured Destruction?, and Other Questions of BMD Deployment,” Hudson Institute Discussion Paper HI-772-DP, 4 November 1966; and D.G. Brennan, “A Start on Strategic Stabilization,” Hudson Institute Paper HI-1076-P, 1 August 1968.

Brennan was not alone among strategic thinkers in his growing discomfort with the doctrine of assured destruction.¹⁰⁶ Since the mid-1960s Freeman Dyson had come all the way around to the view that the U.S. should not only *not* ban ABM systems, but should *encourage* the Soviets to join the U.S. in pursuing ABM. Dyson had first worked out his opinions during summers spent in ACDA's science and technology bureau in 1962 and 1963. Poring over Nikita Khrushchev's speeches and statements, he grew convinced that the Soviet ABM "was only the latest example of a long Soviet tradition of defense-by-bluff—the exploitation of advanced weapons of dubious military value for political and psychological purposes." At the end of the summer of 1962, Dyson wrote an ACDA memorandum arguing (as he paraphrased it) that "the United States should strive by every means in its power to sustain and buttress the Soviet ABM bluff.... We should not contest Khrushchev's claims of technological superiority in this area." Let them spend their money on defense. Why encourage them to spend it on missiles targeted at American cities instead?¹⁰⁷

Dyson, unlike Brennan, had *always* been troubled by deterrence-by-threat, calling the idea of stability a "dogma" that among arms controllers had "come to be identified with the notion of the supremacy of the offensive." Brennan clung to the hope that the arms race could be brought under control. Dyson did not—at least not short of radical political change. "Until we can establish an international control over military research and development," he wrote in 1964, "the choice of stopping the technological arms race does not seem to be open to us." In his opinion the offense-defense competition was "a never-ending one," a perpetual cycle of one

¹⁰⁶ In fact Brennan believed, improbably, that "a majority of the most prominent American academic strategists" were beginning to favor ABM. Brennan, "Post-deployment policy issues," 2.

¹⁰⁷ He added: "If, following our past pattern of behavior, we were to talk the Soviet leaders into abandoning their ABM system, we would be compelling them to transfer vast technological resources from a harmless defense-by-bluff into far more dangerous, because militarily effective, weapons systems." See Freeman Dyson, "Disturbing the Universe—III," *The New Yorker* (20 August 1979): 36-80, on 62 and 64.

technically sweet development after another. The question was which side of the technological coin to land on. Dyson thought the Soviets preferred defense because their military thinkers emphasized long wars of attrition; the American strategists who preferred offense based their thinking on short wars, imaginary nuclear exchanges in which the dust would settle quickly. It was a clever deconstruction of the strategic orthodoxy, and Dyson would become one of the most articulate defenders of missile defense in the 1960s.¹⁰⁸

Of course the arms controllers—even the strategy types—would not allow such arguments to go unchallenged. When Brennan aired his views at the Harvard-MIT arms control seminar, the attendees gave him a vivid preview of the struggle that lay ahead. Thomas Schelling and Fred Iklé pointed out that Brennan’s view rested on the shaky ground of a strategic missile freeze, which they found quixotic. George Quester offered the standard anti-ABM arguments from instability (of the arms race and crisis varieties). Robert Jervis, a visiting doctoral student from Berkeley, said that ABM made “demonstration attacks” (“small” nuclear strikes meant to signal intent rather than inflict serious damage) impossible, hobbling what was thought to be an important limited war tactic. Most remarkable was the transformation of William Kaufmann—once the high priest of the strategies of damage limitation and counterforce—who now came out swinging against ABM, rebutting Brennan with a defensive arms race argument right out of Jeremy Stone’s playbook.¹⁰⁹

* * * *

In the months ahead, however, the venues for discussion of the ABM and the nuclear arms race continued to shift. Debate spilled out from think tanks and closed-door university

¹⁰⁸ Freeman Dyson, “Defense against ballistic missiles,” *Bulletin of the Atomic Scientists* (June 1964): 12-18, quotations on 14 and 16.

¹⁰⁹ “Joint Arms Control Seminar, Minutes of the Eighth Session, March 20, 1967,” Box 3, Folder 7 “Joint Harvard-MIT Arms Control Seminar – Minutes, 1966-67,” *LPB*.

seminars to the rough-and-tumble of public battle. In December of 1967, at the annual meeting of the American Association for the Advancement of Science in New York City, a nuclear physicist and arms controller named Leonard Rodberg organized a panel asking, “Is defense against ballistic missiles possible?” Rodberg thought not. He had arrived at this conclusion while working as chief of policy research in ACDA’s science and technology bureau from 1961-66 (ACDA was on its way to becoming the most forceful voice of ABM opposition in the government).¹¹⁰ For the panel, Rodberg invited several experts on both sides of the ABM question, including George Rathjens, Hans Bethe, and Richard Garwin on the “no” side, and Donald Brennan and Freeman Dyson arguing “yes.” Gerald Piel, the editor of *Scientific American*, was in the audience and, at the end of the session, pulled Garwin and Bethe aside as they stepped off the platform. If they would agree to quickly write an article together, he told them, combining Garwin’s strategic points with Bethe’s physical arguments against ABM, he would publish it immediately. Garwin and Bethe agreed, and got to work drafting the piece over the next few weeks.¹¹¹

Their article would become the standard rebuttal of area missile defense, built entirely from (as the authors put it) “nonsecret information.” Yet Bethe and Garwin did not point out that they had done a lot more than rebut ABM with the laws of physics (nor, for that matter, has any subsequent scholar). They had worked for years—within the government and especially for the research lab of the AVCO Corporation—on the problems of missile reentry, decoys, and missile kill mechanisms. This was expertise born of tactile experience in highly specialized institutions.

¹¹⁰ See, for example, Leonard S. Rodberg, “ABM – Some arms control issues,” *Bulletin of the Atomic Scientists* 23, no. 6 (1967): 16-20. “The conclusion seems inescapable that the deployment of ABMs on either side would lead to a corresponding defense buildup on the other,” said Rodberg.

¹¹¹ Walter G. Berl, “134th AAAS annual meeting, New York City, 26-31 December 1967,” *Science* 158, no. 3806 (1967): 1342-1367, on 1345-1346; Walter Sullivan, “Scientists’ parley debates missile defense merits,” *New York Times* (27 December 1967): 27; Garwin interview, 1987.

Bethe had worked on shock waves and warhead/decoy discrimination at AVCO since the late 1950s, and had recruited Garwin to the same work a few years later. Little wonder, then, that his half of the essay, which focused on “the physical mechanisms by which an ABM missile can destroy or damage an incoming warhead” (Garwin’s half focused more on the strategic and arms control dimensions), described in arresting detail how a typical reentry vehicle would fare in the presence of a thermonuclear blast in the upper atmosphere, and an extended discussion of how a nuclear fireball produces blackout.¹¹²

The insight that when “X-ray energy falls on a reentry vehicle, it will cause the surface layer of the vehicle’s heat shield to evaporate” is not the sort of thing that falls out of Maxwell’s equations of electromagnetism—nor the subsequent idea that “the vapor leaves the surface at high velocity in a very brief time and the recoil sets up a powerful shock wave in the heat shield,” destroying the vehicle in a hail of charred metal. Bethe was no slouch as a physicist, his already impeccable credentials perfected by the 1967 Nobel Prize in Physics for explaining the nuclear processes behind stellar energy production. But on this occasion he spoke less like someone who had peered into nature’s mysteries, and more like a contract consultant who had peered into a shock tube at the AVCO-Everett facility. So rich in sensitive detail was the article that Bethe later said he had shared his AAAS talk with the DOD a full two months before delivering it in New York, to guarantee that no classified information was inadvertently released. Then he spent ten days harassing defense officials over the phone to get them to clear it in the lead-up to the conference. Piel, in any event, was pleased with the result, and published it in

¹¹² Richard L. Garwin and Hans A. Bethe, “Anti-ballistic missile systems,” *Scientific American* 218, no. 3 (1968): 21-31, quotation on 26.

March 1968. It was a touchstone document for ABM opponents and arms controllers for years to come.¹¹³

The year 1968 also saw the first public cage match on the subject of ABM. The Center for the Study of Democratic Institutions—a progressive nonprofit created in 1959 by the famed former University of Chicago president Robert Maynard Hutchins—arranged for a debate on the ABM before an open audience on a November evening at the New York Hilton Hotel. The question: “Anti-Ballistic Missile: Yes or No?” Hutchins asked Jerome Wiesner to give “as full and objective a summary and assessment of the technical data about ABM as can be crowded into half an hour,” before Senator George McGovern would deliver a brief for the “no” side, followed by physicist Edward Teller with his case for ABM.¹¹⁴ When Teller had to cancel, Donald Brennan jumped at the chance to spar with his old arms control colleague. After the *Look* article, Wiesner was now sufficiently well known as an ABM critic that his role as impartial technical appraiser “was objected to by virtually every proponent” of ABM Hutchins had approached; so Hutchins told Wiesner to drop the façade of technical objectivity and make “as forthright a statement as you might care to make about the folly of ABMs.”¹¹⁵

Wiesner’s remarks were light on analysis and heavy on “judgment”—a necessary ingredient, he said, for reaching any conclusion about a complex system like ABM. “The analysis...is very important in allowing you to make a judgment, and giving you some feeling for where you can go and where you can’t go.” He argued for the unknowability of raw technical

¹¹³ Garwin and Bethe, “Anti-ballistic missile systems,” on 27. Garwin later said of this article that it “was probably my first real open publication in science policy,” the start of a long and active career in public criticism. Garwin interview, 1987. On Bethe’s efforts to clear his portion of the talk/article, see Cahn, “Scientists and the ABM,” 90-91. Cahn reports a former member of the Atomic Energy Commission stating that “the Bethe-Garwin article was a shockeroo to me because it divulged so much.”

¹¹⁴ Robert M. Hutchins to Jerome Wiesner, 25 September 1968, Box 19, Folder “ABM – Yes or No?,” *JBW*. On the Center for the Study of Democratic Institutions, see Miles Corwin, “Santa Barbara think tank to move to West L.A.,” *Los Angeles Times* (2 December 1987).

¹¹⁵ Wiesner quoted these remarks directly in Jerome Wiesner, “Unedited extemporaneous speech,” Box 19, Folder “ABM – Yes or No?,” *JBW*.

fact and the constructed nature of the claims. “We don’t have the knowledge... about many things that go into [ABM], and so what we do is pick some numbers, which seem rational, and we use them,” he told the audience. Not only would one set of assumptions (about the reliability of ABM system components, e.g.) lead toward one conclusion, and other assumptions toward another, “some of the assumptions are essentially un-definable.”¹¹⁶ Wiesner claimed to know nothing much about how well the ABM—“probably the most complicated electronic system...that anyone’s ever tried to build”—would work, but he did know that the claims made for it were overblown. On cue, he summarized the ways ABM could be deceived by an attacker. Its defenders said ABM was meant to shift the strategic seesaw toward defense and away from offense. Wiesner doubted this very much. Deterrence was an iron law that each side was bound to obey. “I think a point would come in the development of the defensive system where people would argue that the offenses no longer can guarantee deterrence and you would have to go off in the other direction,” toward the pursuit of even more offensive weapons.¹¹⁷

Donald Brennan took the podium and mischievously flipped Wiesner’s argument-from-ignorance on its head. Yes, the defense might “perform worse than predicted”—but it might also “perform very much better”! He rendered his RAND-style cost-exchange ratio argument in more audience-friendly terms. “If I were convinced that improved ABM defenses could be neutralized for a minor fraction of their cost...I would stop being interested in defense myself.” But the numbers now, with the improved Nike-X design, pointed the other way. Brennan was just as worried about the arms race as Wiesner, he protested. “Would the ABM be likely to lead to an

¹¹⁶ He continued: “Namely we’re talking about things we don’t know anything about. And so you can take the set of assumptions you want. Now, of course it gets even worse than that. If that isn’t bad enough. Because when we design a system, and then analyze it, we usually have to analyze sort of idealized conditions, namely we assume it’s going to work like specified.... This is an untestable system, in my opinion. This is a judgment I’m offering you, not a technical fact.” Jerome Wiesner, “Unedited extemporaneous speech,” Box 19, Folder “ABM – Yes or No?,” *JBW*.

¹¹⁷ Jerome Wiesner, “Unedited extemporaneous speech,” Box 19, Folder “ABM – Yes or No?,” *JBW*.

unending spiral of defense followed by offense, followed by defense...and so on?" Well, no. It would only if the country remained—as so many ABM opponents seemed to be—committed to assured destruction as the criterion of deterrence. But assured destruction was an invention, not “a fundamental requirement of nature.”¹¹⁸

The gloves came off during an expert panel the following morning. Freeman Dyson traveled up from Princeton and delivered the spiciest remarks of the day. He dismissed “the paper wars that are constantly being fought in the community of scholars” and said that the case against ABM was often simplistically presented—as though anyone really believed that ABM promised a perfect defense. Of course it didn’t. It served the purpose of reducing destruction, not eliminating it (and certainly not “assuring” it). “If you are sitting in a city that is not attacked”—because a missile defense system had forced the attacker to unload more of its available force on a different target—“the defense has worked as far as you are concerned, even if you did not solve the decoy discrimination problem.” If the panel wanted to get worked up about something, it should talk about MIRV: “I consider it the most dangerous thing that is coming out,” he remarked, echoing the words of Jack Ruina’s classified letter to George Rathjens four years earlier.¹¹⁹ This precipitated an ugly exchange with Wiesner, who felt that Dyson had made a straw man out of him, a perverse caricature of a deterrence enthusiast. So ugly, in fact, that Dyson was compelled to write Wiesner a letter immediately after returning home in the evening. “What I would like you to understand is that the passion which drives me, which came to the surface in my remarks this morning, and which originated in my years at the R.A.F. Bomber Command Headquarters in World War II, is an unquenchable hatred for strategic offensive

¹¹⁸ Freeman J. Dyson to Jerome B. Wiesner, 20 November 1968, Box 19, Folder “ABM – Yes or No?,” *JBW*. On the history of the political uses of scientific and technological ignorance, see, e.g., Robert Proctor and Londa Schiebinger, eds., *Agnotology: The Making and Unmaking of Ignorance* (Stanford, CA: Stanford University Press, 2008).

¹¹⁹ *Anti-Ballistic Missile: Yes or No?*, 97-101, quotations on 98 and 99.

forces and for all men who take pleasure in them.”¹²⁰ Dyson had clearly worked out for himself the institutional sinews of his own intellectual and moral commitments. Wiesner, for his part, was scandalized, “shocked beyond belief to be accused of being an advocate of offensive missile systems.... [W]hatever you intended to say, your remarks so upset me that I found it very hard to participate in the panel. I regret that even more than the fact that I said some intemperate things to you.”¹²¹

Perhaps the exposed setting of the debate had needlessly fanned the flames. Dyson personally hoped that he and Wiesner would have a chance to talk about ABM “in a less public forum.” The Senator from South Dakota, however, recognized that the event at the Hilton had marked a watershed. McGovern said he was “grateful for the public discussion” of ABM. Defense decisions in the past had “been made by the so-called experts without the light of day focused on them.” Now ABM and arms control were coming out into the harsh, glaring light of day.¹²²

Arms Control in the Suburbs

Not long after McNamara’s ABM announcement speech in 1967, the Army began to turn the DOD’s plans into reality. The “Sentinel” system, as the ABM had been christened, was to comprise fifteen to twenty sites around the United States, each protecting a large area including major cities. In 1967 and 1968, before the site locations were announced, some residents of cities around the country said they hoped to be included in the plan. In Boston the *Globe* touted the

¹²⁰ Freeman J. Dyson to Jerome B. Wiesner, 20 November 1968, Box 19, Folder “ABM – Yes or No?,” *JBW*.

¹²¹ Jerome Wiesner to Freeman J. Dyson, undated, Box 19, Folder “ABM – Yes or No?,” *JBW*. Wiesner would later say of Dyson that his “capacity for consistent thinking I had always found questionable.” Rosenblith, ed., *Jerry Wiesner*, 305.

¹²² *Anti-Ballistic Missile: Yes or No?*, 72. Hutchins had originally discussed holding the expert panel on the second day in a television studio in front of the cameras, but the event was ultimately untelevised. Following their nasty exchange, Dyson and Wiesner must have been relieved.

economic benefits of the ABM, reporting that “the local installation would employ...700 to 1000 persons, about a third of them civilian electronics experts, and have a payroll of about \$3.5 million a year.”

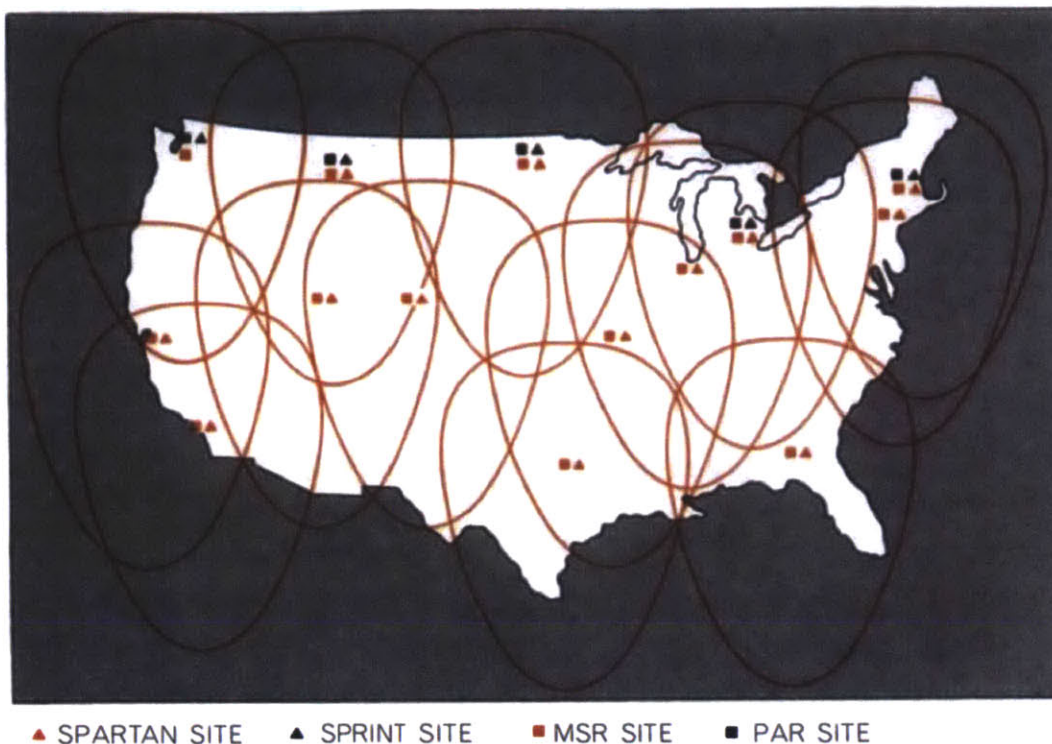


Figure 3.2: The Sentinel system. The antiballistic missiles were comprised of the long-range, larger-yield Spartan, and the short-range, high-acceleration Sprint. The PAR (perimeter acquisition radar) would recognize a missile attack as it came over the pole; the MSR (missile site radar) would guide the Spartans and Sprints to their targets. Unlike other planned sites, the Sentinel battery in Massachusetts was to include all four of the ABM components: Spartans and Sprints, a PAR and an MSR. The lines indicate areas of coverage by each installation. Taken from Herbert F. York, “Military technology and national security,” *Scientific American* 221, no. 2 (1969): 17-29.

By May of 1968 the Army Air Defense Command had narrowed down the list of possible ABM sites in Massachusetts, from seven to two. The Army Corps of Engineers in Waltham began making borings for soil tests at Sharpener’s Pond in North Andover and at Camp Curtis Guild, a parcel of land owned by the Massachusetts Army National Guard stretching across four

separate townships in the suburbs just north of Boston.¹²³ Army officials planned an information meeting at town hall in Reading, one of the townships directly adjacent to the Camp, two weeks later. Peace activists from Massachusetts Political Action for Peace took time from their anti-Vietnam War campaigning to help form a local citizens' committee on the ABM. The committee gathered with selectmen from several of the surrounding communities to decide on an agenda for the discussion with the Army. The troubled Lynnfield town officials were unsure of the line between local interest and national duty, declaring that "unless the national defense is seriously affected, the board would oppose the location in Lynnfield."¹²⁴

At the town hall meeting a few days later, an Army colonel in charge of site acquisition told roughly 100 area residents that the Army was planning to build on the Lynnfield site.¹²⁵ That same month the Army invited representatives of several ABM site communities from around the country to tour some of its Air Defense Command installations—part of what it was calling "Operation Understanding." At the White Sands Missile Range in New Mexico, small-town politicians learned about phased-array radar and met some of the people who operated the huge devices. "These men are highly trained professionals," wrote one town official from Malden, Massachusetts, "exactly the type of person you would expect to find living in Wakefield, Reading, Lynnfield or North Reading," perhaps working at one of the area's high-technology firms along the Route 128 corridor. Asked by some of his fellow residents whether "the Sentinel system location would make a town a target for enemy missiles," the official reassured them, wrongly, that Sentinel was a simple upgrade to the existing air defense system. "Its job is to

¹²³ "2 nearby sites studied in U.S. missile defense system," *Boston Globe* (5 May 1968): 95.

¹²⁴ "Lynnfield," *Boston Globe* (24 May 1968): 18. On local citizen opposition to ABM in Massachusetts, see the brief discussion in Lily D. Geismer, "Don't Blame Us: Grassroots Liberalism in Massachusetts, 1960-1990" (PhD dissertation, The University of Michigan, 2010), 266-272.

¹²⁵ "Lynnfield," *Boston Globe* (29 May 1968): 22.

detect and destroy enemy aircraft,” he said, and “it would take a saturation missile attack” to knock Sentinel out. Operation Understanding had its limits.¹²⁶

In June the Sentinel program survived a bipartisan proposal in the Senate to amend the Defense Appropriations Bill, a move that would have eliminated funding for ABM. Introduced by Senators Philip Hart and John Sherman Cooper, and supported strongly by outspoken Vietnam War and defense spending critic Mike Mansfield, the Hart-Cooper amendment was part of a broader Congressional attack on Pentagon spending in 1968. ABM survived by a vote of 52 to 34. By September the Army had released more details about its plans in Massachusetts: the Spartan and Sprint missiles would be spread throughout Camp Curtis Guild alongside an accompanying Missile Site Radar, and the Perimeter Access Radar facility was to be located at the North Andover site. Town selectmen in Lynnfield, close to where the missiles would live, were unconvinced by the Army’s propagandizing. In the opinion of one North Reading resident, “these communities will be exposed to extremely hazardous propellants, explosives and radioactive materials—the latter directly astride the important Ipswich River watershed.” Lynnfield town officials now tried to use property rights to block construction by purchasing the portion of Camp Curtis Guild lying within the Lynnfield border.¹²⁷ In North Andover, as a local construction firm under Army contract began to bulldoze an access road into the woodland at Sharpener’s Pond, Army officials told an audience at North Andover High School about plans to

¹²⁶ Walter Bilowz, “Sentinel system,” *Boston Globe* (20 June 1968): 20. Operation Understanding calls to mind similar Air Force efforts to improve military-civilian relationships in the Minuteman missile fields of the Great Plains, where as part of an Air Force educational program, local landowners were sometimes invited to tour facilities and watch test missile launches at bases as far away as California. Heefner, *The Missile Next Door*, 128.

¹²⁷ Rachelle Patterson, “Missile site opposition lingers,” *Boston Globe* (13 September 1968): 48. The politics of property rights (though on a larger geographical scale) was used by ranchers in the northern plains to fight the acquisition of land for the basing of the Minuteman missile in the early 1960s. See Heefner, *The Missile Next Door*, esp. chapter 4, “Cold War on the Range,” 77-110.

begin clearing 300 acres of forest by the winter. Massachusetts was to have the first ABM site in the United States.¹²⁸

Against this backdrop of increased public concern, the Senate went into a rare closed session at the beginning of October to consider canceling Sentinel. Wiesner wrote Senator John Stennis, chairman of the Senate Armed Services Committee, and complained that he had been criticized for not testifying. How could he testify when he hadn't been informed of the secret hearings? So he put on the record his "willingness to appear before your Committee on matters related to the antiballistic missile system or on more general questions of the development and deployment of strategic weapons systems..."¹²⁹ The editors of the *Boston Globe* complained about the secrecy, too. "It is hard enough for the average American to understand the complications of defense in the nuclear age with all the facts at hand. When the Senate goes into secret session to talk it over, we are more confused than ever." The *Globe's* science editor Victor McElheny had started reporting on nuclear security and arms control issues just a few months earlier, and soon became the voice of arguments against ABM in the newspaper, developing ties with arms control experts around the Boston area. "Such defense experts as Dr. Jerome B. Wiesner," ran the editorial, had argued that "de-escalating the missile race would seem to be a more rational step than speeding it up—just as open discussion is more rational than secrecy."¹³⁰

Wiesner had been easing himself deeper into the political fray since that summer. In July he and high-profile arms controllers like George Kistiakowsky (who had recently made a very public departure from government service in protest over the escalation in Vietnam) and Herbert York were helping aides for Senator Eugene McCarthy's presidential campaign to prepare a

¹²⁸ William Weir, "Outrageous," *Boston Globe* (4 October 1968): 16; Frank Donovan, "Bay State to have first anti-missile site in U.S.: \$475 million contract to Western Electric," *Boston Globe* (28 September 1968): 1.

¹²⁹ Jerome B. Wiesner to John C. Stennis, 16 January 1969, Box 19, Folder "ABM – Jan. 69-May 69," JBW.

¹³⁰ "Secrecy and missiles," *Boston Globe* (5 October 1968): 4.

position paper on nuclear issues. On the campaign trail McCarthy, following the recommendation of Wiesner and York, called for an ABM ban and an immediate moratorium on testing of the two new MIRVed ballistic missile systems, Poseidon and Minuteman III.¹³¹ By January of 1969, in Evanston, Illinois, where grassroots groups were beginning to organize opposition to a proposed installation near Chicago, Wiesner had taken the dais alongside several scientists from the Argonne National Laboratory to speak out against ABM.¹³²

The suburbs were stirring. Just days after the rally in Illinois, another meeting was scheduled between Army representatives and residents of the outskirts of Boston. By this point, however, Mass-PAX had circulated 20,000 flyers warning about the danger of ABM. The ABM Committee based in Newtonville distributed announcements for the meeting, to take place at the Reading High School. "Is the military establishment completely invulnerable to control by the people and their representatives, or is there a way in which we can influence its decision?" the leaflet asked.¹³³ Victor McElheny wrote in the *Globe* that a prominent group of experts would make an appearance, including Jerome Wiesner, Abram Chayes of the Harvard Law School (and a former legal advisor to the State Department), George Rathjens (who had continued his peregrinations to MIT, where he now worked as a visiting professor in the political science department), and Richard Goodwin, the former JFK speechwriter and special advisor to Lyndon Johnson. Joining them was Leo Sartori, a professor of physics at MIT who had become head of the Greater Boston Branch of the FAS. For several local FAS chapters around the country, whose activity had dwindled in the 1960s, the ABM issue was a boon. For Sartori, ABM was of personal as well as professional concern: he lived in Lexington, not far from where the missiles

¹³¹ "Candidate urges unilateral missile freeze," *Boston Globe* (11 July 1968): 16.

¹³² In Illinois the protests were initiated and led by several scientists at Argonne National Laboratory who were also affiliated with a local chapter of the FAS, including David Inglis and John Erskine. They called themselves "The West Suburban Concerned Scientists." See Cahn, "Scientists and the ABM," 71-75.

¹³³ "ABM committee," undated, Box 19, Folder "ABM - Jan. 69-May 69," *JBW*.

were to be based. One information pamphlet for anti-ABM activists recommended Sartori as the “best Boston area contact” among local experts.¹³⁴

Thirteen hundred people had crammed into the Reading High School auditorium on January 29th.¹³⁵ In contrast to the earlier meetings, the boisterous crowd was in no mood to listen impassively to another of the Army’s “information sessions.” Emboldened by the participation of the experts, they were defiant; it was village-green democracy confronting the military-industrial complex head-on. “Army representatives faced an indignant and noisy peace-oriented, standing-room-only audience in Reading High School last night,” McElheny reported in his front-page dispatch the next day. “A woman in the balcony, the mother of three boys, said she was in the ‘construction business, constructing a family.’ She said she wanted her boys to have unspoiled park land available to them, and then said the Corps of Engineers should turn its attention to construction in America’s troubled cities. At this point she was interrupted by loud and sustained applause.” Brigadier General Robert Young of the Corps of Engineers had his work cut out for him in making the case for Sentinel. After showing a series of slides illustrating the missiles and radars making up the ABM system, Young lectured the crowd on their obligation to the national interest. “When you refer to the ABM as your program, you must remember it is the nation’s program,” he said. This homily—“greeted with loud boos”—did not go over well.¹³⁶

The crowd wanted to talk about danger, not duty. They fixated on a map of the region General Young had propped up at the front of the auditorium. They demanded repeatedly that the General “show...the area that would be damaged by an accidental explosion” of one of the

¹³⁴ Victor McElheny, “Wiesner leads foes of ABM site,” *Boston Globe* (28 January 1969): 1; Victor McElheny, “Reading scene for verbal war over missiles,” *Boston Globe* (29 January 1969): 3; “ABM Committee, Minutes of the third meeting, held Monday 17 February 1969,” Box 5, Folder 5 “Correspondence 1968-February 1969,” *BBS*.

¹³⁵ This apparently exceeded the hall’s maximum capacity by more than 200 people. “ABM committee,” undated, Box 19, Folder “ABM – Jan. 69-May 69,” *JBW*.

¹³⁶ Victor McElheny, “Angry Reading crowd greets ABM delegates,” *Boston Globe* (30 January 1969): 1.

Sprint or Spartan missiles. “From the audience there were repeated cries of ‘Draw the circle,’” McElheny wrote.¹³⁷ “At this point one member of the audience said: ‘We have an expert here from the Mass. Institute of Technology who is fully qualified to answer the question. Why don’t you let him answer?’ This was greeted by applause that nearly lasted for a minute.” The expert in question was George Rathjens, who stood and told the rapt crowd that an accidental detonation of a one-megaton Spartan warhead (he chose the much larger of the two ABM warheads) would result in “nearly total destruction for a radius of five miles.” Rathjens allowed that such an accident was unlikely, but added that there was no good reason to base these missiles near cities given the drastic consequences of an accident. Again the audience erupted.¹³⁸

As the meeting ended, 500 people stayed behind to organize a protest group. The next night a group of 40 met in the Unitarian Church in Reading “to decide how to make an impression at the local level that will reverberate to the national level,” as one participant wrote. They planned to assemble a petition asking Richard Nixon to conduct an official review of the Sentinel program. The group also set up a “Forms of Protest Drive Committee,” which weighed the option of a “baby carriage brigade to be staged at the Camp Curtis Guild entrance.”¹³⁹ Over the coming days the Reading Citizens Committee Against ABM, as they came to call themselves, debated the most appropriate forms of action. Younger members wanted to march on the State House and picket the site; older members wanted to write letters and circulate petitions.

¹³⁷ Nuclear explosion damage maps had evidently achieved a wide familiarity by the late 1960s. A poster for an ABM protest meeting in Tenafly, New Jersey—another prospective Sentinel site—superimposed concentric circles over the New York City area, for example, showing the effects of a 300-kiloton fission weapon going off at the ABM battery. Blast effects would reach Englewood, New Jersey; fire lapped at the northern end of Manhattan; and “body burns” were expected in Central Park. See “What happens if a hydrogen warhead explodes at a missile base in Tenafly?,” in Box 19, Folder “ABM – Yes or No?,” *JBW*.

¹³⁸ Victor McElheny, “Angry Reading crowd greets ABM delegates,” *Boston Globe* (30 January 1969): 1.

¹³⁹ Rachele Patterson, “ABM foes won’t take a no,” *Boston Globe* (31 January 1969): 2.

In the end they agreed to affiliate with the newly formed New England Citizens Committee on the ABM, which began to embrace several smaller ABM protest groups.¹⁴⁰

Two days after the Reading confrontation, the local experts—Wiesner, Rathjens, Chayes, Goodwin, and two former aides to the McCarthy campaign—set out their case against ABM in a *Boston Globe* editorial. It was meant to be a pedagogical tract on the arms race and stability, but its authors understood that in the case of the ABM, even national security politics was local. So they framed the ABM as a danger to the local community as well as a more cosmic threat. Feeding off the anger and anxiety they had witnessed two nights prior, they insisted there was “a remote but real chance” of one of the missiles blowing up in its silo, instantly incinerating a quaint New England town. The Spartan missiles could just as effectively be located 100 miles from any major city, they said, and the location of the Sentinel sites near major cities was “explicable only in the context of the large anti-Soviet system” that experts had disparaged for years.¹⁴¹ (Jeremy Stone was so “embarrassed” by this scaremongering—“the ABM warheads were not, in my opinion, likely to go off by themselves,” he wrote, “and they were not a danger to the cities”—that he never used the tactic himself. But neither did he refute it, once he had seen how “the public reaction was extraordinarily favorable to the anti-ABM cause.”¹⁴²)

Whether one bought the argument about the risk of accidental detonation or not, the fight in Massachusetts was revealing yet another way in which the insider/outsider distinction failed to capture the expert critics of ABM. Many of the arms controllers who fought against the ABM deployment were middle-class residents of the same suburbs where the missiles and radars were

¹⁴⁰ Rachelle Patterson, “Protesters plan their defenses against site,” *Boston Globe* (2 February 1969): 49.

¹⁴¹ “The case for no ABM site,” *Boston Globe* (1 February 1969): 22.

¹⁴² Stone, *Every Man Should Try*, 26. The scaremongers may have been justified, however, given the unreassuringly shaky record of nuclear weapons safety during the Cold War. See Scott D. Sagan, *The Limits of Safety: Organizations, Accidents, and Nuclear Weapons* (Princeton, NJ: Princeton University Press, 1993); Eric Schlosser, *Command and Control: Nuclear Weapons, the Damascus Accident, and the Illusion of Safety* (New York: The Penguin Press, 2013).

to be sited. They could hardly be described as detached scrutinizers of the ABM issue from the outside—even at the level of their homes and localities. The concerned non-scientist citizens of Reading were not alone in worrying about peaceful neighborhoods and property values. The arms controllers weren't simply the impartial disseminators of expert knowledge to a lay public: these experts were part of the same public they sought to sway.¹⁴³

* * * *

No Massachusetts politician could afford to ignore ABM now. Senator Ted Kennedy joined the battle a few days later by mailing Defense Secretary Melvin Laird a five-point rebuttal of the Sentinel program, written with the help of Wiesner and Rathjens.¹⁴⁴ The Massachusetts State Senate president Maurice Donahue offered a resolution calling for President Nixon and Defense Secretary Laird to “suspend or halt” construction of the ABM system in Massachusetts, and to conduct a full review of the program.¹⁴⁵ “The spiraling nuclear arms race in which we and other nations of the world are engaged,” Donahue said, “are that these fateful decisions can no longer be left to a handful of officials in Washington or elsewhere.”¹⁴⁶ Donahue arranged for an open hearing of the state’s Public Safety Committee in Boston’s John Hancock Hall, and planned a debate between experts. Wiesner and Chayes were asked to speak against the ABM, but it proved difficult to find anyone to argue the pro-ABM side.¹⁴⁷ So for a second hearing scheduled

¹⁴³ As the “Lewis Committee” at MIT had noted as early as 1949, a growing fraction of the MIT faculty was moving away from the Boston/Cambridge core and out to the suburbs. On the Lewis Committee and its report, see David Kaiser, “Elephant on the Charles: Postwar growing pains,” in *Becoming MIT: Moments of Decision*, ed. David Kaiser (Cambridge, MA: The MIT Press, 2010), 103–121. Also see David Kaiser, “The postwar suburbanization of American physics,” *American Quarterly* 56 (December 2004): 851–888.

¹⁴⁴ Richard Stewart, “Kennedy urges halt to ABM,” *Boston Globe* (2 February 1969): 1.

¹⁴⁵ “Reading ABM fight due state airing,” *Boston Globe* (4 February 1969): 1.

¹⁴⁶ Crocker Snow, “Citizens lead, politicians follow in anti-ABM fight,” *Boston Globe* (2 March 1969): 30.

¹⁴⁷ “ABM foes to speak: throngs expected at two area meetings,” *Boston Globe* (26 February 1969): 36.

a few days later, a familiar and worthy opponent made the case for ABM: Donald Brennan, who arrived from New York to debate Wiesner and Chayes in front of a crowd in Lexington.¹⁴⁸

By February 1969 the grassroots opposition seemed to be making an impact at the national level. Early that month the ABM debate in Washington took a dramatic turn when Senator Stennis, a Cold War hawk from Mississippi—notorious for rubber-stamping requests for military funding over the years—agreed to hear testimony on the Sentinel program from outside experts, including Jerome Wiesner, who would testify a total of five times in front of Congressional committees. This was an unprecedented development. Never had adversarial experts weighed in before such a high-profile committee on a major national security issue or weapons program. When Herbert York testified before Stennis’s committee, he informed a few of his colleagues that “as far as anyone is able to tell, it marked the first time that the Armed Services Committee had received contrary testimony from outside the administration on a defense issue.”¹⁴⁹ At the same time a bipartisan coalition of Senators, including Kennedy, George McGovern, and the Missouri Democrat Stuart Symington (another stout Cold War hawk who had done an about-face on defense spending in 1969), had joined the attack on ABM in a series of speeches.¹⁵⁰ Ten of the fifteen Senators on the Foreign Relations Committee opposed

¹⁴⁸ Wiesner to Brennan. Crocker Snow, “2 scientists the first to defend ABM system,” *Boston Globe* (1 March 1969): 2.

¹⁴⁹ Herbert F. York to Sidney Drell, Ed Lennox, Larry Lawrence, Harvey Furgatch, and Harold Brown, 20 April 1969, Box 44, Folder 7 “Antiballistic Missile (ABM), Correspondence, 1969,” *HFY*.

¹⁵⁰ James Doyle, “Senate seeks Wiesner’s ABM views,” *Boston Globe* (5 February 1969): 1. For more on the Congressional ABM debate and its place in the longer history of Cold War debates on Capitol Hill, see Robert David Johnson, *Congress and the Cold War* (New York: Cambridge University Press, 2005), esp. 144-189. In Johnson’s assessment, the ABM debate in the Senate amounted to “the most exhaustive attack in U.S. history against a weapons system, and one of the longest debates on any issue in the postwar era.” Johnson, *Congress and the Cold War*, 151.

ABM by this point, including Albert Gore, Sr., whose disarmament subcommittee became one of the critical battlegrounds of the ABM debate.¹⁵¹

As rumors swirled that the administration was backing away from Sentinel, Secretary Laird announced from the Defense Department that the program would be temporarily suspended pending a thorough review. Suddenly, construction at the Sentinel installation in Massachusetts stopped. All but the last 2,500 feet of the access road at Sharpener's Pond had been finished when crews abandoned the site.¹⁵² At a press conference announcing the creation of the New England Citizens Committee on the ABM on February 8th, Abram Chayes warned that Laird's announcement was a bluff, intended merely as "a cooling-off period during which public opposition to ABM deployment would dissipate." Jerome Wiesner was in a more optimistic mood, certain that the experts' arguments were shaping the public discussion. "The American people are finally beginning to realize the absurdity" of ABM, he said. The experts had given the people and their representatives language to recognize that "we are in a mad arms race with ourselves."¹⁵³

Wiesner even had a growing pile of fan mail to prove it. Much of it came from the frontlines in Massachusetts. Amelia Fisk of Pigeon Cove had seen the WGBH coverage of the ABM meeting in Reading, and told Wiesner that it "shows clearly that unless the general public

¹⁵¹ Another opportunity for Congressional opposition to ABM presented itself in hearings on the recently signed Nonproliferation Treaty (NPT) in the Senate Foreign Relations Committee. Article VI of the NPT enjoined the treaty's signers to the "cessation of the nuclear arms race at an early date and to nuclear disarmament." The Senators used the clause as a wedge to force debate on the arms race implications of missile defense. Richard Stewart, "Senate to quiz Rogers, Laird on ABM plans," *Boston Globe* (15 February 1969): 1. For the text of the NPT, see: "Treaty on the Non-Proliferation of Nuclear Weapons," available online at <http://www.iaea.org/Publications/Documents/Treaties/npt.html>.

¹⁵² Victor McElheny, "ABM work stops; project 'dead'," *Boston Globe* (7 February 1969): 1. There was apparently a lag in communication between the Pentagon and the field site. When one local woman heard news of the construction halt over the radio, it came as a surprise: she could still hear construction at Sharpener's Pond. "I found a major and told him that Defense Secretary Laird had halted construction," she told a reporter. "He said, 'Acquisition of land and construction are to stop, but not research, and this is research.'" Work had apparently stopped for good by the end of the day. Rachele Patterson, "The lady told the major: the work must stop," *Boston Globe* (7 February 1969): 32.

¹⁵³ Victor McElheny, "ABM foes step up pressure," *Boston Globe* (9 February 1969): 1.

quickly rises, we shall all be under control of the military-industrial-legislative complex, who will ruin our environment, comfort, and happiness by useless installations.” She added: “We unscientific ones are helpless. All we can do is write Congress or legislatures, who pay us no heed. But they (the complex) are utterly dependent on your brains. Please act!” Shirley Seawell of Belmont shared with Wiesner a letter she had written to Nixon in protest. “That my letter to the President was answered by the Pentagon is really distressing.... Do you think that if I wrote about organized crime I should expect a reply from the head of the Mafia?” Robert Shore in West Bridgewater puzzled through the distinctions between Sprint and Spartan missiles, and turned to Wiesner for guidance. “I make the assumption that the Spartan blast in a relative vacuum and at a distance in the order of a mile—while supposedly defusing the ICBM—would not materially affect the trajectory of the ICBM.” Mr. Shore was led, by a winding trail of logic, to conclude that “the only justification for the [ABM] system is a lack of confidence in its components.” All this pointed to “a need for policy makers to evaluate the limitations of the specialists they must call upon. I believe the term ‘expert’ is an unfortunate one whether applied to specialists from the military-industrial complex or the advocates of liberal domestic programs.” Wiesner didn’t reply.¹⁵⁴

His various appearances and writings had given Wiesner a national following, too. Several people wrote to say they had caught him (alongside Abram Chayes) on NBC’s *Meet the Press* program, where Wiesner had called ABM “a multi-billion dollar mistake.”¹⁵⁵ Swarthmore College senior Elizabeth Kanwit, writing on behalf of her school’s Honors Seminar on American Foreign Policy (which had taken up the ABM debate as its topic of study that semester), asked him for more information about his “stance on the proposals” for ABM. For many who reached

¹⁵⁴ Amelia Fisk to Jerome Wiesner, 31 January 1969; Shirley Seawell to Jerome Wiesner, n.d.; Robert M. Shore to Jerome Wiesner, 23 March 1969, all in Box 19, Folder “ABM – Fan Mail – Spring 1969,” *JBW*.

¹⁵⁵ “Chayes, Wiesner air ABM opposition,” *Boston Globe* (10 March 1969): 5.

out, expert critics like Wiesner were their ambassadors to the remote and abstruse world of defense and nuclear policy. “We ‘lay’ people feel so helpless against the military and industrialists,” wrote Constance Cann of Santa Barbara, “that it is deeply comforting to have people with your expertise to give strength to our convictions that this is not the time nor the thing to do.” The assistance could run the other way, too. When Wiesner was toiling away at his ABM report, he borrowed materials from a Cambridge area activist who had amassed a collection of documents on the activities of the Bainbridge Site Opposition Committee, a group protesting the ABM base planned near Seattle. Wiesner kept the material for so long that when he finally returned it, he was compelled to apologize.¹⁵⁶

For Wiesner, the ABM issue was coming home to roost in every way—even at MIT, his academic base, where early in 1969 students organized a work stoppage and teach-in protesting the Vietnam War and the pervasiveness of military-sponsored research on campus. The event, on March 3rd and 4th, featured five panels of speakers on various topics dealing with the role of the

¹⁵⁶ Arthur O. Shackman to Jerome Wiesner, 18 May 1969; Beatrice S. Rogers to Jerome Wiesner, 10 March 1969; Stanley B. Gale to Jerome Wiesner, n.d.; Elizabeth Kanwit to Jerome Wiesner, 26 March 1969; Constance Cann to Jerome Wiesner, 10 March 1969, all in Box 19, Folder “ABM – Fan Mail – Spring 1969,” *JBW*; and Jerome Wiesner to Alan Forbes, Jr., 19 February 1969, Box 19, Folder “ABM – Jan. 69 – May 69,” *JBW*. Wiesner received letters from critics, too, though the supportive mail was in greater abundance. T.P. Maguire of Miami Beach sent Wiesner a set of challenging questions, arguing for ABM along lines similar to what Thomas Schelling called “the threat that leaves something to chance”—the idea that a would-be attacker is deterred not only by the response they *know* they will receive, but by *not knowing* exactly how the attack and response might unfold. “Even if there is a possible chance that the [ABM] System would not work when the time came to use them—you said nothing about Russia being certain that our [ABM] System would not work. Certainly Russia’s uncertainty would be an effective deterrent—unless she wanted to take a horrible chance,” Maguire wrote. Across this letter Wiesner’s secretary jotted, “How do you ans[wer] this one?” See T.P. Maguire to Jerome Wiesner, 15 May 1969, Box 19, Folder “ABM – Fain Mail – Spring 1969,” *JBW*. Speaking of sophisticated letters from the public, Herbert York received a letter in 1969 from John G. Foss of Ames, Iowa proposing that “instead of building an ABM system to protect our second strike capability why not build dummy missiles in relatively inexpensive ‘soft sites’ and begin a program of randomly rotating the dummies and real ICBMs so that it would not be possible for an aggressor to destroy all of our missiles?” Mr. Foss had made an independent discovery of the “shell game” scheme of missile basing, in which there are more silos than missiles to fill them. (There was no need for “dummy missiles,” however, since the silos were covered and reconnaissance satellites could not see inside them.) York promised Foss to “have it in mind when I engage in further discussion of this whole matter.” And indeed York mentioned shell game basing for the Minuteman missile in a *Scientific American* article later that year. John G. Foss to Herbert York, April 1969; Herbert York to John G. Foss, 14 April 1969, Box 44, Folder 7 “Antiballistic Missile (ABM), Correspondence, 1969,” *HFY*; and Herbert F. York, “Military technology and national security,” *Scientific American* 221, no. 2 (1969): 17-29, on 27.

university and intellectuals in society. At the final discussion—a panel on arms control and national security and chaired by Bernard Feld—Hans Bethe spoke about the technical limitations of ABM. With a dark pleasure he described the intricacies of physical countermeasures like radar blackout—“my favorite penetration aid,” he called it. “Now assured destruction, which is the guarantee against first strike, would be put in question by a really effective antiballistic missile system,” he continued, giving the students a seminar on the basics of deterrence and defense. “It is generally cheap for the offense to offset any amount of ABM by increasing the number of his offensive missiles. This is measured by the so-called exchange ratio.”¹⁵⁷

Jerry Wiesner would have agreed with everything Bethe said. But he was also MIT’s provost. Ever the enigmatic juggler of incompatible interests, Wiesner recused himself from the proceedings entirely. He told the organizers that a single-day’s effort was pointless. “If you could get the scientific community organized to make a continuing contribution to the arms control field, for example, it would really be worthwhile.”¹⁵⁸ Wiesner explained his non-participation to a reporter thus: “The Cold War still exists despite our best efforts,” he said, providing cover for MIT’s retention of its dedicated defense research facilities (the Lincoln Laboratory and Instrumentation Laboratory), which had come under attack by student protesters. “The need for a strong defense persists.” It was perhaps a curious choice of words for someone who had made a new public career out of criticizing missile defense. But Wiesner had always been a study in contrasts.¹⁵⁹

¹⁵⁷ Jonathan Allen, ed., *March 4: Scientists, Students, and Society* (Cambridge, MA: The MIT Press, 1970), on 145 and 147-148.

¹⁵⁸ Wiesner quoted in Allen, ed., *March 4*, xiv.

¹⁵⁹ Victor McElheny, “MIT’s research work and the Defense Dept.,” *Boston Globe* (23 February 1969): 47. On the student protests at MIT, see Stuart W. Leslie, “‘Time of Troubles’ for the Special Laboratories,” in David Kaiser, ed., *Becoming MIT: Moments of Decision* (Cambridge, MA: The MIT Press, 2010), 123-144.

Force and Counterforce

The Pentagon might be “reviewing” its own ABM system, but Ted Kennedy knew that none of ABM’s arms control critics would be asked to participate. From the Senate floor, he joined the chorus denouncing the military’s obscurantism. “We have not had readily available to the public...anything but the official administration reports on national defense projects. It is high time, I think, that a non-Pentagon report be made generally available.”¹⁶⁰ So he phoned Jerome Wiesner and asked him to get in touch with Kennedy’s legislative assistant, Dunn Gifford, to talk about producing a report on ABM for a non-expert audience. Wiesner agreed and promptly asked Abram Chayes to help him edit the report. When the *Washington Post* reported that Kennedy had “hired” his own experts to criticize ABM, Wiesner and Chayes quickly wrote a protest letter. Theirs was a “public service,” they said, inspired by the worry “that the ‘review’ now being conducted in the Pentagon...will not be dispassionate, exhaustive or conclusive.”¹⁶¹

On March 14th, word came from Nixon and from the Defense Department that the administration had reconsidered its missile defense plans, and was now proposing a modified ABM system. Rebranded “Safeguard,” ABM would be deployed in phases. And it would primarily protect strategic missile and bomber forces, rather than cities. The first batteries would guard Minuteman missile sites at Malmstrom Air Force Base in Montana and Grand Forks Air Force Base in North Dakota. Each ABM site—in a planned total of twelve—would be located well away from urban concentrations. The only city to be included in the eventual deployment was Washington, DC, the central hub of the U.S. command and control structure.¹⁶²

¹⁶⁰ Richard Stewart, “Ted doubts Laird; probes ABM on own,” *Boston Globe* (20 February 1969): 1.

¹⁶¹ Rosenblith, ed., *Jerry Wiesner*, 310-311; Abram Chayes and Jerome B. Wiesner to The Editor, *The Washington Post*, 20 February 1969, Box 19, Folder “ABM – Jan. 69-May 69,” *JBW*.

¹⁶² On the role of suburban politics in the Nixon administration’s decision to reconfigure its missile defense plans, see James Cameron, “From the grass roots to the summit: The impact of American suburban protest on U.S. missile defense policy, 1968-1972,” *International History Review* 36, no. 2 (2014): 342–362.

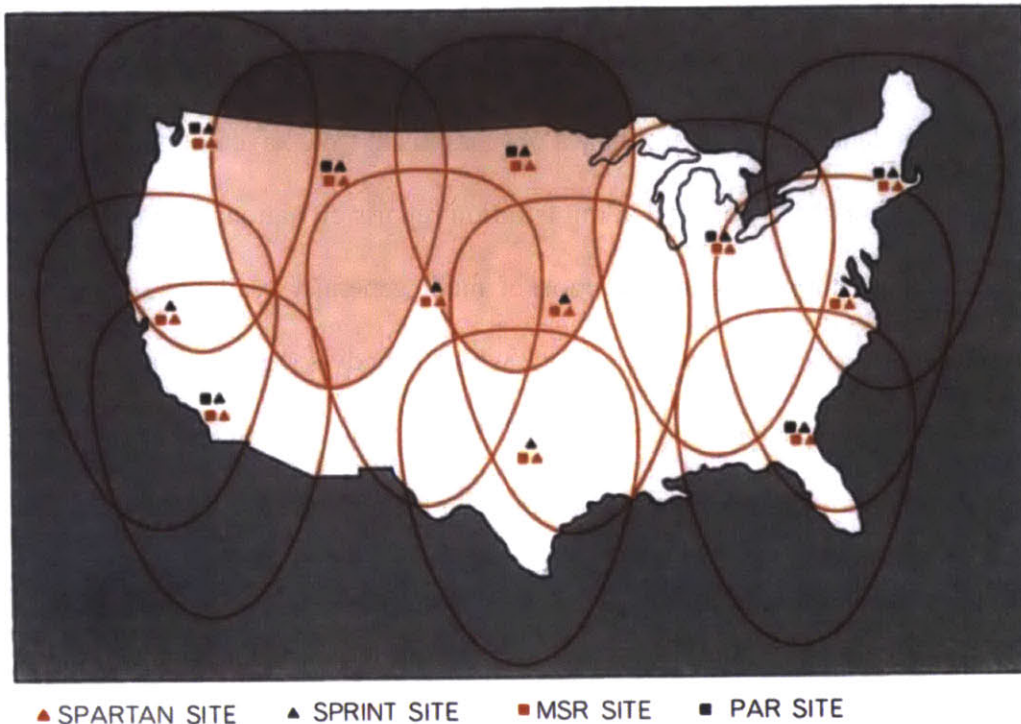


Figure 3.3: The Safeguard system. Compared to Sentinel, the number of installations has been reduced to twelve, with an initial deployment of two, indicated by the shaded areas centered on batteries near Malmstrom Air Force Base in Montana and Grand Forks Air Force Base in North Dakota. Only the site in North Dakota was ever operational. The coverage indicated here is meant to suggest that the full Safeguard deployment would still function as an “area defense,” like Sentinel. Taken from Herbert F. York, “Military technology and national security,” *Scientific American* 221, no. 2 (1969): 17-29.

Some arms control experts initially wavered as they absorbed news of the revised plans. On one hand Safeguard seemed to answer key criticisms of Sentinel. It focused the defensive effort on missile silos and SAC air bases, the sort of “hard point defense” that seemed consistent with both deterrence and arms race stability. Thomas Schelling explained to a reporter that he had “advised against missile defense for the last six years,” and yet “I think President Nixon has found a mode of deployment that is awfully hard to take issue with.” Of course, “one could suppose that this is merely a foot in the door for a ‘thicker’ system,” he qualified. That was

precisely what unconvinced critics did suppose. Jack Ruina remarked, “We’re getting pretty much the same deployment as originally proposed with somewhat fewer missiles, somewhat differently located.” Jerome Wiesner had caught wind of the revised scheme a day or two before it was announced, telling Henry Kissinger over the phone on the 13th that defending Minutemen with an ABM was “one of [the] worst possible decisions that the President could make.” Kissinger demurred and said that he’d made that case already to Nixon, but would “reinforce” it using Wiesner’s name.¹⁶³

The House Foreign Affairs Committee became another site of lively ABM dispute following the announced transformation of Sentinel into Safeguard. Its hearings in March on “weapons and space” featured the likes of Jerome Wiesner and George Kistiakowsky arguing against Safeguard, and a panel headlined by Thomas Schelling and Herman Kahn arguing for it. Schelling and Kahn were dazzling and voluble performers, together filling five hours of the hearing. “Kahn wove his magic spells and pelted the audience with polysyllables,” reported a research analyst from the Aerospace Corporation who was in attendance. In the manner of a carnival barker, Kahn predicted he could convert 80% of any audience to a pro-ABM position. He had no takers that day among the committee members.¹⁶⁴

Wiesner and Chayes got to work on their study, now in light of the new rationale for ABM. They didn’t have to reach very far for contributors to the report. Carl Kaysen, now Freeman Dyson’s boss as the director the Institute for Advanced Study, submitted a paper arguing that Safeguard might well be innocuous as far as deterrence was concerned (at least in theory), but that there were much more cost effective ways of protecting the U.S. second-strike

¹⁶³ Crocker Snow, “The professors: More questions than answers,” *Boston Globe* (15 March 1969): 1; Telcon, Jerome Wiesner and Mr. Kissinger, 3-13-69, Document KA00288, *DNSA*.

¹⁶⁴ Aerospace Corporation, “Congressional Activity Report No. 9,” 21 March 1969, Box 44, Folder 7 “Antiballistic Missile (ABM), Correspondence, 1969,” *HFY*.

force. Bernard Feld pitched in with a paper presenting the action-reaction theory of arms races and the offense-defense link. The physicist Steven Weinberg, who had become a professor at MIT (and an occasional participant in the arms control seminar), went even further. He claimed that not only was Safeguard not cost effective, it had been falsely advertised as a defense of the deterrent force (theoretically unobjectionable from the arms controllers' perspective). In reality Safeguard was "primarily a 'thin' population-defense system, like Sentinel"; and so it "would destabilize the arms race," just like Sentinel. Because Safeguard would use the same missiles and radars as Sentinel, all the old technical objections applied to it. To that end Hans Bethe's half of the Bethe-Garwin paper, the *locus classicus* of technical ABM critique, was also included in the report. Chayes, Wiesner, Weinberg, and George Rathjens coauthored a summary statement, a sweeping rejection of Safeguard as an ineffective system and a destabilizing "area defense" masquerading as a "hard-point defense" of missiles and bombers—an arms control wolf in sheep's clothing. Again Wiesner got on the phone to Kissinger, telling him the more he thought about it, the more "absurd" Safeguard looked. Kissinger replied that it would be all but impossible for Nixon to reverse his decision now.¹⁶⁵

In early May the Wiesner-Chayes report came out. Ted Kennedy had already sent the "loose-leaf, hastily printed monograph" to Secretary of Defense Melvin Laird and his Director of

¹⁶⁵ Telecon, Prof. Jerome Wiesner, April 11, 1969, Document KA00481, *DNSA*. The final study included a total of seventeen essays. Leonard Rodberg provided an essay on ABM's reliability. Adam Yarmolinsky wrote about the military-industrial complex and the internal "momentum" of weapons development. A brief piece on the command-and-control limitations of ABM—the troublingly small degree to which the President would have a say over whether and when to fire an ABM nuclear warhead—was written by the former Lyndon Johnson speechwriter Bill Moyers. Not every submitted study made it into the report, however—even one from the notable analyst William Kaufmann. Kaufmann performed a detailed cost-benefit analysis of Safeguard and recommended, somewhat tepidly, against deployment. The analysis was hedged about with arcane details and qualifications, sensitive to subtle changes in the numerical assumptions, and so Wiesner and Chayes decided not to include it. Kaufmann could not have been pleased, as he told Chayes upon submitting his draft that "I have been doing the calculations very laboriously by hand. If you want to pursue the quantitative analysis further, I think you should get a mathematician and some computer time." See William W. Kaufmann to Abram Chayes, 25 March 1969, as well as the attached report: William W. Kaufmann, "Analysis of the Case for Safeguard," in Box 19, Folder "ABM – Jan. 69-May 69," *JBW*.

Defense Research and Engineering, John Foster, Jr. Within another day or two his office had mailed a copy to every member of the Senate, the Defense and State Departments, and the White House. Wiesner and Chayes were in talks with New York publishing houses to get the report out in wide-release book form. The publishers wanted Kennedy to write a foreword but the Senator resisted, not wanting to impugn the report's objectivity by attaching his name to it. (He yielded quickly enough, as the book came out the same month with his name prominently on the front cover, both in hardback and a pulpy mass-market paperback.)¹⁶⁶

Foster had called a press conference at the Pentagon about five hours before the report was officially released by Kennedy's office. He said it was "of little help." Plagued by "basic errors," it had failed even to meet "the standards of the scientific profession." It was overly optimistic about the ability of the offense to beat the defense, he said, overly skeptical about the effectiveness of defensive systems.¹⁶⁷ Yet Defense Department officials paid the Wiesner-Chayes report the repeated compliment of addressing its claims over the coming weeks. In Congressional testimony later that month, Secretary of Defense Laird elaborated more on the reasons for deploying Safeguard. Pentagon analysts believed the Soviets were gearing for a first-strike capability by the mid-1970s, particularly by MIRVing a large new ballistic missile (dubbed the "SS-9" in NATO lingo; the Soviets designated it the "R-36"), which some in the Pentagon believed could carry a single warhead of 20-25 megatons. (Analysts in the CIA, on the other hand, thought its payload was much smaller, closer to 5 megatons.)¹⁶⁸

¹⁶⁶ Abram Chayes and Jerome B. Wiesner, eds., *ABM: An Evaluation of the Decision to Deploy an Antiballistic Missile System* (New York: Harper & Row, 1969); John W. Finney, "Kennedy-sponsored study of ABM upsets Pentagon," *New York Times* (7 May 1969): 1. Lawrence Freedman calls the Wiesner-Chayes report "the key anti-ABM document." Freedman, *The Evolution of Nuclear Strategy*, 494, n. 4.

¹⁶⁷ Statement by Dr. John S. Foster, Jr., Director of Defense Research and Engineering, 6 May 1969, Box 19, Folder "ABM #2," *JBW*.

¹⁶⁸ George W. Rathjens, Jerome B. Wiesner, and Steven Weinberg, "A Commentary on Secretary of Defense Melvin Laird's May 22 Defense of Safeguard," 27 June 1969, Box 19, Folder "ABM #4," *JBW*; John W. Finney, "SS-9 helps administration score points in missile defense debate," *New York Times* (24 March 1969): 30.

Pentagon staffers were sufficiently alarmed by the report that they wrote their own classified review, carefully leaked within a few days in unclassified form. The review argued that Wiesner, Chayes, and their coauthors had let their skepticism artificially color their analyses. Even more, they had simply used incorrect, unclassified data. For example, Steven Weinberg had based his unfair criticism of ABM missile reliability on “representative [figures] synthesized from unclassified sources...” Nor had the critics guessed the available number of interceptor missiles—an understandable flub, “because the correct information is classified,” but a big one. Meanwhile Bethe’s description of defense countermeasures “is qualitatively correct, but his chapter is written in such a manner as to exaggerate the ease with which penetration aids can be developed.” And as for the report’s claim that Safeguard would prod another step in the arms race, well, that was “open to debate.”¹⁶⁹

The Pentagon rebuttal soon became an arrow in the quiver of ABM supporters. Among these were the former RAND analyst Albert Wohlstetter and the architect of Cold War policy Paul Nitze. Each received leaked copies of the DOD report from a freelance defense consultant with ties to RAND named Anne Jonas, who had gotten hers from Richard Fryklund in the DOD Public Affairs Office. (“This is now for use in any way anyone wants to use it,” Jonas wrote to Nitze, “as long as its origin in DOD is not mentioned.”¹⁷⁰) Nitze and Wohlstetter were ready to put it to good use. They had just formed a pro-ABM Committee to Maintain a Prudent Defense Policy with the former Secretary of State Dean Acheson.¹⁷¹ Nitze and Wohlstetter soon began

¹⁶⁹ “In fact, the section on ABM and the arms race is as much an attack on MIRV as it is on ABM.” Department of Defense, “ABM: An Evaluation of the Decision to Deploy an Antiballistic Missile System,” Box 47, Folder 5 “Anti-Ballistic Missile (ABM) System, Miscellaneous materials, 1969, n.d.,” *PHN*. According to a placeholder in this file, an analyst at IDA also wrote a classified review of the Wiesner-Chayes report that Nitze had access to.

¹⁷⁰ Anne M. Jonas to Paul Nitze, 6 June 1969, Box 47, Folder 5 “Anti-Ballistic Missile (ABM) System, Miscellaneous materials, 1969, n.d.,” *PHN*.

¹⁷¹ The Committee was a response to the recent creation of the New York-based “National Citizens Committee Concerned About Deployment of the ABM,” headed by Roswell Gilpatric and Arthur J. Goldberg, the

building a coalition of pro-ABM expertise. Nitze asked Donald Brennan for his rolodex of non-government experts who supported deployment. Within weeks they had collected the signatures of virtually all the heavy hitters in the pro-missile defense camp (including the physicists Freeman Dyson, John Wheeler, Kenneth Watson, and Eugene Wigner, international affairs experts Edmund Gullion, Richard Rosecrance, and Ciro Zoppo, and Irving Kristol, a star in the emerging neoconservative movement).¹⁷²

In a draft letter to prospective Committee members, Nitze described the group's plan to employ "two or three younger men for two months who would follow the debate, search out facts as members of the Committee may request, circulate papers and in general contribute to the maintenance of objectivity in the debate."¹⁷³ Wohlstetter knew just the younger men for the job, conscripting two of his graduate students at the University of Chicago (where Wohlstetter now taught)—Paul Wolfowitz and Peter Wilson—along with another grad student at Princeton named Richard Perle, to do much of the Committee's grunt-work. Both Wolfowitz and Wilson, as it happened, were writing master's theses in international relations on missile defense. As a college student Perle had dated Wohlstetter's daughter in Los Angeles; he'd been converted from his "traditional sixties" foreign policy liberalism to a more hawkish sensibility during "impromptu

former Supreme Court Justice and U.S. Ambassador to the United Nations who had resigned his post in protest over the Vietnam War. This Committee announced its creation at a press conference in Washington, DC, on April 17th, 1969. George Rathjens was among the leadership present at the press conference. See Harold Ickes to Herbert York, 18 April 1969, Box 44, Folder 6 "Antiballistic Missile (ABM), Correspondence, 1969," *HFY*.

¹⁷² D.G. Brennan, 5 May 1969, Box 74, Folder 11 "Committee to Maintain a Prudent Defense Policy, Formation, 1969," *PHN*; "Acceptances (7/9/69)," Box 74, Folder 9 "Committee to Maintain a Prudent Defense Policy, Administrative materials, 1969," *PHN*. On the origin of the Committee to Maintain a Prudent Defense Policy, also see Robert Zarate, "Albert and Roberta Wohlstetter on nuclear-age strategy," in Robert Zarate and Henry Sokolski, eds., *Nuclear Heuristics: Selected Writings of Albert and Roberta Wohlstetter* (2009), published online by the Strategic Studies Institute, available at <http://www.strategicstudiesinstitute.army.mil/>.

¹⁷³ "Nitze Draft, April 29, 1969," D.G. Brennan, 5 May 1969, Box 74, Folder 11 "Committee to Maintain a Prudent Defense Policy, Formation, 1969," *PHN*.

seminars around Wohlstetter's swimming pool." Dean Acheson called the young men "our Three Musketeers."¹⁷⁴

The grad students Wolfowitz, Perle, and Wilson got down to work studying various aspects of the ABM debate. By the middle of the summer of 1969 the Committee had produced a series of brief study papers rebutting assorted arguments against missile defense. "Will Safeguard Precipitate an Arms Race?" asked one pamphlet. Absolutely not, was its answer. "Many critics of Safeguard have actually proposed that we double our Minuteman force. Such a sequence of offensive deployment constitutes the very arms race we hope to avoid." (This strained the arguments of the arms controllers, who had proposed that it was cheaper and more effective to supplement the offensive force, should the unlikely need arise, than it was to build up the defense.)¹⁷⁵

* * * *

The claim that the U.S. ICBM force was *not* under threat gave Albert Wohlstetter an allergic reaction. The arms controllers thought that ABM was being sold under trumped-up intelligence estimates of the Soviet threat in the coming decade. The question was: could the Soviets, in the next five or ten years, launch an effective first strike and knock out the whole Minuteman force? If so, then the Safeguard ABM might well be a good idea: why not add extra protection to Minuteman, instead of digging yet more Minuteman silos? (Wouldn't that be one way to avoid the "arms race" the ABM critics were so anxious about?) The arms controllers, for their part, found the idea that the Soviets were spoiling for a first-strike capability ridiculous. In

¹⁷⁴ See Nitze's handwritten notes near the back of Box 74, Folder 11 "Committee to Maintain a Prudent Defense Policy, Formation, 1969," *PHN*. On Perle's high school days and Acheson's "Three Musketeers" comment, see Strobe Talbott, *The Master of the Game: Paul Nitze and the Nuclear Peace* (New York: Alfred A. Knopf, 1988), 112-113.

¹⁷⁵ "Will Safeguard Precipitate an Arms Race?," Committee to Maintain a Prudent Defense Policy, Paper No. 3, Box 75, Folder 1 "Committee to Maintain a Prudent Defense Policy, Papers and Reports, 1969," *PHN*.

the Wiesner-Chayes report, Steven Weinberg and Jerome Wiesner had argued in separate papers that the current Minuteman force would be able to ride out a Soviet first strike in sufficient numbers to issue a devastating response. This was true without active defenses or any additional improvements in silo hardening (hardening is the silo's resistance to nuclear blast, provided mainly by concrete shielding, and measured in terms of pounds per square inch "overpressure"—the pressure above normal atmospheric pressure produced by the bomb's blast wave). According to Wiesner's calculations, assuming the Soviets had acquired 500 SS-9 missiles MIRVed with three warheads each (an estimate he considered on the high side), and given missile accuracy, yield, and reliability parameters estimated by the Secretary of Defense himself, nearly 300 (out of a total of 1,054) U.S. missiles would survive the attack before finding their way to Soviet cities in retaliation.¹⁷⁶

In testimony in front of the Gore Subcommittee in March and the Stennis Committee in April, George Rathjens similarly argued that "even if the Soviet SS-9 missile force were to grow as rapidly as the Defense Department's most worrisome projections, even if the Soviet Union were to develop and employ MIRV's with those missiles...a quarter of our Minuteman force could be expected to survive a Soviet preemptive SS-9 attack" of 500 MIRVed missiles. And that surviving quarter would still be more than *twice* what was needed to respond with "assured destruction" behind the Iron Curtain, by Rathjens's lights. As far as the arms controllers were concerned, Safeguard added nothing of value to a situation in which deterrence—as measured by Wohlstetter's own classic formulation of the "secure second strike"—was already overwhelmingly guaranteed.¹⁷⁷

¹⁷⁶ Chayes and Wiesner, eds., *ABM*, 73.

¹⁷⁷ "A statement by G.W. Rathjens before the Committee on Armed Services of the United States Senate," Box 19, Folder "ABM #2," *JBW*.

When Wohlstetter was called to testify before the Stennis Committee in April right behind Rathjens, he asked Rathjens for his calculations the day before the appearance. Rathjens obliged and Wohlstetter pored over the notes. He decided that either the arms controllers had misunderstood the intelligence, or they had simply fudged the numbers. The calculation of the “single shot kill probability”—the likelihood that one offensive missile can disable, or “kill,” one of the adversary’s offensive missiles as it waits in its silo—was enormously sensitive to these specific parameters. And the estimate of a first strike’s chances at success was hugely sensitive to the value of the single-shot kill probability. Lower the Minuteman silo blast resistance by two-thirds (as Wohlstetter did in his revision of Rathjens’s calculation), and the fraction of surviving Minuteman missiles decreased by 9%. Further assume that a Soviet SS-9 could carry three warheads each having a 5-megaton yield (Wohlstetter), rather than four 1-megaton warheads (Rathjens), and the number of surviving Minutemen dropped by another 9%. In this way Wohlstetter tuned the numbers in the Soviet offense’s favor, whittling Rathjens’s estimation of 25% Minuteman survival all the way down to 5%. Should the Kremlin launch an all-out counterforce blow sometime in the late 1970s, they would be able to take out all but 50 of the Minutemen. No problem, replied the arms controllers: 50 missiles landing in the Soviet Union would still wreak incredible havoc. But Wohlstetter—whose constant obsession, rooted in his famed RAND studies of overseas basing in the 1950s, was the vulnerability of strategic forces—found this conclusion horrifying. Missile defense (of the missiles) was the only responsible course.¹⁷⁸

¹⁷⁸ “Appendix III: Treatment of operations-research questions in the 1969 Safeguard debate,” *Operations Research* 19, no. 5 (1971): 1175-1237, esp. 1183; Albert Wohlstetter, “The case for strategic force defense,” in Johan J. Holst and William Schneider, Jr., eds., *Why ABM?: Policy Issues in the Missile Defense Controversy* (New York: Pergamon Press, 1969), 119-142.

Wohlstetter wrote up his calculations in a 21-page report and submitted it as an addendum to his testimony for the Stennis Committee. Word of this was promptly leaked to the *New York Times*, which quoted Wohlstetter proclaiming that the arms controllers' computations had ranged "from the mistaken to the 'plainly absurd.'" The Rathjens calculation was the most irritating, because Wohlstetter judged that he had intentionally plugged in the wrong numbers.¹⁷⁹ Rathjens was affronted by this sudden public rebuke. He quickly sent a letter to the *Times* himself. "I have dealt with Mr. Wohlstetter's criticisms in a classified letter, but also feel I should comment on them publicly," he said. He claimed to have gotten his silo hardness data and other intelligence estimates from "a chart released by Deputy Secretary of Defense [David] Packard and data made available by former Deputy Secretary of Defense Nitze..." Wohlstetter replied that Rathjens was intentionally using outdated numbers (the Nitze figures were from 1967). But he went further, accusing the clique of Wiesner, Weinberg, Rathjens of careless and unprofessional conduct. Rathjens and Wohlstetter continued to trade volleys, bickering like medieval theologians debating the finer points of propositional logic. But whereas these thermonuclear theologians had once conducted their disputes behind the ivied walls of the university or the curtains of government secrecy, now they were conducted in the opinion pages.¹⁸⁰

¹⁷⁹ William Beecher, "Scientist rebuts criticism of ABM: Wohlstetter sees mistakes in some foes," *New York Times* (26 May 1969): 13.

¹⁸⁰ A week later Rathjens replied again, angrily splitting more strategic hairs in the pages of the *Times*. "Mr. Wohlstetter implies that my calculations were meant to apply to the late 1970's. They were not... All references by me are to the mid-1970's or in one case specifically to 1975." The ABM debate might improve in quality "if Mr. Wohlstetter would try to make his case by arguing its merits and by rebutting his opponents' analyses, not by misrepresenting their views." Wohlstetter, a week later: "[Rathjens] wrote me earlier that he assumed a .6 probability for a one-megaton weapon against a Minuteman silo. The lower of Mr. Nitze's two [silo kill] probabilities, not their median, scales to .61 at one-[megaton] using the familiar cube root approximation; and even higher—to .69—using more exact methods." (Just what fraction of the *Times* readership was "familiar" with the cube-root approximation for the dependence of silo kill probability on yield is impossible to know, but one supposes it was small.) The debate would be better served "if we used the authority of science less and its methods more," Wohlstetter spat. See George W. Rathjens and Albert Wohlstetter, "Safeguard missile system is evaluated by two

All these numbers were beside the point, Rathjens decided. There was no indication that the Soviets were actually planning to develop a first-strike force for the 1970s. And if they really *wanted* to, Safeguard was hardly the most effective or cheapest way for the U.S. to compensate, because it could be defeated so easily. And that was where the arms controllers' claims finally came to rest: where they had started, back in the late 1950s. Throughout the month of June, Rathjens, Wiesner, and Weinberg had been at work on yet another report. Tired of the debate with Wohlstetter, the three now argued that Safeguard, even if it performed up to spec, could be overwhelmed by a Soviet attack. All it would take was for the Soviets to build a few extra missiles, a job that would take mere months, and Safeguard would meet its match. Rathjens claimed to have seen a classified chart showing that if the Soviet SS-9 force were increased beyond 420 missiles, Safeguard would be unable to soak up the assault. The Pentagon refused to discuss the mysterious chart publicly, "on the ground it contains secret information on how many interceptor missiles are planned for the Safeguard system." Wiesner, Weinberg, and Rathjens had each been "briefed by the Pentagon" before making their "unofficial estimates" of Safeguard effectiveness. They sent off this latest effort to their political patron, Ted Kennedy, and sure enough it was reported in the *New York Times* just days later.¹⁸¹

The struggle against ABM had stumbled by the end of the summer. In August 1969, Vice President Spiro Agnew cast a tie-breaking vote in the Senate to approve funding for the first phase of Safeguard deployment. It marked the beginning of the end for the ABM debate. But

scientists," *New York Times* (15 June 1969): E17; George W. Rathjens, "Calculations on ABM," *New York Times* (22 June 1969): E15; Albert Wohlstetter, "Minuteman costs," *New York Times* (29 June 1969): E11.

¹⁸¹ The authors wrote in a footnote: "In carrying out the analysis of Safeguard reported here we have assumed that the system will perform in accord with specification (or better). We have grave doubts that the performance of the system would approach the design estimates, considering its complexity and particularly considering that the system will never be tested in an environment that simulates that in which it will have to operate." George W. Rathjens, Jerome B. Wiesner, and Steven Weinberg, "A Commentary on Secretary of Defense Melvin Laird's May 22 Defense of Safeguard," 27 June 1969, Box 19, Folder "ABM #4," *JBW*. Also see John W. Finney, "3 scientists say Soviet could easily crush ABM," *New York Times* (2 July 1969): 4.

Albert Wohlstetter was not one to let sleeping dogs lie. In November he asked the president of ORSA (the Operations Research Society of America) to conduct an inquiry into possible misuses of operations analysis by Rathjens, Wiesner, and Weinberg in their Congressional testimony. The arms controllers said they found the tribunal “absurd.” If ORSA wanted to investigate misconduct they could look into possible collusion between the administration and would-be “outside” experts. “The role of outside consultants, such as Mr. Wohlstetter, was definitely secondary, but the extent to which they received support from the Department of Defense,” including “the use of Air Force aircraft to transport them to public debates, should also be examined.”¹⁸² When the ORSA report finally came out in 1971, Richard Nixon found it on his desk courtesy his Presidential Counselor, Donald Rumsfeld, who had become an energetic supporter of ABM deployment. (This was quite a reversal. In early 1969 a scientist named Jona Cohn had conducted an ABM opinion survey of Illinois politicians, reporting to Jerome Wiesner that Rumsfeld—then still a House Republican and sensitive to the anti-ABM agitation of his local constituents—was “very knowledgeable and strongly opposed to [an] ABM system anywhere.”)¹⁸³

Since its creation, there had been rumors that the Nitze-Wohlstetter Committee was a White House front organization. Nitze had rejected this charge early on by telling a reporter that the Committee had “very little” contact with Nixon’s administration. “They know we are going

¹⁸² George W. Rathjens, Steven Weinberg, and Jerome B. Wiesner to Thomas Caywood, 22 December 1969, in “Appendix IV: Correspondence and Comments,” *Operations Research* 19, no. 5 (1971): 1246-1258, on 1250-1251. ORSA chastised the arms controllers for an assortment of technical misdeeds, but most of all for their “failure to distinguish properly between the roles of the analyst and the advocate.” See “Appendix III: Treatment of operations-research questions in the 1969 Safeguard debate,” *Operations Research* 19, no. 5 (1971): 1175-1237, on 1176. The ORSA feud continued to boil even after the report came out. Wiesner, Weinberg, and Rathjens felt compelled to submit a commentary on the report, and the episode attracted much press attention. See Rosenblith, ed., *Jerry Wiesner*, 317-326.

¹⁸³ “Rumsfeld no friend to arms control scientists,” *Science* 190 (5 December 1975): 962; Jona Cohn to Jerome Wiesner, 5 February 1969, Box 19, Unlabeled Folder, *JBW*.

to do this—but that’s all.”¹⁸⁴ Nitze was cautious. Upon hearing that the White House director of communications was holding a meeting to harmonize the activities of various ABM supporters, Nitze forbade Committee members from attending. But documents in Nitze’s files reveal that the group was extremely close to the government, if not all the way in its pocket. As Richard Perle drew up plans for the first general meeting of the Committee in June, he considered planning a few sessions “open only to members with appropriate security clearances,” including officials from the DOD. The Committee also debated coordinating all of its media commentary with the Pentagon.¹⁸⁵ At one early meeting the members broached the possibility of Acheson or Nitze meeting with Nixon to make sure no one was stepping on any toes.¹⁸⁶ Wohlstetter was in regular contact with Henry Kissinger, telling him during one telephone conversation in July that the “position of the opposition is absolutely ridiculous,” aspects of their calculations “as crooked as can be.”¹⁸⁷ Nitze helped get Perle a six-month clearance at the top-secret level to visit the Westinghouse Defense and Space Center in Waltham, Massachusetts—a major ABM contractor—so that Perle could get a crash course on the ABM system.¹⁸⁸ And during at least one classified Safeguard briefing at the Pentagon, Nitze, Wohlstetter, Perle, and Wolfowitz were all in attendance. Sometimes the Committee even held its own meetings in a room in the E Ring of the Pentagon building.¹⁸⁹

¹⁸⁴ William Chapman, “Acheson forms unit to raise ABM support,” *Washington Post* (27 May 1969): A6.

¹⁸⁵ “Partial Agenda, Meeting, 29 May 1969,” Box 74, Folder 9 “Committee to Maintain a Prudent Defense Policy, Administrative Materials, 1969,” *PHN*.

¹⁸⁶ “Agenda for May 16 Meeting,” Box 74, Folder 9 “Committee to Maintain a Prudent Defense Policy, Administrative Materials, 1969,” *PHN*.

¹⁸⁷ Memorandum of Telephone Conversation, 11 July 1969, Document KA01018, *DNSA*.

¹⁸⁸ “Visit Clearance Form, 17 June 1969,” Box 74, Folder 11 “Committee to Maintain a Prudent Defense Policy, Formation, 1969,” *PHN*.

¹⁸⁹ Ann M. Roll to Security Officer, Office of Secretary of Defense, 10 July 1969; and “Meeting of the Committee to Maintain a Prudent Defense Policy,” Friday, 11 July 1969, Box 74, Folder 9 “Committee to Maintain a Prudent Defense Policy, Administrative Materials, 1969,” *PHN*.

Their opponents could in some ways be just as well connected. For every phone call from Wohlstetter to Henry Kissinger there was one from Wiesner or Ruina or Rathjens. They had all kept security clearances and sat in some of the same DOD briefings. Ruina was serving on ACDA's General Advisory Committee, and pushed an anti-ABM position from there as much as he did from MIT. When Herbert York was preparing to testify to the Gore Subcommittee in 1969, he spent the day before his appearance "getting a final briefing on some of the technical matters" related to ABM at the Aerospace Corporation headquarters in Los Angeles. (He, like Wiesner, had joined Aerospace's board of trustees. All the same, he expensed the trip to the Senate Committee on Foreign Relations, presumably to avoid conflict of interest.)¹⁹⁰

There were no "outside" experts in this fight. Everyone understood that because the debate had gone dramatically public, who was "outside" and who "inside" was a matter of perception, shifting and audience-specific. For the Committee to Maintain a Prudent Defense Policy, the audience was Congress. As Perle planned his general meeting, he stage-managed the affair with remarkable adroitness, telling Nitze that the meeting "should be timed, if it is to be public, for maximum impact." The idea was to keep it closed-door until just the right moment, culminating in a press conference—"an opportunity for major publicity," he added. Actually, maybe "the meeting [should] not be referred to as a conference, or indeed, even as a meeting, but rather as a gathering of members or some such innocuous term." It could be held in someone's home, rather than a "more public facility"—ideally, outside of Washington. "Princeton occurs as a possibility." A cozy get-together for missile defense enthusiasts, with a few top-secret briefings

¹⁹⁰ On Ruina, see Herken, *Cardinal Choices*, 171. For York's Aerospace trip, see Herbert York to J.W. Fulbright, 26 March 1969, Box 44, Folder 8 "Antiballistic Missile (ABM), Correspondence, 1969," *HFY*.

and a press conference at the end. “The effect on the Hill is the most important consideration,” he underlined.¹⁹¹

Actions and Reactions

In the spring of 1968, on the heels of a furtive meeting in Glassboro, New Jersey the previous year, President Johnson and Soviet Premier Alexei Kosygin had agreed to begin negotiations on limiting the superpowers’ strategic nuclear arsenals.¹⁹² With the Strategic Arms Limitation Talks (SALT) looming, the arms controllers raised their action-reaction arms race argument to its highest pitch. And over the course of 1969 MIRV, which had been outshone by ABM’s high wattage as a political issue, came more squarely into view. The claim that ABM and improvements in offensive power, especially MIRV, interacted in a vicious spiral—an idea first pondered in classified circles in the early 1960s, then further developed in closed-door settings over the next few years—was now aired in public at almost every opportunity.¹⁹³

For a long time, George Rathjens had felt that U.S. policy contained a fundamental paradox. The purpose of nuclear forces was to deter aggression and, if deterrence failed, to end war quickly and limit damage to the United States. But as early as January of 1958, as Rathjens was wrapping up a five-year appointment in the Pentagon’s Weapons Systems Evaluation Group (before spending a year at Harvard, then joining the staff of PSAC), he had decided that in the nuclear age it was impossible to have both deterrence and damage limitation. Rathjens wrote a piece that year on “Deterrence and Defense” (initially the Office of the Secretary of Defense

¹⁹¹ Richard Perle to Paul Nitze, 17 June 1969, “Considerations on holding a general meeting,” Box 74, Folder 10 “Committee to Maintain a Prudent Defense Policy, Correspondence and memorandums, 1969,” PHN.

¹⁹² John Newhouse, *Cold Dawn: The Story of SALT* (New York: Holt, Rinehart and Winston, 1973), 103.

¹⁹³ In the judgment of Ted Greenwood, “the later arguments that MIRV could contribute to strategic instability or be detrimental to arms control efforts would have been difficult to sustain” much earlier in the 1960s. MIRV had waited for its time as a political issue, and it came around the middle of 1969. Greenwood, *Making the MIRV*, 109.

distributed copies to a few insiders, before releasing it for publication in the *Bulletin of the Atomic Scientists*). There he argued that in the era of thermonuclear-tipped missiles, there was no such thing as meaningful damage limitation—no such thing as counterforce targeting or missile defense in circumstances where the requirements of deterrence had already built up gigantic nuclear forces. *Any* war would be an all-out affair; the only kind of deterrence anyone could hope for was Herman Kahn's loathed "Type I." The most remarkable thing about Rathjens's paper was its early date. He had evidently pledged himself to a restrained form of deterrence, and worked out an objection to missile defense and strategies of damage limitation, well before such things were debated even privately by most strategists and defense scientists.¹⁹⁴

What was missing in 1958, however, was an arms race component.¹⁹⁵ That was what the intervening decade of debate would supply. By the late 1960s Rathjens was among the most consistent and forceful exponents of the idea that improvements in missile defense and offensive power (as exemplified chiefly by MIRV) were not only futile but dangerous, spurring escalatory reactions on each side and increasing the risk of war.

Rathjens introduced readers of *Scientific American* to the action-reaction theory in an essay on "The Dynamics of the Arms Race" in April 1969, during the most heated phase of the ABM controversy. There were two kinds of instability in the world of nuclear weapons, Rathjens said: "crisis instability (the possibility that when war seems imminent, one side or the other will

¹⁹⁴ Albert Wohlstetter's famous "delicate balance" article was still a year away, for example. I found an early draft of Rathjens's paper in the files of Paul Nitze. It was sent to Nitze by an official in the DOD, who wrote, "As I told you, this document has not yet been released for outside use, therefore, it should be handled accordingly." Paul H. Johnstone to Paul Nitze, 28 January 1958, attached to G.W. Rathjens, Jr., "Deterrence and Defense," 17 January 1958, Box 36, Folder 16 "Rathjens, George W. Jr., 1958, 1968-69, n.d.," PHN. It was published as George W. Rathjens, Jr., "Deterrence and defense," *Bulletin of the Atomic Scientists* 14, no. 6 (1958): 225-228.

¹⁹⁵ It seemed on the tip of his tongue in one sentence: "An incremental improvement in defense will be matched, probably at lesser cost, by an offensive improvement." (Emphasis in original.) But he did not develop this intuition into a claim about the arms race. Rathjens, "Deterrence and Defense," 10. While Rathjens did not discuss the concept of stability anywhere in the paper, given his early faith in the robustness of limited deterrence, he already had a readymade objection to Albert Wohlstetter's assertion (voiced about a year later) that deterrence hung delicately in the balance, and to Thomas Schelling's contention that deterrence could be endangered by short-term stresses.

be motivated to attack preemptively in the hope of limiting damage to itself) and arms-race instability (the possibility that the development or deployment decisions of one country, or even the possibility of such decisions, may trigger new development or deployment decisions by another country).” Rathjens thought the partisans of deterrence vs. damage limitation simply had different intuitions about “the actual role of this action-reaction phenomenon”: the deterrence devotees were gripped by it, the damage limiters were more worried about what would happen *inside* a nuclear war.¹⁹⁶

Of course, one first had to believe that the arms race was a real thing in order to believe in the action-reaction mechanism. Arms race skepticism flourished among many RAND alumni. Thomas Schelling had downplayed the idea of the arms race in a 1959 RAND paper as “a journalistic term.” He doubted that arms races led to war in any clear, meaningful sense.¹⁹⁷ When Jeremy Stone tested out his proposal for an ABM ban in the Harvard-MIT arms control seminar, debuting the action-reaction argument, the RAND veteran Fred Iklé shrugged it off. “Stone seems to be concerned with containing the arms race,” he said, “without defining exactly what

¹⁹⁶ George W. Rathjens, “The dynamics of the arms race,” *Scientific American* 220, no. 4 (1969): 15-25, quotation on 19.

¹⁹⁷ T.C. Schelling, “Toward a theory of strategy for international conflict,” RAND P-1648 (19 March 1959); reprinted in part as T.C. Schelling, “The retarded science of international strategy,” *Midwest Journal of Political Science* 4, no. 2 (1960): 107-137, on 129-130. Thinking about arms races, for Schelling, could nevertheless be a productive exercise for modeling stability. “The basic idea is a dynamic feedback system,” he wrote, citing Rapoport’s 1957 article. Schelling reserved the term “equilibrium” to describe an arms race under control, “a situation in which the urge on each side to accumulate additional military forces or to accumulate forces of a ‘wrong’ kind is not irresistible...” He kept the term “stability” for deterrence. An arms race in equilibrium might well encourage deterrence stability: “Stability may...depend on this equilibrium’s not being too sensitive to changes in force levels...or to technological change,” he wrote. For Schelling, the main event was always the possible precipitation of war, not the arms race itself. He was fascinated by the stability of deterrence under strain: crisis stability. For him, qualitative or technological arms races posed the greater danger, because some improvements in technology (greater warhead accuracy, to cite the most notorious example) might encourage a dangerous temptation to launch the first blow when the chips were down. “An arms race is a leisurely and relaxed phenomenon compared with another dynamic feedback process,” Schelling remarked in the same paper. “This is the race to strike first when one side fears that the other is about to launch an attack. This is the problem of ‘inadvertent war,’ or ‘accidental war,’ war by mistake or misunderstanding, war by mutual panic.”

the arms race is.”¹⁹⁸ More thorough was the RAND economist James Schlesinger. In 1967, not long before beginning his meteoric rise through the government, Schlesinger argued that the “rationalistic approach to arms control, which dominates the public discussion, tends to rest upon speculation and syllogisms”—innocent of messy bureaucratic and economic complexities. Weapons procurement, in reality, was slow and painful. But in the mental world of the arms controllers, “diabolically clever measures and countermeasures follow one another, and we are off into an ingenious—and, incidentally, a costless and frictionless—arms spiral.”¹⁹⁹ For Albert Wohlstetter, too, the arms controllers had abused the nuance of nuclear strategy and policy with the rough tools of arms race theory. “Action-reaction language is vague enough to rationalize events after the fact,” he wrote; it was “a glass through which we saw darkly.”²⁰⁰

But for the arms controllers, MIRV fit snugly onto the other side of the perverse equation set up by ABM. “These systems,” George Rathjens wrote, “one defensive and the other offensive, can usefully be discussed together because of the way they interact. In fact, the intrinsic dynamics of the arms race can be effectively illustrated by concentrating on these two developments.”²⁰¹ As the political winds permitted, the arms control opposition focused its efforts on MIRV. Many of the experts had concluded that time was of the essence. As Freeman

¹⁹⁸ “Joint Arms Control Seminar, Minutes of the Twelfth Session, May 10, 1965,” Box 3, Folder 5 “Joint MIT-Harvard Arms Control Seminar – Minutes, 1964-65,” *LPB*.

¹⁹⁹ James R. Schlesinger, “Arms Interactions and Arms Control,” RAND Paper P-3881 (September 1968), on 1 and 2. This was Schlesinger’s starting point for his own objections to arms control: not because the arms situation demanded flexibility (as many nuclear weapons enthusiasts argued), but because it was so tremendously inert and sluggish. He wrote (on 14): “Much of the domestic debate on arms control has been couched in a hypothetical manner that disregards the budgetary and bureaucratic constraints facing any nation. It presupposes a degree of responsiveness to the deployment decisions of a rival that is historically questionable. What if deployment decisions and plans are, in fact, unresponsive? I would argue that the Soviet program for offensive forces was designed years ago, that it will be completed irrespective of arms control initiatives from the United States, and that it might not be further expanded unless American activities ‘shock’ the Soviets out of a preconceived mold.” For a summary of Schlesinger’s career, see Robert D. McFadden, “James R. Schlesinger, willful aide to three Presidents, is dead at 85,” *New York Times* (27 March 2014): A18.

²⁰⁰ Albert Wohlstetter, “Racing forward? Or ambling back?,” in *Defending America* (New York: Basic Books, 1977), 119-142, on 133.

²⁰¹ George W. Rathjens, “The dynamics of the arms race,” *Scientific American* 220, no. 4 (1969): 15-25, on 15.

Dyson, no friend to MIRV himself, explained the objection in a talk before the Council for a Livable World that year, the “deployment of MIRV will represent a major escalation of the arms-race: provocative, destabilizing, and inconsistent with reasonable measures of arms control.” Dyson reprised his argument of several years earlier—that the U.S. should build its own ABM in response to a Soviet ABM.²⁰² But for Rathjens and his compatriots this would encourage the Soviets to develop their own MIRV in response. To risk having either ABM or MIRV now was to risk having both. It would hazard the kind of instability that had troubled Murray Gell-Mann in 1962—the possibility, as Rathjens worded it, “that one of the superpowers with an ABM system might develop MIRV’s to the point where it could use them to destroy the bulk of its adversary’s ICBM force in a preemptive attack.” The retaliatory response the ABM would have to deal with would then be “much degraded,” increasing the temptation.²⁰³

Suppose the superpowers agreed not to field MIRVed missiles. How would such an agreement be verified? Just retired from several years working on compliance and verification problems within ACDA’s science and technology bureau, Herbert Scoville explained some of these challenges to a subcommittee of the House Foreign Affairs Committee. Scoville cited the standard worries about MIRV—that it had all the trappings of a first-strike weapon, that it would ratchet up the arms race. But MIRVs were also a special arms control nightmare because they invisibly multiplied the number of strategic nuclear warheads, concealed tooth-like inside the nose cone of each missile. Satellites could see missiles, sitting on launch pads or buried in silos; an arms control agreement could plausibly limit the number of deployed missiles. But satellites could not see the individual reentry vehicles inside the missiles. Verifying the number of reentry vehicles would demand more intimate inspection techniques—the dreaded “on-site inspections”

²⁰² Freeman J. Dyson, “Statement on MIRV prepared for the Council for a Livable World,” February 1969, Box 80, Folder “Salt – B.T. Feld, 1963-78 (2 of 2),” *BTF*.

²⁰³ Rathjens, “The dynamics of the arms race,” 21.

that were the millstone around the neck of U.S.-Soviet arms control negotiations. No Moscow politician wanted American inspectors crawling over every missile base and submarine in the Soviet Union. And no American military planner savored the thought of a Soviet inspector (literally) X-raying the business end of an American missile to check the number and characteristics of the enclosed reentry vehicles, more sophisticated than what the Soviets had produced on their own.

“Therefore,” Scoville explained, “if MIRV deployment is to be prevented, it is necessary to control MIRV testing.” This would clip MIRV’s wings: no testing meant limited accuracy and reliability, reducing the chances that MIRV would ever become a credible first-strike threat. Scoville knew from his days at the CIA that “the U.S. has been monitoring Soviet missile firings since before the first Soviet ICBM test in August 1957.” And he also knew that the U.S. could see Soviet missile tests in midcourse phase, where the separation of the reentry vehicles would occur. The Soviets could see the American tests, too. There would be no problem, from a technical standpoint, with verifying a MIRV test ban.²⁰⁴

So testing was where the political fight over MIRV concentrated. By the time the arms controllers began agitating strongly for a MIRV test ban, the MIRV tests had already begun. Jerome Wiesner helped orchestrate a petition sent to the Pentagon in August 1968, asking for the tests to be postponed. It was to no avail. On the 16th the first successful flight-tests of the missiles that would carry MIRV (the silo-based Minuteman III and the submarine-launched Poseidon)

²⁰⁴ “Opening Statement – Herbert Scoville, Jr., Hearings by the Subcommittee on National Security Policy and Scientific Developments of the House Foreign Affairs Committee,” Box 9, Folder 1 “Writings 1954, 1969,” HSP. Scoville also presented these arguments in two *New York Times* articles from early 1971: Herbert Scoville, “The problem of MIRV: I,” *New York Times* (8 February 1971): 33; Herbert Scoville, “The problem of MIRV: II,” *New York Times* (9 February 1971), 39. Also on the rationalization for a MIRV flight test ban, see G.W. Rathjens and G.B. Kistiakowsky, “The limitation of strategic arms,” *Scientific American* 222, no. 1 (1970): 19-29.

were carried out at Cape Kennedy. The dawn launch of the Poseidon “left a vapor trail of many colors,” impressing observers as “one of the most beautiful they had seen.”²⁰⁵

Some of the well-connected arms controllers soon had a chance to try their anti-MIRV case at a high level, in an ad hoc committee convened by Henry Kissinger (apparently because Kissinger distrusted the CIA’s insistence that the Soviets were not developing a genuine MIRV capability, and wanted his own team of analysts to look into the matter). He turned to his Harvard colleague and friend from years of arms control discussions in Cambridge, Paul Doty, to head the group. Joining Doty were Rathjens, Jack Ruina, Richard Garwin, and two Stanford University physicists who had distinguished themselves as trenchant critics of ABM, Sidney Drell and Wolfgang Panofsky. The Doty group, in the words of SALT chronicler John Newhouse, “reached no conclusions, but exhaustively laid out data and identified areas of disagreement, including the reliability of a ban on MIRV testing.”²⁰⁶ In a separate memorandum to Kissinger, however, Rathjens and Ruina argued more fervently that there would be no way to regulate MIRV without a ban on MIRV tests. Current MIRV systems had achieved nowhere near the accuracy needed to carry out counterforce attacks; but should testing continue, “even for only a few more weeks,” the arms controllers would have that much extra trouble guaranteeing that the Soviets had not developed a counterforce capability (and all the more trouble fending off the challenges of Albert Wohlstetter).²⁰⁷ An interagency Verification Panel appointed by Kissinger to prepare the U.S. SALT negotiating position agreed that the problem was time urgent. The U.S. was perfectly able to detect Soviet MIRV tests, but if the Soviets fielded MIRV before a test

²⁰⁵ Rosenblith, ed., *Jerry Wiesner*, 308; John Noble Wilford, “Two new missiles to get test today,” *New York Times* (16 August 1968): 1; John Noble Wilford, “2 multiple missiles pass tests in flight,” *New York Times* (17 August 1968): 1.

²⁰⁶ Newhouse, *Cold Dawn*, 161.

²⁰⁷ George Rathjens and Jack Ruina, “A Moratorium on the Testing of Multiple Warheads,” 2 June 1969, reprinted in Herken, *Cardinal Choices*, 247-249, quotation on 247.

moratorium had been signed, “verification of a ban on actual deployment of MIRVs would be difficult, if not impossible, by national means.”²⁰⁸

The Pentagon, on the other hand, argued that the Soviets could get away with testing their MIRVs and the U.S. would never know. DDR&E John Foster appointed a special panel of outside experts to evaluate MIRV test ban evasion techniques. The Fink Panel—named for its chair, Daniel Fink, the former ARPA official who had joined Donald Brennan in debating against Wiesner on the ABM, and who was now general manager of the Space Division of General Electric, a MIRV contractor—concluded that the Soviets could exploit at least a couple of loopholes. (For example, they could fire their MIRV tests along trajectories completely inside the huge landmass of the Soviet Union.) Staff from ACDA and the CIA who had served on the Verification Panel disagreed angrily with the Fink report at a high-level meeting in the late summer.²⁰⁹

By that point the lid had blown well off the interagency controversy over MIRV testing; it was virtually the stuff of water-cooler conversation. At MIT the physicist FAS organizer Leo Sartori, with his colleagues Philip Morrison and Francis Low, circulated a petition to the entire MIT faculty condemning the arms race in general and MIRV in specific.²¹⁰ Persuaded that the issue could use more of the limelight, Sartori took it to the glossy pages of the *Saturday Review*, arguing in a long article that MIRV tests were detectable “with some confidence.” “The initials MIRV, virtually unheard of six months ago, now appear on the front pages every day,” he

²⁰⁸ Clarence W. Baier, Memorandum of the NSSM-28 Working Group, “Monitoring Soviet Compliance with a MIRV Flight-test Ban,” 10 June 1969, available from the Central Intelligence Agency Freedom of Information Act Electronic Reading Room, <http://www.foia.cia.gov/>.

²⁰⁹ Robert Kleiman, “After ABM: Nixon confronts a momentous decision on the hydra-headed MIRV,” *New York Times* (17 August 1969): E2.

²¹⁰ The petition can be found in Box 80, Folder “Salt – B.T. Feld, 1969-77 (1 of 2),” *BTF*.

noted.²¹¹ Not long after, when *Playboy* interviewed the prominent conservative personality William F. Buckley, Jr., the interviewer, who had just seen Sartori's piece, used it to challenge Buckley's stance on national security. A professor at MIT, the interviewer explained, "implies that some of our ICBMs are aimed at Russia's missiles rather than at her cities. Doesn't this indicate that the U.S. is prepared—to the point of overkill—for a massive first strike against the Soviet Union?" "Look," answered Buckley, bored by all the hairsplitting and handwringing. "The intellectual, attempting to evaluate the military situation, tends to fasten on a frozen position. [...] But it is the responsibility of the military to understand how military confrontations actually work..."²¹²

* * * *

The military understood that it needed more MIRV tests, and more MIRV tests it got. The arms control experts, as they had learned during their experience with ABM, cultivated Congressional allies to pursue the case on Capitol Hill. In the spring of 1969 Senator Edward Brooke, Ted Kennedy's junior colleague from Massachusetts, took the lead in fighting MIRV by co-sponsoring a Senate resolution demanding a halt to MIRV tests. How had this Republican Senator come to oppose the defense program of a Republican administration? Anti-war sentiment, in part—but more immediately because his legislative assistant on foreign policy was Alton Frye, a fellow at the Harvard CfIA who had first grown concerned about the implications

²¹¹ Leo Sartori, "The myth of MIRV," *Saturday Review* (30 August 1969): 10-15. In 1970 the British strategic analyst Ian Bellany published a paper in the journal *Nature* apparently showing that MIRVing strategic missiles to an optimal number of warheads would improve, rather than endanger, strategic stability. Sartori read the paper and had trouble swallowing the argument. He replied with his own calculation challenging Bellany's result, vetting it first with fellow MIT physicists Steven Weinberg and Philip Morrison (Bernard Feld got a copy, too). L. Sartori, "MIRVs and the strategic balance," *Nature* 228 (1970): 53; also see the draft in Feld's papers: L. Sartori, "MIRVs and the Strategic Balance," Box 80, Folder "Salt - B.T. Feld, 1963-78 (2 of 2)," *BTF*.

²¹² William F. Meehan, III, ed., *Conversations with William F. Buckley, Jr.* (Jackson, MS: The University Press of Mississippi, 2009), 5.

of MIRV as a RAND analyst in the mid-1960s.²¹³ Meanwhile another MIT professor who had been caught up in the furor over MIRV, the economist Edwin Kuh (who had first gotten involved in arms control advocacy during the March protests at the university), worked to enlist Senator George McGovern in the push for a MIRV test moratorium. In May, Kuh and Bernard Feld made a trip to Washington to meet with some of McGovern's staff. Within days McGovern's office had drafted a letter over his signature, calling missile defense and MIRV "the most critical and immediate national security issues presently facing the country." Sent to a handful of experts, the letter had McGovern insisting that a unilateral halt to MIRV testing was desirable before the U.S. commenced arms limitation talks with the Soviets.²¹⁴

For some of the arms controllers, however, there was a creeping sense that they had grasped the essence of the problem just in time for it to slip through their fingers. Herbert York was one of the experts to receive McGovern's MIRV letter. York replied that McGovern's intuitions were correct: "The most direct relationship is between offense on one side and defense on the other, and both sides are acting out of fear of something, presumably a first strike on the part of the other." He was happy to support the MIRV test-ban idea, too, "but I suggest it may in practice already be too late." The Soviets would be convinced that the Americans were too far along with their MIRV testing program, therefore pushing ahead with their own testing to match it. The U.S. would read this as a sign of Soviet unwillingness to compromise.²¹⁵

²¹³ John W. Finney, "Debate over military: MIRV may spell a fateful crossroads in the arms race," *New York Times* (8 June 1969): E2; Interview with Alton Frye by Don Nicoll, 12 April 2001, The Edmund S. Muskie Archives and Special Collections Library, Bates College, Lewiston, ME; Alton Frye, "Space arms control: Trends, concepts, prospects," RAND Paper P-2873 (February 1964).

²¹⁴ John Holum to Edwin Kuh, 20 May 1969; Edwin Kuh to Bernard Feld, 28 May 1969; Edwin Kuh to George McGovern, 29 May 1969; "Copy of letter sent to attached list – transcription is approximate," Box 80, Folder "Salt – B.T. Feld, 1969-77 (1 of 2)," *BTF*.

²¹⁵ George McGovern to Herbert York, 4 June 1969; Herbert York to George McGovern, 12 June 1969, Box 44, Folder 7 "Antiballistic Missile (ABM), Correspondence, 1969," *HFY*.

No arms controller embodied arms-race burnout better than York. The day after writing McGovern, he traveled up to Pasadena to give the commencement address at Caltech. In a somber message to the class of 1969, he described for the students how the nuclear contest had molded his career from top to bottom. “My entire professional life has been dominated by the arms race,” beginning in his sophomore year as a physics major at the University of Rochester. His teachers “began to disappear one by one into various secret war programs,” he said; his own graduation had been “hurried-up” so that he could jump into work on isotope separation at the Lawrence Berkeley Laboratory. His Ph.D. in 1949 came just in time for the Soviets to explode their first atomic weapon. Thereafter he was carried up into various “high administrative positions, sometimes deep within the arms race, sometimes outside, but even then always within its shadow.”

York had spent the previous spring “once again completely immersed in the arms race,” battling ABM, “because I believed it would be a grave mistake for a number of reasons, the most important being that it would seriously accelerate the arms race just at a time when there appears to be promise of getting it under control.” MIRV and ABM threatened the impending talks on two fronts: MIRV because it made it harder for one side to see plainly what the other side’s offensive capacity was; ABM because it was impossible to know how well defenses might work. In combination they would “induce further moves along the arms race spiral,” and now, as the hot and unruly summer of 1969 descended, it seemed there was no hope of turning back.²¹⁶

York had drawn much of the speech from a book manuscript he was working on. *Race to Oblivion* would chart the history of the technological arms race to show how these latest developments were links in a longer chain. It was an insider’s account—the first of its kind—of

²¹⁶ Herbert F. York, Commencement Address at the California Institute of Technology, 13 June 1969, reprinted as “A personal view of the arms race,” *Bulletin of the Atomic Scientists* 26, no. 3 (March 1970): 27-31, quotations on 27 and 31.

the major post-Manhattan Project milestones of the nuclear age, from H-bombs to the missile gap. York capped off the book with a section on “unbalancing the balance of terror” featuring a chapter on MIRV and two chapters on ABM. In his public appearances and in print York cultivated weariness and technological fatalism. For fifteen years he had watched one iteration of the arms race after another, cycling from conception to development to deployment with inexorable momentum. Back in 1960 Thomas Schelling had said that meaningful arms control meant channeling strategic weapons technology into more stabilizing forms. But now a decade later, with ABM and MIRV, and the Soviets racing to match the Americans’ calamitously huge nuclear arsenal, York had to conclude that the idea of rationally guiding advanced weapons technology was a delusion. “The technological side of the arms race has a life of its own, almost independent of policy and politics,” he wrote.²¹⁷ The ideas and gadgets that went into making MIRV, the various arguments devised to rationalize it, had intertwined in such a “complex web” that “it is unlikely that the development of MIRV could have been successfully stopped by a conscious administrative decision to do so.”²¹⁸

Trying to axe MIRV by itself was a superficial response to a much deeper problem. It was impossible to kill such a program by challenging it in isolation (with a test moratorium, say). There were too many bureaucratic actors invested in its development, too many reasons to deploy it. MIRV could “only be slowed or stopped by slowing or stopping the arms race as a whole,” York asserted in a solemn echo of Jerome Wiesner’s proposal for comprehensive arms control from 1960. Sure enough, MIRV entered service in a flight of ten new Minuteman III missiles based in North Dakota in June 1970. Brooke’s MIRV resolution failed in the Senate; the MIRV test ban proposal never got past White House review during the SALT negotiations.

²¹⁷ Herbert F. York, *Race to Oblivion: A Participant’s View of the Arms Race* (New York: Simon & Schuster, 1970), quotation on 173.

²¹⁸ Herbert York, “The Origins of MIRV,” SIPRI Research Report No. 9 (August 1973), on 7.

Instead Kissinger insisted that any discussion of MIRV restriction be linked to on-site inspections, something the Soviets were unprepared to accept. (Thus the eventual SALT Interim Agreement of 1972 froze current levels of ICBM “launchers,” but said nothing about MIRV.)²¹⁹ Meanwhile three more Congressional attempts to limit funding for the Safeguard ABM in 1970 also ran aground. York remarked in Senate testimony that year, “We thus see that the whole process has made one full turn around the spiral: Soviet ABM led to U.S. MIRV; U.S. MIRV led to Soviet MIRV; Soviet MIRV leads to U.S. ABM.”²²⁰

It suggests the depth of York’s frustration that he soon found himself arguing in favor of points that the arms controllers had spent months arguing against. The Soviet MIRV program, he now said, would make the Minuteman missile vulnerable to a first strike—a claim that Wiesner, Rathjens and others had worked so hard to downplay and discredit. In the heat of the ABM battle Wohlstetter had used it as a justification for the Safeguard system protecting Minuteman sites. York reached a rather different conclusion: get rid of all the Minutemen, and then there would be no need for ABM at all. He put flesh on this idea in a *Life* magazine article in 1970, unveiling a ten-point plan to “reverse the arms race.” These included propositions to abandon all land-based ICBMs, base the entire deterrent force on a few bombers and submarine-launched missiles,

²¹⁹ See Michael Krepon, *Strategic Stalemate: Nuclear Weapons and Arms Control in American Politics* (London: The MacMillan Press, 1984), 98-101. The text of the Interim Agreement is available online at U.S. Department of State, “Interim Agreement Between The United States of America and The Union of Soviet Socialist Republics on Certain Measures With Respect to the Limitation of Strategic Offensive Arms,” <http://www.state.gov/t/isn/4795.htm>.

²²⁰ In Ted Greenwood’s judgment, a ban on MIRV testing or deployment was never a serious consideration at SALT. “An agreement on MIRVs was not something that was barely missed at SALT I or that just kept eluding negotiators. Neither side really wanted such an agreement and neither side really tried to get one.... All the speechmaking and writing of MIRV opponents inside and outside the Congress, all the editorializing, all the testimony at congressional hearings, all the behind-the-scenes maneuvering that finally resulted in passage of an amended version of Senator Brooke’s Senate Resolution 211, while perhaps contributing to public education and building the Senate’s competence and interest in dealing with complex weapon system and national security issues, did not in any important way inhibit or even delay the MIRV deployments.” Greenwood, *Making the MIRV*, 135. On additional battles over Safeguard, see Robert M. Smith, “Senate defeats new bid to limit ABM expansion,” *New York Times* (20 August 1970): 1. York’s 1970 testimony can be found in “Statement by Herbert F. York before the Subcommittee on Arms Control, International Law and Organization of the Senate Foreign Relations Committee,” 8 April 1970, Box 19, Folder “ABM #3,” *JBW*.

and—remarkably—allow those missiles to be MIRVed. This would at least guarantee that neither side would be tempted to build an ABM defense, since MIRV made ABM useless. Extreme times called for extreme measures. “Unless we nerve ourselves to make the attempt, and make it soon,” York wrote, “we are quite simply doomed.”²²¹

Conclusion

“I believe that the concept of mutual assured destruction provides one of the few instances in which the obvious acronym for something yields at once the appropriate description for it; that is, a Mutual Assured Destruction posture as a goal is, almost literally, mad. MAD.” So Donald Brennan wrote in a two-part *New York Times* essay in 1972, giving writers ever after a concise way to denote and denounce nuclear deterrence at the same time. In the piece, Brennan, channeling Leo Szilard and Herman Kahn, proceeded to highlight the strangeness of MAD by describing the simplest way to implement it. “We could install very large thermonuclear weapons with secure firing arrangements in Moscow, Leningrad, Kiev, and so on,” he wrote, “while the Soviets could install similar weapons and arrangements in New York City, Chicago, Los Angeles, and so on.” No weapons vulnerabilities; no arms race issues. So obviously insane was this proposal that Brennan considered it a “reduction-to-absurdity argument” against the doctrine of assured destruction, a conclusive demonstration that “something must be wrong with MAD as a way of life.” How had anyone ever taken such an idea so seriously? Brennan blamed analysts who had become intoxicated with their strategic models—sophisticated and fun, but dangerous oversimplifications of reality. “Indeed, some few technical people, who have at least had the integrity to follow the logic of their analysis to its conclusion, have been so bemused by

²²¹ Herbert F. York, “Military technology and national security,” *Scientific American* 221, no. 2 (1969): 17-29; Herbert F. York, “We can reverse the arms race: a 10-point plan,” *Life* (11 December 1970): 40-41, quotation on 41.

these models that they have seriously advocated the actual deployment of a mined-city system.” This acidulous compliment was, of course, meant for Richard Garwin.²²²

The ABM and MIRV disputes of the late 1960s had exposed and amplified longstanding intellectual rifts in the community of strategy and arms control experts. Differing intuitions about the meaning of arms control and the appropriate balance between deterrence and defense had prevailed since the late 1950s. A decade later the differences seemed starker, the disagreements louder. For those of an arms control sensibility, deterrence was foundational, but the main event was the arms race, a complex phenomenon with predictable and controllable dynamics.

“The scientists have convinced the political leaders and the public that the supremacy of offensive weapons is an unalterable scientific fact,” Freeman Dyson would write in 1979. “They argue that because the supremacy of the offensive is unalterable, the strategy of mutual assured destruction is the best among the dismal alternatives that are open to us.” That is half true. For the arms controllers the defense of cities was anathema because it tampered with the stability of deterrence, upon which everything else rested. Defense of missiles was acceptable in theory, but too close to city defense, in practice, for comfort. Missile defense enthusiasts argued that improved technology had rebalanced the scales of deterrence, making it possible to rely more on antiballistic missiles than missiles targeted at cities. Donald Brennan believed that; Dyson wrote that “there is nothing in the laws of physics and chemistry” that would prevent it from being true.²²³ But arms controllers knew—from years of personal experience in specific institutions—about the irresistible ability of offensive missiles to puncture the defense. It wasn’t that defense

²²² Donald G. Brennan, “Strategic alternatives: I,” *New York Times* (24 May 1971): 31; Donald G. Brennan, “Strategic alternatives: II,” *New York Times* (25 May 1971): 39.

²²³ Even Jack Ruina had to admit in 1964 that “whereas in the days of Nike-Zeus the issue was whether the defensive system could work at all against an attack designed to cope with it, in contrast now... Nike-X offers a very substantial challenge to an offensive system.” J.P. Ruina, “A Comment on Future Weapons Systems,” Jason Internal Note N-172 (3 September 1964), Document NH00790, *DNSA*.

would not work. It would not work in a way that would revise the fundamentals of deterrence. The deeper problem, which apparently failed to move defense enthusiasts like Donald Brennan, doomsayers like Albert Wohlstetter, and other arms race skeptics, was that missile defense would inflame the most dangerous weapons competition in history. ABM all but compelled efforts to defeat it. By mid-1969, it seemed too obvious that MIRV, and the monstrous numbers of warheads it allowed, had been the result.²²⁴

Most scholars have placed this back-and-forth in somewhat hazy context, framing ABM as a showdown between scientists and the government. So Gregg Herken writes of Hans Bethe—arguably the best-known scientist-critic of ABM—that he was “a vocal and consistent opponent of missile defense in all its incarnations,” and that his 1968 article with Richard Garwin had “represented a certain consensus of scientific opinion” in its famed take-down of the Sentinel system. But a more textured picture emerges when Bethe in particular and the ABM episode in general are set against a deeper background—when Bethe’s arguments are seen in light of his career as a defense consultant, and as a member of the small community of expert arms controllers. It is hard to claim, for example, that Bethe opposed ABM in every incarnation when, during Senate testimony in March 1969 (two days after he had lectured the students at MIT on the exchange ratio), he said he was “in favor” of missile defense of Minuteman silos “at the appropriate time.” “My main reason is that such deployment would stabilize the situation rather than the opposite. You are all familiar with the theory of the stable deterrent...” When the Safeguard system proposal came out a few days later, the arms controllers criticized it a warmed-over version of Sentinel, not because missile defense violated the laws of physics.²²⁵

²²⁴ Dyson, “Disturbing the universe,” 72-73.

²²⁵ Hans Bethe, “Can the ABM protect us?,” *Boston Globe* (10 March 1969): 7; Richard H. Stewart, “Scientists prefer talks to reliance on ABM,” *Boston Globe* (7 March 1969): 15.

If there was a scientific “consensus” against missile defense, it was one Bethe and Garwin had done more than anyone else to construct by writing their article in the first place. They had been able to criticize the Sentinel system in such gripping detail because they had worked on the thing themselves, had helped the AVCO Corporation figure out what happens when a reentry vehicle comes back to earth—had even flirted with censure by the Defense Department for revealing sensitive information. At MIT, Bethe told the student protesters, “I could not have given you the arguments tonight, nor could I have given similar arguments a year ago, if I were not...an ‘in’ man.... In fact, without the in men you probably would never have known that the antiballistic missile system is dangerous.”²²⁶

In or out, private or public: the arms controllers had feet in both worlds. Herbert York said he’d had a “front-row-center seat” to the nuclear arms race as a civilian DOD official. But the metaphor is too passive. York hadn’t been in the audience but on the stage. He had been one of the bureaucrats who had made the decisions and shunted the money to put nuclear deterrence into action. And he knew—as did all the arms controllers who lent themselves to the political clashes of the late 1960s—that the fight was about much more than just a weapons system and whether or not it would work. Just before delivering his address to the Caltech class of 1969, York wrote to Caltech’s president Harold Brown, who had been York’s replacement as DDR&E in 1961. “I don’t think the ABM by itself constitutes a particularly awful step,” York said—a stunning private admission right in the midst of his nationally visible campaign against missile defense. “But it does provide a singularly easy case to discuss in public, and to use as a vehicle for bringing up other points which might not be subject to open debate and criticism.”²²⁷ What the ABM debate had done was to ventilate, for the first time during the Cold War, the premises

²²⁶ Herken, *Counsels of War*, 231. Bethe quoted in Allen, ed., *March 4*, 150.

²²⁷ York, *Race to Oblivion*, 24; Herbert York to Harold Brown, 18 March 1969, Box 44, Folder 8 “Antiballistic Missile (ABM), Correspondence, 1969,” *HFY*.

of nuclear security and the dynamics of the arms race. It had been a watershed event in the relationship between the state and some of its most prized experts.

The lines between “insiders” and “outsiders” in the ABM debate had become blurry in almost every way imaginable. There simply was no such thing as “independent” or “detached” critique of ABM for the arms controllers, who’d been tangled up in defense advising, contract consulting, and classified clearances for years (in some cases decades). And even when many of them contemplated the areas—the specific neighborhoods—in which the massive, menacing ABM architecture would be built, they were thinking of their own homes, not remote and abstract spaces. What had taken place was a radical change of venue for expert discussion about formerly arcane, secretive topics like missile defense, nuclear weapons systems, and strategy. At the end of the 1960s, arms control had gone public in a completely unprecedented way. What was new was that “insider” expert debate had been taken to the public stage—not that “outsider” scientists had suddenly answered an obligation to the truth, and spoken out against the government.

“Anyone can be a hero by talking to the outside,” said an irritated Henry Kissinger over the telephone to a sympathetic Paul Doty in 1970. And yet there was Doty, a few months earlier, standing before a meeting of the South Shore Committee on Peace in Hingham, Massachusetts, telling the assembled that ABM would “reduce stability with the U.S.S.R. and increase the danger of escalation.”²²⁸ Heroic motivations notwithstanding, the arms controllers now spoke to audiences and took part in activities that would have been unthinkable in 1960. There was George Rathjens, keeping up his hectic schedule of speaking in front of town meetings and

²²⁸ Telecon, Professor Doty, February 1, 1970, Document KA02105, *DNSA*; “ABM called ‘useless’ by Harvard’s Doty,” *Boston Globe* (20 February 1969): 5.

Congressional committees.²²⁹ Jeremy Stone, now the national director of the Federation of American Scientists, could be seen criticizing ABM in 1970 on one of the first episodes of the new PBS television show *The Advocates*, hosted by Harvard law professor Roger Fisher, whom Stone had first met in the Harvard-MIT arms control seminar years before.²³⁰

And there were Jerome Wiesner and Bernard Feld, taking part in a rally at New York's Madison Square Garden, onstage with actors Paul Newman, Lauren Bacall, Tony Randall, and George Segal. A scripted skit was supposed to educate the audience about the dangers of missile defense. The actors humorously played up their ignorance and naïve faith in the government's promises for ABM, while the experts chimed in periodically with sober corrections. Wiesner explained the shortcomings of ABM and the dangers of MIRV, running down a list of forthcoming weapons programs and their ominous acronyms. "I think I'm going crazy," Segal said. He turned to Feld and asked him, "Where's all this going to end?" Feld answered: "There are two possibilities. We can keep going the way it has been described, developing weapon after weapon with the Russians keeping up with us weapon after weapon. In that way lies disaster. Eventually they blow up and with it all of us. Or, we can try to control it. The second path we call arms control."²³¹

²²⁹ "2 towns will ask ABM delay," *Boston Globe* (4 February 1969): 16.

²³⁰ Stone, *Every Man Should Try*, 38-39;

²³¹ "Rallies held here and on the Coast to oppose ABM," *New York Times* (26 June 1969): 27. "ABM," Box 80, Folder "Salt - B.T. Feld, 1969-77 (1 of 2)," *BTF*. The actors hammed it up for the audience, getting intentionally lost in the nuclear lingo. Randall: "First of all, understand that A.B.M. is not a missile...oh, no, sir... A.B.M. is an entire system, code-word: 'SAFEGUARD'." Segal: "It's a deodorant?"

CHAPTER 4

Gifted Amateurs: The Government, Private Foundations, and The Support of Arms Control Expertise

I should like to warn against the expectation that any amount of advanced planning and study, no matter how thorough, will see the problem completely solved. But a start must be made. First, and second, and continuing studies must be initiated. Research and development on a large scale are necessary.

— Jerome Wiesner (1960)¹

Sadly, we have found that men and institutions do not have the wisdom, wit or resources to find “answers” to arms control issues.

— William Bader (1971)²

Introduction

“The ‘Charles River Gang’—a disarming band of Harvard and M.I.T. professors who run an arms-control operation that stretches right into the White House—have found themselves pleasantly disarmed by President Nixon.” This bit of wordplay ran in a 1971 *New York Times* article. The writer, frequent defense and foreign affairs reporter John W. Finney, had gone up to Cambridge to talk about the SALT negotiations with a few of the experts who’d had starring roles in the recent ABM debate, including Paul Doty, who Finney described as an “academic confidante” of national security advisor Henry Kissinger; Jerome Wiesner, a little less hopeful

Notes

¹ Jerome Wiesner, “Foreword,” in Donald G. Brennan, ed., *Arms Control, Disarmament, and National Security* (New York: George Braziller, 1961), 13-16, on 15.

² William Bader, “The Foundation’s Programs in Arms Control and International Security,” *The Ford Foundation*, December 1971, Ford Foundation Grant 08000495, *FFG*.

about SALT than Doty; George Kistiakowsky, who thought Nixon's interest in arms control smelled like a PR ploy in advance of an election. "To a man," Finney wrote, "they approve of the President's decision to press ahead with an initial agreement with the Soviet Union limiting defensive antiballistic missile systems," plus a "somewhat vague" guarantee to limit offensive weapons as well.³

The romance, if there had been one, was bound to fade between Nixon and the Charles River Gang. Nixon was privately dismissive of arms control, utterly uninterested in the technical arcana that kept arms controllers tossing and turning at night. One day in 1972, after Kissinger had finished a closed-door discussion with Soviet ambassador Anatoly Dobrynin on a crucial SALT issue, he told the President, "It's highly complex, but nothing you want to bother with. It's how many radars should be at an ICBM defense site." Take care not to get hoodwinked by the Soviet negotiators on some trifling detail, Nixon instructed Kissinger at another point that year. Their conversation, recorded by Nixon's Oval Office taping system, had turned to a possible limit on submarine-launched missiles. "Let me put it like this, Henry: get everything you can." Much as Kistiakowsky had suspected, by Nixon's second term arms control had receded from its prominent billing on the President's public agenda; the administration's secret, at times seething hostility to arms control had started to come out into the open. By 1973 it was revealed that several of the arms controllers, including Wiesner, had earned spots on the White House "enemies list."⁴

³ John W. Finney, "Harvard-M.I.T. arms experts pleased by Nixon's move on talks," *New York Times* (27 May 1971): 2.

⁴ Nixon and Kissinger quoted in United States Department of State, *Foreign Relations of the United States, 1969-1976, Volume XIV, Soviet Union, October 1971-May 1972*, Document 126 and Document 225, respectively. Portions of Nixon's remarks in these meetings are also mentioned in Francis J. Gavin, *Nuclear Statecraft: History and Strategy in America's Atomic Age* (Ithaca, NY: Cornell University Press, 2012), 109. Also see "Lists of White House 'enemies' and memorandums relating to those named," *New York Times* (28 June 1973): 38. Bernard Feld and Jeremy Stone made the enemies list along with Wiesner.

Embattled though it was, nuclear arms control—as a field, a community, and a lobby—had surely become a fixture of U.S. political and intellectual life. And yet there was a feeling in the air in the early 1970s—even during the occasionally hopeful days of SALT—that arms control had entered a period of crisis. It was hard to say exactly how, or why. The fumbling, tortuous international negotiations were one thing. But the real crisis seemed to be within American borders. A feeling of exhaustion—of overused concepts and missed opportunities, of a professional community in stagnation and under constant threat—was bogging arms control down like the industrial sludge polluting the Charles River.

At the Ford Foundation, a few program officers began to look into the matter for themselves. The Foundation, under its president, the former national security advisor and arms control advocate McGeorge Bundy, was beginning to debate a major financial investment in arms control. Some of the staff began to take the temperature of the field, interviewing arms control types at several universities around the country. What they found on their field trip was discouraging. The intellectual health of arms control was, they reported, in a “sad state.” According to some they spoke to, “the basic conceptual work has been done,” and the sense of novelty and excitement that Thomas Schelling had described a dozen years earlier was “long since past.” According to others, “a serious barrier to work in the field has been the increasing isolation of the governmental process.” The executive had closed ranks around Nixon, “narrowing the circle of those responsible and informed in the international security field,” and making it hard for outsiders to get “the information they need and to develop a ‘market’ for their work...” The small and slowing trickle of new ideas from the academy couldn’t get through the walls around Nixon’s inner ring.

What *was* arms control, after all? Yes, it had come to mean high-level negotiations in dignified, musty European palaces, treaties written in elevated legalese. But in the United States arms control was also said to be a “field,” understood as a domain of specialist practice and a kind of knowledge, identified with a group of people called “arms controllers.” In one draft from 1969 the old-guard arms controller Bernard Feld even called it a “new academic discipline,” or perhaps a major branch of the “new, professional academic field of national security studies.” The Foundation officers weren’t so sure. “Is arms control a ‘profession’, a ‘field’, or largely—and more properly—the domain of ‘gifted amateurs’?” they asked in their 1972 internal report. They decided on the latter. Arms control “is not a profession nor even a well-defined academic field. The Killians, Kistiakowskys, Dotys and Panofskys of the arms control establishment, we would argue, are ‘gifted amateurs’ who are good arms controllers because they are good scientists.” Pry them from their academic lab benches (or mega-machines, at least in the case of Wolfgang Panofsky, who directed the Stanford Linear Accelerator Center) and you would cut off the lifeblood of their good sense as policymakers. Pull them permanently out of the universities and stuff them behind government desks, and their creativity would be snuffed.⁵

Whatever it was that made arms control “work” intellectually in the 1960s was missing. The golden age of arms control had “emerged from the collective experience and interaction of ‘clusters’ of political scientists, economists and physical scientists.” When the Ford officers spoke to the Stanford University physicist and arms controller Sidney Drell, he told them that Stanford was still “sub-critical” as a core of arms control expertise—that a good arms control program had to aim for a “‘mass’ of such talent necessary to duplicate that remarkable cluster of

⁵ The Ford Foundation, European and International Affairs Program, “Arms Control and International Security: A Need for University Centers?,” October 1972, Ford Foundation Report 010890, *FFR*. The Feld quotation is from “Proposal for a Center of Disarmament and Arms Control Technology,” Box 20, Folder 195 “Arms Control Studies, 1969-1973 (2 of 2),” *BTF*.

men in the Cambridge area in the 1950s.” Arms control’s sources of expertise and authority were local. It sprouted in academic soil, even though the market for which its intellectual products were tailored would always be in Washington, DC, and at the international negotiating table. It was a field (for lack of a better term) that flourished in a niche environment—somewhere in the space between the seminar room and the executive branch of the government.⁶

More than two decades of scholarship have produced a substantial literature on the “Cold War University.” Much has been revealed about the extraordinary impact of the federal leviathan on the life of scholarship and teaching in the postwar period. The government—especially the military—poured its unmatched reserves into American higher education and academic research, favoring some schools, and some fields, over others. Little was left untouched. Research contracts flooded into the science and engineering labs; entrepreneurial faculty built high-flying careers and administrators built campus infrastructure on the government dime. Giant instruments were constructed, serviced by immense teams of researchers. Graduate study was overhauled; the classroom rosters exploded, thanks in good part to the GI Bill, and teaching styles and content shifted in response. When priorities changed as the federal bankrolls contracted in the 1970s and the government began to pull up its twenty-year-old stakes in the academy, the effects were no less dramatic.⁷

An equally rich literature has explored the role of the private foundations in the life of American learning and research. During the Cold War, swamped by the federal investment in

⁶ The Ford Foundation, European and International Affairs Program, “Arms Control and International Security: A Need for University Centers?,” October 1972, Ford Foundation Report 010890, *FFR*.

⁷ Stuart W. Leslie, *The Cold War and American Science: The Military-Industrial-Academic Complex at MIT and Stanford* (New York: Columbia University Press, 1993); Rebecca S. Lowen, *Creating the Cold War University: The Transformation of Stanford* (Berkeley, CA: University of California Press, 1997); Howard Zinn, Laura Nader, and Immanuel Wallerstein, eds., *The Cold War and the University: Toward an Intellectual History of the Postwar Years* (New York: New Press, 1998); David Engerman, “Rethinking Cold War universities: Some recent histories,” *Journal of Cold War Studies* 5 (2003): 80–95; Christopher P. Loss, *Between Citizens and the State: The Politics of American Higher Education in the 20th Century* (Princeton, NJ: Princeton University Press, 2012); David Kaiser, *American Physics and the Cold War Bubble* (Chicago: University of Chicago Press, forthcoming).

science and engineering, they shifted their missions and their funding targets. The foundations became the major funders of social science research in the United States. And they were integral financiers and shapers of the U.S. Cold War mission abroad. The international divisions of the major foundations—especially Ford and Rockefeller—funneled millions of dollars into major development programs and campaigns that can only be thought of as “political warfare.” They undertook social engineering projects of vast ambitions, attempted to cure disease and construct schools and promote agriculture in the Third World. They rebuilt an infrastructure for science in Western Europe as a bulwark against communism—all in the name of freedom and democracy.⁸

The case of arms control expertise weaves among these narratives about the interlocking interests of the state, the foundations, and the academy, without quite fitting them. Arms control was surely a Cold War project, straddling the universities and the government. And yet it’s hard to claim that arms control (despite the ambitions of some of the arms controllers) was a site in which the interests of the state and the universities were harmonized and shaped to reinforce one another. Arms control will not serve as an example of what Stuart Leslie described as the “iron triangle between the military, the defense industry, and higher education.” Nor does its history trace the pattern provided by historians of the Cold War social sciences, which, like arms control, were buoyed by private foundation funding. Mark Solovey, for example, argues that the military, the National Science Foundation, and the Ford Foundation collaborated in their support for the social sciences, coordinating and “defining their responsibilities and activities in relationship to

⁸ Nils Gilman, *Mandarins of the Future: Modernization Theory in Cold War America* (Baltimore, MD: Johns Hopkins University Press, 2003); Matthew Connelly, *Fatal Misconception: The Struggle to Control World Population* (Cambridge, MA: Harvard University Press, 2008); Nick Cullather, *The Hungry World: America’s Cold War Battle against Poverty in Asia* (Cambridge, MA: Harvard University Press, 2010); David Ekbladh, *The Great American Mission: Modernization and the Construction of an American World Order* (Princeton, NJ: Princeton University Press, 2010); John Krige, *American Hegemony and the Postwar Reconstruction of Science in Europe* (Cambridge, MA: The MIT Press, 2006); David Engerman, *Know Your Enemy: The Rise and Fall of America’s Soviet Experts* (New York: Oxford University Press, 2009); Mark Solovey, *Shaky Foundations: The Politics-Patronage-Social Science Nexus in Cold War America* (New Brunswick, NJ: Rutgers University Press, 2013).

one another”—so much so that he understands them as moored to a common purpose. It is “useful to consider these patrons in terms of a single, albeit loosely integrated system,” he says, one that produced “striking commonalities in their efforts to advance the scientific and practical value of the social sciences.”⁹

But what of nuclear arms control, which stood at the same crossroads between universities, the state, and private interests? It tells a different story. Unlike DOD-funded projects during the Cold War, it was perpetually the unloved stepchild of government. Unlike the social and natural sciences, nuclear arms control had no prewar academic precedents, no robust disciplinary form into which either federal or foundation money could flow. It was all made at nearly the same time: the loosely assembled academic field, then the government agency. At the base of these efforts was, as always, a specific community, a group of people with personal ties and a shared history: the arms controllers. They crafted the new field of strategic arms control in academic and think tank settings, and developed arms control concepts in local studies and seminars during the field’s early days in the 1960s. They promoted the idea of creating a special agency of the executive dedicated to arms control research and negotiation. They helped draft and shepherd the legislation creating the Arms Control and Disarmament Agency (ACDA) in 1961. And when the government support of arms control seemed to crumble in the early 1970s, they came to the field’s rescue by cultivating ties with the wealthiest private foundation in America—the Ford Foundation, whose president was one of their kind. Along the way they created university-based arms control programs whose institutional descendants (if not quite their original mission) still survive today.

This chapter charts the shifting structures of institutional support of arms control expertise, beginning with the birth of ACDA. ACDA has often been written about as the

⁹ Leslie, *The Cold War and American Science*, 234; Solovey, *Shaky Foundations*, 3–4.

brainchild of the forward-looking Senators Hubert Humphrey and John F. Kennedy—or, less charitably, as an election-year political gambit and a sop to the peace movement. I try to picture ACDA in the way arms control experts themselves understood it. It was they and their fellow advisors and staffers, after all, who crafted the ideas and turned them into the speeches and bills that made ACDA possible. For them, the new agency meant government support of arms control negotiation and research; and research was the agency's most sacred function. ACDA meant lodging arms control expertise, for the first time, closer to the heart of national security policymaking.

The chapter then turns to the way arms control was most commonly found in academic settings: practiced and studied in local seminars and discussion groups, funded by private foundation money. The Harvard-MIT arms control seminar was the best known and most respected academic arms control salon. But there were others. The arms control seminar that sprouted at the California Institute of Technology in 1960, infused from the beginning with a substantial contingent from the RAND Corporation, is a good example of arms control in a distinctive, local flavor. This seminar would form the nucleus around which a later reincarnation would grow: the California Seminar on Arms Control and Foreign Policy, created with the help of Ford Foundation money in 1970.

As with the postwar social sciences, the Ford Foundation assumes disproportionate scale in the story of arms control expertise.¹⁰ But in contrast to the social sciences, in the case of arms control the Foundation's support didn't translate to a simple reinforcement of the state's interests. The state's interests in arms control were always murky, less than wholehearted,

¹⁰ Hunter Crowther-Heyck calls the Ford Foundation “easily the most influential single patron of the behavioral and social sciences in the 1950s,” and Roger Geiger deems Ford “the most significant external arbiter of the development of university research outside of the natural sciences” in the postwar period. Both are quoted in Solovey, *Shaky Foundations*, 104–105.

opposed by powerful actors inside the government. Arms control could even be undermined from the Oval Office. In the second term of the Nixon administration, when Nixon threatened to hobble ACDA (by cutting its budget and staff, and by appointing experts who had grown critical of some of arms control's traditional aims to high levels of authority in the agency), the Ford Foundation leapt to arms control's defense. This meant, by the mid-1970s, a multi-million-dollar program of funding for arms control research and teaching, based in a few well-placed universities where arms control expertise had thrived since the 1960s. This was no handshake between the state and the foundations at the Cold War nexus of patronage and power. It was remediation, serving a different vision of the national interest, plugging gaps that the government itself had created.

In this chapter I trace a complicated circulation of people and ideas and dollars across the membranes separating the universities, the government, and private foundations. The outcome of this movement wasn't merely institutional. Debates on critical arms control issues in the United States in the 1970s were shaped by the forms that arms control's institutional support took. Nuclear issues continued to spread out beyond the government's close management in the Nixon era, despite Nixon's efforts to control them. Dangerous strategic nuclear technologies—including ballistic missile accuracy—were studied and debated in open forums. They were turned into headline arms control issues by experts who seemed to have less and less official contact with the government, and a growing dependency on private money.

I. Government Support of Arms Control Expertise

A Fund of Knowledge: The Arms Control and Disarmament Agency

The idea of an official organization dedicated to expert arms control research wasn't cut from whole cloth by John F. Kennedy and Hubert Humphrey in 1960. The politicians took their cues from the expert community of arms controllers, who'd been discussing the idea for years. The plans took several forms. Some wanted to set up an arms control think tank, or perhaps a specialized arms control research office run by the President's Science Advisory Committee. But as debate proceeded through 1959 and 1960, the idea developed of fashioning an entirely new agency of the executive branch. The debate leading up to the agency's creation expressed a tension between centralization and dispersal—between building a powerful bureaucracy to rival the one around nuclear weapons design and deployment, and achieving arms control by making it a special Presidential responsibility. One theme was constant and common to every proposal: the need for arms control research, and mechanisms to increase the flow-rate between the creation of arms control knowledge its application to government policy.

Bernard Feld's Cambridge disarmament committee was the first group to seriously discuss and develop the idea of a new arms control research shop. As early as October 1958, Feld thought such a specialized center might suit "one of the organizations presently concerned with military technology such as the RAND Corp., ITEK Corp., or the M.I.T. Lincoln Laboratory"—natural choices for the Boston arms control crowd, which included members with ties to all three.¹¹ By 1959 the need seemed more urgent. The failure of the previous year's Geneva surprise-attack conference taught the arms controllers about the necessity of research and preparation. Donald Brennan drafted a long letter to James Killian of PSAC in which he

¹¹ "Possible Future Activities of the Committee on the Technical Problems of Arms Limitation," 27 October 1958, Box 11, Folder 98 "Federation of American Scientists, Disarmament Study Committee, 1956-1959 (2 of 3)," *BTF*.

complained that “our delegation was not adequately prepared technically, apart from the problem of political constraints, and the UN report of these negotiations tends to confirm this impression.” Brennan and the Cambridge committee proposed that PSAC serve as a “coordinating center” for arms control research and planning. Brennan likened such a body to “a full-time ‘research arm’ of [PSAC], similar to the function provided by the RAND Corporation for the Air Force.” Edward Purcell, a Harvard physicist and PSAC member who sat in on one of the Cambridge disarmament committee discussions, was impressed enough with the idea that he agreed to hand-deliver the message the Killian.¹²

The letter was timely. At a recent meeting between PSAC and Eisenhower, Jerome Wiesner had advocated for much the same thing (no coincidence, since Wiesner could regularly be spotted in Feld’s discussion group). Eisenhower bristled at the thought of creating a bulky new bureaucracy—“big bodies,” he called them. George Kistiakowsky, as the new head of PSAC in 1959, had meanwhile established the PSAC Panel on Arms Limitation and Control. At the end of its first year the panel submitted a report arguing that the “creation of an [arms control] office, perhaps by statute, within the Executive Offices of the President... would be the preferred organizational solution.”¹³

Meanwhile, there was talk among certain supporters of the Democratic Party about the need to yoke science and technology not to war, but to peace. Scientists affiliated with the

¹² The Committee on Technical Problems of Arms Limitation to J.R. Killian, Jr., 9 June 1959, Box 11, Folder 98 “Federation of American Scientists, Disarmament Study Committee, 1956-1959 (2 of 3),” *BTF*; also see the undated draft memorandum to J.R. Killian, Jr., in Brennan’s handwriting, in Box 2, Folder 13 “AAAS, Committee on Technical Problems of Arms Limitation 1958-1960, 2/3,” *BTF*.

¹³ J.R. Killian, Jr. to Donald Brennan, 26 June 1959, Box 2, Folder 13 “AAAS, Committee on Technical Problems of Arms Limitation 1958-1960, 2/3,” *BTF*; Zuoyue Wang, *In Sputnik’s Shadow: The President’s Science Advisory Committee and Cold War America* (Piscataway, NJ: Rutgers University Press, 2008), 136. On the proposals by PSAC and its Panel on Arms Limitation and Control, see James R. Killian, Jr., *Sputnik, Scientists, and Eisenhower: A Memoir of the First Special Assistant to the President for Science and Technology* (Cambridge, MA: The MIT Press, 1977), 175–178, quotation on 176. Killian credits PSAC’s lobbying in 1959 as the main force behind the creation of the arms control agency. But PSAC’s proposal—lodging arms control in the White House—was very different from the solution that was eventually reached (the creation of a semi-autonomous agency in the State Department).

Democratic Advisory Council (DAC) had formed, in early 1959, an Advisory Committee on Science and Technology. Among its members were some with longstanding ties to military-funded research. The best example was the engineer Trevor Gardner, who as Assistant Secretary of the Air Force for R&D a few years earlier had done more than anyone else to initiate the Air Force's ballistic missile program.¹⁴ A subcommittee of the DAC science committee began to draw up a proposal that year for a "government laboratory for active study in a scientific manner of methods for maintaining peace," which would be "staffed by top-level scientists," and buoyed by a budget of \$50 million per year. Rather than coordinate peace policy through State or through PSAC in the White House (as Brennan and Feld had suggested), the DAC was recommending a more dramatic step: centralizing a lobby for peace within an independent executive agency.¹⁵

Federal organization for disarmament in the 1950s had been, without question, spotty and half-hearted. By far the most important body was the State Department's Disarmament Administration, an office headed by a special assistant for disarmament supported by a staff of perhaps twenty people. But after Harold Stassen's rude dismissal from this post, the disarmament advisor was kept on a tight leash; there really was no "independent voice" for arms control and disarmament within State. Senator Hubert Humphrey had his disarmament subcommittee of the Senate Foreign Relations Committee, aided by a small staff; but Humphrey's group was essentially a voice in the wilderness—heard by everyone in government, but lacking real power. The Defense Department had its Office of International Security Affairs, but it could hardly be

¹⁴ Other members of this committee included the Manhattan Project veterans Samuel Allison, Charles Lauritsen, and Harold Urey, the physicists Polykarp Kusch and John S. Toll, the biologist Bentley Glass, and Frank Goddard of Caltech's Jet Propulsion Laboratory.

¹⁵ Democratic Advisory Council, "For Release to A.M. Newspapers, Wednesday, April 22, 1959," Box 636, Folder "Arms Control Research Institute, 4/22/59 – 11/12/59," *JFK-PPP*; "Science in the news," *Science* (16 October 1959): 966-967.

said in the late 1950s to harbor any serious interest in disarmament. And that was it. Not to mention the fact that disarmament had numerous powerful opponents throughout the DOD, the Atomic Energy Commission, and even within the State Department itself.

By December of 1959, the DAC science and technology committee made its recommendation for a “National Peace Agency.” This new entity—imagined as an analog of the AEC, and directly counterweighted against it—would have career staffers and its own “Laboratory for Peace,” whose research would be unclassified, “to promote the free flow and exchange of new ideas and concepts in the new technology of peace research and development.” The general ethos was one of openness. “Inspection systems,” the proposal said, “plus hard intelligence information, can in fact provide the substitute for trust so badly needed to catalyze the dynamics of progress in disarmament.” In early 1960, when some of Hubert Humphrey’s staff came across the DAC’s proposal, they recognized it as a good fit for the Senator, who was gearing for a Presidential run later that year. They quickly drew up a bill to create the National Peace Agency for Humphrey’s introduction in the Senate, including each of the functions recommended by the DAC science and technology committee, almost verbatim.¹⁶

But the National Peace Agency model wasn’t, ultimately, the one that would survive. At the Harvard Law School, a professor named Archibald Cox read the draft legislation and wrote hastily to Ted Sorensen, aide to Senator John F. Kennedy of Massachusetts. The proposed bill was “rubbish,” Cox said. He was fully behind the idea that disarmament research needed federal

¹⁶ And that wasn’t all the new agency would do. Nuclear test monitoring, of course, but also missile and satellite test monitoring, aerial reconnaissance, the application of operations analysis to peace (“in the same way that ‘war gaming’ is conducted for the military”), the “effects of radiation upon man,” “educational techniques aimed at rendering underdeveloped nations less technologically dependent,” agriculture, water conservation and desalination, overpopulation—arms control, in other words, plus every modernization theorist’s wish-list for global progress. The Committee on Science and Technology of the Democratic Advisory Council, “A National Peace Agency,” Box 636, Folder “Arms Control Research Institute, 12/20/59 – 2/4/60,” *JFK-PPP*; Bennett introduced his bill in January and Humphrey introduced his in February. See 86th Congress, 2nd Session, H.R. 9305, 6 January 1960, in Box 636, Folder “Arms Control Research Institute, 2/5/60 – 10/7/60”; and 86th Congress, 2nd Session, S. 2989, 4 February 1960, in Box 636, Folder “Arms Control Research Institute, 12/20/59 – 2/4/60,” *JFK-PPP*.

backing. But he found the bill “objectionable” on several counts. Most important, it “interferes with the necessary power and responsibility of the President and the State Department.” The government didn’t need another agency: it needed a dedicated disarmament official (like Stassen had been, but with more clout, at the level of an Under Secretary of State), perhaps appointed “directly under the President.” It needed someone with flexibility and authority to play the inside baseball of national security policymaking. It didn’t need a cumbersome bureaucracy. Second, an independent agency would only invite “duplication and jurisdictional conflicts.” After all, the Air Force had long done work on aerial reconnaissance; the AEC could take care of nuclear test monitoring; and NASA could watch missile and satellite launches. (Cox might have added the CIA, which had an extensive monitoring capability.) Third, “the bill plays into the hands of the Soviet propaganda machine because its title implies that none of the rest of the government is concerned with peace.” Among the Washington politicians and their advisors, “peace” wasn’t a term to be thrown around lightly.¹⁷

Ralph Lapp, a nuclear physicist, Manhattan Project veteran, combatant in the test ban debate, and occasional correspondent with the Cambridge arms control crowd, had been observing the peace agency debates from a safe distance. But in February of 1960 he leapt into the fray, trying his own hand at a new proposal. It was as statist as the DAC’s, but contained a crucial development: an institute devoted completely to “arms control” (then still a relatively new term). In Lapp’s version, the new entity wasn’t a “Peace Agency” modeled on the AEC, but a federated system of “Institutes for Peace and Security,” modeled on the National Institutes of

¹⁷ This last argument, a bit of a stretch, would have invited the comment that the motto of the Strategic Air Command—custodian of the largest reserve of raw destructive power in history—was “Peace is our Profession”; but Sorensen’s reply does not survive. Archibald Cox to Theodore C. Sorensen, 28 January 1960, Box 636, Folder “Arms Control Research Institute, 12/20/59 – 2/4/60,” *JFK-PPP*. Cox would go on to serve as U.S. Solicitor General during the Kennedy administration. Years later in 1973, Richard Nixon would remove him from the job of special prosecutor during the Watergate investigation—the famed “Saturday Night Massacre.” See PBS, *American Experience*, “The ‘Saturday Night Massacre,’” available at <http://www.pbs.org/wgbh/americanexperience/features/bonus-video/presidents-power-nixon2/>.

Health. There was “no secure defense against missile systems,” he argued; the threat of “instabilities in deterrence” that made “arms limitations and controls imperative.” Clearly he’d been leafing through the new literature. Three institutes made up Lapp’s system. First was an “Institute for Arms Controls,” equipped with its own laboratory and allowed to dispense contracts to universities and industrial firms for private research.¹⁸

Senator Kennedy since 1959 had accumulated something of a “Brains Trust,” a stable of academics including several members of the Cambridge arms control community. Deirdre Henderson, a research aide working in the Senator’s Boston office, was their point of contact to Kennedy. (Henderson had earlier worked as an assistant to Henry Kissinger, at that time director of Harvard’s Defense Studies Project.) Jerome Wiesner, for example, had helped Henderson with a position paper on “Arms Control and Disarmament” in September 1959. David Frisch had been indispensable to Henderson and Ted Sorensen as they drafted Kennedy’s statements on nuclear fallout and testing in 1959, including a major speech on testing that Kennedy gave at UCLA in November.¹⁹

In February of 1960, Ralph Lapp’s new proposal had come across the desks of Kennedy’s band of thinkers. They liked it more than the earlier Peace Agency idea, but still

¹⁸ An “Institute for Peace Studies” would bring the “study of economic, social, psychological and political factors related to the origin of war” within its scope. It would, in other words, house all the social scientists and mathematicians. Finally, an “Institute for Human Welfare” would be premised “on the basic assumption that a root cause of war and a constant barrier to peace is the divergence in conditions of human welfare among the peoples of the world.” Here the modernization aficionados would have a home. R.E. Lapp, “Institutes for Peace and Security,” 12 February 1960, Box 636, Folder “Arms Control Research Institute, 2/5/60 – 10/7/60,” *JFK-PPP*.

¹⁹ For his troubles, Kennedy had a new umbrella sent to Frisch as a Christmas present. John F. Kennedy to Jerome Wiesner, 25 September 1959, Box 9, Folder 289 “Kennedy – General, 1960-61 3/3,” *JBW*; Richard N. Goodwin to Archibald Cox, 16 February 1960, Box 9, Folder 289 “Kennedy – General, 1960-61 3/3,” *JBW*; Deirdre Henderson to Jerome B. Wiesner, 3 March 1960, Box 9, Folder 289 “Kennedy – General, 1960-61 3/3,” *JBW*; Deirdre Henderson to David H. Frisch, 2 November 1959; and David Frisch to John F. Kennedy, 12 January 1960, Box 1, Folder 2 “Arms Control – correspondence with John F. Kennedy, 1959-1960,” *DHF*. The text of the UCLA speech is available online at John F. Kennedy Presidential Library & Museum, John F. Kennedy Speeches, “Remarks of Senator John F. Kennedy, Student Convocation, UCLA, November 2, 1959,” http://www.jfklibrary.org/Research/Research-Aids/JFK-Speeches/University-of-California-Los-Angeles_19591102.aspx.

thought it was too bulky. Richard Goodwin, one of Kennedy's legal advisors, began to pare the proposal down. In the end only the Institute for Arms Controls (now renamed the "Arms Control Research Institute") was standing. Its main function would be to supervise the test detection work, but would also incorporate the social scientists that Lapp would have segregated in a separate institute. Kennedy's experts were especially fond of Lapp's idea that the agency should fund outside contracts to universities and companies, "with a view to obtaining the best scientific and intellectual resources available." This was a partial solution to the problem of centralization, and it had been time-tested by the Defense Department and Atomic Energy Commission. In response to Archibald Cox's worry about redundancy, Kennedy's revised bill would explicitly demand cooperation between the Arms Control Research Institute and the DOD, AEC, and NASA. In early March, Wiesner paid a special visit to Kennedy's Boston offices to make last-minute adjustments and to help the staff write a speech, which Kennedy delivered at the University of New Hampshire on March 7th. "Peace, like war," Kennedy's writers had him saying, "has become tremendously complicated and technological." What light would illuminate the rocky path ahead? "Research," of course, "can give us the vitally important knowledge which we must have if we are to lay the groundwork for effective control of today's vast and complex weapons systems." The following day, Kennedy introduced his bill in the Senate.²⁰

It soon became clear that an arms control organization was going to have a rough ride in government, even shorn of all the bells and whistles the DAC had originally proposed for it. J. William Fulbright of the Senate Committee on Foreign Relations asked the Atomic Energy Commission for its impressions of Kennedy's bill. As far as the AEC's General Manager was

²⁰ 86th Congress, 2nd Session, S. 3173, Box 636, Folder "Arms Control Research Institute, 2/5/60 – 10/7/60," *JFK-PPP*; Deirdre Henderson to Jerome B. Wiesner, 3 March 1960, Box 9, Folder 289 "Kennedy – General, 1960-61 3/3," *JBW*. Kennedy's New Hampshire speech quoted in "Brief History of Government Steps to Organize U.S. Disarmament Efforts," Box 12, Folder 1 "Arms Control and Disarmament Agency (ACDA), General Advisory Council (GAC), Correspondence, 1961-63," *IIR*.

concerned, the revisions made by Kennedy's experts had done nothing to alleviate the worry about mission creep. The AEC, he said, was working on "detection and control procedures," and was "providing assistance to other nations in the broad area of peaceful uses of atomic energy"—a venture "designed to improve economic conditions and alleviate sources of international tension." Wasn't that what arms control was supposed to accomplish? Apparently the AEC was already doing all of the peace research the government needed.²¹

As Trevor Gardner remarked in the *Bulletin of the Atomic Scientists* in the summer of 1960, the bills to establish Humphrey's National Peace Agency and Kennedy's Arms Control Research Institute had one collective purpose: to commit the Democratic Party to improve government organization for arms control, should the party win in November.²² And they did serve that function. After Kennedy ousted Humphrey as the Democratic candidate, and then won the Presidential election, among the several policy task forces he appointed was one on arms control. The first order of business for this group was the creation of the new agency. The trick was to give it enough power to assert itself without stepping on the toes of powerful, established interests. Kennedy's arms control task force was filled with the movers and shakers of the new field. It was chaired by Jerome Spingarn of the National Planning Association (which had a long association with Feld's Cambridge group, and had just published a report on "Strengthening the Government for Arms Control"). Joining Spingarn were Trevor Gardner, the president of IDA Garrison Norton, the IDA nuclear strategist James King, Jeffrey Kitchen of RAND, Betty Goetz (the staff director of Humphrey's Disarmament Subcommittee and the lone woman in the world of expert disarmament and arms control study in the 1950s and early 1960s), the Columbia University government professor (and expert on Presidential authority) Richard Neustadt,

²¹ A.R. Luedecke to J.W. Fulbright, 29 September 1960, Box 636, Folder "Arms Control Research Institute, 2/5/60 – 10/7/60," *JFK-PPP*.

²² Trevor Gardner, "Organizing for peace," *Bulletin of the Atomic Scientists* 16, no. 7 (1960): 297-300.

Thomas Schelling, Paul Nitze, Jerome Wiesner, and a few others. There would be no fluffy peace-talk in this group.²³

In an urgent-sounding report, the task force recommended that immediately after taking office, Kennedy should assign the State Department Disarmament Administration as his chief “instrument in the formulation of arms control policy,” appoint a new director, and task the director with coming up with the administration’s arms control policy within a few months. In the meantime, Kennedy should submit new legislation to create the arms control agency. It had to be established at the statutory level, *pace* Archibald Cox, for several reasons. Most important, it was the only way to give arms control any independent status in the government, to guarantee that it would get its own Congressional appropriations, and “to give it flexibility in recruitment and personnel policy.” The new agency needed experts.²⁴

On Kennedy’s fifth day as President, he held his first meeting on arms control in the Oval Office. Disagreement reigned on where and how to position a new arms control agency. Once in office Kennedy suddenly seemed to prefer to keep the arms control staff within the White House, defying the plan his advisors had been talking about for months.²⁵ Richard Neustadt soon hit

²³ “Report to the Honorable John F. Kennedy by the Task Force on Disarmament,” 31 December 1960, Box 1073, Folder “Disarmament,” *JFK-PPP*. Also see NPA Special Project Committee on Security Through Arms Control, *Strengthening the Government for Arms Control* (Washington, DC: The National Planning Association, 1960).

²⁴ The fingerprints of the committee’s various members were in evidence. They recommended that Kennedy should “project the image of a man who...reveals a deep sense of dedication” to peace and disarmament (clearly the influence of Neustadt), start publicly admitting that arms control might actually cost more, not less, than an arms buildup (one of Schelling’s pet ideas), and include nuclear test cessation within a more comprehensive framework for arms control policy (vintage Wiesner). “Report to the Honorable John F. Kennedy by the Task Force on Disarmament,” 31 December 1960, Box 1073, Folder “Disarmament,” *JFK-PPP*. On Wiesner’s ideas concerning comprehensive arms control, see Chapter 1. On Neustadt’s ideas about projecting images of Presidential leadership, see Richard Neustadt, *Presidential Power and the Modern Presidents: The Politics of Leadership* (New York: John Wiley & Sons, Inc., 1960).

²⁵ The acting deputy director of the recently created Disarmament Administration in the State Department, Edward Gullion, argued on the other hand that any new arms control agency should report directly to the Secretary of State. A career foreign service officer, Gullion believed that research and negotiation should be done in the same office—and that office should reside in State, whose stock in trade was negotiation. Gullion would soon leave government and become a regular presence in the Harvard-MIT arms control seminar, as director of the Fletcher

upon a compromise. His idea was to create a new agency as planned but make it “semi-autonomous,” establishing it by statute but placing it (bureaucratically and physically) inside the State Department. The director would then answer simultaneously to the President and the Secretary of State. The Republican Wall Street banker John J. McCloy, who Kennedy had picked to serve as his interim disarmament advisor, began working with a small staff to draw up the legislation.²⁶ Joining him was his trusty longtime assistant Shepard Stone, director of the Ford Foundation’s International Affairs program. To work on McCloy’s staff, Stone brought in the experienced disarmament analyst Betty Goetz, McCloy’s associate from the Department of War Adrian Fisher, and the former AEC legal counselor George Bunn. McCloy transmitted the draft to Kennedy in May, and bills were introduced in Congress in June.

The ensuing Congressional debate indicated the huge span of views on what arms control was and what it was for. Former Defense Secretary Robert Lovett worried that it would become “a Mecca for a wide variety of screwballs.” Of course a committed arms controller like Jerome Wiesner wanted the agency; but even Herman Kahn made a special trip to Washington at the behest of Daniel Singer, general counsel of the Federation of American Scientists, to help convince skeptical conservative senators to pass the bill. At the last minute, Senator Barry Goldwater tried to torpedo it by proposing an amendment to prevent the agency from carrying

School of Law and Diplomacy at Tufts University. Edmund A. Gullion to John F. Kennedy, 15 December 1960, Box 69A, Folder “Arms Control and Disarmament Agency,” *JFK-POF*.

²⁶ Among the first big questions Kennedy faced on the issue of disarmament was who to ask to serve as director of the Disarmament Administration. Immediately after the election, Jerome Wiesner wrote to Ted Sorensen to suggest names for various cabinet posts. At the top of his list of suggestions for Secretary of Defense was just the sort of hard-nosed liberal Republican Kennedy was seeking: John J. McCloy. McCloy was a Wall Street titan and political insider—at different times president of Chase Manhattan Bank, president of the World Bank, Assistant Secretary of War under Henry Stimson, U.S. High Commissioner in Germany after the war, chairman of the Council on Foreign Relations, and chairman of the board at the Ford Foundation. Kennedy courted McCloy personally, initially offering the Defense Secretary job that Wiesner had thought McCloy was the right man for. McCloy demurred; he wanted to work part-time and remain based in New York. The disarmament post, it was decided, would be a better fit. Jerome Wiesner to Theodore Sorensen, 22 November 1960, Box 9, Folder 287 “Kennedy – General, 1960-61 1/3,” *JBW*; Kai Bird, *The Chairman: John J. McCloy, The Making of the American Establishment* (New York: Simon & Schuster, 1992), esp. 495-521.

out or contracting for any research. The amendment was rejected, narrowly, and the bills passed in the House and Senate in September.²⁷

On September 26th, 1961, at the Carlyle Hotel in New York City, the President signed the Arms Control and Disarmament Act into law, creating the Arms Control and Disarmament Agency (ACDA). The name, like the legislation, had been a compromise. As Duncan Clarke writes of ACDA's legislative history, "disarmament was a long-term normative objective, arms control the present reality." At the signing Kennedy announced that he would appoint William C. Foster as the first ACDA director. Foster had led the U.S. delegation to the Geneva surprise attack conference in 1958, had just directed a study of possible limitations on warheads and delivery vehicles in the early months of the administration, and was a safe choice, politically—a liberal Republican formed in the same mold as John J. McCloy. The three original drafters of the legislation—Bunn, Goetz, and Fisher—all joined the agency (Bunn as general counsel, Goetz on the staff, and Fisher as deputy director).²⁸

²⁷ Bird, *The Chairman*, 499-500; Duncan L. Clarke, *Politics of Arms Control: The Role and Effectiveness of the U.S. Arms Control and Disarmament Agency* (New York: The Free Press, 1979), 19-23. Lovett quoted in Clarke, *Politics of Arms Control*, 21. On Kahn's advocacy, see Freeman Dyson, *Disturbing the Universe* (New York: Basic Books, 1979), 131-132. On Bunn, see Matt Schudel, "George Bunn, who helped negotiate the Nuclear Non-Proliferation Treaty, dies at 87," *Washington Post* (25 April 2013).

²⁸ "Remarks of the President on Signing H.R. 9118, An Act to Establish the United States Arms Control and Disarmament Agency (At the Carlyle Hotel in New York City)," 26 September 1961, Box 35A, Folder "Remarks in NY on ACDA," *JFK-POF*. And as for McCloy, who declined the full-time job of ACDA's director, he was—appropriately for someone whose nickname was "The Chairman of the Establishment"—appointed chairman of an ACDA General Advisory Committee. Modeled on the widely known and respected General Advisory Committee of the AEC, the ACDA GAC would assemble up to fifteen prominent overseers advising the director, the Secretary of State, and the President on arms control policy. Joining McCloy were familiar names, including Trevor Gardner, Robert Lovett, George Kistiakowsky, Herbert York, and I.I. Rabi. Rabi had served ten years on the AEC's General Advisory Committee, an experience highlighted by the 1950 decision by the United States to pursue a crash program on the hydrogen bomb. A famous GAC report from late 1949 denounced the so-called "Super" as "a weapon of genocide"; Rabi and Enrico Fermi appended a minority report calling thermonuclear weapons "an evil thing considered in any light." On the hydrogen bomb decision and the role of the AEC General Advisory Committee, see Peter Galison and Barton Bernstein, "In any light: Scientists and the decision to build the Superbomb, 1952-1954," *Historical Studies in the Physical and Biological Sciences* 19, no. 2 (1989): 267-347. Rabi was enthusiastically for the idea of an expert arms control agency, but skeptical that it was being run properly. In 1962 he wrote to John J. McCloy, "My own experience...is that a general advisory committee is at its best dealing with broad and fundamental issues of policy and strategy," and not the highly technical nitty-gritty of arms control negotiations, which McCloy had initially proposed for group discussion. "It does much better with a telescope than a

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Initially established in ground-floor offices in the State Department Building, ACDA was endowed with an internal research capability comprised of four bureaus and three supporting staffs. The four bureaus were assigned responsibility for Weapons Evaluation and Control, International Relations, Economics, and Science and Technology. The science and technology bureau, in particular, would become a way station for some of the most prominent arms controllers of the 1960s and 1970s. It began life under the leadership of Franklin Long, a chemist who had temporarily departed a faculty position at Cornell. Because Goldwater had not gotten his way, the first of ACDA's functions was the "conduct, support, and coordination of research for arms control and disarmament policy formulation," and the director was required to "exercise his powers in such a manner as to [accumulate] a fund of theoretical and practical knowledge concerning disarmament." The State Department's Edmund Gullion wrote to Kennedy that "there has been a striking increase in the research and study going on in private institutions, laboratories and foundations. The results of all this cerebration needs [sic] to be tapped for Government systematically." In years to come, defenders of the agency would routinely cite the creation of arms control knowledge and the support of research as central to ACDA's operation.²⁹

microscope," Rabi added. When he resigned in 1969, he told the new ACDA director Gerard C. Smith that "the committee accomplished practically nothing in about seven years of operation, although we had some good meetings." See "General Advisory Committee, Biographic Sketches," Box 260, Folder "ACDA, Disarmament Subjects, General Advisory Committee 3/62," *JFK-NSF*; Edmund A. Gullion to John F. Kennedy, 15 December 1960, Box 69A, Folder "Arms Control and Disarmament Agency," *JFK-POF*; I.I. Rabi to John J. McCloy, 11 June 1962, Box 12, Folder 1 "Arms Control and Disarmament Agency (ACDA), General Advisory Council (GAC), Correspondence, 1961-63"; and I.I. Rabi to Gerard Smith, 12 May 1969, Box 12, Folder 2 "ACDA, General Advisory Council, Correspondence, 1964-69," *IIR*.

²⁹ Public Law 87-297, 87th Congress, H.R. 9118, "An Act to establish a United States Arms Control and Disarmament Agency," 26 September 1961. Franklin Long was named Henry Luce Professor of Science and Society at Cornell in 1969, upon retiring from government, and he was the first director of the university's program on science, technology and society. See Wolfgang Saxon, "Franklin Long is dead at 88," *New York Times* (11 February 1999).

Research was hard-wired into ACDA from the start. Of its typical annual budget in the early 1960s, in the neighborhood of \$10 million, ACDA spent around \$6 million (that is, more than half of the account) funding external research by individuals, academic institutions and industrial firms. This fraction crept upward in the 1960s. In 1964, when the agency requested \$15 million from Congress, it had proposed using \$11 million of it for external research. Its founding legislation was explicit in giving the agency director power to “procure the services of experts and consultants,” and even to pay them at rates above the standard “government schedule” compensation. Franklin Long served as chair of an ACDA Research Council that guided the spending of the research budget.³⁰

In its first two years ACDA shelled out contracts to such mainstays of the defense-industrial-intellectual world as Raytheon (for a study on “progressive zonal inspection to verify arms control and disarmament agreements”), Sylvania (to study a ban on “the placing of weapons of mass destruction in orbit and restricting or halting the flight testing of missiles”), North American Aviation, the Aerospace Corporation, Herman Kahn’s Hudson Institute, and a host of smaller outfits. Mathematica, the consulting firm cofounded by the Princeton economist Oskar Morgenstern in 1962, won an early ACDA contract to apply statistical methods to the verification of arms control agreements, and Morgenstern would continue an occasional consulting relationship with ACDA for twenty years.³¹ Contract ACDA-1, the agency’s first, went to the Michigan-based Bendix Corporation, a manufacturer of automotive brake systems that had expanded into missile and satellite systems in the 1950s. ACDA asked the Bendix

³⁰ Eric Stevenson and John Teeple, “Research in Arms Control and Disarmament, 1960 – 1963,” 30 September 1963, Ford Foundation Report 001254, *FFR*; Arms Control and Disarmament Act, Public Law 87-297 (1961), Sec. 41; Franklin A. Long to Distribution, 1 August 1963, Box 12, Folder 1 “Arms Control and Disarmament Agency (ACDA), General Advisory Council (GAC), Correspondence, 1961-63,” *IIR*.

³¹ U.S. Arms Control and Disarmament Agency, “First Report on U.S. Government Research and Studies in the Field of Arms Control and Disarmament Matters,” 15 March 1963, Box 12, Folder 3 “ACDA-GAC, Reports, 1961,” *IIR*.

Systems Division to do a “study of techniques for monitoring the production of strategic delivery vehicles,” ending up in a huge six-volume classified report, the second volume of which included a complete taxonomy of “strategic delivery vehicle” systems and their manufacturers in both the Soviet Union and the United States.³²

The trunkline of elite arms control expertise, however, still ran between Boston and Washington. In the days before the Kennedy administration proposed its ACDA bill, there had even been talk of basing an arms control institute in the Cambridge area. In the spring of 1961, after a year of Congressional inactivity on the Humphrey peace agency and Kennedy arms control institute bills, Richard Leghorn told the Harvard-MIT arms control seminar about his plan to take matters into his own hands. Leghorn was a habitu  of the early Cambridge disarmament scene. His ITEK Corporation was making reconnaissance equipment for CIA spy satellites, and he had apparently dreamt up a similar institutional scheme for arms control: a private clearinghouse for arms control expertise, contracted to the government. Leghorn said he had already raised \$2.5 million for his “Arms Control Center.” Its staff would number perhaps a hundred people; a site in Boston had already been located to house it. The organization would feature a “document center for military and disarmament research,” and its ultimate purpose would be the design of a “rational world security system”—Leghorn’s code-phrase since the late 1950s for a scheme of mutual surveillance to monitor arms control agreements. After the administration tabled its proposal for an arms control agency later in the year, however, nothing more was heard of Leghorn’s plan.³³

³² The second volume was declassified at my request by the National Archives and Records Administration, through the JFK Presidential Library & Museum. See Box 260, Folder “ACDA, Disarmament Subjects, Bendix Corporation Report Vol. I, 1/63,” Folder “ACDA, Disarmament Subjects, Bendix Corporation Report Vol. II, Sections 1 and 2, 1/63,” and Folder “ACDA, Disarmament Subjects, Bendix Report Vol. II, Section 3, 1/63,” *JFK-NSF*.

³³ “Arms Control Seminar, Abstract of Discussion, Twelfth meeting – April 10, 1961 – M.I.T. Faculty Club,” Box 2, Folder 48 “Joint Harvard-MIT Arms Control Seminar - Minutes, 1960-61,” *LPB*.

Discussion of ACDA in Cambridge was not free of discord. Robert Matteson, senior advisor to the director of ACDA, visited the arms control seminar in early 1963 to report on the agency's first full year of activity. ACDA had gotten off to a slow start, he told the group, but was beginning to gather steam. Most interesting was Matteson's assertion that, quite contrary to ACDA's statute and its physical location, its "main links are with the Pentagon, not the State Department or CIA." Matteson must have realized that his audience included some formidable skeptics. For several years Thomas Schelling had argued that arms control should be consistent with military policy, not opposed to it. But listening to Matteson's pitch for ACDA-Pentagon conciliation, he doubted very much that ACDA was living up to this ideal. The agency's chief sponsors were "the disarmers," he said after Matteson had concluded his opening remarks. There was little use in trying to "paper over disagreement" between the disarmers and the DOD. The trouble with ACDA was that no one quite knew what it was for. "We have no criteria to know if it is doing well until we know what its function is.... It is inhibited by the 'religious' character of the subject: the subject is difficult to discuss intelligently in public..." For a good example, witness "General and Complete Disarmament," the official policy of the Soviet Union, then adopted by the United States, and taken seriously by no one. "The original high expectations are doomed to failure, but if disarmament is essentially a question of verbiage," Schelling concluded, "the Agency may be doing quite well."³⁴

With this outburst complete, MIT political scientist Lincoln Bloomfield returned the discussion to what he regarded as ACDA's chief aim: stimulating research. Morton Halperin had

³⁴ "Joint Arms Control Seminar, Minutes of the Eighth Session, February 25, 1963," in Box 3, Folder 3 "Joint Harvard-MIT Arms Control Seminar 1962-63 Minutes," *LPB*. Matteson had worked on the State Department disarmament staff under Stassen and had actually suggested to Eisenhower, in a 1959 letter, that the disarmament issue be given its own dedicated agency. On Matteson, see David Chanen, "Career government official Robert Matteson dies," *Star Tribune* (25 January 1994). For a summary U.S. statement on "general and complete disarmament" in 1962, see U.S. Arms Control and Disarmament Agency, *Blueprint for the Peace Race: Outline of Basic Provisions of a Treaty on General and Complete Disarmament in a Peaceful World* (Washington, DC: Government Printing Office, 1962).

to agree. While State was preoccupied with the day-to-day negotiations, ACDA could function as “an idea-producing group.” “Research is the vital function,” agreed Harvard Law professor Louis Sohn. William Kaufmann, unconvinced that ACDA was producing very many new ideas, countered that there was “much arms control in the McNamara statement”—the so-called posture statement, which McNamara issued yearly to outline basic U.S. defense policy. (But, then, Kaufmann himself had written a good deal of the posture statement.) Bernard Feld was willing to admit that there was “arms restraint in McNamara,” but McNamara could only “take unilateral arms control decisions, [and] could not say what might be done through negotiations.” Schelling (frowning and arms crossed, one imagines) was unmoved. ACDA wanted disarmament plain and simple, disarmament of the “religious” kind, and it was time to admit that no one had the first idea how to achieve it responsibly. “It is not a question of not doing homework,” he said, “but is more [like] the control of cancer, not knowing how to go about it. The Agency has made no contribution on how to go about achieving disarmament; this is as likely to come from the Pentagon. The Agency does not have a special area of policy, but a competing philosophy of national security.”³⁵

Schelling’s skepticism had certainly been amplified by his own recent evaluation of what total nuclear disarmament might actually mean. In 1960, to equip the Institute for Defense Analyses for the “almost limitless” array of nuclear issues, the IDA board of trustees installed a new “Special Studies Group.” The Special Studies Group would collect IDA’s growing expertise in the new social-scientific approaches to nuclear strategy. Its director was a strategy expert James E. King, Jr.; George Rathjens, who seemed to be everywhere, was also named to the staff.

³⁵ “Joint Arms Control Seminar, Minutes of the Eighth Session, February 25, 1963,” in Box 3, Folder 3 “Joint Harvard-MIT Arms Control Seminar 1962-63 Minutes,” *LPB*.

One of the Special Studies Group's first efforts in 1961 was Project Vulcan, a study of "arms control as a stabilizing force in the politico-military climate of the world."³⁶

The irony of Schelling's opinion that ACDA promoted an intellectually flimsy brand of disarmament advocacy was that his recent study on the topic had been paid for by ACDA. Initially backed by the Disarmament Administration in early 1961, Project Vulcan was subsumed by ACDA when the new agency was born. Several of the arms control seminar members were conscripted for the study, including Lincoln Bloomfield, who wrote a paper on the role of the United Nations in administering arms control agreements, and Morton Halperin, who produced the first sustained analysis of a "no-first-use" doctrine for U.S. nuclear weapons. Schelling had been contracted to write a set of musings on "The Stability of Total Disarmament." If general and complete disarmament were achieved tomorrow, would there be less risk of war? Not likely, though Schelling. "There are no strong a priori reasons for supposing that drastic disarmament reduces the advantages that accrue to haste and initiative in war and disarmament—hence no strong reason for supposing that it necessarily reduces the dangers of surprise attack, pre-emptive war, accidental war, escalation of limited war"—all the standards in Schelling's repertoire. In *Foreign Affairs* a few months later, where Schelling published a condensation of his report (without mentioning ACDA's sponsorship), he told readers that nuclear arsenals might well have bestowed stability on the international environment—that to do away with them was, perhaps, to throw the baby out with the bathwater. "If disarmament is to discourage the initiation of war and

³⁶ Institute for Defense Analyses, *Annual Report Number Five* (Washington, DC: Institute for Defense Analyses, 1961). By 1962 the Special Studies Group had achieved divisional status within IDA; by 1964, now under the heading of the Economic & Political Studies Division, it boasted almost 70 staff members. See Institute for Defense Analyses, *The Tenth Year, March 1965 through February 1966* (Washington, DC: Institute for Defense Analyses, 1966); Institute for Defense Analyses, *Activities of the Institute for Defense Analyses 1965* (Washington, DC: Institute for Defense Analyses, 1965).

to remove the incentives toward preemptive and preventive war, it has to be *designed* to do that. Disarmament does not eliminate military potential; it changes it.”³⁷

Schelling might have doubted the utility of ACDA and the sincerity of its sympathy for military policy goals. But the fact was that in the agency’s first year, he had taken part in a major arms control study done at its bidding. He had even gotten a widely noted article out of the deal. Such a turn of events would have been very unlikely even a year or two earlier. Compared with the scattered and scantily funded disarmament research of the 1950s, by the middle 1960s one survey commissioned by the Ford Foundation counted literally hundreds of research projects in the general area of arms control and disarmament, federally sponsored to one degree or another. And ACDA, despite its embattled position in the government, had commissioned and picked up the tab for much of it.³⁸

II. Golden State Seminars: Private Foundations, Local Arms Control Expertise, and the Undermining of ACDA

California Dreamin’: The California Institute of Technology Arms Control Seminar

The years 1959–61 were a period of government institution building for arms control. For the first time, the United States had a centralized apparatus for arms control research. It was accomplished, of course, in the American style, by distributing contracts to private researchers and organizations. Arms control expertise remained moored to local settings. And though Cambridge was the hub of arms control activity in the United States, it could take root elsewhere—even in sun-bleached Southern California, where perspectives on the meaning of the

³⁷ T.C. Schelling, “The Stability of Total Disarmament,” IDA Study Memorandum No. 1 (6 October 1961), quotation on v; M.H. Halperin, “A Proposal for a Ban on the Use of Nuclear Weapons,” IDA Study Memorandum No. 4 (6 October 1961); Thomas C. Schelling, “The role of deterrence in total disarmament,” *Foreign Affairs* 40, no. 3 (1962): 392-406, quotation on 396.

³⁸ Eric Stevenson and John Teeple, “Research in Arms Control and Disarmament, 1960 – 1963,” 30 September 1963, Ford Foundation Report 001254, *FFR*.

nuclear age could be quite different than those trafficking the Cambridge-Washington corridor. On either coast, it was in the university seminar room—not a windowless government laboratory—where most arms control experts encountered and debated the new field.

At the California Institute of Technology, a forum for arms control discussion and study was created in 1960. It had come along somewhat by accident. For forty years the Carnegie Corporation had funded humanities education and liberal arts programs at Caltech. In late 1959 Carnegie awarded the Caltech Division of Humanities a \$330,000 grant to develop an educational program in the general area of “science and public affairs,” the details of which were to be worked out by the faculty. The historian David Elliot was asked to take charge of this new project. Scottish, Harvard-trained, hired at Caltech after serving in the British civil service in India during World War II, Elliot was saddled with the job because he’d been a longtime teacher of Caltech’s “History 5” course in contemporary affairs (despite his own research expertise in 18th-century British politics). He began by making the rounds of the faculty, asking various people what they thought should be done with the grant.³⁹

Elliot and his interlocutors quickly arrived at the idea of a faculty/student seminar, perhaps supplemented by a study project on an urgent issue in the general area of science and society. The Carnegie grant steering committee decided that “science and public affairs” was “too vague” a heading to work with. It wasn’t long before the idea of focusing the grant on nuclear issues, especially disarmament, came up. (Charles Lauritsen and Harrison Brown, after all, both steering committee members, had just finished coauthoring the Democratic Advisory Council’s report on organizing the government for peace. And Robert Bacher, another steering

³⁹ Interview of David C. Elliot by Carol Bugé, Oral History Project, California Institute of Technology Archives, Pasadena, California, 1986, available online at http://oralhistories.library.caltech.edu/144/1/David_Elliot_OHO.pdf (hereafter cited as Elliot interview, 1986); D.C. Elliot, “Science and Public Affairs,” 14 January 1960, Box 2, Folder 2.2 “Steering and Sub-Committee Reports, 1960-63,” *RCP*.

committee member, had spent much of the previous autumn on PSAC's new Panel on Arms Limitation and Control, acting as a liaison between the RAND nuclear strategists and PSAC.) At a meeting in January of 1960, it was suggested that "the implications of disarmament are not clearly understood. They have not been fully explored in an academic atmosphere." And so the group decided to use the disarmament issue as a substantive window onto the broader questions of the "relation of scientists and government, science and the lay public...etc." It was settled: "We take as our area of study," the committee wrote, "the developing and rapidly changing position of the U.S. in the world—with special reference to the limitation and control of armaments."⁴⁰

When several faculty members eagerly endorsed the Carnegie project, they had an earlier and somewhat notorious study in the back of their minds: Project Vista. Orchestrated by Lee DuBridgely in 1951–52, Project Vista had been carried out at the behest of the Air Force and the Army. A study of tactical atomic warfare, it explored the use of small-yield atomic weapons, mounted mainly on rockets and tactical aircraft, against troops in battlefield situations. Vista had controversially proposed the creation of a Tactical Atomic Air Force—a clear rival to the Strategic Air Command, which had always had the dominant hand in nuclear war planning, and control over most of the deployed U.S. nuclear weapons. SAC's primary mission was World War II-style strategic bombing of "urban-industrial" targets. When SAC officials got hold of the Vista report, they buried it. Ignored by the bomber-generals, the study nevertheless left a large

⁴⁰ D.C. Elliot, "Science and Public Affairs," 14 January 1960, Box 2, Folder 2.2, "Steering and Sub-Committee Reports, 1960-63," *RCP*; "Carnegie Project, Science and Public Affairs, Sub-Committee Meeting, Monday, January 25, 1960," Box 2, Folder 2.2 "Steering and Sub-Committee Reports, 1960-63," *RCP*; "Science and Public Affairs, February 16, 1960," Box 2, Folder 2.2 "Steering and Sub-Committee Reports, 1960-63," *RCP*.

footprint on the life of Caltech (more than a quarter of the entire faculty had participated in one way or another), and in later years was remembered with more than a little apprehension.⁴¹

To some degree, the Carnegie project offered the hope of scrubbing some of Vista's black marks from the Institute's history. Charles Lauritsen, a major player in the Vista study, even suggested that the Carnegie grant could be used to study the Vista experience itself—gleaning, he hoped, the solemn lessons of the Institute's compromise with the military. (Others, however, quickly pointed out that none of the crucial documents had been declassified.) The physicist William Fowler, who had been Vista's scientific director, was gung-ho for a disarmament study as a kind of anti-Vista. He suggested that Elliot set up faculty study groups assigned to specific problems, on the model of military summer studies, with the ultimate goal of assembling a book-length report. "The project would operate something like VISTA," Fowler wrote:

Open seminars would be held as well as closed study sessions. Congressmen, candidates, public figures, pundits, scientists, of known drawing power would be invited to speak in public and in private chambers. Secretaries would keep copious notes.... The final book would create itself if the faculty survived the political controversy. If these best creatures of our culture did not, then where is the hope for the world?

He seemed ready to leap out of his chair. Arms limitation called for "a real sense of immediacy. How do we get going and especially how soon?"⁴²

⁴¹ David Elliot himself would, more than twenty-five years later, write the first history of Project Vista and its implications for military policy and strategy. See David C. Elliot, "Project Vista and nuclear weapons in Europe," *International Security* 11, no. 1 (1986): 163-183. On Vista's place within the history of Caltech and its peculiar institutional structure and academic culture, see W. Patrick McCray, "Project Vista, Caltech, and the dilemmas of Lee DuBridge," *Historical Studies in the Physical Sciences* 34, no. 2 (2004): 339-370.

⁴² Fowler added: "I am more serious about this than the above effusion might indicate." William A. Fowler to D.C. Elliot, 17 February 1960, Box 2, Folder 2.2 "Steering and Sub-Committee Reports, 1960-63," *RCP*.

It was agreed among the faculty that the Carnegie project would not be conducted like a crash program, nor would it seek direct sponsorship by the government. There were other templates available for creating an informal expert community in arms control. Elliot and the steering committee were looking eastward for their model. Harvard's Paul Doty visited Caltech in early April and spoke with Elliot about some of the activities of the disarmament and arms control community in Cambridge. It was agreed that Elliot and the physicist Matthew Sands would make a fact-finding trip to Boston and New York City the following month to speak with some of the experienced people there. In New York they met with Christopher Wright and William T.R. Fox at Columbia; in Cambridge in early May, they met with Henry Kissinger and Gerald Holton at Harvard, and even managed to coax a drink out of Jerome Wiesner at the MIT faculty club. Their timing was beautiful: the *Daedalus* conference was held at MIT's Endicott House during their three-day visit to Boston. As the leading lights of the new field argued and debated, Elliot and Sands stood by and took notes. They were ringside at a founding event for the new field.⁴³

Elliot and Sands returned from Boston and debriefed the steering committee. In their discussions and memoranda, the term "disarmament" was steadily eliminated in favor of "arms control." As Elliot put it years later, "we flowed, as it were, with the tide and got into the arms control issue, which was just arising in 1960. Hitherto, people had talked a lot about disarmament, but not very much about arms control. And this idea of arms control was beginning to surface as people began to realize that...these weapons of one sort or another were here to stay, and the problem was to try and get them under control, and not to wish them out of

⁴³ David C. Elliot to Don K. Price, 15 April 1960; Gerald Holton to David C. Elliot, 19 April 1960, Box 2, Folder 2.15 "Misc Correspondence and documents, 1960-66," RCP; Elliot interview, 1986.

existence...”⁴⁴ Caltech’s hometown of Pasadena sat just a few miles east of the downtown LA core, amid a sprawling landscape stippled with defense and aerospace firms. The RAND Corporation’s headquarters lay just a few miles on the other side of downtown, near the seaside in Santa Monica. The Institute was, in other words, within a freeway commute of experts and institutions that were decidedly *not* in the business of wishing nuclear weapons away.

Contact between RAND and Elliot’s new Caltech discussion group was made right off the bat. Even before his jaunt to the East Coast, Elliot had been in touch with Herman Kahn to tell him that there was “much interest here in your discourse on Thermonuclear War, and I have asked to enquire whether...you might be able to come over and deliver your full lecture series...” Kahn obliged—he had done his Ph.D. in physics at the Institute—and Caltech got the full Kahn treatment the next month. It was the first time Kahn had talked at length about the book anywhere (including, even, at RAND). Less like a lecture and more like a live one-man theater performance of *On Thermonuclear War*, Kahn commanded the stage between 8:30am and 5:30pm for two days, providing a total of fifteen hours of entertainment, not including lunch and coffee breaks. These were sold-out ticket-only events; Kahn’s RAND “agent” had asked Caltech to reserve 50 of the 300 available tickets for Kahn himself, in order to get “as many Rand men there as possible.”⁴⁵

Caltech’s tweedy historians began to find themselves sympathetic to the RAND viewpoint. In 1961 the mathematician James R. Newman began his breathless, spangled denunciation of *On Thermonuclear War* in the magazine *Scientific American* by asking, “Is there really a Herman Kahn?” Newman mused that perhaps Kahn was a collective pseudonym, “the

⁴⁴ Elliot interview, 1986.

⁴⁵ “Carnegie Sub-Committee: Science and Public Affairs, Minutes of meeting of Friday, May 13, 1960,” Box 2, Folder 2.2 “Steering and Sub-Committee Reports, 1960-63,” RCP. Also see Herman Kahn, “A Summary and Outline of Three Lectures on Thermonuclear War”; David C. Elliot to Herman Kahn, 26 April 1960; and “Memo to David from Virginia,” Box 2, Folder 2.9 “Correspondence J-K, 1960-66,” RCP.

Rand Corporation's General Bourbaki," the book "a staff hoax in bad taste." How else to explain this "moral tract on mass murder"? Less well remembered is that Newman devoted equal time to the *Daedalus* 1960 special issue on arms control in the same essay. He found it only marginally less scandalous, "a medley of pieces scored more or less in the Kahn key."⁴⁶ So it was not just to Kahn's defense but to rescue the new project of strategic arms control that Caltech's David Elliot came when he wrote a reply to Newman—never published by the magazine—titled "Mr. Kahn does exist." "War, like sewage, is a distasteful subject," Elliot wrote, "but it has been with us a very long time.... We may well resent being plotted on a curve or punched on an IBM card. To be treated as cyphers is bleak and humiliating for our humanity," he noted, speaking of Kahn's penchant for parsing "tragic but distinguishable postwar states" of 2 million, 5 million, 40 million dead after a nuclear conflict. Elliot had no use for Newman's hysteria, however, since "in determining our attitude to war...the numbers seem to have been of some importance.... Mr. Kahn's figures are shocking, but that is not Mr. Kahn's fault."⁴⁷

When the Carnegie program began in earnest in the summer of 1960, it kicked off with a trial run of seminars on specific topics, almost every one of which was delivered by a RAND staff member. Myron Rush, a RAND Sovietologist, lectured on Khrushchev's strategic views. Andrew Marshall and James Digby—both RAND pioneers of the counterforce strategy during the 1950s—talked about "modes of war initiation" and "active defense and deterrent." The Caltech group heard their first presentation on inspection systems from Amrom Katz; on the history of the Korean War, from Roberta Wohlstetter; and on the nuclear test ban, from her husband, Albert. Henry Rowen told the seminar to bear in mind that arms *control* was not the

⁴⁶ James R. Newman, "Two discussions of thermonuclear war," *Scientific American* 204, no. 3 (1961): 197-204, on 197 and 200.

⁴⁷ David C. Elliot, "Mr. Kahn does exist," attached to David C. Elliot to Editor, *Scientific American*, 10 April 1961, Box 2, Folder 2.9 "Correspondence J-K, 1960-66," *RCP*.

same thing as arms *reduction*. And the brash, youthful Daniel Ellsberg, fresh from a stint in the Harvard Society of Fellows and now one of RAND's hot-shot analysts, gave a talk on "threats, negotiation and war," a capsule version of his theory of nuclear blackmail and coercion. For Ellsberg nuclear weapons were a deterrent to the extent that the threat to use them was credible. "Nothing improves the credibility of a threat more than the effect that it has been carried out before," he observed. To this room of Caltech faculty, including several Manhattan Project veterans, Ellsberg proposed that the bombing of Hiroshima and Nagasaki, whatever else they were, now functioned as a salutary reminder to other nations of America's proven capacity for nuclear violence—a boon to nuclear deterrence. The U.S. shouldn't shy from rattling the nuclear saber when the moment called for it, he said. "Perhaps a little controlled madness would be a useful thing."⁴⁸

In the autumn the project began to take in more heterogeneous input. It was soon agreed that there would be a tripartite division of activity: occasional "lectures on national policy" (though not as exhaustive, or exhausting, as Kahn's); a faculty seminar in the same mold as the Harvard-MIT arms control seminar, but offered for academic credit to interested graduate students; and a faculty arms control working group, which would explore issues raised in the seminar. Henry Kissinger, for example, delivered a lecture on "the political background of arms control," and a seminar on the question "do we want disarmament, and why?" In Kissinger the Caltech crowd witnessed a very different style from the RAND regulars, even if the message was much the same. For Kissinger, arms control was helpful to the degree that it was the outworking of some coherent political aspiration. All of his conclusions meshed comfortably with the views

⁴⁸ "Carnegie Program: Introductory Seminar on Arms Control," Box 1, Folder 1.1 "Preliminary Seminar on Arms Control, Summer 1960," *RCP*; "Session IV, National Security and Arms Control, Harry Rowen, Rand Corp., July 5, 1960"; and "The Theory and Practice of Blackmail," 28 June 1960, Dan Ellsberg [sic], Rand Corporation, Box 1, Folder 1.1 "Preliminary Seminar on Arms Control, Summer 1960," *RCP*. Also see Daniel Ellsberg, "The Theory and Practice of Blackmail," RAND Paper P-3883 (July 1968 [1959]).

of, say, Thomas Schelling (who spoke in the seminar later that year as well); but they were garmented in the language of power and politics, not of strategy and signaling and systems. Most telling was his opinion about organizing the government for arms control policy. Kissinger disparaged the creation of a new bureau, and instead believed arms control should be invested in a powerful, charismatic figure close to the President, employed in the White House.⁴⁹

The Carnegie project attracted many of the usual suspects in the seminar's first couple of years. Elliot and Sands had met most of them at the *Daedalus* conference and used that experience as the point of contact. Jerome Wiesner, James Killian, Wolfgang Panofsky, Ithiel de Sola Pool, and Daniel Lerner all spoke. Kenneth Boulding lectured about the need for a "world social contract" before China acquired the bomb. (With a twinkle in his eye, he also called Los Angeles "the first post-civilized urban agglutination.") There were some celebrity guests from further afield, including General Maxwell Taylor, soon to become Chairman of the Joint Chiefs of Staff, and the scientist-civil servant-novelist C.P. Snow, who told the seminar that the United States should follow the British example and invite more scientists into positions of government authority.⁵⁰

By the fall of 1961 Elliot could report back to Carnegie that "the program has been in part responsible for a general ferment within the Institute. A great many people at all levels of Institute life have been associated in one way or another with lectures, seminars, or general discussion." Some 25 or 30 faculty were regular participants in the seminar on arms control;

⁴⁹ Premonitions, there, of the dashing figure Kissinger would cut as Nixon's National Security Advisor a few years later. And certainly hints, too, of the threats from within that ACDA would face during the Nixon administration. "Carnegie Program: Science and Government, Seminar on Arms Control: Session V," 2 November 1960, Box 1, Folder 1.2 "Carnegie Seminar Fall 1960, Arms Control," *RCP*.

⁵⁰ Snow and his family were also escorted to Disneyland by a Caltech undergrad and, as reported faithfully in the *California Tech News*, the Snow clan liked the Jungle Ride best of all. "Carnegie Program: Science and Government, 10/24/61," Box 1, Folder 1.2 "Carnegie Seminar Fall 1960, Arms Control"; Clipping attached to D.C. Elliot to Kenneth E. Boulding, 16 February 1961, Box 2, Folder 2.6 "Correspondence A-B, 1960-66"; Clipping in *California Tech News* (1 December 1960), in Box 2, Folder 2.13 "Correspondence, Sh-St, 1960-66," all in *RCP*.

around 10 were committed to the arms control working group. Even the legendarily apolitical physicist Richard Feynman acted out of character by showing up to Henry Kissinger's seminar on the politics of arms control.⁵¹ Within a year or so the arms control seminar would evolve into a graduate seminar on "national security," but the basic structure (a visiting speaker, with Caltech faculty sitting in for the discussion) remained largely unchanged for most of the 1960s. In 1961 the historian Cushing Strout described a spirit of optimistic harmony between his university's new endeavors in arms control, and the government's. As he wrote in Caltech's in-house magazine *Engineering and Science*, "The commitment of President Kennedy's administration to the problem reflects, on the national level, the same concern that generated the Carnegie Program on arms control at Caltech."⁵²

* * * *

One topic rose time and again in the early years of the Caltech arms control seminar. It was the idea of *verification*, an issue that achieved heightened salience during the frustrating negotiations over the nuclear test ban. Verification won attention across the spectrum of arms control opinion, from the disarmament and peace activists to the more hawkish thinkers at RAND. Verification was an interesting subject, persistently central to the very idea of arms control.

A nuclear arms control or disarmament agreement between two states is a kind of contract. In exchange for the other's compliance, each promises to take, or abstain from taking, certain actions with respect to its nuclear weapons. Since an arms control agreement cannot live

⁵¹ On the self-created myth of Richard Feynman as a genius jokester completely innocent of questions of politics or morality, see, for example, an excellent recent post by Alex Wellerstein on his *Restricted Data* blog: Alex Wellerstein, "Feynman and the Bomb," *Restricted Data: The Nuclear Secrecy Blog*, 6 June 2014, <http://blog.nuclearsecrecy.com/2014/06/06/feynman-and-the-bomb/>.

⁵² D.C. Elliot, "Carnegie Program: Science and Government, Second Annual Report," 8 October 1961, Box 2, Folder 2.2 "Steering and Sub-Committee Reports, 1960-63," *RCP*; Cushing Strout, "Scorpions in a bottle," *Engineering and Science* 24, no. 7 (1961): 9-13, on 13.

on trust alone, each nation must have some way of assuring itself that it isn't bearing the entire burden of the agreement, that the other side isn't evading its commitments—to know the truth, to verify.

For most who considered verification analytically as a part of arms control and strategy, the best way to get verification (given the essential hostility of the Cold War) was through inspection: holding the other side under scrutiny, seeing for oneself. Amrom Katz, a physicist in the RAND Engineering Division who had worked on aerial reconnaissance since the 1940s (he'd supervised photography of the Operation Crossroads test in the Bikini islands in 1946), was RAND's local expert on inspection. Katz was a champion of using high technology to inspect foreign territory remotely and unilaterally, using technology under the United States' own control (rather than international inspection teams, e.g.). For Katz and those of a similar mindset, remote inspection was much preferred to the problematic "on-site inspections" carried out by officials on the ground. Katz was among the first to recommend to the Air Force, for example, that it initiate its own reconnaissance satellite program, ultimately realized in Project SAMOS (Satellite And Missile Observation System). "We believe in the open society," Katz told the Caltech arms control seminar in 1960. "We advocate 'open skies' and practice 'open mouths'. The Russians play their cards much closer to their chests. Secrecy is to them an essential part of their security system." Katz advocated the simulation of arms control evasion by teams in the United States, to learn how "hidiers and finders" (as he called them) might operate under a real agreement. In one proposal, he suggested conducting an arms control game in which Team A would try to build a missile site somewhere in an area the size of Texas, while Team B used various methods to find out where.⁵³

⁵³ Amrom Katz, "Something on Inspection," 19 July 1960, Preliminary Seminar on Arms Control: Session VII, Box 1, Folder 1.2 "Preliminary Seminar on Arms Control, 1960," *RCP*; Amrom H. Katz, "Hidiers and Finders:

The Southern California community had no shortage of people who could weigh in on the question of verification. Among these was Albert Hibbs, a staff scientist at the Jet Propulsion Laboratory, the Army- and NASA-funded missile and satellite research facility managed by Caltech. One of Richard Feynman's first students at Caltech, Hibbs finished his Ph.D. in physics in 1955. Like his advisor, he was something of a character. As a cash-strapped grad student in the late 1940s, he and a friend made thousands of dollars in Las Vegas casinos by observing imperfections in roulette wheels, then using a statistical algorithm to determine profit-maximizing roulette wagers. (They made enough money to purchase a 40-foot sailboat, sailing it around the Caribbean for an entire year.) In 1950 he returned to California to begin work as a rocket scientist at JPL. He would become especially well known in his role as the "voice of the unmanned space program," describing the science behind various launches and missions on radio and television broadcasts—the beginning of a long career as a science communicator and popularizer. A fixture of the cultural and intellectual scene in Southern California in the 1950s and 60s, he'd even undergone an experiment with LSD in the home of the Los Angeles psychiatrist Oscar Janiger in 1958, part of Janiger's famous early research on the psychotropic drug.⁵⁴

An Approach to Inspection and Evasion Technology," RAND Paper P-2432 (26 April 1961); Eric Pace, "Amrom H. Katz, 81, early expert on space reconnaissance," *New York Times* (15 February 1997).

⁵⁴ Myrna Oliver, "Albert Hibbs, 78; JPL scientist, voice of unmanned missions," *Los Angeles Times* (27 February 2003); John Whalen, "The trip," *LA Weekly* (3 July 1998); A.R. Hibbs, "Experiences Under LSD," 21 September 1958, Box 75, Folder 75.1 "Writings, "Experiences Under LSD," 1958, Sept. 21," *ARH*. Janiger administered acid trips for hundreds of LA-area people in the late 1950s and early 60s, including several Hollywood actors and other prominent figures. After taking the drug under supervision at Janiger's mountain cabin near Lake Arrowhead outside LA, Hibbs recorded his psychedelic experiences in a long letter, opening with a caveat about the futility of describing "by symbols an experience whose central feature is the contradiction of any symbolic representation of reality." Clearly the doors of perception had been opened. In fact Hibbs apparently introduced Janiger to Richard Feynman and, according to James Gleick, Feynman did try LSD (despite Feynman's own ambiguous demurral in his collection of autobiographical essays, "*Surely You're Joking, Mr. Feynman!*", in which he admitted trying marijuana but insisted that he "was reluctant to try experiments with LSD in spite of my curiosity about hallucinations"). If Feynman did satisfy his curiosity about hallucinations with LSD, it seems almost certain that it was under Janiger's care through Hibbs's introduction. See Oscar Janiger to A.R. Hibbs, 20 February 1959, Box 75, Folder 75.1 "Writings, "Experiences Under LSD," 1958, Sept. 21," *ARH*; and James Gleick, *Genius: The*

Hibbs became a regular in the Caltech arms control seminar beginning in 1961. In the spring of the following year, when the Arms Control and Disarmament Agency asked JPL to set up a new “Arms Control Study Group” (ACSG), Hibbs’s experience in the seminar made him the perfect man for the job.⁵⁵ He managed a small staff of about six full-time JPL engineers; a few part-timers, including David Elliot, conscripted by Hibbs from the Caltech seminar, also assisted the ACSG occasionally. Among the tasks of the ACSG was to dream up and evaluate inspection systems tailored to the special problems raised by space technology: to monitor an agreement forbidding nuclear weapons in orbit, for example, or an agreement limiting the manufacture of missiles. The ACSG was a technical link between ACDA’s science and technology bureau and NASA, drawing on JPL’s intimacy with NASA to describe what was possible in the area of space exploration and instrumentation. Sometimes the group was asked to turn out quick studies in response to questions that arose in the middle of negotiations. For example, in the spring of 1963, just as negotiations on the test ban treaty entered their final throes, Hibbs’s group was asked by ACDA whether “background radiations in space...would affect the detection of high-altitude and deep space nuclear bursts.” (No, they answered, they wouldn’t.)⁵⁶

Life and Science of Richard Feynman (New York: Pantheon Books, 1992), 406; Richard P. Feynman, “*Surely You’re Joking, Mr. Feynman!*”: *Adventures of a Curious Character* (New York: W.W. Norton, 1985), 205.

⁵⁵ This was the same year that Hibbs also began hosting a Saturday-morning children’s television show on NBC about science and technology, called *Exploring*.

⁵⁶ “Arms Control Study Group Report, May, 1962-1963,” and “Arms Control Study Group, Jet Propulsion Laboratory,” 1/10/64, Box 18, Folder 18.1 “Consulting Files, U.S. ACDA, ACSG, Reports, 1962-1964, 1967,” ARH. But it wasn’t only ACDA that was getting something out of the arrangement. As a result of his position as liaison between ACDA and NASA, Hibbs soon found himself in a rather awkward position. The NASA official to whom Hibbs reported was Abraham Hyatt, the director of the space agency’s Office of Program Planning and Evaluation. One day in early 1963 Hyatt invited Hibbs to chat in Washington. In a hushed meeting with an air of palace intrigue, Hyatt and two other NASA officials told Hibbs that NASA Administrator James Webb “was interested in the military potentialities of NASA space activities.” Hyatt wanted to know what the DOD planned to do in space, and to that end he had to “acquire suitable reports or other documents from the Department of Defense.” That was where Hibbs came in. He could use the Top Secret clearances afforded him by his ACDA consultancy “to turn over to [Hyatt’s] office all of the various results of our studies in the military application of space research,” especially in the area of reconnaissance satellites. “Then,” as Hibbs recorded Hyatt’s words in a private

As the ACSG's emissary to the growing community of arms control experts working under contract to ACDA, Hibbs was on the road a lot in 1962. That year he attended meetings and briefings at the Aerospace Corporation and the Hudson Institute, all to keep abreast of the work being done on ACDA's dime. He was present at an "evasion team exercise" held in Washington, which considered methods of clandestine treaty violation that had been thought up by the Bendix Systems Division under its contract to ACDA. And he was on the guest list at an International Arms Control Symposium in Ann Arbor, organized by the Michigan-based Institute for Arms Control and Peace Research, which itself had grown out of a new faculty arms control seminar at the University of Michigan. Bendix even set up a new *Journal of Arms Control*, in which Hibbs published some thoughts on the evasion of arms control agreements in 1964. At the symposium, Hibbs participated in a disarmament game—a super-small-scale version of what Amrom Katz's "hidiers and finders."⁵⁷

Hibbs's first major trip in 1962 was to a large ACDA summer study, the first of its kind, held at Woods Hole on Cape Cod and administered (naturally) by the Institute for Defense Analyses. Chaired by Berkeley physicist Owen Chamberlain, the study dissected the question of "verification and response." The main idea explored at the summer study—one that Hibbs would amplify and develop in further writings—was that inspection was but one component of

memorandum for the record (Hibbs had evidently been disturbed by the meeting), "we will see whether or not that material should be passed on to the Arms Control and Disarmament Agency." Hibbs was to tell no one of this interest—and certainly to avoid attributing it to Webb personally. If he was asked when making a request for information, he was simply to withdraw the request without explanation. Absolutely no one was to learn of this civilian space agency's interest in "the military application of space research," exploited through its relationship with the government's disarmament organization. See Hibbs's note dated "2/22/63," Box 17, Folder 17.19 "Consulting Files, U.S. ACDA, ACSG, Correspondence, 1963-1967," ARH.

⁵⁷ Robert E. Reiter, "Research for peace," *Michigan Alumnus* (November 1962): 74-77; "Arms Control Study Group Report, May, 1962-1963," Box 18, Folder 18.1 "Consulting Files, U.S. ACDA, ACSG, Reports, 1962-1964, 1967," ARH; P.E. Chase to Albert R. Hibbs, Box 73, Folder 73.6 "Publications, Evading Arms Control, *Journal of Arms Control*, 1964, Sept.," ARH; A.R. Hibbs, "Evading Arms Controls," JPL Study Memorandum No. 40-2 (26 December 1962).

verification, and verification only the first step in deterring an opponent from violating an arms control agreement.⁵⁸

For most of the participants the study was perhaps less useful as an intellectual exercise, and more productive as an occasion to engage the gears of the new, still rusty ACDA machinery for contract research and consulting. An unusual degree of security surrounded the event. Part of the bargain struck when ACDA was created were remarkably strict requirements for obtaining clearances to work within ACDA and on its various projects. This was to reassure the skeptics (like Robert Lovett and his worries about arms control “screwballs”) that the agency would not compromise the security interests of the U.S. government. But these procedures and background checks were so byzantine and inefficient that, as Hibbs told the Caltech group later that year, it “took so long that some people who were invited had to give up.” Amazingly, Arthur Barber, the Deputy Assistant Secretary of Defense for Arms Control under Paul Nitze (and a participant, before taking office, in the 1960 summer study on arms control in Boston) couldn’t get his own clearance until the summer study had been going for several weeks. Even “some of the people who had actually set up the conference,” Hibbs said, “were unable to get cleared and could not get into the building.” The group had lacked information about the performance of the Air Force’s SAMOS satellite, and no one from the CIA had shown up to tell the group about espionage capabilities. Hibbs, a little sour, doubted the final report would “be useful to anybody...”⁵⁹

⁵⁸ Woods Hole Summer Study, “Verification and Response in Disarmament Agreements,” Summary Report, 20 June 1962, reprinted as “United States: Institute for Defense Analyses Report of Meeting on Verification and Response in Disarmament Agreements (Woods Hole Summer Study),” *International Legal Materials* 2, no. 2 (1963): 320-331; A.R. Hibbs, “Multiple Meanings of ‘Verification’,” Box 75, Folder 75.8 “Publications and Writings, Writings, Multiple Meanings of Verification,” *ARH*.

⁵⁹ “Arms Control Seminars: First Term II, Al Hibbs’ Report on summer projects, November 5, 1962,” Box 1, Folder 1.4 “Carnegie Program, Science and Government, 1961-63, Arms Control Seminar Reports,” *RCP*.

To study verification by itself wasn't enough. A big question remained: what should happen if verification techniques discovered violations? What, then, was the best way to prevent violations? When analysts spoke of a "deterrent to evasion" and "actions in response" to violations of arms control agreements, they always had the work of RAND's Fred Iklé in mind. For Iklé, it wasn't enough to talk about verification or inspection alone—as though the pure weight of the truth, and the mere shame of being found out, were enough to prevent a determined violator from shirking its treaty obligations. In 1960, just after returning to Santa Monica from the *Daedalus* conference in Boston, and working under a RAND contract with ARPA, Iklé wrote an influential study that recast verification in the language of deterrence. Detecting an arms control violation was part of the job, he said. "Yet detecting violations is not enough. What counts are the political and military consequences of a detected violation, since it is these alone that determine whether or not a violator stands to gain in the end."

For Iklé, the notion of deterrence could be widened to apply to more than nuclear war. As with nuclear deterrence, under an arms control agreement what really counted was the credibility of the punishment. Iklé had little patience for vague talk of "world opinion" as a deterrent to law breaking. In response to a breach by the Soviet Union, he said the U.S. should threaten military action—an arms buildup to begin with and, if the situation called for it, military violence. In 1961 Iklé got a slightly stripped-down version of the paper into *Foreign Affairs*, complete with a punchy new title: "After Detection—What?" The article became an instant classic of the arms control literature. Compared to the verification work typified by the pacifist Seymour Melman's 1957 volume *Inspection for Disarmament*, Iklé's essay seemed unsentimental, hard-nosed, and

shrewd. For those who were just learning to see deterrence as a component of arms control, Iklé struck the perfect note.⁶⁰

Iklé, too, became a regular presence in the Southern California arms control world. He was about to head to Boston to try out life as a professor at MIT for a few years, but he wouldn't stay away from the West Coast for long. His ruminations on deterrence and its place in verification hinted at a deeper interest in thinking through basic premises of nuclear strategy and arms control. In coming years he would indeed give much thought to the structure and implications of deterrence—and decide, by the early 1970s, that those premises might not be so sound. Iklé would become a towering figure in the movement to challenge and reassemble prevailing arms control ideas in the 1970s. And he would play a unique and influential role in the complex interaction between the arms control interests of the government and the biggest private foundation in America.

The Ford Foundation, RAND, and the Southern California Seminar on Arms Control and Foreign Policy

“It is a long way from the wonder of Apollo 11 to this morass of division, doubt and danger,” wrote McGeorge Bundy in the October 1969 issue of *Foreign Affairs*. He was speaking, of course, about the noisy battles of the previous summer over MIRV and, especially, ABM. Apollo and these high-tech weapons systems had each been the “products of technology and teamwork.” But the weapons, not the space spectaculars, had reduced the American political scene to a swamp of discord. Bundy offered an explanation. Space exploration, he said, was like

⁶⁰ In 1962, when Iklé was asked (again for ARPA) to write set of reflections on “international organization of disarmament” (in which he drew a distinction between “impartial” verification by an international control organization, and “adversary” verification by the various parties to an agreement), he brought along David Elliot as part of the team. Fred Charles Iklé, “The Violation of Arms-Control Agreements: Deterrence Vs. Detection,” RAND Memorandum RM-2609-ARPA (1 August 1960), quotation on 1; Fred Charles Iklé, “After detection—what?,” *Foreign Affairs* (January 1961): 1-13; and Fred C. Iklé, “Alternative Approaches to the International Organization of Disarmament,” RAND Report R-391-ARPA (February 1962).

golf, a game in which you were responsible for your own performance. But the arms race was more like boxing, a sport played in relation to the moves and countermoves of your opponent—who might well, if the opportunity presented itself, hit you in the teeth.⁶¹

It had always been said that nuclear weapons confounded any rational political use for them. Now in 1969, when “the arms race” had superseded “the bomb” as the symbol of nuclear danger in American discourse, Bundy couched that old wisdom in new language. “The neglected truth about the present strategic arms race between the United States and the Soviet Union,” he wrote, “is that in terms of international political behavior that race has now become almost completely irrelevant.” The contest in technology and numbers far exceeded any conceivable political purpose for it. The “internal politics of the strategic arms race,” he said, “has remained the prisoner of its technology”: “those who oppose the ABM tend to argue that it may not work technically—not that it is irrelevant politically.” Bundy concluded that the mission of arms control was, at the earliest possible date, “to cap the volcano.” This was nuclear competition as natural disaster.⁶²

In Richard Nixon’s 1969 commencement address to the Air Force Academy in Colorado Springs, Nixon had said that unilateral disarmament was unthinkable “because in the real world it wouldn’t work. If we pursue arms control as an end in itself, we will not achieve our end. The adversaries in the world are not in conflict because they are armed. They are armed because they are in conflict,” he said. Such was the basic incompatibility between arms-race theory and the *realpolitik* that characterized the diplomatic style of Nixon and his jet-setting national security advisor, Henry Kissinger. On this score Bundy threw his lot in with the arms controllers. The arms race might be a disaster beyond the compass of politics, but “if we can slow it down, or

⁶¹ McGeorge Bundy, “To cap the volcano,” *Foreign Affairs* 48, no. 1 (1969): 1-20, quotation on 5.

⁶² Bundy, “To cap the volcano,” 11-12.

perhaps even turn it back, will that not perhaps help [the U.S. and the Soviet Union]—even help us *all*—politically?”⁶³

McGeorge “Mac” Bundy: princeling of the foreign policy establishment. The scion of a Boston Brahmin family, Skull and Bones at Yale, taken under the wing of Henry Stimson (a friend of Bundy’s father, who, during World War II, was a special assistant to Stimson as Secretary of War), the young man was groomed for public service from an early age. He was the epitome of the scholar-statesman, foremost among the “wise men” who gave an Ivy League luster to the Kennedy White House. He was, in effect, the first “modern” national security advisor—a close confidante of the President, the whole of U.S. foreign policy, intelligence, and military strategy within his ken.⁶⁴

The nuclear question ran like a thread through Bundy’s life and career. As Stimson’s amanuensis, Bundy had helped assemble the former War Secretary’s memoirs. He had also ghostwritten Stimson’s famous 1947 essay in *Harper’s* magazine, which had justified the U.S. decision to drop the atomic bombs on Japan as necessary to end the war and save American lives.⁶⁵ In 1952, acquiring his first taste of Washington politics, Bundy found himself secretary

⁶³ Richard Nixon, “Address at the Air Force Academy Commencement Exercises in Colorado Springs, Colorado,” 4 June 1969, published online by Gerhard Peters and John T. Woolley, The American Presidency Project, available at <http://www.presidency.ucsb.edu/ws/?pid=2081>; Bundy, “To cap the volcano,” 20. Perhaps most often quoted from this essay is Bundy’s remark (on 10) that “a decision that would bring even one hydrogen bomb on one city of one’s own country would be recognized in advance as a catastrophic blunder; ten bombs on ten cities would be a disaster beyond history; and a hundred bombs on a hundred cities are unthinkable.”

⁶⁴ Andrew Preston, *The War Council: McGeorge Bundy, the NSC, and Vietnam* (Cambridge, MA: Harvard University Press, 2010). The position of national security advisor, now one of the most prominent in government, was so little regarded before 1961 that even a government insider like Jerome Wiesner could be unsure of the actual title of the job. When he wrote to Kennedy aide Ted Sorensen to recommend some names to fill the slot, he told Sorensen “I think that this is the title of the post that Gordon Gray fills.” Gray was in fact the fourth national security advisor, a position created during the defense establishment shakeup of Eisenhower’s first administration. But neither Gray nor any of the previous security advisors had anything like the influence of the next three: Bundy, Walt Rostow, and Henry Kissinger. (Wiesner didn’t recommend Bundy in 1960, incidentally, but he did recommend Rostow.) Jerome Wiesner to Theodore Sorensen, 22 November 1960, Box 9, Folder 287 “Kennedy – General, 1960-61 1/3,” *JBW*.

⁶⁵ Henry L. Stimson, “The decision to use the atomic bomb,” *Harper’s Magazine* 194 (1947): 98–101. Also see Barton J. Bernstein, “Seizing the contested terrain of early nuclear history: Stimson, Conant, and their allies explain the decision to use the atomic bomb,” *Diplomatic History* 17, no. 1 (1993): 35–72.

to a special panel of disarmament consultants appointed by Secretary of State Dean Acheson and chaired by J. Robert Oppenheimer. This group had arrived, at a notably early date, at some of the basic conclusions that would come to be hallmarks of strategic deterrence, and even spoke of a “strange stability arising from general understanding that it would be suicidal to ‘throw the switch’.”⁶⁶ Bundy passed most of the 1950s as a professor of government (sans Ph.D.) at Harvard and, later, dean of the University’s Faculty of Arts and Sciences. All in all a successful experience, but smirched in hindsight by Bundy’s penchant for threatening the jobs of suspected communists on the faculty. He kept up his nuclear interests as an academic, coauthoring with Jerome Wiesner and a few others a letter published in *The New York Times* in 1950 criticizing America’s “misplaced faith in atomic weapons and strategic bombing.” At the end of his Harvard career, he participated in the first semester of the Harvard-MIT arms control seminar in the fall 1960. Once in the White House in 1961, his first briefing on the nuclear war plans was given to him by Daniel Ellsberg. Ellsberg shocked Bundy—as the SIOP tended to shock everyone on a first encounter—with the scale of violence the U.S. was prepared to commit.⁶⁷

As he typed up his essay in 1969, Bundy still wrote from a position of influence—no longer from the National Security Council but from a plush office in midtown Manhattan, kitty-corner to the United Nations headquarters. In 1966 he had become president of the Ford Foundation. He settled into his new job at a time of increased scrutiny and financial strain for the major private foundations. Criticisms mounted in the 1960s that the foundations had amassed huge concentrations of tax-exempt wealth, interfered in the market, and had effectively become political lobbies (to say nothing of recent revelations that the CIA had secretly funneled money

⁶⁶ McGeorge Bundy, “Early thoughts on controlling the nuclear arms race: A report to the Secretary of State, January 1953,” *International Security* 7, no. 2 (1982): 3–27, on 12.

⁶⁷ McGeorge Bundy, et al., “Strategy of defense,” *New York Times* (30 April 1950): E8; Kai Bird, *The Color of Truth: McGeorge Bundy and William Bundy, Brothers in Arms: A Biography* (New York: Simon & Schuster, 1998), 208–210.

through several of them). Calls arrived for greater financial accountability and public vetting of foundation programs. The foundations had traditionally justified their tax-exempt status by claiming to spend “private money in the public interest.” Their freedom to spend as they saw fit encouraged progressive experimentation, they argued—the sort of programs that a bloated, hidebound government bureaucracy couldn’t pursue. The counter-claim was that in light of their tax-exempt status, the foundations were in effect “public trusts,” and should be held responsible as repositories of public—not private—money.⁶⁸

The debonair Bundy represented a “new look” at the responsibilities and roles of private foundations in American society. He was at the forefront of efforts to restore an image of vitality and accountability to private philanthropy in America. He endeavored to put the Ford Foundation on a surer financial footing while pursuing liberal-progressive projects with unapologetic enthusiasm. He liked the job, and took a deep interest in the Foundation’s charitable projects. Vigorous as ever, he ran his 400-person organization much as he had run the NSC—by getting personally involved in almost everything it did. Within months of starting at Ford, he had launched two major initiatives in public television and inner city education. Each venture was mired in controversy—especially the Foundation’s attempt to decentralize the New York City school system, an intervention that met the fierce resistance of the teachers’ union. Conservative critics charged Ford with using its money as a liberal political weapon. Bundy, bounding with confidence, a well-meaning but wealthy, WASPy paragon of the power elite, found himself face to face with the messy, fractious politics of the late 1960s.⁶⁹

⁶⁸ By 1969 the attacks and inquiries had produced a reform of the federal tax code requiring a levy on the foundations’ investment income. The reforms were regarded by the foundation executives and program officers as a serious encumbrance, demanding belt-tightening in the coming decade. See Thomas C. Reeves, ed., *The Foundations Under Fire* (Ithaca, NY: Cornell University Press, 1970), esp. 1–35. On the CIA’s use of foundations as effective money-laundering devices, see Frances Stonor Saunders, *The Cultural Cold War: The CIA and the World of Arts and Letters* (New York: The New Press, 2000).

⁶⁹ Bird, *The Color of Truth*, 376–395.

Much closer to his own strengths and experience was Ford's portfolio in international affairs. This was a legacy investment. In just a three-year period at the height of the Cold War in the early 1950s, the Foundation had shelled out an astounding \$50 million for its international programs. Universities had seen a good chunk of this money. In 1952, along with the CIA, Ford had established MIT's Center for International Studies (CIS). In the late 1960s, now that president Bundy had come out as a public defender of arms control, it seemed clear that some of Ford's attention would shift in that direction.⁷⁰

In fact the Foundation had flirted with arms control research right from the first days of the new field. Way back in 1960, Joseph Slater of the Foundation's International Affairs Program got in touch with Ernest Lefever, a foreign relations aide to Hubert Humphrey (and future member of the Kennedy transition team's arms control task force). Slater asked Lefever to write a report on the possibility of the Foundation's funding arms control research. In short order Lefever organized a lunch meeting at the Brookings Institution, inviting various Washington-area insiders, to talk about creating a "non-government unit of specialists financed by a private foundation."⁷¹ This research unit would consist of a handful of experts, perhaps five or six, "of proved competence in various relevant facets of the arms control field." This wasn't fodder for early-career work or doctoral training—"no job for fresh Ph.D.'s," the group agreed. What was wanted was an elite corps that could translate the diverse products of academic research into

⁷⁰ For the Ford Foundation's international programs in the early 1950s and the establishment of the MIT Center for International Studies, see Michael E. Latham, *Modernization as Ideology: American Social Science and "Nation Building" in the Kennedy Era* (Chapel Hill, NC: The University of North Carolina Press, 2000), 53-55.

⁷¹ It was an early gathering of what would become a familiar crowd. Among the guests were Betty Goetz of Humphrey's staff, Spurgeon Keeny (then still on the technical staff of PSAC), Roger Hilsman (then part of the Legislative Research Service and soon a State Department intelligence official under Kennedy), Jeffrey Kitchen of the RAND Washington office's Social Science Division, Herbert Scoville (still part of the CIA), Helmut Sonnenfeldt (on the Soviet desk in the State Department), and Lawrence Weiler (then working in the State Department in the Office of the Special Assistant to the Secretary for Atomic Energy and Disarmament). A large fraction of this group would, at one time or another during the 1960s, work for the Arms Control and Disarmament Agency.

action-items for government policymakers. Ford had no intention of creating a new organization from scratch; an existing parent institution was therefore needed.⁷²

RAND was the early frontrunner. Lefever counted 37 RAND staff and affiliates working on topics related to arms control. In his estimation RAND had “done more such research than all other universities and research centers combined.” But RAND was hardly a consensus choice, and Lefever’s own view on the matter was muddied somewhat after a meeting with James King, head of IDA’s new Special Studies Group. RAND was “a creature of the Air Force,” King told him, and “many people think that RAND is biased in favor of the Air Force...” RAND exists in “splendid isolation”; it had “created a mood of rivalry and hostility and even displayed some arrogance within the systems research community.” Plus it was too far away from the nerve center in Washington. “The country needs a second RAND.” Naturally he was thinking of the Washington-based IDA. Others Lefever spoke with, however, recommended RAND over IDA “without hesitation.”⁷³

When it was completed later that summer, Lefever’s report announced a “need for a small group of highly competent, operationally-minded experts located in Washington who can serve as a bridge between the government and private individuals and institutions and as a clearing house in this field.” In the entire universe of government policymaking, Lefever guessed there were “perhaps 30 people working explicitly on arms control, and none of them fully on research.” Contrast that with the “20,000 to 30,000 people working on ballistic missiles”—quite a ratio. For the new arms control group, Lefever was asking Ford for close to \$1 million over a

⁷² Ernest W. Lefever to Joseph E. Slater, 3 February 1960, Box 37, Folder 388 “Subject: Arms Control, Correspondence: Ernest Lefever,” *JES*.

⁷³ “RAND Staff and Consultants Who Have Worked or Are Working in Arms Control, June, 1960 (Sent by Ernest W. Lefever)”; and Ernest W. Lefever, Memorandum to the File, 17 June 1960, Institute for Defense Analyses, Conversation with James E. King, June 16, 1960, both in Box 37, Folder 388 “Subject: Arms Control, Correspondence: Ernest Lefever,” *JES*.

five-year period—a sum that would have immediately made the Ford Foundation the most important supporter of arms control in the United States. Yet despite this early burst of attention and ambition, the project fizzled. With ACDA up and running the following year, the enthusiasm for a separate arms control body in the 1960s waned.⁷⁴

A decade later, the conditions had changed, and the arms control mood even more so. In the form of the SALT negotiations, the superpowers appeared to be engaging one another seriously on the basic problems for the first time. Yet the arsenals—greatly expanded, soon MIRVed, potentially defended by ABMs—looked more dangerous than ever.

In April of 1970, McGeorge Bundy appeared alongside Herbert York before the Senate Subcommittee on Arms Control, International Law and Organization chaired by Albert Gore, Sr. Wanting to put distance between himself and the disastrous escalation in Vietnam that had darkened his final years as Lyndon Johnson's national security advisor, Bundy was ready to return to nuclear issues wholeheartedly. "It is wholly false to suppose that the national security is always served by adding strategic weapons and never by their limitation," he said. "In the world of the 1970s the truth is more nearly the opposite." He added his voice to the opposition against ABM and MIRV. He warned of the flippant use of strategic buzzwords like "sufficiency," the Nixon administration's preferred term, meant to suggest an acceptance of the basic equivalence of U.S. and Soviet nuclear offensive power—parity, in the word that would soon come to replace it. These slippery "verbal formulae," he said, could "be used to justify excessive expenditure on unnecessary strategic systems." This wasn't just the heartfelt opinion of a "private citizen" (as

⁷⁴ Lefever attempted to rekindle interest in his Ford-sponsored arms control institute at least twice, but to no success. (Arnold Wolfers of the Washington Center of Foreign Policy Research, for example, told Lefever in 1962 that arms control research was "no longer a 'depressed area'.") In the autumn of 1963, as Lefever renewed his pitch for the last time (now proposing a more directly educational role for the institute), the Foundation dropped the idea indefinitely. See Arnold Wolfers to Ernest W. Lefever, 16 March 1962; Ernest W. Lefever to Joseph E. Slater, 17 April 1962; Ernest W. Lefever to Joseph Slater, 2 October 1963, all in Box 37, Folder 388 "Subject: Arms Control, Correspondence: Ernest Lefever," *JES*.

Bundy called himself); this private citizen had, at the darkest hour of the Cuban Missile Crisis in 1962, counseled President Kennedy to launch an airstrike against the Soviet intermediate-range ballistic missiles. Now, before the Senate in 1970, Bundy was speaking not only as a former government official, but as a well-connected insider at the top of a powerful organization. He was speaking to the Ford Foundation. His testimony was printed and circulated as an internal Foundation report.⁷⁵

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“As much as any man, [William] Bader, a 37-year-old staff consultant to the Senate Foreign Relations Committee, has shaped and guided the ABM debate in the Senate,” reported the *New York Times* in March 1969. As J. William Fulbright’s top aide, Bader scrutinized the major arms control developments of the late 1960s as they passed in front of Fulbright’s powerful Senate committee. He had written the Foreign Relations Committee’s minority and majority reports on the Nonproliferation Treaty in 1968; he had armed Fulbright with facts and arguments about the ABM as the Senator mounted his prominent attack on the weapons system. It was quite an education for the young scholar, trained as an expert in 19th-century European history with a Ph.D. from Princeton. He was making a name for himself in the world of arms control. Just days after the profile in the *Times* appeared, Bader left Washington and started a new job as a program officer with the Ford Foundation, working in its European and International Affairs Program. Bader was “a lean, handsome man with dark eyes,” said the

⁷⁵ Statement of McGeorge Bundy, Subcommittee on Arms Control, International Law & Organization, Committee on Foreign Relations, April 8, 1970, filed as Ford Foundation Report 018705, *FFR*. On the various attempts to wrestle with the meaning and consequences of nuclear parity in the 1970s, see Gavin, *Nuclear Statecraft*, esp. Chapter 6, “That Seventies Show: The Consequences of Parity Revisited,” 120-133.

Times; “urbane,” another writer called him. He was delighted to be Paris-bound as the new chief of the Ford Foundation’s office there.⁷⁶

Ford had acquired Bader because of his experience in arms control, a field the Foundation under its new president was beginning to take serious interest in. It is too simple to say that Ford’s arms control interest simply filtered down from Bundy’s office. The real work was done by its program officers—the “philanthropoids,” as they were sometimes called—who studied the issues intensively, formulated concrete policy out of the general guidelines given by the president and trustees, investigated the local conditions where foundation money might be spent, and formed close working relationships with grant recipients. They did the interviews, evaluated the proposals, and produced the voluminous internal correspondence that considered and reconsidered every award. The program officers were the human gears that meshed foundation philosophy with grant payments and the research they produced.⁷⁷

Within a few months of starting with the Ford Foundation, Bader took a tour of the United States. At Columbia University he met with the Soviet expert Marshal Shulman; in California he talked with the physicist Wolfgang Panofsky at Stanford. And in Chicago, he met with Albert Wohlstetter. “After the usual exchange of pleasantries in his sumptuous lakeside apartment, Professor Wohlstetter asked what brought me to Chicago for dinner,” Bader reported back to Ford headquarters. Bader proceeded to tell Wohlstetter that the Ford Foundation was interested in getting into arms control and defense policy. The “national pool of scientists and

⁷⁶ “Senate ABM examiner: William Banks Bader,” *New York Times* (22 March 1969): 17; Anne Hessing Cahn, *Killing Détente: The Right Attacks the CIA* (University Park, PA: Pennsylvania State University Press, 1998), 11.

⁷⁷ See Joseph C. Goulden, *The Money Givers* (New York: Random House, 1971), 81–110. The point here concerning program officers as “middle managers” who actually implemented foundation programs is similar to a point made by Rebecca Lowen in *Creating the Cold War University*. As she suggests, it was often the mid-level academic administrators who did the most, by managing the contracts and promoting certain interests over others, to allow the “Cold War Universities” like Stanford and MIT to absorb massive amounts of federal money and grow so rapidly.

social scientists who were both concerned and informed on defense questions was not only overworked but diminishing in size,” he said. The younger scientists were “contemptuous of government”; they were “painfully ill-equipped to be the Kistiakowskys or Wohlstetters of the future.” Bader and a few of his colleagues at the Foundation were wondering about the development of “forums—preferably on a regional basis—where national defense issues could be discussed by a mixture of scientists and interested social scientists.”⁷⁸

Wohlstetter liked the sound of this. As he told Bader, the ABM debate (in which Wohlstetter had been one of the major expert combatants) “demonstrated that there is a critical need for diversity of informed national opinion on arms control issues.” He believed that “Congress was largely captured and occasionally [misled] by the Charles River crowd” (this despite the fact that the Safeguard ABM system had passed its crucial Senate vote). The national debate needed more voices than those of the “modern day scientific priests,” and voices in a regional (i.e., non-Cambridge) dialect would be all the better. How about the University of Chicago and its Center for Policy Studies, where Morton Kaplan of the University’s Committee on International Relations might be up for leading an arms control seminar? Or better yet, why not tilt things back toward the West Coast, where Wohlstetter still spent the winter months?⁷⁹ The two began to make plans. Bader made visits the following March to Chicago and Los Angeles. The Chicago group seemed disarrayed and factional (the liberal crowd associated with the *Bulletin of the Atomic Scientists* versus Wohlstetter and Kaplan, who breathlessly told Bader

⁷⁸ Memorandum from William B. Bader to Mr. Swearer, 23 November 1969, Subject: Regional Arms Control Study Groups – Discussion with Albert Wohlstetter, in Ford Foundation Grant 07000495, *FFG*.

⁷⁹ Memorandum from William B. Bader to Mr. Swearer, 23 November 1969, Subject: Regional Arms Control Study Groups – Discussion with Albert Wohlstetter, in Ford Foundation Grant 07000495, *FFG*.

that the Soviets were not only striving for “a first strike capability, but they already have it”). Bader found the conditions in Southern California “most encouraging.”⁸⁰

As David Elliot later remembered it, Harold Brown, who had come back to California after eight years in the DOD (first as DDR&E, then as Secretary of the Air Force), instructed Elliot to present himself at Henry Rowen’s house one evening, where a meeting was to take place to discuss the creation of a new arms control seminar. Rowen had become RAND’s president three years earlier, also after a DOD tour of duty. Assembled at Rowen’s house in West Los Angeles Elliot found Alain Enthoven (no mere RAND analyst, but the Ur-Whiz Kid, who’d joined the comptroller’s office in the McNamara Pentagon and became the first Assistant Secretary of Defense for Systems Analysis in 1965), Frederic Hoffman (a RAND alum from the 1950s and an intimate of Wohlstetter’s circle), and Wohlstetter himself. At the meeting that night, the group soon agreed: they would attempt to fire up the old Los Angeles-area arms control seminar again, but this time under the aegis of Ford money, and with more active involvement by RAND. As Brown had proposed to Bader, he would contact McGeorge Bundy directly by phone. The two presidents chatted and concluded that the Californians should submit an official proposal.⁸¹

Discussions continued over the next few months. By July 1970, the group had proposed that the seminar would publish its own papers (Elliot felt he had “missed a trick” by not setting up a journal for the old Caltech seminar). And it would adopt a “working group” structure (similar to what the Caltech seminar had aspired to, but never quite achieved). The working

⁸⁰ William B. Bader to Mr. Bundy, 12 March 1970, in Ford Foundation Grant 07000495, *FFG*. Bader and Wohlstetter soon became quite fond of one another’s company. In future years, when Wohlstetter would visit his brother’s winery in Bordeaux, Bader would sometimes get an invitation to join him. See, for example, William Bader to Albert Wohlstetter, 14 May 1973, in Ford Foundation Grant 07000495, *FFG*; and Cahn, *Killing Détente*, 11.

⁸¹ Elliot interview, 1986; William B. Bader to Mr. Bundy, 12 March 1970, in Ford Foundation Grant 07000495, *FFG*.

groups would confine themselves to specific research projects and, after developing a take on some issue, would present it to the wider seminar for discussion. This would be no leisured, academic workshop. The ambition was “to combine intensive research and debate on...arms control & foreign policy goals [and] to make new sources of research and analysis in these areas available to the public and government.” The goals were national in scope, but the proposal retained its local flavor. The architects of the seminar envisioned the “active participation of both distinguished residents of greater Los Angeles,” as well as “the participation of promising, young professionals from local universities and research institutes,” including UCLA. It was to be a distinctive Southern California blend of Caltech scientists, RAND strategists, engineers from local defense contractors, conservative business figures and *Los Angeles Times* newspaper editors. The journalists were a special addition. They would, it was hoped, provide an immediate conduit to the public for the intellectual products of the seminar. Unlike a decade earlier, when such discussions had been conducted in shaded seclusion, Wohlstetter and companions understood that their work was as much for use in public battle as for academic discussion.⁸²

By October, the Ford Foundation had awarded an initial three-year grant of \$285,000. Caltech would manage the money since Ford had, in Elliot’s words, gotten the “heebie-jeebies about the thought of giving something to the RAND Corporation in this area, feeling perhaps that there would be a certain bias or slant. They felt more comfortable giving it to an educational institution.” And indeed, when William Bader visited Harold Brown in his office in early 1970, Brown had assured Bader that Caltech was unopposed to managing all the money, keeping it

⁸² Ciro Zoppo, “Brief Introductory Remarks on Seminar,” 20 October 1970, Box 3, Folder 3.12, *RCP*; Elliot interview, 1986. On Brown as an administrator, Elliot remembered one day in particular, very early in Brown’s career as Caltech president, when Elliot was summoned to Brown’s office for an early-morning meeting. In the span of an hour and a half, between 7:30 and 9:00am, he watched Brown restructure the Humanities Division, convincing the current chairman to willingly step down, and another faculty member to willingly take his place. According to Brown’s secretary, Elliot related, it was said that he could keep the office “busy for about a week on the work he did between here and the airport.” Elliot interview, 1986.

clean of RAND's fingerprints.⁸³ In the fall the arms control salon had been revived, this time under a new moniker: the "Southern California Seminar on Arms Control and Foreign Policy." "You're going to be the co-chairman of that," Harold Brown informed David Elliot. There was no debating with Brown, who Elliot recalled as a brilliant and decisive administrator (Secretary of Defense material—a post Brown assumed under President Jimmy Carter). Joining Elliot as co-chairmen of the seminar were Fred Iklé, now returned to Santa Monica from Cambridge as head of the RAND Social Science Department, and Ciro Zoppo, a onetime RAND analyst who took a job in the UCLA political science department but kept an office in the "sanitized" non-secret part of the RAND headquarters.⁸⁴

From the beginning the California Seminar was designed as a non-governmental engine of arms control policy ideas, with a special West Coast bent. As Elliot wrote to Harold Brown, he felt that Caltech "should try and contribute to such matters of public policy," that the Institute had "some obligation to provide an opportunity for...students and younger faculty to contribute in this area," that arms control needed "a new generation of scientifically trained people in the field." And as a later grant proposal to the Ford Foundation put it, the California Seminar "has

⁸³ William B. Bader to Mr. Bundy, 12 March 1970, in Ford Foundation Grant 07000495, *FFG*. At the same time, Ford awarded a slightly smaller grant to the University of Chicago, whose faculty arms control seminar commenced under the leadership of Morton Kaplan. Wohlstetter and Bader were quickly disappointed with it. Bader reported to Ford's leadership that the seminar very quickly became Kaplan's personal soapbox and a vehicle for his own research interests. Kaplan, who had suddenly become quite taken with the ongoing SALT negotiations, scheduled session after session on various aspects of SALT. Wohlstetter, disgusted, wrote a long memorandum of complaint for Bader and quickly distanced himself from the seminar, associating firmly with the California group. By January 1971 an internal note at the Foundation counseled that Bader "keep a very close eye on these operations." By May Bader reported that he was "most unhappy about recent developments concerning the Chicago Arms Control group"; he described Kaplan as "irascible" and "ideological." By 1973, Bader would recommend turning down Chicago's request for extension of its grant, while the California group would be funded by Ford well into the 1980s. See William B. Bader to Roberta Wohlstetter, 2 February 1971; Craufurd Goodwin to William B. Bader, 14 January 1971; William B. Bader to Craufurd D. Goodwin, 21 May 1971; and William B. Bader to The Files (Program Action No. 700-0495), 13 August 1973, all in Ford Foundation Grant 07000495, *FFG*.

⁸⁴ Ciro Zoppo, "Brief Introductory Remarks on Seminar," 20 October 1970, Box 3, Folder 3.12, *RCP*; Elliot interview, 1986; Seminar News Release, 9 October 1970, "Ford Foundation Grant to Establish Southern California Arms Control and Foreign Policy Seminar Under Sponsorship of California Institute of Technology and RAND," Box 3, Folder 3.12, *RCP*; Brown quotation paraphrased by Elliot in Elliot interview, 1986.

much to contribute to the national debate on security issues by applying the expertise of its unique mix of participants. It is further useful in making policymakers aware of ideas developed by West Coast professionals.” But it was also a somewhat awkward setup in the beginning. Elliot complained that Caltech was in the uneasy position of holding the bag while almost all of the major decisions about how to spend the money emanated from RAND. In the days of the Carnegie program, Caltech had not only managed the money but had welcomed the RAND folks as *guests*. “Is Caltech really interested in the project?” he asked Brown bluntly. Elliot felt himself tugged in multiple directions, “peculiarly and complexly responsible to Caltech, to the Rand co-sponsor, to the Seminar, and to the Ford Foundation—quite apart from my own conscience and the Lord God Almighty,” as he put it to Brown. Even if Caltech controlled the bank account, it was clear that RAND was the intellectual driving force of the reincarnated California Seminar.⁸⁵

The Seminar’s first meeting was held in October 1970 at Caltech. Nuclear nonproliferation was the topic; Harold Brown was in the chair, and Zoppo and Victor Gilinsky, head of RAND’s Physical Sciences Department, delivered the opening remarks. It was a nearly four-hour affair, including dinner and an intense discussion that lingered late into the night. Present in the audience that evening were a host of Southern California notables, including Michael Intrilligator (who had mapped the theory of arms races at RAND six years earlier and since joined UCLA as an economist), Charles Wolf (the influential head of RAND’s Economics Department), and the physicist Murray Gell-Mann.⁸⁶

⁸⁵ David C. Elliot to Harold Brown, 20 April 1971, Subject: “Southern California Arms Control and Foreign Policy Seminar,” Box 3, Folder 3.12, *RCP*; “Grant Proposal to the Ford Foundation, December 1983, California Seminar on International Security and Foreign Policy,” Box 3, Folder 3.17 “California Seminar: Ford Foundation, 1983-91,” *RCP*.

⁸⁶ Discussion Meeting of October 20, 1970, on “The Proliferation of Nuclear Weapons in the Coming Decade,” Rapporteur’s Final Digest, Southern California Arms Control and Foreign Policy Seminar, Box 31, Folder 31.14 “Arms Control and Foreign Policy Seminar 1970-71,” *MGM*.

In the 1970s, many strategic analysts and arms controllers believed they had suddenly woken up to a new and frightening world, one more complex and messy than the seeming predictability of the bipolar Cold War. Now smaller powers—less trustworthy nations with unknown military traditions and doctrines, uneducated in the art of stability—would come into the custody of nuclear weapons. Gilinsky argued that the Nonproliferation Treaty of 1968 had “not solved the proliferation problem,” since civilian nuclear programs proceeded apace in numerous countries, and the relatively toothless International Atomic Energy Agency was unable to enforce restrictions—so-called “safeguards”—on the diversion of enriched uranium from civilian to military purposes. “IAEA controls on nuclear materials are nothing more than a warning system against clandestine diversion,” Gilinsky said, “and it is not clear that a warning system is adequate, even if it provides early warning.” Proliferation, too, was a problem of compliance and evasion. As analysts had learned from Fred Iklé a decade earlier, a warning system was not enough to prevent “clandestine diversion.”⁸⁷

Brown, in response to Gilinsky’s remarks, wondered “about the possibility of subnational groups diverting fissile material.... How would safeguards work to prevent or detect that possibility?” They wouldn’t, replied Gilinsky. Ciro Zoppo called such organizations “irredentist and revolutionary groups”; they might have called them nuclear “terrorists,” but that term was still uncommon. He cautioned that they “may prove to be the most intractable proliferation problem of the future. Given widespread nuclear technology and internal political instability, there will be some real possibilities for undesirable groups to gain access to nuclear weapons or at least a nuclear option.” Gell-Mann asked about the chances that such a group could actually

⁸⁷ Discussion Meeting of October 20, 1970, on “The Proliferation of Nuclear Weapons in the Coming Decade,” Rapporteur’s Final Digest, Southern California Arms Control and Foreign Policy Seminar, Box 31, Folder 31.14 “Arms Control and Foreign Policy Seminar 1970-71,” *MGM*. In the mid-1970s Gilinsky would be called up as a Commissioner on the Atomic Energy Commission. See Nuclear Regulatory Commission, “NRC: Victor Gilinsky,” available online at <http://www.nrc.gov/about-nrc/organization/commission/former-commissioners/gilinsky.html>.

build a weapon. Brown thought that “a group of physicists with modest technical abilities would be able to produce nuclear weapons in one or two years if they had sufficient quantities of U-235.” This was unsettling subject matter, a far cry from the *terra firma* of bipolar stability and the familiar “lonely interdependence” of the two superpowers (as McGeorge Bundy had called it in 1969). “We assume the situation is more stable if only two nations possess nuclear weapons instead of three, four, or more,” Brown said, his words already colored with nostalgia.⁸⁸

A few years later James Reston would write that the world had embarked upon a “second nuclear age,” when “nuclear blackmail” could be carried out not by responsible nation-states but by “political desperadoes” holding “entire cities hostage.” Reston’s article quoted heavily from a California Seminar regular, Fred Iklé. Iklé invited Reston to “imagine the morning after a nuclear explosive has destroyed half an American city. How are we going to apply our theories of mutual deterrence, of first strike and second strike, if we cannot tell whose nuclear explosive it was? Or even if we could tell, but it turned out to be an organization...with dedicated people but no clearly defined national territory?” In this terrifying world of proliferating nuclear actors (even stateless terrorists with the bomb), clearly the stock items of bipolar nuclear strategy and arms control—deterrence, first- and second-strike forces, and stability—were in need of drastic reconsideration. That was just the thing for Iklé.⁸⁹

⁸⁸ Discussion Meeting of October 20, 1970, on “The Proliferation of Nuclear Weapons in the Coming Decade,” Rapporteur’s Final Digest, Southern California Arms Control and Foreign Policy Seminar, Box 31, Folder 31.14 “Arms Control and Foreign Policy Seminar 1970-71,” *MGM*. On the origins of the concept of terrorism (as well as the term itself) see Lisa Stampnitzky, *Disciplining Terror: How Experts Invented “Terrorism”* (New York: Cambridge University Press, 2013). For Bundy’s description of the nuclear relationship between the U.S. and the Soviet Union as one of “lonely interdependence,” See Bundy, “To cap the volcano,” 6. On the general theme of the recognition of “the global” (and multilateral, rather than bilateral, “interdependence”) across fields of political and intellectual activity in the 1970s, see, e.g., Niall Ferguson, Charles S. Maier, Erez Manela, and Daniel J. Sargent, eds., *The Shock of the Global: The 1970s in Perspective* (Cambridge, MA: Harvard University Press, 2010).

⁸⁹ The idea of a “second nuclear age” has often been bandied about since the demise of the Soviet Union. For example: Keith B. Payne, *Deterrence in the Second Nuclear Age* (Lexington, KY: The University Press of Kentucky, 1996); Colin S. Gray, *The Second Nuclear Age* (Boulder, CO: Lynne Rienner Publishers, 1999). More recently Paul Bracken uses it as the title of his widely noted book, *The Second Nuclear Age: Strategy, Danger, and the New Power Politics* (New York: Times Books, 2012). Bracken means the term somewhat differently, to suggest

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The California Seminar was initially equipped with four working groups. The first, studying “the impact of changes in Soviet and U.S. strategic forces on deterrence,” was to be directed by Albert Wohlstetter. Another group led by Fred Iklé would look at “nuclear strategy for the long run,” inquiring about “alternatives to deterrence.” Zoppo planned to chair a group on “future proliferation and arms control,” and Zoppo and Iklé together would lead a fourth on “U.S. military security and relations with leftist regimes.” Wohlstetter’s and Iklé’s groups, it is fair to say, would gestate the most important criticism to date of the concepts of stability, the nuclear arms race, and the doctrine of assured destruction—the basic stuff of deterrence and arms control as it had developed since 1960.⁹⁰

Like everyone else, Iklé wanted to prevent a nuclear war. But by 1970 he had become convinced that traditional nuclear deterrence was a shaky and dangerous way to get prevention. As he wrote in a proposal for his working group, an entire spectrum of events and causes might lead to “a massive use of nuclear weapons, but deterrence is designed to prevent only a segment of this spectrum from occurring.” As Iklé imagined it, his group would ask about the *full* spectrum: how all the various risks linked up “with the deterrable processes leading to nuclear war,” and whether those risks in the “residual spectrum” could be mitigated. The whole idea of deterrence premised on the destruction of cities repulsed Iklé. This was a feeling with deep roots in his career. As a young sociologist, Iklé studied the effects of strategic bombing on social

that nuclear weapons, ignored after long decades of successful non-use and arms control progress—regarded as a Cold War relic—have actually been restored to importance on the international scene in recent years. They have returned for a “second act.” None of these later works gives the impression that the idea of a second nuclear age itself has a history, however, rooted in the 1970s and the era’s anxieties about the breakdown of the venerable bipolar structure of the Cold War. Nor do any of them cite James Reston’s article: James Reston, “The second nuclear age,” *New York Times* (11 May 1975): E19.

⁹⁰ David C. Elliot to Interested Faculty and Students, 17 December 1970, Subject: Southern California Arms Control and Foreign Policy Seminar, Box 31, Folder 31.14 “Arms Control and Foreign Policy Seminar 1970-71,” *MGM*.

infrastructure, an interest that grew out of his graduate work at the University of Chicago. Raised in a small Swiss town, Iklé had come to the United States after the Second World War to begin a Ph.D. The shelled cities of postwar Germany offered a fascinating urban laboratory, and Iklé traveled to Hamburg in 1949 to conduct his fieldwork. As Eric Schlosser writes, “Iklé’s dissertation attracted the attention of the RAND Corporation, and he was soon invited to join its social sciences division.”⁹¹

In fact, almost five years separated the publication of Iklé’s dissertation in 1950 and his full-time employment by RAND in 1955. In that intervening period, he worked for Columbia University’s Bureau of Applied Social Research under the direction of the sociologist Kingsley Davis. Davis was a leading figure of the structural-functionalist school of sociology, and he was carrying out research on urban social structure under a contract with the Air Force. He brought Iklé along as a member of the team. Iklé “summoned a functional-ecological model of urban life,” writes the historian Matthew Farish, “arguing that a disaster would upset qualitatively observable human relations,” and that a physically damaged city was (in Iklé’s own words) “capable of making adjustments to physical destruction much as a living organism responds to injury.” One lesson of this work was that cities were more resilient, more organically re-creative, than assumed by the doctrine of strategic bombing, which sought to break the back of a country’s willingness and wherewithal to make war by pounding its urban core into chaos and submission. By 1953, Iklé began consulting for RAND part-time on a project on “urban vulnerability.” In

⁹¹ Fred Iklé, Suggested Topic for a Working Group: “Nuclear Strategy for the Long Run: Alternatives to Deterrence?,” 11-24-70, Box 31, Folder 31.14 “Arms Control and Foreign Policy Seminar 1970-71,” *MGM*; Eric Schlosser, *Command and Control: Nuclear Weapons, the Damascus Accident, and the Illusion of Safety* (New York: The Penguin Press, 2013), 119-122, quotation on 120.

1958 he published *The Social Impact of Bomb Destruction*, the summation of his thinking about the effects of strategic bombing on urban social structure.⁹²

In 1952-53, through a proposal sponsored by Kingsley Davis and fellow Bureau faculty members Paul Lazarsfeld and Robert K. Merton, Iklé won a Rockefeller Foundation grant for a “systematic study of the relation between spatial distance and human interactions.” It was Iklé’s first experience extracting statistical features from large data sets. He and colleague Carl Hammer used records supplied by AT&T, Western Union Telegram Company, the Civil Aeronautics Administration, and the New York Port Authority concerning the numbers of calls, or trips, made by specific (anonymized) people between various cities in the United States. Iklé and Hammer had plumbed this data to figure out how the frequency of communications and travel—the “interactions”—between cities depended on the distance separating them. Then they tested the results against some of the simple models available, as well as a more sophisticated power-law model developed by Iklé. As Iklé later explained to a Foundation program officer, their work demonstrated not only a “statistically significant relationship...between the frequency of interactions, such as telephone calls or traffic, and the distance separating people” between urban centers, but additionally that such relationships could be differentiated in terms of the particular “socio-economic characteristics of the cities” in question.⁹³

When he was working at RAND in the late 1950s, he was presented with a problem that seemed ripe for a similar treatment. It was the specter of an accidental nuclear detonation. “How

⁹² Matthew Farish, *The Contours of America’s Cold War* (Minneapolis, MN: University of Minnesota Press, 2010), 211. The quotation from Iklé is drawn from Fred Charles Iklé, “The social versus the physical effects from nuclear bombing,” *The Scientific Monthly* 78, no. 3 (1954): 182-187, on 182. And Fred C. Iklé, *The Social Impact of Bomb Destruction* (Norman, OK: University of Oklahoma Press, 1958).

⁹³ “Grant in Aid to Columbia University for use by the Bureau of Applied Social Research for a study by Dr. Fred C. Iklé of the quantitative relation between distance and human interactions,” 8 April 1953; John A. Krout to Joseph Willits, 27 March 1953; Fred C. Iklé to Leland DeVinney, 10 January 1955, all in Series 200S, Box 493, Folder 4213 “Columbia University – Iklé, Fred C.,” *RF*. Iklé’s work on spatial-social dynamics culminated in an article: Carl Hammer and Fred C. Iklé, “Intercity telephone and airline traffic related to distance and the ‘propensity to interact’,” *Sociometry* 20, no. 4 (1957): 306-316.

confident can we be that there will be no accident in the future,” Iklé asked, “given the fact that we have observed zero accidents in the past?” Traditional Air Force statistical analysis had determined the probability of accidents and failures according to the frequency with which that sort of event had occurred in the past. (Such an approach is said to be “frequentist,” in the language of statisticians.) The number of past unauthorized nuclear detonations was, of course, zero; but it seemed fatuous to say that the chance of a future mishap was therefore zero. Iklé’s approach was to judge the probability of an accident based on the number of “opportunities” for an accident to occur: the more opportunities, the more likely was an accident. The past contained only a finite number of opportunities for nuclear accident, while the uncertain future might contain any number.

Iklé knew that at a certain level of confidence, one could predict the chance that no accident would occur over a given number of future opportunities, given that it had not occurred in a certain number of past opportunities. So he made up a table, with various combinations of past and future opportunities. “Given only the data on past and future opportunities and the fact that there have been zero accidents in the past, the estimate of the probability that there will be no accident in the future turns out to be as low as .091,” Iklé wrote (assuming, in this case, that there had been 1,000 past accident opportunities and as many as 10,000 in the future). The point wasn’t to say that the probability of avoiding accident *was* that small—only that it *could* be, within the logic of probability theory. In any case it was a shockingly different story than naively assuming *zero* chance of a future accident. It was Iklé’s opening move, and he proceeded in the rest of the report—almost 200 pages’ worth—to dig through Air Force and AEC data he’d

obtained with his top-secret clearance, and show that not only had there been plenty of past opportunities for nuclear accident, there were sure to be plenty more in the future.⁹⁴

It was an eye-popping study, helping to make the risk of nuclear accident a major topic in the world of strategic and defense analysis, and a major concern for the Air Force and the AEC. Iklé wasn't chiefly worried about a technical glitch (a shorted wire, a lightning strike, a downed plane) producing an unauthorized detonation; nor did he share the obsession Thomas Schelling would develop over the next couple of years with an accidental detonation sending the superpowers up the ladder of nuclear escalation. What worried Iklé were people: frail, error-prone, crazy people. He pointed out that human error had accounted for something like half of all aircraft accidents in the Air Force. Fatigue, the necessity "to entrust unspecialized personnel with complex tasks on nuclear weapons," and plain carelessness could all lead to an accident. Even more troubling was the possibility that someone at a critical point in the chain of control over a nuclear weapon might have a psychotic episode without warning. Among Iklé's recommendations was the placement of "a 'combination lock' (or similar feature) as a warhead

⁹⁴ Fred Charles Iklé, "On the Risk of an Accidental or Unauthorized Nuclear Detonation," RAND Memorandum RM-2251 (15 October 1958), quotation on 41-42. Eric Schlosser includes a discussion of this report in Schlosser, *Command and Control*, 190-195. He calls Iklé's report "the first thorough, wide-ranging, independent analysis of nuclear weapon safety in the United States." On the "Bayesian" (rather than "frequentist") character of the approach adopted by Iklé in his estimate of future accident probability, as based on prior accident opportunities, see Sharon Bertsch McGrayne, *The Theory That Would Not Die: How Bayes' Rule Cracked the Enigma Code, Hunted Down Russian Submarines, and Emerged Triumphant from Two Centuries of Controversy* (New Haven, CT: Yale University Press, 2011), 120-128. The Bayesian approach, generally speaking, involves the updating of a "prior" probability assignment in the light of new evidence. Committed frequentists object to the Bayesian approach on the grounds that it requires that one begin with an initial prior estimate of the probability—a guess—before gathering evidence and adjusting the estimate. For a hardcore frequentist, probabilities do not exist outside of frequencies; the probability that an event will occur is directly related to the frequency with which that sort of event actually occurs. Thus, there can be no talk about the probability of an event that has *never* occurred, like an unauthorized nuclear weapon detonation. Bertsch McGrayne attributes the specifically Bayesian color of Iklé's study to the RAND statistician Albert Madansky, who (along with psychiatrist Gerald Aronson) assisted Iklé with the report and contributed an appendix on much of the statistical and mathematical machinery used. And, indeed, there was nothing specifically Bayesian about Iklé's previous work on the statistics of inter-city communications and traffic, since there he was explicitly interested in the frequency of such activity, not in predicting future events or behavior. It was, however, Iklé's first experience doing statistical work; his still earlier studies of urban structure and physical disruption had been entirely descriptive in character. The nuclear accident study was in some sense the marriage of these two approaches—the statistical and the descriptive, animated by a deep anxiety about nuclear weapons and their disruptive potential.

safing device” that would completely disable the weapon until an appropriate authority with the combination could unlock it. Precisely these “locks”—called “Permissive Action Links” or “PALs” in practice—were ordered for U.S. nuclear weapons under NATO command (a decision President Kennedy would make largely at the urging of his science advisor Jerome Wiesner).⁹⁵

Iklé was a consummate wonk and, in matters nuclear, an eminently RAND-esque hardliner. But his intellectual evolution is more comprehensible in the light of his career trajectory. He was, before anything else, an urban sociologist in the structural-functionalist vein. Through his postdoctoral position at Columbia and an association with his mentor’s Air Force contract, then his job at RAND, he adapted his toolset to become an expert on strategic bombing and nuclear weapons. He had given his early career to observing and theorizing the factors that allow social structures to cohere. He had pondered the social dislocation produced by large-scale physical destruction, and the precariousness and uncertainty of future nuclear weapons safety. By the end of the 1960s, all of this had added up to an almost total loss of faith in the theory of nuclear deterrence and the criterion of assured destruction. And it was in the California Seminar on Arms Control and Foreign Policy where Iklé would let his apostasy bloom.

Iklé had absorbed a new lesson from the dustup over ABM and MIRV. He had come to believe that the fast-changing changing character of the U.S.-Soviet strategic relationship, driven by developments in technology, would inevitably imperil the thing that the arms controllers held dearest: stability. “Nuclear proliferation, the ‘arms race’”—Iklé was fond of implicitly questioning many such pious terms by putting them inside quotation marks—“and other possible future trends are seen solely as threats to a ‘stable’ state of ‘mutual’ deterrence, and this state

⁹⁵ Iklé, “On the Risk,” quotations on 16 and vii, respectively. On Permissive Action Links, see Schlosser, *Command and Control*, 265; and National Security Action Memorandum No. 160, “Permissive Action Links for Nuclear Weapons in NATO,” 6 June 1962, online courtesy the John F. Kennedy Presidential Library and Museum, available at <http://www.jfklibrary.org/Asset-Viewer/DOWYUab4b0mVeDyLvL58jQ.aspx>.

itself tends to be treated as if it were an ideal situation to be preserved.” But what if the state were at best metastable, a transient phase? Deterrence covered only a narrow band on the spectrum of factors leading to nuclear use. What guaranteed that events and technologies would remain in this band forever? In 1970, the potential causes Iklé cited for a “massive use of nuclear weapons” had a familiar ring to them, given his work in the late 1950s on the statistics of nuclear accident. “Accidents of various types, human or technical, have, of course, often been mentioned as such a cause. Our Working Group should try to address the accident problem for the long term.” Iklé was worried about “fast reacting weapon systems for lessened vulnerability”—nuclear forces that could be readied and launched quickly, either as part of a preemptive attack or in response to one (the most extreme degree of “fast reaction” was launch-on-warning). But accidents weren’t the only potential trigger of a nuclear cataclysm. The virtues of “stable deterrence” would have absolutely no purchase on a “Hitler-like top decision-maker” who operated according to a perverted logic.⁹⁶

Alternatives to deterrence were needed. Iklé didn’t have an immediate solution. In his proposal to the California Seminar, he mentioned “deterrence-cum-defense,” calling to mind the arrangement Donald Brennan had prescribed in the late 1960s, where a lid would be dropped on the numbers and yields of offensive forces while missile defense batteries would be constructed around the country. And he considered the possibility of “intra-war deterrence,” a notion that Thomas Schelling had explored years earlier, according to which deterrence could be thought of as operating *inside* a nuclear war (not just in prevention), slowing its progress, controlling its spread, and limiting the scale of its destruction.⁹⁷

⁹⁶ Fred Iklé, Suggested Topic for a Working Group: “Nuclear Strategy for the Long Run: Alternatives to Deterrence?,” 11-24-70, Box 31, Folder 31.14 “Arms Control and Foreign Policy Seminar 1970-71,” *MGM*.

⁹⁷ Fred Iklé, Suggested Topic for a Working Group: “Nuclear Strategy for the Long Run: Alternatives to Deterrence?,” 11-24-70, Box 31, Folder 31.14 “Arms Control and Foreign Policy Seminar 1970-71,” *MGM*.

The question at the front of Iklé's mind was "whether arms control agreements might foreclose possibly desirable alternatives" to deterrence. That seemed precisely the risk of the ongoing SALT negotiations. So Iklé and his California Seminar group began to puzzle their way through his questions. In February 1971 they had their first meeting, conducted in tandem with Albert Wohlstetter's working group on "the impact of changes in Soviet and U.S. strategic forces on deterrence." For a few hours a group including Henry Rowen, the RAND counterforce strategy patriarch Andrew Marshall, Fred Hoffman, and the younger RAND quants Dave McGarvey, Thomas Brown, and Amie Hoeber, met at the Times Mirror Building in downtown Los Angeles. Wohlstetter began the meeting by describing "two themes that are being stressed by some writers on arms control.... Proponents of these themes," he said, "often discuss them in a vague fashion that makes it hard to disentangle what it means and conceals various contradictions." He was referring to the notions of "crisis stability" and "arms race stability," those cornerstones of arms control theory, which George Rathjens had spelled out in his 1969 *Scientific American* article. Wohlstetter regarded the idea of the "arms race" at a cool distance. "This notion postulates a certain interaction between the United States and the USSR in the growth and/or technological evolution of their nuclear forces. The interaction is said to be deleterious...by increasing the probability and destructiveness of a nuclear war."⁹⁸

The working group dwelled on the idea of the arms race for a while, sizing it up before deciding how best to undermine it. Certain papers had to be written. "A propositional inventory of statements on the arms race," compiled by a political scientist and a natural scientist or engineer (one grad student each from UCLA and Caltech), would do the trick. They might ask Murray Gell-Mann to update his Pugwash paper on the arms race from 1964. Thomas Brown

⁹⁸ F.C. Iklé, "Memorandum on a Meeting of Working Group I," Box 31, Folder 31.4 "Arms Control and Foreign Policy Seminar 1970-71," *MGM*.

would give a paper on “models of strategic stability.” And they needed an “analysis of the technological ‘race,’ that is, the sequence of technologies that have been...introduced into the strategic forces.” There was a young historian of science at Caltech named Daniel Kevles—perhaps he would be interested in writing the paper with Raymond Orbach, a physicist at UCLA.⁹⁹

In the end, the leading products of these discussions were papers by the two figureheads, Wohlstetter and Iklé. Wohlstetter began, in the early to mid-1970s, to produce a stream of invective against the idea of the arms race, publishing his takedowns in journals and several newspapers around the country.¹⁰⁰ Iklé, meanwhile, drafted a discussion paper for the “future of deterrence” working group. Titled “Can Deterrence Last out the Century?,” in punchy prose and deeply considered doubt, it was the *summa* of his critique of deterrence, stability, and arms control. Iklé named three “far-reaching dogmas,” as he saw them: one, that nuclear forces were purely for “retaliation”; two, that such retaliation would be “swift, inflicted through a single, massive and—above all—prompt strike”; and three, that retaliation would consist in “the killing of a major fraction of the Soviet population.” But Iklé saw these as accidents of history, not eternal verities. They were after-images of the strategic bombing campaigns of the Second World War (whose horror he’d witnessed, and studied, personally in Hamburg). They were leftover

⁹⁹ F.C. Iklé, “Memorandum on a Meeting of Working Group I,” Box 31, Folder 31.4 “Arms Control and Foreign Policy Seminar 1970-71,” *MGM*.

¹⁰⁰ Wohlstetter’s California Seminar working group, and his denunciations of arms race theory, were the starting point for his revitalization of the Committee on the Present Danger in the 1970s. For details, see Cahn, *Killing Détente*, 9-16. Cahn, however, does not mention the existence of the California Seminar, or suggest its importance as a colloquium for an anti-arms control critique. For a couple of Wohlstetter’s arms race myth-busting articles, see Albert Wohlstetter, “Is there a strategic arms race?,” *Foreign Policy* 15 (1974): 3–20; Albert Wohlstetter, “Rivals but no ‘race’,” *Foreign Policy* 16 (1974): 48–81.

jitters from the days when the Strategic Air Command's overseas bases seemed perilously vulnerable, and the new ICBM had seemed tailor-made as a first-strike weapon.¹⁰¹

Two things had always troubled Iklé: the unknowable but real risk of an accident in complex nuclear weapon systems, and the absurd levels of destruction that would obtain in a real nuclear war (at least as nuclear war had been programmed by U.S. planners in the 1950s and 60s). In combination, these two things had produced an obscenity. Arms controllers' glib faith in stability and assured destruction had put the two nations on the brink. No, Iklé felt, deterrence might well not survive the century. "The very fact that well-informed and well-intentioned advisers now recommend, in essence, that the balance of terror should rest on hair-triggered doomsday machines offers a chilling reminder that we cannot rely on unswerving rationality among those who might affect critical strategic decisions," he wrote.¹⁰²

But there was a way out. "Mercifully, no inhuman power condemns us to live perpetually in the grim jail of our own ideas.... By taking advantage of modern technology, we should be able to escape the evil dilemma that the strategic forces on both sides must either be designed to kill people or else jeopardize the opponent's confidence in his deterrent." New missiles with finer accuracy "could enable both sides to avoid the killing of vast millions and yet to inflict assured destruction"—a saner, safer version of this doctrine—"on military, industrial and transportation assets—the sinews and muscles of the regime initiating war." If you want peace, prepare for war. Super-slow-reacting weapons could make it less likely that one would go off half-cocked. Iklé even recommended burying them thousands of feet below ground "with provision for reaching the surface—and their targets—weeks or months after attack." Here was Richard Garwin's "emplaced weapons" scheme flipped on its head. You would solve the

¹⁰¹ Fred Charles Iklé, "Can nuclear deterrence last out the century?," California Arms Control and Foreign Policy Seminar (January 1973), quotations on 2.

¹⁰² Iklé, "Can nuclear deterrence last out the century?," 9.

problems of vulnerability and the arms race together by keeping nuclear weapons at the bottom of a deep dark shaft—not below your enemy’s territory, where a “strike” could be carried out in seconds and never taken back, but below your own.¹⁰³

Iklé’s paper was discussed in the California Seminar in late 1972 and published as a Seminar report in January of 1973. It was a sensation, rippling out well beyond Southern California almost immediately.¹⁰⁴ He published a slightly trimmed version of the paper in *Foreign Affairs*, where it received a prominent billing. Just days later it was the subject of a favorable op-ed in the *Wall Street Journal* (where Iklé was identified not only as a member of RAND, but of the California Seminar).¹⁰⁵

Government arms control seemed ready for a fresh take on well-worn problems. Within two months of the appearance of Iklé’s article, the national media was reporting that the Arms Control and Disarmament Agency was in the midst of “an identity crisis,” apparently in the crosshairs of “a personnel shake-up and...budget cuts.” The chair’s position had been vacant for nearly four months, after the agency’s second director Gerard C. Smith had resigned at the start of Nixon’s second term. Smith had clashed repeatedly with Nixon and Kissinger during the preparations and negotiations for the SALT I accords.¹⁰⁶ The White House now indicated that it was planning to “reshape and revitalize” the agency—foreboding news for most arms control observers. Nixon named a non-ACDA diplomat from the State Department as U.S.

¹⁰³ Iklé, “Can nuclear deterrence last out the century?,” 15-16, quotations on 15.

¹⁰⁴ In Lawrence Freedman’s judgment, Iklé’s was “the most influential critique” of deterrence and mutual assured destruction. See Lawrence Freedman, *The Evolution of Nuclear Strategy, Third Edition* (New York: Palgrave MacMillan, 2003 [1981]), 333.

¹⁰⁵ Fred Charles Iklé, “Can deterrence last out the century?,” *Foreign Affairs* 51, no. 2 (1973): 267-285; “Mutual genocide?,” *Wall Street Journal* (11 January 1973): 12.

¹⁰⁶ Nixon and Kissinger often talked in private about their dissatisfaction with Smith as SALT negotiator. In January of 1972, Kissinger said to the President, “Whatever he’s got, we got for him. He hasn’t advanced it one step.” “But you don’t think we can fire him?” asked Nixon. “No. Not now,” replied Kissinger—they were too close to the SALT agreement. “That’s always the case,” fumed Nixon. “We can’t fire anybody. All right. All right.” See United States Department of State, *Foreign Relations of the United States, 1969–1976, Volume XXXII, SALT I, 1969–1972*, Document 219.

representative to the continuing SALT talks, removing negotiation from the agency's function for the first time. ACDA's General Advisory Committee, once a vigorous group with a line to the President through John J. McCloy, had been sleepy since Nixon's first term. (McCloy had difficulty getting even an occasional meeting with Nixon.) But most troubling was Nixon's proposal to reduce the organization's budget by a third, most of which would be carved out of the research program. ACDA was being gutted of its experienced lineup of experts, among them Lawrence Weiler, general counsel, and Spurgeon Keeny, director of the science and technology bureau. All of this seemed a clear response to the visible, forceful criticisms of powerful Democratic "Senator from Boeing" Henry M. "Scoop" Jackson, who had unleashed a vitriolic attack on the SALT accords as having institutionalized U.S. nuclear inferiority.¹⁰⁷

Less than a month later, Nixon contacted the man he'd chosen to direct his rebranded ACDA: Fred Iklé. He seemed just the person who could cast a cold eye on arms control's excesses. He was well known to Kissinger from Iklé's Cambridge days in the 1960s as an associate at the Harvard CfIA, his two-year stint in MIT's political science department, and his regular attendance at the Harvard-MIT arms control seminar. As a front-page story in the *Los Angeles Times* reported (profiling Iklé as a sort of local hero), he "is said to favor a strong U.S. arms arsenal. His predecessors in the job generally promoted arms reduction policies within the executive branch." He would become "the first academic arms control specialist to head the agency," pointed out the *New York Times*. "He is variously described by his past academic associates as scholarly, deliberate, proper, decent, diffident, detached and cool." Dig deeper, though, and you would discover "a brace of steel." To head ACDA's science and technology

¹⁰⁷ John W. Finney, "U.S. arms-control body faces shake-up and cuts: a state of limbo," *New York Times* (26 March 1973): 18; "Remaking the Arms Control Agency," *Washington Post* (10 April 1973): A18.

bureau, Iklé brought along his old RAND compatriot and veteran of the first incarnation of the Caltech arms control seminar, Amrom Katz.¹⁰⁸

By the middle of 1974, Nixon's administration had fired or transferred every senior official in ACDA and all but stripped the agency of its prominent role in SALT. From a total of 180 employees before the purges, there were now 130. Iklé, however, insisted that he wasn't some vandal at arms control's city walls. In his new ACDA office in Foggy Bottom, he told a journalist that he hadn't "given up a job at Rand and moved 3,000 miles to preside over the death of the agency or to see it converted into 'another Rand,' with a budget one-third the size of Rand's." Iklé suggested, implausibly, that he had unburdened himself of his own intellectual history. "I feel it would be inappropriate for me to come here as a salesman of ideas that I put together last year as an outsider."¹⁰⁹

Around the time that Iklé's *Foreign Affairs* article appeared, Albert Wohlstetter wrote with pride to William Bader at the Ford Foundation:

The California Seminar is something else again. Ciro's accomplishments have been very large. The area is very rich in intellectual resources that have been, up to now, grossly underused.... He has managed to get both experienced senior people and young faculty and students to do some actual work.... I believe these efforts have a fundamental importance, and their effect will be increasingly visible in the next few years. The contrast with the familiar study groups back East, with their few activists always the same, could hardly be much greater.¹¹⁰

¹⁰⁸ "L.A. man heads arms agency: Los Angeles man chosen to direct arms control unit," *Los Angeles Times* (6 April 1973): 1; John W. Finney, "Nominee for arms agency chief: Fred Charles Iklé," *New York Times* (7 April 1973): 12.

¹⁰⁹ Bernard Gwertzman, "New chief intent on bolstering arms-control agency," *New York Times* (11 July 1973): 14.

¹¹⁰ Albert Wohlstetter to William B. Bader, 17 January 1973, in Ford Foundation Grant 07000495, *FFG*.

It was a fair assessment. The gates of the arms control establishment were being thrown open to views very different indeed than those that made the rounds of the Eastern establishment.

III. Academic Arms Control: Ford Foundation University Arms Control Programs and the Problem of Strategic Technology

A Problem of Personnel: The Ford Foundation and Paul Doty's Harvard Arms Control Program

The conclusion of the SALT I accords in the spring of 1972 had apparently carved the doctrine of assured destruction in marble. It permitted each side huge numbers of (mostly MIRVable) strategic missiles while keeping missile defenses to a minimum.¹¹¹ Many arms controllers felt ambivalently pleased and uneasy with this achievement. A month after Nixon and Brezhnev had signed the agreement in Moscow, the *Bulletin of the Atomic Scientists* moved its “doomsday clock” cover image back to “twelve minutes to midnight” (from ten) in cautious celebration. But surely the victory was pyrrhic. The arms controllers had gotten a deal that, while restricting launchers, would still allow the number of warheads on each side to expand without restraint. Bernard Feld wrote in the *Bulletin* that the final stretch of SALT had displayed all the melodramatic traits of “a grade-B television western, including the irreconcilable differences that could be resolved only by the last-minute intervention of the two great statesmen-leaders.” As the credits rolled, he was still “torn between the impulse to cry ‘bravo’ and the desire to shout ‘fraud.’” This expert veteran of fifteen years of arms control debate wasn’t interested in the

¹¹¹ It depended upon one’s precise definition of “assured destruction,” of course. In the mid-1970s, to the critics of assured destruction-as-counter-city targeting, SALT had codified a gruesome policy according to which (as the critics often said) it was good to kill people, bad to kill missiles. But many arms controllers pointed out that SALT said nothing specific about targeting. All it had done was to enforce limits on offensive forces (with the promise of reductions), and restrict missile defenses to such an extent that each country would be permanently mindful of its vulnerability to the nuclear forces of the other. “SALT I does not confirm the doctrine that our only capability should be to throw everything we have at Russian civilians, a charge sometimes leveled at the supporters of SALT I,” Richard Garwin wrote at the end of 1972. See Richard L. Garwin, “Comments on Equality or Parity in SALT II,” 13 December 1972, Box 5, Folder 37 “AAAS, Summer Study on Arms Control in the 1970s 1972-1973,” *BTF*.

theatrics, even if “the accomplishments are very real and important.” The failures (an incomplete ABM ban, and MIRV totally untouched) had been massive. Meanwhile the outright arms control antagonists like Albert Wohlstetter, and the deterrence revisionists like Fred Iklé (no more thrilled with the dubious victory of SALT than Wohlstetter), believed that SALT showcased the arms control philosophy at its most perverse.¹¹²

Staff in Ford’s European and International Affairs (EIA) Program, taking their cue from Bundy and William Bader, began to revisit the question of Ford’s involvement in arms control. Just as the ink was drying on the SALT I agreement, staff at EIA produced an internal report on “arms control and international security.”¹¹³ At the level of “fundamental concepts,” there was a desperate need to rehabilitate the “intellectual ‘capital’ of the field,” which had languished amid all the late-1960s turmoil. The EIA staff judged that “most of the impressive conceptual work was done in the period 1952-1962”; it was even plausible “to say that the arms control community has been living off concepts that have not changed significantly in a decade.... Where this dearth of fundamental research has been most noticeable has been within the American university system,” and that was where the Foundation would try to intervene. Then there was the question of expertise itself, the “recruitment of new cadres for future arms control work.” There was an “urgent need for fresh and first class men to work in the field, particularly in the ‘hard areas’ of science, economics and systems analysis.”¹¹⁴

To help cultivate these fresh and first class men, the Foundation turned to a woman. Enid Curtis Bok Schoettle was a new hire in the EIA Program and a recently minted Ph.D. in political

¹¹² Bernard T. Feld, “Looking to SALT-II,” *Bulletin of the Atomic Scientists* 28, no. 6 (1972): 2-3. Program officer Enid C.B. Schoettle indicated the renewed discussion of arms control in “several parts of the Foundation” in “The Foundation’s Programs in Arms Control and International Security,” Ford Foundation Information Paper, December 1972, Ford Foundation Report 002003, *FFR*.

¹¹³ The less familiar term “international security” was defined in the report as “the implications of modern weapons and weapons technology for international relations.”

¹¹⁴ The Ford Foundation, European and International Affairs Program, “Arms Control and International Security: A Need for University Centers?,” October 1972, Ford Foundation Report 010890, *FFR*.

science. She hailed from a well-established Pennsylvania Quaker family. (Her great-grandfather was the publishing tycoon Cyrus H.K. Curtis; her grandmother founded Philadelphia's Curtis Institute of Music; her father was a Pennsylvania Supreme Court judge; and her half-brother, Derek Curtis Bok, had just become the president of Harvard University, in 1971.) In 1967 Schoettle had written her dissertation on U.S. space policy, built upon a study she had written a few years earlier with her advisor, Robert C. Wood, on the establishment of NASA. After a teaching stint at Bryn Mawr she had arrived in New York.¹¹⁵

Schoettle added her voice to Bader's in a second Foundation report in 1972. The document summed up EIA's arms control inquiry over the past months, and captured the sense there was something rotten in the state of arms control. There was a dangerous "complacency regarding the soundness and stability of a system of mutual deterrence," the officers said. The very language of arms control and nuclear strategy "assume the continued existence of massive nuclear arsenals." Witness, for example, a recent commentary on the SALT agreement issued by the Ford-funded Institute for Strategic Studies (ISS) in London. The ISS paper had reassured readers that without MIRV the Soviet Union could threaten "only about 2,420 [targets, or "aiming points"] with its own missiles. "Considering the fact that each of the 309 Soviet SS-9 ICBMs permitted by the interim SALT agreement can carry a single warhead with an explosive yield of 25 megatons," the Ford program officers replied, "the word 'only' hardly seems appropriate when the 2,420 'aiming points' are given a human dimension."¹¹⁶

¹¹⁵ "Enid Schoettle, foundation executive, weds H.S. Okun, ex-U.N. delegate," *New York Times* (6 January 1991); Enid Curtis Bok, "Making American Space Policy, (1) The Establishment of NASA," MIT Sloan School of Management Working Paper no. 9-63 (1963); Enid C.B. Schoettle, "Space and the Policy-Making Process" (PhD dissertation, Massachusetts Institute of Technology, 1967).

¹¹⁶ Ford Foundation, "The Foundation's Programs in Arms Control and International Security," December, 1972, Ford Foundation Report 002003, *FFR*. The Ford Foundation Records finding aid entry for this report suggests that its lone author was Enid C.B. Schoettle. However, there is no stated author on the report itself, and passages of the report were clearly drawn from earlier writing of William Bader. Thus it seems more likely that Schoettle built on top of Bader's earlier writing, or added his to her own, to produce a document collectively authored by the EIA

Ford's officers warned that arms control risked becoming a project of "the aging and gradually disappearing 'Hiroshima' generation." The raw power and "reality of 100 kilotons exploding over two Japanese cities has clearly been more meaningful to that generation than the abstract concept of 11,400 megatons on 2,400 'aiming points' has been to the new generation of social and physical scientists." And this mattered for Ford, because it indicated that arms control was a *personnel* problem, not just an intellectual or strategic one. The complacency with deterrence had resulted not only in the chilling commentary of the ISS (and similar sentiments often heard among the younger analysts), but in a "critical shortage" of committed arms control experts and fresh intellectual work. This, they judged, was "one of the most notable and regrettable facts of the 1970s." New ideas and new people: arms control needed help.¹¹⁷

In late 1972, Bader, Schoettle, and the EIA Program made their formal recommendation for "a series of programs each consisting of an interdisciplinary arms control course, a faculty seminar, and a fellowship program"—a greatly expanded version of what the California Seminar was in miniature. Rather than create a new "national center" (which seemed as unwise in 1972 as it had in 1960), Ford would infuse existing "centers of activity" with support. Four came immediately to mind: Harvard and MIT ("the most impressive group of scientists, political scientists and economists [and] the most important center of arms control activity"), Stanford, and Cornell.

At a meeting of the trustees in 1972, the custodians of the Foundation's coffers said that they needed input from outside the Foundation before they could proceed with the new arms control program. Board member Robert McNamara and president McGeorge Bundy, of course,

Program. The report was labeled "For Internal Use Only" and "Confidential," and was purely for the reference of Foundation trustees, administration, and program officers.

¹¹⁷ Ford Foundation, "The Foundation's Programs in Arms Control and International Security," December, 1972, Ford Foundation Report 002003, *FFR*.

knew just the right people to ask. Bundy got on the telephone to his old deputy as national security advisor, now director of the Institute for Advanced Study in Princeton, Carl Kaysen. Kaysen agreed to head a group of consultants that would “undertake a comprehensive re-evaluation of all aspects of the arms control and international security issue...” To assist the group Kaysen summoned Richard Garwin at IBM, and Allen Whiting, a former State Department official and an expert at the University of Michigan on China’s nuclear program.¹¹⁸ By the summer of 1973, Kaysen and his committee had submitted their study. Surely “strategic stability” would remain “a fundamental preoccupation of arms control policy,” they said. But in the new decade, stability would find itself far from its ancestral home among the superpowers, in “an increasingly multipolar world characterized by a diffusion of modern weapons technology and economic power.” Analysts and arms controllers would begin to entertain doubts, Kaysen and company wrote, “about the adequacy and utility of deterrence as the principal means of assuring strategic stability.” (They had obviously read their Iklé.)¹¹⁹

The Kaysen group’s recommendations were intellectually inclusive and unapologetically elitist. “The current generation of...arms control critics in the United States is dominated by natural scientists,” many of whom were either part of the Manhattan Project generation or just a few years younger. But clearly the “recruitment of a new group to replenish the old will have to follow a different pattern,” and Kaysen and company wanted to get more social scientists and humanists into the arms control fold. When it came to considering where, geographically, to embrace them, however, the options were decidedly narrow. Harvard was the model host for a

¹¹⁸ McGeorge Bundy to Carl Kaysen, 19 October 1972, Box 41, Folder “Ford Foundation, Day Files, 1972, K-L,” *JFK-MGB*. In addition, a recent Princeton Ph.D. in political science named Harold Feiveson would assist the group as a “one-man staff” (as Kaysen called him) and field agent, making short visits to prospective universities and think tanks around the country.

¹¹⁹ Carl Kaysen, Richard L. Garwin, Allen Whiting, and Harold Feiveson, “Recommendations for a National Program in Support of Research and Training in Arms Control and Related Subjects,” July 1973, Ford Foundation Report 002447, *FFR*.

Ford-funded “Research Center”; there was “no place in the country that can quite match Cambridge in its concentration of actual and potential arms control talent.” Stanford, meanwhile, seemed a good template for a “Teaching Center,” given the success of its undergraduate course on arms control, taught by faculty from the sciences and the humanities.¹²⁰

But a deep anxiety lay at the center of the Kaysen report, centering on the problem of expertise and community, and the structures of domestic support for arms control. By that summer, Fred Iklé’s nomination as ACDA director had cleared the Senate, and Nixon’s slashing of the ACDA research budget was about to come into effect. Much more than any shifting transnational structure of global power, arms control in the United States was battered by colliding storm-fronts of domestic politics and hostile criticisms emanating from sun-drenched Southern California. So Kaysen, Garwin, and Whiting sounded an unequivocal warning:

After ten years of evolution, and within a year after its major involvement in the Moscow SALT agreements, the U.S. Arms Control and Disarmament Agency (ACDA) has been subjected to such major personnel changes and budgetary reductions (especially in its research program) that it is not clear that the nation [can] rely on government support and direction for the production of knowledge, understanding, and informal personnel in support of its own aims in arms control.

Knowledge, understanding, and informal personnel: if the government wouldn’t support it, private money would. Where ACDA had fallen, the Ford Foundation under its dashing president McGeorge Bundy would jump into the breach. At Ford, reaction by the program officers to the

¹²⁰ Carl Kaysen, Richard L. Garwin, Allen Whiting, and Harold Feiveson, “Recommendations for a National Program in Support of Research and Training in Arms Control and Related Subjects,” July 1973, Ford Foundation Report 002447, *FFR*. Kaysen’s committee also identified a need for special “Project Centers,” each employing small clusters of faculty and grad students devoted to specific policy areas (they cited the growing power of China or India as examples). The University of Michigan or UC Berkeley seemed fit for such a task. Ford’s program officers rejected the idea of “Project Centers” as a contradiction in terms, however, since focused projects did not require all of the supporting infrastructure necessary for a “center.” And so this idea was dropped from subsequent Foundation discussion.

Kaysen report was “overwhelmingly favorable.” The trustees had recently authorized an appropriation of \$4.5 million for new arms control programs—a decision ratified by the Kaysen report—and encouraged the appropriation of even more funds at the end of the year.¹²¹

* * * *

When Bundy and Kaysen were looking for help evaluating the new Ford Foundation arms control venture, Paul Doty would have been a natural choice. He was an arms controller’s arms controller; and he had known both Bundy and Kaysen for at least twenty years, when all three had been young Harvard faculty on the make. He had participated briefly in Manhattan Project-related research, had risen high up in the Federation of the American Scientists in the 1950s, and had been an important member of the early Cambridge disarmament and arms control community.¹²² But everyone involved knew that the lion’s share of Foundation arms control money would be headed in Doty’s direction. He was already an experienced institution-builder. A highly respected biochemist and an early pioneer in the study of DNA, in 1967 he had done most of the legwork putting together Harvard’s new Department of Biochemistry and Molecular Biology, becoming its first chairman. He had been at the forefront of the early Pugwash process, and even formed his own Pugwash offshoot to encourage bilateral discussions between the U.S. and Soviet delegations.¹²³

¹²¹ They did reject the idea that university arms control centers should be distinguished according to a focus on research or teaching, however. The centers should do both, the officers decided, whether beneath the palms of Palo Alto or the maples of Cambridge. See the commentary appended to “The Kaysen Report on the Foundation and Arms Control,” part of Ford Foundation Report 002447, *FFR*.

¹²² On October 28th, 1962, the last day of the Cuban Missile Crisis, Doty jotted a note to Bundy: “In case my admiration hasn’t shown thru clearly on earlier occasions let there be no mistake this time. Congratulations on a job well done.” Paul Doty to McGeorge Bundy, 28 October 1962, Box 94, Folder “Doty, Paul,” *JFK-MGB*.

¹²³ In the mid-1960s, this group had come under the sponsorship of the American Academy of Arts and Sciences and was now called the “Committee on International Studies of Arms Control.” Because of Doty’s close ties to Henry Kissinger (old friends from Harvard, Doty began serving as Kissinger’s special advisor on arms control when Kissinger entered government in 1969), the “Doty Group” had remarkably close connections to the White House. As Richard Garwin recalled, after Kissinger had become Secretary of State, the Doty Group would regularly meet with him both before and after their meetings with their Soviet discussion partners. On one occasion, Kissinger invited Soviet ambassador Anatoly Dobrynin to meet with the group in Kissinger’s office, suddenly

Doty and Ford's program officers began formal correspondence in the autumn of 1972 about setting up an arms control unit at Harvard.¹²⁴ As William Bader told Doty that year, the Foundation's arms control interest had "practically reached the boiling stage."¹²⁵ The project would be on a different scale from anything Doty had built before. He worked his way from the ground up. He wrote to Francis Sutton, assistant to Ford Foundation vice president David E. Bell, asking for a small planning budget—mainly to staff a lone, humble office Doty had been allotted at the CfIA. From that room Doty and his hired help would begin planning, recruitment, and "cataloguing [his] own papers and collections in the arms control area"—the seeds of the new program's library. He needed office furniture, file cabinets; he wanted a travel budget so he could begin to recruit would-be program participants in person. He needed to pay some lecturers

giving Doty and his colleagues simultaneous access to high-level diplomatic officials of both sides of the Cold War. The Committee on International Studies of Arms Control was initially supported by the American Academy of Arts and Sciences. But the group eventually morphed, in the early 1980s, into the Committee on International Security and Arms Control (CISAC) under the National Academy of Sciences. See Richard L. Garwin, "The past and future of track-2 exchanges," paper for the conference "25 Years After Reykjavik: Science and Diplomacy for Nuclear Security in the 21st Century," Washington, DC, January 19, 2011, available online at the Federation of American Scientists, "The Garwin Archive – 2010-2019," <http://fas.org/rlg/2010.htm>. Also see Harvard University Department of Molecular and Cellular Biology, "A History of Biochemistry at Harvard College," <https://www.mcb.harvard.edu/mcb/p/history-of-biochemistry-at-harvard-college/>.

¹²⁴ By Doty's own account, he first raised the possibility of establishing Ford Foundation-funded arms control programs in private conversations with McGeorge Bundy in the early 1970s. As Doty summarized it in a memorandum to Joseph Nye in 1988: "In 1970 I entered an arrangement with the Aspen Institute and in 1971 organized a summer workshop on arms control at Aspen, CO. This was seemingly successful and became an annual event.... As a result of discussions with McGeorge Bundy at the early Aspen Arms Control Workshops I proposed to the Ford Foundation the setting up at Harvard of a Program for such studies as a part of the Center for International Affairs. This was generously funded for a 7-year period beginning 1973 and I became its director [on] a half time basis..." Doty here does not discuss the extensive involvement of Ford in funding other arms control studies and seminars (in California, e.g.). See Paul Doty to Joe Nye, Re: Personal items outside of Biochemistry that may be of interest, 15 October 1988, Box 35 (33), Folder 1 "Writing project," Paul M. Doty Personal Archive, 1940–2011 (Accession 18511), Harvard University Archives, Cambridge, MA. My thanks to archivists at the Harvard University Archives for allowing me to examine this collection while it was still being processed. (The box number in parentheses indicates where the document can be located in the final, processed collection; the "old" box number is listed first.)

¹²⁵ Letter to Paul Doty, 15 December 1972 (no sender indicated, but it was sent from Paris, therefore almost certainly by Bader), Ford Grant 07300204, *FFG*.

to substitute-teach one of his courses, allowing him to spend more time planning the arms control program. (Ford obliged with a planning grant of \$33,000.)¹²⁶

It was small potatoes in the beginning, but Doty's vision was large. By October of 1972, he had drafted an initial proposal for the Foundation, sending a slightly revised copy personally to Bundy in November. His plan was to frame Harvard's arms control effort "within a larger unit that embraces the international role of science and technology," The problem of nuclear weapons was, at bottom, a problem of the relationship between science, technology, and society. "Our military establishment, like our society itself and the world at large," he wrote, "has been to a great extent shaped by the technological developments of the last quarter-century." The U.S. needed not just more arms controllers, but more arms control advocates who were on speaking terms with science and technology. By Doty's count, "there are less than twenty persons in this category outside of government employ and almost all are over forty-five," he noted. Harvard's program would train "a new breed of multidisciplinary experts."¹²⁷

Doty called his creation the "Center on Science, Technology and World Order." Ford's officers were nervous about this vague title. Where did arms control reside under such a wide

¹²⁶ Paul Doty to Francis X. Sutton, 30 January 1973, and David E. Bell to McGeorge Bundy, Request No. ID-1604, 13 March 1973, in Ford Foundation Grant 07300204, *FFG*. David Bell had been director of the Office of Management and Budget under John F. Kennedy, and director of the U.S. Agency for International Development beginning in 1962. He moved into the vice president's office at the Ford Foundation in 1966, almost at the same time Bundy became president.

¹²⁷ Paul Doty to McGeorge Bundy, 21 November 1972; "First Draft of a Proposal for a Center on Science, Technology and World Order," 23 October 1972, part of Ford Grant 07300204, *FFG*. I have also drawn from a slightly edited version of a few months later: "A Proposal to The Ford Foundation for the support of a center for Arms Control and Disarmament Studies within a Program for Science and International Affairs at Harvard University," 5 March 1973, Ford Grant 07300204, *FFG*. Doty had to concede the Foundation's officers' suggestion that after a brief period of intellectual ferment—the "early wave," he called it—the wave had retreated, leaving behind the rusted hulks of overused arms control ideas. But there was no complacency or laziness involved, as Bader (channeling Wohlstetter) had argued. Doty said it had been the Soviets' fault, slow in their strategic education under American tutelage, long in their building "symmetric forces" to reach parity with the U.S., much delayed in their willingness to entertain bilateral arms control negotiations. "The game could not be played with less than two players." And now, in the United States, "the more active community studying these problems outside the government" was dwindling, getting old, doing their work "on a part time, and often bootleg basis." Pay bootleg prices and you get bootleg product. Cambridge was just the place for a fresh investment in quality arms control expertise, an "expanding array of competence in the analyses and initiatives that would facilitate major reductions" in weapons and arms spending.

roof? Doty, in response, recalibrated slightly. A few months later he pitched what he now called a “Program for Science and International Affairs” (PSIA). It would function as an “autonomous unit” within some larger administrative structure at Harvard, such as the CfIA or the newly renamed Kennedy School of Government. (The similarity between this arrangement and the position of ACDA as an “autonomous unit” of the State Department would not have been lost on Doty—and he may well have had it in mind.) Within the PSIA, a vanguard group would focus on “Arms Control and Disarmament Studies,” the first research effort of the new program; other units would follow in time, exploring a remarkably broad range of possible topics, from environmental issues to international development.

The Ford people were still uneasy about the idea of placing, Russian doll-like, the arms control function inside a “science and international affairs” program inside an existing Harvard center. It would be too easy for Harvard to use the money as a private account. As program officer Craufurd Goodwin explained to David Bell after reading a second draft by Doty, “One’s assessment of the seriousness of this ambiguity must rest on one’s acquaintance with Doty and on interpretation of Harvard faculty politics.” Goodwin guessed that Doty was “under pressure from a variety of directions within Harvard to make this proposal so vague that at a later date a lot of people can be brought legitimately into the Doty tent without our being able to raise any objections.”¹²⁸ There was some kind of sly plot afoot, surely. Doty had already suggested that he’d bring in Harvey Brooks—iconic dean of the Division of Engineering and Applied Sciences, a noted expert on the public policy of science, and head (with Don K. Price) of Harvard’s embryonic program in science, technology, and society—as an associate director of the PSIA. But wasn’t this just a way to use Ford’s arms control money to prop up a completely unrelated

¹²⁸ Craufurd D. Goodwin to David E. Bell, 22 March 1973, Subject: Latest (Revised March 15, 1973) Proposal for Arms Control and Disarmament Studies – Paul Doty, Ford Foundation Grant 07300204, *FFG*.

effort? It smacked of that familiar “Harvard deftness at repackaging old wine to respond to their perception of the Foundation’s current interests...” William Bader agreed, asking Goodwin, “How do we know that in two or three years that we will not be sponsoring a program of science and technology at the expense of an Arms Control and International Security Program?”¹²⁹

Negotiations continued in the months ahead. Harvard’s and Ford’s accountants haggled over the details of the budget—the faculty salaries, the overhead, the length of a grant. (Doty wanted a seven-year guarantee, the Ford officers thought that three years was about right for starters.) A planning committee was assembled—many of the old Charles River Gang from both MIT and Harvard, plus a few affiliates like Richard Garwin and Spurgeon Keeny (who had joined the MITRE Corporation after getting booted from ACDA). It had become clear that Brooks would be unacceptable as an associate director; a younger person, free of Harvard baggage, was needed. In due time Albert Carnesale, a nuclear engineer who had spent three years as chief of ACDA’s defensive weapons systems division and was a member of the U.S. delegation to SALT, got the call. He was just the right man in Doty’s eyes: he had a technical background and had earned his stripes in government (not to mention an international arms control negotiation). He’d escaped ACDA just ahead of Nixon’s purges.¹³⁰

By mid-August, Ford’s officers had decided that Harvard would get \$1.2 million of the total \$4.5 million appropriation, more than twice the amount of the next-largest arms control

¹²⁹ “Finally,” Goodwin continued his delightfully frank letter, “there is the question of whether ‘science’ is the correct central focus for the Harvard Center. My general reaction is that it is not.” Surely MIT was better able to handle the distinctly scientific aspects of arms control. “Harvard’s glory, it seems to me, should be in its social sciences,” and Doty had utterly neglected them in his proposal. See Craufurd D. Goodwin to David E. Bell, 22 March 1973, Subject: Latest (Revised March 15, 1973) Proposal for Arms Control and Disarmament Studies – Paul Doty; and William B. Bader to Craufurd D. Goodwin, 23 March 1973, Subject: Harvard Arms Control Proposal – Doty,” both part of Ford Grant 07300204, *FFG*.

¹³⁰ Harold Brown, who had also been one of Carnesale’s superiors on the SALT team, recommended him as “technically very well informed, and a doer.” “Center for International Affairs, Arms Control Program, Planning Meeting, Tuesday, June 12, 1973, Attendance List,” Box 3, Folder 26 “Harvard Univ., Center of Intl Affairs - PSIA 1973,” *MIT-CIS*; Harold Brown to Paul Doty, 17 May 1974, part of Ford Foundation Grant 07300204, *FFG*.

grant. Ford would award four such grants in 1973 for the creation of university arms control programs: at Harvard under Doty; at MIT under Jack Ruina; at Cornell under George Quester and Franklin Long; and at Stanford under Wolfgang Panofsky.¹³¹

And so Paul Doty's PSIA was christened in the autumn of 1973. Initially dispersed in offices at multiple addresses around the campus, the program was soon consolidated in a single location in the old Coolidge Laboratory, a four-story red brick structure on Divinity Avenue, just down the street from the main CfIA building. A very modest total of fourteen staff made up the program in its first year, as Doty continued the recruitment effort and built his corps of experts. The Harvard-MIT arms control seminar was rehabilitated, having fallen into during the previous two years, and Doty's new program assumed the work of administering it.¹³² In that first year, the seminar heard the new ACDA chief Fred Iklé discuss "deterrence and arms control in a world of uncertainty"; Herbert York talked about "some alternatives to deterrence"; the former ACDA director and lead SALT negotiator Gerard C. Smith considered the meaning of arms control post-SALT, followed by a very different interpretation delivered by Senator Jackson's aide, Richard Perle; and the former Los Alamos weapons designer Ted Taylor lectured on the "theft of nuclear material." (Taylor, recently retired from his government job, was doing this work on a grant he'd received, appropriately enough, from the Ford Foundation in 1972).¹³³

¹³¹ David E. Bell to McGeorge Bundy, Request No. ID-1756, 13 August 1973, part of Ford Foundation Grant 07300204, *FFG*; Ford Foundation Board of Trustees, "Summary of Discussion on Foundation and Arms Control Studies," December 1973, Ford Foundation Report 010926, *FFR*. At another board meeting at the end of the year, the Foundation set aside an additional \$3 million for academic arms control. The total sum of \$7.5 million was still only about a third of what the Kaysen report had recommended. As program officers' commentary on the report suggested, the staff had "been influenced both by a sense of budgetary stringency and by [skepticism] that the sums suggested by the Kaysen committee could be matched over the next five years with first-quality intellectual resources."

¹³² On the PSIA's early activities, see "Program for Science and International Affairs, 1973-1974, First Annual Report," part of Ford Foundation Grant 07300204, *FFG*.

¹³³ Taylor's work on safeguarding nuclear materials, and his unusual personality and remarkable career at Los Alamos, were made famous by John McPhee's deep-cover profile of Taylor in *The Curve of Binding Energy: A Journey Into the Awesome and Alarming World of Theodore B. Taylor* (New York: Farrar, Straus and Giroux, 1974 [1973]), originally published serially in *The New Yorker* magazine beginning in December 1973. McPhee followed

In planning the program's early research, Doty took cues from a recent summer study on arms control—a scaled-down reprisal of the 1960 original—held in 1972. It had been managed (like the first one) by the American Academy of Arts and Sciences, held over two weeks in Aspen, Colorado. Attended by a handful of the elder statesmen, it also featured a few newer faces. Among these were Barry Carter, who had worked on Kissinger's NSC staff and was now a Washington lawyer, a young Harvard political scientist named John Steinbruner, and Anne Cahn, a research associate at the MIT Center for International Studies. (Steinbruner and Cahn would both soon join Doty's PSIA.) Scrounging for money, the study director Franklin Long looked in a familiar direction, appealing directly to McGeorge Bundy. Thomas Schelling also wrote to Bundy personally to ask for Ford's support. "I have not been thrilled at the prospect of what a summer study might do, beyond rediscovering things that were discovered, or perhaps merely rediscovered, at the rather eventful summer study on the same subject in 1960," he said, characteristically wry. Still, as preparation for Doty's "big, expensive project," the summer study "seemed a good way to canvass what, if anything, is going on or can get started," and "whether we can't begin to get the universities and the government...back on something like speaking terms." The appeal apparently worked, and the Ford Foundation came through with a grant.¹³⁴

The 1972 summer study had an air of faded hope about it; there was none of the hopeful possibility that had animated Bernard Feld's 1960 event. George Rathjens said that the arms

Taylor as he made site visits to various nuclear processing and transportation facilities—all work performed under the Ford grant. McPhee mentions Taylor's Ford Foundation study on 125.

¹³⁴ The two-week Aspen summer study was actually preceded by two weekend-long planning studies, one at the Academy headquarters in Boston and the other at the Wingspread Retreat in Racine, Wisconsin. The latter meeting was paid by the Johnson Foundation, which had sponsored the original *Daedalus* meeting in the spring of 1960. See "Planning Session for the Summer Study on Arms Control in the 1970's, December 15-16, 1972," and "Study on New Directions in Arms Control, convened by American Academy of Arts and Sciences, in cooperation with The Johnson Foundation, March 30-31, 1973," Box 5, Folder 37 "AAAS, Summer Study on Arms Control in the 1970s 1972-1973," *BTF*. Also see F.A. Long to McGeorge Bundy, 3 November 1972, Box 5, Folder 37 "AAAS, Summer Study on Arms Control in the 1970s 1972-1973," *BTF*. And see T.C. Schelling to McGeorge Bundy, 31 January 1973, in Ford Foundation Grant 07300204, *FFG*.

control consensus of the 1960s had been “shattered”; when he referred to the rising concern in the arms control community about the dangers of proliferation, he even put the word “community” inside quotation marks.¹³⁵ But the meeting did reveal what was troubling the Charles River Gang and their friends in the muted afterglow of SALT I. Many expressed the second-nuclear-age anxieties typical of the period. SALT had been “a convenient mark for the end of the Cold War” but the beginning of an even more dangerous era, wrote Abram Chayes. Franklin Long wrote about how “the number and complexity of nuclear weapons continue to rise, as does the number of countries that have deployed them. For how long will [the world] be able to keep the nuclear scorpion in the bottle?” he asked, putting a 1970s spin on Oppenheimer’s vintage metaphor.¹³⁶

What really bothered virtually everyone was technology. A raft of unencumbered technological developments promised no end of difficulty in the 1970s: nuclear-tipped cruise missiles, “smart bombs” that were guided to their targets after being dropped, new supersonic bomber aircraft. The U.S. was on the cusp of a “qualitative arms race,” said Long, a race no longer in brute numbers of weapons but in their technological sophistication and their pernicious effects on the stability of deterrence. “The 1972 SALT I interim agreement led to a ‘freeze’ in numbers of superpower ICBMs. But the qualitative race goes on and has perhaps even been accelerated by the restriction in numbers,” he wrote. The irony that SALT had produced an

¹³⁵ G.W. Rathjens, “Changing perspectives on arms control,” *Daedalus* 104, no. 3 (1975): 201-214, quoted terms on 203 and 204, respectively.

¹³⁶ Oppenheimer had likened the U.S. and the Soviet Union to scorpions trapped together in a closed space, each under constant threat of the other’s sting. Long had made nuclear weapons themselves the scorpion, and efforts at arms control and nonproliferation the bottle containing their danger. See F.A. Long, “Arms control from the perspective of the nineteen-seventies,” *Daedalus* 104, no. 3 (1975): 1-13, on 11; and Abram Chayes, “Nuclear arms control after the Cold War,” *Daedalus* 104, no. 3 (1975): 15-33, on 15. Many of the 1972 summer studiers also hoped that the new “bureaucratic politics” approach, espoused by the political scientist Graham Allison at Harvard, would shed light on the government’s internecine, opaque decision-making processes in foreign policy and defense procurement. The recently published article by Allison and Morton Halperin spelling out some of the approach’s features was even required reading at the Wisconsin planning meeting. See Graham T. Allison and Morton H. Halperin, “Bureaucratic politics: A paradigm and some policy implications,” *World Politics* 24 (1972): 40-79.

acceleration of the arms race along even more dangerous channels, rather than its cessation, escaped no one. Harvey Brooks listed some of the developments of the 1960s that had been particularly nefarious. To ABM and MIRV—the major destabilizing technological systems that arms controllers had gotten upset over in the late 1960s—he added a third: improvements in missile guidance technology.¹³⁷

The *Daedalus* special issue dedicated to the summer study didn't actually appear until 1975. That was almost a year after another tepid agreement, the Vladivostok Accord, signed by Gerald Ford and Leonid Brezhnev and billed as the platform for a future SALT II treaty. Like SALT I, Vladivostok put ceilings on numbers of weapons (including numbers of MIRVed missiles) but made no serious attempt to limit qualitative weapons developments. In Brooks's contribution to the *Daedalus* issue, he wrote that for the first time in the Cold War, arms control now demanded limiting research and development—restricting science and engineering—and not just limiting deployments. “It seems only a matter of time before increases in the accuracy with which re-entry vehicles can be aimed or guided will make fixed land-based missiles vulnerable to attack,” he wrote. “Whether such attack would actually be a serious threat would depend upon the vulnerability of the other components of the...retaliatory force, especially the sea-based deterrent”—the missile-launching submarines. Brooks had named two of the hallmark strategic arms control issues of the years ahead.¹³⁸

¹³⁷ Harvey Brooks, “Memo on Arms Control Priorities,” 4 December 1972, Box 5, Folder 37 “AAAS, Summer Study on Arms Control in the 1970s 1972-1973,” *BTF*. Similarly Richard Garwin, in a paper shared with the group, argued that the ongoing SALT II negotiations should inspire “a general undertaking not to threaten the survival of the strategic offensive force on the other side,” a gesture toward the threat of missile accuracy. Richard L. Garwin, “Comments on Equality or Parity in SALT II,” 13 December 1972, in Box 5, Folder 37 “AAAS, Summer Study on Arms Control in the 1970s 1972-1973,” *BTF*. Also see Thomas C. Schelling and Morton H. Halperin, *Strategy and Arms Control* (New York: The Twentieth Century Fund, 1961), 37.

¹³⁸ “SALT Two, Joint Draft Text of 13 November 1975,” National Security Adviser, Presidential Subject File, published online by the Gerald R. Ford Presidential Library and Museum, The Vladivostok Summit Meeting on Arms Control, Master List of Digitized Documents, available online at

The Prisoners of Technology

Worries about the impact of technology on the “strategic balance” suffused the history of the arms control community, tying together perspectives as diverse as Bernard Feld’s and Thomas Schelling’s. Back in 1960, when Ernest Lefever had embarked on his survey of the arms control landscape for the Ford Foundation, he had framed arms control as a means of restraining technology run amok. “In an arms race there is an almost autonomous element in technology which pushes toward a perfect and efficient and invulnerable weapons system with little regard for the provocative effect of that system upon the enemy,” he wrote, “and its impact upon the delicate military balance.” From his vantage point at the cusp of the missile age, “the chief cause of military instability is technological in character...” Paul Doty, more than a decade later, had written to the Ford Foundation that the pace of new technological developments “seriously out-matches the ability to analyze their need ... [so that] research, development, testing and even deployment race ahead of the prudent examination of need....” Arms controllers found it natural to think, with McGeorge Bundy, that the United States had become the “prisoner of its technology.” The ABM and MIRV debates had imprinted the lesson of technology run amok even deeper. And dangerous new technologies kept coming, apparently without any sign of stopping.¹³⁹

During the Second World War Harvey Brooks had worked on anti-submarine warfare (ASW) in the Harvard Underwater Sound Laboratory, even contributing to the design of a sub-

<http://www.fordlibrarymuseum.gov/library/exhibits/vladivostok/vlist.asp>; Harvey Brooks, “The military innovation system and the qualitative arms race,” *Daedalus* 104, no. 3 (1975): 75-97, quotation on 80.

¹³⁹ Ernest W. Lefever, “Arms Control and the Security Dilemma: A Proposal for a Research Project,” Draft: 15 July 1960, Box 37, Folder 388 “Subject: Arms Control, Correspondence: Ernest Lefever,” *JES*; Paul Doty, “A Proposal to The Ford Foundation for the support of a center for Arms Control and Disarmament Studies within a Program for Science and International Affairs at Harvard University,” 5 March 1973, Ford Grant 07300204, *FFG*.

killing torpedo. But in the 1970s he and his fellow arms controllers learned most of what they knew about the current state of ASW, and the problem of missile accuracy, from their colleagues at MIT. A small group there had been organizing studies of new strategic technologies for a few years already. As ever, Bernard Feld was concertmaster and chief motivator.¹⁴⁰

At MIT 1969 was a tumultuous year, marked by anti-war protests, strained faculty-student relations, and anguished disputes over the significance of the military's presence on campus. Feld, a disarmament campaigner but still, unmistakably, an establishment liberal, was pulled in both directions. As he steered a delicate course through the controversy, his main concern was protecting and promoting arms control work at the Institute. That autumn he seized an opportunity that had presented itself amid the shifting mood on campus. A few faculty members had promoted a new educational initiative to teach courses "concerning outstanding questions of the relation of science and technology with social problems."¹⁴¹ Feld thought arms control was a fine candidate for this venture, and successfully pitched an undergraduate arms control seminar. He taught about twenty students that fall, leading them through a brief history of the arms race, and commissioning short study papers from each of them on some current arms control issue.¹⁴² In the run-up to the course, he'd conscripted several of his colleagues to assign

¹⁴⁰ Lewis M. Branscomb, "Harvey Brooks," *Proceedings of the American Philosophical Society* 154, no. 4 (December 2010): 461-469.

¹⁴¹ H.W. Kendall and K. Gottfried, "Science and Society: An Interdepartmental Educational Program," Draft #2, 3-10-69, Box 28, Folder 271 "Physics 8.19, Undergraduate Policy Seminars, Arms Control Seminar, 1969-1970," *BTF*.

¹⁴² One of his teaching assistants was Richard Smoke, who had been "emphatically" advised by MIT's draft counselors to delay graduation by a year to avoid a tour in Vietnam. Smoke was put in touch with Feld by MIT political scientist and longtime arms controller Lincoln Bloomfield, who jotted a note telling Feld that Smoke had taken a couple of his courses, and was "fluent, quite sure of himself, and moderately bright." In 1974 Smoke would coauthor (with Alexander L. George) the book *Deterrence in American Foreign Policy: Theory and Practice* (New York: Columbia University Press, 1974), earning the authors the 1975 Bancroft Prize in American history. One of the students taking the class was Ted Greenwood, a physics graduate student who was in the midst of transferring to the MIT political science department to work on arms control topics under George Rathjens. Greenwood would end up writing his dissertation on the MIRV procurement process, and shortly move on to work as a research associate in Paul Doty's PSIA at Harvard. See Richard Smoke to Lincoln Bloomfield, 1 May 1969; Lincoln Bloomfield to

readings and lead discussion sessions: George Rathjens and Leo Sartori on “nuclear delivery systems,” Vigdor Teplitz and Steven Weinberg on “nuclear weapons and their effects,” Jerome Wiesner and Louis Sohn on “inspection problems,” Jack Ruina on “new technology and the strategic arms balance.” It was quite an opportunity for the students. Most of these seminar-leaders had much more than an “academic” interest in arms control, jetting down to Washington almost weekly to give testimony, cajole Congressmen, or pop in at Henry Kissinger’s office.¹⁴³

Feld had even been talking about giving arms control a permanent, standalone institutional home at MIT. He put together a proposal for an interdepartmental “Center of Disarmament and Arms Control Technology” at MIT. Amid the campus furor over the Institute’s DOD-funded laboratories, Feld pictured the new Center as a corrective, a rebalancing of MIT’s federally funded, “mission oriented” activities away from weapons design and toward weapons control. It would only perform non-secret research on non-governmental contracts, and what federal money it did accept would come from ACDA. It would make everybody happy—student protesters and action-intellectual faculty alike.¹⁴⁴

The proposal for a center was about two years too early for the Ford Foundation’s big center-building push. So Feld decided to propose arms control case studies instead, done by small groups of campus experts on a part-time basis. He asked Wiesner (MIT’s provost) whether the Institute might cough up money to sponsor an “Arms Control and Disarmament Study Group.” Wiesner was happy to oblige, siphoning funds initially from a recent grant given to MIT

Bernard Feld, n.d.; and B.T. Feld to Robert Schaeffer, 18 January 1971, all in Box 20, Folder 194 “Arms Control Studies, 1969-1973 (1 of 2),” *BTF*.

¹⁴³ Other undergraduate policy seminars offered that season touched on such topics as “world population and food problems,” “environment pollution problems,” and “urban problems.” Raymond L. Bisplinghoff and Victor F. Weisskopf to Jerome B. Wiesner, 8 April 1969, “8.19 – Arms Control Seminar Schedule 1969-70,” both in Box 28, Folder 271 “Physics 8.19, Undergraduate Policy Seminars, Arms Control Seminar, 1969-1970,” *BTF*.

¹⁴⁴ “Proposal for a Center of Disarmament and Arms Control Technology,” 23 May 1969; Leo Sartori to J.B. Wiesner, 23 May 1969; Lincoln P. Bloomfield to Bernard T. Feld, 6 August 1969; handwritten note from George [Rathjens] to Bernie [Feld], n.d., all in Box 20, Folder 195 “Arms Control Studies, 1969-1973 (2 of 2),” *BTF*.

by IBM on the theme of “technology and society.” Feld’s study group began meeting in the autumn of 1969.¹⁴⁵ Soon enough, Feld had caught wind of William Bader’s arms control fact-finding tour for the Ford Foundation and, not one to miss an opportunity, got in touch. “Can I come see you sometime before Christmas to discuss possible Ford Foundation interest in this program?” Feld implored. A few months later, when Feld had submitted a proposal, he complained to Wiesner that Bader was stalling. (Bader, who was by then developing a friendship with Albert Wohlstetter, seemed more interested in setting up “regional” arms control groups—and regional meant California, not Cambridge.) “Can you help us out here?” Feld asked Wiesner. In short order MIT had received a one-year grant for arms control studies from Ford, with the promise of future investment.¹⁴⁶

¹⁴⁵ B.T. Feld to J.B. Wiesner, “Proposed Arms Control and Disarmament Studies at MIT,” 10 October 1969, Box 20, Folder 195 “Arms Control Studies, 1969-1973 (2 of 2),” *BTF*.

¹⁴⁶ Bernard T. Feld to William Bader, 9 December 1969, Box 20, Folder 195 “Arms Control Studies, 1969-1973 (2 of 2),” *BTF*; B.T. Feld to J.B. Wiesner, “Summer program on Arms Control,” 30 April 1970; B.T. Feld to J.B. Wiesner, “Ford Foundation – International Grant for Arms Control Study,” 13 May 1971, Box 20, Folder 194 “Arms Control Studies, 1969-1973 (1 of 2),” *BTF*. One of Feld’s study group’s first significant projects was to consider the effects of a hypothetical nuclear detonation in Massachusetts. The scenario involved the United States launching a counterforce strike against the Soviet Union, for which the Soviets would retaliate by hitting American cities. A one-megaton blast was imagined above Boston—“the epicenter of the blast is over MIT (no chauvinism intended)”—at an altitude of 10,000 feet to maximize blast damage. After a brief survey of the physical effects, which were easy enough to calculate, the group was soon drawn into an Iklé-esque investigation of the social impact of the detonation. Arthur Katz, a research fellow in physics, requested a thick file of reports from the Army’s office of “National Entity Survival Studies,” as well as reports from the National Center for Disease Control on the spread of disease following a nuclear attack. “Does Boston have a vital function for the survival of the population of Massachusetts and New England under these circumstances?” the group asked. They figured the food supply would be undisturbed, as would the power grid, whose power stations were distributed away from the metropolitan core. This small group of faculty, which included the future Nobel Prize-winning physicists Steven Weinberg and Henry Kendall, was moved to contemplate “the impact of disease created by potentially hazardous conditions of decaying bodies and ruptured sewage lines and rotting food.” But they *were* physicists, after all: “Though the question would be explored further, it was assumed with some confidence that this problem could be contained in the stricken area. The vectors of disease for various potential problems appeared not to be inordinately difficult to deal with.” A psychiatrist from the Yale Medical School named Robert J. Lifton visited a subsequent meeting of the group to talk about the long-term psychological effects of nuclear weapons devastation. For his recent book, *Death in Life: Survivors of Hiroshima* (New York: Random House, 1968), Lifton had conducted extensive interviews with members of the only population to have experienced an actual nuclear attack. According to Feld’s own notes on Lifton’s book, it suggested “the overpowering force of the imagery and reality of nuclear war. Unlike other destructive situations, the totality of the experience—the complete immersion, ‘death immersion’, of the individual—is so strong that it strips away normal psychological protection.” Lifton would rise to greatest prominence a decade later as part of the peace movement of the early 1980s. His especially visceral, emotional, psychologically inflected approach to nuclear weapons and war would prove to be a good fit for the antinuclear activism of that later period. Arthur Katz would also hold on to his interest in matters post-apocalyptic, publishing

For the second meeting of Feld's MIT arms control study group in the fall of 1969, Feld had corralled a most interesting speaker: John Craven, lead designer of the Polaris submarine-launched ballistic missile (SLBM) system. Craven was on leave from his position as chief scientist of the Navy's Special Projects Office, the office in charge of the Navy's nuclear missile program. He was visiting the political science department for the academic term, but he'd spent a lot of time at MIT over the years. Charles Draper's MIT Instrumentation Laboratory had designed Polaris's inertial guidance technology, and Craven had routinely interacted with the lab. He and his family were living in the guesthouse of Draper's Victorian mansion during their year in Cambridge. He'd even shown up once at the Harvard-MIT arms control seminar way back in 1962, to give a paper on "the design of weapon systems for an arms control environment." He'd told the seminar that "subtle characteristics of weapons systems can open or close off important possibilities for national policy." The Polaris submarine-launched missile, for example, "could be a counter-force weapon" if designers were allowed to push it in that direction. But would that be responsible from an arms control perspective? Arms control had "not been relevant to weapons design" in the past, "but could be." Now, in November of 1969, Craven gave Feld's group a briefing on "the Polaris deterrent and [anti-submarine warfare]." ¹⁴⁷

Life After Nuclear War: The Economic and Social Impacts of Nuclear Attacks on the United States (Pensacola, FL: Ballinger Publishing, 1982). See "Arms Control Group, General Meeting No. 1, February 16, 1971"; and "Discussion with Robert Lifton," both in Box 6, Folder 46 "Arms Control 1971-1973, 2/2"; Walmer E. Strope to Arthur Katz, 26 May 1971; and Arthur Katz to David Spencer, 17 March 1971, both in Box 20, Folder 194 "Arms Control Studies, 1969-1973 (1 of 2)," *BTF*. On Lifton's later career and his "psychological critique of the arms race," see, e.g., Hugh Gusterson, *Nuclear Rites: A Weapons Laboratory at the End of the Cold War* (Berkeley, CA: University of California Press, 1996), 10-13.

¹⁴⁷ Craven remembered his year at MIT in 1969-70 as productive but filled with enormous turmoil. "In the same year the Instrumentation Laboratory was in fact divested of its defense contracts and reconstituted as the Draper Laboratory. The political science department was bombed; no one was hurt. I was picketed and my children were ostracized at the Newton public schools, but I still accepted every invitation to discuss the issues and defend deterrence and the Draper Lab's crucial role in deterrence." At the end of the year Craven's faculty colleagues presented him with a plaque bearing an inscription taken from the paper of a local chapter of Students for a Democratic Society: "Although Professor Craven wins all his arguments with fantastic logic, he is obviously out of his mind." See John Piña Craven, *The Silent War: The Cold War Battle Beneath the Sea* (New York: Simon & Schuster, 2001), 239-250. Also see Bernard Feld, untitled memorandum, 17 November 1969, Box 20, Folder 195

Like many arms controllers troubled by ABM and MIRV, Feld had become convinced that deterrence would depend more and more on the submarine-launched nuclear force. With the possibility of ABM and MIRV posing a menacing counterforce threat, the land-based missiles and bombers seemed increasingly at risk. Since the late 1950s and early 1960s the SLBMs had been regarded as a key ingredient of stable deterrence, a critical supplement to the Minutemen ICBMs, which most regarded as the heart of the deterrent (Jerome Wiesner, for example, often said this). But now, with a MIRVed attacking force threatening the silo-based ICBMs, the SLBMs suddenly assumed heightened importance. As a product of the ABM debate and the fear about MIRV, by 1970, when arms controllers thought of a secure deterrent, increasingly they imagined it in the oceans, not in underground silos. The idea was that even if each side wiped out the other's land-based missiles and bombers in a dramatic opening strike, the submarines could never be found beneath the seas, whose sheer vastness and impenetrability to radar made submarine detection an improbably difficult task. The sub-launched missiles, therefore, were "invulnerable," and could be trusted to deliver a retaliatory strike whenever and wherever needed. As a bonus for arms controllers, because a submerged craft was incommunicado by radio and couldn't take a star sighting to get its bearings, it couldn't figure out its own geographic location with much precision. For chiefly that reason, the SLBMs were also believed incapable of precision targeting, making them purely counter-city weapons—ideal for the limited deterrence mission of assured destruction.¹⁴⁸

"Arms Control Studies, 1969-1973 (2 of 2)," *BTF*; Harvey M. Sapolsky, *The Polaris System Development: Bureaucratic and Programmatic Success in Government* (Cambridge, MA: Harvard University Press, 1972), 71-72. For Craven's participation in the Harvard-MIT arms control seminar, see "Joint Arms Control Seminar, Minutes of the First Session, October 8, 1962," Box 3, Folder 3 "Joint Harvard-MIT Arms Control Seminar 1962-63 Minutes," *LPB*. Craven began writing publicly about the specific arms control implications of the submarine-launched force in the mid-1960s. See, for example, John P. Craven, "Ocean Technology and Submarine Warfare," *Implications of Military Technology into the 1970s*, Adelphi Paper No. 46 (London: Institute for Strategic Studies, 1968): 38-46.

¹⁴⁸ Graham Spinardi, *From Polaris to Trident: The Development of US Fleet Ballistic Missile Technology* (New York: Cambridge University Press, 1994), 1-8. As Donald MacKenzie writes, "Submarine-launched missiles

New developments in anti-submarine warfare were therefore dangerous from the viewpoint of deterrence and arms control. If the subs could be found, they could be destroyed. If they could be destroyed, their second-strike potential was hardly “invulnerable.” “The importance of these problems,” Feld wrote to William Bader in 1971, had “emerged most clearly out of the considerations at the International Pugwash Symposium” on technology and the arms race that Feld had organized in June of 1970. In fact Feld had wanted to hold a study on the impact of current technology on strategy and arms control since 1968, back when everyone had ABM and MIRV on the brain. His negotiations with the Carnegie Endowment for International Peace fell through at the time; but Ford cut a check for a conference in 1970.¹⁴⁹ Feld, through his contact with John Craven, had managed to get an MIT Instrumentation Laboratory engineer named David Hoag to give a presentation on missile guidance. Packed with amazing, previously unheard-of technical detail, Hoag’s 87-page paper was an unprecedented introduction to the field of inertial guidance technology—nothing like it existed in the open literature at the time. An enthusiast for his craft, Hoag said that with sufficient resources, missile accuracies of only a few meters error were possible. “Small, accurate ballistic-missile warheads have a clear military function. With these available, discrete targets can be engaged without needlessly killing people,” he wrote. While not a spokesman for the Instrumentation Lab or its military sponsors,

were generally seen as intrinsically less accurate than the Air Force’s ICBMs, because of the extra uncertainty about the position, velocity, and orientation of the submarine at the point of launching.” Donald MacKenzie, *Inventing Accuracy: A Historical Sociology of Nuclear Missile Guidance* (Cambridge, MA: The MIT Press, 1990), 240. The submarine could, in principle, discern its geographic location in the location with reference not to the stars or an external beacon, but to the ocean floor, which has features that can be described on a map in much the same way as ordinary topography. Ocean floor mapping was a major project among oceanographers contracted to the U.S. Navy during the Cold War. See Jacob Darwin Hamblin, *Oceanographers and the Cold War: Disciples of Marine Science* (Seattle, WA: University of Washington Press, 2005).

¹⁴⁹ “Proposal for a Study on the Effects of New Technological Development on the Nuclear Arms Race and Prospects for its Control”; Bernard T. Feld and Talcott Parsons to Charles G. Bolte, 7 March 1968; Charles G. Bolte to Bernard T. Feld, 29 April 1968, all in Box 42, Folder 437 “Fundraising, Carnegie Endowment for International Peace, 1962-1973 (1 of 2),” BTF.

Hoag was clearly a champion of the Lab's mission to improve accuracy for counterforce targeting—even, perhaps, for the SLBMs.¹⁵⁰

Feld, meanwhile, remained fixated on submarine vulnerability. He wanted to hold another study, focusing completely on the submarine-launched force and ASW. This time the funding was flipped: Bader at Ford balked, but there had been a fortuitous personnel change at Carnegie. When Herbert Scoville exited the ACDA science and technology bureau at the start of the Nixon administration in 1969, he took a job at the Carnegie Endowment for International Peace. A new arms control program had commenced at Carnegie under Scoville's leadership. And since he'd come to share many of the same worries as Feld about the vulnerability of the submarine-launched force, he enthusiastically recommended that Carnegie fund Feld's conference in November of 1972.¹⁵¹

Scoville even wrote a paper for the study, joining forces with the Instrumentation Lab's David Hoag, whose views were steadily shifting as his contact with the arms controllers became more frequent. (By this point Hoag was actually an employee of "Draper Lab," the Instrumentation Lab's new name following its divestiture from MIT in the aftermath of the campus rancor.) Scoville and Hoag asked about the counterforce capability of submarine-launched missiles. The two argued that counterforce capability was at least technically conceivable for the submarines. For one thing the U.S. deployed enough SLBMs to hit all the Soviet land-based forces. And as far as accuracy was concerned, a submarine could conceivably surface before firing, taking a position/velocity reading before sending their payloads ashore.

¹⁵⁰ D.G. Hoag, "Ballistic-missile guidance," in B.T. Feld, T. Greenwood, G.W. Rathjens, and S. Weinberg, eds., *Impact of New Technologies on the Arms Race* (Cambridge, MA: The MIT Press, 1971), 19–105. Donald MacKenzie calls this paper "the classic technical account" of missile guidance technology: MacKenzie, *Inventing Accuracy*, 2.

¹⁵¹ Bernard T. Feld to Thomas Hughes, 5 August 1971; and Bernard T. Feld to Thomas A. Halsted, 21 June 1972, Box 42, Folder 437 "Fundraising, Carnegie Endowment for International Peace, 1962-1973 (1 of 2)," *BTF*.

(This would reveal the sub's position, of course, leaving it vulnerable to attack—but that was no worry if the subs were regarded as a first-strike force launching a preemptive blow. Their permanent invisibility was only regarded as crucial for their second-strike, retaliatory function.) But real first-strike capability demanded complex and hasty command and control operations—ordering the ships into particular positions, transmitting targeting instructions, closely coordinating the firing of their missiles—and that was where Hoag and Scoville decided the subs were at a disadvantage, since communicating with them from a central command was so difficult. Their conclusion was a welcome one: “Submarine forces are inherently unsuitable for a first strike.” Any developments that promised otherwise were to be shunned.¹⁵²

The question of anti-submarine warfare was trickier. As Richard Garwin argued in his contribution to Feld's study, the United States wanted the ability to fight against (conventional) attack submarines. It couldn't sensibly give up the ability to sink subs that were, say, threatening its merchant marine. But wouldn't the ability to destroy an attack sub also mean an ability to threaten the nuclear missile subs, and therefore interfere with the all-important stability of nuclear deterrence? He didn't think so. There was a distinction between the techniques one would employ to hunt and kill second-strike missile-carrying subs, versus protecting a convoy from run-of-the-mill attack subs. ASW against the second-strike subs required highly specialized vehicles (ships on the surface, or aircraft or helicopters flying just above the surface) equipped with high-frequency sonar, tracking the subs beneath the waves over many miles of travel before

¹⁵² Herbert Scoville, Jr. and David G. Hoag, “Ballistic missile submarines as counterforce weapons,” in Kosta Tsipis, Anne H. Cahn, and Bernard T. Feld, eds., *The Future of the Sea-Based Deterrent* (Cambridge, MA: The MIT Press, 1973), 33–51, quotation on 50.

they surfaced to fire their missiles. Those were the kinds of things a SALT II agreement should prohibit.¹⁵³

A young physicist and research associate at MIT named Kosta Tsipis contributed a paper, too. He summarized the various ways that submarines could be spotted and tracked beneath the ocean surface. Because water is opaque to electromagnetic radiation, subs couldn't be detected with radar—the way that ballistic missiles and their reentry vehicles, say, were detected and tracked from distances of thousands of miles. You had to use sound—either by “actively” bouncing sound waves from an object (sonar), or by “passively” listening with acoustic detectors. (The U.S. had done this along its Atlantic shoreline since 1961, with its SOSUS system of underwater hydrophones.) Sonar was thought to be the more effective for submarine detection, especially because submarines had gotten much quieter in the years since the Second World War. Then, submarines' diesel engines made a tremendous underwater racket; but since the arrival of nuclear-power technology, the subs could glide along without making nearly as much noise. In any case, both active and passive underwater detection involved terrifically complicated physical processes. Tsipis, drawing heavily on several writings by John Craven (the Polaris designer who'd first spoken to Feld's MIT group in 1969), waded through a long discussion of how underwater gradients in temperature and pressure affect sound velocity, so that acoustic waves actually bend in complex curves through the inky depths. Acoustic submarine detection was a problem in picking a signal out of noise—listening for the chopping of its

¹⁵³ Richard L. Garwin, “The interaction of anti-submarine warfare with the submarine-based deterrent,” in Tsipis, et al., eds., *The Future of the Sea-Based Deterrent*, 87–120. Garwin reprinted an excerpt of this paper as Richard L. Garwin, “Antisubmarine warfare and national security,” *Scientific American* 227 (1972): 14–25. According to one scholar of the Navy's nuclear submarine program, during the 1960s “the Navy quietly pursued anti-submarine warfare with apparently great and persistent success. The Navy achieved operational dominance over Soviet submarines in the early 1960s and maintained it for more than two decades.” Harvey M. Sapolsky, “The U.S. Navy's Fleet Ballistic Missile program,” in Henry D. Sokolski, ed., *Getting MAD: Nuclear Mutual Assured Destruction, Its Origins and Practice* (Strategic Studies Institute, 2004), 123-135, on 131, published online at <http://www.strategicstudiesinstitute.army.mil/pubs/display.cfm?pubid=585>.

propellers, the swirl of water in its wake, or the distinctive sonar echo from its large metallic hull. During group discussion, the study participants entertained a ban on all “large fixed active sonars”—a global array of sonar transmitter-detectors that would have tracked every submarine in the ocean around the clock. In the eyes of the assembled, it would have rendered the submarines on both sides completely vulnerable, and ruined the deterrent effect of the SLBMs. This scheme—a little outlandish now, from the safe distance of forty years—was worth worrying about for arms controllers in the heyday of dangerous technology.¹⁵⁴

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By 1973 Feld and his group could cite an impressive and growing body of arms control work, including two recent studies that had put several problematic developments under the microscope. The Ford Foundation, meanwhile, had just announced its plan to create university arms control centers. With Feld’s group’s technical chops, and the impressive range of arms control experience quartered in the political science department and the CIS (Rathjens, Ruina, Kaufmann, Bloomfield, et al.), it seemed clear that MIT should be taking a shot at one of the big Ford arms control grants. And so during late 1972 and 1973, as Doty was doing the same at Harvard, a group at MIT prepared a bid. The application proceeded much as Doty’s had: from informal conversations between Ruina and William Bader in 1972, to an official proposal in early 1973, to a four-year award of \$500,000 later that year. Wiesner, who had become MIT’s

¹⁵⁴ Kosta Tsipis, “Underwater acoustic detection,” in Tsipis, et al., eds., *The Future of the Sea-Based Deterrent*, 169–207. For background on SOSUS, see, for example, Naomi Oreskes, “A context of motivation: U.S. Navy oceanographic research and the discovery of sea-floor hydrothermal vents,” *Social Studies of Science* 33 (2003): 697–742. In the spring of 1974 the Harvard Program in Science and International Affairs held its first two workshops. The second was on anti-submarine warfare, and gathered together much of the same group that had met in 1972. See “Workshop on Anti-Submarine Warfare, April 11, 1974,” part of Ford Foundation Grant 07300204, *FFG*.

president in 1971, greased the wheels by writing directly to Bundy, addressing him familiarly as “Mac” in making his plea for a grant.¹⁵⁵

Feld and his technical group would always occupy an uneasy position next to the political scientists in the MIT arms control program—especially since Ford’s officers preferred that the program be housed within and directed from CIS. Once there was a pie to slice up, different factions eventually began to fight over how large their respective pieces would be. Ultimately, during a second round of Ford Foundation arms control grant competitions in 1977, the infighting would lead to the banishment of Feld’s group from the MIT arms control program. But early on they were a core (and probably the most enthusiastic and active) part of MIT’s effort.¹⁵⁶

If Feld represented the old guard of technical arms control, Kosta Tsipis was its fresh young face. Hailing from Athens, Greece, Tsipis had arrived in the U.S. in 1954 on a Fulbright Fellowship to study electrical engineering at Rutgers University. After getting a doctorate in experimental nuclear physics from Columbia University in 1966, he was hired as a researcher at MIT, working in the Laboratory for Nuclear Science. But it wasn’t long before he’d “developed an increasing interest in the problems of science in public affairs and particularly in arms control,” he said. By 1971 Tsipis had left the physics department and joined biology as a research fellow in what he called “atomic biophysics,” though he wouldn’t last long there, either. He had become executive secretary of the U.S. Pugwash group, and by 1972 he’d organized his own Pugwash symposium on the “dynamics of the arms race.” He wrote to one potential funder

¹⁵⁵ Wiesner wrote several times to Bundy concerning Ford’s funding of MIT’s new program. In early 1973 he wrote to express his worry that MIT would be funded at a far lower level than Harvard. The “Institute’s contributions would suffer substantially if there were to be major imbalances in support, or a structure were created such that the two institutions could not operate as peers.” MIT still got less than half of the amount received by Harvard. Jerome Wiesner to McGeorge Bundy, 27 October 1972; Jerome Wiesner to McGeorge Bundy, 11 January 1973, Ford Foundation Grant 07300723, *FFG*.

¹⁵⁶ Among the great volume of correspondence between MIT and Foundation officials about the split between Feld’s group and the larger arms control program, see, as but one example, the long letter from Bernard Feld to Enid Schoettle, 31 January 1978, in Ford Foundation Grant 07300723, *FFG*.

in 1973, “The establishment of the arms control center at Harvard by Professor Paul Doty has persuaded me to attempt to convert my research and teaching activities from nuclear physics to problems related with the impact of science and technology on the creation and/or resolution of pre-combat conflict.” His new career as an arms control advocate was launched.¹⁵⁷

Tsipis was like Richard Garwin without a security clearance. Energetic, exuberantly technical, bursting with ideas and schemes, Tsipis produced a constant outflow of reports and proposals and unsolicited letters.¹⁵⁸ But unlike Garwin, Tsipis had never had a government post, nor had he consulted for a defense contractor or a DOD-funded think tank. (Nor was he barred because of his citizenship, since he’d naturalized in 1967.) But this hardly slowed him down. He did his work using the growing stock of information about U.S. defense systems and plans that, by the late 1960s and early 70s, could increasingly be found in a variety of open sources. “The independent analysts of the recent years have relied heavily on their past government affiliations,” wrote Wiesner to Bundy in 1972, “but we believe that increasingly the issues are broader and need not be so heavily dependent on classified information.” In fact “the amount of public information about weapons systems and government programs is greater now,” so that

¹⁵⁷ “Kosta Tsipis,” Box 1, Folder “Biographical Information”; Kosta Tsipis to E. Jackson, 4 June 1973, Box 1, Folder “Proposals, 1973–1977 (1 of 2),” *KTP*; “The authors,” *Scientific American* 233, no. 1 (1975): 12. On Tsipis’s many ideas and proposals, see a sample in Box 1, Folder “Proposals, 1970-75,” *KTP*, and several other folders dedicated to Tsipis’s project proposals in the 1970s.

¹⁵⁸ Tsipis’s private notes sometimes reveal his hyper-technical approach to arms control to a degree that his published (and edited) material does not. At one point in the early 1970s, Tsipis drew up a handwritten paper titled “The utility of nuclear weapons.” It began with a “theorem”: “Nuclear weapons[,] if available to two or more countries with symmetrical technologies[,] have no political utility for the countries that possess them.” (He defined “political utility” as the “ability to use the weapon to [coerce] an opponent to accept a solution to a given conflict favourable to the owner of the weapon.”) There followed a “proof,” in which Tsipis proceeded to “calculate” the political utility, U , of a given nuclear exchange in terms of “damage” coefficients d either received or inflicted between one country and another. After a certain point the proof was expressed entirely in mathematical symbols. After a page and a half of work, Tsipis arrived at the result $U = 0$, and concluded the proof by jotting down “q.e.d.” See “The utility of nuclear weapons” (handwritten notes) in Box 1, Folder “Proposals, 1970–75,” *KTP*.

one didn't need a top-secret clearance to criticize a weapons system effectively (as Garwin and Hans Bethe had in, writing their landmark article on ABM in 1968).¹⁵⁹

Tsipis had known Feld from his days in the Laboratory for Nuclear Science, and had started hanging around Feld's arms control crowd sometime in 1970.¹⁶⁰ With a hefty dose of confidence for someone with no experience in the field, he wrote a letter to Feld that year telling him what was on his mind. Arms controllers in the past—with all due respect—had gone about it in the wrong way, he said. They'd been constantly backpedaling, their efforts “largely reactions to the adoption of a new strategy or a new weapons system by the military and political leadership of this country.... [They] have certainly not contributed to a sane strategic posture by the United States.” Take MIRV, the most dangerous technology to emerge from the 1960s: the arms control furor had been too little, too late. And even more dangerous technologies were on the road ahead. “At the present time,” he informed Feld, “D.O.D., through its project ABRES, is sponsoring the development of terminal guidance capability, which as you know will further contribute to the destabilizing effect of MIRV. These projects are at a low funding level at the present time and they are vulnerable. If we are to avert the development and installation of terminal guidance...we should act now.”¹⁶¹

The Air Force's ABRES (Advanced Ballistic Reentry Systems) program, which dated from the 1960s, encompassed several improvements in the offensive capability of missiles. These included multiple warheads, “maneuvering vehicles and similar designs, the radar signatures of different vehicles...and terminal guidance,” according to a declassified Air Force

¹⁵⁹ Jerome Wiesner to McGeorge Bundy, 27 October 1972, Ford Foundation Grant 07300723, *FFG*.

¹⁶⁰ Tsipis attended but did not speak at the 1970 Pugwash symposium on the impact of new technologies on the arms race in Wisconsin organized by Bernard Feld.

¹⁶¹ K. Tsipis to Bernard T. Feld, 18 December 1970, Box 6, Folder 44 “Arms Control 1963-1970,” *BTF*. In fact Tsipis wanted arms controllers to propose alternative defense policy as a more proactive form of arms control, rather than recommending disarmament, which was like treating rather than preventing an illness. In this he was, again, similar to Richard Garwin.

history. An ordinary reentry vehicle executes “ballistic” flight after separation from its booster. It falls back to earth, like a golf ball (a huge, conical, super-high-speed, nuclear warhead-carrying golf ball), after being launched by its rocket. During reentry, as it hits the atmosphere, it is subject to disturbances, such as strong atmospheric winds. These reduce the vehicle’s accuracy because they can’t be mechanically compensated for; the reentry vehicle has no power of its own to change its flight path. “Maneuverability,” on the other hand, referred to the ability of a missile’s reentry vehicle during the atmospheric phase of flight to make intentional corrections to its path. This served one of two purposes: to evade missile defenses (the explosion of a missile defense warhead), or to adjust for those atmospheric disturbances, achieving a degree of accuracy that was virtually impossible with pure ballistic flight. (A maneuverable reentry vehicle could intentionally alter its landing-point by perhaps dozens of miles from an initial target location).¹⁶²

The ABRES system designed by McDonnell Douglas proposed to use two movable “flaps” on one side of the reentry vehicle. Like a subtler version of the control surfaces on an airplane’s wings, the flaps could execute micro-adjustments to change the supersonic airflow over the vehicle, changing its trajectory. The Navy, meanwhile, was working on a maneuverable reentry vehicle, the Mk-500 (nicknamed the “Evader”) for its new Trident missiles. The Trident vehicle featured a bent nose cone, angled off its central axis. The crooked nose was fixed, not adjustable like ABRES’s flaps, so that the Evader was actually incapable of straight-line flight. When it began to reenter the atmosphere, it arced down in a complicated spiral-like trajectory, whose general direction could be adjusted by rotating a ballast weight inside the reentry vehicle.

¹⁶² Bernard C. Nalty, “USAF Ballistic Missile Programs, 1967-1968,” Office of Air Force History (September 1969), 38.

The Navy's maneuverable reentry vehicle, as its nickname suggested, was better for sneaking through defenses than achieving super-accuracy.¹⁶³

To know just how to adjust the control surfaces, the vehicle had to compare information about its present location to information about where it was supposed to end up. This demanded both that it have the ability to "see" the terrain beneath it (with radar, or by optical imaging), and the ability to compare that information to an onboard map. That was "terminal guidance," a kind of "homing" device for ballistic missiles. Unlike the inertial guidance "package" on an ordinary MIRVed missile, which was contained on the "bus" that dispensed the individual vehicles before reentry, on a maneuverable reentry vehicle the guidance package would have to sit within the vehicle itself, requiring either a very large missile or major developments in miniaturization to accommodate the extra cargo.¹⁶⁴

Tsipis would not have seen any of the classified data on ABRES or the Navy's "Evader," or terminal guidance. There's a good chance he first came across it in the work of David Hoag, who briefly described maneuverability and terminal guidance in his 1970 paper for Feld's symposium. And there was other information out in the open. Robert McNamara had mentioned maneuverability as early as 1966, when the Pentagon was advertising its "ICM," or "improved capability missile." The Defense Secretary had promoted maneuverability as an argument against deploying an ABM system, which a maneuverable warhead would defeat even more soundly

¹⁶³ "Science and the citizen," *Scientific American* 229 (1973): 50-56, on 55-56. The guidance computer on each vehicle would be outfitted with a terrain map, created from reconnaissance photography, of its target region. During reentry this map was compared to location data from the vehicle's onboard positioning system. The contract for the terminal guidance technology for ABRES was awarded to a division of the Singer Company, which had evidently diversified well beyond sewing machines. On some of the technical characteristics of MaRVs, see Matthew Bunn, "Technology of Ballistic Missile Reentry Vehicles," MIT Program in Science and Technology for International Security, Report No. 11 (March 1984), esp. 32-62. In the end MaRVs were never deployed on U.S. ICBMs or SLBMs, though they were deployed on NATO's Pershing II intermediate-range ballistic missiles stationed in Western Europe during the 1980s.

¹⁶⁴ Bunn, "Technology of Ballistic Missile Reentry Vehicles," 47-56. Alternatively the vehicle could be guided by a global positioning system.

than a garden-variety reentry vehicle would. (It wasn't until 1973 that the Pentagon unveiled a new acronym for the technology—MaRV, for “Maneuverable Reentry Vehicle.”) Tsipis wasn't troubled by it on those grounds; arms controllers had always warned to the argument that offense reigned supreme over defense in the missile age. What bugged him was the relevance of maneuverability to the counterforce mission. MaRVs could, as they fell, “seek” their targets before hitting them, landing their warheads extremely close to “hard” targets like missile silos. MaRVs could be incredibly accurate. What really bothered Tsipis was missile accuracy.¹⁶⁵

The unsurpassed study of the history of missile guidance technology is Donald MacKenzie's *Inventing Accuracy*. It is a touchstone of the “social construction of technology” approach. In dogged detail MacKenzie revealed the bureaucratic rivalries, the personalities and egos, the budget fights, the minuscule technical choices—the multitude of disturbances large and small—that led “accuracy” to end up in one place rather than another. MacKenzie's book had much to say about the military and those who were contracted to work for it. But there was little discussion of accuracy as it was imagined and debated outside the military and the defense laboratories that engineered the guidance systems.

Accuracy had been recognized as problematic from arms control's early days. In 1960 Schelling and Halperin had listed missile accuracy as their first example of a possibly destabilizing qualitative breakthrough.¹⁶⁶ But accuracy was never treated as an urgent object of limitation because research was not taken to be the sort of thing that should be restricted. “Limitations on research difficult to enforce because it is never really clear what sort of research is going to lead to useful military techniques,” wrote Schelling and Halperin. The commonly held opinion—said time and again of missile defense R&D—was that you had to pursue research

¹⁶⁵ William Beecher, “Air Force plans a giant missile to thwart radar,” *New York Times* (20 June 1966): 1; John W. Finney, “U.S. is developing a warhead capable of evading defense,” *New York Times* (20 January 1974): 1.

¹⁶⁶ Schelling and Halperin, *Strategy and Arms Control*, 37-38.

as aggressively as possible to avoid getting caught flat-footed in the event the Soviets made a critical discovery first. By the late 1960s this view had modulated from “should” to “could”: perhaps some avenues of research and testing *should* be blocked (MIRV was the obvious example in 1969), but there was no way an arms control measure *could* block them. Jack Ruina, in an often-excerpted comment during 1969 Congressional testimony, opined that “on the issue of guidance accuracy, there is no way to get hold of it, it is a laboratory development, and there is no way to stop progress in that field.”¹⁶⁷

The debates over ABM, MIRV, and SALT had surprisingly little to say about accuracy.¹⁶⁸ Feld’s proposals to the Carnegie Endowment in 1968 and 1969 for his study on the impact of new technologies on the arms race, for example, contained no mention of accuracy (even though the Instrumentation Lab’s David Hoag would deliver his paper on missile guidance when the study itself was held a couple years later). A separate report for Carnegie by George Rathjens on the “future of the strategic arms race” in 1968, likewise, went on at vatic length about MIRV but didn’t mention accuracy even a single time. In the furious public debate Rathjens had with Albert Wohlstetter that year over Minuteman vulnerability and Soviet first-strike capability, the two fought over everything but the accuracy estimates, taking those figures

¹⁶⁷ Donald MacKenzie, citing Colin Gray, quotes Ruina (without identifying him) in *Inventing Accuracy*, 3.

¹⁶⁸ A relatively early example of attention to accuracy was an article by a member of the technical staff of Sylvania Electronic Systems, who wrote in 1968 that missile accuracy, in combination with three-to-one superiority in numbers, might give the U.S. a first-strike capability against the Soviet Union. Raphael Miller, “The metaphysical world of strategic systems,” *Bulletin of the Atomic Scientists* 24, no. 10 (1968): 18-22. But he did not single out accuracy for special consideration any more than Schelling and Halperin had. Also see, for example, George Bunn, “Missile limitation: By treaty or otherwise?,” *Columbia Law Review* 70, no. 1 (January 1970): 1-47. As Bunn wrote, speaking of generally MIRV and ABM in the SALT talks, “Assuming, however, that both sides exercise some restraint, the negotiators will continue to be plagued by the problem of advancing technology. They may find it possible to halt the deployment of new missiles, but they can not stop political change or the advancement of science. Almost inevitably, a specific limitation on particular kinds of missiles will have to be revised in the future to prevent evasion by new kinds” (on 4). Bunn was thinking of accuracy, but only as part of the general advance of offensive missile technology, of which MIRV was the shining example.

as fixed and not worth serious dispute. For arms controllers, missile accuracy was typically understood as part of the general threat that was more vividly symbolized by MIRV.¹⁶⁹

Several factors shaped accuracy's emergence as an arms control issue in the early to mid-1970s. The first was the way accuracy fit into the conceptual framework that arms controllers had been constructing for well over a decade. It was immediately obvious to everyone that higher accuracy translated to counterforce capability (RAND's strategists knew this in the 1950s, and Schelling and Halperin knew it in 1960). And for the hardcore arms controllers, counterforce capability meant crisis instability—the escalating temptation to hit first. These worries were only inflamed by the conclusion of SALT I in 1972. As it became clear that the first round of SALT would deal only with numbers of missiles and leave technological developments like MIRV completely unharassed, arms controllers began to worry that accuracy would run away with itself. Bernard Feld said at a 1971 conference, in one of his first comments on the topic, that “the specter of MIRV, coupled with high accuracy guidance, presents a very serious threat to the fixed land-based missiles (ICBM's) of the other side. Even though the requisite guidance accuracy is probably not yet available to either side, its achievement can be foreseen on the basis of currently available technology.” The Kaysen report for the Ford Foundation in mid-1973 concurred, singling out accuracy. “Such technological possibilities as delivery accuracies of a

¹⁶⁹ George W. Rathjens, *The Future of the Strategic Arms Race* (New York: Carnegie Endowment for International Peace, 1969). In Herbert York's 1970 *Race to Oblivion*, he listed “further improvements in guidance accuracy” as one of four “technological advances [that] threaten to upset the strategic stability,” along with improvements in missile reliability, MIRV, and ABM. However, York did not say much about the technology of missile guidance itself, or go into nearly the same analytical detail as Kosta Tsipis later would on the strategic and arms race implications of missile accuracy. And although York listed accuracy and reliability as independent malefactors alongside MIRV and ABM, most of his discussion of accuracy was contained within a chapter devoted to MIRV—making accuracy seem a component of the threat presented by MIRV, more than its own threat. See Herbert F. York, *Race to Oblivion: A Participant's View of the Arms Race* (New York: Simon and Schuster, 1970), esp. 173–187.

few feet,” the authors said, “or maneuvering re-entry vehicles could challenge the present assumptions on which the stability of deterrence rests.”¹⁷⁰

Just as important for accuracy’s rise was an announcement in January of 1974. That month, Nixon’s new Secretary of Defense, the former head of the RAND strategic studies division James Schlesinger, told a group of reporters that the Pentagon had recently been remaking its plans for fighting a strategic nuclear war. And it had begun retargeting its missiles. That month Nixon signed National Security Decision Memorandum 242, on “policy for planning the employment of nuclear weapons.” The memorandum mentioned developing additional nuclear “options” and “selected nuclear operations,” strikes “in which the level, scope, and duration of violence is limited in a manner which can be clearly and credibly communicated to the enemy.” This was a revival of the “flexible response” doctrine of the early 1960s—but this time enacted in the actual war plans, not just in public speeches.¹⁷¹ Quickly baptized the “Schlesinger Doctrine,” the Defense Secretary said that the U.S. had adjusted its war plans to allow for “limited nuclear options”: selective nuclear strikes on a range of military and civilian targets, with an unprecedented degree of variation in the numbers and types of weapons

¹⁷⁰ B.T. Feld, “Current developments and dangers of atomic armaments,” Box 6, Folder 45 Arms Control “1971-1973, 1/2,” *BTF*; Carl Kaysen, Richard L. Garwin, Allen Whiting, and Harold Feiveson, “Recommendations for a National Program in Support of Research and Training in Arms Control and Related Subjects,” July 1973, Ford Foundation Report 002447, *FFR*.

¹⁷¹ National Security Decision Memorandum 242, 17 January 1964, published online by the Richard M. Nixon Presidential Library & Museum, National Security Decision Memoranda, available at <http://nixon.archives.gov/virtuallibrary/documents/nationalsecuritydecisionmemoranda.php>. Also see Fred Kaplan, *The Wizards of Armageddon* (Stanford, CA: Stanford University Press, 1991 [1983]), 356–360. When Richard Garwin learned the news of the new options and targeting policies, he rushed to the Pentagon and demanded an interview with Schlesinger. “He was on a trip so I talked with Don Cotter, the Assistant Secretary of Defense in charge of nuclear energy, those nuclear weapons. They were looking for second-strike counterforce, was a short way to put it. They felt that high quality deterrence, as Paul Nitze used to put it, could be achieved not by destroying or having the ability to destroy Soviet society, factories, ordinary military forces, but one had to be able to destroy the Soviet strategic forces themselves.... [T]hey wanted to ensure, they said, that if a nuclear war started, the Soviets would see that they would lose all of their strategic war making potential and therefore they would be deterred. They wouldn't start the nuclear war because if they did they would suddenly become disarmed.” See WGBH Media Library & Archives, “Interview with Richard Garwin, 1987,” 28 December 1987, available at <http://openvault.wgbh.org/catalog/wpna-bb79eb-interview-with-richard-garwin-1987>.

launched, and the kinds of targets struck. Gone were the cataclysmic, inflexible, single-option spasm attacks of the Eisenhower- and Kennedy-era SIOPs. Counterforce was in the air again as it had not been in well over a decade.¹⁷²

The 1974 announcement occasioned a sudden flurry of debate over the wisdom of counterforce targeting and the role of accuracy. Not long after Schlesinger's revelation, the weekly public television show *The Advocates* took as its topic the question, "Should we develop highly accurate missiles and emphasize military targets rather than cities?" Former Republican Congressman Robert Ellsworth faced off against former NSC staffer Barry Carter in front of a bundled-up crowd in Boston's Faneuil Hall on a cold February night. Each advocate was afforded (in the custom of the show) an expert panel to provide supporting testimony. Joining

¹⁷² When Nixon heard his first briefing on the SIOP in 1969, there were still only five "options" in the plan. It's worth saying that the 1974 Schlesinger Doctrine hadn't really originated with Schlesinger; the Pentagon had been searching for counterforce options for several years, and quite earnestly since the first weeks of the Nixon administration. See William Burr, "The Nixon administration, the 'Horror Strategy,' and the search for limited nuclear options, 1969–1972," *Journal of Cold War Studies* 7, no. 3 (2005): 34–78. Where Burr emphasizes the role of Henry Kissinger and his desire to make nuclear threats "politically plausible" by making them less extravagant, Fred Kaplan makes more of Schlesinger's hand in rewriting the rules of nuclear use doctrine in the 1970s. Schlesinger had authored a widely known RAND memorandum as early as 1968 titled "Rationale for NU-OPTS," referring to a recent project directed by Air Staff officers who were friendly to the idea of tactical uses for nuclear weapons. Their study, and Schlesinger's RAND paper, called for more flexible nuclear options ("NU-OPTS"). "Schlesinger was practically the ideal type to help launch a revival of the RAND tradition," Kaplan writes. "Everything about him spelled 'defense intellectual'—the slightly jaded sensibility, the whiff of arrogance, the pipe-puffing affectation of cool insouciance." Kaplan, *The Wizards of Armageddon*, 356–360, on 357. The original account of the reawakening of counterforce was in fact written by Desmond Ball for the California Seminar on Arms Control and Foreign Policy. Ball insisted not only that the Schlesinger Doctrine wasn't Schlesinger's, but that there was essentially nothing new in it beyond what had been recommended by William Kaufmann and the pioneers of counterforce at the start of the Kennedy administration. More recently, Francis Gavin makes an almost opposite claim: that the "flexible response" doctrine of the early 1960s was operationally indistinguishable from the "massive retaliation" of the Dulles/Eisenhower days, and that Schlesinger's 1974 announcement represented the true policy revolution. Well, there is merit to both arguments. Each plays on the familiarly blurry line dividing continuity from change. In fact, annual reports of the California Seminar from 1974 onward often commented that the shift in nuclear targeting had really been sparked by Fred Iklé's 1973 Seminar paper on the fate of deterrence—an overstatement containing a grain of truth. Unhappiness with deterrence-as-assured destruction had been simmering for several years, and Iklé's essay was a high-water mark. It was a beacon to the disaffected. It helped make credible the idea that assured destruction might be an unacceptable policy, and it prepared ground for Schlesinger's decision. What seems unquestionably true is that a significant overhaul in the nuclear establishment had taken place in Nixon's second term, symbolized by the rise of expert deterrence critics like Iklé and Schlesinger to the top tiers of policymaking. See Desmond Ball, "Déjà Vu: The Return to Counterforce in the Nixon Administration" (California Seminar on Arms Control and Foreign Policy, December 1974); Gavin, *Nuclear Statecraft*, Chapter 2, "The Myth of Flexible Response: American Strategy in Europe During the 1960s," 30–56.

Ellsworth's side arguing for increased counterforce capability was the stern, professorial RAND president Henry Rowen, and Geoffrey Kemp, on the faculty at the Fletcher School at Tufts University and a part-time research associate at Paul Doty's new Harvard PSIA. Helping Carter argue that *any* nuclear use was an incomprehensible disaster, and that counterforce targeting and accurate missile increased the risk of nuclear war, were the experienced arms controllers Morton Halperin and Herbert Scoville.¹⁷³

Rowen called assured destruction "a policy of genocide," and said that it "really means incinerating hundreds of millions of people in the United States or in the Soviet Union and Western Europe. It's a policy that... would be worse than a thousand Hiroshimas." Hence the need for highly accurate, limited, "flexible" nuclear options short of total cataclysm.¹⁷⁴ Scoville's opening remarks, however, received the only spontaneous applause of the night. "The overriding objective of our strategic policy should be to avoid nuclear war," he said firmly. "Even a limited nuclear war is so dangerous and can create such great damage, and it also has a risk of escalating into a full-scale conflagration that we must avoid it at all cost. What we should be doing is not learning how to fight nuclear wars better, but how to avoid them more surely." Ellsworth challenged Halperin during cross-examination later in the evening: "You concede that one of the classic aims of arms control is to limit damage in case war should break out, do you not?" A smile crept across Halperin's face. (He had written almost those exact words in 1960, with

¹⁷³ WGBH Media Library & Archives, "Should We Develop Highly Accurate Missiles and Emphasize Military Targets Rather Than Cities?," 14 February 1974, available online at <http://openvault.wgbh.org/catalog/e97cd3-should-we-develop-highly-accurate-missiles-and-emphasize-military-targets-rather-than-cities>.

¹⁷⁴ In mentioning "incineration," Rowen had misspoken slightly. The Pentagon's nuclear war plans took little heed of post-attack fire in their damage estimates, having always assumed that the principal (because more straightforwardly calculable) means of destruction would be by blast effects. That crucial choice had, in part, led to the targeting doctrines that Rowen found so repugnant, proposing to land thousands of nuclear weapons on the Soviet Union in a U.S. strike. Arguably, it had therefore encouraged procurement decisions allowing the nuclear arsenal to grow to stupendous size in the first place. The definitive account is Lynn Eden, *Whole World on Fire: Organizations, Knowledge, and Nuclear Weapons Devastation* (Ithaca, NY: Cornell University Press, 2006).

Thomas Schelling, as part of their formulation of arms control's basic aims.) In response, Halperin quoted the part that Ellsworth left out: "But one has to weigh that against the danger that [an accurate counterforce mission] stimulates the arms race and increases the likelihood of nuclear war," he told the audience, "and in balancing those I would argue that it is undesirable."¹⁷⁵

But SALT's failures, technology's progress, and the Pentagon's new targeting doctrines alone did not make accuracy a neon-lit arms control issue. That took people. That is, it required particular people to exert themselves in particular ways. And the people who grew most concerned about accuracy, and picked apart its arms control implications with restless devotion, were in Cambridge. They were clustered around Bernard Feld and the Ford Foundation-funded arms control program at MIT. Accuracy would receive its most intense interrogation in the work of Feld's junior partner Kosta Tsipis. In 1973 Tsipis was appointed a research associate in the new MIT arms control program at the CIS, allowing him, for the first time in his career, to apply himself full-time to arms control work. He spent the summer of 1974 at the Stockholm International Peace Research Institute (SIPRI).¹⁷⁶ At MIT he'd been hunkered over calculations on missile accuracy and counterforce capability. From his temporary base in Sweden, he assembled his brief against the strategy of counterforce and the technology of high accuracy.

Tsipis's central claim was that the public discussion had been misguided. The SALT debate had concentrated largely on the numbers of missiles, and their size—their "throw

¹⁷⁵ Schelling and Halperin had written in 1960 that by arms control "we mean to include all the forms of military cooperation between potential enemies in the interest of reducing the likelihood of war, its scope and violence if it occurs, and the political and economic costs of being prepared for it." Schelling and Halperin, *Strategy and Arms Control*, 2. Barry Carter followed up his performance on television with an article a few months later in *Scientific American* arguing against the new counterforce policy: Barry Carter, "Nuclear strategy and nuclear weapons," *Scientific American* 230, no. 5 (1974): 20-31.

¹⁷⁶ Established in 1966, SIPRI began publishing its annual "Yearbook of World Armaments and Disarmament" in 1969, a compendium of global weapons systems and stockpiles. They are still released annually. See *SIPRI Yearbook of World Armaments and Disarmament* (Stockholm: Almqvist & Wiksell, 1969).

weight,” the physical mass they were capable of launching to their targets. These quantities might have been relevant in the 1960s, when explosive yield tracked throw-weight more closely, and the U.S. had possessed clear nuclear superiority over the Soviet Union. But now that the Soviets had (since about 1970) achieved equality in numbers of missiles, the *qualities* of modern strategic warheads and delivery systems were more important. The first dangerous quality was explosive yield, which, thanks to advances in warhead miniaturization, was no longer yoked so simply to throw-weight. Smaller missiles could carry much larger explosives. The second was missile accuracy, whose limits Tsipis said were set at perhaps 30 meters by “the properties of matter.” He explained how accuracy was measured by the “circular error probable” (or CEP, the radius of a circle within which half of the missiles of a given accuracy, aimed at the circle’s center, would land). In combination with the yields that were increasingly available in MIRVed warheads, the U.S. missile fleet by the late 1970s would be a menacing counterforce arsenal. Such developments would, in the predictable manner of action-reaction processes, all but compel the Soviet Union to match them.¹⁷⁷

In a section called “The calculus of destruction,” Tsipis defined a new parameter he termed “lethality,” denoted by the letter *K*, which encoded in a single number only yield and accuracy. This figure by itself (plus a factor called the “reliability,” which measured the probability the missile wouldn’t malfunction) could tell you everything you wanted to know about a given missile’s suitability for counterforce missions—its ability to land very close to a reinforced concrete missile silo and blow it apart.¹⁷⁸ The product between the *K*-value of a

¹⁷⁷ Kosta Tsipis, “Offensive Missiles,” Stockholm Paper 5, Stockholm International Peace Research Institute (1974).

¹⁷⁸ Specifically, Tsipis defined the lethality of a single reentry vehicle against a missile silo as $K = \frac{y^{2/3}}{(CEP)^2}$, where *y* is the warhead’s explosive yield and *CEP* is the reentry vehicle’s circular error probable, the measure of its accuracy. Thus Tsipis could express, in terms of lethality, the “single-shot kill probability” of the reentry vehicle—the probability that, if the vehicle were targeted at a missile silo of blast resistance *H* (or “hardness,” measured in

missile force (the Minuteman IIIs, for example, or the Titan IIs) and the total number of warheads N that the force could deliver was the measure of total lethality—the overall counterforce capacity. Tsipis’s point was that contrary to the doom-saying critics of SALT, the United States *still* maintained a staggering effective superiority over the Soviet Union in terms of aggregate lethality. Its grab for greater yield-to-weight ratios and finer accuracy would only push the Soviet Union to compete in these qualitative areas, too. The Soviets were guaranteed to catch up (Tsipis figured this would happen sometime in the early 1990s). Only the limits of physical laws hemmed in this race to nowhere. “Thus Nature will terminate the competition for increased lethality of nuclear weapons that political leaders seem unable, and the military unwilling, to end.”¹⁷⁹

Before submitting for publication, Tsipis sent a draft to Bernard Feld. Feld told Tsipis that he had overplayed the irrelevance of numbers and throw-weight. These were “not so much irrelevant as more indirectly relevant than the parameters you use,” Feld cautioned. Tsipis politely ignored this comment and proceeded to cast his work on missile accuracy as far and wide as he could. SIPRI issued the first version; the journal *Science* and MIT’s in-house organ *Technology Review* each published subsequent versions; and in mid-1975 Tsipis landed a glossy spread in *Scientific American*. It was the first detailed discussion of the technology of accuracy and its strategic implications in the magazine, and the first major publication on accuracy and its arms control implications anywhere. Tsipis included a schematic diagram of an inertial guidance

pounds per square inch pressure above atmospheric pressure), it would destroy the silo. The single-shot kill probability is $P_k = 1 - \exp\left[-\frac{K}{2H^{2/3}(f(H))^{2/3}}\right]$, where $f(H)$ is a “correction factor” depending only on the silo hardness. (In the expression for the lethality, K , the yield and accuracy are combined in such a way that a reentry vehicle with a greater lethality necessarily has a greater probability of destroying the missile silo at which it is aimed. Different reentry vehicles can therefore be compared in terms of K alone. This would not be true if the lethality were defined in other ways—using other combinations of γ and CEP —subject only to the constraint that it increase with increasing yield and increasing accuracy. Tsipis’s choice of that particular combination of γ and CEP was specific and intentional.)

¹⁷⁹ Tsipis, “Offensive Missiles,” 28.

package (describing the operation of its accelerometers, gyroscopes, and gimbals), explained his scheme for calculating missile lethality, and argued for a limitation on missile testing—a limit he said was “verifiable by national means of inspection and quantitatively negotiable.”¹⁸⁰

Tsipis had his critics. Thomas Brown, the RAND analyst who had participated in Wohlstetter and Iklé’s study groups in the California Seminar years earlier (and was now director of research at Wohlstetter’s new think tank, Pan Heuristics), rebutted Tsipis’s calculations in *Survival* in 1976. Brown took Tsipis to task for his loose and unsubstantiated estimates of the lethality of the Soviet missile force, based on overly optimistic guesses of their accuracy. Brown surveyed a number of estimates of Soviet missile accuracy and found that Tsipis might have lowballed the Soviet lethality by as much as a factor of 16. And in any case, Brown said, none of Tsipis’s policy judgments—that the U.S. would always have superiority in lethality over the Soviet Union, and that increased lethality served no clear military function—followed easily from his calculations. (In 1974 Feld had given Tsipis the warning, unheeded, that he should frame his study as “analysis” rather than “advocacy.”)¹⁸¹ But from Tsipis’s point of view, a major accomplishment was already in the books: accuracy had been transformed into a full-grown arms control issue. In the later 1970s it would assume only greater importance with the advent of the MX missile, a land-based ICBM MIRVed with ten warheads, boasting potential accuracies of a few meters. When the Carter administration issued its Presidential Directive 59 in 1980, which etched counterforce targeting and limited options into the war plans more deeply

¹⁸⁰ Kosta Tsipis to B.T. Feld, 24 July 1974, Box 46, Folder 497 “Stockholm International Peace Research Institute (SIPRI), 1966–1971,” *BTF*. Tsipis’s other publications were: Kosta Tsipis, “Physics and calculus of countercity and counterforce attacks,” *Science* 187, no. 4175 (1975): 393-397; Kosta Tsipis, “The calculus of nuclear counterforce,” *Technology Review* (October/November 1974): 34-47; Kosta Tsipis, “The accuracy of strategic missiles,” *Scientific American* 233 (1975): 14-23, quotation on 23.

¹⁸¹ Thomas A. Brown, “Missile accuracy and strategic lethality,” *Survival* 18, no. 2 (1976): 52-59. Tsipis and Brown continued their fight later that year in a special forum on accuracy in *Survival*: Kosta Tsipis and Thomas Brown, “Forum: missile lethality,” *Survival* 18, no. 4 (1976): 190-192.

than Schlesinger's innovations had, arms controllers could draw on several years of accuracy criticism and debate that had been initiated years earlier by Kosta Tsipis.¹⁸²

The tangled web of strategic policies and ideas, the foundation money that underwrote local expert communities, and arms control concerns, contributed no less to accuracy's "social construction" than secret military-engineering decisions. They do not explain why inertial guidance triumphed over rival technologies, or why a certain kind of gyroscope and not another was used in a particular missile system. But they indicate how and why accuracy became a central arms control issue in the first place, a "black box" worth unpacking. They suggest, in other words, a rich dimension of "the social" that was layered over the technology of missile guidance during the 1960s and early 1970s. It raised accuracy as an issue for careful debate. It made MacKenzie's study possible.¹⁸³

¹⁸² Kaplan, *The Wizards of Armageddon*, 383–386.

¹⁸³ MacKenzie does mention in a footnote that David Hoag presented his "classic" paper at a 1970 Pugwash symposium, but he incorrectly infers this to mean that Hoag's prediction of high possible accuracies "counted more as a warning than as a promise." In fact Hoag was "promising" high accuracy at the symposium, and recommending counterforce capability. In the course of contact with the community of arms controllers over the following couple of years, however, Hoag's views began to shift. MacKenzie, *Inventing Accuracy*, 386. The first footnote in *Inventing Accuracy* cites the paper by David Hoag (which MacKenzie calls "the classic technical account") and work by Kosta Tsipis. MacKenzie references the Hoag paper at fourteen separate points in the book, from beginning to end. *Inventing Accuracy's* schematic of an inertial guidance apparatus (the first figure in the book) is drawn, almost line for line, from a figure in Tsipis's 1975 *Scientific American* article. And in the acknowledgements MacKenzie thanks Matthew Bunn for sharing files of technical papers on guidance and reentry vehicles—a collection stored up from Bunn's days as a master's student in the MIT Program in Science and Technology for International Security, studying arms control under Kosta Tsipis. It therefore seems worth remembering that MacKenzie's project itself wasn't merely a scholarly contribution to the history of technology, but an arms control project—another in the line of efforts to limit accuracy inaugurated by Tsipis in 1974. See Donald MacKenzie, "Missile accuracy—an arms control opportunity," *Bulletin of the Atomic Scientists* 42, no. 6 (1986): 11–16. It's also worth noting that the core of MacKenzie's argument was (in barest outline) suggested as early as the 1973 American Academy arms control summer study. For the *Daedalus* special issue resulting from the study, Graham Allison wrote that "the details of weapons systems (e.g., accuracy of warheads) are determined in large part by the interaction between technical feasibility and organizational interests (in the United States, the services and the research community)." But then Allison reverted to a more traditional perspective, treating technology as an autonomous force. He identified two "laws" tending to put technologies like missile guidance beyond the reach of effective control. The first he termed "Ruina's law," based on Jack Ruina's comment (quoted earlier) that there was no hope of restricting laboratory accuracy research and engineering. And there was "Brooks' law," named after Harvey Brooks's idea that "at least ten per cent of an R&D budget" in general—not just accuracy in specific—"is uncontrollable in detail by a central authority." See Graham T. Allison and Frederic A. Morris, "Armaments and arms control: Exploring the determinants of military weapons," *Daedalus* 104, no. 4 (1975): 99–129, on 126.

“Accuracy” wasn’t just a technology or a process of struggle and choice inscribed in gyroscopes and guidance packages. It was an arms control topic. It was conceived and scrutinized within an intellectual framework that arms controllers had constructed over the previous fifteen years, and within a community whose primary means of support by the 1970s came from the Ford Foundation. It says a great deal about the changing patronage system and institutional structure of arms control by the mid-1970s that accuracy’s most trenchant critic was Kosta Tsipis—an arms controller who’d never looked at a classified document, never worked on a government contract or written a government report, and who spent his days in a university arms control program built entirely with private foundation largesse.

Conclusion

“The world is entering upon perilous times, perhaps the most dangerous period in its entire history,” said Bernard Feld in a 1974 lecture at University College, London. He put the odds of a nuclear weapon being used in a conflict in the next ten years at one in three. He seemed utterly dispirited. “Having devoted half a lifetime to the pursuit of what now appears to be a will-o’-the-wisp, I am forced, reluctantly but inexorably, to the conclusion that the arms control approach simply does not work in today’s world.”¹⁸⁴ The Swedish disarmament activist Alva Myrdal gave poignant expression to the sense of disillusionment in 1977: “I wish it were not too late to start a boycott against the use of ‘arms control’ as an over-all term,” she wrote. “It is nothing but a euphemism, serving regrettably to lead thinking and action towards the acceptance as ‘arms control measures’ of compromises with scant or nil disarmament effect.”¹⁸⁵

¹⁸⁴ Bernard T. Feld, “The charade of piecemeal arms limitation,” *Bulletin of the Atomic Scientists* 31, no. 1 (1975): 8–16, on 8 and 14, respectively.

¹⁸⁵ Alva Myrdal, *The Game of Disarmament: How the United States and Russia Run the Arms Race* (Manchester: Manchester University Press, 1977), xvi.

International arms control negotiations continued on their plodding, faltering course during the remainder of the decade. Hopes briefly flared when the Carter administration arrived in the White House and promised to restore the negotiating responsibilities and even some of the original staff of ACDA. Paul Warnke, the liberal Washington lawyer and former Assistant Secretary of Defense who in 1975 had famously described the strategic arms competition between the United States and the Soviet Union as a race between “two apes on a treadmill,” took the reins at ACDA and became chief U.S. SALT II negotiator. But the SALT II agreement signed in Vienna in 1979 by Carter and Brezhnev had, it was clear, almost no chance of ratification in Congress. This pessimism was confirmed emphatically when Soviet forces invaded Afghanistan late that year. Carter was so sure of the treaty’s inevitable failure that he removed it from consideration by the Senate just days later. It was a grand disappointment, not least for McGeorge Bundy. In his final two years as Ford Foundation president, he’d taken a turn on ACDA’s General Advisory Committee as Carter and his negotiators stumbled their way into SALT II. In a 1978 report he’d advised the President that the superpowers wielded “grossly excessive strategic armaments,” and that the failure of SALT II to survive “would reopen the prospect of an unlimited contest both in megatonnage and in destabilizing technology.” Formal arms control with the Soviet Union would await a future administration.¹⁸⁶

¹⁸⁶ Paul C. Warnke, “Apes on a treadmill,” *Foreign Policy* 18 (1975): 12–29; Richard Rhodes, *Arsenals of Folly: The Making of the Nuclear Arms Race* (New York: Random House, 2007), 118–137. When Bundy was renewing his clearance to serve on the ACDA GAC in 1977, he felt compelled to tell the White House Associate Counsel that “I do of course have direct and continuing executive responsibility for the work of the Ford Foundation, and one important field of activity in this Foundation is the support of research and analysis on matters relating to international security and arms control.... The point which perhaps should be emphasized is simply that the program we have developed here is guided by a conscious determination to support research and analysis across a wide spectrum, so that the scholars and institutions receiving our grants have taken widely different views on particular questions of policy.” McGeorge Bundy to Michael H. Cardozo, 5 October 1977, Box 74, Folder “Subject Files, GAC, Clearance,” *JFK-MGB*. Also see “Outline of a SALT Report by G.A.C.,” Box 74, Folder “Subject Files, GAC, Advice to Carter on SALT II (1 of 2),” *JFK-MGB*.

Yet the 1970s were also a time of fervent arms control institution building in the United States. By the end of the decade the Ford Foundation had awarded multi-million-dollar endowments for the arms control programs at Harvard and Stanford, turning them into permanent university centers. Doty's program, rechristened as the Center for Science and International Affairs, received a \$4-million, ten-year endowment, by far the largest award. Stanford's program, by the early 1980s, would become the Center for International Security and Arms Control. MIT received another injection of \$1 million from Ford in 1979, for what became its rebranded "defense and arms control studies" program. Cornell got an additional \$500,000 for its peace studies effort. And Ford created yet more arms control and international security programs—at UCLA, Indiana University, the University of Pittsburgh, the RAND Graduate Institute (a PhD program in policy analysis created at RAND in 1970), and overseas at the Australian National University, the University of Aberdeen, and the Graduate Institute of International Studies in Geneva. In sum the Foundation had invested at least \$15 million in academic arms control in a span of about seven years.¹⁸⁷

Bernard Feld and Kosta Tsipis, meanwhile, took their technical arms control outfit, which had been ejected from MIT's larger arms control program, and upgraded its status in 1978 to an independent Program in Science and Technology for International Security. It was initially funded with a separate grant from the Ford Foundation, negotiated with program officer Enid Schoettle, who'd risen the ranks at Ford to take over most of its arms control portfolio.¹⁸⁸ Feld and Tsipis began supervising some of their own graduate students and postdocs. And they began

¹⁸⁷ Enid C.B. Schoettle to Craufurd Goodwin, Memorandum on Future Support of Programs for Research and Training in International Security and Arms Control – (Appropriation to PSIA), 28 October 1976, Ford Foundation Report 011062, *FFR*; Ford Foundation Grant 07700365, *FFG*.

¹⁸⁸ "Proposal for the Continuation of Funding of the Program in Science and Technology for International Security (PSTIS)," 18 November 1977, Box 1, Folder "Grants, Funding History, Early Years of PSTIS, 1976–78," *KTP*.

writing “arms control impact statements,” inspired by 1975 legislation requiring that requests for new weapons system authorizations be accompanied by a study of the weapon’s implications for U.S. treaty obligations.¹⁸⁹ In the first few years of Feld and Tsipis’s MIT program, their studies included substantial reports on high-energy particle beam and laser antiballistic missile weapons. The first report, an arms control impact assessment of the proposed MX missile, was released in March of 1978 to some fanfare.¹⁹⁰

The lessons of the previous two decades had been hard ones. Arms control had enjoyed a period of relatively robust federal encouragement in the 1960s. But as the Ford Foundation officer Enid Schoettle had to conclude 1979, “if the private sector is to retain a continuing research capacity capable of ‘bending into the wind’ on crucial security issues when needed, it cannot rely upon the vagaries of government support.” That same year even ACDA had to agree with her, writing in its “Report to Congress on Arms Control Education and Academic Study Centers” that academic arms control “is close to being of a sole source nature,” the sole source being the Ford Foundation. Schoettle concurred: “the Ford Foundation is the only private institution in the world with a major program in this quintessentially public subject.”¹⁹¹

Perhaps there was something appropriate in that. Maybe, contrary to the venerable dictum that arms control should be consonant with military and national security policy, there *was*—in practice, if not in theory—a basic opposition between them. The U.S. government, after all, was proprietor of the most destructive military force on earth, the negotiator of “arms control” agreements that permitted high-accuracy nuclear warheads to grow like weeds in

¹⁸⁹ The arms control impact statements had been inspired by Nixon administration’s new regime of environmental regulation.

¹⁹⁰ Publication List, Program in Science and Technology for International Security Kosta Tsipis, in Box 1, Folder “PSTIS Cumulative lists and publication lists,” *KTP*; Robert Cooke, “New missile assailed by MIT group,” *Boston Globe* (24 March 1978): 7; “Underground MX missile basing faulted,” *Washington Post* (26 March 1978): A4.

¹⁹¹ Enid C.B. Schoettle, “FF Programs in International Security,” March 1979, Ford Foundation Report 006503, *FFR* (ACDA report quoted in the same); Ford Foundation Grant 07700365, *FFG*.

summer. “In the arms control field, privately sponsored analysis of government policies is valuable to the national interest,” said the lofty words of one Ford Foundation report from more optimistic days in 1963. But at the end of the 1970s, from the malaise of the Carter era, Enid Schoettle saw foundation support as an urgent corrective to government irresponsibility. The state wouldn’t necessarily consider with sense and wisdom “the policy issues involved in raising, using or threatening to use military forces...” And that, “perhaps ironically...makes the field an urgent and continuing priority for a private foundation.”¹⁹²

The Ford Foundation’s undertaking in arms control in the 1970s had not been without its own ironies. The most coherent, forceful arms control opposition had amassed in the splendid isolation of California, in an academic arms control seminar created with Ford Foundation assistance. When those ideas and people worked their way into ACDA—the most important arms control voice in the executive—Ford had been driven to make an even heavier investment later in the decade. For all the trouble and disappointment with the international negotiations, the arms controllers and their patrons had come to appreciate one thing for certain: there was no arms control without experts—without people—and no expertise without the hard work of building institutional structures to support it. “Arms control begins at home,” said Franklin Long in 1975. He’d been the first director of the ACDA science and technology bureau in 1961, and was a recipient of Ford Foundation funding for arms control research and teaching at Cornell University in the 1970s. He knew what he was talking about.¹⁹³

¹⁹² Eric Stevenson and John Teeple, “Research in Arms Control and Disarmament, 1960 – 1963,” 30 September 1963, Ford Foundation Report 001254, *FFR*; Enid C.B. Schoettle, “FF Programs in International Security,” March 1979, Ford Foundation Report 006503, *FFR*

¹⁹³ F.A. Long, “Arms control from the perspective of the nineteen-seventies,” *Daedalus* 104, no. 3 (1975): 1-13, on 2.

CHAPTER 5

Assessing Star Wars: Arms Control and Objectivity in the 1980s

When scientists are drawn into the pulling and hauling of “politics,” what happens to the freedom and objectivity of science or scientists?

— Albert Wohlstetter (1963)¹

Can independent researchers working in the arms-control field maintain their integrity and independence of the defense establishment? Will dissidents be frozen out, leaving the field to supporters of current government policies?

— The Ford Foundation, Board of Trustees (1973)²

An Invitation to the White House

On the evening of March 22nd, 1983, McGeorge Bundy received a telephone call at his home in New York City. The caller, from the Office of the White House Social Secretary, wondered if Bundy would be interested in coming down to Washington the next day to attend a special dinner and a briefing at the White House. Bundy was a few years out from his retirement as president of the Ford Foundation, now a professor of history at New York University. It was a surprising invitation. Only five days earlier, he had published an essay in the *New York Review of Books* distancing himself from the “panacea” of unilateral disarmament on one side, and “the miscellaneous nuclear expansionism of the Reagan administration” on the other. At the White House Bundy would be part of a special group: after dinner, the VIPs would collectively watch a

Notes

¹ Albert Wohlstetter, “Scientists, seers and strategy,” *Foreign Affairs* 41, no. 3 (1963): 466-478, on 466.

² Ford Foundation Board of Trustees, “Summary of Discussion on Foundation and Arms Control Studies,” December 1973, Ford Foundation Report 010926, *FFR*.

televised address to be delivered by the President. The speech would be on the defense budget, but no further details could be divulged at the moment. Slightly perplexed, Bundy slept on it and, the next morning, accepted the invitation and flew to Washington. “My own expectation was that we be treated to nothing more than a dog and pony show on the Soviet military buildup,” his notes later said.³

When Bundy arrived at the White House reception hall that evening, he was surprised to find himself in the company of “a number of physicists.” And yet he “did not grasp the significance of their presence until the sessions began at about 6:15.” Ushered into a private room, the group was given copies of the President’s address—mostly the standard early-Reagan-era fare about the Soviets having caught up to and overtaken the U.S. in virtually every major area of national security and military defense, nuclear and otherwise. “But the climactic paragraphs of the speech,” Bundy noted, “and the other background document[s.] were about a Presidential proposal for a new start toward... a defense against ballistic missiles carrying nuclear warheads.” Reagan himself popped in and chatted with the group briefly, telling them he hoped they would support his new proposal, before disappearing again to prepare for his speech.

The briefers arrived at 6:30. There were three of them: the deputy national security advisor Robert McFarlane, the President’s science advisor George Keyworth, and the Under Secretary of Defense for Policy, Fred Iklé. The three officials made the case for the administration’s proposal: a total missile defense shield, one that would render nuclear weapons “impotent and obsolete,” as the memorable text of the President’s speech worded it. “The skeptical questioning began at once,” Bundy wrote. How did this missile defense program, whose technical outlines remained vague, fit into the defense budget, where there was apparently

³ McGeorge Bundy, “Memorandum for the Files, Meeting at the White House, March 23, 1983,” Box 126, Folder “Subject Files: The Anti-Missile Flap,” *JFK-MGB*; McGeorge Bundy, “A matter of survival,” *New York Review of Books* (17 March 1983), <http://www.nybooks.com/articles/archives/1983/mar/17/a-matter-of-survival/>.

no room for it? How would it steer clear of violating the ABM Treaty of 1972, a landmark arms control agreement that put heavy restrictions on missile defense? What about the scope of the system? Would it protect “populations as well as weapons”? An old and important issue—one at the burning heart of the ABM debate of the late 1960s. Iklé confirmed that a “general defense was sought.” The physicist Hans Bethe, a distinguished missile defense critic, immediately objected “that a new effort of this sort would only intensify an arms race by adding a defensive element which would in turn spur increased offensive efforts.” That was the classic arms control argument against missile defense.

But there was something new—in degree, if not in kind—in the officials’ response to the arms control criticisms. It could only be called technological optimism, and the administration seemed to be drawing from unusually deep reserves of it. The assembled group demanded to know what technologies would make up the system. Keyworth answered that the core of the program would be in “directed energy devices”: lasers, particle beams, and the supporting microelectronics. Bundy noted that Keyworth “left out the particular scheme which has had oracular support from [Edward] Teller in recent months,” the X-ray laser, a highly classified weapon of magnificent technical ambitions, designed at the Lawrence Livermore National Laboratory and apparently powered by thermonuclear explosions. Teller, who was present, leapt in and delivered a “characteristically passionate intervention,” Bundy recorded, arguing that “the opportunities here were great.” Keyworth added that as an experimental physicist—unlike Bethe, a theorist—he had greater confidence in the technical possibilities of defense. Iklé joined this hopeful outlook, remarking that in any case it was better for the superpowers to compete in defense rather than offense. It was a competition that was safe to win.⁴

⁴ The argument that a competition in defense was better than a competition in offense, too, was also a timeworn one, made prominently in the 1960s by Donald Brennan and Freeman Dyson. McGeorge Bundy,

At 8:00pm, television cameras turned toward Reagan, seated at his desk in the Oval Office, and he delivered his address. Toward the end of the speech, the President asked American scientists to push the technological limits, to make the world safe from nuclear weapons. For Reagan, missile defense was the surest form of arms control. It was through science and technology, he said, that the U.S. would finally achieve the “ultimate goal of eliminating the threat posed by strategic nuclear missiles.”⁵ The message was punctuated with notes of rejuvenated patriotism and the inviolable security and peace of the homeland. It seemed a momentous speech. Historian Edward Linenthal would later write that the President’s “Grand Vision” for strategic nuclear defense was “one of the most revealing cultural fashions of our time.”⁶

Equally revealing was the ensuing bitter debate Reagan’s proposal ignited among experts, politicians, journalists, and other commentators. McGeorge Bundy, who caught the 9:30 flight home to New York, spent the next couple of days writing an op-ed essay for the *Washington Post*. He called the short, missile defense portion of Reagan’s address “an astonishing passage,” not because of any technical proposal the President had made—Reagan had been murky on the details—but because of what Bundy regarded as Reagan’s failure to consult his expert advisors before going ahead with the announcement. Bundy, at work on a sprawling history of the nuclear age at the time, invited readers to consider Franklin Roosevelt’s decision to pursue the bomb program in 1941. That program had emerged “in immediate response to a firm and clear recommendation from Vannevar Bush, a scientific administrator of the first order,” and was

“Memorandum for the Files, Meeting at the White House, March 23, 1983,” Box 126, Folder “Subject Files: The Anti-Missile Flap,” *JFK-MGB*. News of the X-ray laser, developed under the guidance of George Chapline at Lawrence Livermore National Laboratory, first made the rounds in 1981: Jeff Hecht, “The strange story behind the X-ray laser,” *New Scientist* 92, no. 1275 (15 October 1981): 166.

⁵ The text of Reagan’s “Address to the Nation on Defense and National Security” can be found at <http://www.reagan.utexas.edu/archives/speeches/1983/32383d.htm>.

⁶ Edward Tabor Linenthal, *Symbolic Defense: The Cultural Significance of the Strategic Defense Initiative* (Urbana, IL: University of Illinois Press, 1989), xiii.

founded on the “brilliant” work of high-caliber scientists. Their recommendation had led to an “enormous and lonely decision” by FDR—“not to a speech, but to action.” This bit of history made Reagan’s “decision” look cheap by comparison, poorly thought-out, all for show: “Does his proposal rest on any new scientific insights? His advisers have told the press it does not, and scientists of the first rank, with access to all our present secrets, confirm the absence of any new idea remotely comparable” to the discovery of uranium-235’s explosive properties. As far as the offense-defense relationship was concerned, nothing significant had changed since Bundy’s days as national security advisor to Kennedy. “If our leading men of science could in fact see a good way to put the defense truly ahead of the offense in strategic weapons, we would indeed have a great choice to make. But they don’t, and so we don’t.”⁷

But did they? That would turn out to be a major point of dispute. Reagan’s Strategic Defense Initiative (SDI) would become not only one of the most technologically ambitious defense programs of the late Cold War, but also the most loudly and publicly contested. It was a headline issue in arms control’s public age. “SDI is arms control,” Reagan would say in 1985, while the arms controllers could not have disagreed more.⁸ Soon after Reagan’s announcement, several expert panels were formed, both within the government and by independent organizations, to assess the feasibility and wisdom of the program. Congress’s Office of Technology Assessment, the Union of Concerned Scientists, and the American Physical Society (APS) each conducted important external studies of SDI.⁹ And each of these studies yielded

⁷ McGeorge Bundy, “A ‘quick-trigger’ talk,” *Washington Post* (28 March 1983): A11; McGeorge Bundy, *Danger and Survival: Choices About the Bomb in the First Fifty Years* (New York: Random House, 1988).

⁸ Reagan quoted in Paul Lettow, *Ronald Reagan and His Quest to Abolish Nuclear Weapons* (New York: Random House, 2005), 157.

⁹ In examining expert responses to SDI and framings of the SDI debate, I follow the excellent recent work of Rebecca Slayton. See Rebecca Slayton, *Arguments that Count: Physics, Computing, and Missile Defense, 1949–2012* (Cambridge, MA: The MIT Press, 2013), esp. Chapter 8, “Nature and Technology in the Star Wars Debate,” 173–198. Whereas Slayton compares and differentiates the responses of physicists and computer experts, here I dwell more on the role of physicists in assessing directed energy weapons technology, the specific arms control

negative assessments of the program—as a support to nuclear deterrence, as a move in the arms race, and especially as a technological system. In this chapter I focus on the formation, politics and impact of the American Physical Society Study Group on the Science and Technology of Directed Energy Weapons (APS-DEW). Initiated in 1983, convened in 1985-86, and published in 1987, the APS-DEW study group and its report figure prominently in the history of the SDI controversy. With its report, the study group mounted arguably the most extensive and pointed critique of some of the exotic weapons technologies—including high-powered lasers and particle beams designed to destroy ballistic missiles and their reentry vehicles—under development by the Strategic Defense Initiative Organization (SDIO).

Issues Technical and Political

Historians of the Reagan era, and other commentators on the history of arms control, have often taken sides—explicitly or implicitly—in the SDI debate. More than thirty years have passed since Reagan publicly asked American scientists, “those who gave us nuclear weapons,” to use their knowledge to protect the United States from those same weapons. The public incredulity began when Senator Ted Kennedy, in a commencement address at Brown University in June of 1983, famously ridiculed the Reagan administration’s proposed B-1 bomber as “a supersonic Edsel in the sky,” and its missile defense plans as a “Star Wars scheme for outer space.” It is tempting now to remember SDI in a similar spirit: as a fantasy, a technological fiction, almost comical in its grandiosity and naïve enthusiasm.¹⁰ Some, however, have stressed

implications of SDI, and the importance of objectivity in a complicated, mixed environment of classification and publicity.

¹⁰ Edward M. Kennedy, “Star Wars Speech,” Address at the Brown University Commencement Forum, 4 June 1983, available at http://tedkenedy.org/ownwords/event/cold_war. A classic skeptical history of SDI is Frances Fitzgerald, *Way Out There in the Blue: Reagan, Star Wars and the End of the Cold War* (New York: Simon & Schuster, 2000).

SDI's origins as a legitimate (or at least thinkable) arms control response to the increased nuclear tensions of the early 1980s and the disappointing record of arms control over the previous two decades. SDI, after all, did not emerge from thin air (or the daydreams of a mad scientist); it was founded on more than two decades of research and development in numerous aspects of missile defense.¹¹ The goal of this chapter is not to settle debate about whether or not SDI would have "worked." Rather it is to examine the debate's features and implications. The claim by some that impenetrable nuclear defense was simply impossible, measured against the laws of physics and the limits of technology, did not reduce the seriousness and vigor with which such a goal was supported and debated.

Both those who plumped for SDI and those who ridiculed it made use of a political strategy that seemed increasingly necessary, even obvious, in the midst of the fractious and cacophonous defense and arms control disputes. They claimed to reject politics, appealing to an ethos of strenuous objectivity, staying on one side of a walled border between the "technical" and the "political." For the APS-DEW study members, preserving a posture of technical objectivity was of paramount importance. Reaching for objectivity, they and their rivals sought political invulnerability—a space where hard and uncompromising facts would decide one way or the other, dispelling doubt and silencing debate. But in their heavy reliance on the stance and rhetoric of scientific detachment, and their attempts to avoid making "political" arguments, it proved impossible to entice skeptical parties on either side. The need for objectivity had become so entrenched because the disagreements were already so deeply political.

The APS was a prestigious professional organization, and it presented the DEW panel as uniquely positioned to fairly adjudicate SDI's prospects. Yet critics produced their own equally

¹¹ See, for example, Paul Lettow, *Ronald Reagan and His Quest to Abolish Nuclear Weapons* (New York: Random House, 2005). For a deeper history of SDI's technical origins in earlier defense programs, see Donald R. Baucom, *The Origins of SDI, 1944-1983* (Lawrence, KS: University Press of Kansas, 1992).

well-credentialed experts who rejected the report's conclusions. Each side accused the other of "political" motivation, in an attempt to damage the opposing side's credibility and authority. The APS had abstained from even a modest policy recommendation on SDI for almost the entire duration of the study—the better part of four years, during which SDI hit full stride as a federal program. But at the conclusion of the DEW study the Society decided to issue a formal statement condemning SDI. Published alongside the APS-DEW report, the official APS statement criticized SDI as unsound technically and undesirable politically. But the move proved disastrous for the APS, when its own DEW panel members publicly rejected the official APS position as inappropriate—an unseemly "political" gloss on what had been a purely "technical" assessment. In a quick retreat, the study members immediately distanced themselves from their APS sponsors. Poor coordination and incompatible expectations had thus turned the episode into a debacle for the APS and the DEW study, and the study's critics quickly pounced on the mistake.

The relationship between the modern image of science and notions of objectivity—quantification, fact, reliability, trust, disinterestedness, and similar concepts—has a long history. For many who came of age in the Cold War, it had long been conventional to assume that science occupied a realm distinct from politics.¹² Politics involved the collision of interests, but science was supposedly unmotivated by worldly interests. Such was science's unique advantage in the political forum—that it could inform political action and expand the scope of public knowledge, but remain unsullied above the fray of power and faction.¹³

¹² Lorraine Daston and Peter Galison, *Objectivity* (Brooklyn, NY: Zone Books, 2007); Andrew Jewett, *Science, Democracy, and the American University: From the Civil War to the Cold War* (New York: Cambridge University Press, 2012).

¹³ For an insightful explication of modern notions of objectivity—applied in this case to the realm of economic statistics—see Thomas A. Stapleford, *The Cost of Living in America: A Political History of Economic Statistics, 1880–2000* (New York: Cambridge University Press, 2009), especially the Introduction (1–15) and the Epilogue (382–394). For similar themes in a different key, see Sheila Jasanoff, *The Fifth Branch: Science Advisors as Policymakers* (Cambridge, MA: Harvard University Press, 1990).

Scholars have worked to show how the opposition between the objective and the political was made, not found—constructed, by interested actors, in history. Ted Porter argues that quantification and objectivity were firmly attached to the modern conception of science as early as the nineteenth century. Porter showed how governments came to rely less on local and elite forms of craft-like expertise to manage society, and more on bureaucratic, standardized, rule-bound, “mechanical” forms of objectivity to govern distant populations in increasingly complex societies. So where the conventional wisdom saw technical experts as indispensable for conducting the affairs of state *because* of their objectivity, Porter said the situation was historically reversed—that bureaucratic experts had been stamped with objectivity by their state sponsors to enhance the state’s administrative authority.¹⁴

The connection between science, objectivity, and political authority remained unstable throughout the modern era. In an important recent book, Kelly Moore demonstrates that during the first two or three decades of the Cold War era, scientists themselves played pivotal roles in unbinding public authority from science, fastening it to one or another social group. Scientists, Moore claims, participated in this diminishment of their own authority by allying with broader social movements for progressive change. In Moore’s telling, a “liberal” ideal of elite-informed public debate gave way to more “radical” commitments and tactics beyond the 1960s. The result was a contraction of the scope of American scientists’ public power, and the effective retreat of science from politics.¹⁵ To an important degree, the APS-DEW episode repeats this theme of political posturing and political retreat. Resolutely rejecting advocacy of any kind, the study group members nevertheless engaged in the fully political activity of extending a rhetorical

¹⁴ Theodore M. Porter, *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life* (Princeton, NJ: Princeton University Press, 1995). Also see Theodore M. Porter, “How science became technical,” *Isis* 100 (2009): 292–309.

¹⁵ Kelly Moore, *Disrupting Science: Social Movements, American Scientists, and the Politics of the Military, 1945–1975* (Princeton, NJ: Princeton University Press, 2008).

barrier between their technical efforts and the world of politics. Praising the power of objective science to inform policy debates, the APS made a drastic policy recommendation (to abandon the dream of missile defense), only to alienate its cautious study group members, who continued to insist that their report was a study not of strategy or policy, but of raw technological reality.

Yet in contrast to Moore's narrative of an eclipsed liberal ideology, the APS-DEW episode also highlights the dogged persistence of a liberal faith in public communication and public education. This was, after all, a marquee defense and arms control issue, trumpeted by a highly telegenic President.¹⁶ Since the ABM debate of the late 1960s, missile defense had been a subject for public debate. It was eclipsed by other issues in the 1970s, not to mention the signing of the 1972 ABM Treaty (which severely limited deployed defenses), but had never been extinguished from public consciousness. By the 1980s, the missile defense debate bore a distinct resemblance to other expert controversies of the 1970s, many of which had been conducted in high visibility. The National Academy of Sciences (NAS), for example, orchestrated numerous expert-led studies for a public audience in the 1970s and beyond, on issues ranging from environmental concerns to public health.¹⁷ Stephen Hilgartner has suggestively compared the conduct and presentation of NAS public reports to theatrical performances, showing how experts and their sponsors manage expectations and appearances in order to protect credibility in the face of controversy.¹⁸ Likewise, the APS-DEW episode witnessed all manner of stage management and theatrical posturing: the APS leadership and the panel members squabbled about how the

¹⁶ As Daniel Rodgers says of Reagan's carefully crafted public image, "With the right occasion and the right speechwriter, the tropes that had dominated the mid-twentieth-century presidency would be reinvigorated, propelled by Reagan's exceptional ability to project his own inner confidence and conviction across the television screen." Daniel Rodgers, *Age of Fracture* (Cambridge, MA: Harvard University Press, 2011), 23.

¹⁷ The bipartite structure of the APS-DEW study, with an expert panel adjudicated by an independent executive committee, was consciously and intentionally patterned after the NAS studies.

¹⁸ Stephen Hilgartner, *Science on Stage: Expert Advice as Public Drama* (Stanford, CA: Stanford University Press, 2000).

report should be presented to the public. Consensus on whom, exactly, comprised the report's "public" proved elusive.

Yet the APS study was importantly different from other public studies of complex technical issues, just as it was different from other assessments of SDI (such as that carried out by the Union of Concerned Scientists). The distinguishing feature of the APS-DEW study was its unique relationship with the Defense Department and its access to classified information (which the UCS study, among others, did not have). As this chapter demonstrates, the APS-DEW episode illustrates the career of objectivity and political advocacy in an environment of secrecy.

By definition, SDI was not open to the same degree of public scrutiny as many other federal programs. The APS and its study group wrestled constantly with a fundamental paradox of classified access: to conduct a study of a highly classified program *without* access to crucial secret information would reduce the study's credibility; but to *have* access was to court the distorting and constricting influences of the defense establishment's interests, perspectives, and restrictions. Thus there was no way for the report to be both authoritative and independent at the same time. Despite such dilemmas, as we shall see, the APS and its experts embraced classified access as soon as it was offered to them. The embrace came at a cost, as the APS surrendered significant authority over what the report could say, and the crucial timing of its public release, to the Defense Department. By basing their study on information provided by the SDIO, the APS panel members also relinquished control over the publication date to officials who were in charge of declassifying the report. Thus, when it was finally released, the report could be charged not only with being politically biased but (which was perhaps worse) being simply out of date and uninformed.¹⁹

¹⁹ Secrecy and its history have received more attention from scholars of late. See Peter Galison, "Removing knowledge," *Critical Inquiry* 31 (Autumn 2004): 229-243; Alex Wellerstein, "Knowledge and the Bomb: Nuclear

The Origins of the APS-DEW Study

In early April of 1983, Los Alamos National Laboratory threw a party. The Lab was celebrating the fortieth anniversary of the beginning of the Manhattan Project by gathering together scientists who had helped build the first nuclear weapons. The Reagan administration's new plan for missile defense was on everyone's minds. To a full auditorium of meeting attendees (and to CBS television cameras) Richard Garwin and Hans Bethe spoke out. They declared that SDI's secret weapon—the notorious X-ray laser—had been vastly over-hyped by supporters like Edward Teller.²⁰ Amid the tense and excited atmosphere of the conference, Robert Marshak (a famed nuclear physicist and Manhattan Project veteran) struck up a conversation with the current director of Los Alamos National Laboratory, Donald Kerr. Both men were cautious skeptics of missile defense. The two agreed that perhaps the most effective way for physicists to weigh in publicly on the issue was by commissioning a study through the discipline's most prestigious professional organization, the American Physical Society (APS).²¹

Marshak, a past president of the APS, shared the idea with his former Los Alamos colleague William W. Havens, the longtime Executive Secretary of the Society. Within weeks Havens had drafted a proposal for the consideration of the APS Executive Council, calling for a study of missile defense to be carried out by the APS Panel on Public Affairs (POPA).

Secrecy in the United States, 1939–2008” (PhD dissertation, Harvard University, 2010); Hugh Gusterson, *Nuclear Rites: A Weapons Laboratory at the End of the Cold War* (Berkeley, CA: University of California Press, 1996); Hugh Gusterson, *People of the Bomb: Portraits of America's Nuclear Complex* (Minneapolis, MN: University of Minnesota Press, 2004).

²⁰ Richard L. Garwin and Kurt Gottfried, “Hans in war and peace,” *Physics Today* 58, no. 10 (2005): 52-57. On some of the hype surrounding the x-ray laser, see William J. Broad, *Star Warriors* (New York: Simon & Schuster, 1985).

²¹ See Oral history interview with William W. Havens, conducted by Ronald Doel, Session VI, 14 August 1991, available at http://www.aip.org/history/ohilist/31723_6.html; and Garwin and Gottfried, “Hans in war and peace.”

Discussion among APS officials proceeded throughout the spring of 1983. Soon a “Consultants Group on Arms Control” was established, headed by POPA Chairman Elect L. Charles Hebel, a physicist working at the Xerox Palo Alto Research Center. The Consultants Group included among its members Hans Bethe, the staunch arms controller Herbert York, and the Harvard physicist Nicolaas Bloembergen, who’d consulted on laser antiballistic missile weapons for the Institute for Defense Analyses in the 1960s, and was by now a Nobel Prize-winner for his discoveries in nonlinear optics. The group met throughout the late summer and early fall of 1983 and began to assemble a proposal for funding and institutional support.

As an early draft of the Consultants Group’s proposal put it, the study group would aim to “facilitate responsible public discussion” about the most ambitious and expensive aspect of Reagan’s missile defense plans—so-called directed energy weapons—by “evaluating the physical basis, the technological feasibility and the likely implications of such weapons.” The study planners did flirt briefly with the idea of producing a report tackling technical *and* strategic/political questions. One draft of the proposal suggested that the group might examine the full implications of missile defense, including the relation between the Reagan administration’s plans and existing (as well as possible future) international arms control treaties. But the scope of the planned study quickly narrowed. It would “emphasize physical analysis and technological evaluation,” pointing out the policy implications of its technical assessments while delicately tiptoeing around direct policy recommendations.²² “Proponents and critics agree,” the proposal continued, “that these weapon systems, if fully developed and deployed, would constitute a critical turning point in the arms race. Such a decision deserves extensive comment

²² Hebel to APS Panel on Public Affairs, 28 September 1983, Box 2, *APS-DEW*.

from an informed public who cannot participate responsibly without being aware of the technological foundations.”²³

Three tightly connected sets of questions would come to dominate the planning and structuring of the study. Each bore directly upon the experts’ claims to independence and objectivity. One: how would the panel be funded, and by what organizations? Should the APS seek any government funding at all, or should it intentionally steer clear of official government patronage? Second: what relationship should the study have to the government more generally, especially to the Reagan White House and the Department of Defense? The issue of classification was crucial here. Initially, members of the Consultants Group had proposed to conduct an unclassified study based entirely on unclassified material—scientific and technical literature in the public domain, and various unclassified estimates made by a range of experts.²⁴ If the report were based entirely on unclassified sources in the public domain, would it not make itself vulnerable to easy challenges by military officials who could claim that the panelists had not had access to all the relevant facts in making their judgments? But if the study group *did* have security clearance, didn’t it risk compromising its objectivity by cozying up to the defense establishment, subjecting its experts to the distorting bias of the military’s selective and partial briefings? With or without classified access, what would it even mean to issue an open report on secret subject matter? And three: what sort of experts would actually sit on the panel? Should the study group be composed entirely of scientists and engineers from outside of the defense world? Would it be more advantageous to include some career weapons experts? Should the study draw from the pool of those experts who had already adopted public positions concerning SDI? Or would that also threaten the study’s perceived neutrality and objectivity?

²³ Hebel to APS Panel on Public Affairs, 28 September 1983, Box 2, *APS-DEW*.

²⁴ Hebel to APS Panel on Public Affairs, 28 September 1983, Box 2, *APS-DEW*.

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The persistent question of how the study would be funded was never fully resolved to the satisfaction of the APS. In November 1983, the study group's proposal had received a hearing before the APS Council, where it presented a budget of \$648,000.²⁵ The budget in the coming months would rise only slightly above this figure—to \$660,000—though it would not, in the end, be met entirely by external sources. At the end of April in the following year, the APS's William Havens made a request to the National Science Foundation for a third of the required funding.²⁶ A few days later, Havens made similar requests to the Ford and Rockefeller Foundations, asking for \$150,000 from each.²⁷ By June 1984, the Carnegie Corporation of New York informed APS President Mildred Dresselhaus that it had recently approved a \$200,000 grant to the APS for the study.²⁸ The situation looked promising in the early months, though responses from potential patrons were slow.

The issue of classified access evolved slowly as well. In December of 1983 Richard De Lauer, Under Secretary of Defense for Research and Engineering, wrote to Charles Hebel, now Executive Director of the APS-DEW study, to lend his office's full support. "I believe that directed energy and other strategic defensive technologies could eventually provide, along with offensive arms control, a more balanced approach to the Nation's nuclear deterrence posture." De Lauer was a stalwart Cold Warrior and a believer in missile defense; only the year before, he had predicted that the Soviet Union was within eight years of deploying spaceborne battle stations armed with high-powered lasers that could attack targets in space or on land.²⁹ To Hebel

²⁵ Hebel to POPA Study Consultants, 7 November 1983, Box 2, *APS-DEW*.

²⁶ Havens to Edward A. Knapp, 30 April 1984, Box 3, Folder 1 "1984," *APS-DEW*.

²⁷ Havens to Ms. Enid Schoettle, 1 May 1984; and Havens to Dr. Edwin Deagle, 1 May 1984, Box 3, Folder 1 "1984," *APS-DEW*.

²⁸ Sara L. Engelhardt to Mildred S. Dresselhaus, 20 June 1984, Box 3, Folder 1 "1984," *APS-DEW*.

²⁹ Christopher Joyce, "America debates extra cash for space weapons," *New Scientist* 93, no. 1296 (11 March 1982): 644.

he continued: “Based upon my sincere conviction that the President’s defensive technology initiative could lead to a more correct course for deterring nuclear war in the future, I believe that an independent and impartial study conducted by a prestigious professional organization, such as the American Physical Society, could be highly beneficial in coalescing scientific opinion and creating an informed public opinion in fulfillment of the President’s aims.” De Lauer’s expression of support was a breakthrough, since it promised at least some degree of cooperation from the Department of Defense, without the APS having explicitly asked for it. Brigadier General Robert Rankine, De Lauer’s Assistant for Directed Energy Weapons, was to act as the study’s official contact within the Defense Department, issuing unclassified documents and organizing unclassified briefings for the study group.³⁰ In other words, the Department of Defense would cooperate, but it would not open its vaults. De Lauer offered one additional piece of advice that would prefigure the ensuing controversy: he suggested that the group stick to its “recognized expertise, namely physical analysis, and to refrain from clouding the study with policy evaluations which could detract from its technical credibility.”³¹

In the following weeks the APS made further inroads with the Defense Department and the administration. Solomon Buchsbaum, chairman of the White House Science Council and a high-level adviser to the Strategic Defense Initiative, first put Havens and the APS in contact with General James Abrahamson, head of the SDIO. Informally, Havens and Abrahamson brokered a deal to give APS study members access to classified information, even if military officials would not brief the group as a whole. On December 17th, 1984, APS President Dresselhaus spoke over the telephone with Ralph DeVries, an official with the Reagan administration’s Office of Science and Technology Policy (in charge of the OSTP’s general

³⁰ De Lauer to L. Charles Hebel, 12 December 1983, Box 2, *APS-DEW*.

³¹ De Lauer to L. Charles Hebel, 12 December 1983, Box 2, *APS-DEW*.

science and nuclear physics programs). According to DeVries, George Keyworth, head of the OSTP and Reagan's science advisor, was concerned that the APS-DEW study would not have access to classified information. Dresselhaus reassured DeVries that, indeed, several members of the study, having worked in classified defense contexts before, would be permitted access to such information. Keyworth had requested a briefing package on the DEW study from the APS to bring to Reagan's attention, including the original research proposal, and information concerning the funding of the study—particularly the source and nature of the study's federal funding. (Would all of it come from the NSF? Or should portions also originate in the Department of Energy and the Department of Defense?)³²

The following day, Robert Park in the APS's Public Affairs Office sent off the package of information to DeVries. To Park, it was obvious that the APS-DEW study could never be successfully undertaken without the government's cooperation. Moreover, "however valid the study, its usefulness would depend on the public's perception that the study was neither co-opted by the Defense establishment nor antagonistic to U.S. policy." The panel's independence and objectivity was to be reflected chiefly, he argued, in the balance of its funding: two-thirds from private philanthropy and one-third from the NSF, a federal but non-military source.³³

Still having heard nothing from the NSF well into 1985, the incoming APS President Robert R. Wilson appealed yet again to the NSF, this time to its new director Erich Bloch. The change of directorship was to present a serious obstacle. Whereas the previous director Edward Knapp had warmed to the idea of supporting the APS study, Bloch was uneasy about associating NSF money in any way with the handling of classified information. The NSF's traditional scope

³² "Conversation with De Vries," 17 December 1984, Box 3, Folder 1 "1984," *APS-DEW*. It seems unlikely that Keyworth ever made Reagan aware of the study group, its activities, or its final report. In the various accounts of the Reagan administration's deliberations and politicking around the Strategic Defense Initiative, there is no record of Reagan having issued an opinion on the APS-DEW study or its findings.

³³ Park to DeVries, 18 December 1984, Box 3, Folder 1 "1984," *APS-DEW*.

had always been the promotion of civilian science. Wilson assured Bloch that while it was “true that several members of the Study Group...will be familiar with the classified aspects of this subject and that unclassified but important briefings will be made to the APS Study Group by the DOD and others,” nevertheless “the classified information available to some Study Group members will not be discussed at the meetings of the APS Study Group....”³⁴ The appeal did not move Bloch—nor, indeed, was it truthful, since the APS leadership and the study group had concluded in the previous months that the group *would* indeed discuss classified material. The final blow arrived later that month, when Bloch wrote back to Wilson with a terse rejection: “It is my judgment that it is not in the best interests of the Foundation to be involved in supporting the proposed study.”³⁵ It was not until May 1985 that the John and Catherine MacArthur Foundation pitched in with a grant to match Carnegie’s with an additional \$200,000.³⁶ But no further external funding for the study would be forthcoming; the APS was saddled with the remainder of the bill, and its pursuit of federal dollars would come up empty.

Selecting the study’s membership was somewhat easier. Harvard professor of physics Nicolaas Bloembergen and AT&T Bell Laboratories staff scientist C.K.N. (Kumar) Patel agreed early on to serve as co-chairmen of the panel. Both were distinguished laser scientists. Bloembergen was one of the pioneers of American solid-state physics, an expert in laser spectroscopy who had consulted on missile defense matters in the early 1960s as part of the Institute for Defense Analyses, and a recipient of the 1981 Nobel Prize in physics for helping found a new field known as nonlinear optics (the study of the interaction between matter and intense laser light). Patel had been the principal inventor of the first carbon dioxide laser at Bell

³⁴ Wilson to Bloch, 9 January 1985, Box 3, Folder 2 “January 1985—May 1985,” *APS-DEW*.

³⁵ Erich Bloch to Robert R. Wilson, 24 January 1985, Box 3, Folder 2 “January 1985—May 1985,” *APS-DEW*.

³⁶ Havens to James M. Furman, 21 May 1985, Box 3, Folder 2 “January 1985—May 1985,” *APS-DEW*.

Labs in 1964, and was also a longtime Defense Department consultant.³⁷ Though APS officials had paid some lip service to the idea that the panel should be composed of outsiders to the defense establishment, it was quite clear from the outset that the remainder of the study group would be drawn largely from defense insiders. Initially pegged at fifteen members (though the number would increase to eighteen by the end of the study), several were employed in national weapons laboratories (including members from Sandia National Laboratories and the Air Force Weapons Laboratory), and at least one was a current member of the Jason defense advisory group (no longer a division of IDA, but managed by the MITRE Corporation). Most of the academic panel members had performed consulting work for the Defense Department in the past.³⁸

With the membership of the committee more or less finalized by early 1985, its internal structure was mapped out as well. Six subcommittees would examine various components of the Star Wars system. A “Laser Technology” group, headed by Abraham Hertzberg (a former student of Arthur Kantrowitz, employee of AVCO-Everett, and one of the inventors of the gas dynamic laser), would examine the possibilities opened by new kinds of laser energy—chemical, free-electron and excimer lasers, as well as the nuclear fusion-powered X-ray laser. Another subcommittee, chaired by Kumar Patel, would examine “Particle Beam Technology”—the projection of narrow, focused beams of atomic and subatomic matter (electrons, and positive and neutral heavy ions) to disable ballistic missiles and satellites. A third group was to assess “Beam Control and Delivery,” including the complex optical technologies needed for guiding and shaping the directed energy beams through outer space and the earth’s atmosphere. Other groups

³⁷ For background on Patel see, for example, the IEEE Global History Network web page on Patel at http://www.ieeeahn.org/wiki/index.php/C._Kumar_Patel.

³⁸ “Members of the APS Directed Energy Weapons Study,” Box 3, Folder 2 “January 1985—May 1985,” *APS-DEW*.

examined problems including surveillance and data acquisition, the vulnerability and survivability of system components (in the event of a nearby nuclear blast, say), and general systems issues including cost, power requirements (which were massive), and the timescale of feasible development.

George C. Pimentel, who had originally joined as a co-chairman of the subcommittee on laser technology (he was a laser inventor himself, employed at the University of California at Berkeley), decided in the spring to drop out of the APS panel. He wanted to oppose SDI openly, he explained, and felt that his participation in the study group presented a conflict of interest.³⁹ Pimentel's decision and rationale reflected a larger goal for the organizers of the study: the study group members would have to maintain an appearance of strict impartiality, free from the distortion of preconceived opinion. To have decided one way or the other on the question of SDI's feasibility—much less to have publicly pronounced an opinion on SDI—before the study group had sat for its first meeting was out of bounds. Indeed, as it would turn out, this was precisely the claim that many detractors of the APS-DEW study would later make: that the study's conclusions had been foreordained from day one, and that the group's technical objectivity had been compromised by its political mission.

The Critique of Directed Energy Weapons

The APS-DEW panel would be only the latest in a growing list of major evaluations of directed energy weapons technology. Directed energy weapons, including laser and particle-beam devices, had reentered headlines in the late 1970s amid alarmist claims that the Soviets had acquired a lead in these technologies. "Is the United States in danger of being zapped into

³⁹ Email from Charles Hebel to APS Executive Committee, 20 June 1985, Box 3, Folder 3 "June 1985—December 1985," *APS-DEW*.

submission by Soviet death rays?” asked a *New York Times* editorial in late 1978. “That alarming possibility is being raised again by some weapons-watchers,” especially George Keegan, retired chief of Air Force intelligence. The CIA (and apparently President Jimmy Carter himself) doubted that a “beam-weapon gap” had really opened up. Caution in the area of exotic weapons technologies seemed the order of the day in the Carter administration. In 1979 the assistant director of ACDA, Barry Blechman, told the House Armed Services Committee that “the development of directed-energy weapons could provide the impetus for a new strategic arms race...with unsettling political and military implications.”⁴⁰

But such expressions of concern also came at the end of a decade in which the Defense and Energy Departments had funneled at least a billion dollars into high-power laser R&D. A great deal had happened in the world of high-energy lasers since that fateful meeting of the IDA Laser Advisory Committee almost two decades earlier, in 1961, when Nicolaas Bloembergen argued that a supremely powerful solid-state laser device was possible—at least in principle. Solid-state high-power lasers had given way to gas-dynamic lasers by the late 1960s, which in turn had been superseded by more promising models. Among the most important was the chemical laser. George Pimentel (who would drop out of the APS-DEW study in 1985) had invented the first version of the chemical laser in the mid-1960s. Scaled-up models were built for high power levels at the defense contractor TRW, and elsewhere, in later years. In the chemical laser, two gases (the most common variant used hydrogen and fluorine) were injected into a cavity by a series of nozzles. The gases then reacted chemically, forming molecules (of hydrogen

⁴⁰ Drew Middleton, “U.S. debates Soviet ‘beam’ weapon,” *New York Times* (15 May 1977): 10; “Much ado about death rays,” *New York Times* (10 December 1978): E22; Bernard Weinraub, “Arms-control aide warns of new-weapons race,” *New York Times* (23 April 1979): A7.

fluoride, in the case of the HF chemical laser) in excited states, achieving the “population inversion” necessary for the creation of laser light.⁴¹

The earliest “independent” study of laser weapons had originated at MIT—nearly a full five years before Reagan’s announcement in 1983—in Bernard Feld and Kosta Tsipis’s Program in Science and Technology for International Security (PSTIS). In late 1978 Tsipis had written to arms control program officer Enid Schoettle at the Ford Foundation, requesting support for a “workshop of specialists in high energy lasers, adaptive optics, and arms control issues...to examine the physical limits of feasibility, the technical problems, and the arms control implications” of high-energy lasers. Tsipis’s objectives had been not unlike those of the APS-DEW study years later: to stimulate “reasoned public discussion,” as he put it, and to evaluate laser weapons “in an accurate[,] objective, and independent fashion...” To date, he complained, there had been no “public treatment of any aspect of the weapon,” no “arms control impact statement” concerning lasers. Tsipis wanted to change that.⁴²

His study was very different from APS-DEW in two important respects. On one hand, Tsipis had wanted to look past the “technical” to the “defense policy implications” and “arms control implications” of laser weapons, whereas APS-DEW would try to confine itself completely within the “technical.” And on the other hand, his study sought no access to classified information—Tsipis’s *métier* had always been open-source technical critique—because, as he claimed, “the main feature of directed energy applications to weapons depend on the understanding of basic physical principles,” and basic physical principles were available to everyone. Still, Tsipis had also arranged to involve scientists from government and defense-

⁴¹ For details, see, for example, Kosta Tsipis, “Laser weapons,” *Scientific American* 245, no. 6 (1981): 51–57.

⁴² Program in Science and Technology for International Security, Department of Physics, MIT, “Proposal for the Assembly of a Working Group to Perform an Assessment of the Technological Feasibility and Arms Control Implications of High Energy Laser Weapons,” November 1978, Box 1, Folder “Ford Foundation, 1978–81,” *KTP*.

industrial firms, several of whom held security clearances and worked on classified laser programs. On his list of invitees was a specialist in laser-driven fusion from Los Alamos National Laboratory, a group of high-energy laser experts from AVCO, and other specialists from the Draper Laboratory, Hughes Aircraft, and Rockwell. Rounding out the list were the arms control champion Herbert Scoville (with whom Tsipis had recently authored a report on arms control in outer space), the physicist-consultant Freeman Dyson, and the two eventual chairmen of the APS-DEW study, Nicolaas Bloembergen (whom Tsipis said he invited specifically for his expertise in nonlinear optics) and Kumar Patel.⁴³

Schoettle pondered the funding application, asking a few experienced defense analysts to help her with the decision. One reaction was particularly telling: "I'm not as certain as the [applicants]," the reviewer wrote, "that they can do everything on an unclassified basis." Nevertheless Schoettle awarded Feld and Tsipis the grant in early 1979, and the MIT physicists organized a series of PSTIS workshops on laser weapons over the coming months. By late 1980, Tsipis had coauthored a PSTIS report on lasers with a young MIT research associate named Michael Callaham. In late 1981, Tsipis published a *Scientific American* article on his own, titled "Laser Weapons," in which he offered the conclusions of their research.⁴⁴

Tsipis described the mission of the new laser weapons that had been under development in the 1970s: "boost-phase" missile defense, destroying offensive missiles as they left their silos or submarines, spotted during the few minutes in which their firing rockets released intense heat

⁴³ Program in Science and Technology for International Security, Department of Physics, MIT, "Proposal for the Assembly of a Working Group to Perform an Assessment of the Technological Feasibility and Arms Control Implications of High Energy Laser Weapons," November 1978, Box 1, Folder "Ford Foundation, 1978-81," *KTP*.

⁴⁴ Callaham was a recent MIT PhD in physics, "a serious and concerned young man," as Tsipis related to Feld. Callaham had weighed postdoctoral offers from IDA, ACDA, and the Draper Lab before Tsipis nabbed him for PSTIS. Enid C.B. Schoettle to Bernard T. Feld, 4 January 1979, and attached commentary on the proposal, Box 1, Folder "Ford Foundation, 1978-81," *KTP*; K. Tsipis to B.T. Feld, 11 April 1977, Box 72, Unlabeled Folder 5, *BTF*; M. Callaham and K. Tsipis, "High Energy Laser Weapons: A Technical Assessment," Program in Science and Technology for International Security, Report #6, November 1980, listed in "Publication List," Box 1, Folder "PSTIS Cumulative lists and publication lists," *KTP*; Tsipis, "Laser weapons."

and glare. Boost-phase intercept was advantageous for many reasons. Rockets are more susceptible to damage and failure than reentry vehicles (which are smaller, faster, and harder); and (thanks to MIRV) the number of targets is smaller in the boost phase than during reentry. The lasers would therefore be based in outer space, orbiting high above their potential targets, told when and where to fire their “directed energy” by a network of surveillance and communications satellites.

Tsipis presented the task as an astoundingly onerous one. First, there was the age-old problem of developing laser systems sufficiently powerful to overcome the various obstacles presented by propagation through the atmosphere: “clouds, smoke, dust, fog or thick haze would absorb a beam almost completely,” he wrote. The air, heated and expanded by the laser beam, would also worsen the beam’s ordinary divergence, causing it to lose focus more quickly than in the vacuum of space (a process known as “thermal blooming,” explored by IDA’s Jason physicists in the 1960s). Second, a practical space-based laser system, operating at the power levels required to destroy real targets in the atmosphere, would be so heavy and cumbersome that it would require an astounding technical feat just to put the system in orbit. Most of the bulk would not be in the laser itself, but the laser’s fuel or power source, plus the large mirrors used to reflect and aim the laser light. As with ABM years earlier, many countermeasures were available to the offense. These included techniques to protect its missiles from the laser light: special reflective coatings, for example, or the rapid rotation of the missile (which would quickly distribute and dissipate the light intensity over a wider surface area), or an “ablative” material that would burn off the surface of the missile when struck by the laser light, leaving its substructure safely intact.⁴⁵

⁴⁵ Tsipis, “Laser weapons,” esp. 55–57.

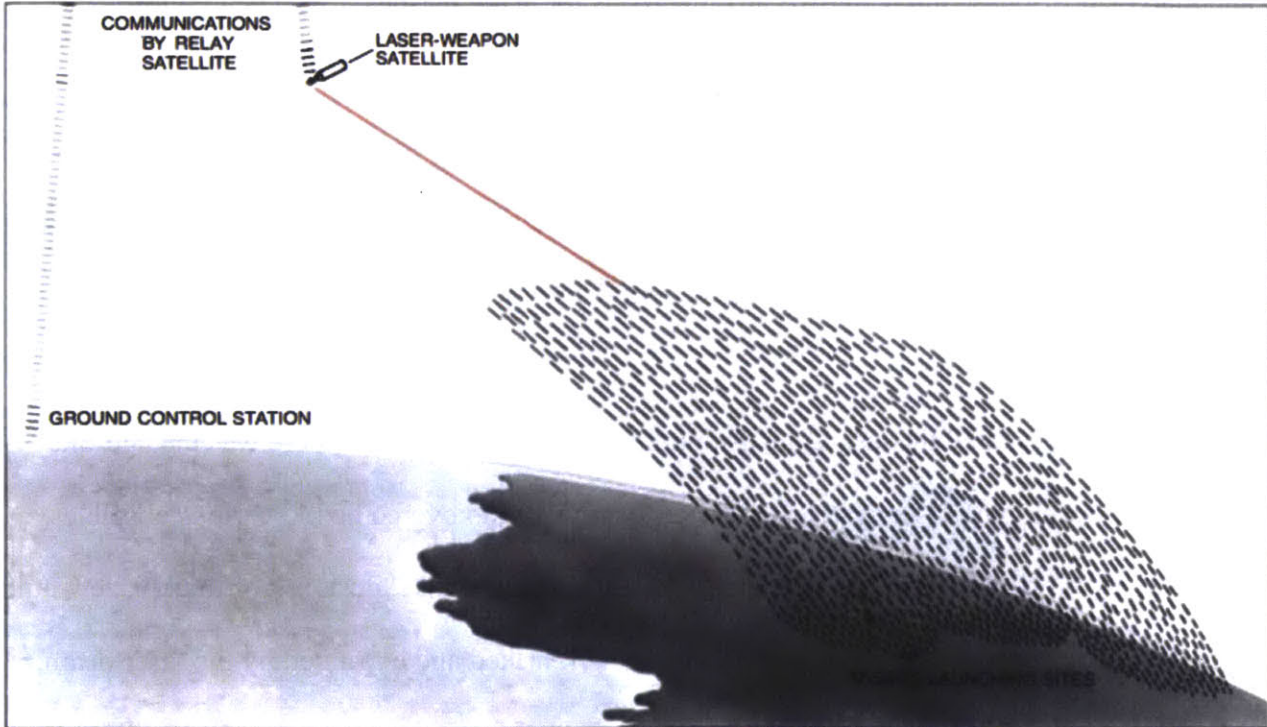


Figure 5.1: Boost-phase intercept of ICBMs by a space-based laser weapon. Here, the artist has sketched an enormous force of launched missiles, opposed by a single laser weapon—the better to emphasize Kosta Tsipis’s argument about the difficulty of boost-phase intercept. From Kosta Tsipis, “Laser weapons,” *Scientific American* 245, no. 6 (1981): 51–57, on 51.

Early the next year, in March of 1982, secret testimony by Under Secretary of Defense Richard DeLaurer was “inadvertently” read in an open session of the Senate. De Laurer warned that a Soviet laser weapon, deployed sometime in the mid-1980s, could destroy U.S. satellites in geosynchronous orbit, among other valuable targets. Senator Malcolm Wallop, a Republican from Wyoming, came out with a statement claiming that space-based laser weapons “would revolutionize the strategic equation...by decisively tipping the balance of modern warfare in favor of the defense.” The *New York Times*, reporting the story, sought the opinion of several experts. As Charles Townes told the newspaper, “It’s a science fiction idea at the moment. It’s a glamorous kind of thing that everyone wants to believe will work, but it’s very much overblown.” Tsipis related to the journalist that his MIT workshops had “concluded that lasers

have little or no chance of succeeding as practical, cost-effective defensive weapons.” In fact, even a statement released by the DOD at the time claimed that the government “does not regard itself to be in a race for laser weapons,” a more cautious approach than De Laurer’s. Reagan’s science advisor George Keyworth, too, had downplayed the short-term threat of laser technology in 1982.⁴⁶

But Reagan’s announcement the following year had put everything in a new light. By the time the APS-DEW study was to get underway in 1985, perhaps the most widely known study of directed energy weapons was one recently authored by Ashton Carter, a young physicist (with a dual bachelor’s degree in medieval history) and Rhodes Scholar who’d redirected his interests toward defense policy as a postdoctoral researcher. In early 1984 he was a postdoctoral associate in the Ford Foundation-funded MIT arms control and defense policy program administered by Jack Ruina and George Rathjens. He’d already done a study for Congress’s Office of Technology Assessment (OTA) on the basing of the MX intercontinental ballistic missile, and had worked as an analyst in the Office of the Secretary of Defense. OTA commissioned an unclassified background paper from Carter in early 1984, this time on “directed energy missile defense.” In the course of Carter’s work, Senators Larry Pressler and Paul Tsongas asked that the OTA provide the report to the Senate Foreign Relations Committee as soon as it was completed. With Reagan’s announcement and the formation of the SDIO the following year, directed energy weapons had become a matter for urgent study by skeptics within the government, as well as outside.⁴⁷

⁴⁶ Philip M. Boffey, “Laser weapons: Renewed focus raises fears and doubts,” *New York Times* (9 March 1982): C1.

⁴⁷ United States Department of Defense, “Ashton B. Carter, Former Deputy Secretary of Defense,” available at <http://www.defense.gov/bios/biographydetail.aspx?biographyid=186>.

Carter's effort was "based on full access to classified information and studies performed for the Executive Branch," he reported, including two DOD studies on directed energy weapons—the "Defensive Technologies Study Team" (the so-called "Fletcher Panel," named for its chairman, James C. Fletcher) and the "Future Security Strategy Study" led by the former RAND analyst Fred Hoffman. It was at once more focused and far broader than Tsipis's had been. Carter restricted himself to evaluating directed energy technologies applicable to a mission he identified as "'Star Wars' proper," that is, boost-phase intercept of land-launched ICBMs by orbiting weapons (and not other types of weapons, or other phases of the trajectory). Tsipis's report given most attention to boost-phase intercept, too, but had roamed more freely, ending with broad recommendations about the importance of industrial laser research. Tsipis's study had also considered multiple ways of achieving boost-phase intercept (not just orbiting lasers on satellites, but ground-based lasers that would fire their beams up to orbiting mirrors, reflecting the beams back downward to their targets).

Carter went into much greater detail about the special logistical issues confronting boost-phase missile defense from orbiting platforms. He discussed something called the "absentee ratio" (a figure that would later become the subject of a dispute between Richard Garwin and a Los Alamos physicist named Gregory Canavan).⁴⁸ As Carter explained, of the few dozen orbiting high-power lasers contemplated for a defensive deployment, given the properties of their orbits, only a few—perhaps two or three—would be somewhere over the Soviet Union at any given time, capable of firing down on any of the 1,400 ICBM launchers dispersed throughout the country. The absentee ratio was the ratio of the total number of orbiting lasers to the number of laser weapons in a position to fire on Soviet missiles. As this ratio increased, the fraction of

⁴⁸ See, for example, Richard L. Garwin to John Bosma, 20 February 1985, with attachment, Box 3, Folder 2 "APS DEW Study Correspondence January 1985–May 1985," *APS-DEW*.

defensive weapons that could participate in a battle (and therefore contribute to the effectiveness of the defense) decreased. Carter estimated this number somewhere in the neighborhood of 10 or 11—and it would only get larger (and less favorable to the defense) if the Soviets decided to cluster their silos together in a small geographical region, rather than dispersing them throughout the country. Then there was the enormous problem of system integration: not just of “killing” missiles with laser light, but of detection, tracking, aiming, firing and re-aiming, confirming hits, and so on. How feasible was it to combine so many complex components into a working ballistic missile defense system? For Carter, “making the technological devices perform to the needed specifications...is not the crux of the technical challenge...” Rather it was to create “a reliable defensive architecture,” a more important if nebulous goal. When he put everything together, he had to conclude that the prospect of a “perfect or near-perfect defense system...is so remote that it should not serve as the basis of public expectation or national policy,” a thoroughly damning assessment. In his view, assured destruction (as a fact, if not an ultimate desire) was here for the long term.⁴⁹

By early 1985, Carter was working as an assistant professor in Harvard’s Kennedy School of Government, and had become a member of the Center for Science and International Affairs (the more firmly established version of Paul Doty’s original arms control program). Thus he was especially handy to Harvard’s Nicolaas Bloembergen, as Bloembergen was beginning to dive into his own study of SDI. Carter was among the first people Bloembergen sought out for advice. During one chat in Bloembergen’s Harvard office, Carter told him that SDI (whose research budget was dilating quickly, now scheduled to suck up around \$25 billion over five years) was a “grab-bag of hardware with short term demonstration objectives,” nothing the U.S.

⁴⁹ Ashton B. Carter, *Directed Energy Missile Defense in Space—A Background Paper* (Washington, DC: U.S. Congress, Office of Technology Assessment, OTA-BP-ISC-26, April 1984), quotations on 81.

should be banking on to provide what the President had asked for. Carter wrote to Bloembergen the following month with remarkable advice:

I would suggest that you put right up front of any product of this study the fact that you are not addressing the military, strategic, or national security issues associated with ballistic missile defense; that you're not addressing the practicality of building a military system (which is the question that is really in dispute today), but only the practicality of building certain of the components; that you are addressing only one phase of the trajectory [the boost phase] and not all of ballistic missile defense; and that you are not addressing the other military missions like the anti-satellite mission to which the technologies you study could be applied. As I said, I am not sure how useful the product resulting from such a restrictive scope will be to the world at large, though it may be helpful to General Abrahamson in shaping his program. You will need to make it very clear that your study is not relevant to the policy discussions going on in the country today.⁵⁰

One could fill volumes with the things Carter believed Bloembergen's study should steer clear of. In Carter's own experience, his OTA report had been vulnerable on the long-contested grounds of strategy and arms control. He had extrapolated too far in talking about the mission of defense, the perpetuity of assured destruction. His claims that the system integration problems were unknowably—even insolubly—complex had invited the criticism that he was merely a dour pessimist, that he had misunderstood the cautious, long-range goals of SDI.⁵¹ Rather than wander into those thickets, Carter advised Bloembergen, why not stay safely restricted to minute

⁵⁰ See Bloembergen's handwritten notes on his meeting with Ashton Carter; and Ashton B. Carter to Nicolaas Bloembergen, n.d., Box 1E, NB.

⁵¹ James Abrahamson rejected Carter's report publicly when it was released, claiming that it was inappropriately pessimistic. See Wayne Biddle, "Study challenges space laser plan," *New York Times* (25 April 1984): A15.

technical analysis of individual components and technologies? Coupled to this was Carter's notable thought that Bloembergen's highly public and unclassified report, based on classified access, might chiefly aid the head of a highly classified government defense program.

Bloembergen was well prepared to take Carter's advice, including its contradictions: that had been the plan of the APS study all along.

The APS-DEW Study Group Convenes

The APS-DEW study opened for business in February of 1985, just as Bloembergen was conferring with Ashton Carter. William Havens and Robert Park of the APS arranged for the panel members to hear an opening briefing on February 18th from two representatives of the Strategic Defense Initiative Organization: Deputy Director and Chief Scientist Gerold Yonas, and head of the Directed Energy Office Louis Marquet, at the United Nations Plaza Hotel in New York City. On February 19th, the group also received a briefing back in Washington, D.C. from Richard Garwin, a longtime defense consultant and expert critic of missile defense, employed by IBM.⁵²

Garwin had written substantial portions of the Union of Concerned Scientists' 1984 report attacking SDI as a technical and strategic mistake. Garwin was already a grizzled veteran of the SDI debates. Only a week after Reagan's 1983 announcement, he and Edward Teller had tangled in the op-ed section of the *New York Times*. There, as worked out in greater detail in the UCS report, Garwin advanced the time-tested argument that he and Hans Bethe had made famous in their 1968 *Scientific American* article—namely, that countermeasures and additional offenses would always be capable of overcoming any defensive system, and at a lower cost. A

⁵² "APS Study of Directed Energy Weapons, Meeting Agenda, February 18-19, 1985, New York," Box 3, Folder 2 "January 1985—May 1985," *APS-DEW*; and Richard L. Garwin to William W. Havens, Jr., 22 February 1985, Box 3, Folder 2 "January 1985—May 1985," *APS-DEW*.

dangerous escalation of the offensive arms race would be the inevitable result. (Teller's op-ed was titled "Reagan's Courage"; Garwin's announced "Reagan's Riskiness.")⁵³ Garwin and his UCS colleagues insisted that it was always possible for the attacking force to confuse and overwhelm defenses at all phases of ballistic missile flight. The release of the UCS report had been timed to coincide with the first major Congressional hearings on SDI in March of 1984, in the Senate Armed Services Committee, at which Garwin had also provided testimony.⁵⁴

Through early 1985 Havens continued to work behind the scenes to guarantee the support of defense officials. He and Gerold Yonas arranged to have General Abrahamson send APS President Robert Wilson an official letter of support from the SDIO—a draft of which was written by Havens himself, and actually hand-delivered by Havens's secretary to Yonas's office at the SDIO for approval. The SDIO—at least as Havens had envisioned it in the letter—would "provide full cooperation in identifying and furnishing unclassified U.S. Government information," as well as make provisions for "unclassified briefings for the Study Group and tours of U.S. Government Directed Energy Weapons research and development laboratories."⁵⁵

Further cooperation with the Defense Department and the Reagan administration was cemented through contacts in the Office of Science and Technology Policy. On the 6th of March, Bloembergen and Patel met with Reagan's science advisor, George Keyworth, in person.

Keyworth expressed positive views about the study, offering full cooperation with the panel, and

⁵³ Edward Teller, "Reagan's courage," *New York Times* (30 March 1983): A31; Richard L. Garwin, "Reagan's riskiness," *New York Times* (30 March 1983): A31.

⁵⁴ The initial UCS report was published as Kurt Gottfried and Henry W. Kendall, *Space-Based Missile Defense: A Report by the Union of Concerned Scientists* (Cambridge, MA: Union of Concerned Scientists, 1984). The UCS also issued a popular treatment of their arguments in paperback format: Union of Concerned Scientists, *The Fallacy of Star Wars* (New York: Vintage, 1984). The early work of the UCS on the SDI issue is recounted in Rebecca Slayton, "Speaking as scientists: Computer professionals in the star wars debate," *History and Technology* 19, no. 4 (2003): 335-364. In 1984 Gottfried and Kendall also advanced some of the same arguments in an article published in *Scientific American* with Richard Garwin and Hans Bethe, the original physicist-critics of ABM. See Hans A. Bethe, Richard L. Garwin, Kurt Gottfried, and Henry W. Kendall, "Space-based ballistic-missile defense," *Scientific American* 251, no. 4 (1984): 39-49.

⁵⁵ Havens to Gerold Yonas, 4 March 1985, Box 3, Folder 2 "January 1985—May 1985," *APS-DEW*.

even promising to arrange for a third of the study's funding through the Department of Energy along with technical briefings even on the most classified aspects of SDI.⁵⁶ In later correspondence and actual agreements, however, he was far less forthcoming. Keyworth, like Abrahamson, had promised an official letter of support; but the weeks rolled by without any word from his office. No letter in hand by the following June, Havens sprang into action again, drafting a letter himself (yet again) for Keyworth to send to Bloembergen and Patel. The letter not only apologized (as it were) for Keyworth's tardiness, but also offered a brief disquisition on the troubles and ambiguities inherent in the task of crafting an unclassified report from largely classified information. Havens's letter for Keyworth agreed that the support of the federal government for the study was "highly desirable"—both for public relations purposes and for the substantive basis of the report's conclusions.⁵⁷

It proved difficult, however, to win the sustained attention of the SDIO. A first substantive, classified briefing from SDIO officials was originally scheduled for the end of March, and then postponed. Another was then scheduled for early May 1985, to be hosted by the SDIO at the Systems Planning Corporation, a classified facility in Arlington, VA.⁵⁸ It, too, was postponed. On the 1st of May Charles Hebel implored Colonel George Hess, Acting Director for Technology at the SDIO, to schedule a briefing within the following two weeks for the vulnerability and survivability subcommittee. The request was not granted. Finally, a week later, Ronald Kerber (a physicist at the University of Michigan and chairman of the vulnerability and survivability subcommittee of the study group) and Charles Hebel made headway on arranging

⁵⁶ Telemail from Havens (WHAVERS) to Robert Wilson (ROBERTWILSON) et al., 8 March 1985, Box 3, Folder 2 "January 1985—May 1985," *APS-DEW*.

⁵⁷ Telemail from Havens (WHAVERS) to Charles Hebel (CHEBEL), 13 June 1985, Box 3, Folder 3 "June 1985—December 1985," *APS-DEW*.

⁵⁸ Nancy Passamente to Judy Lowe, 8 March 1985, Box 3, Folder 2 "January 1985—May 1985," *APS-DEW*.

six days of meetings in June and August, to be located in Washington, D.C. and La Jolla, CA (in the classified facilities at the La Jolla Institute), with secret briefings by at least 30 officials representing the SDIO and various national weapons laboratories.⁵⁹ There was little question now that the study had thoroughly insinuated itself to classified access and the defense establishment's secrecy policies.

That June, when George Keyworth and James Abrahamson finally affixed their names to the letters William Havens had written for each of them, formal Defense Department and Reagan administration support for the DEW study was official.⁶⁰ Recognizing that it had enthusiastically done what it had initially promised APS members and potential funders it would not do—to embrace the sanction of the Pentagon and to boast of its access to classified information—William Havens quickly began drafting a revised statement of purpose for distribution to the scientists and sponsors of the APS. In it, he gave post-hoc justification for the decision. “On reflection, it was decided that a better study would result were the members to consider classified information as well [as unclassified information]....Of course it is frustrating to see events, even decisions of serious scientific impact, seemingly overtaking the APS attempt to consider the scientific basis of DEW....Because the members of the APS study have deliberately been chosen to have a wide spectrum of attitudes and expertise, we can hope that the report will serve as a scientific basis to help get on with a resolution of the proper balance between research on missile defense and nuclear disarmament. We can also hope,” he went on, sounding once again the notes of liberal optimism, “that the study will serve to blunt the confrontational arguments that are

⁵⁹ Havens to Adolph Hochstein, undated (likely May 1985), Box 3, Folder 2 “January 1985—May 1985,” *APS-DEW*.

⁶⁰ G.A. Keyworth to C.K.N. Patel, 13 June 1985; and James A. Abrahamson to Robert R. Wilson, 22 June 1985, Box 3, Folder 3 “June 1985—December 1985,” *APS-DEW*.

presently impeding our usual mutual scientific consensus that should be the important ingredient of national policy.”⁶¹

* * * *

Would the APS take an official stance on the political or strategic propriety of SDI? In December 1985 a physicist at Cornell University named Jay Orear, a veteran of the atomic scientists’ movement and its efforts to eliminate nuclear testing in the early 1960s (as well as a peripheral figure in the Cambridge disarmament community of the late 1950s, who had also attended the 1960 summer study on arms control), drafted his own proposed version of an APS Council statement on Star Wars. As far as Orear was concerned, SDI was an utter fraud, a grand and costly error, and “anyone who claims the possibility of such an invincible shield against all forms of delivery, whether he knows it or not, is engaging in a scientific hoax which could ultimately cost the U.S. taxpayer over a trillion dollars.” Orear went on in his letter to insist that the APS had the “urgent duty to warn the public and the President of what is perhaps the greatest scientific hoax in the history of our country.” The APS had to speak out *now*, Orear argued, even in advance of the APS-DEW study; the study was, at most, guaranteed to add some technical ornamentation and luster to the simple and already widely-appreciated fact that strategic defense of American urban populations was physically impossible. Cities were so geographically dispersed that simply by increasing the number of attacking warheads, the enemy could easily overwhelm even the most stringent defensive measures. The professional prestige of the American Physical Society was exactly what was needed behind such a statement, Orear argued:

⁶¹ Draft #5, The American Physical Society Sponsored Study Concerning the Science and Technology of Directed Energy Weapons, Box 3, Folder 3 “June 1985—December 1985,” *APS-DEW*.

perceived as balanced, objective, and expert, the APS was in a far better position to affect public opinion and political action than many of the other groups which had so far weighed in on SDI.⁶²

Kenneth Ford, an APS Council member (and soon-to-be director of the American Institute of Physics), strongly disagreed with the statement—both with its content, and with the idea of publishing it before the DEW study had finished its work. No one, argued Ford, *really* thought that SDI and its officials were striving for an “invincible” shield against ballistic missiles. A perfectly invincible shield was a chimera, and attacks against the idea were therefore fatuous. Indeed, Reagan had asked for scientists to render nuclear weapons “obsolete”; but vast nuclear stockpiles on hair-trigger alert also rendered such weapons “obsolete,” since their use would be suicidal. So Orear’s proposed statement took aim at a “straw man,” Ford said. It was therefore vulnerable to easy attack, since it would implicate the APS in frivolous criticisms of the military.⁶³

Robert Park, head of the APS’s Office of Public Affairs, took a middle road. In one sense he agreed completely with Orear. It was a “brief, clear and unqualified statement,” he wrote back to Orear, and “I like the use of the term ‘scientific hoax.’” But it was *not* true, contrary to Orear’s claim, that the statement would not damage the APS study. In fact, “the mere existence of the APS study undercuts the statement. To release a statement just weeks before our own study is due out will be taken as proof that the motivation for the statement is political...” But this was precisely the point, Orear believed. For the most ardent of SDI’s scientific critics, opposition to SDI was *not* political; it was scientific. Just because some may tarnish the APS Council’s statement as “political” would not mean that its statement *was* political, Orear replied to Park.

⁶² Jay Orear to APS Council Members, 10 December 1985, Box 3, Folder 3 “June 1985—December 1985,” *APS-DEW*.

⁶³ Kenneth W. Ford to Forum Executive Committee, 16 December 1985, Box 3, Folder 3 “June 1985—December 1985,” *APS-DEW*.

SDI's promise of impenetrable defense was simply scientifically unsound, and a variety of "airtight proofs" of this were available. Orear apparently understood the mission of the APS, therefore, to be the defeat of any possible version of SDI.⁶⁴

An already-tense battle intensified early in 1986. Ford again wrote to the APS top brass, telling them that even *considering* endorsing the inflammatory statement by Orear would be damaging to the Society's perceived disinterestedness, if news of its deliberations got out. The APS, in his view, would do best to avoid making any public statement on the political, strategic or economic propriety—in short, anything beyond the scientific soundness and technical feasibility—of Star Wars, regardless of the status of the directed energy weapons report.⁶⁵ Ford wrote to Orear, taking his side on the question of SDI—he believed SDI was physically and politically unsound, to be sure, just like Orear—even as he disagreed sharply with Orear about what sort of public stance the APS should take. Havens, who had the most authority, sided with Ford.⁶⁶

But the issue of an official APS statement on SDI would not go away. Fisher managed to present Orear's motion at the APS Council meeting in Atlanta on January 26th, whereupon the chairman of POPA (now Thomas H. Moss, physicist at Case Western Reserve University) offered the counter-proposal that the APS form a new committee, appointed by the APS President, to determine whether any official statement on SDI was desirable—and if it were, the committee (rather than Orear alone) would put together the draft.⁶⁷

⁶⁴ Jay Orear to Robert Park, 2 January 1986, Box 3, Folder 4 "1986," *APS-DEW*.

⁶⁵ Telemail from Kenneth Ford (KFORD) to Sidney Drell (SDRELL), with copies to APS executives, 10 January 1986, Box 3, Folder 4 "1986," *APS-DEW*.

⁶⁶ Telemail from William Havens (WHAVERNS) to Kenneth Ford (KFORD), 13 January 1986, Box 3, Folder 4 "1986," *APS-DEW*.

⁶⁷ Telemail from William Havens (WHAVERNS) to Thomas Moss (TMOSS) et al., 30 January 1986, Box 3, Folder 4 "1986," *APS-DEW*.

Strident tones began to pierce through the discussion. Another Cornell physicist, J.A. Krumhansl, sent an even more strongly worded proposed statement to the APS Council. “The public must realize,” he warned, “that the delivery of only one modern warhead to any of our major cities would be a major national and human catastrophe.” In Krumhansl’s view, SDI was—on fundamental physical grounds—incapable of providing failsafe defense of the U.S. population against even one single nuclear warhead, regardless of any possible improvements of technology. It was this message—of the *physical impossibility* of preventing nuclear war by means of missile defense technology—that in his view was most urgent for the APS to disseminate to the public.⁶⁸

In response to the demands, Moss began to work on a possible APS statement on SDI. “My political sense,” he explained to fellow Council members, “tells me that our statement will have the most public and political impact to the extent that we keep it close to those areas where we are recognized to speak with special authority.” In Moss’s version, the APS would remind readers that missile defense technology was plagued by a limitation common to all large-scale, complex technological systems: it was totally, drastically intolerant to error. “Even if a very small percentage of nuclear weapons penetrated a defensive system, they would cause human death and suffering beyond those ever before seen on this planet,” as Moss put it in his draft. Genuinely trustworthy missile defense technology, should it ever be possible, was surely decades away. For the time being, Moss recommended, rather than squandering vast resources on “a technological scheme with basic uncertainties,” the U.S. should give up its fantasy of leak-proof missile defenses.⁶⁹ The APS leadership was in broad agreement with the arguments of Orear and

⁶⁸ J.A. Krumhansl to The Officers and Council of the American Physical Society, 11 February 1986, Box 3, Folder 4 “1986,” *APS-DEW*.

⁶⁹ Telemail from Thomas Moss (TMOSS) to William Havens (WHAVERS), 11 March 1986, Box 3, Folder 4 “1986,” *APS-DEW*. Rebecca Slayton discusses this style of argument at length in *Arguments that Count*, 173–198.

Moss. But not wanting to undermine the impact of the DEW study report—and not wanting to play its hand before the verdict of the study group was in—the APS would wait to issue a formal statement on SDI until the report was ready for publication.

APS-DEW Report Publication and Controversy

On September 25th, 1986, with defense briefings concluded, and writing and initial reviewing of their report complete, Patel and Bloembergen transmitted a draft of the DEW study group's report for classification review at the SDIO. In their transmittal letter to General Abrahamson, the co-chairmen indicated that no classified information was presently included in the report.⁷⁰ All expected that the classification review would be speedy, and public distribution not long off. But the APS and the report authors experienced a setback on October 23rd when John Hammond, the new head of SDIO's Directed Energy Office, wrote to Kumar Patel informing him that SDIO's classification team had discovered "Secret" and "Secret-Restricted" data throughout the draft report. SDIO would therefore confiscate the draft—for an apparently indefinite period—while its staff scrubbed the document of any sensitive information. In two meetings during December 1986—one at Bell Laboratories and one at the Pentagon, including staff from the Department of Energy—the study group met with SDIO officials and agreed on necessary revisions.⁷¹

There was good reason to worry whether their study would ever see the light of day. A report written a few months earlier by staff at the Government Accountability Office, on the overall budget of SDI, had been classified indefinitely by the SDIO. SDIO made a remarkable leap of logic, arguing that even while no single piece of information in the GAO study was

⁷⁰ Patel to Lt. General James A. Abrahamson, 25 September 1986, Box 3, Folder 4 "1986," *APS-DEW*.

⁷¹ John H. Hammond to C.K.N. Patel, 23 October 1986, Box 3, Folder 4 "1986"; and Nicolaas Bloembergen to C.K.N. Patel, 13 November 1986, Box 3, Folder 4 "1986," *APS-DEW*.

technically classifiable, the report taken together as a whole was too sensitive for public release. A livid Senator William Proxmire had, in the words of an APS official, “slapped the SDIO people around” in Congressional hearings about the buried GAO report. But his efforts were to no avail.⁷²

Classification remained a sensitive public relations issue for both the APS and the government. On the one hand, the panel’s classified access was a boast—one that the physicists might make too much of in public, drawing the ire of the SDIO. According to the SDIO’s directions, APS press releases were *not* to make any mention of the panel members having been cleared to view classified information. As panel member Andrew Sessler paraphrased the SDIO’s instructions, the APS was “not to deny it, be truthful in your answers to all questions, but don’t advertise it.”⁷³ On the other hand, secrecy clearance was a source of immense frustration for the physicists, and quite probably used as a tactic by Pentagon officials to dampen the effect of the panel’s recommendations. The report had been “in the government’s hands before the Iceland summit,” study member Thomas Marshall told a student journalist with the *Harvard Crimson* that October, speaking of the high-stakes nuclear arms control summit meeting between Reagan and Soviet leader Mikhail Gorbachev. In Reykjavik Gorbachev had forcefully pressed Reagan and his advisers to abandon missile defense in exchange for significant Soviet concessions on offensive arms, but Reagan clung to SDI. The APS report “might have had a constructive effect if it was properly appreciated,” Marshall lamented.⁷⁴ But it had never had the chance to inform participants at the Reykjavik talks while the SDIO concealed the report behind an interminable classification review.

⁷² “DEW Study Review,” 5 November 1986, Box 3, Folder 4 “1986,” *APS-DEW*.

⁷³ Bob Park to DEW Panel & Release Task Group, 6 February 1987, Box 3, Folder 5 “January 1987—March 1987,” *APS-DEW*.

⁷⁴ Jonathan M. Moses, “Government reviews scholarly SDI study,” *Harvard Crimson* 185, no. 28 (17 October 1986): 1.

Patel, Bloembergen, and their study group colleagues submitted a revised draft of the report, taking into account the SDIO's initial objections, on January 15th, 1987. They heard nothing back immediately.⁷⁵ Anxious weeks ticked by and still no word came from the SDIO about the status of its classification review. Finally, toward the end of February, the SDIO reported that it had transferred its scrubbed copy of the report to the Office of the Secretary of Defense, Caspar Weinberger, for further approval. But there again it was delayed; by early March APS was still in the dark about the report's fate.⁷⁶ By mid-March, Robert Marshak (who had helped originate the idea of an APS study on SDI four years earlier at the Los Alamos 40th anniversary conference) proposed a darkly humorous bet with his colleague (and Carter administration science advisor) Frank Press that the APS-DEW report would *never* be declassified by SDIO.⁷⁷ But finally, on the 22nd and 24th of March, Kumar Patel spoke with John Hammond of the SDIO Directed Energy Office over the telephone. The news was mixed: the report would indeed be declassified; but entire sections (including everything concerning charged particle beams) would have to be eliminated. Patel made plans to travel to Washington to see how much of the report could be salvaged.⁷⁸

APS Executive Secretary William Havens, privy to the discussions with SDIO about the classification issues, later recalled that the negotiations were at times almost comical. When SDIO officials demanded that one particular passage in the section on charged particle beams be deleted, for example, Andrew Sessler (a physicist at Lawrence Berkeley National Laboratory and an expert on the physics of particle accelerators) pointed out that only a month earlier he had

⁷⁵ Patel to John H. Hammon, 15 January 1987, Box 1E, Folder "APS-DEW Study Report: Dec 1986—Jan 87," *NB*.

⁷⁶ "DEW Study—status," 3 March 1987, Box 3, Folder 5 "January 1987—March 1987," *APS-DEW*.

⁷⁷ Marshak to Prof. R. Park, 13 March 1987, Box 3, Folder 5 "January 1987—March 1987," *APS-DEW*.

⁷⁸ Telemail from Kumar Patel (CPATEL) to William Havens (WHAVENS), 25 March 1987, Box 3, Folder 5 "January 1987—March 1987," *APS-DEW*.

published an article including all the same information in *Physical Review Letters*; he produced an offprint of the paper to show the officials in the meeting. It seemed that either the SDIO's own experts were unaware of what already existed in the open literature, declaring "secret" what could be read by anyone with a journal subscription, or they were deliberately placing stumbling blocks in front of the directed energy weapons study.⁷⁹

Meanwhile, awaiting the report's declassification, APS staff began to prepare frantically for its final release. The APS had decided to rely on a classic two-step model of public dissemination: first to the Society's physicist constituents, who could best understand the report's technical content; and from the physicists (vaguely) outward to the general public. As early as the summer of 1986, APS officials had discussed arranging a videoconference in which members of the study group would conduct a live, televised colloquium, giving individual talks on different aspects of the report. The entire production might last several hours. At sites around the country, groups of physicists and other interested viewers could gather to watch the presentations live through a satellite downlink. That way, the APS hoped, the discussion among professional physicists might be kicked off instantly on a national scale. Trained physicists who viewed the teleconference and read at least some of the report might become a "further point of dissemination to the public in their communities." The proposal had the support of the Panel on Public Affairs, the APS Council, and both APS President Sidney Drell and study co-chairman Kumar Patel.⁸⁰

Yet the idea was a stunning failure when presented to the panel members, who were uneasy with the degree of public exposure and drama a teleconference seemed to invite. When Miriam Forman of the APS met with the study group after soon after revisions had concluded in

⁷⁹ Havens oral history interview, Session V, 14 August 1991.

⁸⁰ Telemail from Miriam Forman (MFORMAN) to Charles Hebel (CHEBEL), 26 September 1986, Box 3, Folder 5 "January 1987—March 1987," *APS-DEW*.

December of 1986, she proposed the idea to the group. Almost every member of the panel fiercely opposed it. The televised colloquium would give the appearance of grasping for attention, “which is unseemly for physicists,” they objected, and damaging to the scientists’ credibility and the credibility of their report. Moreover, the panel members objected that a videoconference in the near future would put unnecessary pressure on the SDIO, which had not yet officially released the report from classification review (and might rashly decide to restrict the report if provoked). On top of that, they argued, critics of the APS study would be sure to maliciously excerpt sound bites from a more extensive televised discussion of the report, taking them out of context.⁸¹ The APS would have to settle for a simple press conference and a news press release issued to newspaper and television sources, alongside dissemination of the report through the traditional publication channels of the American Institute of Physics (its monthly magazine *Physics Today* and the more scholarly *Reviews in Modern Physics*).

On April 13th, Kumar Patel at last wrote to Bloembergen with good news: the report had cleared the final hurdles of SDIO and Defense Department secrecy review.⁸² Copies were speeding their way to APS Council members, as well as a special APS Review Committee that had been established on the model of previous APS and National Academy of Sciences technical studies. The Review Committee included veteran arms controllers like Wolfgang Panofsky and Herbert York, laser scientists Charles Townes, Arthur Schawlow, and George Pake, and the former Livermore Laboratory director Michael May.

* * * *

At 10:00am on April 23rd, 1987 at the annual meeting of the American Physical Society in Crystal City, Virginia (just across the Potomac from Washington, DC), the report was

⁸¹ Miriam Forman to Sidney Drell, et al., 23 December 1984, Box 3, Folder 4 “1986,” *APS-DEW*.

⁸² Patel to Bloembergen, 13 April 1987, Box 1E, *NB*.

officially released, accompanied by a brief press conference conducted at the meeting hotel. Hundreds of copies of the report were mailed to journalists around the country, as well as members of Congress, officials in the Pentagon, and staff at the White House's Office of Science and Technology Policy. More than a hundred reporters from the major news outlets around the United States—and even a representative of the Soviet embassy—registered in the newsroom the day of the release.⁸³ Television viewers could have learned about the report by watching the NBC Nightly News or CNN that evening, ABC's Good Morning America the following day.⁸⁴ Though beset by funding problems from the beginning, and a delay of more than six months during an opaque and suspiciously drawn-out classification review, the report was finally out in the world.

It weighed in at 424 pages of dense technical analysis, divided and organized carefully by system component—not unlike Ashton Carter's recommendation to Bloembergen back in 1985. The study was presented as a piece-by-piece, component-by-component analysis of SDI. A long section surveyed various laser designs, laying heaviest stress on chemical lasers, excimer lasers (in which “pumping” of the laser was achieved by exciting a mix of noble and reactive gases with an electron beam), and free electron lasers (which used the radiation given off by an accelerated beam of electrons passed through an alternating series of oppositely polarized magnets, known as a “wiggler”). Another section discussed charged- and neutral-particle beams. A long section went into deep detail on “beam control and delivery,” discussing the mirrors, power generators, and relays in the system, as well as the tricky problems of atmospheric propagation. And another long section discussed “acquisition, tracking, and discrimination,”

⁸³ “People Registering at the APS Newsroom, Crystal City, VA,” April 1987, Box 4, *APS-DEW*.

⁸⁴ “Television News Stories About the DEW Report Release,” Box 4, *APS-DEW*.

explaining, for example, how a missile in the boost phase would be detected by the infrared emissions of its rocket plume.⁸⁵

The APS-DEW report concluded that “even in the best of circumstances, a decade or more of intensive research would be required to provide the technical knowledge needed for an informed decision about the potential effectiveness or survivability of directed energy weapon systems.” Not only were administration and SDIO estimates about the prospects for deployment overoptimistic; the government should put off even thinking about *development* for at least another ten years. The panel’s judgment was complicated, both cautious and damning at the same time. It was conservative in its refusal to say (as critics like Kosta Tsipis had said since 1981, and as many more had since 1983) that the Defense Department’s directed energy programs had proposed concepts and technologies that simply came short of basic physical principles. But it was also firm in its call for technological humility, stretching the R&D timeline for SDI into an indefinite period in the distant future. The authors also emphasized the unknowably difficult task of improving each of the components in way that would make system integration feasible at some point in the future, a job that would “depend critically upon information that, to our knowledge, does not yet exist.”⁸⁶

⁸⁵ In addition to the standalone copies printed by the American Physical Society, the report was published as N. Bloembergen and C.K.N. Patel, et al., “Report to The American Physical Society of the study group on science and technology of directed energy weapons,” *Reviews of Modern Physics* 59, no. 3, Part II (1987): S1–S201.

⁸⁶ “Report to the APS of the Study Group on Science and Technology of Directed Energy Weapons: Executive Summary and Major Conclusions,” *Physics Today* (May 1987): S3. For the chemical and excimer lasers, for example, the report’s authors concluded that the output powers would have to be increased by at least an order of magnitude (in the case of the excimer laser, four orders of magnitude). For other designs, more basic problems and questions necessitated the “validation of many of the physical concepts” before a guess could even be made about the power requirements. As far as tracking and aiming were concerned, “infrared tracking of missile plumes will have to be supplemented by other means,” they concluded, to meet the stringent aiming requirements to hit such tiny targets from thousands of kilometers away. See Bloembergen et al., “Report to The American Physical Society,” S10–S13.

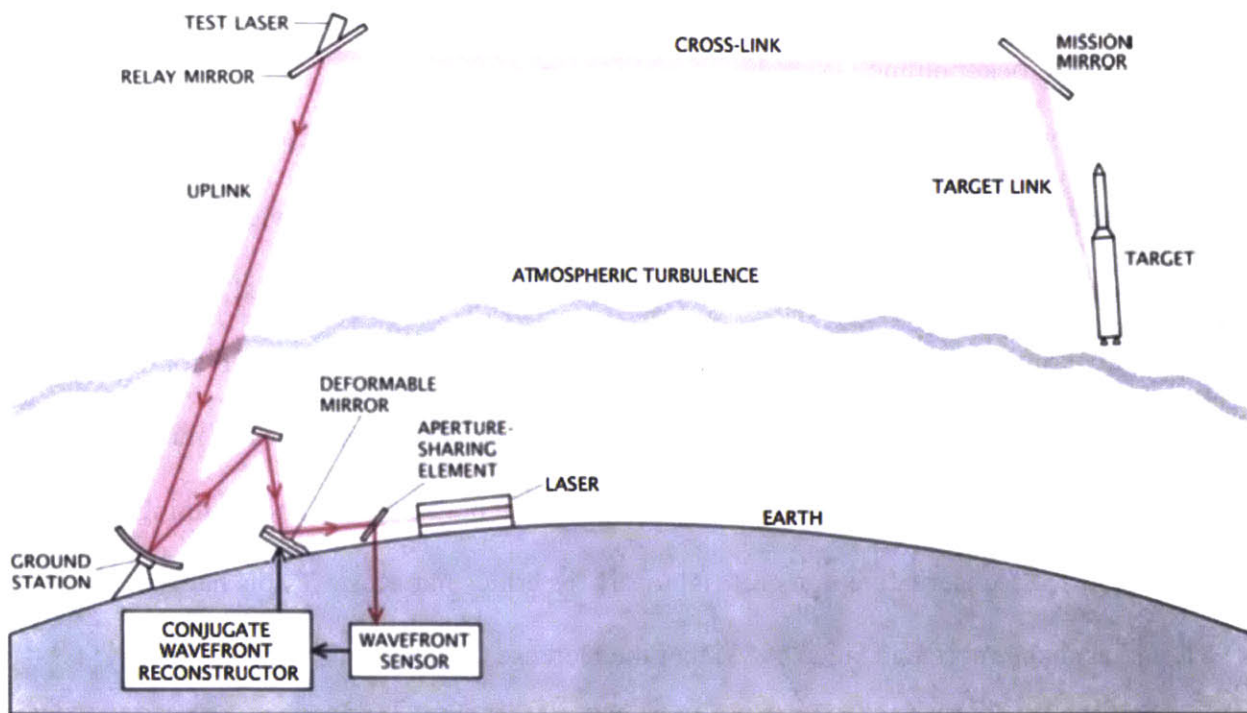


Figure 5.2: Bloembergen and Patel published a *Scientific American* article a few months after releasing their report. This figure, taken from the article, shows in schematic form a collection of technologies known as “adaptive optics,” which, it was hoped, would compensate for atmospheric distortion. A weak laser beam originating at the relay mirror in outer space travels down to the ground and is directed to a “wavefront sensor.” The sensor corrects the precise alignment of the various mirrors, using the information received from this weak signal beam. The high-power beam from the ground-based laser installation is then fired upward along the corrected path, hitting the relay mirror and ultimately its target, the launching rocket. According to Bloembergen and Patel, 10,000 to 100,000 actuators, which adjusted the alignment of the mirrors, would have to be “controlled simultaneously”—a task that had never before been accomplished, and whose prospects remained doubtful. From C. Kumar N. Patel and Nicolaas Bloembergen, “Strategic defense and directed-energy weapons,” *Scientific American* 257, no. 3 (1987): 39–45, on 44.

The Defense Department politely dismissed the report’s criticisms. Later on the day of the release, Louis Marquet of the SDIO held a press conference at the Pentagon. One reporter pointed directly to the report’s call for caution: “What we know about SDI right now is not enough, they say, to make a decision....” Marquet, in reply, attempted to reduce the scope of the study’s applicability, since the APS physicists “of course only looked at one specific area of technology, namely the directed energy area....I think, frankly, that they carried this study out in

a very responsible fashion....We did give them access to our programs; gave them detailed briefings at the classified level.” The result, Marquet said, was a discussion “unique in the annals of an open society reviewing a classified program.” And yet Marquet’s subsequent comments revealed the developing outlines of the Pentagon’s response to so many external evaluations of SDI. Echoing claims made earlier that day by SDIO spokesman Robert Sims, Marquet said that the report was six to eight months out of date (almost exactly the length of time classification review had muzzled the report, as it happened). SDIO staff had even come to the press conference prepared with a handout for the reporters explaining the various ways in which the APS report failed to account for new developments in the interim. “It took a while for them to get going; it took a while for the study to occur; then it finally took a while to get the report out,” Marquet said, without suggesting *why* that final step had been so delayed.⁸⁷

Not only was the report already past its expiration date, critics said; in places it was also factually incorrect. Two weapons scientists—Lowell Wood, an acolyte of Edward Teller at Lawrence Livermore National Laboratory, and Gregory Canavan of Los Alamos—quickly pored over the study’s dense calculations and claimed to have found numerical inaccuracies. Testifying before the House Republican Research Committee on May 10th, Wood and Canavan presented what they regarded as ten separate factual errors. The two physicists argued, for example, that the APS report included an erroneous figure for the power at which chemical lasers had been tested. The study group’s estimate was far too conservative.⁸⁸

The editors of *Physics Today* (which had published a condensed version of the APS-DEW report in its May 1987 issue) ran Wood and Canavan’s complaints alongside the panel’s

⁸⁷ Dr. Louis Marquet, transcript of press conference at the Pentagon, 23 April 1987, Box 4, *APS-DEW*.

⁸⁸ See “APS Directed Energy Study Group: Responses to Critiques by Wood and Canavan,” attached to letter from Thomas H. Johnson to Members, APS Study Group, 8 June 1987, Box 3, Folder 6 “April 1987—June 1987,” *APS-DEW*.

responses, drafted by study group member Thomas Johnson of the U.S. Military Academy. The panel members conceded to some of Wood and Canavan's charges, but they also pointed out that the SDIO and the Department of Energy's own unstable classification practices had helped cause the errors. In a few cases contradictions between figures representing the same quantity existed because, in one round of editing, SDIO officials had demanded that a number be modified "for reasons of security," only to insist in a later round that it could be changed back to its original value. Thus the estimated chemical laser power erroneously appeared as 200 kilowatts and roughly 1 megawatt in different places in the report—not because the study group had fudged the number, but because the SDIO could not decide whether the "real" value was too sensitive to release, resulting in a simple clerical error.⁸⁹

The bigger controversy, however, concerned the statement of the APS Council on SDI accompanying the report's publication in *Physics Today*. The statement—which APS members Orear, Fisher, Krumhansl, and Moss had promoted vigorously for months—exposed and flouted the delicate and unstable technical/political barrier that the APS and its study group members had been trying to maintain so assiduously for nearly four years. As the statement put it, the APS "has a public responsibility to express concerns about the Strategic Defense Initiative that go beyond the issues of DEW covered in the Study." The DEW report had removed all doubt that an effective missile defense was several decades away (if it would *ever* be possible). Because valuable resources and the threat of nuclear war were at stake, the APS insisted that SDI "should not be a controlling factor" in American foreign policy and arms control efforts. An early draft of

⁸⁹ "APS Directed Energy Study Group: Responses to Critiques by Wood and Canavan," attached to letter from Thomas H. Johnson to Members, APS Study Group, 8 June 1987, Box 3, Folder 6 "April 1987—June 1987," *APS-DEW*.

the statement even went as far as suggesting that SDI had simply run afoul of “technical realities.”⁹⁰

The APS’s public recommendation to downgrade and defund SDI amounted to a breach of objectivity and a grievous mistake, as far as the report’s authors themselves were concerned. In a letter to APS President Val Fitch they railed against what one study member called (in private correspondence) an attempt “to make political hay from our report.”⁹¹ The physicists now distanced themselves from the official APS position, complaining (in a draft of their protest letter) that “the Statement was clearly timed to capitalize on the Study’s press coverage, and the connection was reinforced for the press by the format of the release. We object to being thus included in the Council’s statements on matters neither we nor they studied, matters that border on personal political views.”⁹² Likewise, in an interview with the *Harvard Crimson*, Nicolaas Bloembergen complained that “our report was meant to be non-political and non-evaluative in nature. We feel undercut by the [APS] council, which attached a politically-oriented statement to the report.”⁹³ For a study whose premise was founded on a clean separation between science and politics, the APS’s seemingly flagrant political pronouncement could only, in the view of the panel members, discredit their many months of levelheaded technical work.

Debate over the report flared in the news media soon after the release and continued through the summer of 1987. In early May the editors of *The Wall Street Journal* “couldn’t help

⁹⁰ William Havens to Executive Committee et al., 28 April 1987, Box 3, Folder 6 “April 1987—June 1987,” *APS-DEW*.

⁹¹ Thomas H. Johnson to Members, APS Study Group, 8 June 1987, Box 3, Folder 6 “April 1987—June 1987,” *APS-DEW*.

⁹² See the draft letter to Val Fitch, 8 June 1987, attached to Thomas H. Johnson to Members, APS Study Group, 8 June 1987, Box 3, Folder 6 “April 1987—June 1987,” *APS-DEW*. The protest letter (with minor changes) was published over the names of the entire study group in *Physics Today* (October 1987): 9.

⁹³ John C. Yoo, “Bloembergen to protest misuse of SDI report,” *Harvard Crimson* (21 July 1987), in Box 1E, Folder “APS-DEW Newspaper Cuts,” *NB*. John C. Yoo, the student author of this article, would later write the so-called “torture memos” endorsing the use of “enhanced interrogation techniques,” while serving as Deputy Assistant U.S. Attorney General in the administration of George W. Bush.

but wonder what purpose is served by having 17 physicists with other full-time jobs trying to second-guess the Pentagon's multibillion dollar, 2,000-person-strong SDI effort."⁹⁴ The implication was clear: a small group of independent scientists could not hope to comprehensively assess a vast, complex, and mostly classified government program; the real expertise was in the Pentagon; so what was the use? APS President Val Fitch replied (somewhat feebly) in an op-ed a few weeks later that the study group was "not, as implied [by the *Journal*], a bunch of physicist-dilettantes making unjustified assertions," but experts with "unimpeachable technical qualifications" who had been motivated by physicists' "special obligation to their fellow citizens to explain the technical issues."⁹⁵ Gregory Canavan and Lowell Wood chimed in again, writing later that summer in the *Journal* that the APS-DEW report "contains major technical errors, always in the direction of making defense of the U.S. against Soviet missiles further from achievement than it is."⁹⁶ By August Secretary of Defense Caspar Weinberger was arguing in an op-ed in the *New York Times* that "no technical roadblocks stand in the way" of SDI, though "American Physical Society scientists grabbed headlines with a pessimistic study of S.D.I. that, in addition to containing important technical errors," was guilty of narrowly focusing "only on the most advanced technologies." The report had thus consigned itself to irrelevance, in the view of the top defense official in the country.⁹⁷

In *The Scientist* that same summer, Frederick Seitz, chairman of the Advisory Committee of the SDIO and a representative of the conservative George C. Marshall Institute, wrote articles decrying technical flaws in the APS-DEW report and insisting that "the merits of the Strategic

⁹⁴ "The naysayers' report," *Wall Street Journal* (1 May 1987): 22.

⁹⁵ Val Fitch, "Physicists studying SDI aren't dilettantes," *Wall Street Journal* (28 May 1987): 31.

⁹⁶ Gregory H. Canavan and Lowell H. Wood, "Strategic defense of the realm," *Wall Street Journal* (15 July 1987): 29.

⁹⁷ Caspar W. Weinberger, "It's time to get S.D.I. off the ground," *New York Times* (21 August 1987): A27.

Defense Initiative program inevitably will emerge as its development proceeds.”⁹⁸ And a headline in the pages of the neoconservative magazine *Commentary* later in September promised to explain “How Eminent Physicists Have Lent Their Names to a Politicized Report on Strategic Defense.” The article’s author, Angelo Codevilla, mocked the scientists’ claims to objectivity. The “report was not written,” he explained, “by an impartial jury of qualified scientists.” Codevilla claimed, wrongly, that “not a single [member of the study group] has *ever* worked in the practical field of developing directed-energy weapons,” suggesting an explanation for the report’s extensive “errors and internal contradictions.” It was riddled with “assumptions [which were] either erroneous or arbitrarily tendentious,” a mere “work of ideology masquerading as science—a political tool the purpose of which is to convince Americans not to try to acquire defensive weapons.”⁹⁹

The APS-DEW report was thus bruised and battered on the playing field its own authors had chosen: the field of public claims to objectivity, independence, and technical competence. Hamstrung by a delayed classification review, drowned out by the shouting in the media, publicly rebuffed and apparently ignored by officials in the Pentagon, the report’s overall impact on the Strategic Defense Initiative appeared—at least at first blush—to be minimal. Without access to unambiguous declassified records of internal Defense Department discussions regarding the APS-DEW report, however, it is impossible to know the real extent of its impact on the subsequent course of SDI.

⁹⁸ Frederick Seitz, “APS report has numerous errors,” *Scientist* 1, no. 17 (1987): 13; quotation from Frederick Seitz, “The APS report—the flaws remain,” *Scientist* 1, no. 25 (1987): 13. On Seitz’s involvement in various conservative think tank projects, see Naomi Oreskes and Erik M. Conway, *Merchants of Doubt: How a Handful of Scientists Obscured the Truth on Issues from Tobacco Smoke to Global Warming* (New York: Bloomsbury Press, 2010), esp. Chapter 2, “Strategic Defense, Phony Facts, and the Creation of the George C. Marshall Institute,” 36–65.

⁹⁹ Angelo M. Codevilla, “How eminent physicists have lent their name to a politicized report on strategic defense,” *Commentary* 84, no. 3 (September 1987): 21–26.

It seems plausible, however, that both Gregory Canavan and Lowell Wood had access to the initial draft of the APS report while it was under declassification review at the SDIO during the fall of 1986. It is also obvious that both scientists were closely involved in studying, and in a few cases trying to rebut, its claims over the course of late 1986 and early 1987. It therefore seems worth wondering what influence the report's sobering conclusions may have had on their thinking during these months. It was in November 1986 that conversations between Wood and Canavan gave rise to the "Brilliant Pebbles" concept for SDI's architecture: the priority of kinetic warheads (small, heavy pieces of metal accelerated to high velocities) over directed energy devices, including lasers and particle beams. Just a year later, Wood gave General Abrahamson an official briefing on the kinetic energy weapon concept, effectively ending the main directed energy phase of SDI's development for good.¹⁰⁰

Conclusion

In virtually every respect—in the tussles over the study's funding, its membership, and its access to classified material—the officials and experts responsible for the APS-DEW study struggled mightily for an appearance of dispassionate objectivity. It cannot be said, however, that all of the effort to avoid the stain of politics met with unambiguous success. The report's fate was public (if not necessarily private) dismissal by Defense Department officials, lost amid a wreckage of similar studies. The fate of the APS after adopting its public position on SDI was a prompt denunciation by its own experts, who recoiled at the thought of being taken for political advocates. Each of these developments seemed to signal a degree of political failure for

¹⁰⁰ See Donald R. Baucom, "Missile Defense Milestones, 1944-1997" (Washington, DC: Ballistic Missile Defense Organization, 1997), available at <http://www.fas.org/spp/starwars/program/milestone.htm>.

American science and its intervention, on behalf of the public, into the secrecy-cloaked world of defense planning.

The APS leadership and the study group members wielded a traditional tactic—cultivating an image of apolitical objectivity—that proved inadequate to the challenges of a fractious political and media environment. But the tactic was perhaps most powerless in the face of a defense classification regime that was essentially free to do what it pleased with the report. The APS had always been unsure of how to position its study in relation to the secrecy surrounding SDI. By claiming all along that it represented a position of unique independence and technical competence, it exposed the report to the easy charges made against it—that it was flawed scientifically, that it could only scratch the surface of a project as complex and enormous as SDI, and that its conclusions were based on information with a quickly approaching expiration date. The SDIO officials and their supporters were, in their way, correct: the APS's claims to objectivity were no match for a vast, classified defense program.

Most commentators on the report, including conservative critics who argued that it was factually flawed or simply dishonest, and liberal supporters who saw in the report an objective vindication of their anti-SDI beliefs, paid repeated homage to the notion that science stood firmly apart from questions of value and public interest, ready to adjudicate such matters when called upon. So House Representative George Brown, Jr. of California (a staunch SDI critic) could explain in a letter to his Congressional colleagues that “it is often claimed that the only hurdle between us and an effective ‘defensive shield’ is sufficient political will to proceed with SDI deployment. The APS study, however, demonstrates that the principal hurdles before us are not political ones; they are scientific.”¹⁰¹ And Brown's opponent, the conservative Representative Curt Weldon could claim at the same time that his own, more scrupulous scientist-advisers had

¹⁰¹ Brown to Colleagues, 24 April 1987, Box 3, Folder 6 “April 1987—June 1987,” *APS-DEW*.

found the APS-DEW study to be “riddled with errors,” indicating that it had not been conducted in a “professional, objective manner based on accurate scientific findings.”¹⁰²

Such claims and counterclaims miss the heart of the issue, however, for they fail to recognize the degree to which scientific objectivity and technical rationality were political and rhetorical strategies. They were wielded by particular groups and aimed at specific ends; they were speech acts performed for a public audience. In this way the APS-DEW controversy and the larger dispute over SDI brought to a high point several trends in the arms control discourse that had developed over the previous quarter century. The desire of experts and policymakers—especially since the opening of strategic arms control’s public phase in the late 1960s—had been to bleach the political coloring from the arms control process, making it a rational enterprise in the pursuit of stability. But the political shortcomings of this vision had never been more evident than during the bitter arms control struggles of the Reagan era.

¹⁰² Weldon et al. to Havens, 5 June 1987, Box 3, Folder 6 “April 1987—June 1987,” *APS-DEW*.

EPILOGUE

In late 1962 a defense analyst named Robert Levine spoke in the Harvard-MIT arms control seminar. He was about to publish a book called *The Arms Debate*, although its working title for the moment was “Arms and the Intellect.” Levine proposed to uncover “the logical basis of policy disputes,” classifying defense thinkers within schools of thought according to the policy recommendations they espoused. At the fringes of Levine’s scheme were the “systemists,” in both anticommunist and antiwar varieties. They believed that radical social and political change was necessary to bring about a safer world. In the middle were the “marginalists,” who thought that change came more safely in incremental steps, each step tentative rather than final. Among the marginalists there were anticommunist and antiwar flavors, too, but also a “middle marginalist” school. To Levine, someone like Jerome Wiesner or Bernard Feld would have been an antiwar marginalist. Among the middle marginalists Levine counted Thomas Schelling, and himself, and many others who worked for Levine’s employer, the RAND Corporation. He’d asked the seminar to read the twelfth chapter of his manuscript, the chapter devoted to the middle marginalist camp.¹

Notes

¹ Robert A. Levine, *The Arms Debate* (Cambridge, MA: Harvard University Press, 1963). When the book appeared the following year, it was reviewed in the *New York Review of Books* by Marcus Raskin. Raskin had until recently been a National Security Council staffer in the Kennedy administration. He was left-leaning, opinionated and strong-willed, and McGeorge Bundy had slowly shut him out of the inner circle of the NSC before dismissing him. Raskin called his review “The Megadeath Intellectuals,” and the title said just about everything one needed to know about Raskin’s view. He flailed at the book: “The reader is left with the impression that the arms discussion is essentially metaphysical, with no reference at all to numbers of weapons, the size of the budget, or the power of the military establishment itself.” Levine’s monograph, Raskin said, was one more in a long line of attempts to rationalize the growth of U.S. military power. See Marcus G. Raskin, “The megadeath intellectuals,” review of *The Arms Debate*, by Robert A. Levine, *New York Review of Books* (14 November 1963) <http://www.nybooks.com/articles/archives/1963/nov/14/the-megadeath-intellectuals/>. A copy of the manuscript Levine circulated was in Paul Doty’s possession, and I viewed it in Doty’s personal collection (under processing).

“The middle group,” Levine told the seminar members, “sees war or conflict as natural, the Soviets as opponents, rationality as reasonably powerful.” They were a bit conflicted, these middling types—they abhorred neither communism nor the risk of thermonuclear war with a total zeal. “Their values are complex,” he said. The seminar participants weren’t sure whether they were ready to be packed in the boxes Levine had constructed for them. They dug around, trying to unearth what bothered them so much about the classification system. “The range of possible ideas and approaches can be classified,” objected the legal scholar Louis Sohn, “but people change.” The same person might hold several positions on the same issue over the course of a career—even over the course of a few weeks. Thomas Schelling offered that the expression of ideas, in the form of concrete recommendations, wasn’t as abstract as Levine had presented. In fact it was highly context dependent. Ideas need “individual expression, but an individual has roles and values,” he remarked. “Recommendations are related to roles,” but roles were in constant flux, based on the setting and the intended target of the policy recommendation. “Roles change over time,” he said.

But Morton Halperin struck closer to the root. For all the talk of policies and arguments, for all the emphasis on technical feasibility and bloodless rationality—on stability—there was, in the end, no explaining why certain people got behind one policy or another. “There is a problem in analyzing real people,” he said, “their real motives and beliefs,” which “are not part of the public debate.” Levine’s typology worked by sorting thinkers according to public arguments. But revealing the people underneath the statements and positions, knowing their motivations and basic beliefs, was another matter.²

See Box 12, Volume 2 “Joint Arms Control Seminars 1962-63,” Paul M. Doty Personal Archive (Accession 18511), Harvard University Archives, Cambridge, MA.

² “Joint Arms Control Seminar, Minutes of the Third Session, November 5, 1962,” Box 3, Folder 3 “Joint Harvard-MIT Arms Control Seminar 1962-63 Minutes,” *LPB*.

Robert Levine's problem has been mine, too, as I have tried to fit the arms controllers together with their arguments and actions. The interpretive movement between categories of argument and the motivations that underlie them is, as Morton Halperin noted, terribly fraught. In the history of the nuclear age, especially, the perspectives and assumptions we bring to our studies can make it too easy to create caricatures of the experts, thinkers, and decision-makers—to picture them as masterminds, as oracles, as mere technicians, or as cogs in a great machine. I find the nuclear arms controllers too complicated to catalog in a simple filing system. They were neither wizards nor machines, but people—intelligent, privileged but sincere, stuck in their historical moment, in a web of commitments and demands and institutions.

In this dissertation, we have watched the arms controllers practice arms control in many ways and in many venues, draw on many resources, and perform for many audiences. In each of the episodes discussed here, arms control experts and their work crossed the boundaries fixed by a traditional picture of the expert-state relationship. We are hard-pressed to find arms control ideas that were entirely “nuclear” in their origins, or completely obvious in their application. When it comes to the scientific knowledge that was produced in the course of arms control work, it is impossible to regard such knowledge either as pure, free-floating from the Cold War context, or as distorted by Cold War priorities. It somewhere in between—it is neither. Nor do we find working arrangements for arms control—jobs, studies, funding sources, contracts, venues and audiences for ideas and information—that will separate cleanly into state and non-state, open and secret, public and private. In the United States in the 1960s and after, not a single member of the elite arms control community can be properly and easily thought of as a functionary or a dissident. Arms control experts were, in many interesting ways, both insiders and outsiders to the American nuclear state.

In the years leading up to 1960, arms control emerged from the mixing of local communities of disarmament advocates and theorists of nuclear deterrence. Rather than inevitable doctrinal unity, early arms control featured highly contingent aspects, shaped by local factors. The crucial concept of “stability,” in particular, was open to multiple interpretations. Later in the decade, arms control problems also began to motivate groundbreaking scientific research. Elite contract consultants to the government contemplated the use of lasers as weapons for defense against ballistic missiles. As they worked out ideas and performed experiments in the context of classified discussions and studies, they laid the foundations of a new field of physics called nonlinear optics.

In the late 1960s, strategic arms control became a public issue during a complex political dispute over the wisdom of missile defense. Arms control experts acted as mediators of this controversy by bringing arcane issues to the public (including local residents whose neighborhoods would be impacted by missile defense installations), criticizing national policies even as they worked their connections to the highest levels of government authority. Arms control debate, and nuclear ideas and information generally, had started to spill out into uncontrolled spaces, as cracks began to open in the edifice of the nuclear state.

The constant search for support was a prominent feature of the history of U.S. arms control in the 1960s and 70s. Despite its ambiguous status in the national defense establishment, arms control experts and their political patrons had been able to establish a federal agency for arms control in 1961, whose most important function was understood (by the arms controllers, at least) to be research. But under a more conservative administration a decade later, the government arms control infrastructure faced a concerted and powerful attack. In response, program officers at the Ford Foundation began to channel money to well-placed arms controllers,

including Paul Doty at Harvard. One of the best-placed arms controllers of all—Ford Foundation president McGeorge Bundy—stood behind this new foundation policy, a watershed development in the private support of nuclear arms control expertise. The result was the creation of major university arms control centers in the middle and late 1970s—institutions that still exist today.

By the 1980s, arms control issues reached peak visibility amid a politically polarized controversy over the Reagan administration's Strategic Defense Initiative. In arms control's public age, the search for "answers" to the nuclear question—the search for stable deterrence, stable disarmament, effective defense—relied, increasingly, on presenting the right image to the public. For the American Physical Society's study of SDI, as for other similar efforts, the right image was one of technical objectivity in place of political advocacy. When the voices heard were so many, so polarized, and so loud, the need to reach a space above politics seemed dire. This brought the long tension between the "technical" and the "political" in the field of arms control to its highest level. The steady retreat of the experts to the safety of technical objectivity gave arms control's political enemies powerful leverage in the last years of the U.S.-Soviet Cold War.

The narrative of this dissertation has embraced the messiness and diversity of expert arms control practice. Rather than the "logical" structure underlying the arms debate, I have found people, a community with a shared history, and a complex and surprising range of activity. Still, a nagging question: Why? It is the question that Morton Halperin pointed to, and Robert Levine left unanswered. Why did the arms controllers pursue arms control? What moved them? What kept them on planes week after week, kept them meeting in late-night seminar discussions when their day-jobs didn't require it, kept them speaking to audiences high and low, kept them writing—forever writing—for those who would listen and read? When William Bader of the

Ford Foundation interviewed the arms controller Wolfgang Panofsky in 1971, Panofsky told him that he “put a higher value on undergraduate programs in the arms control field” than anything else. “Panofsky believes that we must strive to induce in younger people an ‘emotional commitment’ to the study and understanding of arms control issues,” said Bader’s notes from the interview. When Bader had flown out to Stanford University, he found Panofsky lecturing in Stanford’s new arms control course without compensation or any release from his other considerable duties. What drove him?³

When I ponder what it could mean to be motivated in the field of arms control, I sometimes think of Herbert Scoville. I can’t be sure where Robert Levine would have pegged him, but I would guess somewhere in the antiwar region of the large middle group. To friends and colleagues, he was known as Pete. He has appeared many times in this dissertation, wearing many different badges. He had a knack for showing up at many of the important postwar nuclear events. A physical chemist by training, he’d finished his Ph.D. in 1942 and worked on chemical warfare during the Second World War. Following the war he was hired by the Atomic Energy Commission. He was a bomb tester, assigned to one of the AEC units doing weapons effects measurements, evaluating the potency of nuclear weapons against naval targets. He’d been present at the Operations Crossroads tests in 1946, the first post-Trinity detonations, carried out at the Pacific Proving Grounds in the Marshall Islands. By 1948, at age 33, he had become the technical director of the DOD’s Armed Forces Special Weapons Project (AFSWP)—the military agency (counterpart to the civilian AEC) dedicated to the storage, handling, and testing of nuclear weapons.⁴

³ William B. Bader to Craufurd D. Goodwin, 9 December 1971, “Stanford University Arms Control Course,” Ford Foundation Grant 07300177, *FFG*.

⁴ Neil A. Lewis, “Herbert Scoville, Jr., arms expert; became a critic of nuclear policy,” *New York Times* (31 July 1985): A14. For background on the Armed Forces Special Weapons Project, see Defense Threat Reduction

In March of 1954 Scoville found himself in AFSWP Task Unit 13, part of a group of military scientists making weapons effects measurements on the Operation Castle series of nuclear tests. “Shot 1” of the Castle series, occurring early on the morning of March 1st, codenamed Bravo, was to be the first detonation of a deliverable thermonuclear weapon by the United States. The device, mounted on a manmade causeway jutting off the edge of Bikini Atoll, used a lithium deuteride fuel mixture that was solid at room temperature. This feat of science and engineering had eliminated the need for the cumbersome cryogenic machinery used to cool the liquid fuel of the first American H-bomb, tested in 1952. Bravo’s yield was around 15 megatons, making it the largest nuclear detonation the United States would ever set off. It was larger than planned—by a factor of two or three. Its fireball extended more than four miles in diameter, and the blast ripped a crater 250 feet down into the foundation rock of the atoll. Bravo was so unexpectedly big that it wrecked much of the diagnostic equipment installed nearby, curtailing the AFSWP’s radiation and fallout measurement program. “However, the important military significance of fallout over large areas beyond the blast- and thermal-damage envelopes was demonstrated dramatically,” concluded a later report on the Castle series’ weapons effects.⁵

The Castle Bravo test drizzled radioactive fallout over the atolls downwind of Bikini, toward the east. (It also dropped fallout on the Japanese fishing boat “Lucky Dragon,” poisoning the fishermen aboard and touching off an international incident that helped to kindle the

Agency, U.S. Department of Defense, *Defense’s Nuclear Agency, 1947–1997* (Washington, DC: U.S. Government Printing Office, 2002).

⁵ For some of the details on the Castle Bravo test, see Richard Rhodes, *Dark Sun: The Making of the Hydrogen Bomb* (New York: Simon & Schuster, 1995), 541–543; Brookings Institution, “Castle Bravo: The Largest U.S. Nuclear Explosion,” available at <http://www.brookings.edu/blogs/up-front/posts/2014/02/27-castle-bravo-largest-us-nuclear-explosion-rowberry>. The quotation is from K.D. Coleman, “Operation Castle,” Summary Report of the Commander, Task Unit 13, Military Effects, Programs 1–9, January 30, 1959, Accession Number ADA995123, Defense Technical Information Center.

movement against atomic testing.⁶) The atolls were “contaminated,” said Scoville’s own report on the test, “to such an extent that it became necessary to evacuate the native populations from Rongelap, Alinginae and Utirik Atolls and the military personnel on Rongerik Atoll.” A week after Bravo, a survey of the contaminated atolls was organized. The group in charge, comprised of men from Task Force 7.1 within Task Unit 13, would visit the contaminated areas, taking radiological measurements. There were four men on the survey. Scoville was its leader.⁷

He and his crew traveled aboard the USS Nicholas, a World War II-era destroyer that had been converted to an escort ship. The conditions were “extremely difficult,” Scoville reported: the lagoon waters couldn’t be negotiated by such a large ship, so “it was necessary to make long boat trips [aboard small craft] in high seas and land on tricky coral reefs.” Over three days, Scoville and his team made a survey of five “widely separated” atolls in two small boats. They carried five “Radiac AN/PDR-39” hand-held radiometers to take their measurements, tough little devices “subject to prolonged use under adverse conditions of dampness (to the point of sea water splashing over them), salt deposit and continual rough handling.” The gadgets worked well; only one of them conked out, and that on the last day of the trip.⁸

Scoville and his men recorded average and maximum gamma radiation dose rates at the various measurement sites. His team found that the dose rates in the villages and camps they visited were, on average, “slightly lower” than on the rest of the island. A small stroke of luck, perhaps, but tempered by other findings. The dose rates inside huts, where families lived, were about the same as the dose rates outside. The radioactivity from fallout did not discriminate

⁶ Lawrence S. Wittner, *Resisting the Bomb, 1954–1970, Volume II of The Struggle Against the Bomb: A History of the World Nuclear Disarmament Movement* (Stanford, CA: Stanford University Press, 1997), 2.

⁷ The details of Scoville’s team’s expedition in the following paragraphs are drawn from Report TU-13-54-375, 12 March 1954, Subject: “Radiological Survey of Downwind Atolls Contaminated by BRAVO,” a copy of which is in Box 9, Folder 1 “Writings 1954, 1969,” HSP.

⁸ Report TU-13-54-375, 12 March 1954, Subject: “Radiological Survey of Downwind Atolls Contaminated by BRAVO,” a copy of which is in Box 9, Folder 1 “Writings 1954, 1969,” HSP.

between indoors and out. Military barracks, they found, were a little better protected. “There was no evidence of rain washing off the contaminated material” from the trees and plants and dirt, Scoville stated in his report. He noted, however, that the radiation levels dropped off drastically on the shore below the high-tide mark. An interesting discovery: the notion that the tropical rain, which could descend in such thick torrents in that part of the world, would not wash the contamination away, but the ineluctable ocean carried it off, taking the poison into its searchless volume.

“The radiological survey proved that a large yield surface detonation can produce extremely serious radiological contamination over a distance more than 120 miles downwind,” Scoville concluded when the survey was complete. Fallout landed in an uneven distribution, clung to the earth in a patchwork pattern, like paint thrown against a canvas. “Although the fallout was serious on Rongelap Island located at the extreme southeast tip of the atoll,” Scoville wrote, “the contamination was about ten times greater at the north side of the atoll, twenty miles away.” That, too, was a discovery. He filed his report on the 12th. It was classified secret.⁹

In later years, Scoville traveled restlessly through the world of arms control. His life bears the marks of the major events and institutions mentioned in these pages. In 1955, back in Washington after his far-flung adventures in the nuclear testing program, he became associate (later deputy) director of the CIA, heading its technical research office. There he examined intelligence that helped debunk the “missile gap” scare of the late 1950s, on which John F. Kennedy campaigned to win the White House in 1960. Working with the technical staff of the President’s Science Advisory Committee in 1960, he was one of the first civilians to have his eyes widened by the Strategic Air Command’s SIOP, its plan for massive nuclear war. He was

⁹ Report TU-13-54-375, Headquarters Task Unit 13, 12 March 1954, Subject: “Radiological Survey of Downwind Atolls Contaminated by BRAVO,” a copy of which is in Box 9, Folder 1 “Writings 1954, 1969,” *HSP*.

among the first to see the aerial surveillance photographs showing missiles in Cuba in 1962. He was the second director of the Arms Control and Disarmament Agency's science and technology bureau, helping it prepare for negotiations on the Limited Test Ban and Nonproliferation treaties. He knew everyone in the arms control community, visited all the academic seminars, attended virtually all the studies and conferences. In 1969 he left government and fought against ABM and MIRV, taking a job with the Carnegie Endowment for International Peace. In 1971 he helped found a new not-for-profit organization dedicated to public education for arms control. It was called the Arms Control Association, and following the Nixon administration's purge of the government arms control bureaucracy in 1973, it was said to be a kind of "ACDA-in-exile," joining league with the Ford Foundation-funded university programs created around the same time.¹⁰

In the period after 1969, Scoville wrote and spoke and traveled constantly in the name of arms control, tilting into his work with prodigious energy. In 1972 alone he published at least ten op-ed essays in national periodicals, and four longer articles in major journals; he testified in Congress on six separate occasions, and read papers at several arms control conferences. In the late 1970s his obsession was the MX missile, a large ICBM the Carter administration planned to base in the desert of the American West, in Nevada and Utah. The missiles would reside in bleak but not uninhabited lands, populated mostly by Native American tribes, Mormon communities, and ranchers. Scoville traveled numerous times to those places, trying to explain what MX would mean, why it was so dangerous and should be rejected. Essays and op-eds continued to pour

¹⁰ For the Arms Control Association as an "ACDA-in-exile," see Duncan L. Clarke, *The Politics of Arms Control: The Role and Effectiveness of the U.S. Arms Control and Disarmament Agency* (New York: The Free Press, 1979), 162.

from his typewriter. He wrote an entire book about MX in tremendous haste. He was a man possessed.¹¹

For years Scoville had searched for his 1954 report on the fallout from the Castle Bravo test, trying to get it declassified. He'd had trouble locating a copy to make the request. In the middle of 1980, a door opened. Someone from the Defense Nuclear Agency (DNA), the successor to the Defense Atomic Support Agency (DASA, which itself had succeeded the AFSWP), wrote to him at his home in McLean, Virginia. This official, Lt. Col. Hartman Mowery, was asking Scoville about his health. He wanted to know about the extent of Scoville's participation in the testing program in the 1940s and 50s, to get some estimate of his exposure to radiation. Mowery was heading up a study for the Department of Health, Education, and Welfare, investigating the effects and risks of ionizing radiation on workers.¹²

Scoville wrote back promptly. "In addition to Operation Crossroads I participated in Operation Sandstone, all of the test series at the Nevada Proving Grounds through the spring of 1955, and in Operation Castle in 1954 at Bikini." He said he'd done radiological surveys on almost all of these test series, which had taken him into contaminated areas. "My only unusual health problem has been a degenerative osteo-arthritis of both hips," he informed Mowery. Scoville walked with two canes (visible on television in 1974, when he'd struggled to the stand to argue against the need for high-accuracy counterforce weapons on the public television program *The Advocates*). In filing his health report, however, an opportunity had presented itself to ask for help finding the Castle Bravo report. "I have tried for some time to obtain a copy of the

¹¹ Herbert Scoville, Jr., *MX: Prescription for Disaster* (Cambridge, MA: The MIT Press, 1981). Estimates of Scoville's publishing activity and Congressional testimony are drawn from the finding aid to the collection of his papers at the MIT archives: "Herbert Scoville, 1915–1985, Papers, 1947–1985, Manuscript Collection – MC 224," Institute Archives and Special Collections, Massachusetts Institute of Technology.

¹² Herbert Scoville, Jr. to Lt. Colonel Hartman B. Mowery, 5 May 1980, Box 1, Folder 35 "May 1980," HSP.

survey report I wrote on the Bravo shot but have been unable to locate one,” he said. “If you succeed in locating it I should appreciate very much...getting a copy.”¹³

It arrived in early 1981, declassified by the Defense Nuclear Agency. “Imagine my surprise and joy,” Scoville replied to the declassification officer who had sent him the document, “when I found in my mail last week a copy of the Radiological Survey Report which I wrote back in 1954.” One asks what Scoville thought then, holding those pages for the first time in nearly thirty years. He must have bent his memory back to March of 1954, and his attempt on behalf of the U.S. government to retrieve knowledge from that manmade hole in the turquoise sea. I picture him picturing himself—his small crew, moving through the heavy air in their modest boat. They come ashore at a small cluster of vacated huts; it is quiet, no sound except water against the hull, the wind hissing in palm branches. He does his work with care, holding his radiometric equipment at “waist height,” as he diligently noted in his report, before taking each reading and marking down the number. What, decades later, had he been searching for in the document? Holding it in his hands, what secrets did it disclose? He did not record such thoughts in writing. About four years after receiving his declassified report in the mail, a forum on nuclear weapons published by *Harper’s Magazine* quoted Scoville as saying that “the real problem today remains how *not* to use nuclear weapons.” He would die, of cancer, the following month.¹⁴

Many of the issues the arms controllers confronted remain with us now, a quarter century after the collapse of the Soviet Union. The weapons are substantially still here, as are nations and groups who want them and might use them. The technologies of nuclear delivery, which troubled

¹³ Herbert Scoville, Jr. to Lt. Colonel Hartman B. Mowery, 5 May 1980, Box 1, Folder 35 “May 1980,” HSP.

¹⁴ Herbert Scoville, Jr. to Major H.L. Reese, 12 February 1981, Box 9, Folder 1 “Writings 1954, 1969,” HSP; “Is there a way out?,” *Harper’s Magazine* (June 1985): 35–42, 44–47 (the emphasis is mine).

the arms controllers endlessly, remain in silos and submarines. Nuclear analysts still talk of seeking “strategic stability,” now in a multipolar, post-Cold War environment. A recent series of essays and responses on a scholarly discussion forum and the blog of the *Washington Post*, written by prominent contemporary nuclear scholars, debates the merits of different approaches to the study of nuclear weapons. The highly formalized, statistical methods of political scientists are weighed against the narrative traditions of historians. The contributors recommend that academics today strive for “policy relevance” in their work. We are, after all, not out of the woods. Tribute is paid to the golden age of nuclear study in the 1950s and early 1960s, when thinkers of all kinds bent their tools against the nuclear question. The first-generation arms controllers, who demanded studies and more studies again and again, would have approved.¹⁵

Yet as I consider them and their struggle, it is hard to avoid concluding that things are different now, that something has been lost. Their world is gone, though the weapons are not. Their remarkable circle, the small set of connections and relationships that sustained their large concerns, their ideas, responsibilities, and commitments. Their extraordinary lifelong involvement with nuclear weapons, inside and outside the structures of authority. It seems right to say that theirs was a charmed world, and a haunted one.

¹⁵ See the various pieces and posts on the H-Diplo/ISSF Forum on “What We Talk About When We Talk About Nuclear Weapons,” available online at <http://issforum.org/forums/2-what-we-talk-about-when-we-talk-about-nuclear-weapons>; and “What New Academic Research Can Tell Us About Nuclear Weapons,” *The Monkey Cage* blog, *Washington Post*, July 8, 2014, <http://www.washingtonpost.com/blogs/monkey-cage/wp/2014/07/08/what-new-academic-research-can-teach-us-about-nuclear-weapons>.

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