Scientists and the Ethics of Cold War Weapons Research

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ABSTRACT

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This dissertation examines scientists' views concerning the ethics of U.S. weapons research and military advising, through the changing politics and economy of the Cold War.

After the development of the atomic bomb, the Manhattan Project generation of physicists posed a series of troubling ethical questions: To what extent are scientists responsible for the military applications of their work? What are the political obligations of technical experts? What are the ideal relations among academia, industry, and the military? During the post-Sputnik science boom, many elite physicists used their policy influence to encourage government support for scientific research and to secure stronger arms control measures, an effort that culminated in the ratification of the Partial Test Ban Treaty in 1963. But after the enthusiastic expansion of science advising in the late 1950s, the war in Vietnam sorely tested scientists' support for weapons research and government work. Key controversies that elicited substantial ethical debate included the use of chemical defoliants and gases in Vietnam and the participation of the secretive Jason scientists in developing an electronic barrier to prevent North Vietnamese incursions into South Vietnam.

By the end of the decade, campuses and professional societies were riven by clashes over defense contracting and academic "neutrality" in the context of the war in Vietnam. Whereas ethical debates in the aftermath of the Manhattan Project tended to be framed in individualist terms, the controversies of the late 1960s and early 1970s took place on the much larger scale of governments and institutions. The upheaval produced some changes in university contracting policies, but with ambiguous results, and the public disaffection of some top scientists led the

Nixon administration to dismantle the entire Eisenhower-era presidential science advisory system. The ethical debates of the Vietnam era cast a long shadow, shifting popular attitudes toward science and heavily influencing the character of scientists' opposition to the Strategic Defense Initiative during the 1980s.

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Table of Contents

List of Tables and Graphs…………………..……………………………………………ii

Acknowledgments…………………………….……………………………….. iii

Dedication………………………………………...…………………………………v

From Bomb to Test Ban

Introduction: Los Alamos and the Ethical Dilemmas of the Cold War…………………….1

Chapter 1: Government Scientists, Sputnik, and the Test Ban…………………………......15

The Science of Vietnam

Overview…………………………………………………………………………...... 92

Chapter 2: Chemicals and Ethics……………………………………………………….. 95

Chapter 3: Advising the Pentagon………………………………………………………174

Institutions and Neutrality in Wartime

Overview……………………………………………………………………………..252

Chapter 4: MIT and the March 4 Movement………………………………………....255

Chapter 5: The Problem of Neutrality for Academia and Professional Societies…………315

From Vietnam to Star Wars

Epilogue: Science, Politics, and Ethics after Vietnam……………………………………..358

Bibliography…………………………………………………………………………..383

List of Tables and Graphs

Federal Obligations for Research & Development to Academia, 1955-1964…………..29

Types of Defoliants……………………………………………………………….114

Princeton Research Funding, Selected Departments and Agencies……………………346

Federal Investment in R&D, 1955-1989……………………………………………….367

Federal Investment in R&D, 1965-1974……………………………………………….367

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In loving memory of

Homer J. Bridger

and

Lillian L. Simon

Part I: From Bomb to Test Ban

Introduction: Los Alamos and the Ethical Dilemmas of the Cold War

In 1947, in the aftermath of the bombings of Hiroshima and Nagasaki, physicist Robert Oppenheimer famously observed that physicists had "known sin." In his reflections on the creation of nuclear weapons, and in the many hundreds of essays, memoirs, interviews, testimonies, and oral histories concerning the Manhattan Project scientists, the language of morality and ethics is ubiquitous. Physicists invoked notions of guilt, evil, obligation, patriotism, regret, and disillusionment. Many, like the refugee physicist Victor Weisskopf, joined the project out of a sense of patriotism for the United States and fear of Nazi development of atomic weapons. As Weisskopf recalled in a 1991 memoir, "There was never any thought of turning this down. How could I have refused an offer to join the best people in the country in a project of such enormous importance? How could I have refused to participate in the war effort of a country that had accepted and supported us so generously?"[1] But as he worked on the project, Weisskopf's views evolved, in confusing directions. He began to realize the full destructive potential of the bomb, which frightened him, even as the intellectual challenges and satisfactions of his research grew more compelling. He recalled that:

> Some of us, including myself, secretly wished that the difficulties would be insurmountable. We were all aware that the bomb we were trying to develop would be such a terrible means of destruction that the world might be better off without it. … Then, imperceptibly, a change of attitude came over us. As we became more deeply involved in the day-to-day work of our collective task, any misgivings that we had at the start began to fade, and slowly the great aim became the overriding driving force: We had to achieve what we set out to do.[2]

[1] Victor Weisskopf, *The Joy of Insight: Passions of a Physicist* (New York: Basic Books, 1991), 121.

[2] Weisskopf, 128.

Even after Germany's surrender, Weisskopf and his colleagues, with one notable exception, blithely continued on with their work. Weisskopf later wrote, "the reaction most of us had was both interesting and somewhat depressing. It showed how deeply we scientists got attached to the task that was set before us and to the solution of the remaining technical problems…. in retrospect, I have often been disappointed that, at the time, the thought of quitting did not even cross my mind."[3] Only Joseph Rotblat, the British physicist who would go on to found the international Pugwash arms control movement, objected to the continuance of bomb research.

For some Manhattan Project scientists, the Trinity test of July 1945 proved a turning point in their ethical views of their own work; for others, it was the actual use of the bomb in Japan. Weisskopf was shocked at what he considered the unnecessary, and criminal, decision to drop a second atomic bomb on Nagasaki. For Weisskopf and the other scientists who felt the ethical burden of their wartime contributions, one pathway to redemption lay in political action. As Alice Kimball Smith described in *A Peril and a Hope*, during the war years a small, prestigious, and influential group of scientists organized politically for three goals: to avoid actual use of the bomb in favor of a "demonstration" in an uninhabited location, to share bomb information with the Allies, and, after the war, to establish international civilian control of nuclear energy.[4] To pursue these goals, scientists drafted petitions, lobbied politicians, and founded the Federation of Atomic Scientists (later the Federation of American Scientists) and the *Bulletin of the Atomic Scientists.*

[3] Weisskopf, 147.

[4] Alice Kimball Smith, *A Peril and a Hope: The Scientists' Movement in America, 1945-1947* (Cambridge: MIT Press, 1970). See also Paul Boyer, *By the Bomb's Early Light: American Thought and Culture at the Dawn of the Atomic Age* (New York: Pantheon, 1985).

Only in their last goal—civilian control of nuclear power—did the scientists achieve some success, with the creation of the Atomic Energy Commission. The failure to prevent the use of the bomb—and the contradiction between exquisite control over laboratory experiments and impotence in the face of military applications of new technologies—proved devastating to some scientists. Norbert Wiener, who had declined to work on the Manhattan Project but whose early cybernetic studies for the Air Force enabled more efficient aerial bombardment, wrote in despair after the bombing of Hiroshima and Nagasaki:

> Ever since the atomic bomb fell I have been recovering from an acute attack of conscience as one of the scientists who has been doing war work and who has seen his war work a[s] part of a larger body which is being used in a way of which I do not approve and over which I have absolutely no control.
> …
> I do not know how to publish work without making it available for the strongest hands and I do not like the strongest hands of the present time.[5]

Weiner's reflections raised a host of questions: To what extent could scientists—particularly physicists and chemists, whose work might have weapons consequences—dictate the terms and goals of their own research? To what extent should scientists connect their own research to its potential military applications, and to what extent could they exert influence over these applications? Wiener, along with Leo Szilard and a handful of other distraught scientists, responded to the horrors of the atomic bomb by switching fields entirely, retreating into biology, medicine, and other less clearly weapons-related areas. But most physicists and chemists rejected such action.

Soon after the war, the same community of scientists was wracked by the anguishing debate over whether to develop the hydrogen bomb. On one side, physicist Edward Teller argued in favor of its development, in part because he believed that the advancement of scientific

[5] Peter Galison, "The Ontology of the Enemy: Norbert Wiener and the Cybernetic Vision," *Critical Inquiry* 21, No. 1 (1994): 253-54.

knowledge and technology was inevitable; better for the United States to develop the hydrogen bomb than someone else. As physicist Eugene Wigner explained, "Great weapons will always be developed soon after it is clear that they can be. And the hydrogen bomb was clearly possible in 1946; it would have been invented by 1960, even if Edward Teller had never been born."[6] Unlike Wiener, who had worried about "the strongest hands" grabbing control of his research, Teller's mathematical collaborator Stanislaw Ulam felt plainly that it was not immoral "to try to calculate physical phenomena." After all, "even the simplest calculation in the purest mathematics can have terrible consequences," he wrote, affirming that theoretical research ought to be fundamentally separate from the moral dilemmas of its application.[7] Of course, Ulam had been tasked specifically with determining, mathematically, whether a hydrogen bomb was possible; there was no mystery as to the applications of his research. Teller distilled the ethical problem more precisely, observing that scientists had an obligation to pursue an understanding of the physical world, including knowledge relevant to weapons production. "I am afraid of ignorance," he explained.[8] On the other side were the many Manhattan Project physicists who deemed the so-called "superbomb" an unnecessary and horrific weapon, best left undeveloped. As Hans Bethe recalled in a 1996 oral history, he disagreed deeply with Teller's attitude, explaining, "I would have been happy if we had remained ignorant."[9] These two fundamental questions—whether to work on scientific research with weapons applications, and how to deal with the "the strongest

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[6] Eugene P. Wigner and Andrew Szanton, *The Recollections of Eugene P. Wigner, as told to Andrew Szanton* (New York: Plenum Press, 1992), 262.

[7] Stanislaw Ulam, *Adventures of a Mathematician* (New York: Scribner, 1976).

[8] Quotation from a 1995 oral history of Edward Teller, in Mary Palevsky, *Atomic Fragments: A Daughter's Questions* (Berkeley and Los Angeles: University of California Press, 2000), 53.

[9] Palevsky, 72.

hands" that controlled key military decisions—would form the basis of a half-century of ethical and political debates among scientists.

In the years during and immediately after World War II, the Manhattan Project generation of physicists and chemists were thrust into the political spotlight, an elite group whose understanding of the workings of an atomic nucleus brought them unparalleled access to policymakers and military leaders. Making the most of their own prestige and influence, many parlayed their concerns over atomic weapons into agitating for arms control and international cooperation. To accomplish these goals, they acted both as individuals and through new organizations, lobbying politicians and making public appeals. Although many had disagreed with the decision to drop the atomic bomb in Japan, the development of the hydrogen bomb, and the campaign to strip Robert Oppenheimer of his security clearance in 1954, the Manhattan Project scientists did not harbor deep resentments against their government. By and large, they did not challenge existing political, military, or academic structures; rather, they worked within available channels and trusted in the power of persuasion to promote the goal of arms control. As the reflections of Wiener, Bethe, Teller, Ulam, and Weisskopf attest, they tended to consider the ethics of weapons research in individualist terms; that is, they worried about their own contributions, and felt the ethical burdens of their war work upon their own shoulders. It was the special responsibility of scientists to lobby for nuclear arms control, because scientists had created nuclear arms.

That this generation largely maintained the patriotic views of the war era is evidenced in the enthusiasm with which they welcomed expanded opportunities for government service in the aftermath of the launch of Sputnik in 1957. In response to that dramatic world event, President

Eisenhower called for a resurgence of American science, and opened the doors of the White House to the top scientific minds of the nation, through the creation of the Presidential Science Advisory Committee (PSAC) and a host of other new advisory mechanisms. Men like Hans Bethe and I.I. Rabi used their new positions to push Eisenhower towards a nuclear test ban and a more moderate overall nuclear strategy. These efforts would reach fruition during the short-lived Kennedy administration, during which PSAC members found an arms control sympathizer in Secretary of Defense Robert McNamara, and physicist Jerome Wiesner played a key role in the ratification of the Partial Test Ban in 1963.

The affinity between McNamara and the PSAC scientists stemmed largely from McNamara's endorsement of a no-first-strike nuclear stance and a military approach rooted in "flexible response" rather than massive retaliation. But the same strategic outlook that appealed to scientists on arms control grounds would soon find its outlet in the jungles of Vietnam. Scientists working at the White House and in the Pentagon were drawn into the conflict, asked to provide scientific perspectives on how to use new and old technologies to wage war more effectively. Young physicists employed by the Institute for Defense Analyses' Jason group found themselves focusing less on techniques to enforce a comprehensive test ban and more on techniques to subdue guerilla fighting forces in jungle conditions. Meanwhile, the use of non-nuclear tools of war such as tear gases, napalm, and chemical defoliants brought biologists, ecologists, and botanists into the ethical whirlwind as well, as these scientists debated whether such technologies themselves could be considered immoral, independent of the controversial conditions of their applications.

If the Manhattan Project had constituted problematic research in the service of a noble end, scientists' participation in the Vietnam War was perceived by many as problematic research in the service of a problematic end. George Kistiakowsky, a seasoned science advisor who opposed the war, nevertheless felt obliged to contribute to an effort to design an “electronic barrier” that he hoped would deescalate the war and hasten its end. As had been true of the Los Alamos physicists urging a demonstration of the bomb, Kistiakowsky watched, devastated, as his research was instead used by military leaders to promote the very outcomes he had hoped to prevent. He, along with a small group of other similarly disillusioned science advisors, resigned quietly from government service, maintaining detailed political correspondence with other scientists, but refraining from the kind of outspoken activism increasingly on the rise on university campuses.

It was on these very campuses that a second generation of scientists was coming of age, the students and young faculty members whose beloved dissertation advisors had been wartime heroes at Los Alamos or the Rad Lab at the Massachusetts Institute of Technology (MIT). In the context of Vietnam, however, the esteemed professors serving on President Johnson’s PSAC suddenly seemed cautious, naïve, and complicit. Across the country, campuses erupted in antiwar protests, many of which targeted universities’ lucrative defense contracts, classified research operations, and close relations between faculty and government. At MIT, massive demonstrations led to the creation of the Union of Concerned Scientists and the university’s eventual decision to jettison institutional ties to the controversial Draper Lab. At Princeton, where little weapons research was actually taking place, students and faculty nevertheless engaged in endless debates about the proper role of universities and scientists, questioning the concept of academic “neutrality.” Similar arguments would wrack a host of professional societies, including the American Physical Society, which faced radical internal protests led by

Charles Schwartz, the Berkeley physicist who had once been an adoring student of Victor Weisskopf.

Attacked on the left by antiwar protesters and on the right by the hostile Nixon administration, the Manhattan Project cohort and a wide circle of elite scientists struggled to find appropriate political and ethical ground. Nixon's initial support for an antiballistic missile system and the development of a supersonic transport proved the final straw for presidential science advisors. When a handful of advisors dared to oppose Nixon's desired policies in very public and persuasive ways, he dismantled the entire advising system set up in the aftermath of Sputnik. In the weak new advisory channels that replaced it, government science advisors were increasingly drawn from a pool of talented professionals with backgrounds in military and industrial work, rather than the towering iconoclasts of Los Alamos. Lacking the relative independence of the Eisenhower and Kennedy years, federal science advisors were frequently called upon to rubberstamp administrative decisions and pet projects.

Meanwhile, many of the antiwar groups of the late 1960s found new life in the resurgent antinuclear movement in the late 1970s and early 1980s. The stage was thus set for the dramatic political showdown prompted by President Reagan's announcement of the Strategic Defense Initiative in 1983, during which older and younger generations of scientists finally found common cause, attacking the program on both technical *and* moral grounds, and employing tactics that ranged from personal persuasion to boycotts of the funding structures and institutions that facilitated its development.

In 1981, the American Physical Society conducted a survey of nuclear physicists, requesting both demographic information and reflections on the history and state of the field.

When asked to assess how attitudes toward science had changed over the past several decades, respondents noted with near unanimity that the prestige of physicists had suffered greatly during the Vietnam years. They described the growing “mistrust of science and technology,” how “the war in Vietnam led to suspicion of science and scientists,” how physicists had been targeted by “vocal activist groups,” and how “since about 1967, support for science had decreased and much of science itself [came] under fire from a variety of segments of society.” As one Livermore physicist reflected:

> When I first decided to major in physics, the public had very little understanding of what physics was, and especially why it might have any application to daily life. Shortly thereafter, public awareness of physics, and nuclear physics in particular, blossomed and the field soon became one of glamour in the public image. I have seen this glamour wear thin to the point that now science, particularly physics, and more especially nuclear physics, is suspect.[10]

The years of the Vietnam War changed not just outside attitudes about science, but scientists’ attitudes about themselves. As the development of the atomic bomb had raised deep moral and philosophical questions for physicists about the applications of their research, so too did the participation of scientists in the prosecution of the war in Vietnam, and their indirect responsibility for the recycled technologies now employed in the service of controversial ends. In the face of deep and often vitriolic public scrutiny, many scientists were forced into self-reflection and self-defense. In 1967, Louis Fieser, the Harvard chemist who had invented napalm during World War II, observed defensively that back then, he “couldn’t foresee that this stuff was given to be used against babies and Buddhists.” But he argued that “The person who makes

[10] See, for example, responses by Norman Austern, Louis A. Beach, William Higinbotham, Lewis Slack, R.F. Taschek, Stephen R. White, and Lawrence Wilets in Responses to 1981 History of Nuclear Physics Survey, 1981, American Institute of Physics, Niels Bohr Library & Archives, College Park, MD.

a rifle… he isn't responsible if it is used to shoot the President."[11] At the other extreme, mathematical physicist William Davidon argued in the early 1970s that scientists and engineers had an obligation to halt the complicity of their profession in the prosecution of the war in Vietnam—through political activity and, if necessary, industrial sabotage. The bombing of an applied mathematics laboratory at the University of Wisconsin epitomized one dangerous outcome of such a view. In between these two extremes lay a myriad of other responses and approaches, including that of Arthur Galston, the botanist whose dissertation research had unintentionally sparked the development of Agent Orange, and who subsequently devoted decades of his life to trying to curb the use of what he considered to be a chemical weapon.

At heart, this dissertation is an attempt to elucidate and contextualize this wide range of ethical views, and to chart the ways that scientists themselves approached the scientific and moral challenges of Cold War.

The experience of Los Alamos, the postwar activism of the Manhattan Project scientists, the debates over the hydrogen bomb, and the political economy of Cold War science in the 1940s and 1950s are all topics that have been covered extensively by historians, political scientists, and the participants in these events themselves. Rather than focusing on the well-trod ground of the Manhattan Project itself, this dissertation focuses on the decades afterward. It is not a structural history of Cold War weapons science, but an episodic study of key Cold War debates concerning the ethics of weapons research, war advising, and professional activities, with a focus on the Vietnam years. It follows the ethical travails of several generations of scientists through the aftermath of Sputnik and the expansion of science advising, the arms control debates of the

[11] Clipping, "Napalm Inventor Honored at Harvard," undated (1967), "Retirement dinner, 1967" Folder, Papers of Louis F. Fieser and Mary P. Fieser, Box 1, HUG(FP)20, Harvard University Archives.

1960s, the war in Vietnam, the articulation of New Left critiques of science, and the Star Wars defense boom of the 1980s. The concept of ethics is defined here as broadly as possible, as the intersection of values and actions, the "standards of conduct" which scientists perceived for themselves.[12]

The topic of Cold War weapons science is likewise broad, and most secondary literature has focused not on changing views of ethics, but on the rise of large scale research facilities, academic research during the Cold War, and the political economy of the Cold War period. Few studies extend past 1960, however, and most focus mainly on the work of elite scientists, leaving room for examination of at least some of the demographics and recruitment policies for mid-level scientists and engineers.[13] Studies of university research during the Cold War are more plentiful and varied, though most focus primarily on the decade of the 1950s.[14] Assessments of the political economy of defense research spending—while useful for following long term funding trends—have tended to assess the impact of defense spending on domestic programs and employment, not necessarily the research options and public activities of scientists.[15]

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[12] I have borrowed the "standards of conduct" definition from David B. Resnick, *The Ethics of Science: An Introduction* (New York: Routledge, 1998), 12.

[13] Mendelsohn, Smith, and Weingart, eds., *Science, Technology and the Military*; Peter Galison and Bruce Hevly, eds., *Big Science: The Growth of Large-Scale Research* (Stanford, CA: Stanford University Press, 1992).

[14] For example, Stuart W. Leslie, *The Cold War and American Science: The Military-Industrial-Academic Complex at M.I.T. and Stanford* (New York: Columbia University Press, 1993). For a useful review of this literature, see D. Engerman, "Rethinking Cold War Universities: Some Recent Histories," *Journal of Cold War Studies* 5(3): 80-95. Key titles include: Noam Chomsky, et al. *The Cold War and the University: Toward an Intellectual History of the Postwar Years* (New York: New Press, 199—a collection of essays and reminiscences—and Rebecca Lowen, *Creating the Cold War University: The Transformation of Stanford* (Berkeley: University of California Press, 1997).

[15] For example, Richard Du Boff, "What Military Spending Really Costs," *Challenge* 32, no. 5 (1989): 4-10; Michael Edelstein, "War and the American Economy in the Twentieth Century," in Stanley L. Engerman and Robert E. Gallman, eds., *The Cambridge Economic History of the United States*, (New York: Cambridge University Press, 2000), 329-405; Michael Edelstein, "What Price Cold War? Military Spending and Private Investment in the U.S., 1946-1979," *Cambridge Journal of Economics* 14, no. 4 (1990): 421-437; Leonard Silk and Mark Silk, "Military Spending as Industrial Policy," in *Making Capitalism Work* (New York: New York University Press, 1996), 147-166.

A growing secondary literature on scientists in the public sphere also exists, though few works focus exclusively on issues of weapons science.[16] A handful of studies on presidential science advising mention the ethical concerns of top advisors, but most root their analyses in the frameworks of political science.[17] Other sociological studies and popular accounts provide excellent descriptions of lab culture and politics, as well as extensive first-person accounts, but none of these books traffic in traditional history—studying change over time—though taken together they provide important documentation of attitudes and behaviors at key moments.[18] By focusing on ethics and on the dramatic debates among overlapping groups of scientists, military advisors, politicians, and activists, this dissertation thus offers new perspectives on the trajectory

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[16] P.J. Kuznick, *Beyond the Laboratory: Scientists as Political Activists in 1930s America* (Chicago: University of Chicago Press, 1987); for a sample of popular histories and biographies, see also Kai Bird and Martin J. Sherwin, *American Prometheus: The Triumph and Tragedy of J. Robert Oppenheimer* (New York: A.A. Knopf, 2005); Richard Rhodes, *The Making of the Atomic Bomb* (New York: Simon & Schuster, 1986); Richard Rhodes, *Dark Sun: The Making of the Hydrogen Bomb* (New York: Simon & Schuster, 1995); Paul S. Boyer, *By the Bomb's Early Light: American Thought and Culture at the Dawn of the Atomic Age* (New York: Pantheon, 1985). For a more theoretical approach, see Stephen Hilgartner, *Science on Stage: Expert Advice as Public Drama* (Stanford, CA: Stanford University Press, 2000). For additional articles and case studies, see Lawrence Badash, "Science and Social Responsibility," *Minerva* 42 (2004): 285-298; Gary Downey, "Reproducing Cultural Identity in Negotiating Nuclear Power: The Union of Concerned Scientists and Emergency Core Cooling," *Social Studies of Science* 18, no. 2 (1988): 231-264; Kelly Moore, "Organizing Integrity: American Science and the Creation of Public Interest Organizations, 1955-1975," *American Journal of Sociology*, no. 101 (1996): 1592-1627; Rebecca Slayton, "Speaking as Scientists: Computer Professionals in the Star Wars Debate," *History and Technology* 19, no. 4 (2003): 335-364; Jessica Wang, "Scientists and the Problem of the Public in Cold War America, 1945-1960," *Osiris* 17 (2002): 323-347; Matt Wisnioski, "Inside 'The System': Engineers, Scientists, and the Boundaries of Social Protest in the Long 1960s," *History and Technology* 19, no. 4 (2003): 313-333. See also Matthew Wisnioski, "Engineers and the Intellectual Crisis of Technology, 1957-1973," (Ph.D. diss., Princeton University, 2005), which offers some relevant case studies of engineers' activism during the 1960s.

[17] For studies of government science advising during the Cold War, see William T. Golden, ed., *Science Advice to the President* (Washington, DC: AAAS Press, 1993); Benjamin P. Greene, *Eisenhower, Science Advice, and the Nuclear Test-Ban Debate, 1945-1963* (Stanford: Stanford University Press, 2007); Gregg Herken, *Cardinal Choices: Presidential Science Advising from the Atomic Bomb to SDI* (New York: Oxford University Press, 1992); Joel Primack and Frank von Hippel, *Advice and Dissent: Scientists in the Political Arena* (New York: Basic Books, 1974); Frank von Hippel, *Citizen Scientist* (New York: American Institute of Physics, 1991); James Everett Katz, *Presidential Politics and Science Policy* (New York: Praeger, 1978); Gerhard Sonnert, *Ivory Bridges: Connecting Science and Society* (Cambridge: MIT Press, 2002); Zuoye Wang, *In Sputnik's Shadow: The President's Science Advisory Committee and Cold War America* (New Brunswick, NJ: Rutgers University Press, 2008).

[18] Hugh Gusterson, *Nuclear Rites: A Weapons Laboratory at the End of the Cold War* (Berkeley: University of California Press, 1996); Sharon Traweek, *Beamtimes and Lifetimes: The World of High Energy Physicists* (Cambridge, MA: Harvard University Press, 1988); William J. Broad, *Star Warriors: A Penetrating Look into the Lives of the Young Scientists Behind Our Space Age Weaponry* (New York: Simon and Schuster, 1985).

of the Cold War and the place of American scientists within it.

This dissertation is organized into four sections. The first, which includes this prologue, addresses the ethical dilemmas raised by the development of the atomic bomb, and follows the Manhattan Project generation through the science boom of the post-Sputnik period. The first chapter describes the participation of scientists in setting government and military policy after 1957, with sections devoted to the expansion of science advising and research funding during the second Eisenhower administration and the ways in which scientists used new advisory mechanisms to promote or oppose arms control efforts under Eisenhower and Kennedy. The second and third chapters trace the subsequent role of these scientists and others as participants and critics of the prosecution of the war in Vietnam, including an analysis of the development and use of chemical defoliants and tear gases in Southeast Asia and a detailed assessment of the controversial and secretive Jason group of science advisors. The fourth and fifth chapters examine more closely the ethical debates within academia about weapons research during the Vietnam era, with a case study of MIT's decision to sever ties with its Instrumentation Laboratory and a discussion of the problem of "neutrality" for professional societies and universities. The conclusion of the dissertation tracks the rise of the Massachusetts defense industry in the 1970s and early 1980s and scientists' political activity in response to Reagan's Strategic Defense Initiative in 1983. Taken together, the chapters explain how during the science-friendly Eisenhower administration, elite scientists welcomed government advisory positions and initially embraced and promoted the expansion of military funding for science research for a variety of reasons. The war in Vietnam challenged many ethical assumptions about the value of government and military service, however, and the related revolt against classified and weapons research on college campuses marked a significant reversal and contributed to the privatization and suburbanization of defense research. Reagan's proposed Strategic Defense Initiative—which drew heavily on technologies promoted by the growing defense sector—further polarized scientists, as it simultaneously undermined long-held beliefs about deterrence among the nation's nuclear weapons scientists, and provoked a broad opposition among previously reticent scientists who could now cast their criticisms in moral, political, and technical terms.

Chapter One: Scientists, Sputnik, and the Test Ban

The tumultuous postwar years saw a scientists' movement that pushed for civilian control of nuclear energy, bitter disputes over the development of the hydrogen bomb, the subsequent shift towards thermonuclear weaponry, and deep political discord over the Oppenheimer case. Throughout this period, many elite physicists continued to promote their views on nuclear arms, through organizations like the Federation of American Scientists and the *Bulletin of the Atomic Scientists* and as military and government advisors. During the early 1950s, a small core of top scientists, including Edward Teller, Jerome Wiesner, Lee DuBridge, and others, advised President Eisenhower on nuclear technology and related strategy. Teller in particular contributed to the development of Eisenhower's New Look policy, which depended on extensive nuclear stockpiles. After the launch of the Soviet satellite Sputnik, some of these early science advisors ascended to more powerful and public advisory positions in the White House, but they also brought along a host of Manhattan Project veterans and other physicists who were deeply committed to arms control and at least privately opposed to the tenets of the New Look. This group shifted the advisory balance away from the hawkish recommendations of Edward Teller and its members were arguably responsible for the Eisenhower administration's support for a testing moratorium and test ban negotiations beginning in 1958. These science advisors also encouraged the dramatic expansion of government and military research contracts on university campuses. The new research and advisory mechanisms set up by Eisenhower in the aftermath of Sputnik formed both the apparatus with which scientists would come to exercise their greatest political influence during the Cold War, and the institutional ties that would later bind government scientists to the horrors of the Vietnam War.

A second turning point occurred with the accession of President John F. Kennedy and his appointment of Robert McNamara as Secretary of Defense. McNamara's rejection of the New Look in favor of "flexible response" and non-nuclear military options ingratiated him with the coterie of pro-arms control science advisors who remained in place after the change in administration. Their alliance was crucial for the elevation of limited war research; the resulting contributions to the war in Vietnam are the subject of Chapter 2. In the meantime, Kennedy's science advisors, led by MIT's Jerome Wiesner, pushed hard for a comprehensive test ban, which they viewed as an important step towards disarmament. Their efforts in this regard reveal the extent (and the limitations) of their influence, as well the strong affinities among Kennedy, McNamara, and the community of arms control scientists.

Eisenhower, Nuclear Strategy, and Pre-Sputnik Science Advising

Despite authorizing the use of atomic weapons in Japan and the development of the hydrogen bomb, President Truman had left little in the way of a coherent nuclear strategy for his successor, Dwight Eisenhower.[1] Truman's rhetoric of containment, as espoused in the sweeping policy guide NSC-68, had emphasized "symmetry": U.S. military responses calibrated to the level of an enemy's provoking action. But the allure of the stockpile and pressure from the military services, particularly the Air Force, were too great. The state of nuclear weapons technology in the early 1950s required bomb delivery via aircraft, and the Air Force stood to gain enormously in stature and influence with a shift in security policy that emphasized nuclear capabilities. Pressed by the Joint Chiefs of Staff and Air Force's Curtis LeMay, who headed the

[1] For more detailed discussions of the nuclear strategies of the Truman and Eisenhower administrations, see, for example, David Alan Rosenberg, "The Origins of Overkill: Nuclear Weapons and American Strategy, 1945-1960," *International Security* 7, No. 4 (Spring, 1983), 3-71, and John Lewis Gaddis, *Strategies of Containment: A Critical Appraisal of American National Security Policy During the Cold War* (New York: Oxford University Press, 2005).

Strategic Air Command, Truman eventually approved a dramatic expansion of the nation's nuclear stockpile.

The ascendant Air Force thus viewed Eisenhower's election in 1952 with optimism. David Rosenberg writes in his chronicle of nuclear weapons policy during this period: "Where Harry Truman viewed the atomic bomb as an instrument of terror and a weapon of last resort, Dwight Eisenhower viewed it as an integral part of American defense, and, in effect, a weapon of first resort."[2] Whatever Eisenhower's private views of nuclear weapons, his nuclear strategy, dubbed the New Look, relied on enormous first-strike capabilities, what Secretary of State John Foster Dulles referred to as a "deterrent of massive retaliatory power." As John Lewis Gaddis writes, the new posture of asymmetry rested upon "the implied threat to use nuclear weapons upon minimal provocation."[3]

Curiously, the shift in policy arose from Eisenhower's desire to avoid the kind of "garrison state" he feared would be created with enormous military budgets. The New Look, with its emphasis on nuclear weapons rather than conventional forces, was *cheap,* and military expenditures, calculated as a percentage of GDP, actually declined during the Eisenhower years. Moreover, by promoting deterrence through fear of nuclear attack and relying on allies to provide lesser conventional forces, Eisenhower hoped to avoid American involvement in protracted limited wars. [4]

All this meant that in the mid-1950s, the nation's nuclear scientists and the Air Force's Strategic Air Command enjoyed the benefits of government funding and political power. At the

[2] Rosenberg, "The Origins of Overkill," 28.

[3] Gaddis, 145.

[4] Gaddis, 162-196.

urging of Edward Teller, the Lawrence Livermore Laboratory, a massive nuclear weapons research facility meant to rival Los Alamos, was constructed in southern California in 1952. From 1953 to 1958, the lab's staff expanded from just under 700 to over 3,000 employees, with a budget increase of over 1500%.[5] Meanwhile, Air Force leaders engaged in a process critics dubbed "bootstrapping"; as David Rosenberg explains, "Air Force-generated target lists were used to justify weapons production, which in turn justified increased appropriations to provide matching delivery capability. … Air Force target lists steadily outpaced accelerating stockpile growth." In 1953, the nuclear stockpile contained roughly 1,000 weapons; by 1960, it had expanded eighteenfold.[6] All the while, Curtis LeMay pushed for massive strike capabilities and lengthy target lists of Soviet cities and industrial centers.

The prominence of the Strategic Air Command and the New Look did not go unchallenged. Arrayed against SAC and its supporters were the Joint Chiefs of Staff, much of the Army and Navy leadership, and even Rand consultants, who rejected the SAC focus on urban areas as key targets. Within the Army, Maxwell Taylor led the push for a reorientation toward limited war and conventional forces. He debated military priorities with the president on multiple occasions, urging "mutual deterrence" rather than "massive retaliation," but Eisenhower resisted.[7]

Scientists were caught in this infighting, increasingly asked to weigh in on the value and feasibility of various weapons systems. Princeton mathematician John von Neumann had chaired

[5] Hugh Gusterson, *Nuclear Rites: A Weapons Laboratory at the End of the Cold War* (Berkeley: University of California Press, 1996), 26; Herbert York, "The Origins of the Lawrence Livermore Laboratory," *Bulletin of the Atomic Scientists,* 1 September 1975.

[6] Rosenberg, "The Origins of Overkill," 23.

[7] Rosenberg, "The Origins of Overkill," 40.

a Strategic Missiles Evaluation Committee that recommended the development of intercontinental ballistic missiles (ICBMs), a technology that would strip the Air Force of its nuclear dominance by allowing nuclear weapons to be delivered via missiles rather than bombers. In March 1954, the Office of Defense Mobilization's Science Advisory Committee met with Eisenhower to discuss the state of the country's "technological capabilities" in the context of nuclear war. The committee had already been investigating the state of military technology at the behest of Trevor Gardner, the Air Force's Assistant Secretary for Research and Development. (James Killian, the president of MIT and a key member of the ODM-SAC, later described Gardner as "technologically evangelical" and enormously influential.[8]) At the meeting, Eisenhower warned the scientists of the nation's vulnerability to a surprise attack, and requested that they evaluate potential technical solutions. Soon, a special task force was established, headed by a steering committee of MIT's James Killian, Caltech's Lee DuBridge, Polaroid's Edwin Land, and a handful of other ODM-SAC advisors, assisted by forty additional scientists and engineers serving as a "professional staff."[9]

The 1954 report of the Technological Capabilities Panel, as it came to be called, offered a prescient view of American vulnerabilities and the future of the arms race. The panel urged numerous improvements: enhanced intelligence, including a program of high-altitude U-2 surveillance; expanded communications capabilities; greater support for basic science; and better preparedness in the case of a surprise attack. On this last point, the scientists pinpointed existing SAC weaknesses and urged that bases be "hardened" and nuclear resources be dispersed or airborne to prevent easy targeting. Most crucially, the panel predicted that by the end of the

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[8] James Killian, *Sputnik, Scientists, and Eisenhower: A Memoir of the First Special Assistant to the President for Science and Technology* (Cambridge, MA: MIT Press, 1977), 68.

[9] Killian, 70.

decade, the age of the bomber would have waned and the age of intercontinental and intermediate range missiles would begin. As von Neumann had earlier recommended, the panel called for development of both land-based and submarine-based missiles, and for early "theoretical and experimental" investigation into anti-missile defenses.

Eisenhower was smitten by both the advice and the advisors; three years before Sputnik and the creation of the Presidential Science Advisory Committee (PSAC), the panel had established an important precedent regarding the value of science advising. As David Rosenberg writes, "Eisenhower was very much impressed by the TCP report, and from this time on he relied increasingly on the advice of scientists, whom he viewed as honest brokers with regard to the complex and often politicized issues of a nuclear strategy."[10]

If Eisenhower was learning to appreciate scientists in the summer and fall of 1954, the wider scientific community was learning to fear the federal government during the same period. In the spring of 1954, Robert Oppenheimer, accused of Communist sympathies, had been stripped of his security clearance after lengthy hearings during which many of the leading Manhattan Project physicists testified on his behalf. Edward Teller had taken the opposing side, a position which would permanently alienate him from much of physics' academic elite. The case came to symbolize the excesses of McCarthyist America. Years later, Killian would credit the TCP panel with helping to re-knit the frayed relations between scientists and the federal government. The scientists on the panel were "citizens who felt an obligation to their country that overrode their dismay about a single administration," he wrote, and Eisenhower's respect and request for assistance paved the way for renewed cooperation.[11] Killian's retroactive gloss

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[10] Rosenberg, 39.

[11] Killian, 77-79.

likely overstated the panel's impact, but within three years, in the aftermath of Sputnik, the bonds between the White House and elite physicists would be strengthened enormously.

Science and the Sputnik Boom

If the Manhattan Project catapulted elite scientists into the political sphere and the Oppenheimer case alienated them, the launch of Sputnik in October 1957 brought them overwhelmingly back into the government fold. James Killian, the MIT president soon to be tapped as Eisenhower's Special Assistant for Science and Technology, later reflected that "while scientists possessed immense prestige in Washington during the years following World War II and historic actions were taken during the Truman administration to institutionalize science and technology in government, science had a uniquely close relationship to the presidency during Eisenhower's second term and extending into the Kennedy administration."[12] The pivotal event was the public demonstration of Soviet achievements in space and rocket technology, embodied by Sputnik and potentially signifying a "science gap" afflicting the United States. The event led to the creation of an extensive federal science advisory apparatus, a splurge in funding for research and development, and a short-lived "golden age" for the political influence of science advisors.

Eisenhower's initial reaction to the satellite had been to downplay its significance, but intense media coverage quickly required further presidential action. In response, Eisenhower announced the creation of a new cabinet position: the Special Assistant for Science and Technology, who would also head up the new Presidential Science Advisory Committee

[12] Killian, xv.

(PSAC). His choice for this influential position was James Killian, a non-scientist who had amassed an impressive track record in science administration, first with the WWII-era National Defense Research Committee and, since 1948, as president of MIT. As Killian knew, the PSAC was not an entirely novel creation; as a later White House history explained, Eisenhower had "transferred the Science Advisory Committee of the Office of Defense Mobilization to the White House office, reconstituting and enlarging it" into the PSAC.[13] Killian had been a prominent member of ODM-SAC, as it had been called, and had witnessed firsthand the influence of its Technological Capabilities Panel in 1954. The ODM-SAC had operated largely behind-the-scenes, however, and Killian was honored to chair the more exalted PSAC, which, as he put it, "was to be positioned at the very summit of government."[14]

In his letter to Killian explaining his new duties as the Special Assistant, Eisenhower directed him to keep abreast of "the use of science and technology in relation to national security" and to advise the president on all related matters. The president granted Killian "full access to all plans, programs, and activities involving science and technology in the Government, including the Department of Defense, AEC, and CIA," and invited him to National Security Council and other classified meetings.[15] He also tasked Killian with staffing and organizing the PSAC, which subsequently grew to comprise an extraordinary collection of the nation's top scientists. PSAC members included Hans Bethe, the Cornell physicist and future Nobel Prize winner; James Killian, the president of MIT; Jerome Wiesner, the future president of MIT; George Kistiakowsky of Harvard; Edward Purcell and I.I. Rabi, both Nobel Prize winners in

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[13] "Administrative History," LBJ, Administrative History, Office of Science and Technology, Box 1, Folder Volume I Administrative History.

[14] James Killian, *Sputnik, Scientists, and Eisenhower: A Memoir of the First Special Assistant to the President for Science and Technology* Cambridge, MA: MIT Press, 1977, xv.

[15] Killian, 35-36.

physics; Livermore director Herbert York (who considered himself the token representative from the "nuclear weapons establishment"); and others with similarly prestigious pedigrees.[16] These scientists, in many ways the architects of the Sputnik boom, were largely academic physicists. Veterans of the Manhattan Project or radar research during World War II, they were patriotic, anti-communist, and idealistic, happy to offer part-time or full-time government service while maintaining their academic positions. Whatever disillusionment had been spawned by the Oppenheimer case, the PSAC scientists were enthusiastic about their new, expanded roles as government advisors; they considered national service and national security part of their obligation as scientists. As Killian recalled, perhaps a bit rosily in hindsight, "The group was held together in close harmony not only by the challenge of the scientific and technical work they were asked to undertake but by their abiding sense of the opportunity they had to serve a president they admired and the country they loved."[17]

Not surprisingly, much of the PSAC's early work concerned science and technology related to defense and the space program. Although some PSAC materials remain classified and no formal minutes of their meetings were kept "in order to promote full and uninhibited discussion," internal summaries of their work are now available.[18] During the first years of its existence, the PSAC participated in defense budget reviews, submitting "specific recommendations … concerning strategic delivery systems, air and ballistic missile defense, limited war, anti-submarine warfare, communications, and intelligence." The scientists' concerns were discussed at the top levels of government: by the President, the Secretary of Defense, and

[16] Herbert York, *Making Weapons, Talking Peace: A Physicist's Odyssey from Hiroshima to Geneva* (New York: Basic Books, 1987), 105.

[17] Killian, xix.

[18] "Administrative History," undated, "Volume I Administrative History" Folder, Box 1, Administrative History, Office of Science and Technology, LBJ Library.

the Joint Chiefs of Staff. With its memberships organized into technology-specific panels, the PSAC also provided "progress reports" for all the major "missile and satellite programs." George Kistiakowsky, who would succeed Killian as Eisenhower's second Special Assistant for Science and Technology, chaired the enormously influential PSAC Ballistic Missiles Panel. In March 1958, Kistiakowsky and Killian met with Eisenhower to discuss the ballistic missile program, after the panel had prepared a technical report building on the conclusions of the old Technological Capabilities Panel and offering a "national program for ballistic missile development over the coming years." That year, Kistiakowsky's panel called for the cancellation of the Jupiter missile, the continuation of Thor, and the accelerated development of solid, storable propellants. (These solid propellants would soon get their own ad hoc panel, led by Kistiakowsky and including the crème de la crème of academic and industrial chemists.) The panel offered qualified support for the Minuteman program, but reserved its greatest enthusiasm for the submarine-based Polaris missile system, which it deemed "less vulnerable (and of comparable cost to) the several land based systems proposed." Thus PSAC quickly revealed itself as unafraid to call for the cancellation of major weapons programs. In nearly every case, members opted to elevate technologies necessary for minimal deterrence over those required for massive first and second strike capabilities.

Also prominent was Jerome Wiesner's panel on ballistic missile defense. In its very first report, the panel noted the "extremely difficult nature and the great uncertainties involved" in the interception of ballistic missiles, and as early as the spring of 1958, Wiesner was warning about "the decoy problem"—the challenge of identifying and destroying an incoming missile if it were surrounded by decoy projectiles. Though the panel supported an "experimental" anti-ballistic missile system, they opposed any "large-scale" development of "the presently conceived Zeus system," partly on the grounds that it was vulnerable to "tactics of confusion and decoy." Instead, the panel recommended that at least through 1965, a better option to interception was "passive defense, i.e., dispersal, hardening, concealment, and quick reaction." In 1960, Stanford physicist Wolfgang Panofsky took over an expanded version of the panel, but despite the leadership change, "the Panel saw no reason to change the major PSAC conclusions previously drawn to the effect that large-scale production of Zeus was not warranted, and that the presently configured system could not provide an effective defense against a determined enemy." Thus, from its earliest years of existence and despite changing panel membership, the PSAC was a strong source of opposition to the development of a large-scale anti-ballistic missile system and a key check on its expansion.

Meanwhile, other PSAC panels explored the problems of anti-submarine warfare, non-lethal chemical and biological weapons, surprise attack, and, as will be discussed, arms control and the enforcement of a nuclear test ban. Outside of these explicitly defense-related topics, the PSAC also issued reports on science education and basic research. The PSAC's influence extended beyond the panels and reports, however. PSAC members met formally and informally with a host of key government officials, and, particularly during the Eisenhower and Kennedy administrations, enjoyed extensive access to the president and top cabinet members. As Killian recalled, "PSAC was usually able to go directly to the president with its advice," and Eisenhower was known in later years to continue to refer to PSAC as "my scientists." On his deathbed, Eisenhower reportedly told Killian that PSAC had been "one of the few groups that I encountered in Washington who seemed to be there to help the country and not help themselves."[19]

[19] Killian, 115, 241. Also quoted in Robert A. Divine, *The Sputnik Challenge* (New York: Oxford University Press, 1993).

The influence of scientists expanded beyond the White House as well, through the reorganization and invigoration of advisory mechanisms in the Pentagon and the military services. The Air Force's Scientific Advisory Board (AFSAB) expanded its membership from 51 in 1958 to 88 in 1962, with the number of outside consultants more than doubling.[20] The Department of Defense Reorganization Act of 1958 established a new Pentagon position, the Director of Defense Research and Engineering, reporting directly to the Secretary of Defense and tasked with supervising Pentagon research activity. Herbert York was the first to hold the new title. The Advanced Research Projects Agency (ARPA), created in February 1958 "as a separate operating agency with the Department of Defense," was dedicated to identifying and supporting innovative, cutting edge science and technology projects. (After 1959, ARPA's Chief Scientist reported to the new Director of Defense Research and Engineering.) As the new agency alerted potential contractors, ARPA projects might have obvious military applications or not; the goal was to "'leap frog' the present state of the art in assigned fields to attain a dynamic, forward approach to defense of the United States." Flexibility and collaboration were key, as for any given problem, " all possible approaches are considered and the selected approach is accomplished by utilizing the best capabilities of both government and industry." Early areas of research included anti-ballistic missile defense, materials research, toxicology, alternative energies (including nuclear and solar power), pre-NASA space technology, solid propellant chemistry for rocket development, and the VELA nuclear test detection program.[21]

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[20] In 1962, the Kennedy administration would restrict AFSAB's hiring practices to avoid conflicts of interest, and would cap its membership at 70. Not surprisingly, the number of AFSAB reports, memos, and meetings dropped off significantly between 1962 and 1963. See: Thomas A. Sturm, *The USAF Scientific Advisory Board: Its First Twenty Years, 1944-1964* (Washington, D.C.: Office of Air Force History, 1986), 89, 117.

[21] ARPA pamphlet, "The Advanced Research Projects Agency," August 1960, in Box 175, Folder 1, Papers of Lee A. DuBridge, Archives, California Institute of Technology (hereafter DuBridge).

Another key development of the post-Sputnik years was the inauguration of the Jason program under the auspices of the Institute of Defense Analyses (IDA). The IDA had been established in the 1956 as a federally funded contract research center (FFCRC), and served as a mechanism to provide civilian experts from academia to the Pentagon and military services, particularly the Weapons Systems Evaluation Group (WSEG), itself a postwar creation linking military and civilian intellectual resources. James Killian served as the IDA's first Chairman of the Board of Trustees, and its founding member universities included Caltech, the Case Institute of Technology, MIT, Stanford, and Tulane. In the years after Sputnik, IDA's collaborations expanded beyond WSEG to include work for ARPA and the launch of a special facility dedicated to mathematics and communications research, located on the Princeton University campus. Columbia, Penn State, and Stanford joined as institutional members. In 1959, IDA created the Jason Division, envisioned as an "opportunity for outstanding academic physicists to devote their consulting time to scientific problems with defense implications while remaining in the academic community."[22] Participants would devote one day a week to Jason work during the academic year, and several weeks during the summer, with pay at roughly $200 a day.[23] Herbert York and John Wheeler personally sent recruiting letters to young academic superstars like Caltech's Murray Gell-Mann, describing the search for "15 or 20 of our best scientists with a strong interest in the defense of our country."[24] The assembled scholars, who included multiple future Nobel Prize winners, would go on to provide significant advice to the Pentagon on

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[22] IDA Annual Reports, 1956, 1966, Box 175, Folder 3, DuBridge.

[23] Townes to MGM, 20 February 1961, Box 35, Folder 2, Murray Gell-Mann Papers, 10219-MS, Caltech Archives, California Institute of Technology (hereafter MGM); Gell-Mann's Jason contract, Box 35, Folder 3, MGM.

[24] Wheeler to Gell-Mann, 1958, Box 57, Folder 1, MGM.

weapons systems and arms control measures, as well as appropriate technologies and strategies for the nascent war in Vietnam.

Research and Education

Beyond his own desire to expand scientific expertise in the government, Eisenhower also faced considerable public pressure to increase federal funding for science and engineering research and education. This was quickly translated into policy: The National Defense Education Act of 1958 provided federal money for scholarships and instruction in math and science. Federal funding for scientific research and development expanded dramatically, particularly to NASA, the Atomic Energy Commission, and the Department of Defense and the military branches. $5 billion dollars was added to the defense budget of 1958.[25] NASA's funding would double every year between 1958 and 1964.[26] The budget for the National Science Foundation increased by 300% for 1959.[27]

These agencies, in turned, expanded their research contracts with universities, where much of the actual research would take place. Around the country, the new funding fueled the creation or the expansion of a spate of special university labs, including MIT's Lincoln Lab and Instrumentation (or Draper) lab, Berkeley's Livermore Lab, and Stanford's Applied Electronics Lab. To paraphrase Randolph Bourne's observation that "war is the health of the state," during the late 1950s and early 1960s, the Cold War was the health of academic science.

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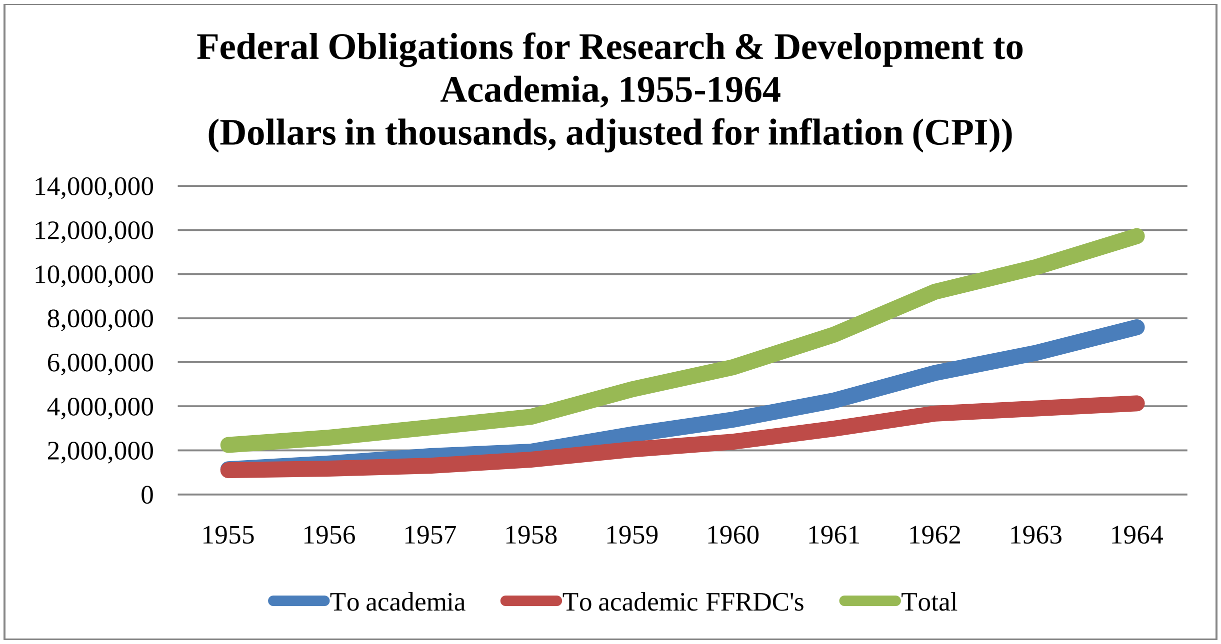
[25] Robert A. Divine, *The Sputnik Challenge* (New York: Oxford University Press, 1993), 171.

[26] Noted in Megan Prelinger, *Another Science Fiction: Advertising the Space Race 1957-1962* (New York: Blast Books, 2010), 139.

[27] David Kaiser, "The Physics of Spin: Sputnik Politics and the American Physicists in the 1950s," *Social Research* 74, No. 4 (1 December 2006), 1225-1252.



Source: National Science Foundation, Federal Funds for Research and Development, Detailed Historical Tables: Fiscal Years 1951-2002, http://www.nsf.gov/statistics/nsf03325/ (accessed 30 May 2008).

As had been true during WWII, this expansion and coordination was a process encouraged and organized in large part by scientists, particularly academic scientists. Although a handful of prestigious scientists, including James Conant of MIT and Alan Waterman of the National Science Foundation, had urged Eisenhower to exercise caution in the rapid expansion of funding for graduate science education, on the other side was a host of equal prominent experts recommending the opposite, including I.I. Rabi, whom Eisenhower knew well from his years at Columbia University. David Kaiser writes, “Meeting just a week and a half after Sputnik’s launch, Rabi pressed Eisenhower to use the satellite as a pretext for bulking up American scientific manpower.”[28] Despite the cautious attitudes of a few high-level advisors, overall scientists’ enthusiasm for at least some expansion of science education and research support was overwhelming, and extended far beyond the moderate scientists who had Eisenhower’s ear. At the hawkish end of the political spectrum, Edward Teller, father of the hydrogen bomb,

[28] David Kaiser, “The Physics of Spin”: 1235.

promoted "more applied research" at universities, government labs, and industry."[29] At the dovish end, Eugene Rabinowitch, founder of the *Bulletin of the Atomic Scientists*, encouraged the expansion of American science and applied research "unhampered by budgetary considerations."[30]

From almost the moment of its creation, PSAC proved a powerful advocate for expanded support for science education and research. In December 1958, the PSAC Panel on Research Policy, chaired by physicist Emanuel Piore, released a report,"Strengthening American Science," which urged better national coordination of key research programs, resulting in March 1959 in the creation of the Federal Council for Science and Technology. Caltech president Lee DuBridge headed a Science and Engineering Education panel, which predictably urged improved standards and "public support to meet these goals." Berkeley chemist and Nobel laureate Glenn Seaborg oversaw the production of "Scientific Progress, the Universities, and the Federal Government," a report emphasizing research and training. Other panels tackled government research and development policies, including support for national laboratories and general contracting mechanisms.

In the summer of 1960, the PSAC issued an urgent call for increased federal support for basic science, "in order for the United States to maintain its pre-eminent position in basic science." Because basic research is "high risk," the scientists argued, many "profit-motivated individuals and corporations" were unwilling to offer sufficient investment and support: "Only the Federal Government can afford to support basic science on the scale necessary to insure continued national progress." National progress included national security, living standards, and

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[29] Teller to Daddario, 18 September 1963, Box 171, Folder 4, DuBridge.

[30] Eugene Rabinowitch, "After Missiles and Satellites, What?" *The Bulletin of Atomic Scientists,* December 1957, 346-350.

public health. In this vein, a host of PSAC panels focused on specific areas of scientific research that merited greater support, the "hitherto little supported areas of science" now deemed to be "of critical importance to the national welfare." These included "materials science, oceanography, and atmospheric science," emerging "interdisciplinary" fields in which research was conducted in "both universities and government-owned establishments." In conjunction with the Federal Council for Science and Technology, the PSAC panels recommended the establishment of "interdisciplinary laboratories on university campuses," and long-term support for research facilities and training.[31]

David Kaiser has demonstrated the primacy of scientists themselves in the creation of a pivotal 'Sputnik moment' for science. "Sputnik had no automatic political valence," he writes, "technopolitical events rarely do." Rather, the "determined lobbying by physicists and others" transformed the launch of a satellite into "a political event requiring a specific political response." The political response enriched scientists, particularly physicists, as it "helped to drive an unprecedented explosion in physics enrollments in the United States, outstripping every other field in rates of growth."[32] Kaiser suggests obliquely that self-interest may have underlay the lobbying efforts of scientists in the aftermath of Sputnik. Certainly, they—and their research institutions—benefited financially from the new policies.

But for many of the future members of PSAC, Sputnik raised genuine concerns about Cold War security and the state of American science. Perhaps even most importantly, the elevation of elite physicists to positions of political and military influence also offered an

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[31] "Major Actions of the President's Science Advisory Committee, November 1957-January 1961," 13 January 1961, "PSAC 1/61-3/61" Folder, President's Office Files (POF), Box 86, John F. Kennedy Library (JFKL).

[32] Kaiser, "The Physics of Spin": 1247.

opportunity more precious than the millions of dollars of research funds: the chance for nuclear redemption in the form of arms control.

Imagining a Test Ban

Even as Eisenhower promoted the nuclear-heavy New Look, his administration also oversaw a parallel, contradictory effort to negotiate a test ban treaty, nonproliferation measures, and disarmament with the Soviet Union. In the winter of 1954, the United States conducted a series of nuclear tests from facilities at Bikini Atoll, part of the Marshall Islands in the Pacific, including the "BRAVO test" of a fifteen-megaton hydrogen bomb on March 1. Newspapers initially reported that the March 1 blast had "obliterated its test island" and "unleashed violence so tremendous that even its designers were amazed."[33] But within days the press was reporting something else: the presence of a Japanese fishing boat that had been "showered with radioactive ash" during the testing. The ship, the Fukuryu Maru, had been logged outside the location of the testing area, yet all 23 of the fishermen aboard suffered symptoms of radiation sickness and were immediately hospitalized. Pressed for an explanation, the AEC's Gen. Strauss finally attributed the disaster to "an unexpected shift in the direction of the prevailing winds in the higher altitudes," an explanation rooted in surprising weather patterns, not surprises in the performance of thermonuclear detonations.[34] Later reports estimated that over three hundred—and perhaps thousands—of observers and island residents had been exposed to varying levels of radioactive fallout.[35]

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[33] "Hydrogen Bomb Blast Amazes Atom Experts," *Los Angeles Times,* 14 March 1954, 1.

[34] "AEC Statement on Bomb's Effects," *Los Angeles Times,* 16 February 1955, 14.

[35] Martha Smith-Norris, "The Eisenhower Administration and the Nuclear Test Ban Talks, 1958-1960," *Diplomatic History* 27, No. 4 (September, 2003), 506; Gregg Herken, *Cardinal Choices: Presidential Science Advising from the Atomic Bomb to SDI* (New York: Oxford University Press, 1992), 84.

Almost immediately, the international community registered its disapproval; a spate of countries, including Japan and India, called for an immediate testing halt, as did antinuclear activists in the United States. Much of the early controversy focused on the dangers of fallout. As the Duke University physicist L.W. Nordheim put it, "Dangers due to radioactivity seem to stir human emotions to a much greater extent than other man-produced hazards; understandably, since radioactivity can be neither seen nor felt… [it] can travel in the air over large distances, and its very nature and action are unknown and unfamiliar to most people."[36] In 1955, the *Bulletin of the Atomic Scientists* published an influential series of related articles by physicist Ralph E. Lapp. A Manhattan Project veteran and former head of the nuclear physics division for the Office of Naval Research, Lapp had been one of the signers of the 1945 University of Chicago petition urging President Truman not to use the bomb on Japanese civilians. Now, shocked by the BRAVO test, he turned his efforts to research and writing, producing exposes on the dangers and the extent of the BRAVO fallout, government secrecy on the matter, and the inadequacies of current civil defense preparations.[37] The articles presaged a full-length 1957 book, *The Voyage of the Lucky Dragon,* a best-selling account of the ill-fated Japanese fishing ship. More formally, the Federation of American Scientists called on the United Nations to conduct a study of the "potential dangers in atomic and thermonuclear bomb tests" on the grounds that the BRAVO test had shown that "effects cannot be restricted within national boundaries, and that the lives and

[36] L.W. Nordheim, "Tests of Nuclear Weapons," *Bulletin of the Atomic Scientists,* 1 September 1955. Nordheim did not actually oppose to testing. Noting that testing was necessary to ensure that the United States maintained a deterrent nuclear force, he wrote that "The dangers of war are so immeasurably greater that the hazards of tests pale in comparison."

[37] See, for example, Ralph Lapp, "Radioactive Fall-out," *Bulletin of the Atomic Scientists,* 1 February 1955; "Fall-out and Candor," *Bulletin of the Atomic Scientists,* 1 May 1955; and "Global Fall-out," *Bulletin of the Atomic Scientists,* 1 November 1955. Lapp's worldview was deeply influenced by his nuclear experiences; in 1965 he published an iconoclastic study of science in American society, dedicated to Szilard and warning of the widening gap between powerful science experts and non-scientist citizens. See Ralph E. Lapp, *The New Priesthood: The Scientific Elite and the Uses of Power* (New York: Harper & Row, 1965).

health of people in other countries are endangered." The group warned of possible radioactive contamination of the earth's atmosphere, worsening international relations, and the risk of genetic mutations. The time had come, they wrote, to bring "one facet of the atomic armaments race and the threat of war into the spotlight of human morality."[38]

As warnings about the fallout risks of above-ground nuclear explosions proliferated, arms control advocates saw a new potential path to disarmament. In November 1954, David Inglis, a physicist at the Argonne National Laboratory and founding FAS member who had worked on the Manhattan Project, wrote an impassioned appeal for a testing ban in the *Bulletin.* His goal was not simply to reduce the risks of fallout, but to alter the course of the arms race itself. He acknowledged that an internationally-enforced test ban "provides no disarmament and provides arms limitation only indirectly by limited the development of new types of arms." But it could nevertheless achieve something important: "*it would slow down the rate of development of new techniques of offense and allow the techniques of defense to come closer to catching up.*" He imagined a world where the two superpowers would maintain only aging stockpiles of "old-fashioned" H-bombs, but would pursue cutting-edge new technologies of detection and interception. Meanwhile, in the absence of testing, no other nations could develop thermonuclear weapons. Inglis was part of a small but influential group of scientists urging support for this "small but significant and practical measure of arms limitation." Inspired, the Federation of American Scientists, after conducting a poll of its membership, added a nuclear test ban to its political goals.[39] But even on the pages of the *Bulletin*, some disagreement about the necessity of

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[38] Federation of American Scientists, "Proposal for a United Nations Commission to Study the Problem of H-Bomb Tests," *Bulletin of the Atomic Scientists,* 1 May 1955.

[39] W.A. Higinbotham, "A Brief History of FAS," *FAS Newsletter,* November 1962, 2. This was not the first call for a test ban; for example, Vannevar Bush had called for an end to thermonuclear testing as early as 1952. See S.S. Schweber, *In the Shadow of the Bomb: Oppenheimer, Bethe, and the Moral Responsibility of the Scientists* (Princeton, NJ: Princeton University Press, 2000), 168.

a test ban persisted, with scientists weighing in on both sides of the issue. (Tellingly, Inglis's initial statement had been accompanied by a sidebar advertising employment at Lockheed's Missile Systems Division.[40])

Such dissension existed at the highest level of government as well. The BRAVO disaster had prompted the first of a series of Soviet proposals for disarmament measures and a testing moratorium.[41] Smith-Norris reports that although Dulles saw a "political advantage" in agreeing to a two-year testing halt, Eisenhower was initially "unenthusiastic" and views within the administration pitted the State Department against the Pentagon, Joint Chiefs, and the AEC, preventing consensus.[42] In the aftermath of the BRAVO test, Eisenhower was advised by the President's Special Committee on Disarmament Problems, led by Harold Stassen, the Special Assistant for Disarmament, and including representatives from the State Department, the Pentagon, the CIA, and the AEC (whose contingent included Princeton's John von Neumann). Teller and Strauss argued strenuously against a testing halt, and their position proved ascendant throughout Eisenhower's first term.

The remaking of federal science advising after Sputnik changed the dynamics of the debate. The influx of new PSAC scientists, many of whom had sharp political and personal disagreements with Teller, slowly helped shift attitudes about testing. Though Eisenhower's various advisory groups had failed to reach a consensus on the issue, in the spring of 1958, Khrushchev and Eisenhower both agreed to study the possibility of a monitored test ban. A PSAC panel on Arms Limitation and Control was established to research the "military and

[40] David R. Inglis, "Ban H-Bomb Tests and Favor the Defense," *Bulletin of the Atomic Scientists,* 1 November 1954.

[41] Smith-Norris, 507

[42] Smith-Norris, 508.

technical aspects of possible arms limitation agreements," and ultimately recommended the establishment of an executive office devoted to arms control, the US Disarmament Administration.[43] Hans Bethe chaired an ad hoc PSAC working group dedicated to the test ban. 1958 also saw the origins of the VELA nuclear test detection program, a collaborative effort by the Pentagon and the Atomic Energy Commission to explore techniques to monitor nuclear testing remotely. Much of the research and design was conducted under the auspices of Los Alamos and ARPA, with support from PSAC and significant advisory input from the young members of the Institute for Defense Analyses' Jason group. By 1960, VELA would have three component programs underway, corresponding to the major testing environments. VELA-Uniform covered seismic and other forms of monitoring to detect and distinguish underground nuclear tests from earthquakes or non-nuclear explosions; VELA-Sierra explored earth-based means of detecting high-altitude tests; and VELA-Hotel was concerned with space-based monitoring technologies.[44] Without adequate monitoring to prevent cheating, neither the US nor the USSR was likely to agree to a test ban. (Not surprisingly, VELA was an enormously popular project for arms-control minded scientists and other intellectuals. In April 1961, Grayson Kirk, the president of Columbia University, wrote to PSAC member Lee DuBridge to request a meeting to discuss "how best university scientists can contribute to the success of the nation's efforts in seismology," specifically, ARPA's VELA program.[45])

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[43] "Major Actions of the President's Science Advisory Committee, November 1957-January 1961," 13 January 1961, "PSAC 1/61-3/61" Folder, POF, Box 86, JFKL.

[44] Los Alamos Lab Policy Committee notes, 7 June 1960 and 10 January 1961, Series IV, Box 2: Subject Files I-N, "Los Alamos Scientific Laboratory Policy Committee [minutes, outlines] 1960-61" Folder, Stanislaw Ulam Papers, American Philosophical Society; Pamphlet, "The Advanced Research Projects Agency," August 1960, Box 175, Folder 1, DuBridge.

[45] Grayson Kirk toDuBridge, 13 April 1961, Box 175, Folder 1, DuBridge.

While scientist-activists like Barry Commoner and Linus Pauling emphasized the health risks of fallout from atmospheric testing, the Federation of American Scientists promoted a test ban as a step towards arms control and disarmament. The membership rolls of the antinuclear organization SANE swelled to over 25,000, and Dulles and Eisenhower felt, in the words of a later internal history, a "growing pressure on the United States Government to make some move toward the cessation of nuclear testing."[46] In response to the substantial domestic and international clamor for a testing ban, on July 30, 1958, a new compromise proposal came from an unexpected source: Livermore's Edward Teller and the AEC's Willard F. Libby. Teller, fearing that Eisenhower would be pushed to embrace a sweeping, comprehensive ban, called instead for a "limited test moratorium."[47] To debate the details of this plan, in August the State Department convened a new entity, the "Committee of Principals," initially "an ad hoc group of high ranking officials meeting… on an issue too urgent and specialized to be put into the machinery of the National Security Council." Attendees included Allan Dulles of the CIA, John McCone and Gen. Alfred Starbird of the AEC, Killian, and MIT professor of electrical engineering and PSAC member Jerome Wiesner. Though little documentation of this first meeting has survived, the Committee of Principals surely influenced Eisenhower's announcement later that year of a testing moratorium and the commencement of test ban negotiations with the Soviet Union.

The US conducted its last aboveground test before the moratorium on October 30, 1958, at Yucca Flats in Nevada, and negotiations began the following day at the Geneva Conference on

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[46] "Background, Origin, and Evolution of the Committee of Principals," August 1963, Historical Studies Division, Bureau of Public Affairs, Department of State, "ACDA Comm of Principals 8/63-10/63" Folder, National Security File (NSF), Box 267, JFKL.

[47] Ibid.

the Discontinuance of Nuclear Weapons Tests.[48] The Committee of Principals met throughout that autumn, and more than twenty-five times in 1959 and 1960. On May 1, 1960, an American U-2 surveillance plane was shot down over the Soviet Union, and relations between the two superpowers quickly deteriorated. The breakdown in negotiations coincided with the presidential transition in the United States. Though the majority of his science advisors stayed on through the next administration, Eisenhower left office with the New Look still in place, and with prospects for a permanent test ban dim at best.

Historiography: Eisenhower and the Test Ban

Not surprisingly, the topic of Eisenhower and the test ban is the subject of substantial debate among historians. Since the mid-1960s, scholars have analyzed Eisenhower's views, words, and actions relating to arms control and disarmament as a means of evaluating his overall presidential leadership. Like Smith-Norris, an early school of Eisenhower critics described the president as inherently wary of arms control negotiations with the Soviet Union and unable to exert control over his fractious cabinet.[49] A wave of "revisionist" scholars in the 1980s and 1990s offered a more sympathetic assessment, however, viewing Eisenhower as a competent leader whose support for a test ban was tempered by legitimate external factors, including the weaknesses and pitfalls of existing monitoring and enforcement technology.[50] Smith-Norris and

[48] *Los Angeles Times*, 3 March 1962.

[49] See, for example, Harold Jacobson and Eric Stein, *Diplomats, Scientists, and Politicians* (Ann Arbor: University of Michigan Press, 1966); and Robert Divine, *Blowing on the Wind: The Nuclear Test Ban Debate, 1954-1960* (New York: Oxford University Press, 1978). For additional discussions of Eisenhower historiography, see introductory discussion in Martha Smith Norris, "The Eisenhower Administration and the Nuclear Test Ban Talks: 1958-1960," *Diplomatic History* 27, No. 4 (September, 2003), 503-541.

[50] Examples of the "revisionist" school include Stephen Ambrose, *Eisenhower* (New York: Simon and Schuster, 1984) and Robert Divine, *Eisenhower and the Cold War* (New York: Oxford University Press, 1981).

other "post-revisionists" have challenged this view; as Smith-Norris summarizes, "the United States did not seriously seek an arms-control agreement in the late 1950s… Eisenhower failed to take a decisive stand on the testing issue until the final year of his presidency." In Smith-Norris's view, Eisenhower's commitment to the New Look, with its emphasis on maintaining an overwhelming stockpile, precluded genuine support for arms control and disarmament, for which a test ban constituted a crucial preliminary step.[51]

More recently, and of greater relevance to the discussion here, a fourth school of historians have revived the debate, again presenting Eisenhower as supportive of a test ban, but focusing their analysis on the role of science advisors. Benjamin P. Greene has argued that during the pre-Sputnik years of Eisenhower's first term, nuclear policy was heavily influenced by science hawks Edward Teller and AEC chairman Lewis Strauss, both of whom had earlier promoted the development of the hydrogen bomb, orchestrated the political marginalization of Oppenheimer, and advocated expansion of the weapons stockpile. Greene suggests that these men dissuaded Eisenhower from pursuing a test ban, despite the president's supportive inclinations, and misleadingly suggested a scientific consensus backing their views. Sputnik—and the attendant expansion of science advising—proved a crucial turning point. The new coterie of PSAC advisors entering government in 1957 presented a near-unified front in favor of the test ban and arms control more generally, revealing Teller as a mouthpiece, albeit an influential one, for only a tiny scientific minority. Under the influence of esteemed PSAC members such as Bethe and Rabi, Eisenhower agreed to a testing moratorium in 1958 and entered negotiations for a comprehensive test ban, which, despite periodic setbacks, would culminate in the success of

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[51] For "Post-revisionist" assessments, see Smith-Norris, "The Eisenhower Administration and the Nuclear Test Ban Talks"; Gaddis.

the partial ban during the Kennedy administration.[52] This view is corroborated by Herbert York, the former Livermore director and ARPA Chief Scientist, who later wrote that the approach to arms control "underwent a sea change at the White House level" in the years after Sputnik, a change that included his own growing political commitment to securing a comprehensive test ban.[53]

Taking this assessment a step further, Paul Rubinson has focused on the failure of Eisenhower and Kennedy to achieve a comprehensive test ban, blaming, in part, the weak arguments of his science advisors. These advisors, Rubinson argues, were paralyzed by McCarthyism and "adopted the amoral stance mandated by the Oppenheimer hearing," eschewing bolder moral claims in favor of narrow technical and strategic arguments. Rubinson compares the actions of government 'insider' scientists to outsiders like Linus Pauling, who made what Rubinson considers a stronger moral case against testing, and members of the Pugwash movement, who shifted from moral to technical arguments in the hopes of achieving greater political influence. Framing the debate in these terms meant that the success of the treaty depended on the resolution of tricky technical problems of detection and monitoring, about which sufficient scientific doubt existed to fuel opposing voices like Teller. Rubinson goes so far as to claim that the debates between PSAC members and Edward Teller "revealed science to be a house divided."[54]

These two arguments— that the scientific community was genuinely divided over the test ban, and that unlike outsiders like Pauling, PSAC members abstained from moral arguments out

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[52] Benjamin P. Greene, *Eisenhower, Science Advice, and the Nuclear Test-Ban Debate, 1945-1963* (Stanford, CA: Stanford University Press, 2007).

[53] York, 117.

[54] Paul Rubinson, "Crucified on a Cross of Atoms": Scientists, Politics, and the Test Ban Treaty," *Diplomatic History* 35, No. 2 (April 2011), 283-319.

of fear—deserve further scrutiny. Rubinson's claim that scientists were divided over the test ban suggests that Edward Teller and his Livermore colleague, E.O. Lawrence, represented the views of a substantial segment of the scientific community. In the absence of comprehensive polling or survey data, it is difficult to know how widespread support for and opposition to the ban really were. But scientists' political activity on behalf of the test ban was far more expansive than the public movement against it, which consisted mainly of Teller himself. Thousands of prominent scientists signed petitions in favor of the test ban, but no organized movement of scientists existed to oppose it. Reporters who covered the debate sought opposing voices and, apart from Teller, found only a handful of scientists willing to explain on record why they hadn't signed. For the most part, these men cited concerns about the proper political role of scientists. (For example, in 1957 J.H. Hildebrand, a chemist at the University of California, told *Science* that he opposed entering "the realm of international diplomacy where a scientist possesses no peculiar knowledge or wisdom."[55]) Even Rubinson cites Herbert York's statement to Congress that "the majority [of scientists] agrees with me rather than Dr. Teller… they are in favor of proceeding with the test ban."[56] Throughout both the Eisenhower and Kennedy administrations, the vast majority of scientists making public statements about the test ban were urging support.

Rubinson's second claim concerns the weakness of the technical rhetoric of Eisenhower's science advisors. Rubinson concludes that "Limited to technical advice and confronted by nationalist fears, government scientists failed to develop a rhetoric effective enough to challenge the supporters of thermonuclear weapons." In contrast, Linus Pauling had "mobilized a significant number of Americans against fallout by arguing that nuclear testing was immoral

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[55] Quoted in *Federation of American Scientists Newsletter*, 17 June 1957.

[56] Rubinson, 313.

because it harmed the people, including children, it was meant to protect." But PSAC members, many of whom were also members of the Federation of American Scientists, had long warned of the dangers of fallout as well. Fallout was a risk relevant only to atmospheric testing, however, and not underground testing, where the technical problems of monitoring were at issue. (Atmospheric tests were easy to detect; underground tests were not.) The partial test ban, which eliminated atmospheric testing but not underground testing, sufficiently addressed the moral argument concerning fallout risks so heavily promoted by Pauling, but also exposed the limits of such arguments for the promotion of a comprehensive ban.

The moral case against underground testing was one rooted in eventual disarmament, a far more radical position than the anti-fallout position. Moreover, to make a purely moral case, that is, to ignore the technical concerns over whether the Soviet Union could effectively cheat the system, meant committing to a form of disarmament that was potentially unilateral. Although some PSAC scientists could offer only qualified support for such a position, Kennedy's science advisor, Jerome Wiesner, and Director of Defense Research and Engineering Herbert York both argued on multiple occasions that moral and political concerns outweighed the minor technical hurdles to a comprehensive ban. In 1963, York told a Senate committee that "this dilemma of steadly increasing military power and steadily decreasing national security has no technical solution." The only answer was peaceful political arms control negotiation.[57]

More realistically, implicating science advisors in Eisenhower's failure to secure a test ban (and Kennedy's ultimate achievement of only a partial ban) likely overstates the role of both scientists and domestic politics in the outcome. Other scholars taking a broader, international view of the Cold War have argued convincingly for the importance of the U-2 incident in ending

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[57] York, 199.

test ban hopes at the conclusion of the Eisenhower administration (since Khrushchev angrily withdrew from negotiations in response), and the changing postures of the Soviet Union—themselves rooted in political developments in Germany, China, Romania, and Cuba, among other places—for the eventual passage of a limited ban in 1963. Vojtech Mastny writes that for all the parties involved, "higher priorities" of Cold War politics in 1958-1962 left test ban negotiations "an exercise in exasperating futility," accompanied by "similarly sluggish nonproliferation talks." Not until most of these other priorities were resolved could any kind of test ban treaty succeed.[58] During the Kennedy administration, US negotiators presented two draft treaties to the Soviet leadership—a comprehensive test ban and a partial test ban—and it was only the latter that proved acceptable to Khrushchev.

The participation of scientists in the treaty process nevertheless deserves further examination, particularly as an indication of the political and ethical strategies of prominent scientists during this critical Cold War moment. Though Greene and Rubinson have offered detailed assessments of the work of science advisors during Eisenhower's second term, a similar study of the Kennedy years has yet to be undertaken. As Rubinson demonstrates, the scientists' movement in support of the test ban included the work of outsiders like Pauling and the Federation of American Scientists, international organizations like Pugwash, and the efforts of insiders such as Kistiakowsky, Wiesner, and York. All of these scientists operated according to ethical impulses that pushed them toward reducing the risk of nuclear war, and all of them, even Pauling, assumed that appropriate tactics included trying to influence the policymakers in Washington through traditional means, whether via the circulation of petitions and public statements, providing analyses of technical problems, or personal advice and persuasion.

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[58] Vojtech Mastny, "The 1963 Nuclear Test Ban Treaty: A Missed Opportunity for Détente?" *Journal of Cold War Studies* 10, No. 1 (Winter 2008), 3-25.

Activism on behalf of the test ban thus addressed the policies of the Eisenhower administration through participatory means, in the case of the PSAC, and through the gentle external prodding of activists like Pauling and Barry Commoner. These tactics continued through the Kennedy administration, where Secretary of Defense Robert McNamara would prove a sympathetic ear, simultaneously promoting arms control while dismantling the New Look, and thereby ingratiating himself with PSAC and the wider disarmament community.

Wiesner and McNamara: From the New Look to Flexible Response

In 1954, Eisenhower had expressed enthusiasm for the work of the Technological Capabilities Panel. But despite the mutual appreciation, the panel's scientists and Eisenhower parted ways on the fundamental strategy of the New Look. Killian and his colleagues had pushed for a renewed emphasis on technologies appropriate for non-nuclear "limited wars" to little avail. Killian himself later acknowledged that "in making these and other recommendations, the panel clearly was dissatisfied with the 'new look' defense policy and the concept of 'massive retaliation.'" But Eisenhower was not persuaded, and only in the waning days of his administration would attention to limited war resurface.[59]

In the meantime, the early development of ICBMs, spurred by the Technological Capabilities Panel, exacerbated the rift between the Air Force and the other services, as each redundantly pursued its own separate prototypes of intermediate- and long-range missiles. The Air Force promoted its "Minuteman" missile, so named because it was designed to be launchable on short notice, and therefore could be airborne before a Soviet surprise attack could disable it. But the Navy countered with the Polaris submarine-launched intermediate-range missile—a

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[59] Killian, 79.

mobile, concealed system nearly impossible for an enemy to locate and destroy. Rather than amassing an ever-expanding battery of quick-launch ground-based missiles, as the Air Force proposed, the Navy promoted a smaller vision of deterrence, one based on an effective but elusive nuclear arsenal. The technology itself bolstered the strategic case against "massive retaliation," and Maxwell Taylor and other Army and Navy leaders continued their sharp criticisms of LeMay and the Strategic Air Command. They attacked SAC for its land-based vulnerability and for the potential dangers from radioactive fallout in their massive bombing plans.

To end the infighting, in 1960 Eisenhower created a Joint Strategic Planning Staff (JSPS), tasked with developing a "Single Integrated Operational Plan" (SIOP) to govern nuclear weapons policy. The JSPS was dominated by representatives of the Air Force's Strategic Air Command, however, and the resulting SIOP reflected SAC priorities, to the irritation of Army and Navy leaders. Rosenberg describes the SIOP as "a *capabilities* plan, aimed at utilizing all available forces to achieve maximum destruction." In the face of predictably intense Navy criticism, Eisenhower dispatched his science advisor, George Kistiakowsky, to evaluate the plan. Kistiakowsky sided squarely with SAC's critics, opposing both the specifics of the plan and the general principles of massive retaliation. The SIOP would "lead to unnecessary and undesirable overkill," he explained. It was not necessary "to kill 4 and 5 times over somebody who is already dead." Unlike the similar criticisms by Killian set forth in 1954, in 1960 Kistiakowsky's cautionary words carried weight with Eisenhower. As Rosenberg explains, "Kistiakowsky, a scientist who represented no parochial service interest, had made the President realize that the SIOP might not be a rational instrument for controlling nuclear planning, but rather an engine generating escalating force requirements."[60] Eisenhower was convinced, but 1960 was too late for a major policy change. The incoming Kennedy administration, elected on a dubious 'missile gap' platform, would inherit the SIOP and its accompanying factional and strategic tensions.

Among the scientists who had worked with von Neumann and Gardner on the ballistic missile program was Jerome Wiesner, a Los Alamos veteran and electrical engineer at MIT's Research Laboratory of Electronics, the successor to the Radiation Lab. In the fall of 1960 and through Kennedy's inauguration, Wiesner provided the candidate with a detailed history of major science and technology-related concerns. This included a frank discussion of nuclear strategy and an analysis of the competing views within the government and military. As Wiesner summarized it, the major debate was between "creating only an adequate deterrent capability versus building a massive first-strike capability." The issue "has divided the Pentagon for fifteen years," he wrote, "with the Army and the Navy on one side and the Air Force on the other; an issue so basic that it essentially controls almost every other military decision as well as many foreign policy questions." The Army and Navy wanted to ensure that the nation could retaliate after a Soviet attack—that is, they wanted to ensure some measure of nuclear survivability—while the Air Force, led by LeMay, preferred to focus on the US ability to cripple the Soviet Union in a single attack.

Wiesner patiently explained the tenets of the Air Force approach to Kennedy, including its self-serving reliance on a large bomber force and "an intelligence network capable of keeping track of all the Soviet bomber force and hopefully providing adequate warning if it were planning to attack the US." His skepticism was obvious. Although he acknowledged that "our

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[60] Rosenberg, 8.

forces were awesome and the posture taken by LeMay and Dulles sufficiently belligerent to make the Soviet leaders very cautious," he did not believe that sufficient intelligence and bombing capabilities existed to execute the Air Force vision of both knowing and destroying all Soviet weapons locations in a single attack. Moreover, the Strategic Air Command's influence in military planning had led to a monopolization of resources that starved other worthy endeavors. This extended throughout the military, as the Air Force's "preoccupation with massive retaliation" had "spread to the other services, who were forced to get on the bandwagon to survive—or so they thought," leading to various redundant or wasteful programs.

This misallocation was especially problematic in light of the necessary transition away from Air Force bombers carrying nuclear weapons, to "a major dependence upon missiles"—first Atlas, and eventually the "less vulnerable Polaris and Minuteman." In recounting these ICBM projects to Kennedy, however, Wiesner emphasized the difficulty in estimating Soviet capabilities; the funding and timing of American missile programs and defense measures were often tied to faulty or incomplete predictions of Soviet nuclear might, to the detriment of US security. Polaris and Minuteman were improvements over the SAC bomber system, but not without their own dangers and deficiencies.

Wiesner also cautioned Kennedy against the use of "tactical nuclear weapons." When the US had a nuclear monopoly, he wrote, tactical nuclear weapons had been a means to compensate for the Soviet Union's "numerical advantages" in conventional forces and to "deter any Soviet ground movements." But this deterrence grew suspect as Soviet nuclear stockpiles increased and "more and more people, especially in Europe, began to doubt that we would carry out our retaliatory blow against the Soviet cities if the Soviet armies moved into Western Europe." Deterrence based on tactical nuclear weapons yielded a particularly high-risk kind of stability.

In addition to these technical and logistical reasons for caution, Wiesner urged Kennedy to reject Air Force nuclear doctrine on arms control grounds. The emphasis on massive attacks antagonized the Soviet Union and sped the arms race. “In spite of what we said,” wrote Wiesner, “it must have been impossible for most of the Soviet military men to accept our claims that SAC was primarily retaliatory in intent. At a minimum, the Soviet military leaders were provided with a convincing argument for building up their forces.” A smaller deterrent force of missiles and bases could help reduce “political controversy and embarrassment.” Wiesner’s stance was clear. “For the next few years it is probably wise to maintain some air defense,” he wrote, but “the new administration should examine critically the wisdom of maintaining the present level of air defense effort indefinitely.”[61]

Kennedy soon tapped Wiesner as his science advisor, and thus elevated a strong voice for arms control to his inner circle. The president eventually chose another nuclear non-hawk, Robert McNamara, as his Secretary of Defense. Though McNamara had worked with Curtis LeMay in analyzing Air Force efficiency during World War II, he had not followed in his footsteps in support of developing massive first strike capabilities. (McNamara later reported that he had advised both Kennedy and Johnson never to use nuclear weapons.[62]) Almost immediate upon appointing him, Kennedy tasked McNamara with a thorough review of Eisenhower’s proposed military programs and defense budgets for the current and subsequent fiscal years. The result was an early articulation of McNamara’s approach to national security: a dramatic turn

[61] Memo, Jerome Wiesner to John F. Kennedy, 23 February 1961 (reprint of memo from September 1960), Folder 16, President’s Office Files, Box 67, JFKL.

[62] McNamara statement in *Foreign Affairs* cited in “Science and the Citizen,” *Scientific American* 249, No. 6 (December 1983): 76.

away from the New Look and its priorities in favor of deterrence and the flexibility to respond to small-scale foreign conflicts through non-nuclear means.

To begin, McNamara laid out his central security objectives. The United States had obligations to protect its allies and the "free world" in general, but how did these translate into strategy and policy? First, McNamara explicitly rejected the SAC doctrine of massive first strike capability: it was not necessary to maintain a "pre-attack" massive stockpile of nuclear weapons as SAC claimed; rather, the nation needed only "survivable retaliatory power," which would be sufficient to ensure deterrence. To this end, McNamara recommended improved protection of military forces and weapons through expanded air defense, civil defense, accelerated work on the Polaris program and the hardened bases necessary for Minuteman missiles, and improvements in the "highly vulnerable" systems of command and control. (McNamara's promise to develop "systems with greater endurance and flexibility under conditions of thermonuclear attack" would fit well with the creation of the decentralized ARPANET, the precursor to the modern internet.)

But McNamara went further than just endorsing this alternate view of deterrence. He criticized the excessive reliance on nuclear weapons more generally, complaining that focusing on missiles at the expense of other military options limited US decision-making. "We have been forced into a single strategy for retaliation," McNamara explained. "At the present we have little ability to make decisions in the event of an attack…" All the general war planning pointed toward a nuclear response, which was limited and dangerous in its own right, but the Pentagon suffered as well from an "over-emphasis on general war" in the first place. What about smaller, localized conflicts, what McNamara referred to as "limited war"? Eisenhower may have hoped that the New Look would deter US involvement in limited wars by dint of its nuclear emphasis, but McNamara saw only the danger—and likelihood—of nuclear overkill. "Our forces designed to fight overseas, those we would call on to fight in limited conflict, are, in fact, strongly oriented in their war plans, current capabilities, material procurement, and research and development, towards general nuclear war," he warned. "This is at the expense of their ability to wage limited and especially non-nuclear war." With an eye toward potential interventions in Southeast Asia, Latin America, the Middle East, and Africa, McNamara called for an expanded "ability to deal with guerilla forces, insurrections, subversion… sub-limited war capabilities."

McNamara's budget statement was a blueprint for the two key components of his strategic legacy: the emphasis on choice and options, which came to be known as "flexible response," and the commitment to developing the capability for military intervention with varying and increasing degrees of non-nuclear force, or "graduated escalation." Both would soon guide American involvement in Vietnam. But for arms control scientists in 1961, McNamara's declared interest in a host of postcolonial disputes was less important than the underlying nuclear message. McNamara wanted to "raise the threshold of our local non-nuclear defense capability, and reduce our dependence on nuclear war, a type of warfare which it will increasingly be in our interest to avoid." He not only adamantly rejected the New Look, but offered financial resources to back his words. His budget review noted pointedly that "We are doing too little research and development on non-nuclear weapons," and called for a "substantial increase" for non-nuclear armaments.[63] Arms control scientists suddenly had an ally at the very top of the Pentagon.

Kennedy, Scientists, and the Partial Test Ban

As with much high-level political decision-making, in reviewing the Kennedy test ban negotiation process it can be difficult to determine which exchanges were examples of honest

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[63] Budget review, prepared by Robert McNamara, 21 February 1961, "Department of Defense Review of FY61 and FY62 Military Programs and Budgets 2/21/61" Folder, NSF, Box 273, JFKL.

disagreements, and which were conducted in the interests of political or propagandistic advantage. Speechwriters carefully crafted the public rhetoric of the Cold War, which, on both sides, tended towards bombast, hostility, and sweeping idealism. By the end of the Eisenhower administration, both the United States and the Soviet Union had publicly professed support for a testing ban and for radical disarmament measures. As a 1960 State Department press release explained, the "ultimate goal" of negotiations was "general and complete disarmament under effective international control," including the "cessation of production" of nuclear weapons and "their complete elimination from national arsenals," except for any deemed appropriate for "an international peace force."[64] Such language did not reflect the actual priorities of US policymakers; it clearly served other Cold War purposes.

Inside the Kennedy administration, ideological and strategic differences shaped policy debates, as did partisan concerns and interdepartmental rivalries. The following account draws only slightly on the speeches of Kennedy and Khrushchev; rather, much of the evidence is culled from internal memos, reports, and the minutes of what appear to be fairly candid meetings of the Committee of Principals assembled to work on problems of testing and arms control. Some of the technical discussions relevant to the test ban remain classified, particularly on topics such as tactical nuclear weapons, the neutron bomb, and aspects of Soviet surveillance. Nevertheless, enough documentation exists to give at least a partial glimpse into the Kennedy administration's approach to arms control negotiations, the contributions of scientists, and how both moral and technical concerns shaped the character of the debate. The following account describes the evolution of test ban negotiations, the decision to resume atmospheric testing in 1962, and the circumstances of the eventual ratification of the Partial Test Ban Treaty in 1963.

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[64] State Department press release, 27 June 1960, "Disarmament – Nuclear Test Ban Negotiations 6/2/60 – 12/60 Part I" Folder, POF, Box 100, JFKL.

Newly installed in office, President Kennedy quickly established arms control as a key goal of his administration. He oversaw the elevation of the US Disarmament Administration from its position within the Department of State to full-fledged agency status in the form of the US Arms Control and Disarmament Agency (ACDA), headed by director William C. Foster. (To ensure that the necessary ACDA legislation was passed, Kennedy was aided by the Federation of American Scientists, who held a "series of briefing breakfasts" for key members of Congress and their staffers on the subject of disarmament.[65]) Kennedy also continued both the pursuit of a test ban and other disarmament measures, the Eisenhower-initiated moratorium, as well as the practice of convening the Committee of Principals charged with addressing these concerns. Perhaps most importantly, his selection of Jerome Wiesner as his Special Assistant for Science and Technology ensured that the topic would receive substantial attention. The Federation of American Scientists hailed the choice of Wiesner, a Pugwash attendee and committed arms control advocate.[66]

Less than two months after the inauguration, the Principals were hard at work reviewing definitions of terms and inspections requirements for various proposals. Secretary of State Dean Rusk had expressed qualified support for arms control, including a test ban, and early meetings featured numerous debates on the technical aspects of an enforceable ban. At issue was the problem of underground testing. While atmospheric tests were easier to detect and therefore monitor, underground tests were trickier, and in some circumstances difficult to distinguish from earthquakes or other seismic activity. During its meetings in early March 1961, the Committee of

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[65] *FAS Newsletter*, November 1961.

[66] *FAS Newsletter*, February 1961.

Principals reviewed the state of detection technology and the requirements for an underground ban. They discussed the number of likely annual earthquakes in Soviet territories, and how many on-site inspections would be necessary to ensure that no clandestine testing was taking place. On-site inspections were a particular sticking point with Soviet negotiators.

Even in these technical discussions, political objectives were the subject of debate, not scientific evidence. Whereas Glenn Seaborg of the AEC wanted the number of inspections linked to the number of earthquakes with no upper limit, a standard unlikely to meet Soviet approval, Wiesner offered a far more flexible view. He observed that 'one-for-one' inspections were not necessary, since any inspections would be a powerful deterrent against cheating, and, realistically, 'one clandestine nuclear test would not be significant.'[67] Neither Wiesner nor Seaborg disputed the seismological data or the state of US detection techniques, but their interpretations and recommendations plainly revealed their differing political priorities.

Two influential reports from this period further reveal how technical information was marshaled in arguments both for and against a test ban. An "Ad Hoc Panel on the Technological Capabilities and Implications of the Geneva System," led by Bell Labs physicist James B. Fisk, had been convened in the winter of 1961 "to compile the technical material which has bearing on the broader questions of policy formation in connection with the Geneva Conference on Cessation of Nuclear Tests." The panel was almost entirely composed of scientists, with a handful of military representatives, and included Cornell physicist Hans Bethe, Harold Brown, Richard Latter, J. Carson Mark, Wolfgang Panofsky, Frank Press, Herbert Scoville, Herbert York, and Gen. Alfred Starbird, Gen. Austin Betts, and the retired Army general Herbert Loper. Although much of the panel's March 1961 report remains classified, a redacted and "sanitized"

[67] Memorandum of Conversation, Meeting of Principals, 2 March 1961, "ACDA Disarmament Committee of Principals Memos of Conversation 3/61-11/63" Folder, NSF, Box 267, JFKL.

version released in 2008 reveals lengthy explanations of US detection capabilities and limitations, and analyses of several predicted outcomes for test ban scenarios.

The Fisk panel wrote that the problem of the test ban was not one "where positions should be controlled by the technical issues." Political and military considerations remained paramount. Nevertheless, the most crucial technical aspect of the test ban was "verifying violations." On this topic, the report noted that although the US possessed a long-range detection system with "acoustic, seismic, electromagnetic, and radioactivity components… deployed around the USSR and its satellites," it could not definitely distinguish underground nuclear explosions from natural seismic activity. "There is no known way to identify an underground nuclear explosion by its seismic signals alone," the panelists observed. To resolve this problem, the panel urged restored and expanded funding for the VELA program, as per ARPA's request.

The report also addressed other technical concerns: the reliability of the US stockpile (for which the panelists did not find testing to be urgent), the ability of non-nuclear countries to develop nuclear weapons (the panels doubted a ban would prevent the development of "a simple, heavy" fission bomb), the construction of an anti-ballistic missile system (the panel listed some advantages of testing, but deemed the prospects for ABM development "not encouraging" in any case), and US advances in new classes of weaponry, including lightweight warheads and the "enhanced radiation" neutron bomb. On this last point, the panel observed that progress "depends in various degrees on testing." But the larger question was one of security—what would be the overall consequences for US security if there were no test ban, a comprehensive and enforced test ban, or a test ban under which the Soviets engaged in clandestine testing? Answering these questions required an evaluation of the country's strategic posture. The panel determined that for counterforce, rooted in the ability to launch massive first and second strikes, the particular refinement of warheads via testing mattered little—quantity, not quality, was what mattered. For deterrence, with its emphasis on survivability and mobility, the development of lighter warheads might be more important. But in either case, the panelists observed, existing resources were vast. The warheads currently available in both systems could "completely over-kill the population and over-destroy the floor space of urban area targets by blast and fire." The panel thus predicted that with no test ban, additional testing would improve first-strike survivability for both sides. With an enforced test ban, both sides could potentially "maintain a very strong deterrent strategy" simply on the basis of weapons that could be stockpiled without further testing. Even with an unenforced test ban with maximal Soviet cheating, US deterrence capabilities might be "degraded," though hardly nonexistent.[68]

The report was not an uncritical push for a comprehensive test ban, but its structure—opening with an acknowledgement of the limitations of detection and enforcement but closing with a relatively mild assessment of the worst-case scenario of Soviet cheating—provided valuable fodder for the administration's test ban advocates. John McCloy, Kennedy's chief disarmament advisor, summarized the panel's work in a memo to the president, writing that "This report, in my judgment, from a technical standpoint buttresses the conclusion that it is in the overall interest of the national security of the United States to make a renewed and vigorous attempt to negotiate a test ban agreement…" McCloy referred to "the consensus of scientific thinking and analysis contained in this report," thus preempting any claims that technical aspects were in dispute. The risks of clandestine Soviet testing could be ameliorated with further VELA

[68] Report of the Ad Hoc Panel on the Technological Capabilities and Implications of the Geneva System, 2 March 1961, "Disarmament – Fisk Panel on Technical Capabilities of the Geneva System 3/61" Folder, POF, Box 100, JFKL.

research, he noted, but in any case, they were risks that were, "on balance, worth taking." He closed with a final appeal for a test ban that went beyond fears of fallout or hopes to freeze a US stockpile advantage. A test ban "would be a significant step in the field of arms control," he wrote. Like the FAS scientists, McCloy viewed a test ban as a steppingstone to disarmament.

Naturally, the report had its detractors as well. One member of the Fisk panel, the retired Army General Herbert Loper, attached a dissenting view to the final report. Many of his objections remain classified, but he closed with an argument reminiscent of the hydrogen bomb debates. "Any action on the part of the United States which denies its scientific and engineering community the opportunity to apply its maximum capabilities to its nation's defenses cannot result in a military advantage to the country," he wrote pointedly.[69] More influentially, the Fisk panel's implications were explicitly refuted in a similarly-themed RAND report issued a week later, one of whose authors, Richard Latter, had been a member of the Fisk panel. From its first sentence, the RAND report set out to challenge the Fisk view:

> There is near-universal agreement among scientists that certain nuclear tests cannot be detected. Despite this, there is strong pressure for a test ban based on the supposition that nuclear weapons technology has reached that point of diminishing returns where no new discoveries can upset the balance of military power. In a word, that it does not matter whether or not we continue to test or whether or not the Soviets cheat on a test ban agreement.
>
> This RAND special report identifies five new aspects of the nuclear problem that say, in sum: it does matter that we test. The report has been reviewed by Dr. Edward Teller and John S. Foster of Livermore Radiation Laboratory who verify its technical authenticity.

The authors went to argue that should a test ban be imposed and Soviet testing proceed anyway, the US would suffer numerous disadvantages. Soviet researchers would likely discover unforeseen vulnerabilities in US forces and in the US stockpile and the US would miss out on

[69] Loper to McCloy, 2 March 1961, "Disarmament – Fisk Panel on Technical Capabilities of the Geneva System 3/61" Folder, POF, Box 100, JFKL.

key anti-ballistic missile defense advances. If anything, a test ban might leader to a greater arms buildup, since the US would be forced to diversify and expand its deterrent forces.

Like the Fisk report, the RAND work also emphasized the lack of technical disagreement among scientists, thus characterizing the nature of the debate as strategic and political. The authors offered a single recommendation: “adhere to a principle of adequately controlled tests.” Since adequate control did not yet exist for undergrounding testing, any test ban should be partial: “atmospheric tests, some space tests, and underground tests above a threshold.” Thus, even with its implicit criticism of the Fisk report, the RAND scientists also endorsed a test ban, only modified with an underground threshold high enough to ensure that cheating could be effectively monitored.[70]

The difference between the technical aspects and the political implications of the comprehensive test ban merit further clarification. Whereas the Fisk committee members and the RAND experts agreed that remote monitoring of low-yield underground nuclear tests was, with current detection techniques, difficult if not impossible, they differed on the political, moral, and strategic implications of this impossibility. How threatening was clandestine Soviet testing? Did the risks outweigh the potential benefits for arms control and peace? It is worth noting that while these questions were debated at the highest levels of government, many liberal scientists saw the technical aspects of detection as an opportunity to contribute resources and expertise in the service of arms control. If detection techniques could be improved, then the political scales might tip toward support for a comprehensive test ban. As it turned out, even when detection technology improved, the moral and political arguments on both sides remained the same.

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[70] A.L. Latter, R. Latter, E.A. Martinelli, W.G. McMillan, “Some New Considerations Concerning the Nuclear Test Ban,” 10 March 1961, “Disarmament – Nuclear Test Ban Negotiations 3/1/61 – 4/7/61” Folder, POF, Box 100, JFKL.

The greatest opposition to a test ban came not from scientists of PSAC or RAND, but from non-scientists at the Pentagon and in the arms services who were anxious to resume testing. In the spring of 1961, National Security Council staffer Robert Komer complained that the Soviets had "impaled" the US on a "the hook of a self-imposed test ban." He worried that while the US was observing the testing moratorium, the Soviets were likely preparing to test in secret.[71] Meanwhile, the Joint Chiefs were preparing their own attack on arms control, noting in a May report that "no practicable arms control agreement and inspection systems can be envisaged that would eliminate the danger of surprise attack altogether." In other words, arms control would not make the country more secure.[72]

Through the spring of 1961, Kennedy and his top advisors, including Wiesner, met to sort through the conflicting recommendations coming from various quarters, and to determine the US negotiating strategy. At a meeting in early May, McCloy reported that he thought Soviet leaders wanted a test ban, but not the on-site inspections required to ensure enforcement of the underground component. Herbert Scoville of the CIA, generally a strong advocate of arms control measures, objected to the prospect of Soviet inspectors arriving in Nevada.

In the meantime, Komer, McNamara, the Joint Chiefs, and Harold Brown, the new Director of Defense Research and Engineering at the Pentagon, were all preparing arguments in support of a resumption of testing, on the grounds that new advances could be made with applications to limited nuclear war and an anti-ballistic missile system. Kennedy considered

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[71] Robert Komer, "The Case for Resumption of Nuclear Tests," "Nuclear Weapons Testing 2/61 – 4/61" Folder, NSF, Box 299, JFKL.

[72] Joint Chiefs of Staff, "Arms Control Measures Affecting Risk of Surprise Attack," "DoD "Arms Control Measures Affecting Risk of Surprise Attack" 5/61" Folder, NSF, Box 273, JFKL.

resumption to be a tricky matter, however, largely because of the risk of a public opinion backlash.[73]

At least in part, the resurgent call for new tests resulted from the failures of negotiation. At the meeting of the Committee of Principals on May 23, 1961, Ambassador Arthur Dean reported that prospects for a test ban treaty seemed 'pretty dim.' Without Khrushchev's acceptance of on-site inspections, even Wiesner worried about the consequences of an unenforced ban.[74] Throughout the spring and summer of 1961, then, discussions drifted away from hopes for a test ban to debates over if, when, and how the US should resume nuclear testing.[75] John Kenneth Galbraith wrote to Kennedy, urging against a resumption of testing.[76] Wiesner hoped that at the very least, new tests would not produce fallout. Rusk and McNamara seemed to agree.[77] Marc Raskin, an advisor to McGeorge Bundy, wrote an impassioned appeal to his boss, arguing that testing would hurt the US in the eyes of the world, would "quicken the pace of the arms race, not slow it down," and, should it lead to expanded production of tactical nuclear weapons, raise the risk that limited US interventions abroad would lead to nuclear war.[78]

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[73] "Record of Meeting on Nuclear Test Ban Issue," 4 May 196, "Disarmament – Nuclear Test Ban Negotiations 4/28/61 – 3/62" Folder, POF, Box 100, JFKL; Memo, McNamara to National Security Council, 15 May 1961, "Nuclear Weapons Testing 5/61" Folder, NSF, Box 299, JFKL.

[74] Memorandum of Conversation, Committee of Principals, 23 May 1961, "ACDA Disarmament Committee of Principals Memos of Conversation 3/61-11/63" Folder, NSF, Box 267, JFKL.

[75] Some discussion of disarmament planning continued, however. As Komer wrote to Bundy and Rostow in June, "neither they nor we are ready for serious talks" on disarmament, but nevertheless the US should undertake "the real homework necessary to come up with a sensible disarmament program against the time when genuine negotiations may be possible." Komer to Bundy/Rostow, 28 June 1961, "ACDA Disarmament Comm of Principals 4/1/61-7/15/61" Folder, NSF, Box 267, JFKL.

[76] Galbraith to JFK, 12 June 1961, "Nuclear Weapons Testing, 6/61" Folder, NSF, Box 299, JFKL.

[77] Memorandum of Conversation, Committee of Principals, 23 May 1961, "ACDA Disarmament Committee of Principals Memos of Conversation 3/61-11/63" Folder, NSF, Box 267, JFKL.

[78] Marc Raskin to McGeorge Bundy, 16 June 1961, "Nuclear Weapons Testing, 6/61" Folder, NSF, Box 299, JFKL.

Even the nation's top nuclear scientists were not anxious to resume testing. Norris Bradbury, head of Los Alamos, confirmed to Wiesner that renewed testing was not imperative; resumption would not lead to "radical change in national strength" or "new dimensions in warfare." Plenty of bomb-related work could be done in a laboratory setting, and crucial delivery systems could be tested regardless of a nuclear moratorium. But Bradbury's relaxed attitude was not simply a matter of minimal scientific need. He, like Wiesner, was committed to arms control and the important step that a test ban represented. He wrote, poignantly:

> The current test ban negotiations, although disappointing, represent the first real attempt to alter the course of history with respect to the nuclear arms race. It will be a grave disappointment to have to admit failure of even this poor attempt… So serious does the eventual world nuclear weapon situation seem to be that, without clear evidence that the Russians were testing, I would personally prefer that the United States not be the first to resume this activity.[79]

Bradbury attached a statement from his Los Alamos colleague, Carson Mark, who viewed the weapons situation with a similar lack of urgency. In his view, the country already possessed a vastly destructive stockpile of effective weapons. "No advances by testing can alter the fact that with systems available both to us and to potential opponents each can inflict physically and psychically insupportable damage on the other," he wrote. "Improved designs may make it easier and cheaper (in some sense) but not more fearful; while less advanced designs may make it more costly and cumbersome but not less certain."[80]

In late July, PSAC's Ad Hoc Panel on Nuclear Testing, led by Stanford physicist Wolfgang Panofsky, issued an influential report on the resumption of testing that largely echoed the conclusions of the earlier Fisk report. Members of the panel included Cornell's Hans Bethe,

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[79] Norris Bradbury to Wiesner, 17 July 1961, "Nuclear Weapons Panofsky Panel 8/4/61-9/5/61" Folder, NSF, Box 302A, JFKL.

[80] Attachment by Carson Mark, 17 July 1961, "Nuclear Weapons Panofsky Panel 8/4/61-9/5/61" Folder, NSF, Box 302A, JFKL.

Los Alamos's Norris Bradbury, Livermore's John Foster, Harvard's George Kistiakowsky, Caltech's Frank Press, and Fisk himself. As in the Fisk report, the new document opened by emphasizing that technical concerns were not the most important factors at hand: "the final decision on whether or not to resume testing also involves very important non-technical or military issues." Nevertheless, their task was technical, and so they set about evaluating what would be gained and lost in the resumption decision. Resumed testing would likely speed the development of lighter warheads, "enhanced neutron radiation weapons," ICBMs , and an anti-ballistic missile system. They noted that if only atmospheric testing were banned, but undergrounds tests allowed to proceed, this research would not be "seriously impaired," only "more difficult and costly." From a larger strategic standpoint, however, a complete, observed test ban would probably freeze the US advantage in place, while unlimited testing by both sides would likely allow the US and the USSR to "approach the same level of warhead technology." Like the Fisk report, the Panofsky report examined possible outcomes of cheating and observance, and noted that in all cases, whether counterforce or deterrence was the goal, the US already possessed "over-kill" capabilities.[81]

At the top levels of government, the report was received in predictable ways. Marc Raskin, already a strong arms control advocate, interpreted it as a clear statement that "there is nothing critical in the short run which would impair our military posture by not testing."[82] George Ball agreed, writing to Kennedy that the US ought to hold off on resumption in the hopes of successfully negotiating a test ban treaty. He summarized the Panofsky report for the

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[81] "Report of the Ad Hoc Panel on Nuclear Testing," 21 July 1961, "Nuclear Weapons Testing Subjects Rostow File 7/21/61-9/20/61" Folder, NSF, Box 301, JFKL.

[82] Raskin to McGeorge Bundy, 25 July 1961, "Nuclear Weapons Testing, 7/16/61-8/9/61" Folder, NSF, Box 299, JFKL.

president, noting that it confirmed that the testing decision "can be governed primarily by non-technical considerations."[83] AEC head Glenn Seaborg sent his concurrence to Weisner. He was "in general agreement" with the Panofsky report, though he added his own request for expedited preparations in the event of testing resumption and expanded yield limits for laboratory testing in the meantime.[84]

On the other side, high-level Pentagon and military officials registered their dissatisfaction. Livermore's John S. Foster, a member of the panel, wrote separately to Wiesner to explain that while the Panofsky group had disagreed about "the question of urgency," they still had concluded that the US should eventually resume testing.[85] More pointedly, Harold Brown, Director of Defense Research and Engineering, complained to Wiesner that the risks of Soviet cheating were far greater than expressed in the Panofsky report, as were the potential benefits to an anti-ballistic missile system. Though he accepted that a delay of a few months would matter little, he nevertheless urged that at the very least, "weapons testing underground should be resumed as soon as it is politically expedient."[86] Maxwell Taylor agreed, arguing that testing was critical for the development of lightweight warheads and tactical nuclear weapons. He wrote to Kennedy, "Unless the most compelling of political arguments can be adduced against it," testing should resume.[87] McNamara, suspecting that the Soviets might already be testing clandestinely,

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[83] George Ball to JFK, 4 August 1961, "Nuclear Weapons Panofsky Panel 8/4/61 – 9/5/61" Folder, NSF, Box 302A, JFKL.

[84] Seaborg to Wiesner, 4 August 1961, "Nuclear Weapons Testing Subjects Rostow File 7/21/61-8/20/61" Folder, NSF, Box 301, JFKL.

[85] Foster to Wiesner, 2 August 1961, "Nuclear Weapons Panofsky Panel 8/4/61 – 9/5/61" Folder, NSF, Box 302A, JFKL.

[86] Harold Brown to Wiesner, 3 August 1961, "Nuclear Weapons Testing Subjects Rostow File 7/21/61-9/20/61" Folder, NSF, Box 301, JFKL.

[87] Maxwell Taylor to JFK, 7 August 1961, "Nuclear Weapons Panofsky Panel 8/4/61 – 9/5/6" Folder, NSF, Box 302A, JFKL.

reiterated his support for a test ban but recommended that testing preparations commence nonetheless. [88]

The Joint Chiefs offered the harshest assessment of the Panofsky report. Rather than interpreting or tweaking its findings to support resumption, they directly challenged the findings themselves. They complained of "inaccurate" points, "unconfirmed intelligence estimates" (particularly concerning the state of the Soviet stockpile), and "opinions and military judgments with which the Joint Chiefs of Staff do not agree." Its conclusions were "conjectural and subject to gross error." The Chiefs pointedly disagreed with the implication that "there is little urgency connected with US resumption of testing," proposing instead that new tests were both urgent and necessary. Moreover, they staunchly opposed a continued ban on atmospheric testing, citing a report from the Chief of the Pentagon's Defense Atomic Support Agency arguing that "world-wide fallout from past tests has not produced a biologic hazard."[89]

The criticism of the Joint Chiefs disturbed Kennedy. Were technical and intelligence estimates actually in dispute? The president wrote to Taylor in confusion: "The Joint Chiefs took a very strong position against the Panofsky report on testing. I wonder who prepared their analysis. Was it done by one, or two, or three men? Was it done outside of the Defense Department by a group of scientists, or what?" He seemed surprised that the JCS could take the position they had when "the Chairman of the AEC seems to find himself 'in general agreement in the findings and conclusions of the report.'"[90] The following day, Kennedy raised the issue at meeting of the National Security Council, during which Panofsky personally presented his

[88] McNamara to McCloy, 28 July 1961, "Nuclear Weapons Testing 7/16/61-8/9/61" Folder, NSF, Box 299, JFKL.

[89] "Comments of JCS on Report of the Ad Hoc Panel on Nuclear Testing," 2 August 1961, "Nuclear Weapons Testing Subjects Rostow File 7/21/61-9/20/61" Folder, NSF, Box 301, JFKL.

[90] JFK to Maxwell Taylor, 7 August 1961, "Nuclear Weapons Panofsky Panel 8/4/61 – 9/5/6" Folder, NSF, Box 302A, JFKL.

panel's report. In addition to McNamara, Allan Dulles of the CIA, and other key Council members, Wiesner was in attendance, as were the heads of the two nuclear labs, John Foster of Livermore and Norris Bradbury of Los Alamos.

The ensuing discussion, at turns bitter and fractious, revealed deep differences in both understanding and interpretation of the issues at hand. Panofsky pled his case that a test ban would limit US weapons development, but only in the long run. 'In the short run, the matter is not critical,' he told attendees. Gen. Lemnitzer, Chairman of the Joint Chiefs, argued with CIA head Allan Dulles over the quality of US intelligence regarding the Soviet stockpile, and Kennedy eventually ended the bickering by requesting that Taylor, Lemnitzer, Dulles, and Panofsky meet separately to 'define the disagreements and narrow them if sensible.' In the meantime, the president seemed swayed by the need for eventual resumption of testing, considering the matter mainly in terms of timing. He worried about the negative political effects of resumption given the upcoming meeting of the General Assembly of the United Nations, and promised to reach a decision soon.[91]

The decision would come less than two weeks later, in favor of resumption.[92] But before any official US statements could be made, the Soviet Union rendered the political anxiety moot by announcing their own resumption of nuclear tests. (Though a detailed assessment of Soviet reasons for resumption—and the role of Soviet scientists in the decision—is beyond the scope of this dissertation, it is noteworthy that in September 1961, Paul Doty, an American physicist

[91] Minutes of the National Security Council meeting, 8 August 1961 (minutes prepared by McGeorge Bundy, 5 September 1961), "Nuclear Weapons Panofsky Panel 8/4/61-9/5/61" Folder, NSF, Box 302A, JFKL.

[92] Arthur Dean reported that the decision to resume testing was made in JFK's office on 17 August 1961. Arthur Dean to John McCloy, 18 August 1961, "Nuclear Weapons Testing 8/10/61 – 8/30/61" Folder, NSF, Box 299, JFKL.

attending a joint US-Soviet meeting in England, reported to Walt Rostow that Soviet scientists were "depressed and defensive" about the upcoming Soviet test series.[93])

Avoiding any mention of the parallel decision-making at home, White House speechwriters wasted no time in demonizing the Soviet move in the harshest language possible. "The Soviet government's decision to resume nuclear weapons testing is in utter disregard of the desire of mankind for a decrease in the arms race," one press release read.[94] Robert Komer, grateful that the US was still several weeks away from test readiness, now urged Bundy to play up the delay, making the most of "looking peaceful while they look warlike."[95] Edward R. Murrow, then serving at the U.S. Information Agency, seized on the political opportunity. Like Komer, he recommended holding off on testing and using the time to advance US propaganda: "This can be done not only by the exposure of Soviet duplicity, but also by playing heavily upon the fears of hazards to health and future generations." Moreover, he observed, "This time, if properly employed, can be used to isolate the Communist Bloc, frighten the satellites and the uncommitted, pretty well destroy the Ban the Bomb movement in Britain, and might even induce sanity into the SANE nuclear policy group in the country."[96] Murrow's advice revealed with painful clarity that while the words of Wiesner, Bradbury, and other moderate arms control advocates might be respected by the administration, the activism of men like Barry Commoner and Linus Pauling was not.

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[93] Walt Rostow to Gen. Clifton, 9 September 1961, "Nuclear Weapons Testing Subjects Rostow File 7/21/61 – 9/26/61" Folder, NSF, Box 301, JFKL.

[94] White House press release, 30 August 1961, "Nuclear Weapons Testing 8/10/61-8/30/61" Folder, NSF, Box 299, JFKL.

[95] Komer to McGeorge Bundy, 31 August 1961, "Nuclear Weapons Testing Subjects Rostow File 7/21/61 – 9/26/61" Folder, NSF, Box 301, JFKL.

[96] Edward R. Murrow to JFK, 31 August 1961, "Nuclear Weapons Testing 8/10/61-8/30/61" Folder, NSF, Box 299, JFKL.

As US testing preparations began in earnest in the autumn of 1961, the next set of high-level debates addressed the nature and extent of the tests. Wiesner, dogged in his arms control efforts, seemed to concede the inevitability of testing, but at least hoped to prevent atmospheric tests. Late in September, he informed Bundy that there were no “critical requirements for nuclear tests in the atmosphere” and the minimal advantages ought to be weighed against “the political problems from fallout.”[97] Wiesner was not present at the subsequent meeting of the Committee of Principals, but his viewpoint was expressed ably by Bundy and Rusk. Although both McNamara and Seaborg professed strong support for atmospheric testing, Bundy and Rusk warned of the widespread, international and domestic opposition to testing in general, and to fallout in particular.

Rusk also inserted the prospects of a test ban back into the discussion, reiterating that despite the Soviet actions, a testing agreement was still in the national interest, and ‘If the USSR says it is now ready to sign a test ban treaty, we would presumably agree.’ Rusk went so far as to suggest that the US ought to offer the test ban treaty again to the Soviet Union, and if they refused, only then commence testing.[98] Over the next few weeks, consensus settled around Rusk’s proposal; appearing on “Meet the Press” at the end of the month, Seaborg stated publicly that should the Soviet Union accept the terms of the US comprehensive test ban proposal, the US

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[97] Wiesner to McGeorge Bundy, 29 September 1961, “Nuclear Weapons Testing 9/27/61-10/10/61” Folder, NSF, Box 299, JFKL.

[98] Memorandum of Conversation, Meeting of Principals, 10 October 1961, “ACDA Disarmament Committee of Principals Memos of Conversation 3/61-11/63” Folder, NSF, Box 267, JFKL.

would forego atmospheric and underground testing.[99] But with continued Soviet opposition to on-site inspections, no agreement would be reached.

Throughout the winter and early spring of 1962, Wiesner swam against the tide of sentiment favoring a resumption of atmospheric testing. As details of the Soviets' massive series of tests emerged, some members of the Committee of Principals grew skittish about even pursuing a test ban at all. Whereas Rusk held fast that a treaty was still in the national interest, William C. Foster worried that Soviet achievements had shifted the advantage away from the US. Wiesner, however, was staunch in his position. In November 1961, he told the committee 'that the key issue was not whether the United States was equivalent to the Soviet Union in every aspect of nuclear weapon technology but whether the United States is missing any of the things it should have for its security.' Soviet gains and possible imbalances were not the only factors relevant to testing decisions, he pleaded.[100]

Wiesner's prestige and influence was challenged by the reemergence of Edward Teller, who had played little role in the Kennedy-era test ban discussions to that point, other than his review of the earlier RAND report on the subject. Now, at the president's request, he provided detailed advice on a proposed course of atmospheric tests. Teller paid lip service to the cause of arms control, noting that he "had hoped" that the new tests would not be necessary, but "in this hope I have been wrong." Without atmospheric resumption, he warned, "a dangerous situation will arise in the mid-1960's," when the Soviet Union succeeded in applying their newfound expertise into "a hard-hitting first strike force." Many of Teller's specific assessments remain

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[99] Transcript, "Meet the Press," 20 October 1961, "Nuclear Weapons Testing 10/30/61-10/31/61" Folder, NSF, Box 299, JFKL.

[100] Memorandum of Conversation, Meeting of Committee of Principals, 11 November 1961, "ACDA Disarmament Committee of Principals Memos of Conversation 3/61-11/63, sanitized June 2006" Folder, NSF, Box 267, JFKL.

classified, but his support for testing was clearly linked to his desire for improvements in lighter, cheaper warheads and an anti-ballistic missile system. If anything, he endorsed a more expansive series of tests, with additional testing in "deep space."

Teller also delivered a decidedly non-technical message, an appeal for presidential intervention regarding popular attitudes toward nuclear testing. "The men working on the development of nuclear explosives," Teller wrote, have been "subjected to considerable strain" because "public opinion continued to frown upon [their] activities." This had created a manpower problem, in which it had become "increasingly hard to induce excellent young people" to pursue weapons research. To ameliorate the situation, Teller invited Kennedy to visit the two weapons laboratories and perhaps "make a public statement directed to the scientists of our Country," explaining that "the development of nuclear explosives can be used to provide us with the strength that insures peace."[101]

Teller's rhetoric—invoking the dire specter of Soviet superiority and describing nuclear power as "the strength that insures peace"—could not have been further from that of Wiesner. In December 1961, Wiesner once again urged Kennedy to reject atmospheric testing. There was no technical or military need, he wrote, and even if some military advantages were to be gained, the tests were still "not critical or even very important to our overall posture." Without testing, the nation could still "maintain an extremely effective deterrent." (It was deterrence, not counterforce, that concerned Wiesner.) His final plea reiterated that the question of atmospheric testing was a matter of politics, not technology. Wiesner wrote encouragingly to the president,

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[101] Teller to JFK, 7 December 1961, Box 299A, "Nuclear Weapons Testing 12/7/61-12/18/61" Folder, NSF, Box 299A, JFKL. Teller's relationship with Kennedy was notably poor; Teller later recalled in his memoir that "My interactions with President Kennedy were the low point in my career as a diplomant." See Edward Teller with Judith Shooler, *Memoirs: A Twentieth-Century Journey in Science and Politics* (Cambridge: Perseus Publishing, 2001), 466.

"You have the flexibility to make whatever decision on this matter best supports your broader foreign policy and national security objectives."[102]

Wiesner's allies repeated this argument. Raskin implored Bundy not to allow atmospheric testing, on the grounds that "The world today is searching for some kind of moral and political leadership… For the first time in many years the United States can reclaim such moral and political leadership."[103] The distinguished non-scientist Arthur Schlesinger, Jr. described the problem in similar terms. "Technical evidence will not yield a clearcut answer" to the question of atmospheric testing, he wrote to Kennedy. "The decision, in short, is back in the political field." He cited a Gallup poll showing that popular opinion was evenly divided on the issue.[104]

Among the president's top advisors, however, opinion was overwhelmingly in favor of atmospheric testing. As Bundy acknowledged in a memo to the president, he, Rusk, Seaborg, McCone, Brown, and Lyndon Johnson all supported atmospheric testing. But Bundy was candid: the final decision belonged to the president, not his advisors. "I believe that if you personally care enough, and want to make the argument strongly enough, you can carry a decision against atmospheric testing with the Congress and the country," he wrote in a memo. Despite his own view that "on balance," the military advantages were real and the political risks and advantages "even," Bundy acknowledged to the president that an atmospheric ban was a plausible, "safe" option. If Kennedy chose to pursue it, his advisors would support him (though John McCone might be a tough sell). But Bundy also cautioned the president that the decision should be "yours

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[102] Wiesner to JFK, 19 December 1961, "Nuclear Weapons Testing 12/19/61-12/20/61" Folder, NSF, Box 299A, JFKL.

[103] Raskin to McGeorge Bundy, 20 December 1961, "Nuclear Weapons Testing 12/19/61 – 12/20/61" Folder, NSF, Box 299A, JFKL.

[104] Arthur Schlesinger Jr. to JFK, 27 December 1961, "Nuclear Weapons Testing 12/21/61 – 1/8/62" Folder, NSF, Box 299A, JFKL.

alone—not yours with support from politically vulnerable disbelievers like Wiesner." Though Wiesner's advice was sought and treated with respect at high level meetings, Bundy clearly considered him a potential political liability.[105]

Indeed, Wiesner was growing more agitated in his position by the day. As support coalesced around the resumption of atmospheric testing, he urged Kennedy even more vigorously to pursue not just an atmospheric ban, but a comprehensive test ban. In January 1962, the Air Force's Twining Commission delivered an influential report favoring a massively expanded testing program to Curtis Lemay, and Bundy forwarded a summary to the president, alerting him that it was "probably of high political importance."[106] That same week, Wiesner urged Kennedy to consider "two more comprehensive proposals which I believe would have greater political appeal than at atmospheric test ban." To achieve the comprehensive ban, the US should drop its requirements for on-site inspections down to the three that the Soviets had proposed. The ban could then be used as a "first stage" of an even more sweeping disarmament proposal. If the Soviets agreed, wrote Wiesner, "we will have made a great gain for world peace."[107]

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[105] McGeorge Bundy to JFK, 30 December 1961, "Nuclear Weapons Testing 12/21/61 – 1/8/62" Folder, NSF, Box 299A, JFKL.

[106] Bundy informed Kennedy that the report was "probably of high political importance, because men like Bethe and Baker have also signed it," and because it was being circulated among members of Congress. Bethe eventually submitted a strong dissenting addendum to the report, however, noting "the wide divergence of opinion between the majority of the Committee and myself." He wrote that he had signed because he supported the establishment of a Weapons Effects Laboratory, but he disagreed with the alarmist tone of the report, believed the US possessed sufficient "land based hardened missiles and Polaris" in the case of a Soviet attack, and considered additional testing to be "desirable but not a 'must'…" McGeorge Bundy to JFK, 17 January 1962, "Nuclear Weapons Testing 1/16/62-1/22/62" Folder, NSF, Box 299A, JFKL; Bethe to Twining, 26 January 1962, "Nuclear Weapons Testing Twining Committee on Military Implications of 1961 Soviet Nuclear Testing Report 1/5/62" Folder, NSF, Box 302A, JFKL.

[107] Wiesner to JFK, 25 January 1962, "Nuclear Testing 1962-1963" Folder, POF, Box 104, JFKL.

Less than a month later, Wiesner offered the president an even more radical proposal, a test ban and disarmament plan with three components: "1) a complete ban on nuclear weapons tests in all environments; 2) the cessation of all research and development on nuclear weapons; and 3) a complete cut-off of the production of fissionable material except for agreed quantities to be used for peaceful purposes." Rather than quibbling over "elaborate" inspections requirements in the service of more moderate measures, the drastic terms of the complete testing and research ban would, obviously, "justify a higher level control." Each side would have 20 annual inspection opportunities, and the ban on weapons research would be enforced "by placing permanent inspectors in all weapons laboratories and by maintaining a check on the activities of all scientific personnel previously engaged in weapons work." Wiesner's reaction to the prospect of resumed atmospheric testing was to support a sweeping ban on all nuclear weapons research.[108]

At the very least, Wiesner's proposals expanded the spectrum of possibilities open to the president, shifting any compromise position closer to the side of arms control. And, should Kennedy consider an atmospheric test series as a final, intensive research period before the imposition of a ban, Wiesner had provided support for the strongest arms control outcome yet. Kennedy, in fact, had evinced interest for just such an outcome. In late February 1962, even as the decision was being made to resume atmospheric tests, Kennedy met with Wiesner and top representatives from ACDA and, as described in the meeting minutes, "indicated the great importance he attaches to being prepared to offer a test ban treaty immediately," along with

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[108] Wiesner to JFK, 21 February, "Nuclear Weapons Testing General 2/17/62-4/4/62 and undated" Folder, NSF, Box 300, JFKL.

"across-the-board cuts in armaments."[109] Despite historical claims that Kennedy's science advisors compromised their ethical commitment to arms control by choosing technical arguments over political and moral statements, the actions of Wiesner demonstrate just the opposite—a dogged insistence on keeping a comprehensive ban in the picture, even in the face of unified opposition among Kennedy's top cabinet members, and the impending onset of atmospheric testing.

Outside of the halls of government, of course, far greater support for a comprehensive ban and opposition to atmospheric testing existed, particularly among elite scientists. In mid-February, 147 Cornell staffers urged Kennedy via telegram not to resume tests. Less than two weeks later, 72 Cornell faculty members, led by mathematician Jacob Wolfowitz, sent an alternate message: a general expression of "confidence" in the president to make a wise decision, and faith that should Kennedy opt to resume testing, it would be because it was necessary, not because he was swayed by reckless or irresponsible forces on either side of the debate.[110] (Thus, even the opposition message did not explicitly support new tests.) The Federation of American Scientists reiterated their call for an atmospheric ban, noting that resumed testing implied "that our security can in the long run be maintained solely by military strength," rather than by working politically for peace. In addition to this political argument, FAS warned of the danger of global fallout, which, though it would affect "only a very small fraction of the world's population," could nonetheless harm those who "have no voice in the decision to test."[111]

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[109] Memorandum for the Record, 28 February 1962 (meeting held 27 February 1962), "Disarmament Eighteen-Nation Disarmament Committee 3/6/62-3/14/62, 11/20/62 & undated" Folder, POF, Box 100, JFKL.

[110] "72 at Cornell Back Kennedy Over Tests," *New York Times*, 25 February 1962.

[111] *FAS Newsletter*, February 1962.

Fundamentally, FAS argued in an echo of Wiesner's words, both sides already possessed the technical power to destroy each other, and therefore "the social and political repercussions are quite as important as, and perhaps even more important than, the technical and military factors."[112]

When Kennedy did finally announce the new series of tests, to include atmospheric detonations, his national speech emphasized reluctance and thoroughness. "Every alternative was examined," he explained, and "No single decision of this Administration has been more thoroughly or more thoughtfully weighed." Kennedy reviewed the circumstances leading to the review and potential resumption of testing: the voluntary agreements of 1958 and Soviet violation of those agreements. He assured his audience that top advisors, including "the most competent scientists in the country" had reviewed testing policy, and that "Careful attention has been given to the limiting of radioactive fall-out, to the future course of arms control diplomacy, and to our obligations to other nations." With the "unanimous recommendations of the pertinent department and agency heads," Kennedy announced that he had authorized the Atomic Energy Commission and Defense Department to conduct a new series of tests, beginning in April, both underground and in the atmosphere over the Pacific.

Kennedy devoted a significant portion of the speech to the problem of radioactive fallout, emphasizing efforts to minimize the inevitable but small amounts of additional radiation anticipated. He regretted that he was forced to "balance" the hazards of radiation against "the hazards to hundreds of millions of lives" at stake in the arms race, but the recent Soviet tests—including tests of smaller and more explosive weapons—required a US response. Specifically, the United States planned to conduct several different kinds of tests: "proof tests" of current

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[112] FAS, "Scientists Appraise Atmospheric Tests," *Bulletin of the Atomic Scientists,* 1 April 1962, 33.

systems, “effects tests” of how various defensive technologies could survive a nuclear attack (for example, hardened silos or command and control centers), and tests of new and developing technologies. On this last point, Kennedy observed evocatively that “if we are to maintain our scientific momentum and leadership—then our weapons progress must be not limited to theory or to the confines of laboratories and caves.”

Nonetheless, Kennedy repeatedly emphasized US commitment to arms control. Anticipating an international outcry at the testing resumption, Kennedy reminded potential critics “that this country long refrained from testing, and sought to ban all tests, while the Soviets were secretly preparing new explosions.” Instead, he argued, the United States was “seeking an end to testing and an end to the arms race” in good faith, while protecting freedom around the world. Given the recent Soviet test series, national security—not psychology or politics—required a resumption of US testing, and a failure to follow through would indicate weakness and “a failure of will.” Blame for the failure of arms control efforts lay with the Soviet leadership. The US had tried, but “the basic lesson of some three years and 353 negotiating sessions in Geneva” was that the USSR had refused an enforced ban, preferring an “uninspected moratorium” under which they could test in secret.[113]

Although the *New York Times* editorial page offered strong support for the President’s decision, in other quarters the prospect of resumption met with condemnation. Linus Pauling responded with a scathing telegram to Kennedy, highlighted the fallout risks to unborn children and referring to atmospheric testing as a “monstrous immorality” that could render the president as “one of the most immoral men of all times and one of the greatest enemies of the human

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[113] Transcript of Kennedy’s speech (2 March 1962), *New York Times,* 3 March 1962.

race."[114] The *Los Angeles Times* interviewed four West Coast scientists for an article titled "Sky Tests Win Support of Scientists," but two of the subjects couched their support in bittersweet terms. "I was sad about it but there was no other course," observed the Manhattan Project pioneer and Nobel laureate Harold Urey. Caltech geneticist E.B. Lewis offered similar sentiments: "It's very discouraging, very depressing."[115]

The Federation of American Scientists took a more muted position than that of Pauling, officially opposing atmospheric testing but expressing appreciation for Kennedy's overall commitment to disarmament. The following month, however, a soul-searching essay on "The Future of FAS" demanded a reassessment of the organization's goals and tactics, given the dramatic shifts in nuclear politics since its World War II-era founding. Michael Amrine, a former editor of the *Bulletin of the Atomic Scientists,* observed that FAS scientists no longer held a monopoly on nuclear expertise, nor was their political influence as powerful as it may once have been. He noted, "When our Council voted to ask the President not to resume nuclear tests, we did so quite openly, knowing that he could and probably did have information—as well as councils of scientific discussion—not open to us. Just what is it that we, the FAS brains, knew that the President might not? What special virtues do our councils possess?"[116]

One new approach came from Leo Szilard, the nuclear pioneer turned Pugwash member who had met with Khrushchev personally in the fall of 1960 to urge him towards arms control. Through the winter of 1962, he crisscrossed the country on behalf of his "Scientists' Committee for a Liveable World," recruiting over 2000 volunteers to commit 2% of their income to elect

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[114] *New York Times,* 3 March 1962.

[115] "Sky Tests Win Support of Scientists," *Los Angeles Times,* 3 March 1962, 20.

[116] *FAS Bulletin,* April 1962.

pro-peace candidates.[117] It was not enough for him—or other elite scientists—to attempt to persuade politicians personally, he explained to his university audiences, for "these distinguished scholars and scientists would be heard" but not necessarily "listened to." Votes were what mattered. In pursuing his radical dream of disarmament and the abolition of war, Szilard urged that most traditional of American activities: electioneering.[118]

While Szilard, FAS, and other activist groups criticized the "Dominic" series of atmospheric tests that were underway through the spring and summer of 1962, inside the administration, Wiesner kept up the pressure for strong disarmament measures and for a comprehensive test ban, now urging that the threshold for underground explosions be dropped entirely.[119] In a detailed memo to the president, Wiesner fleshed out his reasoning in both the required "technical-military" terms and in his own moral and political language. In a prescient analysis "of a non-technical nature," Wiesner noted that the military supporters of the threshold had an ulterior motive, the "hope for future tests." He warned that "if the present treaty [with a threshold] is signed there will be continued pressure to renounce the new moratorium or to not renew it, just as there was last spring and summer. You will, in fact, concentrate all of the pro-testing forces on this one loophole which will be left in the treaty."

Technically, he argued, the Soviets were correct to complain that a treaty with a threshold was not a true ban, since low-yield tests—anything less than the equivalent of a 4.75 magnitude

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[117] *FAS Newsletter,* May 1962.

[118] Leo Szilard, "Are We on the Road to War?" *Bulletin of the Atomic Scientists,* 1 April 1962, 23-30.

[119] In the concurrent discussions of disarmament measures, Wiesner endorsed a "30% across-the-board cut of all armaments applied by type…and a complete production cutoff." Memorandum for the Record, 6 March 1961 Meeting on Disarmament, 7 March 1962, "Disarmament-Nuclear Test Ban Negotiations 4/28/61-3/63" Folder, POF, Box 100, JFKL.

earthquake—could continue underground according to currently proposals.[120] In Wiesner's estimation, removing the threshold would make Soviet cheating *more* difficult, not less so, since any seismic event would then be open to investigation. In the meantime, with no major technical advances on the horizon, "the threshold proposal is fundamentally a scientifically indefensible position." Any such proposal was rooted in "hope that the threshold will 'go away' through some technical discovery, while in fact there is no such hope," he wrote. Rather than get bogged down in exactly what could or could not be detected, the US was better off dropping the threshold entirely.[121]

There was, in fact, a breakthrough of sorts on the horizon. Beginning in 1958, the Air Force scientists who ran the detection system for nuclear explosions had begun to suspect that their estimates of naturally-occurring Soviet seismic activity might by incorrect. By 1961, VELA researchers working with more accurate instrumentation systems expressed similar doubts, and an ACDA investigation confirmed that the US had drastically overestimated the likely number of earthquakes that could potentially be confused with underground nuclear tests. (The revised number, in fact, largely agreed with the estimates the Soviets had offered in 1958.)[122] At the same time, detection technologies themselves had improved, allowing longer-range monitoring from fewer observation stations.[123]

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[120] The 4.75 number had been determined by 1959 PSAC Panel on Inspection Problems, headed by Robert Bacher. "Major Actions of the President's Science Advisory Committee, November 1957-January 1961," 13 January 1961, "PSAC 1/61-3/61" Folder, POF, Box 86, JFKL.

[121] Wiesner to JFK, 9 March 1962, "Nuclear Testing 1962-1963" Folder, POF, Box 104, JFKL.

[122] Kaysen to JFK, 20 July 1962, "Nuclear Weapons Testing General 4/15/62-7/30/62 & undated" Folder, NSF, Box 300, JFKL.

[123] Bundy to JFK, 26 July 1962, "Nuclear Testing 1962-1963" Folder, POF, Box 104, JFKL.

As Wiesner had predicted, however, the technical revisions did not substantially shift the existing support and opposition to a comprehensive, no-threshold ban. At the Principals' meeting in July 1962, ACDA proposed just such a ban, with 'no right to test at all' and a monitoring network of international and national resources. Due to the new seismic information, the required number of detection stations and on-site inspections could be reduced to numbers more amenable to the Soviets. Should that plan fail, a ban on atmospheric testing alone could be a backup. While Wiesner endorsed the first option, representatives from the AEC preferred the second. Seaborg told his colleagues that 'AEC's chief concern was the effect of stopping underground testing on the vitality of our laboratories,' and Leland Haworth, the AEC Commissioner, added that 'an atmospheric ban, with continued underground testing, would permit us to maintain a posture of readiness.' Suddenly the problem of Soviet cheating was not the sticking point, but the real consequences of a test ban for US nuclear weapons development.

Other Principals quickly rallied around the atmospheric ban. Rusk, noting that Soviet negotiators might not accept even a reduced number of on-site inspections, preferred to start with an offer of an atmospheric treaty and then try "making it comprehensive as soon as possible." In terms of world opinion, atmospheric tests were all that mattered. Murrow agreed. But Wiesner persisted in his call for more sweeping measures. 'Why not try to get a total ban,' he asked his colleagues. Why start with the limited, narrowest option?[124] In a memo written that same day, Bundy summarized the new data and its possible consequences for the test ban to the president. The new technology and revised data "opens the way to simple systems of detection and probably to smaller numbers of on-site inspections," Bundy explained, but "it is still necessary, in the present state of the art, to have a right to inspect. This is the unanimous view of British and

[124] Memorandum of Conversation, Meeting of Committee of Principals. 26 July 1962, "ACDA Disarmament Committee of Principles Memos of Conversation 3/61-11/63" Folder, NSF, Box 267, JFKL.

American technical experts and of all your noisy advisors." The ultimate decision about the type of test ban to pursue belonged solely to the president, and Bundy reminded him of the political stakes. On the one hand, the test ban was critical for "preventing proliferation," which he felt had become "more and more urgent for both us and the Russians." On the other hand, reducing treaty demands—even if supported by technical advances—might be perceived as caving to Soviet wishes, and could "hand the opposition a juicy issue for November." Bundy warned Kennedy that "many of your advisors will argue … that this is no time for concessions of any sort." He now credited Wiesner's tireless efforts to keep the full range of treaty options open. He counseled the president: "What you can do—and here, by a good deal of in-fighting, Jerry [Wiesner] and I have fully preserved your freedom of choice—is to decide."[125]

The political pressure regarding the appearance of concession was perhaps more powerful than Bundy anticipated. Even William C. Foster of ACDA sided with Rusk in suggesting that an atmospheric ban be presented first, so as not to appear to be "scaling down" the "safeguards that this administration and the prior administration have been insisting was the minimum necessary for national security."[126] In Congress, Chet Holifield of the Joint Committee on Atomic Energy warned the president not to make any hasty negotiating decisions based on the new data.[127] California Republican Craig Hosmer expressed skepticism about the "supposed advances in the science of seismology," and called the media coverage of ARPA's Project VELA nothing more than "propaganda to try to drum up support for the test ban treaty

[125] Bundy to JFK, 26 July 1962, "Nuclear Testing 1962-1963" Folder, POF, Box 104, JFKL.

[126] William C. Foster to JFK, 30 July 1962, "Disarmament-Nuclear Test Ban Negotiations, 7/30/62" Folder, POF, Box 100 (Overflow), JFKL.

[127] Holifield to JFK, 25 July 1962, "Joint Committee on Atomic Energy Testing Hearings 7/19/62-7/23/62, 7/19/62-8/2/62" Folder, NSF, Box 282, JFKL.

highlights." Edward R. Murrow "wouldn't recognize a seismograph if he saw one," Hosmer complained, and VELA didn't justify "accommodation" to the Soviet position unless one read the evidence after "putting on rose colored glasses and chewing tranquilizers." More broadly, the test ban itself was an empty "symbol of peace," whose value had "become artificially inflated by the hypnotic effects of constant misleading propaganda."[128] Amid stiff Congressional opposition and the constant reminders from the AEC that underground testing would "keep our laboratories alive and vigorous,"[129] by the end of July, Kennedy had been convinced that political will was allied against any kind of treaty. Better chances at Senate approval for any agreement would have to wait until the fall.

Unfortunately for Kennedy, the autumn of 1962 was no time for a massive public campaign on behalf of a test ban. Though September and October saw continued discussions of various disarmament measures and additional monitoring techniques (including the Pugwash-recommended system of 'black boxes'—automatic devices to detect and record seismic activity within a host country), the Cuban missile crisis diverted personnel and resources away from test ban treaty negotiations. Lower-level deputies stood in for their absent bosses at the November 1 meeting of the Committee of Principals, and when the top advisors finally returned ten days later, Rusk reported that prospects for disarmament negotiations with the Soviets looked "gloomy," though the president still hoped to make progress on a test ban. William Foster of

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[128] Hosmer press release, 31 July 1962, "Nuclear Weapons Testing General 4/5/62 – 7/30/62 & undated" Folder, NSF, Box 300, JFKL.

[129] Quoted in memo attachment, Bundy to JFK, 30 July 1962, "Disarmament-Nuclear Test Ban Negotiations, 7/30/62" Folder, POF, Box 100 (Overflow), JFKL.

ACDA observed mournfully that the missile crisis had revealed the perils of the arms race, and the rationale for seeking ‘a more stable world,’ even as relations deteriorated.[130]

But by February 1963, Kennedy and the Principals were back to debating threshold levels and numbers of on-site inspections (the Soviets wanted no more than three; McNamara was prepared to offer six). Maxwell Taylor, now the Chairman of the Joint Chiefs, had joined the Committee of Principals and represented a strong new voice opposed to a comprehensive test ban.[131] To bolster the Joint Chiefs’ case, the 1962 Twining report prepared for Curtis LeMay had now been updated to take into account the recent atmospheric tests by both the US and the USSR. Signed by John Foster of Livermore, Simon Ramo of Thompson Ramo-Wooldridge, Edward Teller, Stanislaw Ulam, John Wheeler, and William McMillan, the report asserted that “A test ban would involve greater risks to the national security than perhaps have been realized.” The administration’s claims that a ban would protect the US nuclear advantage and minimize the risks of a surprise attack were, “from a scientific and military viewpoint… not valid.”

In a separate appendix, William McMillan, a University of California chemist, RAND advisor, and now Chairman of the Defense Research & Engineering Ad Hoc Weapons Effects Group, laid out a detailed list of potential tests, ordered by priority, that would be eliminated with a test ban. These included tests of the survivability of hardened missile sites, reentry vehicles, and communications systems, as well as tests relevant to antiballistic missile system capabilities, implying that gains in all these areas would be lost with testing restrictions. Another appendix affirmed the urgency for continued weapons testing and quoted a report from the Air

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[130] Memorandum of Conversation, Meeting of Committee of Principals, 10 November 1962, “ACDA Disarmament Committee of Principals Memos of Conversation 3/61-11/63” Folder, NSF, Box 267, JFKL.

[131] Meeting minutes, 18 February 1963, “Meetings and the President Test Ban Treaty 2/18/63” Folder, NSF, Box 317a, JFKL.

Force Science Advisory Board on potential new advances in tactical nuclear weapons. It was not true, the authors argued, that no new technological leaps were on the horizon. "If we do not keep the scientific leadership," they warned ominously, "others will take it." Distaste for the administration's position was clear: "If the United States is to renounce a revolutionary device which others can then secure without our knowledge, no portion of this responsibility should attach to the Air Force."

In a dramatic closing section, the report reiterated Teller's concerns about the effects of the test ban on morale and recruitment at the weapons laboratories. The authors repeated the old hydrogen bomb arguments in stark terms:

> The technology of nuclear energy has its own laws and its own internal structure. It can be stopped by the hand of man as little as the advance of weaving machinery could be stopped by the Luddite mobs. But we can be stopped by our own actions. Out of a developing nuclear technology, we see emerging better defense and peaceful uses of nuclear energy. But the most important peacetime use of nuclear energy is the preservation of the peace.[132]

The implications were clear: scientific knowledge and technical development were inevitable. Voluntary avoidance of research was naïve, misguided, and dangerous; it would not prevent the development of weapons by others, and it would endanger US nuclear superiority and, therefore, national security. Unlike the similar debates over the production of the hydrogen bomb, in this case Teller and Ulam would ultimately find themselves on the losing side.

Resistance in Congress was also still strong, particularly after members of the Joint Committee quizzed representatives from Los Alamos and Livermore in closed sessions about

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[132] "Report of the Twining Committee: Military Implications of US and Soviet Nuclear Testing," 4 March 1963, "Nuclear Weapons Twining Committee Report to Chief of Staff of US Air Force, Military implications of US and Soviet Nuclear Testing" Folder, NSF, Box [302A], JFKL.

what kinds of clandestine testing might take place, and what potential "significant advances" the Soviets might achieve.[133] Meanwhile, Wiesner tried to combat the dire warnings of the updated Twining report by reassuring the president that the additional tests proposed by the AEC were actually "unimportant and will not contribute to our national security."[134] By the time of the April 1963 meeting of the Committee of Principals, tensions were high but a fragile foundation of support for a comprehensive treaty seemed to exist. Maxwell Taylor reported that the Joint Chiefs still considered a treaty with no threshold to be "unsatisfactory," but meeting minutes confirmed a consensus "that the text was adequate and that a test ban treaty was still in the national interest of the United States." William C. Foster reported that the only unresolved issues were conditions for peaceful nuclear research under the Plowshares program. On this front, Seaborg hoped to allow for additional testing, while Wiesner warned that Plowshares tests, no matter how innocently described, 'almost certainly would contribute to weapons development.'[135]

The primary obstacles to a successful treaty, of course, were not Plowshares tests but Soviet recalcitrance and Senate opposition. The comprehensive treaty proposal sent by the Kennedy administration after the April Principals' meeting met with a frosty Soviet response.[136] Congress seemed more malleable. In May 1963, a prominent contingent of arms control

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[133] John Pastore to JFK, 8 March 1963, "Joint Committee on Atomic Energy General 1963 and undated" Folder, NSF, Box 281a, JFKL.

[134] Wiesner to JFK, 13 May 1963, "NSAM 210: Underground Nuclear Tests, 12/12/61, 12/62 – 12/63, and undated" Folder, NSF, Box 339, JFKL.

[135] Meeting of the Committee of Principals, Actions Taken on Agenda Items, and Memorandum of Conversation, 17 April 1963, "ACDA Disarmament Committee of Principals Memos of Conversation 3/61-11/63" Folder, NSF, Box 267, JFKL.

[136] The *Baltimore Sun* noted that it had "produced no progress" and that the Soviets had evinced "a declining interest in a test ban." See "Scientific Study Asked On Test Ban," *Baltimore Sun,* 1 June 1963.

scientists began circulating a public statement supporting a test ban. They were led by Argonne's David Inglis, who had first called for a test ban in the pages of the *Bulletin of the Atomic Scientists* in 1954. Of the nine major signatories, four—Freeman Dyson, Donald Glaser, Hans Bethe, and Francis Low—were Jason members or advisors. (Other Jasons added their names as co-signers, along with Harvard's Salvador Luria, Matthew Meselson, and a dozen others.) The scientists emphasized the advances in detection techniques from 1958 to 1963, noting that with current technologies, "any significant series of [underground] tests would be almost impossible to conceal." More importantly, they reiterated that both sides already possessed "over-kill capabilities" that would maintain deterrence even with the most stringent testing ban. A test ban, in their view, would be a powerful tool in reducing the risks of the arms race and nuclear war. *New York Times* coverage quoted the statement's moral message—that the ban stood in "the best interests of the United States and world peace"—and cited by name the three Nobel Prize-winning signers: Donald Glaser, James Watson, and Albert Szent Gyorgyi. Within weeks, a wide range of intellectuals and cultural icons had signed on to similar and expanded statements of support, as prominent scientists reached out to the leading lights of arts and literature, including Leonard Bernstein, John Steinbeck, Aaron Copland, John Huston, and Elia Kazan.[137]

A new round of negotiations with Khrushchev was announced in early June. With the Principals' tentative consensus on a comprehensive ban and the stirrings of a movement of scientists and intellectuals prepared to lobby Congress and public opinion, the prospects for success seemed promising. The *New York Times* called the planned talks "a glimmer of hope."[138]

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[137] Statement, 13 May 1963, "Disarmament-Nuclear Test Ban Negotiations 4/62-8/63" Folder, POF, Box 100, JFKL; Statement, 13 May 1963, "Text of Scientists Statement Supporting Test Ban Treaty" Folder, POF, Box 100 (Overflow), JFKL; "27 US Scientists Urge Test-Ban Pact," *New York Times,* 13 May 1963; "Education Urged On A-Tests Ban," *Washington Post,* 27 May 1963.

[138] "New Hope for a Test Ban," *New York Times,* 11 June 1963.

But three days later, the newspaper reported that Khrushchev had once again rejected US inspection terms, indicating that he might even withdraw entirely the previous Soviet acceptance of three on-site inspections. At the Committee of Principals' meeting on June 14, Rusk quickly laid the main issues on the table: he had little hopes for the current talks, given that Khrushchev had claimed 'that inspection is a form of espionage' and the US was unwilling to sign a treaty without any on-site monitoring whatsoever. But, Rusk noted, the president's commitment to arms control ran deep. Meeting minutes quoted Rusk as reporting that "the President feels the mission should be made because this may be our last chance to avoid a larger and more difficult arms race … In 10 or 20 years it will be important that the US made as great an effort as possible to achieve a test ban."

Perhaps Rusk hoped that these stirring words would rally the Principals; instead, the meeting rapidly degenerated into an angry, fractious argument over US security, the worth of a test ban, and the Joint Chiefs' threat to testify against the treaty in Congress. As before, Maxwell Taylor complained that the current treaty draft would permit Soviet cheating and was therefore 'not in the national security interest of the United States,' but now he implied that the military leadership was prepared to state their opposition openly in Senate hearings. When pressed by Rusk, Taylor suggested that the Joint Chiefs might be able to support an atmospheric-only test ban. Rusk seemed shocked that the Joint Chiefs would dare to oppose the official administration position. According to meeting minutes, Rusk noted pointedly that "he would not feel free to take a foreign policy position that disagreed from that of the President." In defending himself, Taylor cited the updated Twining report and the technical information provided by the weapons labs. The room erupted in argument. Wiesner, true to form, reiterated that the matter was political, not technical, and that whatever their theories about Soviet cheating, "the laboratory directors are not in a position to judge the overall policy considerations." Rusk noted bitterly that he "didn't think Edward Teller was talking as a technical man when he talked about the test ban." Put on the defensive, Taylor retorted that the Chiefs 'were conscientious men who were sincerely concerned about our national security,' and invited ACDA representatives to convince the military leadership that their concerns were unfounded (William C. Foster accepted the challenge). As McNamara weakly called for more studies and meetings, Rusk, incredulous, replied that "members of the Committee had all agreed, he thought, that the risks to national security from an unlimited arms race outweighed the risks inherent in a test ban treaty."[139] But the Joint Chiefs had not, it seemed, and with their political threat to undercut the proposed ban in Congress by citing the analyses of Edward Teller and the Twining report, the prospects for a comprehensive treaty looked as dim from the American side as they did from that of the Soviets.

The stage was set, then, for the success of the compromise partial test ban treaty, which forbade testing in all environments—in the atmosphere, outer space, and underwater—except underground. The treaty satisfied the moral arguments about the dangers of fallout, and offered a tentative step in the direction of arms control, with hopes for future advances. More importantly, it was acceptable to Khrushchev, and prospects for ratification seemed promising in the US Senate.

For the most part, arms control scientists supported the treaty. Biologist and antinuclear activist Barry Commoner later called it a triumph for environmentalism.[140] FAS sent physicist

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[139] Memorandum of Conversation, Nuclear Test Ban Treaty Meeting of Committee of Principals, 14 June 1963, "ACDA Disarmament Committee of Principals Memos of Conversation 3/61-11/63" Folder, NSF, Box 267, JFKL.

[140] Michael Egan, *Barry Commoner and the Science of Survival: The Remaking of American Environmentalism* (Cambridge: MIT Press, 2007), 11.

and Jason member Freeman Dyson to testify before the Foreign Relations Committee in support of the ban, and after ratification, FAS hailed the treaty as "a first and significant step to slow the pace of the arms race and reduce the danger of nuclear war."[141] Kennedy's PSAC offered a strong statement of support in mid-August, timed to encourage Senate passage. The scientists acknowledged immediately that the treaty raised "many important questions other than those of a technical nature." Nevertheless, the PSAC, drawing on its own expertise and "the assistance of outstanding scientists and engineers throughout the United States," chose to emphasize technical discussion. They affirmed that sufficient detection methods existed to enforce the terms of the treaty, and that the test ban would not prevent the development of defensive techniques for hardening missile sites or exploring an anti-ballistic missile system. In reality, with the elimination of the underground component of the treaty, little technical debate actually existed within the government leadership, but the patient explanations of scientists seemed a useful tool for rallying public opinion in support of the ban.[142] In a press conference held at Los Alamos, lab director Norris Bradbury told a pool of reporters that the treaty would cause little or no change in the staffing and research routines of the lab. Bradbury tried to appeal to both sides of the debate. The test ban would not halt nuclear research or, realistically, the development of new types of weapons, he noted reassuringly. And fundamentally, the importance of any bomb research—and the ethical justification for the existence of Los Alamos—was deterrence. "Los Alamos," he reflected,

> has no fondness for atomic weapons per se; people in the Laboratory don't work on atomic bombs because they like to kill people, think of them killing people. They have only worked in this over the last 20 years because we thought in some

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[141] *FAS Newsletter,* September 1963.

[142] Public statement by PSAC on Nuclear Test Ban Treaty, 24 August 1963, "Disarmament-Nuclear Test Ban Part I Negotiations 7/63 Meeting in Moscow" Folder, POF, Box 100 (Overflow), JFKL.

> way this provided a strength for the country to avoid war, to bring about, ultimately, as there seems today to be a start, a step toward the abolition of war.[143]

The words of Bradbury and PSAC were carefully planned, as was the administration's public relations campaign in support of the treaty, which drew heavily on the efforts of prestigious scientists. In a note to Ted Sorensen from Kennedy staffer Fred Dutton, Dutton summarized the work of pro-test ban efforts by business and agricultural groups, churches, the AFL-CIO, and other "voluntary constituent organizations." Of the latter category, Dutton wrote that he wished to "contain" groups like "the Friends, SANE and others who have had a major interest in a test ban" because "I personally do not think that we pick up any support, but only suspicion, if they lobby the Hill or get out in front publicly on the treaty." Instead, Dutton hoped to elevate the actions of elite scientists, such as the Nobel Prize winners who had already registered their support, and a small group of "particularly effective" men, including Kistiakowsky, Killian, Rabi, and Herbert York, who were already planning a series of private meetings with key Senators. Another contingent of life scientists had also been tapped to "dramatize the fall-out problem."[144] Though many of these men maintained moral and ethical commitments to arms control not entirely unlike the convictions of SANE members and other activists, the administration preferred their ability to emphasize technical and scientific language when offering their political endorsement of the treaty. In his history of the early years of PSAC, Zuoye Wang has described this kind of political maneuvering in another way. The PSAC, in his analysis, acted according to "their recognition of the necessity to view scientific and technological solutions within a social and political context," which "underlined their insistence

[143] "Bradbury Tells Newsmen Treaty to Have Little Effect on Lab," *Los Alamos Scientific Laboratory News,* 1 August 1963, clipping in "Disarmament-Nuclear Test Ban Part I Negotiations 7/63 Meeting in Moscow" Folder, POF, Box 100 (Overflow), JFKL.

[144] Fred Dutton to Sorenson, 16 August 1963, "Disarmament – Nuclear Test Ban Part II Negotiations – 7/63 Meeting in Moscow" Folder, POF, Box 100 (Overflow), JFKL.

on examining not only the means, but also the ends of technological programs of the government."[145] In the case of the test ban, the PSAC, led by Jerome Wiesner, were committed fully to both the technical means and the moral and political ends.

In the end, the treaty passed the Senate by a vote of 80-19, though Senators added stipulations ensuring that weapons production would continue despite the test ban. As Gaddis has noted, nuclear stockpiles increased during the Kennedy administration, such that by 1964 there was "an increase of 150 percent in the number of nuclear weapons available, a 200 percent boost in deliverable megatonnage, the construction of ten additional Polaris submarines (for a total of 29) and of 400 additional Minuteman missiles (for a total of 800) above what the previous administration had scheduled."[146] Though the Committee of Principals continued to meet up until the week before Kennedy's assassination, no new progress would be made on a comprehensive test ban for another three decades.

Outside the halls of government, research on nuclear weapons and an anti-ballistic missile system would proceed apace at the nation's weapons labs, but, as Teller had predicted, by the mid-1960s the morale of lab staffers and the prestige of their work had indeed begun to decline. A 1965 *Washington Post* article about weapons scientists observed that "few young scientists and engineers regard nuclear weapons work as the cutting edge of science as it was when Fermi and Oppenheimer, Rabi and Teller were at Los Alamos." Los Alamos in the 1960s, the authors observed, was "suffering from the same doubts that plague a middle-aged man who wonders whether he is as secure as he thought." Worried about national security, their jobs, and

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[145] Zuoye Wang, *In Sputnik's Shadow: The President's Science Advisory Committee and Cold War America* (New Brunswick, NJ: Rutgers University Press, 2008), 9.

[146] Gaddis, 217.

their declining social status, many scientists threw their political support behind the arms buildup and against restrictions on testing or other arms control measures.[147] This shift in thinking about nuclear weapons research—from its 1945 image as simultaneously glamorous and destructive work demanding personal atonement to its 1960s incarnation as regular employment requiring political protection—offers a glimpse at some of the ways in which ideas about weapons work changed as the Cold War progressed.

Although some arms control activists were dismayed by the compromised outcome of their efforts, the consequences of the test ban for the Manhattan Project generation of physicists were largely positive. Elevated to high government service by their expertise and their desire to contribute to the expansion of American science, PSAC members during the Eisenhower and Kennedy administrations had seized the opportunity to promote arms control ends, with notable success. In 1963, their public prestige and influence was as great as ever, as evidenced in the prominent role Kistiakowsky and others played in securing Senate passage of the test ban, and in the weight afforded by the popular press to any scientist with a "Nobel laureate" appellation and an opinion on weapons research. In the late 1970s, Killian would look back on the 1950s and recall that "Those were memorable and exciting times when government, industry, and the universities felt themselves in a symbiotic relationship and achieved a powerful creative collaboration."[148] Though many PSAC advisors of this period welcomed Eisenhower's famous words about the dangers of a military-industrial complex, events of the late 1950s and early 1960s suggested ways in which elite scientists could work within the system toward salutary ends.

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[147] *Washington Post*, 25 April 1965.

[148] Killian, 104.

In the most intimate, high-level meetings of the Committee of Principals, Wiesner consistently offered an impassioned, uncompromising voice in favor of the most radical arms control positions of anyone in the room. He had the President's ear and was respected, listened to, and consulted. Years later, one of Wiesner's colleagues recalled that "The Test Ban for Jerry was a great achievement, and a great failure (because it was only partial)."[149] When Wiesner's successor, Donald Hornig, was announced shortly after the successful passage of the test ban, Wiesner prepared to leave government with every reason for pride in his contributions to government service.

Fittingly, Wiesner exited his advisory role, where he had pushed successfully for expanded federal funding for academic science, to resume his academic and administrative work at the Massachusetts Institute of Technology. Even before Sputnik, MIT had been a major recipient of defense research contracts, but the post-Sputnik boom saw the dramatic expansion of research money and military-related work. The 'military-industrial-academic' complex, a step beyond the expansive system about which Eisenhower had warned in 1960, was in full swing in Cambridge, Massachusetts. When Wiesner returned to his academic home in 1963, this work was still considered prestigious and worthwhile. Less than ten years later, the campus would erupt in protests over the very defense contracts and government affiliation Wiesner had welcomed as a member of PSAC. The shift in attitudes toward the relations among government, military, academia, and industry was dramatic, but not surprising; between 1963 and 1973 lay the tragedy, devastation, and polarization of the Vietnam War. How that war shaped both the ethical concerns of elite scientists and the institutions in which they worked and researched forms the heart of the following four chapters.

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[149] Emma Rothschild, "Continuing Communication," in Walter A Rosenblith, ed., *Jerry Wiesner: Scientist, Statesman, Humanist* (Cambridge: MIT Press, 2003), 161.

Part II: The Science of Vietnam

The expansion of science advising during the Eisenhower Administration and the appeal of Secretary of Defense McNamara's reduced emphasis on nuclear weapons created a cadre of loyal, committed scientists, ensconced at all levels in the Kennedy Administration, just in time for the rapid expansion of U.S. involvement in Vietnam. During the Kennedy and Johnson years, presidential science advisors, Pentagon consultants, and military researchers would be called upon to contribute to the development of weapons technology and strategic planning necessary for counterinsurgency operations in Southeast Asia. Scientists pioneered and then evaluated the efficacy of defoliant operations, assessed the merits of bombing campaigns, proposed new applications for sensors and communications technology in order to design 'electronic barriers,' and offered countless other forms of input and criticism. Scientists both within and without government reacted to the expansion of the war with a range of responses, from disillusionment and protest to fervent support and voluntary assistance. Within the ranks of government and military advisors, some scientists found their consciences challenged by the work they were asked to perform. Others felt a responsibility to mitigate a bad situation by using technology to prevent escalation. Yet a third group, proud of their contributions to the war effort, stepped into the public spotlight in order to counter what they perceived as uninformed outside criticism. This outside criticism came not just from student protesters, but from academic scientists as well, many of whom took it upon themselves to conduct their own independent evaluations of the effects of new weapons technologies, particularly the controversial use of defoliants and tear gases, considered by many to constitute chemical warfare.

As a few contemporary observers pointed out, scientists' contributions to the war in Vietnam consisted largely of "applying engineering skills to produce weapons and equipment from items which were already available," including napalm, defoliants, and sensor technologies. There was no mobilization on the scale of the Manhattan Project or OSRD.[1] Some top scientists also participated in high-level decisions concerning bombing escalation and anti-infiltration techniques. This small group perhaps earned a share of the stigma accompanying explicit weapons and war work, although they later suffered criticism disproportionate to their actual contributions. But the stigma of Vietnam extended far beyond just this handful. The deep anger and opposition to the war subjected every new wartime technology, and in most cases, recycled wartime technology, to scrutiny and outrage. Much of this criticism came from scientists themselves, who in some cases succeeded in shaping and reforming policy. At the same time, antiwar sentiment fueled an attack on American scientists more broadly, one that purported to expose the applications of science and technology to an immoral war and scientists' existence as part of a military-industrial complex. As we shall see in subsequent chapters, critics attacked scientists' funding ties to the military, their advisory roles (even if their advice was often far less hawkish than actual enacted policy), and, in a deeply critical analysis, their perceived complicity in nearly every dehumanizing aspect of U.S. foreign policy, consumer culture, and capitalism itself. That the key architects of the war—from McNamara to Westmoreland—regularly invoked the language of science and experimentation only magnified the opposition.

The following chapters do not offer a comprehensive account of every manner in which scientists contributed to the war in Vietnam, although many are discussed, but rather an assessment of the various kinds of roles that scientists played; the ethical debates concerning controversial technologies and strategies (particularly defoliants and gases, bombing campaigns, and the electronic barrier); and the ways in which the progression of the war laid the groundwork

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[1] Luther J. Carter, "Vietnam: Jungle Conflict Poses New R&D Problems," *Science* 152 (8 April 1966), 188-189.

for a deep antipathy towards scientists and engineers, a sentiment that would appear in force on university campuses throughout the late 1960s and early 1970s.

Chapter Two: Chemicals and Ethics

Limited War

Shortly before the 1960 election, Jerome Wiesner wrote a lengthy memo to John F. Kennedy discussing key military, technical, and political problems facing the country. Although Wiesner wrote mainly about nuclear weapons and the arms race, he also warned the future president about the prospect of "limited war"—non-nuclear military conflicts that might include "jungle fighting in the far east," and could require "specialized weapons for the different areas where we might have to fight and specialized for the military situation involved."[1] Wiesner's memo reflected much of the work and many of the concerns of Eisenhower's Presidential Science Advisory Committee (PSAC). Earlier that year, George Kistiakowsky had sent Wiesner a PSAC budget review containing similar warnings. "We believe there are serious deficiencies in our limited war capabilities," wrote Kistiakowsky. He urged the renewed development of smaller conventional weapons to correct what he perceived as a dangerous imbalance favoring the nuclear arsenal. He blamed military leaders for developing "the big weapons systems required for general war to the neglect of the specialized weapons and systems needed to deal with limited war." Additionally, Kistiakowsky wanted improved surveillance and reconnaissance technologies and expanded research on non-nuclear technology, including chemical and biological weapons. On the latter topic, Kistiakowsky criticized the Navy and Air Force for failing even to request funds for "delivery and dissemination of these agents."[2] In these

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[1] Memo, Jerome Wiesner to John F. Kennedy, 23 February 1961 (reprint of memo from September 1960), Folder 16, President's Office Files, Box 67, John F. Kennedy Library.

[2] Memo, George Kistiakowsky to Jerome Wiesner, 12 January 1960, "PSAC 1/61-3/61" Folder, POF, Box 86, JFKL.

discussions, Kistiakowsky summarized the conclusions of two key PSAC panels—the eight-member Panel on Biological and Chemical Warfare, which in 1959 had recommended expanded BW and CW research programs, and H.P. Robertson's Limited War Panel, which criticized both the lack of conventional weapons research and the "organizational structure of the military establishment" responsible.[3]

As discussed in the previous chapter, Kennedy's selection of Robert McNamara as Secretary of Defense pleased science advisors who supported arms control, and for whom "flexible response" was a welcome alternative to Eisenhower's defense posture based on the threat of massive nuclear strikes.[4] Within the first two months of Kennedy's presidency, the Pentagon was echoing Wiesner's warnings. A Defense Department report released in January 1961 noted that "Of all the areas around the periphery, U.S. ability to conduct a limited war in mainland Southeast Asia is the most questionable. …The United States and its allies presently do not have an adequate capability for counter-guerilla type limited military operations."[5] As one staff memo put it, "Rather than major limwar a la Korea we must be most prepared to fight on the order of Laos, Vietnam, Congo, or Lebanon."[6] McNamara himself reiterated this point shortly after he took office as Secretary of Defense, in his February, 1961 budget review. He

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[3] "Major Actions of the President's Science Advisory Committee November 1957—January 1961," Jerome Wiesner to John F. Kennedy, 31 January 1960, "PSAC 1/61 – 3/61" Folder, POF, Box 86, JFKL.

[4] See, for example, John Lewis Gaddis, *Strategies of Containment: A Critical Appraisal of American National Security Policy During the Cold War* (New York: Oxford University Press, 2005).

[5] Department of Defense budget review, January 1961, "DoD General, 1/63" Folder, National Security Files, Box 273, JFKL.

[6] Staff memo, 14 February 1961, "Staff Memoranda Walt W. Rostow Guerilla and Unconventional Warfare 2/1/61-2/16/61" Folder, NSF, Box 325, JFKL.

warned Kennedy that "We have too little ability to deal with guerilla forces, insurrections, [and] subversion" and recommended "substantial increase" in relevant research and development.[7]

Although Alex Roland has argued that the concept of limited war was developed as "a theoretical rationale for the tactical nuclear weapons that were rising from the laboratories of the late 1950s and early 1960s," McNamara and the government scientists in the Kennedy administration defined limited war in terms of its non-nuclear character.[8] Seymour Deitchman, a Defense Department and IDA physicist who wrote a book on limited war dedicated to Kennedy, located the origins of U.S. limited war in policy in Korea, which, in his view, exemplified war with limited scope and objectives. Although Korea had initially prompted a brief reactionary turn toward "massive retaliation," Cold War realities and the election of John F. Kennedy had quickly corrected the course of US policy. Deitchman later described the Kennedy administration as representing "the complete renewal of leadership in the areas of foreign and defense policy," lauding especially its "flexibility" in its actions to "resist non-nuclear aggression."[9]

Thus, from almost the moment Kennedy took office, his science advisors and his Pentagon leadership, united in their skepticism of the previous massive nuclear buildup, pushed

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[7] Budget review, 21 February 1961, "Department of Defense Review of FY61 and FY62 Military Programs and Budgets 2/21/61" Folder, NSF, Box 273, JFKL.

[8] Alex Roland, "Technology, Ground Warfare, and Strategy: The Paradox of American Experience," *Journal of Military History*, Vol. 55, No. 4 (October 1991), 447-468. Some Kennedy-era thinkers shared Roland's view; for example, Herman Kahn distinguished between nuclear and conventional forms of limited warfare (see Herman Kahn, *On Thermonuclear War, 2nd Ed.* (Princeton, NJ: Princeton, 1961), 540-543). For an alternate view, see Seymour Deitchman, *Limited War and American Defense Policy: Building and Using Military Power in a World at War* (Cambridge, MA: MIT Press, 1964). Even skeptics of tactical nuclear weapons such as Robert Osgood acknowledged in 1957 the possibility of their inclusion in a limited war arsenal, but by the late 1970s Osgood had defined limited war as largely non-nuclear, a conflict "fought for ends far short of the complete subordination of one state's will to another's, using means that involve far less than the total military resources of the belligerents and leave the civilian life and the armed forces of the belligerents largely intact" (see Robert E. Osgood, *Limited War: The Challenge to American Strategy* (Chicago: University of Chicago Press,1957), 248-249 and Robert E. Osgood, *Limited War Revisited* (Boulder, CO: Westview Press, 1979), 3-5.)

[9] Deitchman, 3-4.

for renewed emphasis on research and development for conventional weapons appropriate for limited warfare, with an emphasis on counterinsurgency. And they found receptive audiences among Kennedy and his top military brass. Faced with cutbacks in other areas, the Joint Chiefs "unanimously favored" the Defense Department's proposed increase of $100 million for 'limwar' research and development. They welcomed additional funding packets to the three service branches for work on "development of CB/BW" and "weapons for guerilla warfare" (Army); "new assault helicopters" and "improvements to existing ordnance, biological and chemical weapons, and fuzes" (Navy); and "development of sensors for use against small tactical targets under all-weather conditions, low level attack capabilities, [and] improved anti-personnel weapons and improved fuzes" (Air Force). In March 1961, the Army's Chief of Research and Development ordered the rapid intensification and expansion of research related to special warfare, including guerilla combat, and the Army added a limited war laboratory at its Aberdeen Proving Ground in Maryland, staffed largely with civilian engineers, with approval for biological and chemical research, including defoliant systems.[10] The Navy similarly set up a "Sub-Limited Warfare Research Project" at its testing facilities in China Lake, California, and the Air Force's Special Air Warfare Center at Eglin AFB in Florida led the service's counterinsurgency-related research into new aircraft design, munitions including napalm and white phosphorus, and chemical defoliants.

Kennedy, in turn, continued to encourage the work of his PSAC on limited war problems. As tensions with the Soviet Union escalated over crises in Cuba, Laos, and Vietnam, Kennedy

[10] Undated document, "Questions Concerning Counter-Guerilla Programs," in "DoD (B) Subjects Special Warfare 2/61-5/61" Folder, NSF, Box 279, JFKL; JCS report: "Development Status of Military Counterinsurgency Programs, Including Counterguerilla Forces," "DoD (B) Status of Military Counterinsurgency Programs 9/18/63" Folder, NSF, Box 280, JFKL; Luther J. Carter, "Vietnam: Jungle Conflict Poses New R&D Problems," *Science* 152 (8 April 1966), 189.

created a "Special Group" devoted to counterinsurgency (which itself oversaw a special R&D committee), and by 1963 had established a Committee of Principals on Chemical and Biological Weapons. Members of Kennedy's cabinet encouraged his enthusiasm through their own gee-whiz attitude toward clever new technological possibilities. Walt Rostow, for example, cheerfully referred to the anti-guerilla "special gadgets" being developed by the Army as "fun and games."[11] Perhaps most importantly, in June of 1961, the Defense Department's ARPA launched Project AGILE, an R&D program devoted to non-nuclear "remote area conflict" in coordination with defense contractors like Raytheon and Sperry.[12] The Pentagon had quintupled ARPA's budget for research into counterintelligence, surveillance, and psychological warfare.[13] Research topics now included improved communications for use by "friendly indigenous forces," new "tactical helicopter" technology, "incendiary weapons," and lightweight rifles. Under ARPA auspices, some of the Vietnam War's most controversial weaponry, including defoliants and napalm, would be refined and prepared.

Where did scientists fit in this new world of limited war preparation and shifting military research priorities? At the top advisory levels, they stood exactly where they had already been. Much of the roster of Eisenhower's original PSAC remained intact; Wiesner, York, Bronk, Purcell, Fisk, Kistiakowsky, Zacharias, Land, Killian, and Rabi had all been serving continuously since 1957. By and large, they were patriotic men anxious to continue their

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[11] Memo, Rostow to JFK, 23 February 1961, "Staff Memoranda, Walt W. Rostow, Guerilla and Unconventional Warfare, 2/17/61-2/28/61" Folder, NSF, Box 325, JFKL.

[12] JCS Report, "Development Status of Military Counterinsurgency Programs, Including Counterguerilla Forces," "DoD (B) Status of Military Counterinsurgency Programs 9/18/63" Folder, NSF, Box 280, JFKL.

[13] Ibid.

government service, pleased by McNamara's defense budget review, with its lack of emphasis of nuclear weaponry, and sympathetic to Kennedy's anticommunism.

The same was true of scientists at large. In the armed services and at the Pentagon, top brass could reach out to the scientists already employed within the network of universities, private industry, and non-profit organizations created by Eisenhower-era defense contracts. In a letter to Kennedy in April, 1961, Wiesner extolled the virtues of these networks, lauding the contributions of government-sponsored university labs at MIT, Columbia, and the University of California, which contributed to defense research and development, as well as non-profit groups like IDA and RAND, which offered operations analysis. Wiesner assured Kennedy that this type of contracting was the best "means of getting highly skilled and critically needed assistance which could not otherwise be obtained."[14]

Within the next five years, the networks described by Weisner would be tapped in pursuit of strategies and technologies related not just to limited war generally, but to specific military needs in southeast Asia. The war in Vietnam would thus draw on the entire range of scientists who had offered their services in the aftermath of Sputnik: PSAC members, lab personnel, industrial researchers, and academics working as part-time consultants with IDA, JASON, Rand, ARPA, and the military services themselves. For many of these scientists, what began as support for non-nuclear weapons alternatives and peaceful space research soon meant implication in a long, bloody, and massively unpopular war.

A parallel group existed on the fringes of these networks: a small number of academic scientists who considered themselves outsider voices. In 1960, members of the Society for Social

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[14] Memo, Wiesner to JFK, 13 April 1961, "PSAC 1/61-3/61" Folder, POF, Box 86, JFKL.

Responsibility in Science hosted an open forum on chemical and biological weapons at Harvard's Sanders Theater. Some of the scientists in attendance belonged to the Boston Area Faculty Group on Public Issues (BAFGPI). BAGFPI boasted over 90 members in 1962, mostly professors at New England universities, including the radical Nobel-winning MIT biologist Salvador Luria, Harvard biologists John Edsall and Matthew Meselson, linguist Noam Chomsky, physicist Bruno Rossi, and others. BAFGPI had been created through the circulation of an open letter to President Kennedy on civil defense, and saw itself as an "informal organization… limited to members of the academic community" who were protected by academic freedom and united by arms control convictions. Members focused on foreign and nuclear policy, including nonproliferation, civil defense, and the budding conflict in Vietnam.[15] Over the next two decades, BAFGPI members would become some of the most vociferous critics of the war in Vietnam, influencing military policy and heightening public awareness of the range of weapons and technologies deployed in Vietnam, particularly the use of tear gases and the dioxin-laced chemical defoliant Agent Orange.

ARPA, the CDTC, and Defoliants

In the early years of the Kennedy Administration, ARPA took the lead in limited war research. To work on the problem of guerilla warfare, ARPA created several new fulltime positions for "generalist" scientists and organized a twenty-member advisory team, headed by Lloyd Berkner and Luis Alvarez, to report to ARPA and PSAC. In August 1961, Robert Johnson

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[15] BAGFPI letter, 1962, "Boston Area Faculty Group on Public Issues #1" Folder, Box Series IIa, Subject Files An-Ce, Salvador E. Luria Papers, American Philosophical Society. On academic freedom: "Independence is afforded to us by the tradition of academic freedom; it is our professional habit to evaluate evidence critically and to submit our conclusions to the free scrutiny of others." The same folder contains very interesting correspondence, including a 1962 letter from Gar Alperovitz, then an aide to Rep. Robert Kastenmeier of WI, analyzing the problematic politics of US involvement in Vietnam.

described the ease with which ARPA could reach out to the corporate-academic-military networks—now "captive" resources—that had been expanded during the Eisenhower administration. Johnson wrote to Walt Rostow:

> Letters have been sent to all of the "captive corporations" and to the universities that might help to ask them to nominate an individual to serve as the point of contact between ARPA and their institution in this problem area [guerilla warfare]. As specific research problems then arise, an appropriate institution will be asked to make available a particular scientist to work on it. The scientists would be sent to the field to study the problem and then to come back and work out a solution.

As IDA and the Jasons had already discovered, hypothetical planning for guerilla warfare lacked the intensity, intellectual challenge, and excitement that had drawn scientists to the Manhattan Project. Johnson initially hoped that the participation and encouragement of distinguished PSAC members would help "to convince physicists and others that these problems were as important as the problem of developing nuclear weapons on which they had worked during the war."[16]

Soon enough, U.S. foreign policy would catch up with Johnson's efforts, providing the urgency needed to raise the stakes of limited war research. Research and development policy went hand-in-hand with U.S. military needs. Although Vietnam was hardly a foreign policy priority during the early Cuba-oriented years of the Kennedy Administration, the continued presence of U.S. advisors and other personnel, combined with encouragement from the Diem regime, positioned the region as a key site for weapons experimentation. As early as April 1961, Walt Rostow had encouraged Kennedy to send "a military hardware research and development

[16] Memo, Robert Johnson to Walt Rostow, 5 August 1961, Folder: "Vietnam, General, 8/61" Folder, NSF, Box 194, JFKL.

team to Vietnam."[17] A month later, the Department of Defense prepared a "Concept of Action," offering contingency planning for the inevitable moment Kennedy decided "conditions in Vietnam are critical." The document provided administrative suggestions for the formation of a Vietnam task force, stated general U.S. policy ("pacification," "stabilization," and "unification") and recommended that the U.S. loosen its strict interpretation of the Geneva agreements. The authors also pushed expanded weapons-related research, urging leaders to "concentrate U.S. military research and development to develop better military equipment for use in resolving insurgency problems in Vietnam. *The area should be treated as a laboratory and proving ground, as far as this is politically feasible.*"[18]

Implementation of this final recommendation took shape almost immediately. Plans for testing the Army's Mohawk aircraft in the "actual combat environment" of South Vietnam were put in place in the spring of 1961, while a "Limited War RDT&E Task Group" organized a visit to South Vietnam in July.[19] By the spring of 1963, the Joint Chiefs had ordered the creation of a Joint Operations Evaluation Group "to test tactical concepts and doctrine" on the ground in South Vietnam.[20]

Most notably, ARPA's Project Agile included the establishment of ARPA-run "Combat Development and Test Centers" (CDTC) in South Vietnam and Thailand, tasked with taking on

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[17] William Buckingham, Jr., *Operation Ranch Hand: The Air Force and Herbicides in Southeast Asia, 1961-1971* (Washington D.C.: Office of Air Force History, 1982).

[18] "Vietnam: Concept of Action," 1961, "Department of Defense General 4/61-5/61" Folder, NSF, Box 273, JFKL. Emphasis added.

[19] Memo, Harold Brown to Ed Lansdale, 8 June 1961, and Memo, Lansdale to Walt Rostow, 21 June 1961, Folder: "DoD General 6/61-7/61" Folder, NSF, Box 273, JFKL.

[20] JCS Report, "Development Status of Military Counterinsurgency Programs, Including Counterguerilla Forces," "DoD (B) Status of Military Counterinsurgency Programs 9/18/63" Folder, NSF, Box 280, JFKL.

both the technological as well the ideological challenges of counter-insurgency.[21] By the fall of 1961, the Saigon CDTC, housed initially in the Joint Command headquarters, employed ten full-time civilian and military personnel, as well as fifteen other professional and clerical staffers. As one Pentagon report explained, the centers would "provide a mechanism through which the special talents of the U.S. scientific laboratories and industry may be brought into physical contact with the problems of South East Asia on a continuing basis."[22] The center therefore hosted extended ARPA-funded visits from civilian scientists and other technical experts on loan from industry, to conduct research on the problems of guerilla warfare and "infiltration in remote areas."[23] Projects included night vision technology, lie detectors, and the testing of new lightweight weaponry, including the Armalite AR-15 rifle, a model deemed more appropriate for the "smaller stature and body configuration" of ARVN soldiers.[24] With the goal of preventing NVA passage into South Vietnam, the lab workers developed chemical markers, scents detectable to dogs and other "labeling agents" designed to identify interlopers, as well as acoustic and magnetic sensors triggered by clandestine movement. ARPA also arranged, with encouragement from the Diem regime, preliminary defoliant research, for the purposes of clearing jungle canopy shrouding routes into South Vietnam. A MAAG status report from 1961 predicted the CDTC would become "one of our most important agencies for determining and

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[21] Memo, William Yarborough to W.B. Rosson, 2 May 1962, "DoD(B) Subjects Special Warfare 1962-63" Folder, NSF, Box 279, JFKL; Memo, Lemnitzer to JFK, 28 December 1961, "DoD Joint Chiefs of Staff General, 1961" Folder, NSF, Box 276, JFKL.

[22] Report, "RDT&E Annex, Report on General Taylor's Mission to South Vietnam," 3 November 1961, "Vietnam Report on Taylor Mission—November 1961" Folder, Box 210, Country File, Vietnam, National Security File, LBJ Library.

[23] Report, "First Twelve Month Report of Chief MAAG, Vietnam," Lt. Gen. Lionel MmcGarr to Walt Rostow, 1 September 1961, "Vietnam General, McGarr Information Folder for Rostow, 10/25/61" Folder, NSF, Box 194A, JFKL.

[24] Report, "RDT&E Annex, Report on General Taylor's Mission to South Vietnam," 3 November 1961, "Vietnam Report on Taylor Mission—November 1961" Folder, Box 210, Country File, Vietnam, National Security File, LBJL.

field testing materiel and doctrine peculiar to anti-guerilla warfare."[25] (Eventually, test center control would largely be turned over to ARVN. By 1965, US officials were describing the lab as "the RVNAF counterpart agency to OSD/ARPA."[26])

In 1961, Pentagon visitors to the lab acknowledged that "no sure-fire, absolute, and very few 'secret weapons' are on the immediate horizon," but praised the lab for its imaginative research nonetheless.[27] Indeed, much of the new equipment actually generated by this expanded funding push was rather mundane. A 1963 military listing of completed and distributed counterinsurgency-related items included a "small fire bomb," but also new lightweight poplin uniforms, hammocks, ponchos, jungle boots, tropical hats, nets, audiovisual equipment, Styrofoam boats, and signal flares.[28] Nevertheless, the idea that Vietnam—its countryside and its inhabitants—constituted a lab bench for U.S. weapons scientists would have a dramatic on both the short- and long-term character of the war. The history of defoliant use for jungle-clearing and crop destruction illustrates some of the consequences of this attitude.

Herbicide Development

While the use of external substances to promote or deter plant growth had existed for decades, the key research relevant to the defoliant use in Vietnam began in the mid-1930s, with

[25] Report, "First Twelve Month Report of Chief MAAG, Vietnam," Lt. Gen. Lionel MmcGarr to Walt Rostow, 1 September 1961, "Vietnam General, McGarr Information Folder for Rostow, 10/25/61" Folder, NSF, Box 194A, JFKL.

[26] The reference appears in a sociological study of Vietnam: "Simulmatics: A Socio-Psychological Study of Regional/Popular Forces in Vietnam," Folder One, Box 239, Country File, Vietnam, National Security file, LBJL.

[27] Report, "RDT&E Annex, Report on General Taylor's Mission to South Vietnam," 3 November 1961, "Vietnam Report on Taylor Mission—November 1961" Folder, Box 210, Country File, Vietnam, National Security File, LBJL.

[28] JCS Report, "Development Status of Military Counterinsurgency Programs, Including Counterguerilla Forces," 18 September 1963, "DoD(B) Status of Military Counterinsurgency Programs 9/18/63" Folder, NSF, Box 280, JFKL.

the discovery by academic and industrial scientists that certain organic acids could mimic two key kinds of plant hormones, auxins and ethylenes. In the early 1940s, a young botany graduate student, Arthur Galston, was researching auxins and the physiology of soybean flowers at the University of Illinois. His work focused on ways that a compound called TIBA could help speed the flowering process. In the course of his research, however, Galston noticed that at high concentrations, TIBA could cause abscission: the weakening of cellulose at the juncture of leaf and stem, resulting in the shedding of leaves. Galston finished his degree in 1943 and devoted the following three years to unrelated war research on rubber production. Unbeknownst to him, researchers at Camp Detrick had noticed his graduate school discovery and undertaken a new research program on TIBA, with an eye toward the potential tactical uses of chemical defoliation in the Pacific theater of the war.[29]

Eventually, the Fort Detrick researchers abandoned TIBA and focused instead on two phenoxyacetic acids that worked through similar means: 2,4-dichlorophenoxyacetic acid (abbreviated 2,4-D) and 2,4,5-trichlorophenoxyacetic acid (2,4,5-T). The Department of the Army contracted with the University of Chicago for further research on the acids, and accompanying experimentation at Camp Detrick included aerial test spraying in the Everglades.[30] As Galston later recalled, the sudden abundance of research money to university botanists was "greeted like manna from heaven."[31] Nevertheless, he had been surprised in 1946

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[29] Arthur Galston, "An Accidental Plant Biologist," in *Plant Physiology* 128 (March 2002): 786-787; Galston, Arthur W. Interview by Shirley K. Cohen. Pasadena, California, October 8, 2002. Oral History Project, California Institute of Technology Archives. Retrieved 1 May 2011 from the World Wide Web: http://resolver.caltech.edu/CaltechOH:OH_Galston_A ; Arthur Galston, "Falling Leaves and Ethical Dilemmas: Agent Orange in Vietnam," in A.W. Galston, E.G. Shurr, eds, *New Dimensions in Bioethics* (Boston: Kluwer Academic Publishing, 2001).

[30] David A. Butler, "Connections: The Early History of Scientific and Medical Research on 'Agent Orange,'" *Journal of Law and Policy* 13, 527 (2005).

[31] Galston, "Falling Leaves," 108.

when two Camp Detrick senior scientists visited him at Caltech to inform him that his thesis work had served as a model for current military research on defoliants.[32]

Ultimately, the Army rejected the explicit use of defoliants for crop destruction during World War II, out of concern that it would be perceived as chemical warfare. (German crop destruction—via flooding—was later deemed a war crime at Nuremberg.) Nevertheless, the military branches continued to conduct in-house research on herbicides throughout the 1950s, including the development of the first "tactical herbicide," Agent Purple, during the Korean War.[33] From 1954 to 1964, funding for CBW (chemical and biological warfare) research would rise by 1000%. Camp Detrick would expand into the massive Fort Detrick, occupying over a thousand acres of land, complete with livestock farms and manufacturing facilities.[34] Throughout the 1950s, the US Army Chemical Corps and the Crops Division of the Biological Warfare Laboratories, based at Fort Detrick, undertook "the evaluation of thousands of compounds for herbicidal activity," including 2,4-D, 2,4,5-T, and cacodylic acid, an effective grass and rice killer with a high arsenic content.[35] Other key experimentation sites included Eglin Air Force Base in Florida, Fort Drum in New York, Fort Ritchie in Maryland, and Dugway, Utah.[36]

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[32] Ibid.

[33] Alvin Young, "The History of the US Department of Defense Programs for the Testing, Evaluation, and Storage of Tactical Herbicides," prepared for the Office of the Under Secretary of Defense, US Army Research Office, December 2006, http://www.dod.mil/pubs/foi/reading_room/TacticalHerbicides.pdf (accessed 16 July 2010).

[34] Funding statistic and Detrick description in Milton Leitenberg, "Biological Weapons," *Scientist and Citizen*, August-September 1967: 163.

[35] Alvin Young, "History."

[36] Researchers at the US Army Biological Laboratories at Fort Detrick also received information and assistance from other military and industrial research centers; for example, a 1965 Fort Detrick report on Agent Orange acknowledged assistance from the Dow Chemical Company and Edgewood Arsenal (Army Chemical Center). (See Richard Hensen, "Technical Memorandum 74: Physical Properties of Normal Butyl Esters of 2,4-D, 2,4,5-T, and 'Orange,'" United States Army Biological Laboratories (Fort Detrick), August 1965, in the Alvin L. Young

At the same time, American chemical companies were also hard at work producing commercial herbicides and pesticides. As early as 1948, a Monsanto plant in West Virginia had begun mass production of 2,4,5-T, later a key component of Agent Orange.[37] A recent Pentagon report on the history of military herbicide research and use has stressed that the development of commercial herbicides and weed-killers has followed a separate path from the military development of "tactical herbicides," but the two undeniably share crucial chemical ingredients. When the time came for mass production of the tactical herbicides, chemical companies such as Dow and Monsanto already had the necessary equipment and expertise.

Ranch Hand

In Vietnam, the use of herbicides greatly appealed to military planners determined to halt guerillas who used the cover of foliage to cross from North Vietnam into South Vietnam, or who planned ambushes from behind obscuring tree canopy and roadside vegetation. The Diem regime enthusiastically endorsed the idea, having already sought American technical assistance not only for defoliation but for crop-spraying activities as well. Since much of the available botanic research concerned the effects of chemicals on North American plant life, planners promoted additional defoliation research both at Fort Detrick and on the ground in South Vietnam, hoping to refine concentration calculations and delivery methods for maximal effect in

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Collection on Agent Orange, National Agricultural Library, http://www.nal.usda.gov/speccoll/findaids/agentorange/text/00016.pdf (accessed 16 July 2010); Buckingham.

[37] Donald Barlett and James Steele, "Monsanto's Harvest of Fear," *Vanity Fair* May 2008. The article describes a 1949 explosion at the plant in which, after intensive exposure to the herbicide, 226 employees exhibited symptoms of illness, including chloracne. The same plant produced Agent Orange during the 1960s, and Monsanto later settled several major lawsuits brought by dioxin-exposed former employees who claimed Monsanto had known of the risks of their chemical exposure.

the Vietnamese jungle.[38] In the summer of 1961, the first shipments of defoliants and related supplies arrived in Saigon for testing, and a South Vietnamese helicopter, outfitted with American equipment, flew its first defoliation test in August.[39] Meanwhile, scientists at the Plant Sciences Laboratories at Fort Detrick, mobilized through Project AGILE, undertook new research on jungle defoliation, and began stocking large quantities of 2,4,5-T and cacodylic acid, the chemicals that would soon be adapted into Agent Orange and Agent Blue. In Saigon, CDTC planners, mulling the problem of crop destruction and awaiting additional chemical supplies, proposed using napalm to burn off mature rice crops.[40]

By November of 1961, McNamara and Gilpatric were urging Kennedy to approve an expanded defoliation program, with multiple long-term goals: to clear roadside foliage in order to lessen the risk of ambush; to remove obscuring vegetation in the vicinity of Viet Cong bases and infiltration routes in order to allow better surveillance; and to destroy rice, manioc, corn, and other crops in order to starve Viet Cong into submission. The Defense Department assured the president that the chemicals to be used were "commercially produced in [the United States] and have been used for years in industrial and agricultural plant growth clearing operations," with "no harmful effects on humans, livestock or soil."[41] In fact, as a later Pentagon history reported, the exact composition of the herbicides to be tested had been developed by military scientists and

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[38] Memo, Harold Brown to Lansdale, 22 September 1961, in "Staff Memoranda Walt W. Rostow Guerilla and … Warfare 9/61" Folder, NSF, Box 326A, JFKL.

[39] "Status Report on the Presidential Program for Viet-Nam," 28 July 1961, in "Vietnam, General, Presidential Program Status Reports" Folder, NSF, Countries, Box 195A, JFKL; Buckingham, 11, 26.

[40] Report, "RDT&E Annex, Report on General Taylor's Mission to South Vietnam," 3 November 1961, "Vietnam Report on Taylor Mission—November 1961" Folder, Box 210, Country File, Vietnam, National Security File, LBJ Library; Alvin Young, "The History of the US Department of Defense Programs for the Testing, Evaluation, and Storage of Tactical Herbicides."

[41] Buckingham, chapter 2; "National Security Action Memo 115," November 1961, "NSAM 115 Defoliant Operations in Vietnam" Folder, NSF, Box 332, JFKL.

technicians at Fort Detrick and elsewhere; they were not available commercially, nor were they regulated by the USDA. (Although some of the *components* of the herbicides had been produced commercially, the particular "formulations and concentrations…greatly exceeded how the commercial components of these tactical herbicides…were formulated and used in the United States.") The companies contracted to manufacture them—Dow, Monsanto, Dupont, and others—worked according to military specifications.[42]

But Kennedy's advisors explained only that additional testing would be needed to tailor the existing defoliants to the Vietnamese terrain. U. Alexis Johnson, Deputy Undersecretary of State, urged that experimental defoliant operations be carried out on areas of the Vietnamese jungle, to determine maximal effectiveness. McNamara later explained to Kennedy that herbicides and delivery methods would have to be adapted to the "great variety of vegetation" and weather conditions in Vietnam.[43] The necessary tests would take place over operationally important areas such as ammunition dumps and roadsides, so as to gauge any immediate military advantages. An internal document from the period called for the "experimental defoliation of selected strips through Zone D."[44] Thus, researchers would "test" the effectiveness of the new defoliants on Vietnamese targets by using them on Vietnamese targets.[45]

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[42] This was distinct from the military use of pesticides in Vietnam, for example; those chemicals were subject to approval and regulation by the USDA and other agencies. It was also distinct from the herbicides used on the military bases in Vietnam, which were subject to separate regulatory processes (including USDA regulations) and were not considered "tactical herbicides." See Alvin Young, "The History of the US Department of Defense Programs for the Testing, Evaluation, and Storage of Tactical Herbicides."

[43] Buckingham, 43.

[44] Report, "RDT&E Annex, Report on General Taylor's Mission to South Vietnam," 3 November 1961, "Vietnam Report on Taylor Mission—November 1961" Folder, Box 210, Country File, Vietnam, National Security File, LBJ Library

[45] In April 1962 Gen. Harkins reported to Adm. Felt that "There is need to conduct R&D sprayings with changed spray rates and dosages as recommended by Gen. Delmore and technical group from OSD and *I selected the site based on operational considerations.*" [emphasis added] in Harkins to Felt, April 1962, "Vietnam, General 4/11/62-4/16/62" Folder in NSF, Box 196, JFKL.

With presidential approval secured in January 1962, expanded testing and spraying began, carried out by the Air Force's newly created Operation Ranch Hand. The first chemicals tested were Agents Purple, Pink, Green, and Blue (see table below).[46] As a later Pentagon history would describe it, the effort was "a test program for evaluating tactical herbicides for vegetation control in South Vietnam."[47] It began with American C-123 planes spraying swaths of roadside outside Bien Hoa with Agent Purple, a phenoxyacetic defoliant, and included constant evaluation and additional research by visiting scientists. Overseeing much of the testing at the CDTC was James W. Brown, an Army scientist who had served stints at Fort Detrick and Camp Drum before arriving in Saigon. In Vietnam, Brown was frustrated by the lack of botanic expertise on the local flora. Whereas McNamara demanded limited experimentation devoted solely to obtaining operational results, Brown and his ARPA colleagues often submitted lengthy, technical reports, trying in to fill in gaps in botanical knowledge by detailing growth cycles of local fauna and other key observations.[48]

In the spring and summer of 1962, the Defense Department sent Brigadier General Fred Delmore, head of the US Army Chemical Corps' Research and Development Command, to lead a follow-up team of experts including USDA scientists Warren Shaw and Donald Whittam, ARPA's Levi Burcham, and the Chemical Corps's Charles E.Minarik. This group carefully assessed different types of vegetation, growing seasons, viscosity of defoliants, and effects of droplet size. They devised quantitative ratings systems in keeping with McNamara's known

[46] Alvin Young, "The History of the US Department of Defense Programs for the Testing, Evaluation, and Storage of Tactical Herbicides."

[47] Alvin Young, "The History of the US Department of Defense Programs for the Testing, Evaluation, and Storage of Tactical Herbicides."

[48] Buckingham, Chapter 3.

affinity for statistics, and recommended additional testing and experimentation with additives.[49] While the team expressed doubts about the effectiveness of current spraying technologies, they endorsed crop destruction and emphasized how successfully the herbicides had cleared Vietnamese mangrove swamps.[50] Top advisors seized on these findings: Michael Forrestal, aide to McGeorge Bundy, praised the group's work to Kennedy and enthusiastically promoted extensive mangrove spraying with the assurance that "Mangrove growth has no economic value."[51] The experiences of the visiting scientists illustrate two important aspects of herbicide use in Vietnam: the lack of extensive background research on local ecology and the effects of defoliants before test spraying began, and the tendency of military officials to endorse scientific evidence that bolstered existing plans to increase defoliant use.

While the USDA scientists conducted their research in Vietnam, other Army and Air Force scientists participated in additional defoliant testing at multiple sites at home and abroad. These included over 3,400 acres in western Thailand and 150 acres of Canadian forest in New Brunswick, and areas throughout the southern United States, Hawaii, and Puerto Rico. Fort Detrick researchers conducted tests of Agent Orange and Agent White in Georgia swampland and alongside mountainous stretches of TVA power lines in Tennessee, and oversaw testing on the island of Kauai carried out by the University of Hawaii's Department of Agronomy and Soils. ARPA researchers conducted test sprayings of hundreds of acres leased from private landowners in Texas and oversaw the defoliation of tropical forest areas in Puerto Rico. Additional research on herbicide characteristics and handling procedures occurred at Army and

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[49] Buckingham, 51.

[50] Buckingham, 51, 54.

[51] Memo, Forrestal to JFK, 2 August 1962, in "NSAM 178 Destruction of Mangrove Swamps in South Vietnam" Folder, NSF, Box 338, JFKL.

Air Force labs at Eglin AFB, McLelland AFB, Kelly AFB, Aberdeen and, primarily, Fort Detrick. The work at these test sites and labs focused military attention on the chemicals that would come to dominate aerial spraying in Vietnam: Agents Blue, White, Purple, and Orange.[52]

President Kennedy approved the initial testing program in January 1962, and within the year he authorized additional mangrove spraying efforts, and, after much wariness, crop destruction programs. Early results were mixed, but the rest of the decade saw a massive campaign of chemical spraying, primarily to clear forest canopy and other foliage surrounding communication lines, transportation routes, and base perimeters, but also including, according to the *New York Times*, "50,000 to 75,000 acres" of crop destruction in the spring of 1965.[53] By the end of the war, Ranch Hand pilots had dropped over 50 million kilograms of herbicidal chemicals on over 4.2 million hectares, largely in South Vietnam, but with a small number of missions in Cambodia, Laos, the DMZ, and North Vietnam.[54] Twenty percent of the jungle area in South Vietnam was sprayed, and over a third of its mangroves.[55] In 1965, Agent Orange became the dominant chemical used, and the peak years of spraying occurred between 1967 and

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[52] The southern states were: Alabama, Arkansas, Florida, Georgia, Hawaii, Maryland, and Texas. See Alvin Young, "The History of the US Department of Defense Programs for the Testing, Evaluation, and Storage of Tactical Herbicides." For more information on defoliant testing in Thailand, carried out by the Joint Thai-US Military Research and Develop Center and with results shared with Fort Detrick, see "Vegetation Analysis of the Pran Buri Defoliation Test Area 1," Joint Thai-US Military Research and Development Center, January 1966, in the Alvin L. Young Collection on Agent Orange at the National Agricultural Library, http://www.nal.usda.gov/speccoll/findaids/agentorange/text/00019.pdf (accessed 16 July 2010). In 1974, more than a decade after the MRDC testing at Pran Buri, the National Research Council of the National Academy of Sciences conducted a follow-up study in the same area to test for residual TCDD and 2,4,5-T in the soil, with positive results.

[53] Charles Mohr, "U.S. Spray Planes Destroy Rice in Vietcong Territory," *New York Times*, 21 December 1965, 1.

[54] Arthur Westing, *Herbicides in War: The Long-Term Ecological and Human Consequences* [Stockholm International Peace Research Institute] (Philadelphia: Taylor & Francis, 1984), 5-6; Buckingham, 126.

[55] Richard Kohn, "Foreword," in Buckingham, iii-iv.

1969.[56] As shown in the table below, the chemicals, named for the color-coded identifying stripes of their supply drums, included phenoxyacetic acids and arsenical compounds that mimicked plant hormones to disrupt growth (Agents Orange, Green, Pink, Purple) and desiccants made from cacodylic acid (Agent Blue). The 11 million gallons of Agent Orange, accounting for roughly 60% of all the chemicals sprayed, were produced primarily by six Pentagon-contracted companies: Diamond Shamrock, Dow Chemical, Hercules, Monsanto, North American Phillips, and Northwest Industries.

Types of Defoliants

Type	**"Agent" Color**	**Chemical Description**	**Mechanism**	**Use**	**Years in Use**
Phenoxyacetic	Orange I and II	Orange I: 50:50 mix of n-butyl esters of 2,4-D and 2,4,5-T Orange II: 50:50 mix of n-butyl ester of 2,4-D and isooctyl ester of 2,4,5-T	Compounds mimic plant hormones to disrupt plant metabolism	General defoliation of forest cover and crops	Orange I: 1965-70 Orange II: 1967-68
	Pink, Green	esters of 2,4,5-T			Pink: 1962-64 Green: 1962
	Purple	n-butyl esters of 2,4-D and 2,4,5-T, plus isobutyl esters of 2,4,5-T			1962-65
Cacodylic	Blue	Cacodylic acid—mixture of Na dimethyl arsenate and dimethyl arsenic acid (54.29% arsenic)	Compound disrupts plant moisture retention to cause desiccation	Rice and grasses	1961 (in powdered form), 1966-71 (in liquid form)

[56] Agent Orange superseded Agent Purple beginning in "late 1964"—see Buckingham, 122. Buckingham estimates that over 1.6 million acres were sprayed in 1967 alone (Buckingham, 129); SIPRI estimates that "The three peak years of herbicide spraying—1967-1969—were about equal in magnitude and together accounted for over three-quarters of the volume of total wartime expenditures." See Westing, ed., *Herbicides in War,* 5.

Picloram/Tordon	White	Triisopropanolamine salts of 2,4-D and 4-amino-3,5,6-trichloropicolinic acid (picloram,Tordon)	Compounds mimic plant hormones to disrupt plant metabolism	Persistent defoliation; used in forests and around Saigon	1960-1970

Chart adapted from J.B. Neilands, "Vietnam: Progress of the Chemical War," *Asian Survey* 10, No. 3 (March, 1970), 221; Arthur Westing, *Herbicides in War: The Long-Term Ecological and Human Consequences* [Stockholm International Peace Research Institute] (Philadelphila: Taylor & Francis, 1984); Alvin Young, "The History of the US Department of Defense Programs for the Testing, Evaluation, and Storage of Tactical Herbicides," prepared for the Office of the Under Secretary of Defense, US Army Research Office, December 2006, http://www.dod.mil/pubs/foi/reading_room/TacticalHerbicides.pdf (accessed 16 July 2010).

Debating the Chemicals

The decision to use defoliants in Vietnam did not occur without considerable debate. Public concern ran parallel to the physical increase in defoliation operations, particularly later in the decade as reports of ecological destruction in Vietnam coincided and contributed to the growing environmental movement in the United States. But in the early 1960s, debates about the risks and merits of herbicide use occurred largely behind closed doors in Washington, and mostly concerned evaluations of the efficacy of the chemicals involved and the potential propaganda risks associated with defoliation. Sensitive to world opinion, military planners acted with caution: the first shipments of chemicals arrived in unmarked drums, and early testing plans called for disguising U.S. planes with Vietnamese insignias.[57]

Perhaps of most concern was the risk that crop destruction would draw accusations of chemical warfare. In the early days of defoliant decision-making, Kennedy's advisors warned him of the risks of using herbicides, particularly for crop destruction. As Gilpatric acknowledged to Kennedy in 1961, "the use of chemicals to destroy food supplies is perhaps the worst application in the eyes of the world."[58] Walt Rostow wrote a telling memo to Kennedy in November 1961, explaining that a presidential authorization was needed for chemical crop

[57] Buckingham, 26-28.

[58] Memo, Gilpatric to JFK, 23 November 1961, in "NSAM 115 Defoliant Operations in Vietnam" Folder, NSF, Box 332, JFKL.

destruction because “this is a kind of chemical warfare.”[59] Others in the president’s inner circle assured him that such action did not constitute chemical warfare, however, nor did it violate international law—the British, they argued, had used similar tactics in Malaya.[60] Kennedy was skeptical.

The definition of chemical warfare was a tricky thing. Language banning “asphyxiating” and “poisonous” gases had been inserted into the Treaty of Versailles in the aftermath of the deadly use of mustard and chlorine gas during World War I, but applied only to the defeated country of Germany. In 1925, the United States had signed but not ratified the Geneva Protocol banning the first-use of chemical and biological weapons. The protocol prohibited “the use in war of asphyxiating, poisonous or other gases, and of all analogous liquids, materials or devices” as well as “bacteriological methods of warfare.” This left a number of questions unanswered: Did “asphyxiating” gases include tear gas, as the British had argued? What was meant by “other gases”? Where did chemical defoliants and incendiaries fit?[61]

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[59] “Viet-Nam Status Report,” Rostow to JFK, 21 November 1961, in “Vietnam, General 11/18/61-11/20/61” Folder, NSF, Box 195, JFKL.

[60] Memo, Rusk to JFK, 24 November 1961, in “Vietnam, General, Memos and Reports 11/17/61 – 11/30/61” Folder, NSF, Box 195, JFKL. Beginning in the mid-1950s, British military forces in Malaya engaged in aerial spraying of a 2,4,5-T and 2,4-D-containing compound, for the purposes of defoliating communications lines and crop destruction (See Westing, ed.,*Herbicides in War*, 4).

[61] Matthew Meselson has attributed the failure of the U.S. Senate to ratify the Geneva Protocol to organized lobbying by “the American Chemical Society, the Army Chemical Corps, the American Legion, and parts of the chemical industry.” See Matthew Meselson, “Controlling Biological and Chemical Weapons,” in Jonathan Allen, ed., *March 4: Scientists, Students, and Society* Cambridge (MIT Press, 1970), 151-160. After the ACS officially endorsed the Protocol in 1970, reversing its 1925 stance, William Bailey of the ACS’s Committee on Chemistry and Public Affairs explained that the reversal was due only “partially” to evolving views of the “humaneness” of chemical weapons. Rather, it was due to evolving practices of war itself: During World War I, “chemical warfare was restricted to a narrow battle zone by technological limitations and affected virtually only the combatants.” The same could not be said for chemical warfare in 1974, when Bailey was speaking. See William J. Bailey, “Introductory Remarks,” in *Chemical Weapons and U.S. Policy: A Report of the Committee on Chemistry and Public Affairs* (Washington, DC: American Chemical Society, 1977), Othmer Library, Chemical Heritage Foundation.

No answers were forthcoming. During World War II, the United States had refrained from defoliant use, but had dropped napalm, an incendiary considered a chemical weapon by some critics, during firebombing campaigns in the Japanese theater.[62] Roosevelt had specifically enunciated a no first use policy for chemical and biological weapons, but had also authorized CBW research programs headquartered at Camp Detrick. No clear delineations regarding non-lethal chemicals had been set. As critics would later point out, postwar Army manuals informed soldiers that "The United States is not a party to any treaty, now in force, that prohibits or restricts the use in warfare of toxic or nontoxic gases, of smoke or incendiary materials, or of bacteriological warfare."[63] In the mid-1950s, the Army Chemical Corps was expanded and reorganized to accommodate expanded CBW research programs. Although Roosevelt's no first use policy had never been publicly revised, a 1959 Congressional resolution reiterating the restriction was soundly defeated, in the face of vehement opposition from the State Department and the Department of Defense.

Top decision-makers in 1961 drew on this complicated history in their debates over crop destruction, but while they argued, the practice was already underway, through the policy of "testing" on targets and the provision of chemicals to the South Vietnamese military. A State Department memo from the spring of 1962, before approval for the full-scale crop destruction program had been secured, noted that "Results of the few crop destruction experiments reported to ARPA not conclusive." The memo further instructed officials in Saigon that should the program be approved, chemicals should be supplied to the South Vietnamese on a "covert basis"

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[62] The U.S. military refrained from the use of defoliants and gas (to clear underground tunnels) in the Pacific theater, though there is some speculation that chemical crop destruction might have occurred had the Japanese not surrendered after Hiroshima and Nagasaki. Incendiary gels were use in firebombing. A more detailed account of decision-making regarding CBW during World War II is beyond the scope of this dissertation.

[63] Quoted in Victor Sidel and Robert Goldwyn, "Chemical and Biologic Weapons—A Primer," *New England Journal of Medicine* 274 (6 January 1966): 21-27.

and efforts should be made to "disassociate US publicly with actual operations which would be conducted solely by GVN."[64] Some reports have suggested that the first crop destruction operations began even earlier, in the summer and fall of 1961, when the first defoliant shipments arrived in Vietnam, but these early efforts likely consisted of ARVN troops hand-spraying crops and dropping the chemicals from their own helicopters, without the use of U.S.-provided aerial spraying technologies.[65]

In the meantime, early non-crop defoliation was attracting significant anti-American press, both in Vietnam and throughout the Soviet bloc.[66] In a way, the attention was liberating. As McNamara wrote to JFK shortly after one such media barrage, "I am inclined to believe that the propaganda impact has now been made and that we can use herbicides without causing a serious new international incident."[67] This rationale, combined with input from the ARPA scientists confirming the efficacy of the chemicals on mangroves, underlay official approval for widespread mangrove spraying. Kennedy's "hold-off order" on aerial crop destruction, however, remained in effect through the summer of 1962.[68] Kennedy worried that enemy crops would be indistinguishable from friendly crops, and that the negative propaganda regarding "food warfare"

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[64] State Department memo, 30 April 1962, in"Vietnam General, 4/17/62-4/30/62" Folder, NSF, Box 196, JFKL.

[65] These reports are cited in Seymour Hersh, "Our Chemical War, *New York Review of Books,* 25 April 1968. Buckingham claims the first SVN helicopter spraying occurred on August 10, 1961, and the following month Diem formally requested U.S. assistance for crop destruction, a request Kennedy did not approve. Buckingham also argues that Army research on defoliants had long been focused on crop destruction, such that Army technical advisors in Vietnam already had some background in this area. In any case, non-chemical crop destruction was already underway, through SVN practices of "pulling, cutting, burning, strafing or dropping napalm." (Buckingham, Chapter 5).

[66] Various claims that unnamed scientists and Red Cross officials have "condemned the criminal acts of the US imperialists…" are cited in US Army press reprints, April 1963, in "Folder: Vietnam General 4/1/63-4/18/63" Folder, NSF, Box 197A, JFKL.

[67] Memo, McNamara to JFK, undated, in "NSAM 178 Destruction of Mangrove Swamps in South Vietnam" Folder, NSF, Box 338, JFKL.

[68] Memos from JFK, in "Vietnam, General 7/20/62-7/30/62" Folder, NSF, Box 196A, JFKL.

would be extreme.[69] By the fall, State Department frustration was mounting. Frederick Nolting, the U.S. ambassador to South Vietnam, reported with annoyance that the optimal testing window for crop destruction during the growing season was closing, and that

> Washington should consider seriously giving us authority to work out with GVN another target area for test and evaluation purposes where possibility effective results could be maximized. Without carrying out such test operation careful preparation for which will take some time, we will never, really be able determine whether crop destruction can be effective weapons against VC without involving serious disadvantages to our side. Feel constrained point out that matter of crop destruction has been under consideration for long time, with GVN still waiting for decision from us. However we will be able soon inform them at least of decision proceed with test operation of spraying from air.[70]

Nolting's words typified the language of testing and experimentation constantly invoked by US advisors in Vietnam.

Eventually reassured by Vietnamese officials that propaganda risks could be minimized and targets chosen responsibly, Kennedy finally authorized chemical crop destruction in South Vietnam in October 1962, to be approved on a case-by-case basis, based on initiating requests from Saigon.[71] Spraying began the following month, with South Vietnamese helicopters dropping U.S.-supplied chemicals over 750 acres of rice, beans, and manioc in Phuoc Long

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[69] Embassy telegram, 25 September 1962, in "Vietnam, General 9/22/62-9/29/62" Folder, NSF, Box 196A, JFKL.

[70] Telegram, Nolting to State Department, 1 September 1961, in "Vietnam General, 9/1/62-9/14/62" Folder, NSF, Box 196A, JFKL.

[71] State Department telegram, 26 September 1962, in "Vietnam, General 9/22/62-9/29/62" Folder, NSF, Box 196A, JFKL; Memo for the Record, 2 October 1962, in "Vietnam General 10/1/62-10/6/62" Folder, NSF, Box 197, JFKL; State Department airgram, 12 August 1965, "Vietnam Memos (A) vol. XXXVIII 8/1-12/65 [1 of 2]" Folder, Box 21, Country File, Vietnam, National Security File, LBJL.

Province, followed soon after by targets in Thia Thien Province.[72] American advisors were not allowed on board the helicopters during these initial spraying operations.[73]

In February of 1963, the first press reports of Ranch Hand crop destruction missions appeared.[74] That spring, a UPI reporter based in Saigon and a correspondent for the *Minneapolis Tribune* provided corroborating accounts, describing the use of crop-killing defoliants in the central highlands.[75] Kennedy began to rethink his decision. After a meeting with British Ambassador David Ormsby Gore, during which the British officials present doubted the effectiveness of the defoliants and warned that Asians had a historical aversion to unknown chemicals, the president requested a review of all defoliation and crop destruction programs.[76]

Meanwhile, Kennedy faced contradictory pressures resulting from the new publicity.[77] What McNamara had initially considered liberating now had the effect of hardening the American commitment to chemical herbicides. As one official wrote in April 1963, even though "defoliation is at best only partially effective militarily," stopping the use of herbicides now "would tend to confirm Bloc charges and invite further such campaigns because of their proven effectiveness against us."[78] The issue, as was the case in so many other instances of Vietnam decision-making, was credibility. To Kennedy's advisors, a retreat in defoliation policy constituted a retreat in the war at large.

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[72] State Department telegram, 15 March 1963, in "Vietnam, General 3/1/63-3/19/63" Folder, NSF, Box 197, JFKL.

[73] Nolting memo #547, 26 November 1962, in Folder: "Vietnam General 11/26/62-11/30/62" Folder, NSF, Box 197, JFKL; see also Buckingham, 79-80.

[74] Buckingham, 81-82.

[75] Hersh, "Our Chemical War."

[76] Memo of Conversation, 4 April 1963, in "Vietnam General 4/1/63-4/18/63" Folder, NSF, Box 197A, JFKL.

[77] Memo, 4 April 1963, in "Vietnam General 4/1/63-4/18/63" Folder, NSF, Box 197A, JFKL.

[78] Memo, April 1963, in "Vietnam General 4/1/63-4/18/63" Folder, NSF, Box 197A, JFKL.

But increasingly, the “charges” were coming not from the Soviet bloc, but from Western scientists and political activists. In Congress, Senate Majority Leader Mike Mansfield and Wisconsin Representative Robert Kastenmeier worried publicly about the reported crop destruction.[79] In the *New York Times,* Bertrand Russell characterized the use of chemicals in Vietnam as an “atrocity” of “chemical warfare.”[80] In March of 1963, the *New Republic* published “One Man’s Meat,” an article damning American deployment of chemicals in Vietnam. The magazine noted that herbicides marketed in the United States contained detailed warning labels conveying their toxicity and the risks of human exposure, suggesting that their use in war violated the 1925 Geneva Protocol. The article also offered a new, influential analysis of the use of herbicides as weapons: it was both the ends and the means that mattered. In other words, defending defoliants on the grounds that they were simply common chemicals publicly available in the United States, as the *New York Times* had in response to Bertrand Russell, was insufficient. It was the manner in which they were used, in heavy concentrations and sprayed over large swaths of land, and the end effects of that use—crop destruction, widespread human exposure—that elevated them to the status of chemical weapon.[81]

The *New Republic* article was also emblematic of a nascent public awareness of the ecological risks associated with chemical herbicides and pesticides. In 1962, Rachel Carson’s bestseller *Silent Spring* offered a devastating accounting of the risks of DDT use. Carson’s form of environmentalism, rooted in an awareness of the delicate balances required for the survival of complex ecosystems and drawing on analyses of new chemicals and their potential damage to

[79] Buckingham, 82.

[80] *New York Times,* 9 April 1963.

[81] “One Man’s Meat,” in “The Week,” *New Republic* 148, No. 12 (23 March 1963), 3-7.

plants and animals, found a receptive audience in a nation primed by civil defense exercises, fears of nuclear fallout, and, increasingly, direct experience of smog and pollution.[82] In time, the ecological costs of the war in Vietnam would be a powerful link bridging the environmental and antiwar movements of the late 1960s, but in 1963 the public outcry was small and easily ignored; the efforts of Bertrand Russell and the *New Republic* had little discernible effect on policy. Kennedy's requested task force conducted its review, and existing crop destruction policies were reaffirmed in October 1963.[83] The following year, the Air Force's Ranch Hand program was officially made permanent, and what had been an experimental unit with "temporary duty status" became a fixture of the American presence in South Vietnam.[84]

From Kennedy to Johnson

Lyndon Johnson's accession to the presidency after Kennedy's assassination saw continuity and expansion of existing commitments to counterinsurgency and non-nuclear weapons technology. As McNamara reminded the Democratic Platform Committee in August, 1964, the new president had declared that "the United States is, and will remain, first in the use of science and technology for the protection of its people." McNamara emphasized that in the mid-1960s, overwhelming nuclear might was no longer "enough," and new weapons technologies were needed:

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[82] Adam Rome, "'Give Earth a Chance': The Environmental Movement and the Sixties," *Journal of American History* (1 September 2003); Ralph Lutts, "Chemical Fallout: Rachel Carson's *Silent Spring*, Radioactive Fallout, and the Environmental Movement," *Environmental Review* 9, No. 3 (Autumn 1985), 210-225. Lutts writes of the publication of *Silent Spring*: "The nation was steeped in years of debate about nuclear weapons and fallout which served as a point of reference to help people understand the hazards of pesticides and as a fearful symbol to motivate action."

[83] State Department airgram, 12 August 1965, "Vietnam Memos (A) vol. XXXVIII 8/1-12/65 [1 of 2]" Folder, Box 21, Country File, Vietnam, NSF, LBJL.

[84] Buckingham, 99.

> The effectiveness of the strategic nuclear deterrent we have assembled against our enemies has driven them to acts of political and military aggression at the lower end of the spectrum of conflict. The Communists now seek to test our capacity, our patience, and our will to resist at the lower end of this spectrum by crawling under the nuclear defenses of the free world. … in the twilight zone of guerilla terrorism and subversion.[85]

McNamara's views continued to be popular with many scientists, and the presidential election of 1964 further cemented the support among scientists for the Johnson administration. Chapters of groups such as "Engineers and Physicians for Johnson and Humphrey" and "Scientists and Engineers for Johnson" proliferated, spurred by hopes for arms control and fears of the extreme hawkishness of Johnson's challenger, the Arizona conservative Barry Goldwater. George Kistiakowsky, Herbert York, and Jerome Wiesner were all members. Donald Hornig, the Los Alamos veteran and Princeton chemist chosen as Johnson's science advisor, spoke to one such group in South Carolina, two weeks before the national election. "Although traditionally scientists and engineers don't take an active role in political campaigns," he observed, the high stakes of the election had brought greater political involvement. As Hornig put it pointedly, "Many [scientists and engineers] have been involved in the development of military power which could, if improperly used, destroy mankind. They want to assume that, if possible, it not be used and that the responsibility for its use be exercised at the highest level of the government—in fact, by the President and by a responsible President. …"[86]

As promised, after his election President Johnson followed through on the commitment to emphasize limited war weapons and technologies. Pentagon cost-cutting efforts from the early 1960s, including efforts to shift from cost-plus contracting to competitive fixed-price contract,

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[85] Statement before the Democratic Platform Committee, Robert McNamara, 17 August 1964, "DoD 11/63 vol I, 1 of 2" Folder, Box 11, Agency File, NSF, LBJL.

[86] Speech to the Engineers and Physicians for Johnson and Humphrey, Donald Hornig, 23 October 1964, "Addresses and Remarks by Donald Hornig, 1964" Folder, Box 8, Papers of Donald Hornig, LBJ Library.

were matched by an 800% increase in counterinsurgency programs.[87] In Vietnam, Westmoreland oversaw the creation of the Joint Research and Test Activity (JRATA) to coordinate the work of the three services and ARPA. By 1966, JRATA's 150-person staff included thirty "civilian engineers and scientists."[88]

Crop destruction operations likewise expanded after 1964, into more populated VC-controlled areas in central Vietnam. Beginning in December 1965, aerial defoliation also extended across the border into Laos, and included a small number of unpublicized crop destruction missions.[89] In 1965, State Department officials estimated that 9,310 hectares (roughly 23,000 acres) of cropland had been destroyed, representing 54 million pounds of food. The results seemed to justify the high social costs; although planners privately dismissed a RAND study warning of the adverse effects of spraying on popular attitudes, they nevertheless continued their efforts to dissociate the U.S. publicly from crop destruction operations. Operation "Farmgate" employed mixed U.S.-Vietnamese crews piloting Air Force C-123 planes deliberately sporting only "VNAF markings."[90] US officials reported in August that after a MACV defoliation mission outside Bien Hoa resulted in damage to the trees and vegetables of

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[87] Statement before the Democratic Platform Committee, Robert McNamara, 17 August 1964, "DoD 11/63 vol I, 1 of 2" Folder, Box 11, Agency File, NSF, LBJL; "Cost Reduction Program" pamphlet, Department of Defense, "DoD 11/63 vol I, 1 of 2" Folder, Box 11, Agency File, NSF, JFKL. Cost-plus contracts guarantee payment for all project costs plus profit minimums.

[88] Luther J. Carter, "Vietnam: Jungle Conflict Poses New R&D Problems," *Science* 152 (8 April 1966), 187.

[89] Buckingham, 117-119.

[90] State Department telegram, 5 August 1965, "Vietnam Cables vol. XXXVIII 8/1-12/65 [1 of 2]" Folder, Box 21, Country File, Vietnam, NSF, LBJL; State Department airgram, 12 August 1965, "Vietnam Cables vol. XXXVIII 8/1-12/65 [1 of 2]" Folder, Box 21, Country File, Vietnam, NSF, LBJL; Buckingham, 101.

two neighboring villages, "Intensive psywar is being conducted" and "prompt compensation" pursued in order to minimize backlash.[91]

Despite these efforts, public criticism, sporadic in the early 1960s, grew increasingly widespread by the middle of the decade. In reaction to reports that the strategic hamlet of Cha La had been accidentally defoliated, the *Washington Post* ran an op-ed that referred to defoliants as "chemical weapons" that were "totally unsuited" for the war in Vietnam.[92] A pair of *New York Times* articles by Charles Mohr, published in December 1965, also proved influential. Mohr estimated that the US military was dropping "enough chemicals to cover 20,000 acres a month," and described the progress of defoliation in evocative language: "Within three days after a single spraying with the non-poisonous weed killer, the effects are noticeable. Within a week there is an 'autumn' look. But three months must pass before the 'winter in Vermont' effect is achieved. Four months after that, however, the foliage begins to grow back."[93] Mohr also reported that although "Air Force officers say they are forbidden to discuss it," herbicidal operations in Vietnam included deliberate crop destruction. Mohr described the spraying of "small areas of major military importance where the guerillas grow their own food," with an effectiveness rate of 60-90% if the chemicals were applied during the growing season. He also

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[91] State Department telegram,13 August 1965, "Vietnam Cables vol. XXXIX 8/13-31/65" Folder, Box 21, Country File, Vietnam, NSF, LBJL. Seymour Hersh argued in 1968, based on internal documents and conversations with sources, that at the key moments of decision-making, the State Department tended to take a more cautious stance than the Pentagon, in regards to both the use of defoliants and tear gases. Hersh describes the 'bitter' opposition by Averill Harriman to Pentagon defoliation testing in Thailand, for example. (See Hersh, "Our Chemical War.")

[92] Quoted in Buckingham, 94.

[93] Charles Mohr, "U.S. Spray Planes Destroy Rice in Vietcong Territory," *New York Times,* 21 December 1965, and "Defoliation Unit Lives Perilously," *New York Times*, 20 December 1965. Mohr himself was a key figure in the journalistic refutation of many administration claims during the Vietnam War. He had a stormy relationship with the *New York Times*: he resigned in 1963 over his Vietnam reporting, but returned a year later and embarked on a dangerous career as a war correspondent, including suffering shrapnel in his leg and receiving a Bronze Star for rescuing a wounded soldier during the Tet Offensive. He won a Pulitzer Prize in 1986 for his coverage of Reagan's Star Wars.

cited attempts to destroy existing rice caches ("Even with thermite molten-metal grenades, it virtually will not burn") or render them unpalatable through the addition of "yellow dye and shark repellant."

For Arthur Galston, the botanist whose graduate work had helped spawn the development of Agent Orange, Mohr's words were shattering. Galston suddenly saw himself implicated in a potential ecological nightmare; years later, he described his graduate TIBA research as "the scientific and emotional link that compelled my involvement in opposition to the massive spraying of these compounds during the Vietnam War."[94] The use of Agent Orange in Vietnam was deeply disillusioning, an ethical turning point that, in Galston's words, "violated my deepest feelings about the constructive role of science, and moved me into active opposition to official US policy."[95]

Galston immediately composed a critical resolution to present at the annual meeting of the American Society of Plant Physiologists. When the Executive Committee declined to present it, Galston began collecting signatures on a similar petition, written in the form of a letter to President Johnson. The document expressed the "serious misgivings" of about a dozen scientists, particularly regarding the unintended ecological consequences of spraying, the persistence of chemicals in the soil, and the possible food-denying effects on Vietnam's children. Johnson failed to reply, but an Undersecretary of State wrote back to Galston, assuring him the chemicals used were harmless and that crop destruction only occurred in remote areas, with advance

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[94] Galston, "Falling Leaves."

[95] Galston, "An Accidental Plant Biologist": 786-787; Galston, "Falling Leaves."

warning.[96] John Edsall, the Harvard biologist, had a similar experience. Early in 1966, he wrote to McNamara, expressing his opposition to crop destruction, and Maj. Gen. Michael Davison replied, asserting that the chemicals used were common weed-killers that "harm neither humans nor animals, and do no harm to the soil or water supplies in the concentrations used."[97] With letter writing efforts at a seeming dead end, Galston and his colleagues turned their attention to circulating petitions and urging the American Association for the Advancement of Science (AAAS) to open new scientific inquiries into defoliation. It was a quest that would culminate, four years later, in the creation of an $80,000 Herbicide Assessment Commission and a Department of Defense review.[98]

In the meantime, the Mohr articles had attracted non-scientist attention as well. In March of 1966, a landscape architect, Robert Nichols, staged a hunger strike to protest crop destruction in Vietnam and the hypocrisy of US policymakers who claimed that the United States was assisting Vietnamese food production. In response, State Department spokesman Robert McCloskey acknowledged publicly the U.S. role in crop destruction, but argued that only 'one-third of one per cent of the total area under cultivation in Vietnam' had been affected.[99] McCloskey's attempt at reassurance foundered, however, as critics added the confirmed charge of intentional starvation to their allegations of chemical warfare.

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[96] Galston, "Falling Leaves": 116-117. But Buckingham reports that in July 1965, Ambassador Lodge had requested authority "to change the May 1963 guidelines to allow crop destruction operations in more populated and less remote areas of South Vietnam," which resulted in a liberalization of policy. See Buckingham, 113-114.

[97] Hersh, "Our Chemical War."

[98] Galston, "Falling Leaves": 118.

[99] Briefing transcripts, 8 March 1966, "Vietnam Fasting by Robert Nichols against Vietnam policies" Folder, Box 197, Country File, Vietnam, NSF, LBJL.

Tear Gases and Chemical Warfare

The use of chemicals for defoliation and crop destruction were not the only military actions that attracted criticism. Increasingly, antiwar critics' arguments included damnations of another controversial military practice: the use of gas. The problems posed by "nonlethal gases"—specifically tear gas, super tear gas, and the nausea-inducing adamsite—elicited internal debate among Pentagon and administration insiders as well as the public at large.[100]

As early as 1961, military planners had discussed the use of gas in Vietnam. In a report prepared after a visit to South Vietnam by Taylor, Rostow, and other Pentagon, CIA, and State Department personnel, officials proposed using "anti-personnel" chemical warfare agents in areas where "the population may be essentially 100% Viet Cong." For example, they explained, "a part of Zone D might be used as a proving ground with perhaps advance notice being given…" The chemicals, presumably provided by the United States to ARVN soldiers to use, would not be subject to "the usual objections," since "this proposed application would be in one's own country." In other words, the United States would be exempt from blame if *South Vietnamese* military forces used gas against their guerilla opponents. Of course, the report continued, "From the point of view of political acceptability, incapacitating agents are probably to be preferred to lethal agents, but on the basis of technical feasibility, only the latter may be possible. This remains to be determined."[101]

While there is no evidence of explicitly lethal gas use during the war, the attitude suggested by the document was one of enthusiasm for experimentation ("proving ground"),

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[100] This chapter addresses the ethical debates concerning the particular tear gases used in Vietnam. For a more detailed Cold War history of nerve gas and other lethal chemicals, see Jonthan B. Tucker, *War of Nerves: Chemical Warfare from World War I to al-Qaeda* (New York: Pantheon Books, 2006).

[101] "RDT&E Annex, Report on General Taylor's Mission to South Vietnam," 3 November 1961, "Vietnam Report on Taylor Mission—November 1961" Folder, Box 210, Country File, Vietnam, NSF, LBJL.

especially if it were the South Vietnamese who actually carried out the work. Indeed, the following year saw the first transfer of tear gases to the government of South Vietnam, to be used at ARVN's discretion, with no particular US approval process.[102] The three major gases eventually provided to South Vietnamese military forces were tear gas (chloroacetophenone, abbreviated CN) and super tear gas (chlorobenzulidenemalononitrile, abbreviated CS), first delivered in 1962, and adamsite (diphenylaminochloroarsine, abbreviated DM), a nausea-inducing chemical provided at least as early as 1964.[103]

Both CN and DM had originally been developed during World War I, while CS had been discovered in the United States in 1928 and weaponized by British researchers in the 1950s. Although solid at room temperature, the chemicals could be packed into grenades and sprayed as aerosols to create gaslike clouds.[104] Of the three, the flowery-smelling CN produced incapacitating conditions of the shortest duration: intense irritation to the eyes, skin, and respiratory passages lasting several minutes. CS's effects were similar, but lasted five times longer and could include nausea as well. By far the most potent of the chemicals was DM, nicknamed "vomit gas" by the British soldiers who had used it in Bahrain. DM produced irritation to the eyes, nose, and throat, as well as intense nausea and vomiting, for a period ranging from 30 minutes to several hours. Army instruction manuals from the early 1960s warned that DM should not be used in "any operations where deaths are not acceptable."[105] In 1965, at least three American companies were manufacturing versions of these gases

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[102] Report from Westmoreland in Saigon, undated, "Gas, Vol I" Folder, Box 194, Country File, Vietnam, NSF, LBJL.

[103] McGeorge Bundy, memo, 26 March 1965, "Gas, Vol I" Folder, Box 194, Country File, Vietnam, NSF, LBJL; Seymour Hersh, "Poison Gas in Vietnam," *New York Review of Books,* 9 May 1968.

[104] Hersh, "Poison Gas in Vietnam."

[105] Quoted in Hersh, "Poison Gas in Vietnam."

commercially: Federal Laboratories in Saltsburg, PA; Lake Erie Company in Cleveland, Ohio; and Fisher Laboratory in New Jersey.[106]

On multiple occasions in the late fall and early winter of 1964-65, some or all of these gases were used in the course of military operations in Vietnam. During riots in Saigon in November 1964, South Vietnamese military forces reportedly used CS against protesting Buddhist monks and students. Later that winter, a mission to rescue American prisoners believed to be held at Tam Giang in Xuyen Province employed CS and smoke grenades, dropped from a helicopter, but "no contact was made." A similar effort two days later at Thanh Ham in Tay Ninh resulted in the use of 550 grenades containing a mixture of CN-DM, 100 CS grenades, plus another 300 lbs of CS dropped by helicopter on an area later determined to be unoccupied. In late January 1965, the South Vietnamese Air Force conducted a search-and-destroy mission on Phu Lac Peninsula in Phu Yen Province, employing 900 grenades containing either CS or CN-DM, as well as additional helicopter-dropped CS. Westmoreland reported to officials in Washington that tear gas had wafted into a nearby village, prompting a Radio Hanoi report of chemical weapons, but "the effect was very slight."[107] A Defense Department cable reported that 88 VC had been killed during the raid, but that ARVN's use of "CS and CN/DM agents… left much to be desired." Nevertheless, "this was a first experience for the troops involved and lessons were learned."[108]

Two months after the Phu Yen mission, two young AP reporters, Peter Arnett and Horst Faas, broke the story to a global audience, describing the experimental use of nonlethal gases in

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[106] Report, 23 March 1965, "Gas Vol I" Folder, Box 194, Country File, Vietnam, NSF, LBJL.

[107] These incidents were described in some detail in Report from Westmoreland in Saigon, undated, "Gas, Vol I" Folder, Box 194, Country File, Vietnam, NSF, LBJL.

[108] Department of Defense cable, January 1965, "Gas, Vol I" Folder, Box 194, Country File, Vietnam, NSF, LBJL.

Vietnam.[109] In the flurry of press conferences and newspaper coverage that followed, administration insiders emphasized the nonlethality of the gases and reiterated that any "experimentation" had been one of tactics, not new "secret types of military gas." The tactic in question, the dropping of tear gas on villages from helicopters, was explained as "a way of attacking guerillas mixed in with civilian populations without killing the civilians or destroying their villages."[110] Secretary of State Dean Rusk publicly asserted that the gases were not prohibited by the Geneva Convention of 1925 and were best described as "gases which have no lethal effect, which have a minimum disabling character."[111] Meanwhile, inside the administration, McGeorge Bundy summarized Westmoreland's report of the December incidents of tear gas use for President Johnson: "It sounds to me like no one even cried."[112] Johnson in turn professed to have had no prior knowledge about the gas use, but he nevertheless expressed frustration with critics who were more concerned that someone's eyes had watered that with "our soldiers who are dying."[113]

But public opinion balked at reports from the *Los Angeles Times* and other newspapers that the gas used had been a mixture of tear gas and adamsite, a combination whose reported vomit-inducing properties could last from 30 minutes to two hours and which, in prolonged, heavy doses, could potentially cause death.[114] Soviet bloc news outlets referred to the gas use as

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[109] Hersh, "Our Chemical War"; William M. Hammond, *Public Affairs: The Military and the Media, 1962-1968* (Washington, DC: Center of Military History, US Army, 1988); Howard Margolis, "Police-Type Gas Used By Saigon," *Washington Post,* 23 March 1965, A15.

[110] Quoted in Howard Margolis, "Police-Type Gas Used in Saigon," *Washington Post,* 23 March 1965. The *Los Angeles Times* published a similar account on 23 March 1965.

[111] Statement of Dean Rusk, 24 March 1965, "Gas, Vol II" Folder, Box 194, Country File, Vietnam, NSF, LBJL.

[112] Memo, Bundy to LBJ, 23 February 1965, "Gas, Vol I" Folder, Box 194, Country File, Vietnam, NSF, LBJL.

[113] Hersh, "Poison Gas in Vietnam."

[114] *Los Angeles Times*, 23 March 1965. Hornig's office estimated the duration of incapacitation at up to four hours.

chemical warfare, and newspapers in London and Paris published critical editorials.[115] In the *New York Times,* Max Frankel's front page coverage placed the tear gas and nausea agents squarely in the context of World War I era blister gases, and quoted at length Senator Wayne Morse, who considered adamsite use a violation of international law.[116]

Scientists offered key support to the many public figures raising concerns about gas use. On March 25, Wisconsin Congressman Robert Kastenmeier, with 15 Congressional co-signers, wrote to President Johnson in reaction to the reports of gas use. In 1959, Kastenmeier had introduced a resolution reaffirming American policy against the first use of biological weapons or "poisonous or obnoxious gases." The measure, although largely a reiteration of the Roosevelt policy, had been defeated at the urging of the Defense Department.[117] Now Kastenmeier requested an official statement of administration policy on the use of gas, and demanded that decision-making authority be transferred from local commanders to the president himself. He quoted anti-gas statements by Eisenhower and Roosevelt, and warned that the "The first use of gas in warfare, however innocuous its variety or effective its results, subjects the using country to the censure of the civilized world."[118]

Government officials quickly assured Kastenmeier that the gases were humane alternatives to more lethal tactics and in no way constituted gas warfare. As such, it was appropriate for local commanders to authorize their use. Only true chemical weapons—not the

[115] *Los Angeles Times*, 23 March 1965; "The Perils of Even a Humane Gas," *The Guardian*, 23 March 1965.

[116] Max Frankel, "U.S. Reveals Use of Nonlethal Gas Against Vietcong," *New York Times,* 23 March 1965.

[117] Quoted in Victor Sidel and Robert Goldwyn, "Chemical and Biological Weapons—A Primer," *New England Journal of Medicine* 274 (6 January 1966): 21-27.

[118] Letter, Kastenmeier et al. to LBJ, 25 March 1965, "Gas, Vol I" Folder, Box 194, Country File, Vietnam, NSF, LBJL

"riot control agents" used in Vietnam—required presidential approval. Moreover, they explained, the chemicals had been used in cases where "the Communist Viet Cong had taken refuge in villages, using innocent civilians as shields. Riot control agents were employed in an attempt to subdue the Viet Cong without exposing the South Vietnamese civilians and prisoners being held by the Viet Cong to injury from more lethal weapons such as rifles and machine guns."[119] The unsuccessful outcomes of these actions were not mentioned.

For the benefit of Kastenmeier and his Congressional cohort, Pentagon officials defined CN as "a lacrimatory agent which is an irritant to the respiratory passages and sometimes to the skin, and which incapacitates for about three minutes"; CS as "a more recently developed lacrimatory agent with effects similar to CN but which, in concentration, sometimes leads to nausea and which incapacitates for 5-15 minutes"; and DM as "a pepper-like irritant to the eyes, throat and mucous membrane, which may also cause vomiting and which incapacitates for 30 minutes to two hours."[120] But internal correspondence with the office of Donald Hornig, Johnson's science advisor, suggested far greater risks, particularly from DM. Hornig's office described DM:

> Adamsite (DM) Medical effects – when inhaled, causes pain, malaise with aching in eyes, joints, and teeth. Similar to influenza in effect. Vomiting occurs because of pain and malaise. Action comes in two to four minutes and lasts for 2-4 hours. The safety factor is between 8 and 10-fold, i.e., **a dose 8-10 times that causing incapacitation from pain and malaise may cause death. (This is a small safety factor particularly if children and old people are exposed). At the incapacitating dose, the mortality rate may be as high as 1 percent.**
> Prior use – Used in combination with tear gas for riot control as in Koji-Do, Korea, during POW riots.
> Commercial Availability - ~~Unknown~~ Available to US Police Departments[121]

[119] Letter from Office of the Deputy Secretary of Defense to Cong. Robert Kastenmeier, 30 March 1965, "Gas, Vol I" Folder, Box 194, Country File, Vietnam, NSF, LBJL.

[120] Ibid.

[121] Report, 23 March 1965, "Gas, Vol I" Folder, Box 194, Country File, Vietnam, NSF, LBJL.

According to Hornig, DM was potentially lethal, with an estimated affective period twice the duration reported to Kastenmeier. Hornig recommended an immediate halt of the use of both CS and DM.

McNamara responded to the outcry by ordering a moratorium on tear gas use in Vietnam, an unpublicized ban that lasted from January through the end of the summer of 1965. The same period saw a dramatic escalation of the war, including the onset of the massive bombing campaigns of Operation Rolling Thunder. In September, a young Marine colonel, apparently unaware of the ban, ordered the firing of 48 CN grenades into a tunnel and bunker system in which over four hundred Vietnamese soldiers and civilians had been hiding.[122] The action succeeded in forcing the occupants out of the underground complex, and prompted Westmoreland to request a lifting of the ban, so that tear gases could be employed to clear tunnels and underground shelters, as an alternative to other, more lethal, weaponry.[123] (Alternate responses to occupied tunnels included the use of flamethrowers, grenades, and sealing and wholesale demolition.[124]) Westmoreland's recommendation was supported by JCS chairman Earle Wheeler, who informed McNamara that the "Mighty Mite," a light-weight blower designed to fill tunnels and underground spaces with gas quickly, had already undergone testing in Vietnam. He assured McNamara that any policy shift would be kept quiet: there would be no

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[122] Telecom, 18 September 1965, "Gas, Vol II" Folder, Box 194, Country File, Vietnam, NSF, LBJL.

[123] Memo, Wheeler to McNamara, 11 September 1965, "Gas, Vol II" Folder, Box 194, Country File, Vietnam, NSF, LBJL.

[124] Cable, 9 September 1965, "Gas, Vol II" Folder, Box 194, Country File, Vietnam, NSF, LBJL

public announcements and responses to any questions would be deliberately "low key." [125] McNamara was persuaded; he informed President Johnson that he favored the shift, and, following Wheeler's advice, passed along instructions to Westmoreland to refer to the chemicals in public as "tear gas" rather than chemical agents, and to emphasize their humanitarian aspects. With Bundy's assurance that "even the New York Times is resoundingly with us on this," the ban was lifted, and by early October tear gas—CN and CS—was once again in use on the ground in Vietnam.[126]

The decision-making had not been unanimous, however. Arthur Goldberg, the U.S. Ambassador to the U.N., objected to the resumed use of gases out of concern for bad publicity.[127] Donald Hornig, who had previously urged a halt to the use of both CS and DM, reiterated his opposition to McGeorge Bundy two weeks after reports surfaced of Col. Utter's use of tear gas. Hornig wrote to Bundy:

> The effects produced by CS are much more violent than those resulting from CN, including chest constrictions and bronchial symptoms; there is less knowledge of the toxicity of CS in closed spaces; and in high concentrations it may produce nausea. The different between the effects of CS and CN would certainly be apparent, and this might appear to some critics as an escalation of normal tear gas.

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[125] Memo, Wheeler to McNamara, 11 September 1965, "Gas, Vol II" Folder, Box 194, Country File, Vietnam, NSF, LBJL.

[126] Memo, McGeorge Bundy to LBJ, 23 September 1965; Memo, McNamara to LBJ, 22 September 1965; and Circular 8 October 1965, all in "Gas, Vol II" Folder, Box 194, Country File, Vietnam, NSF, LBJL. A *New York Times* editorial from 11 September 1965 described the use of tear gas in clearing tunnels and caves in Vietnam as basically humane. In 1968, Seymour Hersh, writing in the *New York Review of Books,* claimed that the Utter case was "apparently a sham, a carefully planned trial balloon designed to make tear gas operational once again in Vietnam without public outcry." He argued that whereas the Haas allegations had taken Americans by surprise, which led to outrage, reports of later incidents of gas use were more carefully managed by Pentagon officials, who publicly repeated that there had never been an official ban on gas use, and promoted its use as a nonlethal alternative. In Hersh's view, this was all preparatory public relations for the real intended use of gas—as an agent which, when dropped from planes over jungle areas, would flush the Viet Cong out from their bunkers, in advance of a major bombing barrage. (See Hersh, "Poison Gas in Vietnam," *New York Review of Books,* 9 May 1968.)

[127] State Department Cable, Goldberg, 15 September 1965, "Gas, Vol II" Folder, Box 194, Country File, Vietnam, NSF, LBJL. Seymour Hersh quoted a State Department source in 1968 who recalled that the State Department, historically skeptical of gas use since the 1925 agreement, had only with great reluctance confirmed the legality of gas use in 1964 for a Pentagon review (see Seymour Hersh, "Poison Gas in Vietnam").

> More important, however, is the fact that the use of CS may considerably complicate our public relations problem. Our case for using tear gas would be considerably strengthened by the fact that tear gas is widely accepted as a humane civilian riot control agent. I think, however, there is a real question as to whether CS has in fact been used in this country or abroad and, if so, in more than a few scattered cases, by civilian authorities in normal riot control circumstances. I have not been able to obtain clear answers to these questions which we must certainly understand if authorization is granted for the use of this agent.
>
> I am also not aware that the DOD has made the case that CN will not adequately accomplish the purpose of clearing Viet Cong and civilians from tunnels and enclosed areas. In the event that CN proves ineffective for this purpose, CS can always be subsequently authorized.[128]

Hornig's concerns evoked McNamara's trademark commitment to graduated escalation—if CN was sufficient to achieve the objective, why resort to the more dangerous CS unnecessarily? But forwarded Hornig's memo by Bundy, McNamara himself was unmoved by the argument. He responded by producing an alternate source of technical expertise to counter Hornig, noting pointedly that "our Army technical experts assure me that CS is not as lethal as CN when used in inclosed spaces." He also dismissed Hornig's complaint of the lack of historic examples of civilian use of CS as irrelevant, since most social disturbances were quelled by military—not civilian—forces. He attached a list of examples of such use, ostensibly to demonstrate CS's acceptability. The list noted that US military forces had used CS against civil rights demonstrators in Cambridge, Maryland and Oxford, Mississippi, and against rioters in Panama. CS had also been used by the West German border police, British forces in Cyprus, the French in Algeria, and the South Vietnamese military against Buddhists and student protesters.[129]

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[128] Memo, Hornig to McGeorge Bundy, 17 September 1965, "Gas, Vol II" Folder, Box 194, Country File, Vietnam, NSF, LBJL.

[129] Memo, McNarama to McGeorge Bundy, 22 September 1965, "Gas, Vol II" Folder, Box 194, Country File, Vietnam, NSF, LBJL.

Scientist Activists

That winter, public outcry over gas use merged with anti-defoliation activism, as newspapers now carried reports of both, and accusations of chemical warfare proliferated. In the fall of 1966, a small group of Harvard scientists discussed taking public action to express their discomfort with U.S. use of chemicals—both defoliants and gases—in Vietnam. The group included biologist John Edsall, physicists Freeman Dyson and Bernard Feld, BoAS co-founder Eugene Rabinowitch, and the young Harvard biologist Matthew Meselson, who at 29 was an ACDA advisor, former student of Linus Pauling at Caltech, and, along with Franklin Stahl, hero of the famous Meselson-Stahl experiments confirming the semiconservative reproduction of DNA. The result was a highly-publicized letter to President Johnson, signed by 22 prominent scientists ("including seven Nobel laureates," noted the *New York Times*). The letter urged Johnson to declare, as policy, that the U.S. would not initiate CBW use. But the scientists went further, explaining why supposedly "non-lethal" gases and defoliants represented a far more serious threat than might initially appear. Gases that might not directly cause death could nonetheless be used in lethal ways, they argued. For example, "…when, in Vietnam, we spread tear gas over large areas to make persons emerge from protective cover to face attack by fragmentation bombs or when we use tear gas so that a moving target cannot move so fast, we use gas to kill."[130] (Two years later, Seymour Hersh would make similar allegations in a widely-cited article in the *New York Review of Books*, documenting at least two major aerial bombing attacks in 1966 and 1968 that had been preceded by widespread tear gas drops. [131])

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[130] *New York Times,* 20 September 1966.

[131] Seymour Hersh, "Poison Gas in Vietnam."

Perhaps most importantly to the 1966 group of scientists, the weapons represented a dangerous world precedent:

> The United States, along with other nations, recognizes that the use of even the smallest nuclear artillery shell in war would raise issues of extreme gravity. It would break down barriers to the use of more powerful nuclear weapons, and no one could tell where the escalation might end. The use of chemical or biological weapons, even relatively mild ones, involves similar dangers.[132]

The letter circulated through the top levels of the Johnson administration, and, although Johnson himself advised officials not to answer questions regarding the letter, his staffers officials readied themselves for a media barrage. They drafted answers to expected questions and circulated the results among officials, urging an emphasis on the "non-toxic" nature of the substances used and the careful restrictions on gases and herbicides that would prevent any escalation to more dangerous chemicals.[133] The *New York Times* could report only that, "despite protests by 22 leading American scientists," the Pentagon had confirmed the continuation of chemical use.[134] But in the face of the scientists' criticism, and in the aftermath of a massive, failed attempt to defoliate and burn, using napalm and white phosphorus, the Boi Loi woods, top decision-makers quietly eased the number of crop destruction and defoliation missions in Vietnam.[135] That same year, the United States endorsed a Hungarian-introduced United Nations resolution affirming the Geneva Protocol, but with the explicit caveat that the Protocol had been intended to curb poisonous gases, not the use of common riot control agents.[136]

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[132] Quoted in "22 Scientists Bid Johnson Bar Chemical Weapons in Vietnam," *New York Times,* 20 September 1966.

[133] Memo from "Brom" to Rostow, 20 September 1966 and accompanying draft, dated 9 March 1966, "10: Chemical and Biological Weapons" Folder, Box 212, Country File, Vietnam, NSF, LBJL.

[134] Benjamin Welles, "Pentagon Backs Use of Chemicals," *New York Times,* 20 September 1966.

[135] Buckingham, 112.

[136] Meselson, "Controlling Biological and Chemical Weapons."

This exclusion did not satisfy Meselson and his colleagues, who embarked on an expanded petition drive that would ultimately collect thousands of new signatures, including 127 members of the National Academy of Sciences. Beyond Meselson, other scientists and experts at Harvard were also linking defoliants and gas under the common heading of chemical warfare, with all its horrifying implications. In January 1966, Victor Sidel and Robert Goldwyn, two doctors from Harvard Medical School writing in the *New England Journal of Medicine,* disputed the nonlethality of the gases, noting that “they can kill under certain circumstances: extremely high concentration of agent of highly susceptible victim, such as the very young, the very old, or the very sick.” Defining chemical warfare as “the employment of chemicals toxic to men, animals or plants”—a definition that included both tear gas and defoliants—they listed the gases in their descriptions of chemical and biological weapons, alongside mustard gas, chlorine, phosgene, LSD, sarin, and anthrax. Like Meselson’s group, they worried that even the use of the most “humane” chemical alternatives could still open “Pandora’s box,” setting a path for use of more dangerous weapons.[137]

Doctors were also among the first scientifically-minded outsiders to visit Vietnam to document the medical effects of the war. In early 1967, four members of the Physicians’ Committee for Social Responsibility (an organization to which Sidel belonged) issued a report entitled “Medical Problems of South Viet Nam,” which described, in stark language, the dietary deficiencies, infectious diseases, and poor medical facilities in South Vietnam. The doctors also described in harrowing detail the war’s new kinds of casualties, including vicious burns inflicted by napalm, the jellied incendiary developed during World War II and increasingly employed as an “anti-personnel” weapon in Vietnam. Survivors of napalm wounds faced “a living death,” the

[137] Victor Sidel and Robert Goldwyn, “Chemical and Biological Weapons—A Primer.”

doctors wrote, while those who perished suffered "death…by roasting or by suffocation."[138] The frequently cited document was presented during Senate hearings on Vietnamese refugees.

An influential CBW-themed edition of *Scientist and Citizen*, which appeared in the summer of 1967, also cited the work of these doctors. The magazine had started out as a project of the St. Louis Committee for Nuclear Information, a team of scientists and concerned citizens that in the late 1950s had undertaken the St. Louis Baby Tooth Survey to test for levels of strontium 90 in baby teeth. From 1958 to 1963 they had published the periodical *Nuclear Information*, which in 1963 was renamed *Scientist and Citizen*. (In 1969 it would be renamed yet again, this time *Environment.*) In its 1967 incarnation, a collaboration with the Scientists' Institute for Public Information, the publication was infused with post-Manhattan Project notions of ethics and scientific responsibility, including the obligation to educate the public on science-related current affairs. An article introducing members of the new Science Advisory Board, including John Edsall and Milton Leitenberg, proclaimed that: "The scientists of *Scientist and Citizen* have a special responsibility to provide their fellow citizens with the information that is relevant to these decisions in an understandable form, free of technical jargon and political bias."[139] John Edsall offered a more radical interpretation. "The social mission of *Scientist and Citizen*," he wrote, "is precisely to enable the general public to escape the tyranny of the expert."[140] Or, as the back cover routinely warned, "It's your world—Don't leave it to the experts." Articles covered problems of nuclear proliferation, fallout, and civil defense, but also

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[138] J.L. Collins et al, "Medical Problems of South Viet Nam, January 1967, Prepared for the Physicians' Committee of Social Responsibility," in "The Committee of Responsibility, Inc." Folder, Series IIa, Box Ch-F, Papers of Salvador Luria, American Philosophical Society.

[139] *Scientist and Citizen*, January 1967.

[140] *Scientist and Citizen*, February 1967.

the supersonic transport, pollution, pesticides, and other consumer dangers. Contributors ranged from dove to hawk, from Victor Paschkis to Eugene Wigner.

With a stark cover featuring a Ranch Hand plane, rocket delivery systems, and magnified images of microbes arranged in the shape of a gas mask, the September-October special issue was devoted exclusively to chemical and biological warfare. Harvard microbiologist John Edsall wrote the introductory essay, in which he reiterated the argument made in the handful of petitions and letters sent to the administration: despite some possibly humane applications of chemical weapons, the "dangers of escalation" were omnipresent. He invoked the language of older nuclear debates: "Once either side begins using weapons of this category," Edsall wondered, "can any dividing line be drawn at which both sides, locked in conflict, will uphold a binding agreement that says 'At this point we will stop; we will use no weapons more deadly than this.'"[141]

Elsewhere in the issue, Harvard nutritionist Jean Mayer warned of the exacerbating effect of crop destruction on malnutrition and disease in Vietnam, citing the work of the Physicians for Social Responsibility and describing in detail the medical consequences for the population: elevated child mortality; degradation of the heart, stomach, lungs, and intestine; psychological distress, blindness, anemia, and deadly infection. He also presented an account of previous attempts to starve enemy forces, from the Franco-Prussian War to World War II, arguing that "food denial in war affects the fighting men least and last," taking its toll instead on the civilian population.[142] Sidel and Goldwyn, the doctors who had written the weapons "primer" for the

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[141] John T. Edsall, "Introduction," *Scientist and Citizen*, August-September 1967: 114.

[142] Jean Mayer, "Starvation as a Weapon: Herbicides in Vietnam I," *Scientist and Citizen,* August-September 1967: 114. Mayer's analyses, beginning in 1966, spawned a RAND study which concluded that crop destruction was ultimately a propaganda boon to the Viet Cong. See Cecil quoted in Wilbur Scott, *Vietnam Veterans since the War* (Norman, OK: University of Oklahoma Press: 2004), 81.

New England Journal of Medicine, contributed a similar article describing the chemistry and effects of many categories of chemical weapons.

The centerpiece of the issue was Arthur Galston's in-depth discussion of the ecological dangers of defoliation. Like other critics of defoliation, he mentioned the risk of escalation and the warning labels that appeared on domestic herbicides. Galston acknowledged the difference between defoliants and far more lethal weapons used in war, but emphasized that ecological damage posed enormous and unknown risks. He wrote evocatively:

> To damage or kill a plant may appear so small a thing in comparison to the human slaughter every war entails as to be deserving of little concern. But when we intervene in the ecology of a region on a massive scale, we may set in motion an irreversible chain of events which could continue to affect both the agriculture and the wildlife of the area—and therefore the people, also—long after the war is over.

Galston offered a startlingly accurate assessment of the experimental nature of the sprayings. Though he provided explanations of the general physiological processes of chemical defoliation in plants, he wrote that scientists did "not understand at all well" the changes in plant life caused by external chemicals, and since scientists had only studied a few species carefully in this context, they could hardly predict the ways herbicides might affect a complex ecosystem. Given these uncertainties, the spraying in Vietnam itself constituted a kind of experiment:

> …when we spray a synthetic chemical from an airplane over a mixed population of exotic plants growing under uninvestigated climatic conditions—as in Vietnam—we are performing the most empirical of operations. We learn what the effects are only after we perform the experiment, and if these effects are larger, more complex, or otherwise different from what we expected, there is no way of restoring the original conditions.[143]

Galston's eloquence in the pages of *Scientist and* Citizen was accompanied by diligent efforts to work within all available institutions to try to halt defoliation and gas use. Beginning in

[143] Arthur W. Galston, "Changing the Environment: Herbicides in Vietnam II," *Scientist and Citizen,* August-September 1967: 122-129. (Many print runs of this issue contained printing errors that omitted the last half of this article.)

the mid-1960s, to complement their letter-writing efforts and circulation of petitions, Galston, Edsall, Meselson, and other biologists and botanists began working within the AAAS, hoping to bring greater institutional backing to their efforts. In 1966, E.W. Pfeiffer, a zoologist from the University of Montana, presented an AAAS resolution calling for an expert study of chemical warfare in Vietnam. Pfeiffer's resolution followed a tortuous path through various committees and votes, culminating in language which, stripped of its reference to Vietnam, nevertheless warned that "the full impact of the uses of biological and chemical agents to modify the environment… is not fully known." The effort seemed to elicit a reaction— the Defense Department's ARPA contracted with the Midwest Research Institute to conduct a review of existing literature regarding herbicide use. The result was a 369-page summary of over 1,500 related studies and dozens of interviews.[144] AAAS's Board of Directors also corresponded with John Foster, the Director of Defense Research and Engineering, who assured attendees at the 1967 national meeting that although questions of detrimental "long-term ecological impacts" of herbicide use had not been answered "definitively," planners had consulted "qualified scientists, both inside and outside our government," who "judged that seriously adverse consequences will not occur."[145] Like McNamara in his dismissal of Hornig's concerns about CS and DM, Foster invoked the support of unnamed researchers to deflect the concerns of civilian scientists.

But despite Foster's assurances, the MRI report contained little detailed information about actual spraying in Vietnam, focusing mainly on summarizing research performed on other vegetation and in other contexts, which was then extrapolated to conclude that the risks of spraying in Vietnam were either negligible or unknown. AAAS solicited commentary on the

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[144] Philip M. Boffey, "Defense Issues Summary of Defoliation Study," *Science* 159 (9 February 1968): 613.

[145] J.B. Neilands, "Vietnam: Progress of the Chemical War," *Asian Survey* 10, No. 3 (March, 1970); *Science* 161, No. 3838 (19 July 1968).

report from the National Academy of Sciences and other experts, and the Board ultimately expressed deep dissatisfaction with the report and with the failure of the research cited to confirm explicitly Foster's rosy assessment. A majority of AAAS board members wrote in *Science* in 1968 that "many questions concerning the long-range ecological influences of chemical herbicides remain unanswered." They called for a field study of conditions in Vietnam, to be overseen by the United Nations, and a temporary halt to the use of the arsenical Agent Blue, until further research on its degradation could be conducted. Other board members, led by Barry Commoner, went further, demanding in a supplementary statement that the use of 2,4-D and 2,4,5-T be halted as well.[146] In *Scientist and Citizen*, associate editor Sheldon Novick complained that "The report as a whole is an emphatic repetition of the fact that we are engaged in a gigantic experiment in Vietnam, and have little idea what its outcome may be."[147]

Frustrated by the slow progress of the AAAS, a small cadre of members, including Galston, Edsall, Pfeiffer, and Berkeley biochemist J.B. Neilands, formed the Scientists' Committee on Chemical and Biological Warfare. The group aimed to promote a strict interpretation of the Geneva Protocol and to conduct field studies on the ground in Vietnam. They made presentations to other scientific organizations, including the Federation of American Societies for Experimental Biology and the American Society for Microbiology, and two members, Pfeiffer and Gordon Orians, prepared for a 1969 research trip to South Vietnam, the results of which included descriptions of the "*very* severe" ecological consequences of defoliation and warnings that the immediate establishment of an international study of long-term consequences of defoliation was critical "if the U.S. scientists wish to maintain—or regain—the

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[146] *Science* 161, No. 3838 (19 July 1968).

[147] Sheldon Novick, "The Vietnam Herbicide Experiment," *Scientist and Citizen,* January-February 1968: 21.

respect of scientists in Southeast Asia."[148] But in 1970, Neilands mourned the overall lack of institutional involvement in the anti-herbicide campaign: "Within the professional science organizations, only the AAAS has shown any concern about the defoliation of Vietnam and this has required constant prodding from Pfeiffer. Perhaps most distressing of all has been the inability of the AAAS to follow through with any kind of action program."[149] The fervent petition drives and the introduction of resolutions were not yielding results.

Beyond Academic Scientists

Gauging the overall opinion of scientists on the problem of Vietnam is difficult. In the summer of 1967, Penn State biophysicist Ernest Pollard wrote a letter to *Science*, soliciting volunteers to help with the war effort, through "ingenuity regarding weapons," operations research, bombing analyses, or any other form of assistance. He invoked the great contributions of scientists during World War II, offered despite the participants' variety of political viewpoints, and wondered why the current war had failed to evoke similar forms of voluntarism. He wrote, "If indeed it is true that the university scientists as a whole are opposed to the opinion of the majority of the people of the United States regarding support of the war in Vietnam, then obviously we are in a bad situation."[150]

Writing in response two months later, MIT biologist Salvator Luria and Woods Hole marine biologist Albert Szent-Gyorgyi chastised Pollard for promoting President Johnson's disingenuous justifications for war, arguing instead that the war was "a national catastrophe and

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[148] E.W. Pfeiffer and G.H. Orians, "Mission to Vietnam Part 2," *Scientific Research* 23 (June 1969).

[149] Neilands, "Vietnam: Progress of the Chemical War": 229.

[150] Ernest C. Pollard, "Call for Scientific Help," *Science* 157, No. 3790 (18 August 1967): 755-756.

a moral blight for our country." They replied that yes, thousands of university professors, "including a large percentage of scientists," did oppose the war. Rather than volunteer to help the military, scientists ought to examine their own activities "to make sure that they do not unnecessarily contribute to the waging and prolongation" of the war.[151] A week later, Luria followed his own advice, vowing in the *MIT Tech* to "publicly disassociated himself from any research or work on defense projects in protest against the Vietnam War." He refused to pay the portion of his income tax for war funding.[152]

Even if, as Pollard feared, university scientists largely opposed the war, there were still thousands of other non-academic scientists—industrial scientists, engineers, and researchers, for example—who were either supportive or indifferent. Throughout the late 1960s, Lt. Alvin Young, a research scientist in the Bio-Chemical Division at Eglin Air Force Base, compiled outreach rosters and attended meetings of the Weed Society of America, a professional organization with a substantial corporate demographic and friendly attitude toward military defoliation.[153] In 1967, the same year that Meselson and Edsall collected 5,000 scientists' signatures on a request that President Johnson "repudiate the use of chemical weapons in

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[151] Salvador Luria and Albert Szent-Gyorgyi, "Vietnam: A National Catastrophe," *Science* 158, No. 3797 (6 October 1967): 47.

[152] Clipping, *MIT Tech*, 17 October 1967 in "Vietnam #2" Folder, Box: Series IIa: subject Files, V-W & Series IIb: Personal Material A-L, Papers of Salvador Luria, American Philosophical Society. Other similar exchanges occurred in the letters pages of *Science*. For example, in the summer of 1969, Orians and Pfeiffer wrote in to report their observation of unintentional defoliation due to chemical drift in South Vietnam. In the same issue, psychologist Clarence Leuba expressed his irritation at biologists prioritizing concern for "plants and animals" over the lives of American and South Vietnamese soldiers. "No wonder that the opinions of most academic and scientific people regarding national and international matters command little respect," he wrote acidly. See G.H. Orians, E.W. Pfeiffer, and Clarence Leuba, "Defoliants: Orange, White, and Blue," *Science* 165 (1 August 1969): 442-443.

[153] Memorandum, "Trip Report: Las Vegas Nevada, 10-14 Feb 69," Alvin L. Young to Branch Chief BCW, 4 March 1969, in Young collection, accessed at http://www.nal.usda.gov/speccoll/findaids/agentorange/text/03656.pdf 12 September 2010. In this report, Young noted his "surprise" that, despite wearing his Air Force uniform and attending sessions on herbicides, he was treated with "respect, friendliness, and interest." He attributed this response to presentations by Tschirley and Capt. Jon Arvik of ATCB expressing support for herbicide operations in Vietnam, and noted that after the meeting's conclusion he received additional reports from Tschirley and "23 volumes" of Dow journals on pesticide research.

Vietnam," the American Society for Microbiology voted 600-34 in favor of continuing its advisory role to the Army's biological warfare lab at Fort Detrick.[154] At the top levels of government, Donald Hornig expressed his relief at the ASM result to President Johnson, observing that "This is a helpful development in view of strong criticism from some segments of the biological community."[155] The vote underscored the differing viewpoints of the small group of largely tenured university scientists who opposed the war, or at least its tactics, and the majority of working microbiologists. At the same time, Hornig's report indicated just how powerful those minority viewpoints were—attracting the attention of the president and his science advisor, and forcing an organization-wide referendum on military advising.

Not all corporate researchers supported defoliation, and not every report from government-sponsored scientists offered a clear-cut endorsement of operations. In September 1968, two Monsanto scientists, "speaking as individuals and not as representatives of Monsanto," contributed a detailed and harrowing article to *Scientist and Citizen* about the effects of Tordon (picloram), the Dow-produced component of Agent White. They revealed that unlike the shorter-lived phenoxyacetic acids, the effects of picloram on vegetation—particularly forest growth—could last two years, and the chemical could remain in the soil for that duration or longer. It could pass intact through the digestive system of a mule. Moreover, it was unclear exactly what the targeted areas were. Charles Minarik, the Fort Detrick scientist who had worked on defoliation in Vietnam during the Kennedy years, had implied that Agent White was used

[154] Neilands, "Vietnam: Progress of the Chemical War"; also described in *Federation of American Scientists Bulletin,* May 1967.

[155] Hornig to LBJ, 2 June 1967, in "Donald Hornig Chronological File: April-June, 1967, Box 5, Papers of Donald Hornig, LBJL.

only for control of conifers, but the two Monsanto scientists observed that military procurement of picloram far exceeded the amount necessary to target Vietnamese pine forests. They cited a warning by New Zealand researchers that picloram sprayed aerially over large areas could lead to widespread contamination, and warned "Large areas *are* being treated aerially in Vietnam, but no studies of contamination by movement have been forthcoming from that field." Noting that no published research existed on the ecological effect of Agent White, a mixture of picloram and 2,4-D, and echoing the views of so many other scientist-critics, the two men emphasized the lack of existing knowledge and the experimental nature of the spraying.[156]

That fall, another report from an unlikely source fueled further scientific opposition to defoliants. Beginning in the winter of 1967-68, the State Department, long a source of moderate resistance to the use of chemicals,[157] had requested that a small group of scientists, including Charles Minarik, Director of the Department of Defense's Plant Sciences Laboratories, and Fred Tschirley, Assistant Chief of the Crop Protection Research Branch of the Agricultural Research Service, review various aspects of the Ranch Hand program. The combined reports of the scientists generally upheld the current policy as offering greater benefits than detriments, but the views of Tschirley, later published for a wider audience on the pages of *Science*, offered some troubling observations.[158] Although constrained by time and safety, Tschirley nevertheless toured multiple regions where herbicides had been employed, and conducted aerial surveys of

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[156] George R. Harvey and Jay D. Mann, "Picloram in Vietnam," *Scientist and Citizen* (September 1968): 165-171.

[157] See Hersh, "Our Chemical War"; Buckingham, 101. Buckingham attributes at least some of this moderation to diplomatic concern.

[158] Buckingham, 145-146. Other reports included Minarik's 1969 assessment of damage to Cambodian rubber plantations, which he concluded had not, as had been initially suspected, been caused by defoliation drift from Tay Ninh operations, but most likely due to direct overhead spraying in Cambodia, which he attributed to "an unknown party." See Charles Minarik, "Report of Cambodian Rubber Damage," 11 December 1969, in the Alvin L. Young Collection on Agent Orange at the National Agricultural Library, http://www.nal.usda.gov/speccoll/findaids/agentorange/text/03124.pdf (accessed 9 September 2010).

target areas. Much of his report was dedicated to dismissing fears: that defoliants had caused changes in local climate, for example, or had permanently poisoned the soil. He did observe, as earlier ARPA scientists had at the beginning of the decade, that mangroves were particularly susceptible to Agent Orange. "The trees were not simply defoliated, but were killed," he wrote. He estimated that mangrove areas sprayed in 1962 could take as long as 20 years to redevelop, with possible repercussions for soil erosion and marine life. He acknowledged that little was known about "the effect of killing mangrove on animal populations."

Tshirley's trip coincided with the onset of the dry season—a period of natural defoliation for Vietnam's forests—and he encountered difficulty distinguishing between normally denuded trees and those defoliated chemically or by fires. He was unable to estimate the effects of defoliants on Vietnam's semideciduous forests, relying instead on his own previous USDA research on defoliation test sites in Puerto Rico and similar reports from Thailand. Even then, the multiple applications of herbicides in Vietnam surpassed the test sprayings elsewhere. Whereas Tschirley estimated that Vietnam's dense forest canopy could regenerate within a few years of a single dose of Agent Orange, "A second application during the period of recovery would have a wholly different effect." Again, noting the "scanty" research on tropical forest regeneration, Tschirley turned instead to studies of the island of Krakatau, destroyed so thoroughly by volcanic eruption in 1883 that "The only living thing a visitor saw in May 1884 was one spider." The flora of Krakatau did eventually begin to reappear—a development reassuring to Tschirley, who noted optimistically that Vietnam was neither totally defoliated, covered with ash, nor an island.

Tschirley also observed, as some scientists had predicted, the increased presence of opportunistic grasses and bamboo in defoliated areas. Once again, he extrapolated from his research in Puerto Rico to suggest that the sprayed Vietnamese forest would surely recover within a few years, were it "not for the probable invasion by bamboo." He could conclude only that "the time scale for succession in a deciduous forest in RVN is unknown" and "The effect of defoliation on animal populations is truly unknown." On the latter point, however, he noted somberly, "I suspect that bombing, artillery, fire, human presence, and hunting have had a far greater effect than has defoliation." His message was clear: war itself was far more devastating for the ecology of Vietnam than the tactic of defoliation alone.

Tschirley offered one final, interesting note on the "toxicity of herbicides," which echoed the warning of the Monsanto scientists. The risk, he wrote, came not from the phenoxyacetic acids such as Agent Orange, but from the organic arsenical compounds in Agent Blue. These constituted only a low risk to mammals, however, so he concluded that "There is no evidence to suggest that the herbicides will cause toxicity problems for man or animals." He offered no discussion of Agents Orange, Purple, or White. As he had mentioned repeatedly early in the report, however, little data of any kind concerning the defoliants' effects existed. The lack of evidence demonstrating toxicity was the same lack of evidence demonstrating safety. [159]

Dioxins

Tschirley's report, republished for a wider scientific audience in the winter of 1969, elicited concern from both inside and outside the government, particularly regarding the heavy use of picloram. In the aftermath of the report's publication, Galston wrote to *Science* that "While I continue to oppose most aspects of our chemical warfare operation in Vietnam, if it is to continue it would be better to use the readily biodegradable 2,3-D than picloram, which is so

[159] Fred H. Tschirley, "Defoliation in Vietnam," *Science* 163 (21 February 1969): 779-786. The *Science* article was based on Tschirley's September 1968 report to the State Department.

resistant in some clay soils that under 5 percent disappears each year."[160] The State Department's Herbicide Policy Review Committee shared this concern over picloram, but still endorsed the overall defoliation program. Their qualified views were countered by an Air Force study of Ranch Hand and a review of crop destruction by the Joint Chiefs of Staff, both of which contained vigorous reiterations of support for the programs.[161] But within six months, all of these areas of concern—the effects of picloram in Agent White and arsenic in Agent Blue, the military effectiveness of defoliation and crop destruction—would be superseded by a new source of worry: the dioxin contamination of Agent Orange.

In 1949, an explosion at a Monsanto plant in Nitro, West Virginia had exposed over two hundred workers to a chemical by-product of herbicide production, a polychlorinated dibenzodioxin abbreviated TCDD, commonly referred to as dioxin. The exposed workers were stricken with chloracne, a vicious and sometimes permanent inflammatory skin disorder. In 1964, before the major contracts to produce Agent Orange for the U.S. military had been signed, researchers at the Dow Chemical Corporation noticed the dioxin contamination of their own defoliation products. Dow officials responded by setting a safety threshold for dioxin of one part per million (1 ppm), and monitoring manufacturing processes to ensure that no herbicides contained dioxin above that level. The following year, representatives from several major chemical companies met to discuss the problem of dioxin, and Dow representatives observed that some of their competitors—particularly Monsanto—were producing highly contaminated products. The existence and nature of this meeting was not publicized, and details of what

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[160] Arthur W. Galston and Edwin O. Willis, "Lesser of Two Evils," *Science* 164 (25 April 1969): 373-375.

[161] Buckingham 134-135, 151. The JCS review was a response to a 1967 RAND study concluding that crop destruction program had been a failure thus far, alienating allies and failing to diminish VC food supplies.

transpired were not revealed until more than a decade later, during the proceedings of a class action lawsuit concerning the effects of Agent Orange on Vietnam veterans.[162]

Nevertheless, in that same year, 1965, President Johnson's PSAC encouraged further study of the effects of Agent Orange, and the National Cancer Institute sponsored new research on the effects of defoliants on animals, to be conducted at the Bionetics Research Lab in Bethesda, Maryland.[163] The action may have been due more to the nascent environmental awareness spawned by *Silent Spring* and other works than by the early efforts of Edsall and Meselson in that year. As David Butler, a senior program officer at the National Academy of Sciences, later explained, "The mid-1960s was… a time of burgeoning interest in studies of the mutagenic, carcinogenic, and reproductive effects of chemicals. The National Cancer Institute launched an investigation of the tumorogenic, mutagenic, and teratogenic potential of a number of insecticides and herbicides in 1965 and gave the contract to Bionetics Research Laboratories."[164]

The Bionetics study, involving mice fed large quantities of the herbicide 2,4,5-T, was completed in 1966, but the results were not reported to the Food and Drug Administration until the fall of 1968. Additional testing then confirmed the conclusions of the Bionetics researchers: Agent Orange exposure caused high rates of birth defects in mice. In October of 1969, President Nixon's newly appointed science advisor, Lee DuBridge, announced an abrupt shift in Pentagon

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[162] Peter H. Schuck, *Agent Orange on Trial: Mass Toxic Disasters in the Courts* (Cambridge: Belknap Press, 1986). Information about the 1964 Dow standard and the 1965 meeting was revealed during the 1970s Agent Orange class action lawsuit, in the form of a 1965 memo from Dow officials describing the meeting.

[163] Scott, 168.

[164] Butler, "Connections."

herbicide policy.[165] Citing the Bionetics study, DuBridge warned of a possible link to elevated birth defects, and stated that Agent Orange would no longer be applied in or near populated areas in Vietnam. In a remarkable turnaround from previous official descriptions of Agent Orange as a “common” herbicide in widespread domestic use, DuBridge reassured Americans that their risk of contact was low, since “almost none” of the chemical was used in homes or gardens in the United States; rather, it was only applied in non-residential areas.[166] The USDA would conduct new studies of the chemical, and would restrict government sprayings in the interim.

Reporters and researchers followed up on the “yet unpublicized experiments conducted by the Bionetics Research Laboratories,” in which pregnant mice heavily exposed to the substance spawned a high rate of deformed fetuses.[167] The AAAS quickly passed a resolution calling for an immediate halt to the use of 2,4-D and 2,4,5-T in Vietnam.[168] Four months later, the USDA revealed that the birth defects had likely been caused by a contaminant to the herbicide, possibly skewing the results.[169] The contaminant was 2,3,7.8-tetrachloro-para-dibenzodioxin (TCDD), otherwise known as dioxin, the dangerous substance responsible for the Nitro chloracne cases and detected by Dow researchers in 1964. Arthur Galston later described in more technical detail how the process of synthesizing 2,4,5-T created dioxin as an “unwanted” byproduct:

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[165] Robert Smith, “U.S. Curbs Use of Weed Killer That Produces Rat Deformities,” *New York Times,* 30 October 1969; Morton Mintz, “Wide Used Herbicide Tied to Birth Defects,” *Washington Post,* 8 April 1970.

[166] Robert Smith, “U.S. Curbs Use of Weed Killer That Produces Rat Deformities,” *New York Times,* 30 October 1969.

[167] Joel Kramer, “Yesterday Cyclamates, Today 2,4,5-T, Tomorrow DDT?” *Science* 166 (7 November 1969): 724.

[168] Buckingham, 164.

[169] “Contaminant in Pesticide Linked to Defects in Mice,” *New York Times,* 7 February 1970.

> [Agent Orange] is synthesized by combing 2,4,5-trichlorophenol with a modified acetic acid under alkaline conditions. But during synthesis at elevated temperatures designed to make the reaction proceed more quickly, an unwanted side reaction occurs. Two molecules of the chlorinated phenols react with each other to form a tricyclic planar compound with four chlorine atoms at the periphery of the plane. Such compounds are able to insert themselves into the groove between the two complementary chains of the duplex DNA molecule, thereby interfering with basic replicative processes essential to the cell. These inadvertently produced dioxins, such as 2,3,7.8-tetrachloro-para-dibenzodioxin (TCDD), turned out to be extremely toxic to both humans and animals.[170]

The revelation that Agent Orange contained dioxin pushed Galston's activism to a new level. With the selection of DuBridge as science advisor, he also now had an inside line to the White House. Galston and DuBridge had been colleagues at Caltech, and had forged good relations during the contentious debates over McCarthyism and loyalty oaths in the 1950s. Now Galston reached out to DuBridge and to Meselson, who, as a former student of Linus Pauling, had his own ties to Pasadena. The three men scheduled a private meeting with key "military scientific advisors," during which Galston and Meselson explained the inevitability of dioxin contamination, the inherent risks, and pushed for a ban of herbicide use. Galston later credited the meeting with a monumental result: "DuBridge's recommendation to Nixon that the spray operation be terminated."[171] In the spring of 1970, in the face of mounting criticism, the Pentagon announced the suspension of all Agent Orange use in Vietnam, and HEW and the USDA strictly curtailed the domestic use of the component 2,4,5-T.[172]

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[170] Galston, "Falling Leaves": 114.

[171] Galston, "Falling Leaves": 119.

[172] Buckingham, 166.

Scientists, Congress, and the Protocol

Galston and DuBridge were not the only influential voices who could claim credit for shaping policy. In the spring of 1969, spurred by recent United Nations meetings and hopes for the upcoming round of détente discussions between President Nixon and Soviet Premier Alexei Kosygin, the Senate Committee on Foreign Relations held secret hearings on chemical and biological weapons. Meselson, called to testify, carefully addressed the politicians' questions concerning the controversial weapons used in Vietnam: tear gas, herbicides, and napalm. Acknowledging that "there are pros and cons to the use of tear gas in war," Meselson reported that many nations considered tear gas banned by the terms of the Geneva Protocol, and that even as a nonlethal chemical weapon, it ran the risk of creating "a highly undesirable escalation." Herbicides, on the other hand, were not explicitly mentioned in the Protocol at all. Unlike J.B. Neilands and other members of the SCCBW, Meselson did not consider incendiaries such as napalm and white phosphorus to constitute chemical weapons, because they caused injury through "intense burning," and not "because of a poisonous action." But whatever their interpretive leanings, Meselson urged the committee to work toward a coherent policy. "The important thing," he told them, "is that there be a uniform rule."[173]

Before the year was out, Meselson got his wish. On November 11, 1969, the anniversary of the armistice of World War I, President Nixon formally requested that the Senate ratify the Geneva Protocol of 1925. By then, the agreement had been ratified by more than 60 countries, including the Soviet Union, China, and all other NATO and Warsaw Pact countries. But where the 1925 agreement had banned all "asphyxiating poisonous gases" and "bacteriological methods

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[173] Matthew Meselson, Testimony, 30 April 1969, U.S. Congress. Senate. Committee on Foreign Relations. before Senate Committee on Foreign Relations, *Chemical and Biological Warfare*. 91st Cong, 1st Sess., 30 April 1969, LexisNexis (accessed 7 May 2011).

of warfare," Nixon added his own qualification, stipulating that these descriptions did *not* include riot control agents and chemical herbicides. (A month later, the United Nations General Assembly would vote to confirm the exact opposite interpretation.[174]) The process of ratification stretched into the mid-1970s, and after the final 90-0 Senate vote was taken, President Ford maintained the riot control and herbicide exceptions through executive order. He tried to temper his controversial interpretation with a qualified no-first-use policy, forbidding the "first use of herbicides in war except use, under regulations applicable to their domestic use, for control of vegetation within U.S. bases and installations or around their immediate defensive perimeters" and the "first use of riot control agents in war except to save lives, such as, use of riot control agents in riot situations, to reduce civilian casualties, for rescue missions, and to protect rear area convoys."[175]

Despite the achievement of Meselson's stated hope for a coherent policy, these exceptions to exceptions to exceptions pleased few of the critics who had initially pushed for the Protocol's ratification. Besides the influence of the holdover Johnson PSAC and the Senate Foreign Relations Committee, which heard testimony from expert scientists, the concerns of the academic biologists and botanists who had criticized gases and herbicides were largely ignored. Their warnings of the ecological dangers of defoliation were explicitly rejected by the language of Nixon's and Ford's exceptions. The problem of escalation was addressed only partially: legal boundaries had been set, but they did not allay scientists' fears that the use of nonlethal chemicals would subtly prepare leaders and populations for the introduction of more dangerous

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[174] Buckingham, 161.

[175] Quoted in William J. Bailey, "Introductory Remarks," in *Chemical Weapons and U.S. Policy: A Report of the Committee on Chemistry and Public Affairs* (Washington, DC: American Chemical Society, 1977), Othmer Library, Chemical Heritage Foundation.

chemical and biological weaponry. More concretely, the new policy codified the rejection of all the ecological arguments made by Galston and others, and the humanitarian concerns over super tear gas voiced by Donald Hornig. For Meselson, who had hoped for a "uniform rule" that banned tear gas and anticrop chemicals, it was a Pyrrhic victory.

Agent Orange: From Temporary to Permanent Ban

Despite their allowance under the Protocol's exemptions, herbicides were still subject to a severe debate, rooted largely in the new dioxin revelations. During the temporary ban, a series of influential studies were conducted, by the AAAS, the National Academy of Sciences, and a contingent of Pentagon engineers. Meselson was deeply involved in the first of these, arranging for the procurement of $80,000 from AAAS to conduct, finally, a month-long field study of the effects of herbicides on the ground in Vietnam. The AAAS in turn created an Herbicide Assessment Commission headed by Arthur Westing, which dispatched a team to South Vietnam that included Meselson, Westing, John Constable of the Harvard Medical School, and Robert Cook, a Yale ecology graduate student. The Commission's preliminary report, issued in December 1970, offered a depressing picture: mangrove populations had not regenerated, even four years after spraying; dead trees and colonizing bamboo was observed in South Vietnam's hardwood forests; and observations of crop destruction targets suggested that civilians had suffered more from the food loss than VC.[176]

[176] Buckingham, 170-171.

Meanwhile, inside the administration, science advisors DuBridge and Ivan Bennett Jr. had urged an end to Agent Orange use and crop destruction practices, while the Joint Chiefs of Staff argued vociferously for the resumption and continuation of these programs.[177]
After DuBridge was replaced by Edward David as science advisor, David continued to push for an end to defoliant use. In November 1970, anticipating harsh reports from the AAAS Commission and fearful of jeopardizing other delicate foreign policy concerns, David proposed a compromise policy: that domestic standards for chemical use in the United States be applied to Vietnam.[178] The military leadership balked at the idea and downplayed the risks of dioxin, but Secretary of Defense Melvin Laird sided with David, cementing the ban on Agent Orange, ordering a phasing out of all herbicides, and ending the practice of crop destruction in early 1971.[179] All stockpiles of Agent Orange in Vietnam were to be returned to the United States as of September 13, 1971.[180]

In October 1970, Congress instructed Laird to conduct a review of herbicide use in Vietnam.[181] The Pentagon in turn contracted separately with both the National Academy of Sciences and with the Army Corps of Engineers' Strategic Study Group (ESSG). The NAS study was headed by Anton Lang, a Russian-born plant physiologist and head of Michigan State's Plant Research Laboratory, and conducted by dozens of international scientists who served as

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[177] Buckingham, 171.

[178] Namely, the Geneva Protocol hearings. See Buckingham, 172-173.

[179] Buckingham, 174.

[180] Young, "The History of the US Department of Defense Programs for the Testing, Evaluation, and Storage of Tactical Herbicides."

[181] Buckingham describes this as a move "prodded by scientists."

committee members and advisors.[182] Their final report, issued in 1974, echoed the earlier work of Tschirley: they saw evidence of mangrove devastation and forests heavily damaged by all forces of war, but lacked sufficient data to confirm any of the alleged human consequences.[183] The three-volume ESSG study similarly offered only qualified support for the use of herbicides. After the first two volumes were released to *Science*, Deborah Shapley wrote that the ESSG report's "faint praise" was actually quite damning. (She called on Les Aspin, then a Democratic Congressman from Wisconsin fresh from a stint as a Defense Department systems analyst, to decode the "Pentagonese" of the report.) Shapley concluded that despite "the general unanimous pro-herbicide position taken by DOD in public," much internal disagreement existed behind the scenes. Surveys of officials suggested limited enthusiasm and widespread belief that herbicides would not be useful "in future conflicts." Moreover, Shapley concluded that the report's assertion that "At most, the crop destruction program harassed the enemy" was a "Pentagonese" acknowledgment that the program had largely failed. Of course, the same line might also be interpreted as a Pentagon attempt to downplay the destructiveness of a controversial program.

Shapley also offered an insider's view of the internal tensions among the services and the Defense Department's civilian leadership. These factional disputes included Air Force resentment at "offering combat support to the Army" (e.g., dropping herbicides for Army benefit, at great physical cost); Navy reluctance to transport chemical weapons that might accidentally be released into a vessel's closed system; and, perhaps, most importantly,

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[182] For a brief memoir by Lang, omitting the details of his Vietnam work, see Anton Lang, "Some Recollections and Reflections, *Annual Review of Plant Physiology* 31 (June 1980): 1-27.

[183] Buckingham, 189-191. As the NAS researchers had not conducted dioxin testing during their initial visits to Vietnam, they later sent a research team to the Thai spraying sites. See "Vegetation Analysis of the Pran Buri Defoliation Test Area 1," Joint Thai-US Military Research and Development Center, January 1966, and "The Effects of Herbicides in South Vietnam (National Academy of Sciences, 1974), both in the Alvin L. Young Collection on Agent Orange at the National Agricultural Library, http://www.nal.usda.gov/speccoll/findaids/agentorange/text/00019.pdf (accessed 16 July 2010).

significantly different views on herbicides among the Joint Chiefs and the Office of Defense Research and Engineering, who favored their use, and the more skeptical Office of the Secretary of Defense. As with similar debates over the intensity of bombing campaigns, hopes for resolution lay in technological development. Shapley concluded by quoting the responses of "two former DOD analysts" who noted happily that herbicides would soon be outmoded anyway by sensors and new detection technology. They observed: "sensors can provide surveillance of an area without stripping vegetative cover for friendly use…sensors can be delivered or used fairly independent of weather, and…an enemy is not likely to know that a sensor is present, whereas he would be aware of defoliation."[184] The implicit suggestion was that detection technologies, discussed further in the following chapter, provided cover for the shift away from defoliants.

Postwar Controversy

Since the dissolution of the Air Force's Ranch Hand unit and the imposition of the ban in Vietnam, Agent Orange has attracted a controversy far greater and louder than the ecological and political concerns put forth by many scientists during the war years. The Bionetics study spawned extensive new research on dioxin and the potential risks of herbicide exposure to humans. In 1973, Matthew Meselson helped develop highly sensitive tests to detect the presence of small amounts of the substance, and reported that dioxin had been detected in fish and shellfish samples at multiple sites in South Vietnam. Other researchers confirmed the direct links between dioxin exposure and birth defects and chloracne. And although often overlooked by

[184] Deborah Shapley, "Herbicides: DoD Study of Viet Use Damns with Faint Praise," *Science* 177, No. 4051 (1 September 1972): 776-779.

their US counterparts, after the war, Vietnamese scientists also took up the mantle of researching the human and ecological consequences of defoliation.

In 1977, the first Vietnam veterans began reporting a variety of illnesses and disorders they believed were direct consequences of their exposure to herbicides. The following year, a television documentary, *Agent Orange: Vietnam's Deadly Fog,* aired on a Chicago CBS affiliate station, based on multiple cases of sick veterans and veterans whose children had birth defects, as well as interviews with Meselson and Barry Commoner. As Wilbur Scott wrote in a later history of Vietnam veterans: "In one dramatic swoop, Agent Orange went from the private rumblings of a handful of veterans to the center of national attention."[185]

According to Scott, the VA's initial reaction was to try to quell fears, and their hastily assembled Agent Orange Policy Group, populated by former researchers from Monsanto and DuPont, offered a skeptical assessment of the veterans' claims. After conducting their own epidemiology studies, the EPA officially banned 2,4,5-T in 1978. A personal lawsuit filed that year expanded into a class-action affair on behalf of veterans and their families, with seven corporate manufacturers of Agent Orange named as defendants. Arthur Galston provided key explanations of the dangers of dioxins. Much later, Galston would reflect on the difficulties of proving causality in such a case—both in terms of whether exposure to Agent Orange and its dioxins caused the variety of illnesses among veterans, and whether illnesses connected to dioxins stemmed in fact from the specific dioxins contained in Agent Orange, rather than those from another source. Like many scientists at the time, he saw clear links to chloracne and birth defects, which would have affected pregnant Vietnamese, but not necessarily male veterans.[186]

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[185] Scott, 87-89.

[186] Galston, "Falling Leaves": 121.

Nevertheless, after six years of litigation, the lawsuit culminated in a $180 million dollar settlement, which effectively ended claims against the Agent Orange manufacturers. Appeals to the U.S. government and Pentagon-sponsored studies of Ranch Hand veterans and other exposed groups, however, would continue into the twenty-first century, with scientist experts called upon by both sides in the debate.[187]

Morality, Technology, and Expertise

The controversy over defoliant and gas use, the special issues of *Scientist and Citizen* and field reports published in *Science*, the AAAS prodding of Pentagon officials and the heavily critical reaction to the MRI report all revealed two key trends in antiwar scientists' political activity during this period. First was the tendency to emphasize the novelty of the chemicals used and to describe their application as experimental. Such language was consistent with the terms used by Pentagon planners themselves, who had, at the beginning of the decade, referred to Vietnam as a "proving ground" for new weapons technologies. For critics of the war, however, the language of experimentation also evoked images of Nazi doctors and the horrors of World War II. A second related trend was the inclusion of defoliants and tear gas in the category of chemical warfare. Phrases such as "chemical warfare" and "asphyxiating gases" instantly conjured images of World War I, of the "guttering, choking, drowning" men of Wilfred Owen's poetry. Such language targeted the specific and the general—the specific technologies used, and the general horrors of war—but not the righteousness of American involvement in Vietnam.

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[187] For a skeptical view of the connection between Agent Orange and many of the ailments now recognized by the federal government as related, see Michael Gough, "The Political Science of Agent Orange and Dioxin," in Michael Gough, ed., *Politicizing Science: The Alchemy of Policymaking* (Stanford, CA: Hoover Institute Press, 2003).

The writings of Barry Commoner and his colleagues typify this trend. As they wrote in *Science*, "Apart from the morality of the war itself, which is not at issue here, continued use of a weapon with effects that are so poorly understood raises serious moral and political questions for the U.S. government and for the American people."[188] They chose to attack as immoral the technology of the war, rather than attack the war directly. Their commitment to curbing chemical and biological weapons was surely genuine, but it seems doubtful that they—and the scientists so busy circulating petitions and penning letters to the President—were quite so indifferent to the cause and nature of the war itself. Criticizing technology was simply a more effective entryway into Vietnam debates for scientists. From the standpoint of credibility and expertise, a chemist expressing sympathy for Ho Chi Minh could be dismissed by war planners alongside hippies and student protesters as ill-informed, naïve, or an example of the "wild-eyed radical." What did a chemist know of Vietnamese politics? A chemist voicing concerns about chemical weaponry and persistent toxins, however, was far more likely to be taken seriously by policymakers. Scientists opposed to the war were trying, consciously or unconsciously, to make the most of the kinds of influence they had, and media outlets reinforced these tendencies by elevating scientists' commentary about technology over broader critiques of the war.

Yet for these very reasons, antiwar scientists found their arguments self-limiting. Criticizing the morality of the technology of Vietnam meant wading into a morass of ethical ambiguity and murky historical precedent. In their essay in *Scientist and Citizen*, the doctors Sidel and Goldwyn had specifically defined chemical weapons as "agents which produce their effects directly as a result of their chemical properties rather than as a result of blast, heat, or other physical effects of a chemical reaction." Thus, the destruction produced by oxidizing

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[188] *Science* 161, No. 3838 (19 July 1968).

gunpowder or the burning of napalm were not included, and not explicitly criticized. Why? If part of the risk of nonlethal chemical weapons was the potential escalation to lethal forms, why not criticize existing lethal weaponry already used in war? For many scientists, the answer lay in a modification of the classic nuclear nonproliferation arguments: if unchecked, even nonlethal chemical and biological weapons could set a dangerous precedent of use, eventually initiating widespread and uncontrollable dangers far beyond the scope of conventional weapons. In the context of Vietnam, however, warning of the dangers of escalation destabilized support for McNamara's "flexible response." The technical arguments of many antiwar scientists demanded, in essence, gradual escalation without the escalation.

Also problematic for scientist critics was the fact that opposition to chemical weaponry had, for historical reasons, followed an unusual path of development. As Matthew Meselson explained to the Senate Foreign Relations Committee, "the record shows that the governments and peoples of the world have come to practice and expect a degree of restraint against the use of chemical and biological weapons not found for any other class of weapons, except nuclear ones." Meselson attributed this restraint to fears that both technologies could "open up an unfamiliar and highly unpredictable dimension of warfare."[189] But in the aftermath of the Vietnam gas revelations of 1965, Hanson Baldwin, a scholar of World War I, considered the worldwide reaction against gas to be part of "the growth of curious and inconsistent distinctions between weapons systems." Baldwin explained this outcome as due to the persistent and frightening images of "blue-faced men at Ypres, choking to death."[190] Americans dropped fire bombs and

[189] Matthew Meselson, Testimony, 30 April 1969, U.S. Congress. Senate. Committee on Foreign Relations. before Senate Committee on Foreign Relations, *Chemical and Biological Warfare*. 91st Cong, 1st Sess., 30 April 1969, LexisNexis (accessed 7 May 2011).

[190] Hanson Baldwin, "After Fifty Years the Cry of Ypres Still Echoes," *New York Times,* 18 April 1965.

atomic weapons during World War II, but refrained from the use of "inhumane" gas. Besides reported Italian use of mustard gas in Ethiopia, all of the fighting parties during World War II had avoided using gas. To Baldwin, the perceived morality of weapons usage arose from the vicissitudes of historical precedent. Weapons that caused injury through flame, explosion, or force had a long history of use in warfare, and were therefore expected and understood. Weapons that caused harm through invisible mechanisms—toxic gases, bacteria, radiation—were unfamiliar and terrifying. Louis Fieser, the Harvard chemist and inventor of napalm, indirectly promoted this interpretation, proclaiming in 1967 that despite current applications, he felt "no guilt" about his napalm work during World War II, because it was "better than working on poison gas," which he "didn't feel good about… at all."[191]

But examples in other contexts refute Baldwin's claims. In a 1969 article about police departments' use of mace, Seymour Hersh described reactions to the practice as a kind of "index of popular opinion." For example, "white liberals" expressed outrage at the use of gas against civil rights protesters at Selma, but went silent when the same chemical was used to restrain white protesters blocking James Meredith's enrollment at the University of Mississippi.[192] The context mattered more than the usage itself. The activism of scientists and the public at large during the Vietnam War reflects this point. Of the controversial weapons technologies and tactics used, almost all—napalm, intensive aerial bombardment, and tear gas—had been employed

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[191] While Fieser opposed napalm's use "against babies and Buddhists," he didn't consider the technology itself to be intrinsically immoral, like that of poison gas. He told one audience, "The person who makes a rifle…he isn't responsible if it is used to shoot the President." Clipping, *Sunday Herald Traveler,* 13 November 1967, "Retirement dinner, 1967" Folder, Papers of Louis F. Fieser and Mary P. Fieser, Box 1, HUG(FP)20, Harvard University Archives.

[192] Seymour Hersh, "Your Friendly Neighborhood MACE," *New York Review of Books,* 27 March 1969. In 1955, Eugene Rabinowitch offered yet a third assessment in the *Bulletin of the Atomic Scientists,* writing, "Poison gas is a cumbersome, relatively ineffective and indecisive weapon," whereas atomic weapons "are immensely effective, easily transportable, and potentially decisive." Eugene Rabinowitch, "Living With H-Bombs," *Bulletin of the Atomic Scientists,* 1 January 1955.

during World War II and the Korean War without provoking the intense outcry characteristic of the Vietnam era. The use of herbicides and crop destruction in war was new for the United States, but it is plausible that had chemical herbicides been used against Nazi-supporting farms, domestic criticism in 1945 would likely have been far more muted than what ensued in the 1960s and 1970s.[193]

At the same time, the practice of scientists drawing on their credibility as experts to criticize weapons technology had its own strange path of development. It was a form of political intervention honed in the aftermath of the Manhattan Project, and its reappearance during the Vietnam era reflected the influence of that extraordinary precedent. The idea that scientists held a unique authority—an *obligation*—to comment on weapons technologies was evident in Arthur Galston's reflection that with the use of defoliants in Vietnam, "the botanist, probably the last of the scientific innocents, was unexpectedly catapulted into the same ethical hot pot as other scientific colleagues."[194] It was evident in Milton Leitenberg's warning in *Scientist and Citizen* that "As physicists learned to create destruction with nuclear weapons, microbiologists are learning to create disease with biological weapons."[195] And it was evident in the deep ethical concerns described by Sidel and Goldwyn in the *New England Journal of Medicine.* A doctor asked to help develop biological weapons, they wrote, "must carefully evaluate his own attitudes toward the rights and duties as a citizen and as a doctor." They warned of the dangers of inaction, invoking the awful specter of Nazi science and citing an AMA editorial mourning "the failure of

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[193] Alvin Young, the Air Force colonel who later wrote a detailed account of the herbicide research program, observed this trend, from a critical perspective, in a 1989 interview. He recalled: "The agenda [of the 1969 visiting AAAS members] was not an agenda that talked about the health of the Vietnamese people. Their agenda was… they were wanting the military of the United States out of Vietnam…" Quoted in Scott, 82.

[194] Galston, "Falling Leaves": 108.

[195] Milton Leitenberg, "Biological Weapons," *Scientist and Citizen,* August-September 1967: 166.

German medical organizations and societies to express in any manner their disapproval" of Nazi experimentation. If Arthur Galston saw his early work on plant hormones as the ethical impetus for action, the two doctors felt the weight of the Hippocratic oath in a similar way. They closed their article with an invocation of lines from the famous oath: "…Neither will I administer a poison to anybody when asked to do so, nor will I suggest such a course…"[196] Their authorship of the article suggested a third obligation: to speak out against others' administration of perceived poisons.

Manhattan Project scientists had not opposed fighting World War II, nor was every scientist who worried about chemical weapons intrinsically criticizing American involvement in Vietnam. But for those who parlayed antiwar sentiment into allegations of immoral technology, the policy implications of their strategy turned out to be extremely limited: beginning at the end of the decade, in the face of expanding scientific opposition and growing evidence of the risks of human exposure to the dioxin-laced Agent Orange, wartime policies regarding herbicides eventually began to change. But the war itself continued unabated, spilling outward into Cambodia and Laos, and fueling deeper and more radical modes of protest.

There were, of course, some exceptions to this trend, as many outspoken scientists did denounce the war itself, without manufacturing technological or scientific justifications for their political opposition. And another, broader variant of the argument came from scientists connected to the growing environmentalism movement. These researchers were more likely to criticize the overall effects of war, including not just the use of chemicals but the impact of all aspects of battle. For example, Michael Newton, a forestry researcher affiliated with Oregon

[196] Victor Sidel and Robert Goldwyn, "Chemical and Biological Weapons—A Primer."

State University, wrote in a 1968 letter to *Science* that "The philosophical argument against the use of unsolicited biological agents is understandable. But such tactics should not be criticized on the basis of genocidal, biocidal, ecological, or economic considerations because the land and the organisms it supports will recover from such treatment more quickly than from various other instruments of war, and with far less pain. Wouldn't it be more constructive to recommend ways of making the use of all such instruments unnecessary?"[197] Pfeiffer and Orians offered a similar acknowledgment in a 1970 article based on their research trip to Vietnam, noting that "Although it has not attracted the concern of American scientists, the damage caused by raids with B-52 bombers is of considerable ecological significance."[198] Arthur Westing, a botanist at Windham College in Vermont, traveled to Vietnam four times from 1969 to 1973 to research the ecological effects of the war, and published his results though the Stockholm International Peace Research Institute (SIPRI) in 1976. He investigated not just chemical use, but also "mechanized landclearing," fire, flood, use of explosives, and other destructive forces, all of which resulted in the "severe abuse" of the Vietnamese ecosystem, a process he and others termed "ecocide." Westing closed his report with some philosophical reflections, including a criticism of the human tendency to consider the environment only in terms of "anthropocentric concerns"—in other words, the tendency to care about protecting the environment in order to ensure human needs and comforts. He then asked, "But should not living things, and nature as a whole, have some level of immunity in their own right?" Were they not "'noncombatant bystanders' to man's martial foibles? What, indeed, is man's fundamental relationship to the land?"[199] These views were

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[197] Michael Newton and L.T. Burcham, "Defoliation Effects on Forest Ecology," *Science* 161 (12 July 1968): 109.

[198] E. W. Pfeiffer and Gordon H. Orians, "Ecological Effects of the War in Vietnam," *Science* 168, No. 3931 (1 May 1970): 552.

[199] Arthur Westing, *Ecological Consequences of the Second Indochina War* (Stockholm: Stockholm International Peace Research Institute, 1976), 80-89.

echoed by Barry Weisberg, who in 1970 put together a book entitled *Ecocide in Indochina: The Ecology of War*, drawing on Arthur Galston's definition of ecocide as "the willful destruction of the environment."[200]

Although their concerns about ecocide had not seemed to resonate with the government and military planners running the war, scientists like Westing nevertheless continued their research on Vietnam, following up on the environmental toll in the decades afterward. In 1983, Westing worked with SIPRI to organize an "International Symposium on Herbicides and Defoliants in War," held in Ho Chi Minh City and including 72 scientists—ecologists and physiologists—from 20 countries, plus a contingent of 56 Vietnamese scientists. The book that resulted from the proceedings, *Herbicides in War*, published in 1984 and edited by Westing, documented the ecological and human consequences of defoliant use. The short-term consequences of spraying were organized by ecosystem. In "dense inland forest" areas, effects included soil erosion, loss of animal habitats, and replacement of destroyed vegetation by opportunistic grasses. In the mangrove swamps along the southern coast, SIPRI scientists reported that "Virtually nothing remained alive after even a single herbicide attack and the resulting scene was weird and desolate." Unlike the inland forest area, these defoliated regions were not even colonized by aggressive grasses or bamboos after the initial devastation. SIPRI described mangroves as "clearly the ecosystem most seriously affected" by the war; their loss hastened erosion and resulted in declines in the fish and shellfish populations. And of course, the SIPRI-gathered scientists noted the presence and persistence of dioxin, and described a host of ailments plaguing humans exposed to defoliants, including: allergies, temporary nausea,

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[200] Barry Weisberg, *Ecocide in Indochina: The Ecology of War* (San Francisco: Canfield Press, 1970). This kind of "deep ecology" can itself be considered a new kind of ethics. See David B. Resnick, *The Ethics of Science: An Introduction* (New York: Routledge, 1998).

headaches, and respiratory problems, as well as exacerbation of existing illness and malnutrition. Defoliation had also resulted in mass human displacement and the increased spread of disease.[201]

Not all scientists who had criticized defoliant use in Vietnam shared this commitment to ecological preservation. On the concept of ecocide, Arthur Galston himself later reflected that "This term was coined to evoke the specter of the parallel crime of genocide, justly condemned after the Nuremburg trials."[202] But in Vietnam, he noted decades later, defoliation "did not permanently destroy the productivity of the ecosystem," although it did considerable damage, resulting in soil erosion, destruction of mangroves as fish habitats, and the proliferation of "junk" vegetation like bamboo overtaking former teak habitats. Galston also noted the unintended consequences of food destruction operations: the accidental destruction of H'mong crops, which led to exodus from "ancentral homelands" and in many case, emigration to the United States. But Galston was blunt and honest: despite his own deep discomfort with herbicide use, he had to concede that it had not been ecocidal, and that, despite all its ecological damage, "the herbicidal campaign in Vietnam produced certain military advantageous results which may, in the end, have justified their use in a bitterly contested war." The costs had been steep, though, wrote Galston, steep enough to make repeat use highly unlikely. This prospect seemed a source of great relief.

Credit and Blame

In the mid-1980s, Ranch Hand veteran Paul Cecil wrote in his history of the operation that its demise in 1971 was a result of "internal and external political pressures on the US

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[201] Westing, ed., *Herbicides in War*, 9-15.

[202] Galston, "Falling Leaves": 122.

government."[203] CINCPAC's Admiral John McCain Jr., the father of the 2008 presidential candidate, blamed two overlapping groups: scientists and antiwar radicals.[204] J.B. Neilands, the biochemist who co-founded the Scientists' Committee on Chemical and Biological Warfare (SCCBW), credited "the science community and the Congress" with reform, particular the New York delegation of Bertram Podell, Richard McCarthy, and Ed Koch. [205] Michael Gough, a biologist who served at both the VA and the Congressional Office of Technology Assessment during the peak of the postwar Agent Orange controversy**,** surmised that the Pentagon had abandoned the defoliant due to dioxin's link to cancer and allegations of chemical warfare. In a more comprehensive evaluation, William Buckingham attributed the shift in policy to several factors: a reduction in spraying in line with Nixon's stated goal of "reducing the American presence in Vietnam," evidence linking Agent Orange to birth defects, and the mounting criticism from scientists, particularly the members of the AAAS's visiting team of scientists, whom Buckingham credited with helping "to hasten Ranch Hand's demise."[206]

Whether it was the allegations made by an international chorus of critics that included both scientists and non-scientists, the revelations about dioxin, the meeting between Galston and DuBridge, general public disapproval, or the Congress-ordered Pentagon review that was most responsible for the shift in policy is hard to determine. But one conclusion is clear: scientists' carefully-articulated concern during the earlier years of the war—focusing almost exclusively on ecology and the risks of escalation—did not halt the practice of defoliation in Vietnam. It did

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[203] Paul Cecil, *Herbicidal Warfare: The Ranch Hand Project in Vietnam* (New York: Prager, 1986), 1.

[204] Buckingham, 166-167.

[205] Neilands cites this group specifically in his assertion that opposition to chemical use in Vietnam came largely from two sources: "the science community and the Congress." Neilands, "Vietnam: Progress of the Chemical War."

[206] Buckingham, 160, 169.

spur the significant political pressure from Congress, but it was not as influential as scientists' later research and concern regarding the links between Agent Orange and dioxin, and dioxin to birth defects, combined with Pentagon acknowledgment of the mediocre efficacy of the defoliation program itself, both of which contributed far more significantly to the change in policy.

This outcome may have been due in part to the particular character of the debate over the weapons systems employed in Vietnam. Unlike earlier arguments about the hydrogen bomb or the enforceability of a test ban, the controversy over defoliants and gases never included questions of feasibility, for which scientific input could more easily shape political concerns. Instead, the CBW issues raised by scientists were either moral in nature or rooted in the murky field of ecology, with its difficult and sometimes speculative predictions about the complex ecosystems of South Vietnam, for which little background knowledge existed in the United States. Advocates of defoliation and tear gas use were able to circumvent the broader moral arguments through the ratification and exemptions of the Geneva Protocol, and they were able to find numerous researchers with more charitable interpretations of the complicated ecological data being collected. Agent Orange, once it was linked to birth defects, was quickly eliminated, but the use of alternative herbicides continued. The conclusions of the in-house studies conducted by Minarik, Tschirley, and others were decidedly less ominous than the outsider studies of Pfeiffer and the AAAS. For every cry of ecocide, there was an internal scientist with a confidently positive assessment for State Department and Pentagon bosses, a welcome confirmation that "The effects of defoliation have not been as disastrous as anticipated."[207] By

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[207] Quoted in report from Saigon Embassy, 1968, in Herbicide Policy Review, Alvin L. Young Collection on Agent Orange at the National Agricultural Library, available online at http://www.nal.usda.gov/speccoll/findaids/agentorange/text/03124.pdf (accessed 7 May 2011).

the time the longitudinal studies detailing the exact consequences could be completed, the war and any opportunities to change its tactics would be long over.

Chapter Three: Advising the Pentagon

While biologists and chemists inside, outside, and on the peripheries of government developed and debated the chemicals used in Vietnam, the handful of elite physicists who had offered their services through the President's Science Advisory Committee and other key groups were also hard at work. The advisory committee continued with almost identical membership through the administrations of Eisenhower, Kennedy, and Johnson. The topics they were asked to address, however, changed substantially as the war in Vietnam escalated. The Institute for Defense Analyses' Jason group of physicists, who stood with one foot in academia and one in the Pentagon, also made the transition from nuclear concerns to problems of counterinsurgency and limited war in Vietnam. At the same time, the ranks of in-house military and industrial weapons workers—including scientists, engineers, and technicians—swelled above previous Cold War levels, as the prolonged "hot war" demanded a constant diet of new supplies and technological innovation. These transitions occurred just as the political influence of scientists was itself being tempered by the accession of Lyndon Johnson, a president less involved in science affairs than his two predecessors, and as rising tensions emerged among key decision-makers, including, most critically, McNamara and the Joint Chiefs of Staff.

This chapter assesses the contributions of three overlapping groups of scientists during the Vietnam War: the PSAC, in-house military scientists, and the Jasons. Of the three, the Jasons most exposed themselves to the dangers and delusions lurking in the new opportunities for wartime advising. Their own ambitions, self-criticism, and unwanted notoriety reveal the painful ethical reckoning confronting scientists as they faced their own contributions in the final years of a deeply unpopular and devastating war. Moreover, the trajectory of the Jasons during the war involved more than personal ethical crises for many of its members. It also affected the trajectory of the war itself, and as will be related in subsequent chapters, it fueled a deep and angry backlash among fellow scientists, one that would have profound implications for the character of academic science in the 1970s.

The Jasons, Part I

Whereas the Jasons' work in the early 1960s had been wide-ranging, with an emphasis on the technologies necessary to enforce a nuclear test ban—VELA detection techniques for multiple environments, for example—by 1964 problems of counterinsurgency were appearing with more frequency on the agendas for the famed summer sessions in La Jolla and Cape Cod. The Institute for Defense Analyses (IDA) had also shifted focus, producing a new round of reports on topics such as "Human Smog as an Ambush Detector" and "Power Sources for Remote-Area Counterinsurgency," and organizing an IDA-wide "show-and-tell meeting on counterinsurgency." At the Jason meeting in the spring of 1964, the familiar sessions on missile technologies and deterrence took place, but a new panel, headed by William Nierenberg, addressed issues of counterinsurgency and limited war. The group listened to reports from White House representatives and Advanced Research Projects Agency (ARPA) personnel explaining Project AGILE and "remote area conflict." Seymour Deitchman, the physicist and limited war advocate from the Pentagon's Defense Research and Engineering department, presented a personal account of his recent trip to Vietnam.[1] Bernard Fall, though not a scientist, was a member of the panel.

[1] Goldberger correspondence, September 1964, Box 35, Folder 5, Murray Gell-Mann Papers, 10219-MS, Caltech Archives, California Institute of Technology (hereafter MGM). Other members of the Nierenberg panel included: Orlansky, Blumstein, Enke, Deitchman, Holmberg, de Sola Pool, Fall, Raylor, Weinecke, Vallance, Gell-Mann. And also MacDonald: According to a 1984 reflection by Gordon MacDonald, the earliest Jason attention to Vietnam-

Berkeley physicist Kenneth Watson later recalled that the Jasons' involvement in Vietnam began in the summer of 1964, "before the actual war," and was initially "a marginal thing." Bernard Fall offered lectures on Vietnamese culture and history throughout the summer session, but to Watson, "it was all background."[2] For other Jason members, however, 1964 marked the start of more serious involvement. Throughout that spring, Jason administrator David Katcher forwarded reports on counterinsurgency in southeast Asia to panel members Seymour Deitchman and Murray Gell-Mann.

The gradual inclusion of counterinsurgency and limited war topics coincided with a decrease in enthusiasm among Jason members. In September of 1964,Caltech's Marvin Goldberger wrote to Jasons on the occasion of the group's sixth anniversary. He acknowledged that "it is hard to put aside one's regular academic research and teaching duties during the course of the year," but nevertheless worried that it was becoming increasingly difficult "to get people to take initiative to get involved and stay involved." Goldberger and others discussed the creation of new programs to attract younger scientists, or "junior Jasons," such as post-doctoral fellowships, partnerships with Jason mentors, and consulting opportunities. Goldberger seemed unsure whether the problem was a lack of youthful enthusiasm, or deeper ambivalence about the relevance or appropriateness of the group's projects. He offered a last-ditch pep talk: "Jason has

related problems took place "during the summer study of 1961 at Bowdoin College, where Murray Gell-Mann led a small study group." Gordon MacDonald, "JASON and the DCPG—Ten Lessons" (speech delivered at the Jasons' 25th Anniversary Celebration),30 November 1984, Box 37, Folder 12, MGM.

[2] Interview of Kenneth Watson by Finn Aaserud on 10 February 1986, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA, by Finn Aaserud, http://www.aip.org/history/ohilist/4939.html (accessed 2 May 2011).

nothing to be apologetic about. We have made definite contributions and these are widely acknowledged in the defense community."[3]

Whether actual ambivalence existed or not, Jason members acknowledged among themselves the controversial aspects of their work. As the Nierenberg Panel prepared its final report on counterinsurgency, panelists debated how widely to distribute the document. Katcher reported that Harold Hill of ARPA wanted the Jasons' explicit approval of distribution plans, because "the nature of the work would make the group vulnerable to criticism." In Katcher's view, though, "As far as I can tell, no one is ashamed of this paper and would want it hidden from view. … After brooding for a short while I called him back to make sure he himself had no strong feelings that there were positions in it which no reasonable man could support (I am aware of the paradox.)."[4]

Katcher did not attribute the Jasons' malaise to the shift toward counterinsurgency and limited war. In a 1966 letter, he linked falling productivity to boredom, noting behavior that was surprisingly "passive for a group of individuals selected because they aren't." He hoped organizational shakeups might help, whether in the direction of decentralization or greater institutionalization.[5] Other explanations were in the air, however. In July of 1965, Jason members and IDA's Jack Ruina had discussed ways to encourage the creation of a British equivalent of Jason. They acknowledged that nuclear war technology and strategy had drawn the first wave of promising young physicists to Jason: "The problem of antimissile defense was in this country one of the powerful forces that motivated busy young scientists to take time out for

[3] Memo, Goldberger to Jason members, 30 September 1964, Box 35, Folder 5, MGM.

[4] Memo, Katcher to Nierenberg, 31 March 1965, Box 35, Folder 6, MGM.

[5] Katcher to Jason steering committee, 22 January 1966, Box 36, Folder 1, MGM.

Jason work."[6] But in 1965, antimissile defense was arguably no longer the top area of research for the Jasons. Kenneth Watson later recalled that from 1965 to 1969, work connected to the war in Vietnam became "a predominant Jason activity," and "a major Jason effort."[7]

Watson's recollection confirms the impact of Vietnam on the organization's sense of work and mission, but whether it truly 'predominated' is difficult to gauge. Of the more than forty Jason publications in 1965-1966, the majority concerned problems of nuclear weaponry: missile penetration, effects of nuclear explosions, and aspects of anti-ballistic missile technologies. Six items issued under the heading "Remote Area Conflict" were clearly connected to the war in Vietnam: "Tactical Nuclear Weapons in Southeast Asia" (S-266); "Interdiction of Trucks from the Air at Night" (P-289); "Manned Barrier Systems: A Preliminary Study" (P-322); "Air Sown Mines" (P-315); "Tactical Nuclear Weapons in SE Asia; Questions Requiring Further Study" (N-406); and a top secret assessment, "Air Supported Anti-Infiltration Barrier (C)" (S-255), prepared by Deitchman, Gell-Mann, Nierenberg, and a dozen others.[8] The first and last of these reports would later draw enormous attention and controversy, though they constituted less than five percent of Jasons' output that year.

Kistiakowsky

A year after he had campaigned for the president with Scientists and Engineers for Johnson, the Harvard chemist George Kistiakowsky, Eisenhower's former science advisor,

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[6] Sharp and Wheeler to Goldberger and Ruina, 14 July 1965, MGM.

[7] Interview of Kenneth Watson by Finn Aaserud.

[8] "Jason Publications" 1965-1966, Box 35, Folder 6, MGM. In MGM documentation, S-255 is referred to only as "…Barrier," but the complete report, declassified in 1990, is available at the LBJ Library and online at http://www.dtic.mil/cgi-bin/GetTRDoc?AD=ADB954899&Location=U2&doc=GetTRDoc.pdf (accessed 2 May 2011).

watched with alarm as the war escalated dramatically. 1965 saw the intensive bombing of North Vietnam, the deployment of hundreds of thousands of US combat troops, and the first heavy American casualties. The day after reports of gas use appeared in nearly every major American newspaper, Spurgeon Keeny, formerly a technical assistant to James Killian and an arms control advocate within the Johnson administration, appeared at a Harvard seminar on Science and Public Affairs organized by Carl Kaysen. Afterwards, he met with Kistiakowsky, who informed him that a number of distraught faculty members were organizing among the old Scientists and Engineers for Johnson group to send a letter to the president expressing opposition to current Vietnam policy. Kistiakowsky confided in Keeny that he sympathized with the activists, even though he had declined to take a leadership role in their effort. He offered to meet with McGeorge Bundy to describe and explain Cambridge attitudes toward the war. Reporting the conversation to Bundy the following day, Keeny repeated Kistiakowsky's characterization of the activists as "responsible, sober citizens and not the Alex Rich and Bernie Feld variety." Keeny urged Bundy to meet with Kistiakowsky, both to allow Kistiakowsky an outlet "to unburden himself" and to allow Bundy the opportunity personally to present the Administration's position to Kistiakowsky. Keeny emphasized the value of Kistiakowsky's opinion: "I think he could be very influential in keeping some members of the Cambridge community from straying too far off the reservation."[9] A few disgruntled scientists could be marginalized easily as 'wild-eyed' cranks, but a broad, organized coalition of Harvard and MIT scholars could pose a serious political problem.

In 1965, Kistiakowsky was still receptive to personal outreach from key administration officials, and he was himself an important insider liaison to outsider scientists. But he too would

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[9] Memo, Spurgeon Keeny to McGeorge Bundy, 24 March 1965, "Gas, Vol I" Folder, Box 194, Country File, Vietnam, National Security File, LBJ Library.

soon stray 'far off the reservation.' In January 1966, Kistiakowsky wrote a lengthy letter to President Johnson, invoking his status as one of the president's "Advisors on Foreign Policy" and urging de-escalation, possibly through a new strategy of establishing secure noncommunist "enclaves" around Saigon. His suggestions were curtly rebuffed by McGeorge Bundy. Reaching out to likeminded colleagues at Harvard and MIT, Kistiakowsky formed the "Cambridge Discussion Group," as an alternate means to promote de-escalation. Fellow member John Kenneth Galbraith described the informal organization to President Johnson as "a group of your well-wishers and supporters here, several on the scientific side derived from the original Scientists and Engineers for Johnson," who "have been meeting on the problems of Viet Nam." Galbraith assured Johnson that the group's purpose "is assistance and not criticism." Other members included Jerome Wiesner and Jerrold Zacharias of MIT, Frank Long, Carl Kaysen, and Richard Neustadt.[10]

That spring, the Cambridge intellectuals began meeting with key Pentagon representatives to discuss the possibility of a special summer session to address possible technological solutions to the war, including the feasibility of a kind of anti-infiltration barrier to prevent Viet Cong entrance into South Vietnam, an idea that had earlier been broached by Harvard Law professor Roger Fisher to John McNaughton. Although initial military reactions to the barrier idea had been unenthusiastic, McNamara himself later met with members of the

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[10] For example, see "A Free Zones Policy for Vietnam," March 1966; "Draft Paper by GBK," 23 February 1966; "My Involvement in DCPG," June 1968; and Bundy to Kistiakowsky, 25 February 1966, in "Vietnam, 1963-1968" Folder, HUG (FP) 94.18 (Vietnam War, ca. 1963-1973), Papers of George B. Kistiakowsky, Harvard University Archives (hereafter Kistiakowsky). Kistiakowsky recounted this correspondence in the document "My Involvement in DCPG," in which he explained that he had sent his letter to the president through Bill Moyers, so as to bypass McGeorge Bundy. Kistiakowsky considered Bundy's eventual reply to be "extremely curt and deliberately insulting."

Cambridge Discussion Group to seek their advice.[11] Simultaneously, at the Jason spring meeting on the west coast, Nierenberg was organizing a group to study technical approaches to interrupting transit through the Ho Chi Minh Trails.

On April 15, 1966, Zacharias wrote to Gell-Mann to inform him that he and his Cambridge colleagues had been meeting with Defense Department officials to discuss "a special study of the military and technological options open to the U.S. in Vietnam." The catalyst, according to Zacharias, was frustration with the escalation of the war and the heavy death toll. "Our hope," he wrote, "is that by re-examining the present military tactics, especially in the light of technological opportunities that may not have been adequately considered, military alternatives might emerge that would be less costly and more likely to lead to a political solution." He invited Gell-Mann to join the group for an exploratory meeting, to be held in the penthouse of the MIT Faculty Club in early May.

An attached draft proposal clarified the group's goals more precisely. In order to "enhance the probability of achieving military objectives," the group should evaluate a variety of military options, including further escalation, continuation of current policy, or de-escalation (in various forms), drawing on potential new innovations in communications, transportation, reconnaissance, weapons systems, and "geographical barriers (the possibility of "sealing off" South Vietnam)." Ideally, a six-member steering committee would lead a team of up to fifty scientists and engineers during a multi-week summer study. Recipients of this proposal included

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[11] Paul Dickson, *The Electronic Battlefield* (Bloomington: Indiana University Press, 1976), 20-21; Gordon MacDonald, "JASON and the DCPG—Ten Lessons" (speech delivered at the Jasons' 25th Anniversary Celebration),30 November 1984, Box 37, Folder 12, MGM; Galbraith to LBJ, 19 April 1966, in "Cambridge Discussion group" Folder, HUG (FP) 94.18, Kistiakowsky. Kistiakowsky's 13 January 1966 letter in the same file recommended that the US "pursue an essentially defensive strategy that would rest on the establishment and securing of suitable enclaves along the cast and around Saigon…"

Luis Alvarez, Seymour Deitchman, Eugene Fubini, Richard Garwin, Murray Gell-Mann, Killian, Kistiakowsky, Land, Lauritsen, Rabi, and Wiesner.[12]

Zacharias's invitation appealed to scientists who, like him, hoped to contribute to a de-escalation of the war. For those with deeper objections to the war itself, the prospect of the summer study offered only small hope, and stirred other, complex concerns. Such views were conveyed with devastating clarity by George Rathjens, then head of the IDA's Weapons System Evaluation Division. In a letter to Kistiakowsky in early April 1966, Rathjens expressed interest in the summer study, but concluded with a painful assessment of his frustration and his ethical reservations about both the war and his tenure at IDA:

> I am extremely upset about the whole Viet Nam business. This has been one of the things that has made the IDA job so difficult for me. …[Last summer] if I had then foreseen our getting involved in Viet Nam as we have (and my reacting as I have) I probably wouldn't have done it. I now have the feeling that I am to a substantial degree an instrument for a policy with which I am very much in disagreement but which I have damned little chance of influencing. The bombing of the North just about brought me to the point of resigning despite my commitment to stay two years, but I have stayed on arguing with myself that I have, or may have, more opportunity to influence things than if I left abruptly. But the hell of it is that I'm not sure whether this is really right or just a rationalization for doing the easy thing. Events since the resumption of bombing, and most recently the Rostow appointment, haven't made things any easier. I am writing all this because I want you to know that I do feel very strongly about this business and would welcome the opportunity to help in any way I can in your efforts. Unfortunately, I am afraid I am least qualified to help with respect to the political and moral aspects of the problem where I think we are most clearly in error, and qualified, if at all, only on the military side… I feel awfully frustrated and uncomfortable about not being able to find some mechanism for trying to affect what we're doing…[13]

Kistiakowsky replied with appreciation and honesty. "As you know I feel about Vietnam just like you do," he wrote. But he planned to attend the summer study, explaining, "I personally

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[12] Zacharias to Gell-Mann, 15 April 1966, Box 36, Folder 1, MGM.

[13] Rathjens to Kistiakowsky, 6 April 1966, in "Cambridge Discussion group" Folder, HUG (FP) 94.18, Kistiakowsky.

have considerable misgivings about anything useful coming out of that kind of undertaking but will participate to the best of my ability because I know of no obviously better alternative."[14]

With Kistiakowsky's qualified enthusiasm, Nierenberg's Jason panel and members of the Cambridge group (now dubbed "Jason East") quickly consolidated their efforts, arranging for two two-week IDA-sponsored summer sessions to be held at the Dana Hall private school in Wellesley.[15] McNamara requested that among the other projects, the scientists specifically address the prospect of "a fence across the infiltration trails, warning systems, reconnaissance (especially night) methods, night vision devices, defoliation techniques and area denial weapons."[16] But the scientists' hopes for altering the course of the war went beyond plans for a barrier. As one attendee wrote to his colleagues, "The substantive question is, considering the overwhelming philosophical power assembled in Wellesley, whether or not some effort should be made to enlist outstanding physiologists, biochemists etc. in order to explore the possibility that a 'Manhattan District' effort could not in fact produce an effective system in time to stop this war in Vietnam..."[17] Such a sweeping project never materialized; instead, the summer sessions were devoted to studies of the bombing campaign and the design for a potential anti-infiltration barrier.

Three Reports and Three Reactions

1. Bombing

[14] Kistiakowsky to Rathjens, 21 April 1966, in "Cambridge Discussion group" Folder, HUG (FP) 94.18, Kistiakowsky.

[15] Ruina to MGM, 3 May 1966, Box 36, Folder 1, MGM.

[16] Dickson, 22.

[17] Memo, Lederman to Zacharias et al, 24 June 1966, in "Vietnam, 1963-68" Folder, HUG (FP) 94.18, Kistiakowsky.

The Dana Hall summer meeting resulted in a series of Jason reports related to Vietnam, three of which—concerning the bombing of North Vietnam, the construction of the 'electronic barrier' to cut off the Ho Chi Minh Trails, and the use of tactical nuclear weapons—later became subjects of intense criticism and controversy.

The Jasons' first study assessed the effect of Operation Rolling Thunder, the campaign of aerial bombing in North Vietnam that adhered strictly to McNamara's concept of gradual escalation. As John Lewis Gaddis has written, "The bombing campaign against North Vietnam was intended to be the most carefully calibrated military operation in recent history."[18] Over the objections of military leaders who preferred more intensive bombardment, Rolling Thunder began under heavy restrictions, many which would be gradually lifted over time. Gaddis estimates that "the scale and intensity of the bombing progressively mounted, from 25,000 sorties and 63,000 tons of bombs dropped in 1965 to 108,000 sorties and 226,000 tons in 1967." But in Gaddis's estimation, "None of it produced discernible progress toward what it was supposed to accomplish: a tapering off of infiltration into South Vietnam, and movement toward negotiations."[19]

It was in the context of this failure of Rolling Thunder, combined with the large-scale introduction of ground troops beginning in 1965, that the Jasons undertook their evaluation of the effectiveness of the bombing campaign. The effort would eventually result in two reports, the second of which, a weighty four-volume affair, offered the more thorough analysis and assessment. But it was the first, completed in August 1966 and clearly intended to affect policy

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[18] John Lewis Gaddis, *Strategies of Containment, Revised and Expanded Edition* (New York: Oxford University Press, 2005), 245.

[19] Gaddis, 245. For broad histories of the Vietnam War, see Marilyn Young, *Vietnam Wars, 1945-1990* (New York: Harper Perennial, 1991) and John Prados, *Vietnam: The History of an Unwinnable War, 1945-1975* (Lawrence: University Press of Kansas, 2009).

quickly, that created controversy and exacerbated tensions between scientists and military leaders.

The result, titled "U.S. Bombing in Vietnam: The Effects of U.S. Bombing on North Vietnam's Ability to Support Military Operations in South Vietnam and Laos: Retrospect and Prospect," was based on the "extensive official analyses" of the US military and intelligence communities and supervised by Carl Kaysen and E. Bright Wilson of Harvard. In it, the scientists analyzed the outcome and effectiveness of the bombing campaign, as well as the underlying logic and assumptions behind it. The report reached five major conclusions: that the bombing had had "no measurable effect on Hanoi's ability to mount and support military operations in the South at the current level"; that aid from China and the Soviet Union had offset the damages caused by bombing; that escalating and expanding the scale and scope of the bombing would be unlikely to alter North Vietnam's ability to support military actions in the South; that it would be difficult if not impossible to define a damage level at which North Vietnam would capitulate; and that in the face of a year of bombing, Hanoi's infiltration into the South had actually accelerated.

In providing the context for their work, the Jasons noted astutely that they were intervening in an ongoing disagreement between the Pentagon's civilian leadership and top military brass. As the bombing had progressed thus far, McNamara's strategy of graduated escalation had formed the essential premise of Rolling Thunder:

> The basic U.S. strategy underlying this decision stemmed from an assumption that such graduated military pressures directed systematically against NVN's ability to support the insurgencies in South Vietnam and Laos might cause the Hanoi regime to yield and enter into a negotiated settlement. … This whole sequence of military actions was designed to give the impression of a steady, deliberate approach and to give the U.S. the option at any time—subject to enemy reaction—to proceed or not, to escalate or not, or to quicken the pace or not. Concurrently, the U.S. would be alert to any sign of yielding by Hanoi, and would

> be prepared to explore negotiated solutions that attained U.S. objectives in an acceptable manner. Reprisal strikes against selected North Vietnamese targets could be undertaken at any time in response to Viet Cong provocations in South Vietnam, and such reprisal strikes might also be used to initiate the program of graduated pressures.

The Jasons acknowledged that this strategy of graduation had been adopted over the objections of the Joint Chiefs of Staff, who had called for a campaign using “the full limits of what military actions can contribute.” Instead, Rolling Thunder began with limited strikes on military targets, and gradually escalated to include a wider array of targets, lasting until the bombing pause of December 1965. During the pause, the entire campaign was evaluated, with the general military and government consensus that it had failed to deter North Vietnam’s activities in the South. The Joint Chiefs blamed the failure on the limited nature of the bombing, arguing instead for a vastly increased campaign. But when the evaluation period ended, only a limited resumption of bombing began, on January 31. As the Jasons were writing the report, considerable disagreement still existed between the Joint Chiefs, who wanted to target petroleum resources, transportation systems, economic and industrial sites, and to mine the major harbors, and the intelligence community, who were, in the words of the Jasons, “pessimistic about the prospect of achieving a level of interdiction that could significantly reduce the flow of essential war materials through North Vietnam or even prevent the flow from reaching higher levels than in recent months.” The North Vietnamese, intelligence officers suggested, were too good at compensating, improvising, and finding alternate routes for southern assistance.

The Jasons largely sided with the intelligence experts, agreeing that further bombing would be disruptive, but likely insufficient to achieve Rolling Thunder’s original goals. The Jasons observed accurately that “The Joint Chiefs of Staff have never fully accepted the strategy of ‘graduated escalation’ that was finally adopted for the air attacks on North Vietnam.” While the scientists agreed that the gradual escalation of bombing had failed, they still considered the Joint Chiefs' current request for "the swift application of ample military force" to be misguided. North Vietnam was not a "complex industrial society" vulnerable to attacks on its infrastructure; rather, it was "relatively primitive," so that the assumptions underlying the proposed campaign of the Joint Chiefs would have "little applicability."

Despite their agreement with the conclusions of the intelligence reports, the Jasons also argued that the analysis and methodology of the intelligence community and Pentagon planners was deeply flawed. The scientists were particularly skeptical of the assumption that key aspects of the war could be calculated and predicted with any meaningful reliability. For example, they wrote, intelligence analysts tended to evaluate the bombing campaign in isolation, without considering it in concert with other operations, such as the trajectory of the war in the South. As a result, "The fragmented nature of current analysis and the lack of an adequate methodology for assessing the net effects of a given set of military operations leaves a major gap between the quantifiable data on bomb damage effects…and policy judgments about the feasibility of achieving a given set of objectives." The Jasons concluded damningly that "there is currently no adequate basis for predicting the levels of US military effort that would be required to achieve the stated objectives—indeed, there is no firm basis for determining if there is any feasible level of effort that would achieve these objectives." In a passage later quoted by the Senate Committee on Foreign Relations, the Jasons noted pointedly that bombing planners had ignored centuries of historical lessons and clung instead to flawed and overly optimistic assessments of the bombing's success:

> Initial plans and assessments for the Rolling Thunder program clearly tended to overestimate the persuasive and disruptive effects of the US air strikes and, correspondingly, to underestimate the tenacity and recuperative capabilities of the North Vietnamese. This tendency, in turn, appears to reflect a general failure to

> appreciate the fact, well-documented in the historical and social scientific literature, that a direct, frontal attack on a society tends strengthen the social fabric of the nation, to increase popular support of the existing government, to improve the determination of both the leadership and the populace to fight back, to induce a variety of protective measures that reduce the society's vulnerability to future attack, and to develop an increased capacity for quick repair and restoration of essential functions. The great variety of physical and social countermeasures that North Vietnam has taken in response to the bombing is now well documented in current intelligence reports, but the potential effectiveness of these countermeasures was not stressed in the early planning or intelligence studies.[20]

In this area, as elsewhere, the Jasons offered criticism rooted in commonsense analysis and observation, devoid of any particular scientific or technical analysis.

Reactions to the report were predictable. Robert Ginsburgh, assistant to the Joint Chiefs of Staff and member of the National Security Council's staff, agreed with some of the report's conclusions, particularly in its descriptions of the limited effects of the bombing. But he considered the Jasons' overall predictions ill-founded and overly pessimistic. How could anyone know that mining Haiphong and other harbors would not have a substantial effect? Ginsburgh forwarded the Jasons' report to Rostow with an accompanying memo noting that despite the Jasons' arguments, it would be "very difficult…to prove conclusively" what the effects of an expanded bombing program would be. Similarly, Ginsburgh questioned the Jasons' assertion that it was impossible to determine the limit at which bombing would finally halt North Vietnamese military activity. To Ginsburgh, asserting that the limit was unknowable "is the kind of statement that could be made right up to the time that NVN decides to sue for peace." Similarly, the Jasons' claim that North Vietnamese "will" seemed undiminished "is the kind of statement that

[20] Jason report, "U.S. Bombing in North Vietnam," 29 August 1966, "Vietnam—the Effects of U.S. Bombing on NVN's Ability to Support Military Operations in SVN and Laos" Folder, Box 192, Country File, Vietnam, NSF, LBJL; Section also quoted in U.S. Senate Committee on Foreign Relations, *Bombing As a Policy Tool In Vietnam: Effectiveness* (Washington, DC: US Government Printing Office, 1972).

could be made right up to the moment when Hanoi's leaders change their mind." Ginsburgh considered the report an argument for deescalating Rolling Thunder, which he informed Rostow would be "a grievous mistake."[21]

Ginsburgh's criticisms epitomized the Joint Chiefs' attitudes towards expanded bombing. All through the fall, the Pentagon was awash in reports and discussions concerning the bombing, including an additional Rand report by Oleg Hoeffding offering conclusions similar to those of the Jasons.[22] But Rostow was simultaneously receiving memos from Westmoreland lauding the "significant impact" of Rolling Thunder, even while the general complained that the policy of "creeping escalation" was far less effective than his proposed "shock action" of intensive attack.[23] Westmoreland was frustrated by the gradualism of gradual escalation, but his protests failed to convince McNamara, who felt bolstered by the Jasons' report, even with its deep criticisms of the bombing. As a later Senate report noted, this first Jason study had a "powerful and perhaps decisive influence in McNamara's mind."[24]

The Jasons would reiterate their arguments in the expanded, second study of the bombing, released in December of 1967. In a devastating assessment, that report asserted that the bombing "has had no measurable effect on Hanoi's ability to mount and support military operations in the South."[25] Indeed, China and the Soviet Union had more than compensated for

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[21] Memo, Ginsburgh to Rostow, 13 September 1966, "Vietnam—the Effects of U.S. Bombing on NVN's Ability to Support Military Operations in SVN and Laos" Folder, Box 192, Country File, Vietnam, NSF, LBJL.

[22] Draft report, Oleg Hoeffding, "Bombing North Vietnam: An Appraisal of Economic and Political Effects," October 1966, with accompanying note, "To RG from W," "Vietnam Rand Report "Bombing North Vietnam: An Appraisal of Economic and Political Effects" (U)" Folder, Box 192, Country File, Vietnam, NSF, LBJL.

[23] Memo, Westmoreland to Rostow, 24 October 1966, "10: History File 18-29 October 1966" Folder, Box 9, Papers of William C. Westmoreland, LBJ Library.

[24] U.S. Senate Committee on Foreign Relations, *Bombing As a Policy Tool In Vietnam: Effectiveness.*

[25] Ibid.

the costs of the bombing, so that from an economic perspective, North Vietnam had actually *gained* from the bombing. The influx of money and supplies, in turn, improved North Vietnam's military capabilities, making it "a stronger military power than before." At best, the bombing improved morale in South Vietnam, but only in "transient" ways. In no uncertain terms, the Jasons were arguing against the effectiveness of further bombing.[26]

2. Barriers

The second major report born out of the Dana Hall meetings flowed naturally from the conclusions of the first, and concerned the electronic barrier plans of Kistiakowsky and Zacharias. The plan—a system of sensors that could trigger military strikes against people and supplies crossing from North Vietnam to South Vietnam—offered hopes of de-escalation through deterrence, an alternative to the devastating bombing of Rolling Thunder. It also promised to minimize American casualties. For scientists like Kistiakowsky, who had reservations about the war, the barrier was a path to de-escalation, an attempt to save the McNamara approach and preserve the arms control liberalism that had gained ascendancy during the Kennedy administration.

The Jasons' report proposed the electronic barrier as a means to solve two key problems: the movement of supplies and the movement troops, from North Vietnam into South Vietnam. In its simplest form, the barrier consisted of a variety of mines placed throughout key areas and the "profuse use of simple sensors" that upon detection of any interloper would trigger air strikes. In principle this system was not new, but the novelty of the Jasons' approach lay in its extent and intensity, specifically "the very large scale of area denial, especially mine fields kilometers deep

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[26] Ibid.

rather than the conventional 100-200 meters." Supplies tended to be transported by trucks, boats, animals, or individual people on bicycles or on foot, through many different secondary road systems. Due to the high degree of "redundancy and flexibility" in available trails and transportation routes, an effective barrier would have to be "applied over sizeable areas," and the scientists offered several possible geographic options, mostly near the Laotian border.

More importantly, they offered fairly detailed descriptions of the kinds of sensors and weaponry necessary for a functional barrier. In the case of the anti-troop barrier, the mine and sensor field, potentially a strip of 500 square kilometers, would be "constantly renewed" with Gravel mines and "button bomblets." As one historian has described it, a Gravel mine "looks like a piece of ravioli" and is intended "to blow off the foot that steps on it."[27] Button bomblets, in contrast, were tiny "aspirin-sized" mines that when triggered emitted noises to alert acoustic sensors, but otherwise would not injure "a shod foot." For the most lethal "anti-personnel" swath of the barrier, the Jasons proposed that Gravel mines be "sown" at a density of 50,000 per square kilometer. Every thirty days, the minefields would be "reseeded."

While the Gravel mines would deter crossing on their own, the range of acoustic sensors, photo reconnaissance systems, and traversing P-2V planes equipped with infrared detection would also trigger additional airstrikes, composed of Gravel mines and SADEYE/BLU clusters. The SADEYE "Bomblet Dispenser Weapon" was deemed particularly effective: each cluster could be rigged to pass effectively through jungle canopy, and contained roughly 600 "anti-personnel/anti-vehicle" bomblets, themselves filled with steel pellets. Due to the clusters' indiscriminate lethality, the Jasons considered the SADEYE the "canonical" weapon of the barrier, "on the basis that area coverage with high kill probability will be needed to compensate

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[27] Dickson, 27.

for uncertainties in target location." In other words, the SADEYE's large and deadly range of impact rendered the minor imprecision of sensor data irrelevant.

For the anti-vehicle and supply barrier, acoustic detectors would be placed along targeted roads at mile intervals, and would in turn trigger SADEYE cluster air strikes. Additionally, every evening, air patrols "would distribute self-sterilizing Gravel over parts of the road net," ensuring that "road-watching and mine-planting teams" could still access the area.

Rather than requesting new areas for scientific research or hypothetical new detection tools, the Jasons had focused instead on developing a system that "could be largely operational… using nearly-available weapons, aircraft, and equipment," though "some component engineering will be necessary."[28] In a section headed "Some Comments on the Orientation of the Study Team and the Task," the authors noted that they could have devoted more time to new technologies useful for the barrier, but instead thought it was more valuable to focus on "the more prosaic task of trying to see how one could assemble hardware that will soon be available, with some minor modifications, into a system that could begin to function within about a year from go-ahead." Although the group hoped that over time, the barrier would employ new sensor technologies and new computing power (described as "information processing/pattern recognition techniques"), the focus of the report adhered to the spirit of Kistiakowsky's words to McNamara during the summer of 1966: "we do not propose to become involved in a broad effort at inventing new gadgets."[29]

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[28] Jason report, Institute for Defense Analysis Jason Division, Study S-255, "Air Supported Anti-Infiltration Barrier (C)," August 1966,"Vietnam Barrier, 2D, 9/66-9/68 [1 of 2]" Folder, Box 74, Country File, Vietnam, NSF, LBJL.

[29] Kistiakowsky to McNamara, 23 June 1966, in "Cambridge Discussion group" Folder, HUG (FP) 94.18, Kistiakowsky.

In researching the barrier, the Jasons had been briefed by numerous military personnel, from generals to specific field experts, as well as representatives from the CIA, ARPA, and the Pentagon's DDR&E office. Despite the Jasons' self-identification as independent, candid outsiders, their report frequently invoked many of the administration's political assumptions and analytical approaches, such as discussion of the importance of "deniability," acknowledgment of the need for secrecy about the geographic scope of the war, and the euphemistic phraseology and quantitative analysis characteristic of McNamara's Pentagon. In a section titled "Political Constraints," the Jasons wrote that in Laos, "Everything we do must satisfy the principle of deniability, to give the Soviet Union the opportunity to close its eyes to our operations… To this end, the North Vietnamese have never publicly admitted their infiltration operations in Laos, nor have we officially admitted the air or ground reconnaissance operations in all their scope." In describing the potential effectiveness of the barrier, the Jasons calculated confidently that "The system is designed for probability <<1 of a small group penetrating the denied area; for probability ~1 that all moving targets on roads or well-used trials are detected; and for ~0.3 kill of the moving targets, on the presumption that the enemy will not continue to "run the gauntlet" at that price. All probabilities are for the basic system design in the absence of countermeasures." But countermeasures were of course almost guaranteed, as the scientists themselves acknowledged in a separate section. These might include moving troops individual by individual, rather than as a unit; using groups of local tribesman as advance porters to 'sweep' areas; deploying decoys; moving sonobuoy acoustic sensors; constantly firing into the minefield to 'spoof' the system; or constructing a network of foxholes and bamboo bridges. The quantitative analysis lent a veneer of scientific objectivity to rosy assessments of the barrier's effectiveness, while the probable countermeasures were detached from the calculation and addressed only qualitatively. Such calculations, when marshaled in support of expanded bombing campaigns, had been deeply criticized by the Jasons in their report on Rolling Thunder.

Only twenty copies of the Jasons' barrier report were produced, but throughout the following year they circulated at the highest levels of government. Robert Ginsburgh, the National Security Council staff member who had criticized the Jasons' earlier report on the bombing, now suspected the Jasons of tailoring their technical assessments to political aims, inflating their assessment of the barrier while offering unduly harsh analysis of the effectiveness of Rolling Thunder. In a memo to Rostow, he noted drily that:

> It seems to me that if the Jason Group had applied to the barrier concept the same rigor that they applied to the bombing of North Vietnam the report would have been decidedly less enthusiastic about the infiltration barrier. Conversely, if they had applied the same standards to the bombing that they used for the barrier they would have concluded that the bombing was considerably more worthwhile than they indicated.[30]

Despite Ginsburgh's complaints, in September of 1966, McNamara officially approved the barrier project. As one participant later recalled, this final moment of decision-making occurred at Zacharias's summer house in Cape Cod: "The occasion was highly informal—maps were spread on the floor, drinks were served, a dog kept crossing the demilitarized zone as top secret matters were discussed." McNaughton and DDR&E's John Foster were both present.[31] Kistiakowsky later recalled that although he had recommended further study before implementation, McNamara enthusiastically preferred to "gamble immediately on putting our

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[30] Ginsburgh to Rostow, 26 September 1967,"Vietnam Barrier, 2D, 9/66-9/68 [1 of 2]" Folder, Box 74, Country File, Vietnam, NSF, LBJL.

[31] Gordon MacDonald, "JASON and the DCPG—Ten Lessons" (speech delivered at the Jasons' 25th Anniversary Celebration),30 November 1984, Box 37, Folder 12, MGM.

rather sketchy proposal into effect."[32] Soon afterwards, McNamara named Lt. Gen. Alfred Starbird as Director of the Joint Task Force charged with setting up "an infiltration interdiction system." McNamara instructed Starbird to work closely with John Foster and to keep John McNaughton and the Joint Chiefs fully informed. He also advised Starbird that additional experimentation and development of new features would be required, in areas such as "foliage penetration, moisture resistance, and proper dispersion of gravel; development of a better acoustic sensor than currently exists; aircraft modifications; possible modifications in BLU-26B fusing; [and] refinement of strike-navigation tactics." Additional input from scientists would be necessary. McNamara wrote explicitly, "I expect you to make use of an advisory group of non-government experts, including Dr. George Kistiakowsky."[33] The advisory group, known as the Defense Communications Planning Group (DCPG), was tasked with the necessary design and engineering work required to make the barrier operational.[34]

It was this last instruction that rankled Westmoreland. After meeting with Starbird shortly after his appointment to the barrier project, Westmoreland recorded his recollection of their conversation: "My general comment was that I was very much in support of the development of new weapons and devices by scientific and engineering communities, but I did not think it wise to have the scientists deeply involved in tactical employment."[35] The following month, Westmoreland expressed further skepticism; in his view, McNamara expected "that great

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[32] Kistiakowsky, "My Involvement in DCPG," June 1968, in "Vietnam, 1963-68" Folder, HUG (FP) 94.18, Kistiakowsky.

[33] Memo, McNamara to Starbird, 15 September 1966, "8 (History File, 17 July-17 Sept 66)" Folder, Box 9, Papers of William C. Westmoreland, LBJL.

[34] Gordon MacDonald, "JASON and the DCPG—Ten Lessons" (speech delivered at the Jasons' 25th Anniversary Celebration),30 November 1984, Box 37, Folder 12, MGM.

[35] General Westmoreland's Historical Briefing, 6 October 1966, "9 (History File 19 Sep-17 Oct 66)" Folder, Box 9, Papers of William C. Westmoreland, LBJL.

dividends might accrue from [the barrier] which I am very doubtful of." Without mentioning the Jason scientists specifically, he wrote of his "suspicion that somewhere along the line certain parties feel that if a barrier could be established to stop infiltration, the bombing in the North could be stopped. This is completely unrealistic thinking."[36] Moreover, as he wrote to General Starbird, diverting precious resources to the untested barrier would likely "degrade MACV's overall mission capability."[37]

Westmoreland was not the only skeptic. Kistiakowsky himself harbored reservations, from the opposing side of the political spectrum. In late 1966, he was appointed as General Starbird's senior advisor and commenced his formal involvement in war planning. In a 1968 recollection of his work with the DCPG, Kistiakowsky described his initial reaction to his appointment:

> Upon reading the directive I telephoned McNamara from Hornig's office and told him that I hesitated accepting this assignment because I was opposed to the administration's Vietnam policy. I wanted to work towards its modification and felt that accepting the job within DCPG…would cost me the freedom that I would have otherwise about speaking on Vietnam policy matters. I asked McNamara. therefore, whether he agreed with our summer study conclusions that the barrier could and should be used as a means for de-escalating the war and specifically making the bombing of North Vietnam unnecessary, because it was only with this understanding that I would take the job. I received assurances from McNamara that if the barrier were successful he, McNamara, would make an effort to use it for the de-escalation of the war through cessation of bombing. With this assurance I approached a number of Jason West summer study members and asked them to become members of the DCPG…[38]

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[36] General Westmoreland's Historical Briefing, 6 November 1966, Papers of William C. Westmoreland, Box 10, Folder #11 History File, 30 Oct-12 Dec 66, LBJL.

[37] Westmoreland to Starbird, 17 December 1966, "12 History File 13 Dec 66-26 Jan 67" Folder, Box 10, Papers of William C. Westmoreland, LBJL.

[38] Kistiakowsky, "My Involvement in DCPG," June 1968, in "Vietnam, 1963-68" Folder, HUG (FP) 94.18, Kistiakowsky.

McNamara offered Kistiakowsky his assurances, and, perhaps to mollify Westmoreland, John Foster provided the general with his own scientist, the UCLA chemist and Air Force advisor William McMillan. McMillan proved sympathetic to Westmoreland's commitment to expanded bombing, and Westmoreland found his bluntness refreshing. McMillan conducted an early study of the current state of the "air-interdiction campaign" in March of 1967 and concluded that while analysis was difficult because "reliable figures" were hard to come by, the painful truth was that the present system was "simply not very effective."[39]

Thus, with Kistiakowsky reassured and Westmoreland at least tentatively supportive, the stage was set for implementation of the barrier. That summer, Rostow informed LBJ that installation would begin in November, followed by "several months" of adjustments "to perfect the system."[40] But in July 1967, aspects of "the System" were already being tested. Kistiakowsky, closely involved in the process, recorded his hopes for the project in terms that justified Westmoreland's suspicions. The barrier could be "a tool for de-escalation," Kistiakowsky wrote in a memo. He acknowledged the possibility that North Vietnam might simply accept the higher attrition caused by the barrier and continue infiltration, but he maintained that "the System could provide Hanoi with a face-saving device to reduce the infiltration into the South without seeming to have been forced to yield to a bombing campaign." To achieve this preferable outcome, Kistiakowsky urged that the barrier be used in connection with a drastic reduction in aerial bombing, while communicating to North Vietnam that any increased infiltration attempts would be considered acts of escalation. As he witnessed the

[39] Memo, McMillan to Westmoreland, 18 March 1967, "17 History File 1-31 May 67" Folder, Box 12, Papers of William C. Westmoreland, LBJL.

[40] Rostow to LBJ, 15 July 1967, "Vietnam Barrier, 2D, 9/66-9/68 [1 of 2]" Folder, Box 74, Country File, Vietnam, NSF, LBJL.

limited successes of the early testing, however, Kistiakowsky understood that the barrier's functionality might initially be very limited. Therefore, he desperately hoped that mere description of the system would prove an effective deterrent, and that a bombing halt would not be postponed while waiting for improved barrier results. Immediate de-escalation was critical, he wrote, even if it meant that "Somebody will have to assert convincingly that [the barrier] will work without really knowing that this is so."[41]

Kistiakowsky's exuberance was shared by his colleagues. In a later oral history, Richard Garwin, who had traveled to Vietnam to assist personally with implementation in February 1968, described the centrality of deterrence and de-escalation for the scientists' barrier plans: "Sensors don't keep anybody from coming through, so the idea was that you would have such an effective capability of striking trucks … that they wouldn't come at all. It's like a perfect mine field or a fence; there's no sense coming, you won't get through; so you don't hurt anybody." A successfully deterrent barrier would remain untriggered, and casualties would be nonexistent.[42]

Within the administration, however, few shared the conviction that de-escalation could proceed without the barrier's demonstrated functionality. Richard Neustadt, the political scientist and Johnson advisor, was among those who emphasized the necessity of proving that the barrier was effective. Although only "marginally involved" in the barrier-related summer sessions, Neustadt considered the work integral to his larger goal of securing Johnson's reelection in 1968 and then "getting the war off the President's back so his third term isn't burdened like his second." In a memo written a year after Dana Hall, Neustadt clarified his assessment of the

[41] Kistiakowsky memo, 12 July 1967, in "Cambridge Discussion group" Folder, HUG (FP) 94.18, Kistiakowsky.

[42] Interview of Richard Garwin by Finn Aaserud on 24 June 1991 at IBM Research Lab, Croton-Harmon, NY, American Institute of Physics, College Park, MD, USA, http://www.aip.org/history/ohilist/5075.html (accessed 3 May 2011); confirmation of the February 1968 trip by Garwin, Kendall, and others in Ann Finkbeiner, *The Jasons: The Secret History of Science's Postwar Elite* (New York: Penguin, 2006), 98.

barrier's potential political importance, as "both the symbol of our purpose and the center of our effort." It would reduce the American military presence in Vietnam, which could be replaced by an "international police-force," thus dramatically de-escalating the war. Neustadt understood, however, that political goals depended on technological success. The barrier had to *work.* Otherwise, U.S. claims would lack credibility and any political benefits would vanish. Neustadt thus pinned his hopes on Kistiakowsky and the other Jasons, whom he labeled "our scientist-weaponeers," and their ability to create a functional barrier.[43]

3. Tactical Nuclear Weapons

Not every Jason member shared Kistiakowsky's brand of antiwar sentiment and corresponding action. Steven Weinberg, Jason member and future Nobel Prize winner, later recalled that the war in Vietnam posed ethical problems for the scientists, to which they responded in three ways. "Some members looked at [the war] as a purely military problem, to which the expertise of JASON members might make a useful contribution," Weinberg wrote. "Some thought of it as nasty business, which could best be ended by winning the war. Others simply wanted nothing to do with it. I was in the last group."[44]

Freeman Dyson, the Princeton physicist and Chairman of the Federation of American Scientists during the years of the test ban debates, later wrote about his decision-making process in his book *Disturbing the Universe:*

> I was invited to join the Barrier project and considered with some care the ethical questions that it raised. According to my general principle of preferring defensive

[43] Neustadt memo, 27 June 1967, "Vietnam Barrier, 2D, 9/66-9/68 [1 of 2]" Folder, Box 74, Country File, Vietnam, NSF, LBJL.

[44] Steven Weinberg, "What is JASON?" Nautilus Institute for Security and Sustainability, http://www.nautilus.org/projects/foia/essentially-annihilated/what-is-jason-author-steven-weinberg/ (accessed 25 December 2010).

> strategies, the Barrier was theoretically a good idea. It is morally better to defend a fixed frontier against infiltrators than to ravage and batter a whole country. But in this case, if one believed that the war was wrong from the beginning, a shift to a defensive strategy would not make it right. I refused to have anything to do with the Barrier, on the grounds that the ends it hoped to achieve were illusory. But I do not condemn my friends who worked on it with good conscience, believing that it would save many lives and mitigate the effects of the war on the civilian population of Vietnam.[45]

While the DCPG worked on the barrier project, yet a third Vietnam-related Jason report was in the works. This stemmed only indirectly from the Dana Hall summer session. For antiwar scientists like Dyson and Weinberg, who had opted out of other Vietnam studies, the frequent contact with the military leaders and Pentagon planners waging the war instilled a significant fear of the possible use of tactical nuclear weapons (TNW). In a later memoir, Dyson wrote that tactical nuclear weapon use was indeed broached at high-level meetings during the war, including the circulation of a memo entitled “Situations in Which the Use of Tactical Nuclear Weapons Is Plausible.” He recalled a meeting in 1966 during which a senior official (whom Dyson tactfully referred to only as “official Z”) suggested, “I think it might be a good idea to throw in a nuke now and then, just to keep the other side guessing.” Dyson was horrified at the proposal, and sought counsel from three other Jason scientists who were present: Robert Gomer, Steven Weinberg, and S. Courtenay Wright. The four “decided that something must be done,” and “concluded that the only way we might exert some real influence was to carry out a detailed professional study of the likely consequences if Z’s suggestions were followed.”[46]

The extent to which war planners seriously considered using nuclear weapons is the subject of debate, with most scholars agreeing that whatever idle chatter among military

[45] Freeman Dyson, *Disturbing the Universe* (New York: Basic Books, 1979), 150.

[46] Freeman Dyson, *Disturbing the Universe*, 148-149.

personnel might have occurred, Johnson and McNamara were firmly opposed.[47] Nevertheless, Seymour Deitchman, the physicist and limited war expert, later remembered "recurring talk around the Pentagon" in the spring and summer of 1966 about "using nuclear weapons to block passes between North Vietnam and Laos, especially the Mu Gia Pass, a key part of the supply route heading south." Both RAND and the Research Analysis Corporation had conducted war games addressing targeting and strategic problems relevant to TNW use, and contingency plans existed, including nuclear weapons use, should Chinese forces enter the war. But as a later assessment in the *Bulletin of the Atomic Scientists* concluded, Dyson and Weinberg "were undoubtedly responding to loose talk about using nuclear weapons from lower-level officials," rather than the serious consideration of top decision-makers.[48]

Whatever the actual risk, the four scientists determined to present, in military-style language scrupulously scrubbed of any moral or ethical taint, the case against TNW in Vietnam. Their goal was to influence, as effectively as possible, an audience of "official *Z*" and his peers, for which they determined a kind of 'value-free' approach would work best. They stated this aim clearly in the opening passage of the report itself, writing, "The purpose of this study is to evaluate the military consequences of a US decision to use tactical nuclear weapons (TNW) in Southeast Asia," and rather than relying on "intuitive judgment" or "moral" reactions, the scientists promised to provide "detailed analysis" and technical assessment to reach their

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[47] See relevant discussion in Peter Hayes and Nina Tannenwald, "Nixing Nukes in Vietnam," *Bulletin of the Atomic Scientists,* May/June 2002.

[48] Peter Hayes and Nina Tannenwald, "Nixing Nukes in Vietnam," *Bulletin of the Atomic Scientists,* May/June 2002.

conclusions.[49] It was, in Dyson's later words, an attempt to present "the narrowest military point of view disregarding all political and ethical considerations."[50]

Despite their careful language, the scientists' own personal motivations were entirely rooted in their values and their moral commitment to preventing nuclear war. Weinberg later wrote, "It was clear from the beginning that the report should not go into ethical issues. For us to raise such issues would cast doubt on the impartiality of our analysis." But his motivation was almost entirely ethical: "The analysis was honestly done, but I have to admit that its conclusions were pretty much what we expected from the beginning, and if I had not expected to reach these conclusions then, for the ethical reasons that we left out of the report I would not have helped to write it."[51] Indeed, the report's conclusions were stark: tactical nuclear weapons would not be "cost-effective," would offer few if any improvements over conventional weapons, and could potentially escalate the war to include "bilateral use" in Vietnam and possibly "general war" with the Soviet Union or China. Politically, the consequences could be "catastrophic."

As promised, the scientists reached these conclusions not through moral or ethical arguments, but through a patient review of the many scenarios in which TNW might be employed, and the possible consequences and outcomes. For example, "Bridges, airfields, and missile sites" might be "good TNW targets," but in many cases alternative high-power but non-nuclear weaponry could offer similar results. Using TNW to block roads and trails would also be effective, but only temporarily, since downed trees or other roadblocks could be cut through or

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[49] F. Dyson, R. Gomer, S. Weinberg, and S.C. Wright, "Tactical Nuclear Weapons in Southeast Asia, Study S-266," JASON Division, Institute for Defense Analysis, March 1967, http://www.nautilus.org/projects/foia/essentially-annihilated/dyson67.pdf/view (accessed 25 December 2010).

[50] Dyson, *Disturbing the Universe,* 149.

[51] Steven Weinberg, "What is JASON?" Nautilus Institute for Security and Sustainability, http://www.nautilus.org/projects/foia/essentially-annihilated/what-is-jason-author-steven-weinberg/ (accessed 25 December 2010).

cleared eventually. As an anti-personnel weapon, TNW would only be effective against "large masses of men in concentrated formations," not small, clandestine groups.

The scientists also made heavy use of the war games and simulations of RAND and RAC, which they criticized for being too short in duration and involving improbably large battle forces, so that the results tended to "exaggerate the effectiveness of TNW." Even so, the games revealed that "the outstanding difficulty in the use of TNW lies in locating troop targets accurately and striking before the location becomes obsolete." In other words, "target acquisition, rather than firepower," was the challenge facing US military forces. In an analysis that could extend far beyond the planners of war games to the actual planners of war, the scientists noted that the simulations did not "credit the enemy with the ability to hide and maneuver in the jungle, an ability that he has already demonstrated in Vietnam" and "are played on much too short a time scale; the proper time scale for war in Southeast Asia is almost certainly years, rather than days or months, with or without TNW."

In the report's most alarming section, the scientists addressed the risk of TNW use eliciting a nuclear response from the Soviet Union or China. In a passage still partially redacted for security reasons in 2010, they described how the USSR might provide nuclear weapons to Vietnamese forces, the ways in which these weapons could be transported and used against US forces, and possible consequences: "If about 100 weapons of 10-KT yield each could be delivered from the base perimeters onto all 70 target areas in a coordinated strike, the U.S. fighting capability in Vietnam would be essentially annihilated." Even if "only a few weapons could be delivered intermittently, US casualties would still be extremely high and the degradation of US capabilities would be considerable." Furthermore, should US use of TNW in Vietnam prompt the Soviet Union to provide similar weapons to the North Vietnamese, a dangerous precedent might be set, resulting in Soviet provision of nuclear weapons "to her friends in South America or Africa." Thus, using TNW in Vietnam might lead to "nuclear guerrilla operations in other parts of the world." (The scientists went so far as to list places "where dissident groups armed with TNW could do particularly grave damage": Panama, Venezuela, the Middle East, and South Africa.) More critically, as the scientists worked their way through different nuclear scenarios, all paths seemed potentially to lead to nuclear disaster. "The ultimate outcome is impossible to predict," they wrote. "We merely point out that general war could result, even from the least provocative use of [nuclear weapons] that either side can devise." Even in the best outcome, with no corresponding Soviet or Chinese nuclear response, the United States would suffer from world condemnation for 'crossing the nuclear threshold.' The scientists closed with a strong, clear warning: "In sum, the political effects of US first use of TNW in Vietnam would be uniformly bad and could be catastrophic."

Official response to the report is difficult to gauge. Dyson wrote in his memoir: "We handed the report to our sponsors in the Defense Department. That was the last we saw of it."[52] Seymour Deitchman later recalled that both John McNaughton and McNamara were briefed on the scientists' conclusions, but his time frame of summer 1966 does not match the publication date of the report, in March 1967. In 2002, when the Nautilus Institute's Nuclear Policy Project successfully obtained a copy of the document through the Freedom of Information Act, writers for the *Bulletin of the Atomic Scientists* contacted McNamara, who had no recollection of the report or a related briefing, but acknowledged that such a meeting might have occurred.

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[52] Dyson, *Disturbing the Universe,* 149.

Nuclear weapons—tactical or otherwise—were never deployed in Vietnam, but the prospect surfaced again briefly in early 1968. That winter, during the siege at Khe Sanh, Westmoreland reportedly set up a secret but short-lived "study group" to look into nuclear options.[53] In February, McNamara personally reassured concerned scientists Killian, Kistiakowsky, and Rabi that the Joint Chiefs had never discussed using nuclear weapons in South Vietnam, and "because of terrain and other conditions peculiar to our operations in South Vietnam," nuclear warfare against North Vietnamese forces was "inconceivable." In June, William McMillan, the UCLA chemist who served as Westmoreland's science advisor, AFSAB member, and PSAC consultant, discussed with a Reuters reporter the possibility of using nuclear weapons in Vietnam. He explained that he wouldn't recommend their use "on a scientific basis" because they were "not suited to the kind of warfare being waged." But, he added, "I certainly would not suggest the use of nuclear weapons by the United States in South Vietnam—and I think it is a very low key possibility that we will ever use them against the North. But I can see circumstances under which nuclear weapons could be used."[54] Whether the Jasons' report on tactical nuclear weapons affected the views of Westmoreland, McNamara, or McMillan remains unknown, but their statements suggest at least the plausibility of Dyson's hopes for his effort. "I have no way of knowing whether anybody ever read our report," he wrote later. "I have no way of knowing whether there was ever any real danger that Johnson would use nuclear weapons in Vietnam. All I know is that if Johnson had ever considered this possibility seriously and had

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[53] Peter Hayes and Nina Tannenwald, "Nixing Nukes in Vietnam," *Bulletin of the Atomic Scientists,* May/June 2002.

[54] State Department memo, June 1968, "Vietnam 7F (3) 4/68-10/68 Congressional Attitudes and Statements [1 of 2]" Folder, Box 102 [2 of 2], Country File, Vietnam, NSF, LBJL.

asked his military staff for advice about it, our report might have been helpful in strengthening the voice of those who argued against it."[55]

Kistiakowsky and the DCPG

While the nuclear recommendations of Dyson and his colleagues seemed to vanish into the depths of the Pentagon bureaucracy, press speculation about the Jasons' barrier project began to appear in force beginning in the fall of 1966. As information leaked out of the Senate Preparedness Subcommittee, public accounts edged closer to the truth of the sensor-and-airstrike system. Andrew Hamilton of the *New Republic* reported on McNamara's "Practice Nine," a study of possible barrier technologies that, in the words of a quoted official, drew on "Everybody and his brother." Hamilton estimated that the proposed system of barriers and fencing would cost $1 million per mile to build and operate, and offered a pessimistic assessment of its chances for success.[56] On September 7, the *Washington Post* carried the first detailed descriptions and a critical analysis by Joseph Kraft, who considered the barrier an indication that the U.S. military was now taking a purely reactive approach to the war (rather than "taking the offensive on raids"), which could lead to "terrible casualties." While supporting the barrier in theory, he expressed skepticism as to its effectiveness.[57]

Kraft's article appeared on the same day as a press conference on the barrier held by McNamara. Shortly afterward, McNamara further clarified the nature of the barrier in a memo to President Johnson, calling the word itself a "misnomer," as it was an "anti-infiltration system"

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[55] Dyson, *Disturbing the Universe,* 149.

[56] Andrew Hamilton, "Vietnam-Fencing in the North," *New Republic* 8 July 1967.

[57] Joseph Kraft, "Policy of Creeping Gavinism Affects U.S. Vietnam Posture," *Washington Post,* 7 September 1967.

rather than a kind of Maginot Line. True, the overall project included “an obstacle line” of barbed wire, mines, and fortifications south of the DMZ, but its larger component was the “unique” sensor and air support system. McNamara cited both the optimistic estimates of Kistiakowsky and the applicability of the system being developed to other limited war operations. As he put it, “if effective at all, the components should be useful in other parts of Southeast Asia (or the world) where selective detection and strikes are desired.” In this regard, McNamara was reconfirming the mutual support for ‘flexible response’ he shared with Kistiakowsky and many of the other Jasons.

But McNamara’s note to the president contained other messages. Despite his citations, he expressed some skepticism about “Dr. Kistiakowsky’s forecasts,” and cautioned, “There is the risk, too, that expectations for impressive early results will create clamor to substitute the new anti-infiltration system for other military measures.” He did not specify the bombing of North Vietnam as among these measures, but such was the hope of many of the Jasons, particularly Kistiakowsky himself. McNamara also noted pointedly that while Gen. Earl Wheeler endorsed the project, military support among Westmoreland and the Joint Chiefs was lukewarm at best.[58] McNamara clearly understood the tensions inherent in the contradictory concerns and prognostications of the Jasons and the Joint Chiefs, the irreconcilability of the views of Kistiakowsky and Westmoreland.

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[58] Memo, McNamara to LBJ, 11 September 1967, “Vietnam Barrier, 2D 9/66-9/68 [2 of 2]” Folder, Box 74, Country File, Vietnam, NSF, LBJL. The Kraft article was the first of several major leaks concerning the barrier. In late October, Jack Robertson’s detailed account of the barrier in *Electronic News* prompted Donald Hornig to write to Walt Rostow, “This is a shocking commentary on our security.” Dean Rusk reiterated that technical information about Muscle Shoals should only be shared with those “with true need to know,” because “effectiveness will in large measure depend upon enemy ignorance of how it works.”

In the meantime, the Jasons and the DCPG continued to work; in the spring of 1967, Val Fitch and Leon Lederman submitted a study of "Air-Sown Mines for the Massive Barrier," recommending "a pencil-shaped, fin-stabilized device which would be capable of soil penetration to a predetermined depth, so that a plunger-activator projects just slightly above the trail surface."[59] Regular meetings with McNamara took place in the form of the "DCPG Advisory Committee," whose bicoastal members included physicists David Caldwell of the University of California, Gell-Mann and Zachariasen from Caltech, Harold Lewis of the University of California, and, from the east coast, Marvin Goldberger of Princeton, IBM's Richard Garwin, Henry Kendall and Jack Ruina from MIT, and Harvard's Kistiakowsky, among others.[60]

But as McNamara's memo to the president had intimated, the Jasons were no longer in control of the project's trajectory, if they ever had been. Plans for the barrier were expanding beyond the Jasons' commitment to using existing technology, and drawing on experts outside of the original Jason participants. Flush with money, including massive funding for further research and development, the DCPG was endowed with its own "Engineering Directorate" that drew in engineers and technicians from the military labs and the private sector. Contracting to the country's major defense engineering firms began in earnest. According to *Electronic News,* private sector companies involved in sensor production included General Dynamics, Defense Electronics, Texas Instruments, and Teledyne, among others. Many of the key military agencies devoted to limited war research were involved: the Rome Air Development Center, the Elgin Air

[59] V.L. Fitch and L. Lederman, Abstract, "Air-Sown Mines for the Massive Barrier" (Alexandria, VA: Institute for Defense Analysis, Jason Division, May 1967), http://www.nps.edu/Library/Research/Bibliographies/LandMines/LandMinesBibTechRptsDH.html (accessed 25 December 2010).

[60] Memo, Kistiakowsky to Polly Yates, 6 November 1967, in "Cambridge Discussion group" Folder, HUG (FP) 94.18, Kistiakowsky.

Force Base Limited War Group, and the Army Limited War Laboratory at Aberdeen. These sites worked on acoustic, chemical, and seismic detectors.[61]

Another major feeder was the MITRE Corporation, the Sputnik-era MIT spinoff with a decade of experience conducting defense research for the Air Force. Paul Dickson, who documented the creation of the DCPG in his history of the electronic battlefield, recalled the excitement and prestige felt by the new, non-Jason personnel. “DCPG alumni…talk about the organization in reverential and enthusiastic terms,” he wrote, quoting one “typical” MITRE veteran’s recollection that “as an engineer it is what you dream about.” In Dickson’s estimation, participants swooned at the prospect of “being present at a turning point in the technology of warfare” and “being a charter member of a new technological fraternity.” As experts in sensors, acoustics, and signal processing convened, MITRE’s Casper Woodbridge remembered the thrill of feeling part of “a new community, the sensing community.”[62] To an increasing extent, the old guard of Manhattan Project veterans and Jason physicists existed only on the peripheries of this “new community.”

As the barrier implementation efforts foundered during the early months of testing in 1967, Kistiakowsky continued to push for de-escalation during his private meetings with McNamara. McNamara, in turn, warned Kistiakowsky that no change in policy could occur until “the barrier proved itself militarily.” When the Jasons conducted a second barrier study in the summer of 1967, arguing, in Kistiakowsky’s words, “that to make the barrier effective politically it was necessary to stop the bombing of North Vietnam very soon after putting the barrier in

[61] Jack Robertson, “Viet ‘Wall’ Will Sense Enemy, Flash Warning to Main HQ,” *Electronic News,* 30 October 1967.

[62] Dickson, 32-35.

operation," McNamara still resisted immediate deescalation. He stalled Kistiakowsky by requesting an additional assessment of the effectiveness of the bombing in North Vietnam, resulting in the second Jason report on the topic, the massive, four-volume study reiterating earlier conclusions.[63]

Meanwhile, the bombing continued, and for Kistiakowsky, the failure of the Jasons' barrier plan to be implemented alongside bombing reduction constituted a betrayal. Since his work with the Cambridge Discussion Group beginning in early 1966, he had urged de-escalation and a change of course in Vietnam, to no avail. The barrier had come to represent the last, best hope for scientists to work toward these goals within existing political and military channels, and by the fall of 1967, Kistiakowsky had transferred all of his political energy to its design and implementation. Privately, however, he feared its futility. When fellow chemist George Pimentel recruited him for an antiwar effort in November, Kistiakowsky declined on the grounds that he was occupied with a major "classified project" whose success he didn't want to jeopardize. But he wrote, ominously, "It is, of course, not only possible but probable that the project I am involved in will be essentially a failure, in which case it is becoming clear to me that I will have no alternative than to go into opposition."[64]

Kistiakowsky was not alone in his growing skepticism. In a startling letter written to Kistiakowsky in the spring of 1968, Fredrik Zachariasen of Caltech reviewed the wholesale disillusionment that had plagued the entire DCPG Scientific Advisory Committee in the fall of

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[63] Kistiakowsky, "My Involvement in DCPG," June 1968, in "Vietnam, 1963-68" Folder, HUG (FP) 94.18, Kistiakowsky; Jason study, January 1968, "Vietnam Four Volume Study of the Bombing of North Vietnam 1/8/68 [1 of 2]" Folder, Box 247, Country File, Vietnam, NSF, LBJL.

[64] Kistiakowsky to Pimentel, 15 November 1967, , in "Vietnam, 1963-68" Folder, HUG (FP) 94.18, Kistiakowsky.

1967, and confirmed that the prospect of a mass resignation had not only been discussed but largely endorsed by members:

> Beginning in the fall of 1967, I, and most of the other members of the committee, were becoming more and more concerned about the US policy of slow, steady escalation of the war in Vietnam and in particular about what this implied about the use to which the DCPG project would be put. I recall telling you, one morning before one of our meetings, that I was not interested in helping you with a project that would be used merely as an additional escalatory step, and that I felt that the administration had no interest in using the DCPG effort in the way all of us had originally hoped it would be used, as part of a general de-escalation of the war. You said you felt the same way, and I know that all but at most one or two of the other members of the committee did too.
>
> In later discussions, both at DCPG and at the Cosmos Club, dissatisfaction with US Vietnam policy was expressed by everyone present. You said, at various times, that you thought you should eventually resign from the committee, and several of us, myself included, thought it would be better if the whole committee resigned together. You, however, felt that this was a decision for each of us separately and did not think that anything should be done by the committee as a group. Nevertheless, a number of us, again including me, told you that if you resigned we intended to do so too; and that we still felt all of us should quit together.[65]

DCPG attendees meeting with Robert McNamara, 9 November 1967:

Dr. David Caldwell
Department of Physics
University of California

Dr. Richard Garwin
IBM Watson Laboratory

Dr. Murray Gell-Mann
Department of Physics
California Institute of Technology

Dr. Henry Kendall
Department of Physics
Massachusetts Institute of Technology

Dr. George B. Kistiakowsky
Department of Chemistry
Harvard University

Dr. Harold Lewis
Department of Physics
University of California

Dr. Jack Ruina
Vice President for Special Laboratories
Massachusetts Institute of Technology

Dr. Leonard Sheingold
Sylvania Corporation

Dr. F. Zachariasen
Department of Physics
California Institute of Technology[66]

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[65] Zachariasen to Kistiakowsky (first letter), 7 June 1968, in "Vietnam, 1963-68" Folder, HUG (FP) 94.18, Kistiakowsky.

[66] Memo, Kistiakowsky to Yates, 6 November 1967, , in "Vietnam, 1963-68" Folder, HUG (FP) 94.18, Kistiakowsky.

Both Kistiakowsky and Henry Kendall argued against a mass resignation, although Kistiakowsky agreed to warn McNamara of such a possibility in an attempt to influence policy.[67] In December, however, McNamara abruptly left his post as Secretary of Defense, an event described alternately as a resignation and a dismissal. Kistiakowsky, who had considered McNamara his “only channel for offering advice on policy matters,” remained loyal to the end, defending McNamara to critical colleagues and assuring his former boss of his respect and sympathy. But as promised, Kistiakowsky informed McNamara that with his departure, and with clear evidence that no change in policy was likely, “most of the members” of the DCPG advisory committee planned to resign quietly. Kistiakowsky cited his own frustration at the committee’s complete lack of policy influence, and wrote that he found it “intellectually unacceptable to continue participating in a project that in the framework of present policies will tend to further expand the war.”[68]

At the DCPG committee’s next meeting on January 13, 1968, Kistiakowsky announced his resignation to his colleagues. He would retain his posts as a PSAC “member-at-large” and ACDA advisor, but his Vietnam work was over. Despite his words to McNamara, he rebuffed calls by Gell-Mann and others to lead a group effort, and instead urged that any resignations be individual matters of “conscience.” He also suggested that any accompanying statements emphasize opposition to general policy, rather than specific failures of the DCPG. Perhaps

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[67] Kendall to Kistiakowsky, 10 June 1968, , in “Vietnam, 1963-68” Folder, HUG (FP) 94.18, Kistiakowsky.

[68] Kistiakowsky, “My Involvement in DCPG,” June 1968, , in “Vietnam, 1963-68” Folder, HUG (FP) 94.18, Kistiakowsky; Kistiakowsky to McNamara, 11 December 1967, in “Vietnam, 1963-68” Folder, HUG (FP) 94.18, Kistiakowsky.

Kistiakowsky still held out some tiny hope that sensors and minefields could yet lead to de-escalation.[69]

Zachariasen, for one, was caught "by surprise" at the quickness of Kistiakowsky's decision, and his immediate reaction was to resign as well. The remaining members of the advisory group later met to discuss their response, and, in Zachariasen's words, "it became clear that in fact very few wanted to quit immediately, and most preferred to wait for a little while, though nearly all were still completely disenchanted with US policy and, they said, had no intention of remaining on the committee for very long." Zachariasen worried that his individual decision would have little impact unless the entire committee resigned. Soon after, Johnson's announcement that he would not seek reelection cemented the group's decision to stay, as it held out "the possibility of a real change in policy."[70]

In the interim, Kistiakowsky selected an audience of roughly one hundred acquaintances, many of them members of the National Academy of Sciences, and distributed to them a carefully-worded letter describing his views on de-escalation and 'hinting' at his resignation. In short time the letter had been widely circulated among physics and chemistry departments at nearly every major research university. Kistiakowsky's words and deeds prompted an outpouring of praise, hand-wringing, and political reflection among scientists, largely expressed in private correspondence to Kistiakowsky himself.

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[69] Kistiakowsky, "My Involvement in DCPG," June 1968, , in "Vietnam, 1963-68" Folder, HUG (FP) 94.18, Kistiakowsky.

[70] Zachariasen to Kistiakowsky (first and second letters), 7 June 1968, , in "Vietnam, 1963-68" Folder, HUG (FP) 94.18, Kistiakowsky.

Kistiakowsky estimated that he received roughly seventy letters of support, and one angry denunciation. A survey of the correspondence reveals the deep respect with which Kistiakowsky was regarded, and the power of his words and actions to spur serious self-reflection among his colleagues, nearly all of whom wrote of their inner turmoil and felt compelled to articulate exactly why they had chosen to remain in their posts. This is particularly apparent in the letters from fellow Jasons and DCPG members. Physicist and DCPG committee member David Caldwell wrote that he agreed with Kistiakowsky and had considered resigning, but felt his effectiveness would be equally minimal as an outsider. Leonard Sheingold of DCPG concurred, as did Jack Ruina, who lauded Kistiakowsky's analysis of the war as "excellent" while defending his own decision to remain on the committee. William Nierenberg, a Jason leader on Vietnam work, likewise wrote in support of Kistiakowsky's words and actions.

In the most prestigious halls of academia, Kistiakowsky's letter elicited similar reactions. Nearly every respondent expressed hopes for de-escalation and political observations about the state of the war and prospective candidates in the upcoming presidential election. Caltech's Max Delbruck lauded Kistiakowsky's actions but chided him for his faith in McNamara, whom Delbruck considered guilty of "dreadful errors of policy." His colleagues Norman Davidson, Jesse Greenstein, George Hammond, and John Roberts all wrote in support, ranging from the effusive to the qualified. Other high profile supporters included Mark Kac of Rockefeller University, George Fraenkel of Columbia, Kenneth Pitzer, Emanuel Piore and Wolfgang Panofsky.

Piore and Panofsky, in particular, homed in on the relative merits of acting as either insiders or outsiders. As Piore put it, "I'm ambivalent about how best to apply pressure—whether completely to dissociate oneself and possibly get some satisfaction that one has made the break, or stay with it and try to apply pressure internally." Wolfgang Panofsky echoed these views, admitting to doing some "soul-searching" about his advisory work and wondering "whether I could be more helpful outside or inside the present system, and I am afraid the conclusion is that neither appears too promising."

In nearly all the dozens of letters received by Kistiakowsky, his elite colleagues expressed frustration, bitterness, anger, and deep ambivalence about their own work and their political options. In perhaps the most telling example of the stark shift in political outlook, Berkeley chemist George Pimentel, whose own efforts to persuade President Johnson to de-escalate had been personally rebuffed by Walt Rostow, wrote Kistiakowsky to propose the creation of a new organization: "Scientists and Engineers Against Johnson."[71]

Despite this response, Kistiakowsky considered his resignation an act of personal conscience, not a public statement of political opposition. This stance was evident in his objection to a mass resignation of the DCPG advisory committee (although he had invoked the possibility of such an act in his letter to McNamara), and in his own explanations to colleagues and friends. When he wrote to a colleague that his resignation "doesn't mean that I have grown long hair and have become a 'peacenik,'" he firmly separated himself from the youthful antiwar movement. Instead, he explained that he had circulated his letter without publication because he didn't want to "reduce my usefulness to a candidate for the presidential office that I hope to work

[71] See letters to Kistiakowsky from David Caldwell, 1 February 1968; Leonard Sheingold, 2 February 1968; Jack Ruina, 17 January 1968; William Nierenberg, 13 March 1968; Max Delbruck, 2 February 1968; Norman Davidson, 8 February 1968; Jesse Greenstein, 1 March 1968; George Hammond, 21 February 1968; John Roberts, 31 January 1968; Mark Kac, 9 February 1968; George Fraenkel, 26 January 1968; Kenneth Pitzer, 27 January 1968; Emanuel Piore, 25 January 1968; Wolfgang Panofsky, 7 February 1968; George Pimentel, 23 January 1968; all in "Vietnam, 1963-68" Folder, HUG (FP) 94.18, Kistiakowsky.

for (most likely Rockefeller)."[72] Sensitive to the classified nature of his work and reluctant to reveal too much, he felt he had "to walk a tight rope" in his public actions.[73]

Kistiakowsky's efforts to contain the publicity predictably failed, however. Over his "remonstrations," in March Daniel Greenberg wrote a one-page account of the resignation in *Science*, describing Kistiakowsky as "one of the federal government's most influential science advisers," at "the heart" of the so-called "scientific establishment," and someone "noted for his prudence and conservatism." Greenberg therefore interpreted the resignation as potential evidence of 'massive disaffection' among elite scientists and an indication of the Pentagon's "increasing difficulty…in attracting top-level scientific talent to work on military matters." More ominously, the resignation came "against a background of worsening relations between the academic world and the military."[74] The story was picked up by the *New York Times*, which emphasized Kistiakowsky's prestigious scientific past, his recent status as a recipient of the National Medal of Science, and noted widespread "speculation over the political ramifications of the action."[75]

The Jasons during Vietnam, Part II

Jason work connected to Vietnam did not end with the Dana Hall summer study and the creation of the DCPG. In August 1966, Kenneth Case, Leon Lederman, and Malvin Ruderman wrote to Marvin Goldberger at Princeton's Palmer Physical Laboratory, proposing the

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[72] Kistiakowsky to Fisher, 14 March 1968, and Kistiakowsky to Mather, 14 March 1968, , in "Vietnam, 1963-68" Folder, HUG (FP) 94.18, Kistiakowsky.

[73] Kistiakowsky to MacDonald, 7 February 1968, , in "Vietnam, 1963-68" Folder, HUG (FP) 94.18, Kistiakowsky.

[74] D.S. Greenberg, "Kistiakowsky Cuts Defense Department Ties over Vietnam," *Science* 158 (March 1968): 958.

[75] Evert Clark, "Top Scientist Cuts All Links To War," *New York Times*, 1 March 1968.

development of new categories of non-lethal weapons for use among police forces and US and UN operations in a variety of countries, including Vietnam, where military encounters included "close engagement of enemy and friendly troops and also engagement in the presence of villagers whose minds and hearts we would like to leave intact." The scientists acknowledged that "many moral and political, as well as military, arguments have been raised against such a program," on the grounds that "fluctuations in applied dosage from such weapons and also in the human sensitivity to them preclude their really being humane, effective, and limited to specific areas." But they believed in the possibility of development of "effective, controllable, non-lethal weapons" that could reduce civilian fatalities. They described one particular plan for a kind of anesthetic dart gun:

> We have in mind, as an example, a system consisting of an anesthetic agent administered by a very small flechette which would act in several seconds to several minutes, producing some kind of incapacitation, preferably somnific, for from 4 to 20 hours. …In a combat situation a barrage of appropriately tipped flechettes dispersed from a gun or a bomb could give fairly uniform coverage over a specific area. … Clearly, one of the many technical burdens that "conventional" non-lethal agents have—that of the small lethality tail—is not very important in this context since the flechette replaces a bullet. … The main consequence of a non-lethal military capability might, hopefully, be a start at least toward a more humane way of waging war. Certainly, appropriately controlled non-lethal warfare is, in principle, not less humane than conventional projectiles, explosives, etc.
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> **Emphasis on humane approaches may well make possible the involvement of specialists who previously have been repelled by the whole subject. We feel that the competent scientific community has not been sufficiently engaged.** This community should be asked to undertake a study of the feasibility of developing a non-lethal weapon**. Clearly this approach should be separated as far as possible from conventional CW and BW work.** It should include study of delivery problems, military tactics and political consequences**.**

From the Dana Hall summer sessions to the time of Kistiakowsky's resignation, the Jason Steering Committee was deploying the organization's top members to the problem of Vietnam technology. Gell-Mann urged Alvarez to assume control of the Infiltration Group, and Panofsky met with Garwin to discuss "what technical help is needed now for Vietnam." Plans for the 1967 Falmouth summer study were underway, with proposed sessions on pacification, night operations, non-lethal weapons for crowd control, war sensors and communications, and American actions in Thailand, as well as the traditional meetings on anti-ballistic missile technology and other nuclear weapons related problems.

But morale problems continued as the war in Vietnam progressed. After a period of "soul-searching" among ARPA and Jason leaders, the Jason Steering Committee met in March and April of 1967 and agreed to 'start over,' with a new list of topics for Jason sub-groups to choose from (topics that would be "important but not urgent"), and wider allowances for the Jasons to produce either original studies or critiques of existing programs and proposals. Once again, committee members emphasized lagging productivity and the importance of recruiting and retaining new members; specifically, that "an effort should be made to keep ingesting young Jasons and keeping them from becoming senior scientific statesmen too fast."[76] Throughout 1967, in addition to studies on weather modification, anti-ballistic and missile guidance systems, naval warfare, lasers, turbulence, transportation problems, and other topics, Jason continued its Vietnam work, drawing on the expertise of Marvin Goldberger, Gell-Mann, Lewis, Nierenberg, Kistiakowsky, Townes, York, Neustadt (first as a non-Jason consultant, later a Jason consultant), and Yarmolinsky (a Jason consultant).

As information about the barrier began to appear in the press, reports of the existence of the secret Jason group began to percolate out into the academic communities of the members, attracting unwanted attention and difficult questions. In November of 1967, the Jason scientists were instructed to direct any questions on the connections between Jason and IDA to James

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[76] Jason Steering Committee meeting minutes, 1967, Box 36, Folder 1, MGM.

Cross, IDA's Secretary and soother of university-IDA trustee relations. IDA also assured Jason members that they had the right to take on whatever consulting projects they chose without university interference, and as for storing classified documents in special cabinets on campus, this was no different from "locking one's desk or other container with valuables."[77] Two months later, John Martin wrote worriedly to Gell-Mann that "two gentlemen by the names of Kraslow and Loorie have been making inquiries about the Jason-sponsored work at Dana Hall in '66," and were aware of the Jason members' identities and at least "some ideas of the matters which transpired at that time."[78] For the first time, the Jasons were beginning to face public scrutiny of their war-related work.

PSAC during Vietnam

With its overlapping membership and function, PSAC predictably followed a similar trajectory to that of the Jasons, increasingly tackling problems of counterinsurgency and warfare in southeast Asia. Overseeing, or at least participating in this shift, was Johnson's special assistant for science and technology, Donald Hornig, appointed in 1964.

Hornig's professional trajectory was typical of elite scientists of his generation. He was born in Milwaukee in 1920, and, after receiving degrees in chemistry from Harvard, where he studied with E. Bright Wilson, Hornig joined the Manhattan Project during a two-year stint at Los Alamos from 1944 through 1946. He taught at Brown and served as dean of the graduate school, then left for Princeton in 1957. From there he joined with his colleagues in entering government and military service; his memberships included the Advisory Committee of the Air

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[77] Martin to Jason members, 13 November 1967, Box 36, Folder 1, MGM.

[78] Martin to MGM, 3 January 1968, Box 37, Folder 1, MGM.

Force's Office of Scientific Research, the National Academy of Science's Space Science Board, and the PSAC under both Eisenhower and Kennedy.

Now Hornig was tasked with heading both PSAC and OST. He and his key staffers, Spurgeon Keeny, Donald Steininger, and Vincent McRae, guided the development and allocation of PSAC panels and other expert resources to key issues of national security. Defense-related PSAC panels included those devoted to anti-ballistic missiles, anti-submarine warfare, tactical naval warfare, biological and chemical warfare, ground warfare, and, eventually, the Ad Hoc Panel on Vietnam and the Special Sub-Panel of PSAC on Vietnam. A later report on Hornig's tenure described the delicate process by which OST struggled to influence Pentagon programs, relying largely on "persuasion," exercised through "informal meetings" such as regular lunches with McNamara and other key officials, and conferences hosted by PSAC panels. Not surprisingly, Hornig often found that "strenuous efforts" were required to secure implementation of even partial recommendations.[79]

A recently declassified report on the OST during this period notes that from 1963 through 1965, top-level science advisors had little to do with the conflict in southeast Asia, with the exception of the PSAC military aircraft panel and "some limited activity of the BW-CW Panel." The PSAC panel on CBW, for example, weighed in on the use of chemical and biological weapons in Vietnam, arguing against the use of DM ("vomit gas") and recommending against unspecified "proposed extensive biological test programs." The military aircraft panel, led by Vincent McRae and Richard Garwin, advised the Air Force on effective radar systems and "electronic warfare."

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[79] Narrative Statement, "Volume II Documentary Supplement [1 of 3]" Folder, Papers of Donald Hornig, LBJL.

As the war escalated, however, so did OST and PSAC involvement. By 1966, PSAC meetings were addressing Vietnam in greater depth, while Pentagon officials from DDR&E more frequently sought expert science advice. As PSAC "became increasingly concerned and questioned the way in which research and technology were being used in support of the military operations there," additional action seemed necessary. In the spring of 1966, the OST report states, "certain members and ex-members of the President's Science Advisory Committee (acting in an individual capacity) urged a broad study of technical problems associated with Vietnam." Hornig, apparently skeptical of the influence of PSAC and OST, suggested that the Pentagon undertake the study through other means, such that it would be guaranteed to "receive very high level attention and implementation of its findings would be much easier."[80] The result, of course, was the Jasons' Dana Hall summer study of 1966.

Hornig's suggestion was not an attempt to remove Vietnam-related topics from the purview of PSAC or the OST, however. According to OST's own administrative history, by the end of 1966 "almost one-fourth of the PSAC discussions…were on topics directly related to Vietnam." The establishment of an "Ad Hoc Group on Vietnam" quickly followed, which included IBM's Richard Garwin and the heads of PSAC's four military panels devoted to aircraft, naval warfare, ground warfare, and strategy. The scientists recommended the allocation of greater scientific resources to the war effort, including an expanded technical staff assigned to McMillan, as well as greater flexibility in funding structures.

Hornig himself was a tireless champion of close relations between scientists and the Johnson administration. Late in 1965, when informed of a small budget transfer away from

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[80] "Activities Related to Vietnam," "Volume II Documentary Supplement [1 of 3]" Folder, Papers of Donald Hornig, LBJL.

PSAC, Hornig warned Johnson that "PSAC would be extremely sensitive to any thought that their distance from you is being increased."[81] A year and half later, Hornig complained to Johnson about the lack of scientists included in an 'intellectuals' lunch' hosted by the president. (Hornig also complained about his own lack of an invitation, which could harm "my relations with the academic community, particularly the scientists, who also consider themselves intellectuals.") But more importantly, the president had missed an opportunity to reach out to scientists, whose political power he had clearly underestimated. "It would be a serious error to discount the interest and influence of the scientific community, either in Viet Nam problems or in social progress," Hornig wrote. "They are among the most worried and hard to deal with in connection with Vietnam and we continue to need their support."[82]

Indeed, much of Hornig's workweek consisted of speeches to and correspondence with other scientists, including those who wrote to complain to him about the progress of the war. Whatever Hornig's personal views, he was tasked with defending administration policies. As he wrote to one such acquaintance, a Welsh chemist, "I would rather we were corresponding now about infrared spectroscopy or hydrogen bonding than Vietnam…" Hornig acknowledged "the strong opposition of many U.K. and European intellectuals to our operations in Vietnam," but he characterized much of it as "a highly emotional response based on incorrect information." Instead, he defended the war as an attempt to repel "direct aggression by North Vietnam," and invoked the wartime example most compelling to scientists: "We all learned very painfully in

[81] Hornig to LBJ, 28 December 1965, "Donald Hornig Chronological File October-December 1965" Folder, Box 3, Papers of Donald Hornig, LBJL.

[82] "Intellectuals to Johnson: War's the Rub," *New York Times*, 22 May 1967; Hornig to LBJ, 29 May 1967, "Donald Hornig Chronological File: April-June 1967" Folder, Box 5, Papers of Donald Hornig, LBJL. Harold Brown, physicist and Secretary of the Air Force, was in attendance.

World War II the consequences of postponing the evil day when one would have to face the campaign of indirect and direct aggression by Nazi Germany."[83]

With this stated commitment, Hornig oversaw the assignment of Vietnam-related work to the military PSAC panels. By the winter of 1967, the Ground Warfare Panel was addressing a host of relevant topics: hamlet evaluation systems, search-and-destroy and base defense operations, and military advisor training, for example.[84] The creation of PSAC's Naval Warfare Panel further typifies the transition away from the previous emphasis on conventional aspects of the Cold War arms race. In 1965, a PSAC panel devoted to anti-submarine warfare (ASW) was deeply enmeshed in establishing an Underseas Warfare Technical Center and bickering with Navy leaders and other top advisors about helicopter carriers, equipment performance, and the status of Soviet submarine capabilities. In 1967, however, Hornig oversaw the creation of a new PSAC Naval Warfare Panel. The new group initially took on a similar complement of projects connected to Soviet naval activity, "until requested by [Hornig] to devote all of its attention to a pressing problem relating to the war in Vietnam," namely, "a technical review of US military capability for reducing the quantity of material imported by sea into North Vietnam." This included studying the potential effectiveness of blockades and of mining or bombing the harbor at Haiphong.[85]

This review came as part of a more sweeping PSAC decision, made at the August 16, 1967 meeting, to assign the four military panels to undertake "an intensive study of those aspects

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[83] Hornig to Mansel Davies, 1967, "Donald Hornig Chronological File: July-September, 1967" Folder, Box 5, Papers of Donald Hornig, LBJL.

[84] Hornig to Rostow, memo with attached meeting schedule, 13 December 1967, Box 69, Country File, Vietnam, NSF, LBJL.

[85] "Activities Related to Vietnam" narrative statement, "Volume II Documentary Supplement [1 of 3]" Folder, Box 1, Office of Science and Technology, Administrative History, LBJL.

of the Vietnam problem which fall within their areas of interest," and to consider "the effective use of operations analysis" for specific problems. Each panel was also explicitly instructed to address a pressing issue: how technology could contribute to the effort "to secure and hold areas and hamlets."[86] That winter, the full PSAC met with President Johnson to discuss their potential contribution to the war effort. The meeting eventually culminated, in the spring of 1968, in a widely-distributed report on the effectiveness of previous bombing campaigns in North Vietnam, and predictions about the likely outcomes of various potential future campaigns. The context was President Johnson's bombing halt in March 1968, and, like the Jasons a year and a half earlier, the PSAC report authors presented detailed analysis to support the pause and additional de-escalatory efforts.

Indeed, two of the report's contributors—Marvin Goldberger and Gordon MacDonald—had been authors of the Jasons' barrier study and contributors to the Jasons' four-volume bombing study in 1967. As with those previous reports, the PSAC members qualified their conclusions with the acknowledgment that substantial data remained unavailable: "much of the information relating to this problem is subject to large uncertainties or simply non-existent." Nevertheless, the pages of careful analysis culminated in a set of key conclusions relating to bombing plans and an accompanying series of technical recommendations. The conclusions stated that bombing had improved morale in South Vietnam but not weakened "resolve" in North Vietnam; bombing had not prevented the transfer of supplies, either from North Vietnam or through Laos; and though additional bombing in Laos aided by new technologies might yield improved results, further bombing campaigns in North Vietnam—including the mining of Haiphong—would at best only "marginally" affect operations. Overall, the scientists wrote,

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[86] Memo to members of the PSAC Ad Hoc Vietnam Group, 25 August 1967, Folder "Donald Hornig Chronological File: July-September, 1967" Folder, Box 5, Papers of Donald Hornig, LBJL.

"None of the studies we have seen of possible expanded campaigns makes a convincing case for a campaign of expanded scope in the northern route package."

These conclusions were accompanied by six technical recommendations, several of which drew on DCPG and barrier-related work as potential means to improve the accuracy and effectiveness of existing operations. Specifically, the PSAC members urged military planners to identify and procure new equipment to improve interdiction campaigns: "High priority should be given to an intensified development and production program to equip aircraft with a system of night-viewing sensors and displays and weapons of high accuracy," they wrote. This would include work on laser-guided bombs and a "crash program to improve the capability of the Mark 36 and BLU 45 bomblet land mine," among other weapons. Aerial photographic reconnaissance should be expanded, as well as electronic surveillance and analysis of 'enemy logistic systems.' The effectiveness of the barrier should be reviewed, to ensure "its most effective use as an integral part of future interdiction campaigns."

The main body of the report assessed the widespread physical damage wrought by previous bombing campaigns in North Vietnam, including the high civilian death toll and destruction of infrastructure, but still concluded that costs had successfully been passed on to the Soviet Union and that North Vietnam could likely withstand "even greater costs." North Vietnamese transportation routes were rapidly repaired so that communications and transfers of supplies were actually improving, despite the bombing. The possible exception to this trend was the bombing of Laos, but even in that scenario, the PSAC scientists offered only cautious assessments. Operations in Laos had not succeeded in hampering the flow of supplies, they wrote, but "there appears to be ample opportunity for increasing the level of attack in Laos," which could result in an increased "fraction of supplies destroyed," or perhaps the imposition of "an additional burden." Improved technology would be useful, including laser-guided bombs and additional sensor technology—in other words, greater application of equipment currently being developed as part of the barrier project.[87]

Drafts of the report were distributed to top Pentagon and military leaders. As with the Jason bombing reports, reactions predictably split along the military-civilian divide, with consensus coming only on suggestions for technical improvements. McNamara's successor, Secretary of Defense Clark Clifford, wrote to President Johnson that he thought the report's "general conclusion is probably valid" and "is consistent with other bombing studies which have been made from time to time." He was particularly impressed by the technical recommendations, however, promising "to ensure that full use is made of the Committee's Report in our continuing efforts to improve the effectiveness of our interdiction programs." He confirmed that much of this work was already underway.[88] Earle Wheeler, Chairman of the Joint Chiefs of Staff, agreed on this point: he informed the president that he was "in accord" with the recommendations, many of which were already being addressed. "Laser, Electro-Optical (EO), and Infrared (IR) guided bombs" were being tested, he reported, and "The Laser Guided bomb is under combat evaluation in Southeast Asia."

But Wheeler also noted pointedly, "I do not agree with the rationale and conclusions of the study." Where the scientists discussed matters of technology, he accepted their advice, but in their analysis of strategy, bombing assessments, and implications for morale and politics, he

[87] Draft, "The Effect of Air Strikes in North Vietnam and Laos: A Report by a Special Subpanel of PSAC," 26 April 1968, "Vietnam Material including a report by the President's Science Advisory Committee on the effects of the bombing of NVN 4/68-5/68" Folder, Box 250, Country File, Vietnam, NSF, LBJL.

[88] Clifford to LBJ, 21 June 1968"Vietnam Material including a report by the President's Science Advisory Committee on the effects of the bombing of NVN 4/68-5/68" Folder, Box 250, Country File, Vietnam, NSF, LBJL.

simply dismissed their views.[89] Walt Rostow evinced a similar attitude in his correspondence with President Johnson. "In disposing of Dr. Hornig's report on bombing," he wrote casually, "I focused my questions around the 6 technical recommendations which were not essentially controversial."[90] In this regard he likely took his cues from the president himself. After reading Hornig's initial memo introducing the report, Johnson had referred the entire matter to Rostow, along with the dismissive observation that "I basically do not regarding bombing as a matter of science."[91] As McNamara had the previous year, the president and his top advisors easily drew out the desired technical advice from the nation's elite scientists while quietly disposing of their political and strategic recommendations.

The PSAC members working on Vietnam-related problems did not appear to suffer the same ethical crises as many of the Jason participants in the DCPG steering committee, but they did engage in self-reflection. In January 1968, Sidney Drell, the Stanford physicist who was working with both the Jasons and PSAC, wrote a long letter to Hornig describing some of his views on the war and related "political-philosophical" remarks. Drell was no pacifist; as head of the Ground Warfare Panel he offered a Hornig a range of "long-range problems" for PSAC to address, including how technology could be used in missions crossing into Cambodia and Laos, how expanded "search and destroy operations" in the Mekong Delta could change the character

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[89] Wheeler to LBJ, 17 June 1968, "Vietnam Material including a report by the President's Science Advisory Committee on the effects of the bombing of NVN 4/68-5/68" Folder, Box 250, Country File, Vietnam, NSF, LBJL.

[90] Rostow to LBJ, 22 June 1968, "Vietnam Material including a report by the President's Science Advisory Committee on the effects of the bombing of NVN 4/68-5/68" Folder, Box 250, Country File, Vietnam, NSF, LBJL.

[91] LBJ to Rostow, 4 June 1968, "Vietnam Material including a report by the President's Science Advisory Committee on the effects of the bombing of NVN 4/68-5/68" Folder, Box 250, Country File, Vietnam, NSF, LBJL,

of the war, and how vocational and agricultural education programs and other forms of development aid could be useful tools.

If Drell had reservations about current war policies, they were rooted more in domestic political concerns than his assessments of Vietnamese society. He urged a halt to bombing in the "far north" of North Vietnam, though not at the DMZ, in order to win back the trust of the alienated youth in the United States, who were disillusioned and skeptical that their leaders were seriously pursuing peace. Drell similarly opposed draft exemptions for science and engineering graduate students "for reasons of improving the empathy with the youth in college."[92] When Hornig forwarded Drell's letter to Rostow, he took care to portray Drell as responsible and moderate. Drell "continues to be a positive force," Hornig wrote, "and although he has some worries, he has not let them get in the way of trying to produce technological gains which could contribute to the solution of our military and political problems in Vietnam."[93]

Given the military reactions to most of the war-related PSAC and Jason reports, Hornig's words encapsulated the administration's definition of a useful science advisor in the age of Vietnam: someone who provided the necessary technical expertise without excessive political complaint. It was a far cry from the idealistic enthusiasm of the Eisenhower days, and heralded worse conditions ahead. Less than five years later, the fragile relations between the president and the PSAC would finally disintegrate, after a political battle in which Drell himself would play a small but crucial role. As will be related in the conclusion of this dissertation, the PSAC, born

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[92] Drell to Hornig, 30 December 1967, "Vietnam 6A 1/66-3/68 Bombing Pauses in Viet Nam" Folder, Box 93, Country File, Vietnam, NSF, LBJL.

[93] Hornig to Rostow, 18 January 1968, "Vietnam 6A 1/66-3/68 Bombing Pauses in Viet Nam" Folder, Box 93, Country File, Vietnam, NSF, LBJL.

out of the Sputnik boom, perished during the Vietnam bust, its lifespan stretching from the idealism of 1957 to the disillusionment of 1973.

Military and Industrial Scientists during Vietnam

The experiences of the Jason and PSAC scientists during the Vietnam War reveal some of the ways in which elite academic scientists contributed to the war effort while still maintaining a kind of 'outsider' status, even while they remained on government and military payrolls. They enjoyed a measure of autonomy in their work and felt free to criticize particular programs and strategies. But theirs was not the only kind of scientific contribution. The military engineers and researchers employed at the limited war labs and in war-related industries conducted important studies to ensure the effectiveness of aircraft, instrumentation, incendiaries, bombs, and chemicals used in Vietnam. Some of this work consisted of retrofitting older weapons to new applications, as in the case of napalm and defoliants, but these engineers also developed cutting-edge new technologies rooted in laser research and advances in computing.

Assessing the political and ethical views among these non-academic researchers is difficult, as, unlike high-profile Manhattan Project physicists, few recorded their thoughts in memoirs and interviews, left voluminous personal papers in university archives, or were surveyed by political scientists.[94] An unscientific poll of Los Alamos employees conducted by Citizens for Peace in Vietnam in 1967 revealed strong minorities both supporting and opposing

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[94] A 1969 survey of over 60,000 academic faculty members, for example, found that physicists were among "the most liberal in the natural sciences," including in their antiwar stance: 67% opposed the war. When the pool of physicists was limited to just those deemed "achievers"—i.e., productive scientists employed at elite universities, the number opposing the war rose to 80%. "Survey Finds Physicists on the Left," *Physics Today* (October, 1972), 61-62.

the war, with a plurality of respondents refusing to state their political position.[95] In recent years, a handful of social scientists have conducted anthropological studies of the laboratory environment, but their work has largely focused on areas of nuclear research or the particular methodology and epistemology of science, rather than the specifics of Vietnam-related work.[96] In 1968 and 1969, however, doctoral researcher Jeffrey Schevitz conducted a series of 34 interviews among “weaponsmakers” in the Santa Clara valley, a region in which 60% of the working population was employed in defense and aerospace fields. Schevitz focused his attention on the 19 respondents who expressed antiwar views and at least some moral qualms about their work. He set aside the remaining 15 subjects, categorizing them as ‘flag wavers’ and ‘do-nothing patriots’ who assumed their work was moral and worthwhile. (Schevitz clearly assumed the opposite.) He attributed their professional decisions to the corporate pay and benefits of the defense industry, the opportunity for advancement, and the intellectual challenge of their work. Although Schevitz’s conclusions strongly reflected his own antiwar outlook, his interviews and employee profiles revealed a wide range of attitudes toward the war and the ethical implications of weapons-related research. His subjects were aware of the context of their work, and either embraced its applications (as in the case of the 14 respondents who supported the war effort), rejected them and either changed jobs or tried to organize for change within their place of employment, or, in Schevitz’s analysis, stuck by their jobs and “rationalized” their moral situation.

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[95] Mrs. Joseph Mather to Rep. E.S. Walker, 1 April 1968, in “Vietnam, 1963-68” Folder, HUG (FP) 94.18, Kistiakowsky.

[96] See, for example, Hugh Gusterson, *Nuclear Rites: A Weapons Laboratory at the End of the Cold* War (Berkeley: University of California Press, 1996); Sharon Traweek, *Beamtimes and Lifetimes: The World of High Energy Physicists* (Cambridge: Harvard University Press, 1988); Bruno Latour and Steve Woolgar, *Laboratory Life: The Construction of Scientific Facts* (Princeton, NJ: Princeton University Press, 1986).

Though not a large and scientific survey of defense workers in the Vietnam era, Schevitz's study nevertheless offers a glimpse of the prevailing attitudes in this demographic, which mirrored, to some extent, national views. The majority of his respondents either supported the war or had qualms too minor to outweigh the benefits of their employment. An energetic minority expressed open opposition, resigning in protest or working from within to try to change policy. Over time, of course, self-selection would fill these lost positions with additional pro-war or apolitical candidates; by the 1980s scientists employed by defense firms tended to have more favorable views of various military programs than academic and other non-defense scientists.[97]

In the spring of 1968, George Kistiakowsky observed that academic scientists were being pushed out of their Pentagon advisory roles, which were instead "largely taken over by professional military scientists and those in the aerospace industry and think-tanks."[98] This trend was visible in the influx of Mitre engineers contracted to work on DCPG projects, and in the makeup of the Air Force's Science Advisory Board, whose membership in the 1960s increasingly drew on experts from industry and the nation's military labs.[99] But the shift was not just from academic to non-academic. Even as many younger scientists took strong stands against the war, others in their cohort held contrasting political views, and enthusiastically entered the defense world with a realistic understanding of their role within it. Two key military advisors, William McMillan and John Baldeschwieler, illustrate this trend.

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[97] Peter D. Hart Research Associates, *A Survey of Physicists' Attitudes Toward the Strategic Defense Initiative,* Box 2, Folder 4, American Physical Society, Records of Directed Energy Weapons (DEW) Study, 1983-1988, American Institute of Physics, Niels Bohr Library, College Park, MD 20740, USA.

[98] John W. Finney, "Pentagon Scored on Scientist Loss," *New York Times,* 21 May 1968**.**

[99] For a breakdown of membership from 1946 to 1964, see Appendix C in Thomas A. Sturm, *The USAF Scientific Advisory Board: Its First Twenty Years, 1944-1964* (Washington, DC: Office of Air Force History, 1986).

Born in California at the end of World War I, McMillan studied chemistry at UCLA and chemical physics at Columbia, where he received his PhD and contributed to Manhattan Project materials research. After a brief stint working for Edward Teller at the University of Chicago, he landed at UCLA, where he was conveniently located for consulting work at Livermore and RAND during the boom years of the 1950s. He also spent time as a member of the Air Force Scientific Advisory Board, chair of the Weapons Advisory Group, and chair of the Divisional Advisory Group at Eglin Air Force Base.

McMillan later explained his advisory work at RAND as "driven by patriotism" and encouraged by friends and colleagues from UCLA. In his early years there, he researched the effects of ionizing radiation, contributed to a study devoted to reducing the size of thermonuclear warheads and, in 1961, co-wrote the influential RAND report "Arguments in Support of the Proposed Atmospheric Nuclear Effects Tests," which offered justifications for the temporary resumption of atmospheric testing in 1962.

In an oral history conducted nearly thirty years later, McMillan reflected on the character of science advising during the 1960s. In some ways, the decade was a kind of golden age "when we could [get] things done rapidly"—for instance, when work on ionizing radiation suggested potential problems for guidance computers, RAND scientists gained quick access to top military commanders and millions in further research funding. But in other ways, the 1960s seemed a step backwards—McMillan mourned the retreat of academic scientists back to their university labs after the extraordinary cooperation of World War II. Working with the DDR&E Harold Brown, McMillan helped found the Defense Science Seminar, a Jason-like effort to recruit a younger generation of scientists as military advisors. The seminars, held over the summers on the

UCLA campus, included presentations from prominent speakers, field trips and demonstrations, and other efforts to "pump them up so they would know what some of the issues and problems are."[100]

In 1966, John Foster, whom McMillan had known from a summer spent at Livermore, invited McMillan to serve as a science advisor to Gen. Westmoreland. McMillan agreed, and ended up spending two years in Vietnam, working for both Westmoreland and his successor, Gen. Creighton Abrams. In this capacity, McMillan enjoyed widespread respect and unique access to military top brass. McMillan shared a house with Westmoreland, and so had the ear of the general and a front-row seat for military planning decisions. Though a staunch defender of Westmoreland, McMillan was not uncritical—he was shocked at the lack of genuine debate and discussion at military meetings, for example, and convinced Westmoreland to take a cue from academia and encourage honest dissent. As a scientist—a military outsider—McMillan was sometimes more easily able to take critical positions without disrupting the hierarchical deference prevalent in military circles. On one occasion he gently chastised Westmoreland's unfounded fixation on the likelihood of a post-Tet coup by citing a Peanuts comic strip in which Lucy, informed that what she thought was an insect queen was actually a jelly bean, wonders aloud how a jelly bean ever got to be queen. Westmoreland laughed and acknowledged the message that "we mustn't become enamored with our theory." Indeed, Westmoreland was quite taken with McMillan's ability to offer brilliant, blunt, "practical and pragmatic" advice, which could quickly "disabuse the staff of any self-satisfaction and complacency." He particularly valued McMillan's role as a kind of "devil's advocate," noting that if someone of McMillan's

[100] William G. McMillan, Interview by James J. Bohning in Los Angeles, California, 25 March 1992 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0104).

intellectual stature "questioned why we did things a certain way, it compelled us to re-examine our methods."[101]

In addition to this rhetorical role, McMillan also contributed to hundreds of small efforts to use science to improve U.S. military efforts. He helped pioneer the use of Fragmacord linear mines, which would be set along drainage ditches on trails prone to enemy infiltration. U.S. soldiers would wait along one side of the trail and open fire on infiltrators, who would then dive into the opposite side ditch, lined with the Fragmacord. He also worked with ARPA on a failed effort to use blimps as silent surveillance devices, on the DELILAH system targeting the radar of enemy surface-to-air missiles, on studies of the effectiveness of the bombing campaign, and on tunnel detection. McMillan also continued his recruiting work, inviting "scientists, engineers, and social scientists… to join the Office of the Science Advisor in Vietnam as consultants for periods ranging from one month to over one year. These individuals were drawn from government laboratories, nonprofit institutes and—where possible—from the academic community. At its maximum this group numbered about 40, with a total of about 100 during my tenure."[102] To create this program, McMillan worked closely with John Foster, Generals Westmoreland and Abrams, and other key Pentagon personnel. He described the positive effects: "The stimulation of seeing the Vietnam problems firsthand and of being able to grasp the opportunity provided by technology to help out in a constructive and substantive way gave rise to an almost unparalleled esprit among these consultants."[103]

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[101] William Westmoreland, *A Soldier Reports* (Garden City, NY: Doubleday, 1976), 267-268.

[102] W.G. McMillan, "The Scientist in Military Affairs," in Jonathan Allen, ed., *March 4: Scientists, Students, and Society* (Cambridge: MIT Press, 1970).

[103] McMillan, "The Scientist in Military Affairs," in Allen, 20-21.

In addition to collaboration with ARPA and Army engineers, McMillan also worked with Los Alamos director Harold Agnew. In 1967, as plans for the creation of the DCPG barrier and other more traditional barrier constructions took shape, McMillan reached out to Agnew and the research scientists at his disposal. The problem was how to build a barrier in a particular area where the North Vietnamese had set up artillery and were constantly disrupting construction. As McMillan later recounted, Westmoreland's first response was to instruct the Air Force to attack enemy territory, to 'bomb them out of existence.' But indiscriminate bombing rarely destroyed artillery pieces that had been carefully shielded—only pinpoint accuracy would be successful. Researchers at Los Alamos, however, worked out an acoustic locator system and built several prototypes, which McMillan tried desperately to have mass produced and distributed. He had control over the creation of prototypes, but mass production and adoption depended on frustratingly slow bureaucratic processes. Even funding became a problem—when McMillan approached John Foster about reimbursing LASL's research expenditures, Foster reportedly refused, exclaiming, 'For Christ's sake, they've got a two hundred million-dollar budget!" Gen. Creighton Abrams eventually endorsed the acoustic system, but only a handful were ever distributed. McMillan came to deplore bureaucratic impediments to what he considered obvious technical improvements, inefficient decision-making processes, and, in the case of Robert McNamara, a lack of basic scientific knowledge (he recalled McNamara once asking a team of radar specialists what a decibel was). Despite McMillan's best efforts, the glory days of coordinated war research and development seemed hopelessly lost. It seemed that only for projects that could be easily addressed on the ground in Vietnam, without resorting to the slower stateside Pentagon channels, would results come quickly.

Unlike Kistiakowsky, however, McMillan's frustrations led him to embrace the opportunities for weapons work available in the private sector. He returned from Vietnam more interested in tactical weaponry, and started his own business, McMillan Science Associates, specializing in "operations analysis" and working with major contractors on a variety of projects, including the development of SCUD missiles. He later became a champion of President Reagan's buildup of high tech weapons and their successful use during the first Gulf War.[104]

John Baldeschwieler was one of the "students" in McMillan's 1966 Defense Science Seminar. Baldeschwieler's youth and his scientific background—a mix of elite academic, military, and industrial experience—seemed ideal for McMillan's program. Baldeschwieler had attended Cornell on a Standard Oil scholarship, where he studied chemical engineering and held a summer job at DuPont and Los Alamos, joining ROTC during the Korean War. He was, as he explained in a 2001 oral history, interested in "the military and the weapons side of things." Specializing in infrared spectroscopy, but known for his talent with instrumentation, he did his graduate work at Berkeley, completed his military service in the form of "neutron permeability" research at Aberdeen, spent six years at Harvard (where he frequently taught Kistiakowsky's classes when the advisor was off on government work), and landed at Stanford in 1965.[105]

At Stanford, Baldeschwieler was first chosen for the UCLA summer seminar, for which he was a "natural." An invitation from the Army Scientific Advisory Panel quickly followed, which, to Baldeschwieler, made "good sense." He described the work as "interesting and fun"

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[104] John D. Baldeschwieler, interviewed by David C. Brock and Arthur Daemmrich at the Chemical Heritage Foundation in Philadelphia, 13 June 2003 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0280).

[105] Baldeschwieler, John D. Interview by Shirley K. Cohen. Pasadena, California, January-February, 2001. Oral History Project, California Institute of Technology Archives. Also available online at: http://oralhistories.library.caltech.edu/154/01/Baldeschwieler_OHO.pdf

and was proud to have the opportunity to do "hopefully useful things." As for the war itself, Baldeschwieler recalled (albeit thirty years later), "I was very loyal to the US government, and I remain very loyal. My attitude at the time was that the war itself didn't make a lot of sense to me, but the young servicemen who were exposed were my age and my colleagues, and I felt… I would do the best I could to give them tools that would help save their lives."

During a month-long visit to Vietnam, Baldeschwieler worked with a team from General Electric on a potential "people sniffer"—a portable sensor that could detect human odors and other exuded chemicals, to be used in dark jungle conditions. The technology proved less successful than hoped, but Baldeschwieler enjoyed the experience. He also became involved in the implementation of the DCPG barrier, and the sensor technologies used at Khe Sanh and along the Ho Chi Minh Trails. Like McMillan, he found himself frequently challenging conventional wisdom, providing much-needed doses of scientific skepticism. He recalled one such instance, when junior high-school math skills allowed a particularly harsh criticism of military policy:

> I was given a briefing by a young lieutenant colonel in a nicely starched uniform who talked about the hamlet pacification program. You'll remember that McNamara at that time had stressed the quantitative evaluation of everything. So there was a hamlet pacification index. This was calculated by having a team go in and interview the village father and count the number of foxholes around and the other defensive measures. All kinds of things. You'd check this off, and it went into a computer. The colonel showed me a plot of the hamlet pacification index as a function of time. It was a straight line. It started in the mid-sixties and went on up through the time that I was there. … Well, I looked at the curve and I asked, "When were US forces introduced into Southeast Asia in large numbers?" I then said that what the curve showed was that the introduction of US forces into Vietnam made no difference at all. And the room was utterly silent. Do you remember when the presidential candidate from Michigan—George Romney—went to Vietnam? He came back and said he had been brainwashed, and that ultimately led to his withdrawing from the Republican presidential primaries and his loss of credibility. Well, I'm sure I heard the same briefings, the same stuff. It really didn't make much sense. I can tell you many, many such stories.[106]

[106] Ibid.

Baldeschwieler's competence and candor attracted the attention of Donald Hornig, who invited him to join the relevant PSAC ground warfare subpanel. Eventually, Baldeschwieler became a full-fledged regular PSAC member, at the moment of transition from President Johnson to President Nixon and Donald Hornig to Lee DuBridge as chief science advisor. Baldeschwieler recalled feeling a bit out of place: "it was an interesting assignment, because…most of the people involved had backgrounds in either nuclear physics or were from the MIT Radiation Lab… I was the token chemist." He remained active in science advising, however, even taking a leave from Stanford at the start of President Nixon's second term to join the Office of Science and Technology, home to the remaining government science advisors after the dismantling of PSAC.

To the generals and presidents he served, Baldeschwieler was a brilliant applied mind. To an increasingly radical subset of the antiwar population, however, he embodied a kind of technocratic false consciousness. Even during his 2001 oral history interview, Baldeschwieler's casual reflections could have been the stuff of New Left anti-science pamphlets decrying the supposed neutrality of science. Describing the dangers of his work in Vietnam, Baldeschwieler observed that "the fighting was everywhere, and I was shot at a number of times. I recall wondering, 'What are they shooting at me for?' I hadn't done anything—I was just trying to get the technology straight."[107]

The Fate of the DCPG

While McMillan argued with Westmoreland and Baldeschwieler worked with the Army's Scientific Advisory Panel, the early results of the Jasons' Dana Hall reports had taken physical

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[107] Ibid.

shape in the two-part "Muscle Shoals" system, later renamed "Igloo White," consisting of MUD RIVER, the anti-vehicular barrier located largely in Laos, and DUMP TRUCK, the anti-personnel barrier to be located along the western half of the DMZ into Laos. Results were far from the near-perfect success rates anticipated by the Jasons. A Pentagon report from April 1968 complained that "the value of any intelligence estimate based on sensor information is questionable." Particularly near Khe Sanh, enemy forces seemed to be eluding detection. Frequent artillery use and passing planes overstimulated the local sensors.[108] Reviews of MUD RIVER conducted in the spring of 1968 noted that almost half of the targets relayed by sensors were never investigated, often due to poor weather conditions, and of the targets confirmed, only half were pursued, with a further 67% success rate. As one official complained, "it costs us more than $55,000 for each enemy vehicle destroyed or damaged."[109]

Pilloried in the press and deemed expensive and ineffective by military analysts, the barrier was never fully installed. By the spring of 1968, its function had already shifted to a kind of data collection; in the words of Earl Wheeler, the Chairman of the Joint Chiefs of Staff, the system had "evolved from one which initially produced 'real time' intelligence on truck targets to one which now provides an in-depth analysis of traffic movement patterns, choke points, truck parks, and bypasses within the Laotian Panhandle transportation complex," enabling more effective air strikes "on the most lucrative truck targets in Laos." Moreover, after "antipersonnel" sensor technology had been employed during the battle of Khe Sanh, the Pentagon approved plans for use of the barrier's technical "assets" to support "ground operations in South Vietnam."

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[108] Department of Defense messages, 4 April 1968 and February 1968, "Vietnam Barrier, 2D 9/66-9/68 [2 of 2]" Folder, Box 74, Country File, Vietnam, NSF, LBJL.

[109] Memo, Earl Young to Bromley Smith, 9 September 1968, and attached report, "Southeast Asia Analysis report for April 1968," "Vietnam Barrier, 2D 9/66-9/68 [2 of 2]" Folder, Box 74, Country File, Vietnam, NSF, LBJL.

By the summer of 1968, the stage was set for testing "eight different combat applications" of former barrier technologies, with an eye toward integrating DCPG "surveillance techniques into ongoing ground tactical operations." In 1967 Kistiakowsky had worried that the barrier would not be accompanied by a reduction in bombing. Now, rather than offering a stepping-stone to de-escalation, the fruits of the Jasons' early work on the barrier were being transferred to the ground war in South Vietnam.[110]

As Dickson observes, "the McNamara Line did not so much die as it became the 'Westmoreland Umbrella,'" defined as the "flexible and mobile" application of new and updated technologies throughout "South Vietnam and into Laos, Cambodia, and Thailand."[111] Despite the travails of its advisory committee, the DCPG itself continued for another five years after the Dana Hall reports, overseeing the development of new kinds of sensors, mines, and laser and TV-guided bombs, as well as the refurbishing of older technologies for application in southeast Asia. These included "spider bombs," the air-dropped weapons whose swath of wire triggers could produce an "anti-personnel" detonation covering 200 feet; "daisy cutters," the updated World War II-era blockbuster bombs that could destroy enough jungle vegetation to create "a bald spot about the size of a football field"; and "cluster bombs" refitted to carry tear gas or flechettes, which Dickson describes as "steel darts whose prime function is to shred human flesh or pin people to trees."[112] The most influential technical developments promoted by DCPG, however, were advances in sensor technology, signal processing, and computing, areas of

[110] Wheeler to LBJ, 17 June 1968, and PSAC report, "Vietnam Material including a report by the President's Science Advisory Committee on the effects of the bombing of NVN 4/68-5/68" Folder, Box 250, Country File, Vietnam, NSF, LBJL.

[111] Dickson, 44.

[112] Dickson, 45.

research that had roots in earlier VELA work, were expanded during Vietnam, and would form the controversial underpinnings of the Reagan-era Strategic Defense Initiative.

The Jasons during Vietnam, Part III

In 1984, at a celebration in honor of the 25th anniversary of Jason, Gordon MacDonald, who had been Maxwell Taylor's deputy at IDA during Vietnam, asserted that the war had not been "a major consideration among those who founded Jason" and that work related to Vietnam was largely "sporadic and, for the most part, the result of individual initiatives." The exception, however, was the DCPG. MacDonald presented a series of lessons learned from the experience. First, in order for a "study" to have effective results, there must be "a customer with the authority and access to resources to implement the findings of the analytic effort." In this case, the customer was McNamara, who had been involved from the very beginning, and "gave it the personal attention that was required." Additionally, the group performing the study should be familiar with the topic and work according to its strengths—in this case, although the Cambridge group was less familiar with limited warfare, the California Jasons had greater expertise, and the group was able to produce a largely technical study that avoided administrative or managerial recommendations. Their work wasn't rigidly technical, however—they allowed for plenty of flexibility in the development and implementation of the actual system. Thus, in MacDonald's view, the very aspects of the DCPG that had so disillusioned Kistiakowsky were actually strengths: the Jasons' lack of political influence, the evolution of the project to include new technologies with expanded wartime applications, and the power of a single administration official, McNamara, to set the course of development and application.[113]

[113] Gordon MacDonald, "JASON and the DCPG—Ten Lessons" (speech delivered at the Jasons' 25th Anniversary Celebration),30 November 1984, Box 37, Folder 12, MGM.

Such a view of the DCPG, offered a decade and a half later, at the height of the Reagan boom, probably did not reflect the actual ambivalence among Jason members in the late 1960s. It is hard to gauge exactly how problematic Vietnam was for Jason members. “Ninety percent of us are doves,” one member reported anonymously to *Science* in 1973, but the practical meaning of the term was left unexplained.[114] In a series of oral history interviews conducted years and sometimes decades later by Finn Aaserud for the American Physical Society, recollections were mixed. Kenneth Watson, for example, acknowledged that antiwar sentiment “may have contributed to people’s leaving,” but there was no “great exodus.”[115] Internal documents from the period, however, suggest that the war and the fate of the DCPG project caused substantial angst among many Jasons. Without mentioning Vietnam explicitly, minutes from the January 1968 Steering Committee meeting reveal a new level of turbulence, exemplified by a wave of resignations and withdrawals: Gomer, Matthias, and Caldwell had resigned, with Dyson tentatively offering to do so as well, while Luis Alvarez and Donald Glaser had opted to retreat into “inactive” status. When the same group met three months later, they agreed that more recruitment was necessary, and hoped that committee members could “suggest possible candidates with the emphasis on ‘bright applied types.’” Something had changed—the original conception of the Jasons as young, iconoclastic geniuses was fading, replaced by a new vision marked by that telltale modifier: “applied.”

Although early schedules for the spring 1968 meeting included planned sessions on the barrier and the North Vietnamese air defense system among the standard ABM, satellite systems,

[114] Deborah Shapley, “Jason Division: Defense Consultants Who Are Also Professors Attacked,” *Science* 179 (2 February 1973).

[115] Interview of Kenneth Watson by Finn Aaserud on 10 February 1986 in La Jolla, CA, American Institute of Physics, College Park, MD USA, http://www.aip.org/history/ohilist/4939.html (accessed 3 May 2011).

over-the-horizon detection, and command-and-control topics, by February, Jason was reaching out to the Department of Transportation, expressing interest in projects related to air traffic control, noise abatement, and other less explicitly defense-related topics.[116] In March 1968, the Jasons participated in a day of special sessions to discuss the larger political implications of their work, and "major events followed quickly."[117] The revised spring schedule now included an informal Saturday morning discussion about Jason and the war, to be followed by an executive session on the same topic with Maxwell Taylor. Attendees reportedly expressed "a wide spectrum of opinions" centered on "a sense of moral outrage at the war."[118] The summer session followed suit, as Steering Committee members planned for a "postmortem" after inevitable "JASON-and-the-war conversations" among members. That summer and fall, the Steering Committee met to discuss possible reasons for poor attendance at the 1968 summer meeting. Blankenbecler, Drell, Dyson, Foley, Goldberger, Kendall, Ruderman, Salpeter, Sands, and Weinberg—all of whom had attended in 1967—had opted to skip the following year. The committee proposed that perhaps coincidence, competing meetings, or subpar remuneration were to blame. They never mentioned a possible lack of desire or ideological dissatisfaction. The following month, they voted to increase compensation for summer meeting attendance.

By the spring of 1970, it had become clear that whatever administrative or organizational problems had been troubling Jason, deep-rooted political and ideological concerns had not been

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[116] These Vietnam-related topics are listed on the February summary of the May agenda, but not on the April summary.

[117] This meeting is mentioned in a later memo from Hal Lewis to the Jasons, 22 May 1970, in Box 37, Folder 3, MGM.

[118] Hal Lewis's report on the meeting is quoted in Ann Finkbeiner, *The Jasons: The Secret History of Science's Postwar Elite* (New York: Penguin, 2006), 91.

addressed. In May, Edwin Salpeter submitted a letter of resignation to Hal Lewis, cc'd to the entire Jason membership. He wrote:

> As you know, for several years now I have had misgivings about DoD consulting while the Vietnam war continues, but on the whole I felt (as Herb York expressed in a letter last year) that one can do more good by "working from within." In principle I still feel this way, but in practice I consider the present Nixon-Agnew administration's behavior so reprehensible that I am simply unable to "do business as usual."
>
> For this reason I herewith wish to resign from JASON. I hope reason will eventually prevail in the executive branch so we can all reevaluate the situation.[119]

Salpeter was joined in his action by other members for whom the Cambodian invasion of 1970 proved a tipping point. The science journalist Ann Finkbeiner has estimated that as many as nine Jason members resigned in the aftermath of the barrier debacle. Some never formally resigned, but simply stopped attending.[120]

In his response to Jason members, Lewis acknowledged that "many members of JASON have long felt anguished about the fact and the prosecution of the war in Viet-Nam, an anguish which has been exacerbated by the Cambodian adventure, and which I share." As in 1968, he suggested setting aside a day of the upcoming summer study "to talk about Viet-Nam, Cambodia, JASON, the Universities, etc., and to decide whether JASON, as an entity, should do anything."[121]

Besides the shift in membership, the Jasons did not really "do anything" during the early Nixon years. Rather, the organization continued its defense-related work, with some additional attention to non-military topics. Meetings in 1970 and 1971 included top secret sessions on Cambodia, communications, Soviet expansion in the Mediterranean, nuclear explosions as

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[119] Salpeter to Lewis, 12 May 1970, Box 37, Folder 3, MGM.

[120] Ann Finkbeiner, *The Jasons,* 91

[121] Lewis to Jason members, 22 May 1970, Box 37, Folder 3, MGM.

possible earthquake triggers, reactor technology, anti-hijacking technology, housing and transportation technologies, lasers, and the supersonic transport. Internal correspondence focused on upcoming administrative changes in 1973, when the Jason contract with IDA was set to end, to be replaced by new institutional ties to the Stanford Research Institute.[122]

The Jasons and the New Left

Although the Jasons' parent organization, the Institute for Defense Analyses, had long been a target of antiwar protests (resulting, for example, in the termination of its special housing arrangements at Princeton in the early 1970s), only a few partial reports of the Jasons' wartime activities had dribbled out in the years since the barrier. In 1971, however, the publication of the Pentagon Papers thrust the organization into a withering public spotlight. For the first time, detailed descriptions of the Jasons' work were revealed, including summaries of the Jasons' bombing reports, particularly the sweeping and detailed criticisms of the1967 multivolume study, as well as the initial planning and development of the electronic barrier, including its vast and lethal minefields. Some titles of Jason reports were also released, including the infamous "Tactical Nuclear Weapons in Southeast Asia," without accompanying synopses or explanations.

Overall, the depiction of the Jasons in the Pentagon Papers was neither overstated nor inaccurate, and in the larger sweep of revelations about the prosecution of the war and Pentagon decision-making, the participation of scientists might have seemed a minor footnote. But among the growing New Left, rooted as it was on college campuses, the account of the Jasons' work both confirmed suspicions of the reach of the military-industrial-academic complex and, at two of the most politically active campuses in the country, Berkeley and Columbia, it offered up

[122] Lewis to MGM, 12 April 1973, Box 37, Folder 7, MGM.

convenient local targets of protest. In a bitter and sometime violent attack, New Left critics rejected the Jasons' defensive claims that they had been working for de-escalation. Viewed in another, much more critical light, the Jasons appeared to be prestige-hungry collaborators who had created a lethal, civilian-killing component in an immoral war, all while cloaking themselves in the guise of dovish intellectuals trying, ineffectively, to work from within. Covering the controversy for *Science,* Deborah Shapley quoted Fred Bramfman of the research organization Project Air War, who charged that the electronic battlefield constituted a war crime, built upon personnel bombs that "cannot distinguish between soldiers or civilians." Bramfman acknowledged that the Jasons may "have had a more beneficial effect than some others." But in his view that simply meant "they are lesser, rather than greater, war criminals. They are dramatic examples of how it is possible to be a moderate, well meaning, decent war criminal." He called on the Jasons either to resign from their posts or, following the precedent of the Pentagon Papers, to sabotage Defense Department operations through further document leaks.[123]

Jason scientists faced sudden hostility from academic audiences the world over, as students and professors at international conferences and symposia protested their appearances and picketed their presence. French scientists rejected Murray Gell-Mann's attempt to speak at the College de France, and Jason physicists at a symposium in Trieste faced a crowd of 300 protesters, in a confrontation that ended with the deployment of French riot police.[124] Protesters accosted Sidney Drell in Rome and in Corsica.[125] The Jasons' most vigorous opponents, however, were the members of the Scientists and Engineers for Social and Political Action

[123] Deborah Shapley, "Jason Division: Defense Consultants Who Are Also Professors Attacked," *Science,* 2 February 1973

[124] SESPA, "Science Against the People," December 1972, Box 5, Folder 3, Papers of Brian Schwartz, 1966-1977, Niels Bohr Library, American Institute of Physics, College Park, MD.

[125] Shapley, "Jason Division: Defense Consultants Who Are Also Professors Attacked."

(SESPA), an organization discussed in greater detail in Chapter Five. In New York, the local SESPA chapter picketed and leafleted every week outside Columbia University's Pupin Hall, home of the physics department and five Jason members. The Jason members, in turn, complained of harassment: hate mail, irate phone calls, vandalism. On April 24, 1972, the "New York Anti-War Faculty," a coalition of professors and students from twenty New York universities, occupied Pupin Hall for four days to protest the Jasons' war work.

Unprepared for this level of hostility and anger, most Jasons responded with deep defensiveness; they closed ranks and publicly portrayed their opponents as thuggish and misguided. Columbia physicist Malvin Ruderman told *Science*'s Deborah Shapley, "It's impossible to resign under this kind of tactic. Nothing could be better designed to draw us together."[126] Under the cover of anonymity, however, a handful of members confided to Shapley in early 1973 that their organization had made grievous mistakes. One Jason stated bluntly, "Obviously we blew it. When McNamara came to us in 1966 we should have told him to shove it and made a public statement." Others explained only that they had failed to achieve their goal of de-escalation, or had antagonized the military brass and found themselves with little meaningful influence. Some members tried to emphasize the group's laudable work on arms control. One Jason even violated security regulations to assure Shapley that "Tactical Nuclear Weapons in Southeast Asia," despite its title, gave "all the reasons why you wouldn't use nuclear weapons in Vietnam." Though Shapley's 1973 article afforded the Jasons an important forum through which to defend their actions, Shapley herself characterized the group as stuck in a predicament of its

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[126] Ibid.

own making. "Like the Jason of mythology," she wrote, the Jasons had "sewn a field with dragon's teeth which have sprung up into a host of hostile soldiers."[127]

Among those hostile soldiers were the members of the Berkeley SESPA chapter, led by the young physicist Charles Schwartz. In December 1972, Schwartz spearheaded the group's publication of a forty page expose of the Jasons' Vietnam work, titled "Science Against the People." It was, as the cover proclaimed, "The story of Jason—the elite group of academic scientists who, as technical consultants to the Pentagon, have developed the latest weapon against people's liberation struggles: 'automated warfare.'" The authors quoted at length the sections of the Pentagon Papers that mentioned the Jasons, placing the group squarely in "the technological wing of the military-industrial complex." Whatever the Jasons' intentions had been, they had offered their services to the Pentagon, and the result was the construction of the barrier, an "intervention contributing decisively to the prolonging of the Indochina war."

Schwartz and his colleagues interviewed five Jasons in the summer of 1972, and published summaries in the SESPA report. The activists offered clear admiration for the brilliance of their subjects, noting that three of the scientists—Charles Townes, Donald Glaser, and Luis Alvarez—had received Nobel Prizes for their discoveries. But they found the Jasons' justifications of their action thoroughly unconvincing. Marvin Goldberger told SESPA that he had been disillusioned about the war since 1966, and had hoped the barrier would be a means to end the war. Glaser noted that at its best, Jason could help the Pentagon avoid irresponsible decisions and investments in wasteful systems. With the barrier, however, he felt that military planners "used us technically but didn't listen to us."

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[127] Ibid.

SESPA responded that traditional categories like "dove" and "hawk" did not apply to scientists, because while a physicist like Edward Teller was clearly a hawk, it was dovish, gentle Oppenheimer who oversaw one "the most lethal innovations in modern warfare." Many of the Jasons who had promoted arms control were brilliant, creative physicists and caring and conscientious teachers. But while they might "publicly profess to be against the war, they continue to contribute their scientific talents to the military." One by one, SESPA rejected the kinds of justifications and excuses offered by the Jasons: that they weren't actually a powerful group and their advice was frequently ignored; that they were 'boring from within' and working for the same change as outsiders like SESPA; that if good scientists didn't give advice, the Pentagon would look to mediocre scientists; that they hadn't known the true nature of the war early on; and, in what SESPA dubbed the "positive integral theory," that while Vietnam was a bad war, it was outweighed by other, more US constructive military projects.

SESPA countered that whatever the Pentagon reaction, the Jasons offered advice that was intended to be followed, with the goal of improving military effectiveness. The Jasons never disputed the objectives of military policy; they only evaluated the means: "Not whether to suppress guerillas in Thailand, but only how." As for the claims of ignorance, SESPA noted that Jason members, like everyone else, had access to the writings of Jean LaCouture, Bernard Fall, and David Halberstam. (They didn't know that Fall himself had addressed the 1964 Jason summer session.) Instead, they proposed that the Jasons had fallen prey to their own vanity and ambition, succumbing to the allure of secrecy and power.

But whatever the real reason "Why They Do It," the problem of accountability remained. Jasons contended that they were indeed concerned about the applications of their work, and considered their decisions important matters of personal conscience. SESPA utterly rejected this standard. Matters left to the Jasons' "private conscience" affected the entire world, they argued, and had the power to destroy human life. "In such circumstances," the SESPA authors wrote, "a posture of 'I will decide what is best' is enormously arrogant."[128] Personal conscience was no match for the military industrial complex.

Since the end of the war in Vietnam, public speculation about the Jasons has predictably ebbed, but never disappeared entirely. The Jasons themselves still exist, preparing classified reports for Pentagon clients on traditional matters of weaponry and defense, but also covering new fields like cyber-security, bioengineering, nanotechnology, data mining, quantum computing, urban surveillance, and climate change.[129] Several of the organization's Vietnam-era reports, including those connected to the barrier and bombing in North Vietnam, have in recent years been declassified, in redacted form, and are available at the LBJ Presidential Library. In the 1980s and 1990s, the Danish scholar Finn Aaserud conducted a series of oral history interviews with former Jason members, all of which reside in hard copy at the American Physical Society's Niels Bohr Library, and most of which are available online. In 2002, after a lengthy wait, the Nautilus Institute's Nuclear Policy Project successfully obtained a redacted copy of the "Tactical Nuclear Weapons" report under the Freedom of Information Act, and publicized both the report itself and, for the first time, the detailed recollections of participants. The Jasons' internal correspondence, memoranda, and membership rosters are included in the recently opened

[128] SESPA, "Science Against the People," December 1972, Box 5, Folder 3, Papers of Brian Schwartz, 1966-1977, Niels Bohr Library, American Institute of Physics, College Park, MD.

[129] Partial listing of recent Jason reports available through the Federation of American Scientists, at http://www.fas.org/irp/agency/dod/jason/ (accessed 16 January 2011).

Murray Gell-Mann Papers at the Caltech Archives, and these newly available documents, in addition to the sources listed above, form the basis of this chapter.

In 2006, the science journalist Ann Finkbeiner published the first book-length treatment of the Jasons, based largely on recent interviews with current and former members, many of whom were still requesting anonymity even decades after their years of service had ended. Writing about the aftermath of the Jasons' work in Vietnam, Finkbeiner defended the group from most of the accusations of SESPA and other New Left critics. The Jasons had been naïve and made mistakes, she wrote, but they had not been immoral. "Perhaps at the conjunction of science and the military," Finkbeiner explained, "no universal moral law exists that says, this side of this line, good; this side, bad." Fundamentally, any decision about the proper location of the ethical line had to be left to the individual scientist. She agreed with Sidney Drell that "scientists must decide for themselves."[130]

In the academic world of the early 1970s, however, it was not the individualism of the Jasons but the sweeping ethical reckoning of SESPA—formulated on an institutional and global scale—that was ascendant. On campus after campus, radical critics argued that a scientist's personal handwringing meant little if his academic funding rested on multimillion dollar Pentagon contracts and his professional societies welcomed visits from the President and his cabinet. Reports on technological inefficiencies did nothing to address the moral implications of US foreign policy or the global death toll facilitated by the military industrial complex. A new era of ethical calculation was dawning, and it rendered insufficient and archaic the old Los Alamos ideal of patriotism tempered by personal conscience.

[130] Finkbeiner, 115.

Overview: Institutions and Neutrality in Wartime

By the late 1960s, the disillusionment with the Defense Department and the war in Vietnam felt by government experts like George Kistiakowsky extended to large segments of the American population as a whole, including many young scientists and engineers. For the most radical of these critics, *any* recipients of military funding, from university lab staffers to individual professors conducting seemingly basic research, were now guilty of collaboration in an immoral war effort. Also under attack were the professional societies that supported academic scientists, facilitated the distribution of research and funding information, and organized forums through which scientists could potentially provide influential collective statements. Thus, the widespread unpopularity of the war in Vietnam brought new scrutiny and new levels of hostility toward scientists far beyond the small circle of advisors whose work directly contributed to the war, with much of the criticism coming from scientists themselves.

This section addresses some of the effects of this shift in thinking about scientists and complicity. The first chapter explores the fate of the special laboratories at the Massachusetts Institute of Technology, which housed weapons-related research projects and embodied the financial and intellectual ties between the research university and the military. The second chapter describes internal debates among members of the American Physical Society (APS) and an internal review of research funding policies at Princeton University in 1971. In the case of MIT, a well-organized movement of students and faculty demanded an end to explicit weapons work, on the grounds that such work was both immoral and inappropriate for the university campus. The protests rocked the campus, led to the creation of the Union of Concerned Scientists, and eventually resulted in the school severing ties to one of its two special research facilities, the Instrumentation Laboratory. For Princeton, where the links between research and the war in Vietnam were less obvious, internal debates focused heavily on the idea of academic neutrality and the risks of allowing Defense Department contracts to alter university research programs and curricula in subtle ways. At the same time, the APS and other professional societies faced internal movements to democratize decision-making and allow the issuance of political statements. Activists at both Princeton and the APS called into question the very existence of neutrality, and the neutrality of basic science in particular, given researchers' deep dependence on Defense Department contracts.

In all three of these examples, a younger generation of scientist-activists pushed for reform based on a moral and political reckoning focused on institutions, rather than individuals. In their minds, the ethical obligations of scientists did not consist of mere personal decisions to embark on particular projects, but included rigorous analyses of funding sources, laboratory affiliations, and military consequences, accompanied by obligatory political action. Institutions themselves became targets, rhetoric veered toward the postmodern, and the new terms of debate alienated career lab staffers and inspired students to revolt against their university administrations. Many sympathetic Manhattan Project veterans found themselves caught awkwardly in the crossfire between their radical students and their hawkish colleagues, seeking out a middle ground unlikely to please either side.

The controversy over university defense contracting and the problem of neutrality had both short-term and long-term consequences. University policies and funding structures changed, though not always in the ways hoped for and certainly not on the scale imagined by protesters. The failure to "convert" weapons laboratories to civilian purposes and their eventual purging from elite campuses contributed to the rise of defense research at second-tier schools and in private suburban research firms. At the same time, high-profile protests brought debates about the ethics of weapons work back into public view on a scale not seen since the days of the Manhattan Project, putting many scientists on the defensive and forcing public justifications of research projects and funding sources. This new discourse, often heavily critical of science because of its perceived links to weapons development and war, contributed to the general anti-science attitudes of the late 1960s and early 1970s. Combined with Richard Nixon's attacks on scientists and his dismantling of the PSAC system in the early 1970s, these campus and professional debates prompted a growing self-awareness among scientists of their own political value and skepticism about their potential abuse, facilitating a new kind of independence that would prove influential during the Star Wars debates of the 1980s.

Chapter Four: MIT and the March 4 Movement

Since its 19th century founding, the Massachusetts Institute of Technology had cultivated and maintained strong ties to the nation's industrial and military establishment.[1] It was one of the country's first engineering schools, an institute devoted explicitly to the practical fields of industrial science. ROTC was born on the MIT campus in 1911, and faculty members served on the War Industries Board and National Research Council during World War I. During the first postwar period, the school enjoyed substantial research support from key business and philanthropic entities, including the Rockefeller Foundation, GE, Du Pont, AT&T, and others, and funneled its graduates into top and mid-level executive and research positions. Though the Great Depression devastated this system of corporate-sponsored university research, the massive mobilization of World War II sent funding levels skyrocketing. The bulk of military contracts went to industry, but academia also attracted vast new investments, with MIT and Caltech leading the pack.

During the war, former MIT vice-president and Carnegie Institution head Vannevar Bush urged President Roosevelt to create the National Defense Research Committee, which soon became the influential Office of Scientific Research and Development (OSRD), which Bush chaired. From this position Bush was able to steer funds to MIT researchers, and the school, with its undergraduate population reduced by nearly two thirds due to the draft, transformed itself into

[1] For overview histories of MIT's military and industrial ties, see relevant sections of Noam Chomsky, et al., *The Cold War and the University: Toward an Intellectual History of the Postwar Years* (New York: New Press, 1998); David Kaiser, ed., *Becoming MIT: Moments of Decision* (Cambridge, MA: MIT Press, 2010); Daniel Kevles, *The Physicists: The History of a Scientific Community in Modern America* (New York: Vintage Books, 1979); Stuart Leslie, *The Cold War and American Science: The Military-Industrial-Academic Complex at MIT and Stanford* (New York: Columbia University Press, 1993); and David Noble, *America by Design: Science, Technology, and the Rise of Corporate Capitalism* (New York: Alfred A. Knopf, 1977). For a review of this literature, see D. Engerman, "Rethinking Cold War Universities: Some Recent Histories," *Journal of Cold War Studies* 5(3): 80-95.

a kind of national center for war technology, achieving extraordinary new breakthroughs in electronics and radar. In creating the institute's Radiation Lab, Bush drew on all of his academic and industrial ties. The Rad Lab employed thousands, spent more than a billion dollars, and yielded crucial radar technology that was instrumental in Allied military success. Bush recruited top outside scientist-managers to run the lab, including Lee DuBridge, the future Caltech president and science advisor to Richard Nixon, and future Nobelist I.I. Rabi. The indisciplinary nature of the Rad Lab, as well as its ties to government and industry, inspired MIT scientists and administrators to incorporate the lab more closely into the MIT campus and to create other facilities along similar models. By 1944-45, the institute's sponsored research contracts brought in over thirty-four times the total amounts of 1939-1940,[2] and MIT ranked first as the country's top "nonindustrial defense contractor," with Caltech and Harvard a distant second and third.[3]

As Stuart Leslie has shown, despite Bush's enthusiasm for the coordination of military, academic, and industrial resources, he "kept his relationships with universities and industry contractual, restricted in time and in scope... He always considered OSRD a strictly emergency operation and dismantled it promptly at the end of the war, despite considerable opposition from the Pentagon and the White House."[4] In this sense, Bush was an anomaly. His fellow military planners largely resisted demobilization, preserving contracts with academic laboratories and even encouraging new institutions, such as the Office of Naval Research (ONR), to take on the former duties of the OSRD. The Rad Lab was officially dismantled in 1945, but with support from ONR and other military funders, its resources shifted to a new entity, MIT's Research Lab

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[2] Dorothy Nelkin, *The University and Military Research: Military Research at MIT* (Ithaca: Cornell Press. 1972), 18.

[3] Leslie, 14.

[4] Leslie, 7.

of Electronics (RLE). Partly the brainchild of Norbert Weiner, the interdisciplinary RLE devoted itself, in the words of physicist Victor Weisskopf, to "almost anything related to human communication," from Weiner's invention of cybernetics to Noam Chomsky's study of linguistics.[5] But it was also through the RLE during these early postwar years that Project Lincoln and other air defense and missile guidance projects got their start, and the first small defense technology spinoff companies began to populate the areas adjoining what would become Route 128.[6]

The gentle dip in federal R&D funding after the war lasted only a few short years, until growing Cold War tensions elevated contract levels and in Leslie's estimation "made the university, for the first time, a full partner in the military-industrial complex."[7] The 1950s saw the creation of a spate of new special university labs, including MIT's Lincoln Lab, Berkeley's Livermore Lab, and Stanford's Applied Electronics Lab, all pumped full of defense money. By 1969, MIT was, in the words of *New York Times* columnist Walter Sullivan, "one of the pillars of American military research."[8] The U.S. government paid for almost 90% of MIT's on-campus research, with the Defense Department (including the three service branches and ARPA) accounting for more than a third of the total in 1967, including 40% of all engineering research.[9] By far, the largest magnets for government funding were the university's two "special labs": the Lincoln Laboratory, a pioneer in multiple weapons-related research fields located near Hanscom

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[5] Victor Weisskopf, *The Joy of Insight: Passions of a Physicist* (New York: Basic Books, 1991), 166.

[6] Leslie, 29.

[7] Leslie, 8.

[8] Walter Sullivan, "Fighting the Misuse of Knowledge," *New York Times*, 9 February 1969.

[9] "Data on Universities," Box 1, Folder 8, Records of the MIT Review Panel on Special Laboratories 1969-1971 (hereafter MIT), Massachusetts Institute of Technology.

Air Force Base in bucolic Lexington, and the Instrumentation Lab in Kendall Square, specializing in inertial guidance systems and run by Charles Stark Draper, the Montana-born MIT physicist and gyroscope pioneer.

MIT's Special Labs

Leslie locates the origins of Lincoln Lab in the U.S. military response to Soviet nuclear testing in 1949. At that time, the Air Force's Science Advisory Board created an ad hoc committee on air defense, packed with MIT representatives. Within a year, the outbreak of the Korean War pushed Air Force top brass to request that MIT expand its early air defense study efforts to include full-scale research laboratory work. Though RLE researchers Jerome Weisner and Jerrold Zacharias initially balked at the expansion of military projects, a succession of small summer studies and well-funded, carefully-designed initiatives eased MIT faculty into the new work. Project Charles, conducted in the summer of 1951, paved the way for the official creation of Lincoln Laboratory out of RLE resources.

The off-campus location facilitated its expansion, and Lincoln grew rapidly throughout the 1950s, particularly after the 1957 funding boom. Lincoln also provided a convenient new home for the talented Rad Lab veterans whose expertise lay in the fields of radar technology, communications, and data processing. New Cold War projects like the SAGE (Semi-Automatic Ground Environment) system and Whirlwind computer required just such a specialized staff. The latter led to major collaborations and spinoff projects with IBM, AT&T, and H.P. Robertson's System Development Corporation. The former provided the impetus for the creation of MITRE Corp., a portmanteau of "MIT Research," which removed responsibility for SAGE's "systems engineering" and technical support from Lincoln Lab. (At the time, MIT did not consider systems engineering a sufficiently academic line of research "for an educational institution."[10]) Lincoln continued its research on weapons systems and detection techniques, however, including work on ARPA's Vela Uniform.

By the late 1960s, 98% of the lab's funding came from the Department of Defense, through a Federally Funded Research and Development Center (FFRDC) contract. In addition to its weapons-related work, the lab also took on projects in radio physics, radar observations of planets, and tests of relativity. Though the details of many Lincoln projects were classified, the lab published general descriptions of its major research areas; according to MIT's 1969 Directory of Current Research, Lincoln projects in 1969 included seismic data analysis for Vela Uniform as well as research on masers, thermal tracking, computer memory, radio astronomy, atmospheric studies, lunar surface and planetary properties, and satellite communications.[11] The lab also ran ARPA's Project PRESS (Pacific Range Electromagnetic Signature Studies) program, part of the larger Project DEFENDER, an effort to study the physical traces left by missiles, decoys, and other ICBM components. Such research was crucial for devising mechanisms for an anti-ballistic missile system that could distinguish between incoming nuclear weapons and incoming decoy projectiles. Researchers studied the types of ionization, optical radiation, fluid flow fields, and other phenomena produced by various objects traveling through the atmosphere, in order to work out methods of reverse analysis—to see, in the words of one Lincoln associate division head, if given ionization and radiation data, for example, they could "infer the body."[12]

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[10] Everett to Pounds, 6 May 1969, Box 1, Folder 3, MIT.

[11] "Background on Research at Massachusetts Institute of Technology," 4 March 1969, Box 1, Folder 8, MIT.

[12] Herlin testimony transcript, 1 May 1969, Box 2, Folder 21, MIT.

Related work in this area included "radar systems analysis"—a study of the efficacy of radar technology in decoy identification—and general "defense system analysis and synthesis."

Perhaps most controversially, the lab's radar work also included development of techniques for detecting underground tunnels and distinguishing the movement of people and vehicles through dense foliage; newspaper reports indicated that Lincoln employees had even traveled to Vietnam to demonstrate the technology for military leaders.[13] For MIT's antiwar contingent, it was these two categories of research—ABM and Vietnam-related work—that made the lab a major target of protest.

MIT's Instrumentation Lab followed a similar trajectory of expansion. Charles Stark Draper, a young MIT physicist and engineer from Montana, had founded the lab in 1932, with a substantial Depression-era investment from Sperry Gyroscope. Draper was primarily interested in "shipboard gunsights"—optical, gyroscopically-stabilized devices to facilitate aiming a weapon from a moving ship. This early exploration into inertial frames of reference led to more complicated aiming, tracking, and guidance systems, drawing on improvements in gyroscopes, accelerometers, servomechanical feedback systems, and radar. Leslie notes that the content of Draper's teaching was almost entirely shaped by the nature of his instrumentation research, and many of his students pursued Sperry-approved dissertation topics and were rewarded with corporate positions upon graduation.[14] Among the lab's early successes were the Mark-14 gunsight used during World War II, the A-1 sight for fighter aircraft (employed in Korea against

[13] "MIT Directory of Current Research," 1969, Box 1 Folder 10, MIT; "Text for MIT's President's Report, July 1968" (Lincoln/ARPA Project PRESS pamphlet), Box 1, Folder 18, MIT; *Boston Globe,* 3 June 1969.

[14] Leslie, 81. For a deeper history of Draper's role in the development of guidance systems, see Donald Mackenzie, *Inventing Accuracy: An Historical Sociology of Nuclear Missile Guidance* (Cambridge, MA: MIT Press, 1990).

Soviet MIGs), and the "Black Warrior" automatic pilot system capable of loops and complex evasive maneuvers.[15]

World War II led to a broad expansion of laboratory research at MIT generally, but during the postwar boom, it was the Instrumentation Lab that was singled out by the Eisenhower administration for ballistic missile research, largely through the influence of key advisor and MIT president James Killian and several well-placed Instrumentation Lab alumni. Navy and Air Force contracts came rolling in throughout the 1950s; as Leslie notes, "In the post-Sputnik missile buildup (1957-63) the Air Force pumped $9 million a year into the Instrumentation Laboratory for ballistic missile guidance research and development."[16] And as had been true before the war, the curricular components of the "teaching laboratory" promoted by Draper heavily reflected the military orientation of the lab's funders. Officer-students, of which there were many, could take courses in "Weapons Systems Engineering," and Navy and Air Force administrators worked closely with MIT faculty to design optimal graduate programs for officers. Summer courses and seminars attracted researchers from military labs and computing and aerospace corporations, many of whom might end up hired as "resident engineers" at the lab. Employment statistics document the work done by engineers from AC Spark Plug, Raytheon, Bendix, Honeywell, and other companies. Draper remarked that "he kept salaries low to encourage turnover to industry." Throughout the late 1950s, Draper's push factor met corporate pull factors—the establishment of new local research facilities in towns like Wakefield, Burlington, and other suburban sites.[17] Military contracts funded both the research results which

[15] Woodbury testimony transcript, 30 April 1969, Box 2, Folder 19, MIT.

[16] Leslie, 92.

[17] Leslie, 95-98.

were funneled to industry for construction, and the researchers themselves who were funneled to industry for further employment.

By the 1960s, the Instrumentation Lab had become a world leader in inertial guidance systems, technologies enabling "extremely precise guidance of a vehicle without magnetic compass or ground control."[18] As one lab researcher would later explain, in the parlance of a professor of freshman physics: "This is what inertial navigation allows you to do. You go in a closet, you shut the door, you move it any place you want, it tells you where you are, how fast you are going, and what direction you are going."[19] Lab researchers developed guidance systems for planes, ships, submarines, space vehicles, and missiles, most notably for the Apollo program and the Thor, Polaris, and Poseidon missile systems. They developed VTOL helicopter technology, allowing vertical takeoffs, hovering, and backwards flight.

According to the lab's public literature, its guidance research in 1968 included the SABRE ballistic missile program and the controversial MIRV system intended for use with the submarine launch vehicles of Poseidon.[20] On the Poseidon project, the I-Lab held a contract with the Navy's Special Projects Office, but subcontracted out various components and electronics to Bendix-Honeywell-Nortronics, Raytheon, Hughes, and GE. The lab's ties to private industry extended beyond collaborations through subcontracting, however. I-Lab staffers had consulting arrangements with an extensive list of defense firms: Boeing, Lockheed, Convair, Northrup, Rand, Hughes, Grumman, Westinghouse, Systems Development Corp, GM, Nordtronics, Itek, Control Data, Allied Research, Honeywell, GE, Sperry Rand, Watertown Arsenal, Raytheon,

[18] "Background on Research at the Massachusetts Institute of Technology," 4 March 1969, Box 1, Folder 8, MIT.

[19] Woodbury testimony transcript, 30 April 1969, Box 2, Folder 19, MIT.

[20] For a short, useful summaries of MIRV and MIT, see Nelkin, 48-53, and for MIRV generally, see George Rathjens and George Kistiakowsky, "The Limitation of Strategic Arms," *Scientific American* 222, No. 1 (January, 1970).

Avco, Mitre, ITT, IBM, Sperry Gyroscope, Univac, and General Dynamics, to name some of the most well-known.[21]

Records from 1968 show that the Instrumentation Lab employed about 1900 people, roughly a third of them "professional engineers and scientists," a third technicians and assistants, and a third administrative and support personnel. Of the technical staff, the largest group were electrical engineers, with mechanical engineers, aerospace engineers, physicists, and mathematicians also represented. Everyone on the lab payroll—including students—was required to have security clearance. In 1967, the lab spent nearly $19 million on salaries and wages, $10.4 million on overhead, $11 million on materials and services, and $12.9 million on subcontracts.[22] More than half the budget was supplied by the Department of Defense.[23] As with Lincoln Lab, decisions on the acceptance of new contracts were made by the laboratory directors in consultation with Jack Ruina, the former ARPA head currently serving as MIT's vice president for Special Laboratories.[24]

"The Federal Presence at MIT"

Given this history, MIT's ties to government and industry had been debated many times before the March 4th strike in 1969. In 1945, MIT president Karl Compton recalled that since the first moments of crisis during World War II, MIT had vowed "*never* to let the self-interest of the institution prevail over the interests of the nation." The wartime effort had demonstrated that the

[21] "Poseidon Program Organization" chart collection, April 1969, Box 1, Folder 12, MIT.

[22] "Instrumentation Laboratory" booklet, April 1968, Box 1, Folder 13, MIT. Nelkin estimates total lab employment at 2248 in 1969.

[23] John Walsh, "MIT: Panel on Special Labs Asks More Nondefense Research," *Science* , Vol. 164, No. 3885 (13 June 1969), 1264.

[24] Transcript, 29 April 1969, Box 2, Folder 18, MIT.

country's "educational institutions rank with our manufacturing industry and transportation system as the principal supporting lines of military power in time of war…," a sentiment that would be reiterated by an MIT lawyer in 1966, who described the school as "a scientific arsenal of democracy."[25] Compton's views were similar to many of the Manhattan Project scientists; he saw no differences between institutional and individual obligations in times of war. Universities, like the citizenry, must serve their nation, but should take strict care they did not profit financially from their efforts. In MIT's case, "the prestige gained from our war research" should be reward enough.[26]

MIT administrators trod cautiously in the postwar period, dismantling the Rad Lab but continuing to host other defense-sponsored research. Leslie notes that in the 1950s, Louis Smullin, head of the RLE's microwave tube lab, proposed that MIT pursue civilian goals with the same laboratory zeal that characterized the creation of Lincoln Lab, but his vision "received no support whatsoever."[27] Many other prominent MIT faculty members—including Victor Weisskopf, Philip Morison, Martin Deutch, and Bernard Feld—held leadership positions in the Federation of American Scientists, which throughout the 1950s and 1960s issued warnings and public statements on the dangers of university militarization. By the late 1960s, FAS's position was clear:

> Except in times of national emergency, the university should not be a part of the military establishment and should not directly or indirectly take part in military operation or participation in the collection of military intelligence. The university should not enter into any contract supporting research the specific purpose of which is the development of weapons or devices designed to destroy human life or to incapacitate human beings, nor should it provide administrative services for

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[25] Quoted in Nelkin, 20.

[26] Karl Compton, "Annual Report of the President: The War Record of the Institute," 1945, Box 2, Folder 3, MIT.

[27] Leslie, 43.

> government weapons laboratories…. Not only does secrecy in research run counter to the values and basic function of a university, but so does secrecy or misrepresentation in the support of research even if the research itself be nonsecret.[28]

"Times of national emergency" was a phrase open to interpretation, however. In 1967, then-provost Jerome Wiesner weighed in on "The Federal Presence at MIT" in the pages of *Technology Review*. Although he acknowledged that MIT's general policy was "to strive always for the maximum of free, open, uninhibited discussion—in other words, for unclassified research," he agreed with Compton that moments of crisis—such as World War II, the war in Korea, or key periods of the Cold War— justified drawing on MIT's unique technological expertise in the service of the national interest. Moreover, MIT's avoidance of key areas of classified research could mean falling behind other elite institutions. Wiesner summed up bluntly: "Either we have enclaves of secret research on or near to the campus; or we bypass interesting and vital areas of modern technology." But Wiesner didn't confine his analysis just to MIT. He noted that government funding for academic research had become more restricted and more spread out in recent years, so that more schools received smaller amounts of money. The long-term effect, in his view, would be the disappearance of the kind of "big centers" that had proved so innovative and productive in the past, and the emergence of widespread, homogenized, mediocre research.[29]

But Wiesner's invocation of MIT's cutting-edge reputation could not mask the significant unpopularity of certain areas of research in the "big centers," especially ABM. By the early 1970s, even former MIT president James Killian acknowledged that the positive attitudes of the

[28] Quoted in "The Physicist with a Cause—An Interview with Jay Orear," *Physics Today* May 1968, 85-91, Box 4, Folder 7; Papers of Brian Schwartz, 1966-1977 (hereafter BSP), Niels Bohr Library, American Institute of Physics, College Park, MD.

[29] Jerome Wiesner, clipping: "The Federal Presence at MIT," *Technology Review,* April 1967, in Box 1, Folder 25, MIT.

Sputnik boom years—the era of SAGE and Whirlwind and Project Charles—had soured; the days when enthusiasm for air defense research had sometimes even exceeded military support for research programs were long gone. Killian nevertheless defended the heavy levels of defense funding at MIT, convinced that the university was still able to "preserve the policies and academic environment" of the institute. MIT was no Pentagon lapdog, he observed, joking that "Somebody once in Washington made the comment that you didn't enter into a contract with MIT, you negotiated a treaty."[30] In the late 1960s, however, all university contracts with the Pentagon were suspect, no matter how well-negotiated. The young MIT SDS chapter put it succinctly in their 1967 pamphlet "MIT and the Warfare State," an early articulation of the kinds of criticisms soon to become prevalent on campus:

> MIT does not directly service the war in Vietnam. Nevertheless, MIT Personnel advise and co-operate with the government, do studies on Viet Cong Defectors and Prisoners, and, most important, accept grants from the Defense Department to produce the technology capable of waging a war like the one in Vietnam, 10,000 miles from American shores. MIT must bear the responsibility for its action. The man who designs the gun is just as guilty as the man who pulls the trigger, for without him, there would be no gun. … Scientists are not instruments of society, they are MEN with conscience. Technology is not neutral, it is directed.[31]

Activism at MIT

Although MIT through much of the 1960s was hardly a hotbed of student unrest, both student and faculty activism nevertheless existed, though perhaps more muted than at other, more notoriously volatile campuses.[32] Undergraduate activist groups included a small chapter of

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[30] Transcript, 15 May 1969, Box 3, Folder 5, MIT.

[31] SDS, "MIT and the Warfare State," 1967, Box 1, Folder 21, MIT.

[32] See John Walsh, "MIT: Panel on Special Labs Asks More Nondefense Research," *Science* , Vol. 164, No. 3885 (13 June 1969), 1264-1265. Walsh observes that "there is little sign of the radicalization of the undergraduates at MIT" and "By tradition and still dominantly in atmosphere, MIT is an engineering school, and engineering students have been a conspicuously inert group in most universities during the current upheavals." He credits much of the activism on campus to graduate students, liberal faculty members, and outside groups.

the Society for Social Responsibility in Science, the Rational Approach to Disarmament and Peace (RADP), and Students for a Democratic Society (SDS). By the end of the decade, however, SDS, Resistance, Sanctuary, and other antiwar groups had attracted much larger followings: in 1967, hundreds of MIT students protested against the campus recruitment efforts of Dow Chemical, and over a thousand attended a lunchtime panel discussion among prowar and antiwar professors, a medical expert on the effects of napalm exposure, and a Dow representative. In the fall of 1968, hundreds of students occupied a portion of the student center in order to provide sanctuary to an AWOL soldier, and a student poll from that year showed majority support for Eugene McCarthy's antiwar presidential bid. Faculty members formed chapters of Scientists and Engineers for Johnson-Humphrey in 1964 and Scientists and Engineers for McCarthy in 1968, and participated in the Boston Area Faculty Group on Public Issues and Group Delta, a short-lived coalition of antiwar faculty members.[33]

Murray Eden, an electrical engineering professor, described the March 4 movement as originating from a dinnertime discussion among physics graduate students on the possible impact of an antiwar science research strike. The students—Joel Feigenbaum, Alan Chodos (at MIT on a postdoctoral fellowship), and Ira Rubenzahl—quickly began reaching out to students and faculty in their own and other departments, eventually recruiting Bernard Feld, Salvador Luria, Bruni Rossi, Victor Weisskopf, and Noam Chomsky, among others, to join their cause, as well as Hans Bethe and George Kistiakowsky from outside the MIT community. Jonathan Kabat, a biology graduate student, signed on as another enthusiastic organizer. The result was a draft statement endorsed by prestigious faculty members on the 'misuse of science' and the creation of a new

[33] Murray Eden, "Historical Introduction," in Jonathan Allen, ed., *March 4: Scientists, Students, and Society* Cambridge (MIT Press, 1970).

group, the Science Action Coordinating Committee (SACC). A one-day research strike was planned for March 4, 1969.

Initially, students comprised the major organizing force, drawing on faculty support in order, in Eden's words, to "provide prestige, enhance publicity, and help to persuade eminent men of science and politics to participate." Many soon became frustrated by what they perceived as the "well-intentioned but timid liberals" of the MIT faculty.[34] Tensions came to a head over the text of an open letter written to Lee DuBridge, Nixon's science advisor and former Caltech president, which many faculty members found alienating, and students and professors argued fiercely over appropriate responses to an early *Boston Globe* article on strike planning. The conflict caused an organizational split: activist students remained within SACC, while faculty members, led by physicist Herman Feshbach, formed a new group, the Union of Concerned Scientists (UCS). Meanwhile, the MIT administration itself obviated the divisive characterization of the planned action as a "strike" by agreeing to cancel classes and devote the day to open discussions of the applications of scientific research to military causes.[35]

As scheduling efforts progressed, the planned day of events stretched into additional evening and weekend sessions. SACC and UCS reached out to other schools, labs, and cities, coordinating simultaneous strikes throughout much of the Ivy League and at major state research universities. Organizers recruited supporters at conferences and annual meetings, and the efforts

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[34] Murray Eden, "Historical Introduction," in Jonathan Allen, ed., *March 4: Scientists, Students, and Society* Cambridge (MIT Press, 1970), xv.

[35] Ibid.; *Boston Globe,* 21 January 1969. The letter to DuBridge complained about the militarization of science, charging that funding emphases on military applications had led to the creation of "unnecessary" and dangerous ABM and CBW technology. Despite the SACC-UCS split, the original DuBridge letter was signed by numerous members of MIT's faculty, including Feshbach, Luria, Jerome Lettvin, and Noam Chomsky. Of all the MIT departments represented, Physics provided the largest number of signers, followed by Biology and Mathematics. Also see *New York Times,* 21 January 1969 and *Boston Globe,* 24 January 1969.

yielded encouraging editorials in the *New York Times* and *Chemical and Engineering News.*[36] Bell Labs agreed to send "a busload of participants."[37] Both SACC and UCS also circulated petitions and issued multiple public statements, with demands both specific and sweeping. SACC proposed a series of MIT policy reforms, including an end to military-related co-op jobs, the elimination of Defense Department research funding, the eviction of ROTC, and the conversion of the university's special labs to non-military projects.[38] But the group also issued a more general call for *involvement*: an end to scientists' invocation of political neutrality. "When a scientist—or any other citizen—claims political detachment and non-involvement," they wrote, "his decision perpetuates militarism and waste, for that is the thrust of America's status quo." The ethical problem was not a matter of individual morality or personal culpability for the misuse of science, but the *institutional framework* in which scientists operated. SACC explained:

> Science is misused by men, but men who function within an institutional framework. … The pressures upon the men who make policy flow from deep within the structure of this society. The limits on their actions are not solely the boundaries of pure reason. The men who rule have been screened again and again, at each step of advancement, so that those who agree with the status quo are chosen, while those who might rebel are weeded out. Those who are chosen find that their way of life, their livelihood, their very lives come to depend on the maintenance of the established order. … Set against the forces of self interest, acculturation, pressure from peers and threats from higher up, we predict that reasonable arguments will not alone suffice. The question is one of power as well as reason.[39]

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[36] Murray Eden, "Historical Introduction," in Jonathan Allen, ed., *March 4: Scientists, Students, and Society* Cambridge (MIT Press, 1970), xxi; clipping, *New York Times*, in Box 4, Folder 8, BSP.

[37] Clipping, *Science News*, Vol. 95 (22 February 1969), 185, in Box 4, Folder 8, BSP.

[38] Clipping, *Catalyst*, March 1969, in Box 4, Folder 8, BSP.

[39] SACC flyer, undated, Box 4, Folder 8, BSP.

UCS reiterated this analysis in their founding faculty statement, expressing deep disillusionment with government policies and hope that a silent but "concerned majority" of scientists could rise up:

> Through its actions in Vietnam our government has shaken our confidence in its ability to make wise and humane decisions. … The response of the scientific community…has been hopelessly fragmented. There is a small group that helps to conceive these policies, and a handful of eminent men who have tried but largely failed to stem the tide from within the government. The concerned majority has been on the sidelines and ineffective. We feel that it is no longer possible to remain uninvolved.[40]

The UCS's "concerned majority," however, did not materialize everywhere. Though by March 1969 the group included roughly a hundred members from MIT (a third of total membership), many Boston area academic scientists opposed or ignored UCS outreach efforts. Counter-petitions circulated in MIT's Engineering and Political Science departments. *Electronic News* reported that "There is little sign that the activist efforts of the physicists are extending into engineering or the electronics industry" and quoted Lee Dubridge's dismissal of the protesting scientists as "extremist elements," a phrase picked up in a negative editorial in *Industrial Research.* At the Argonne National Lab in Illinois, researchers formed a "Federation of Responsible Scientists" and announced a 16-hour "work-in" to coincide with the MIT strike.[41]

The reaction to the strike reflected this division of viewpoints. The *New York Times* reported that the "1200-seat Kresge Auditorium" was "often filled" by a "shifting audience of professors and students." Noam Chomsky, Howard Zinn, Victor Weisskopf and others delivered speeches to packed houses. But as the *Tech* reported, "business was about normal in most

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[40] Union of Concerned Scientists, "Faculty Statement," in Jonathan Allen, ed., *March 4: Scientists, Students, and Society* (Cambridge: MIT Press, 1970), xxii-xxiii.

[41] Clippings, *Electronic News,* 17 February 1969 and 24 February 1969, Box 4, Folder 7, BSP; clipping, *Industrial Research,* March 1969, Box 4, Folder 8, BSP.

laboratories": though some researchers took breaks to attend talks or other meetings, no major work stoppage occurred. Around the country, about 50,000 students at over fifty universities expressed their discontent with the militarization of science: the University of Pennsylvania cancelled classes, and at Columbia, several hundred students and faculty—many referring to themselves as "Sputnik children" funneled into defense science careers—participated in a research strike. At Rockefeller Center in New York, thirty members of the Computer People for Peace distributed literature and voted not to "program death." And in the U.S. Congress, Senator George McGovern symbolically introduced a bill proposing a national commission to oversee the country's 'conversion to a peacetime economy.'[42]

Back at the MIT campus, highlights of the many planned activities and speeches included historical reflections from Howard Zinn ("War is interdisciplinary"), assessments of the meaning of academic freedom and participatory democracy during wartime, a warning from Thomas Schelling on the dangers or moral crusades, speeches on protesting the draft, a debate about arms control and disarmament featuring Hans Bethe and Gar Alperovitz, and public statements from the organizers, SACC and UCS. The rhetoric of the day emphasized three broad themes. First, both students and faculty members meditated on the peculiar responsibilities of scientists, but differed as to whether it was the individual actions of scientists or the aggregate policies of institutions that required special ethical oversight. In a similar vein, many speakers emphasized the *agency* of members of the MIT community, as graduate student Jonathan Kabat did when he reminded his audience that "You're MIT. You people are MIT. If all of the people left those buildings, the buildings would have very little character. In a larger sense, you are the country."

[42] Clippings, *New York Times* 5 March 1969, *The Tech* 7 March 1969, *Science News* 95 (15 March 1969), 257, Box 4, Folder 8, BSP.

[43] Kabat warned about the dangerous value systems promoted by various systems and institutions, but the MIT "system" was still composed of individuals, capable of collective action. These collective actions might take many forms, as elucidated by the third group of speakers, who focused on the intricacies of policy reform at MIT, offering suggestions for contracting alternatives, research standards, and laboratory regulations. From the abstract and philosophical to the concrete and economic, the MIT community was debating the ethics of defense research on campus.

Two key panels, on intellectuals' obligations and the reconversion of weapons labs, deserve closer analysis. The first, straightforwardly titled "The Responsibility of Intellectuals," featured short statements by SACC student organizer Joel Feigenbaum, linguistics professor Noam Chomsky, Physics Department Chair Victor Weisskopf, and UCLA chemist, RAND physicist, and military advisor William G. McMillan. The four men espoused very different ethical outlooks. Feigenbaum, the youngest, offered a clear articulation of SDS and SACC analyses. He opened with an impassioned condemnation of the failure of intellectuals to empathize fully with the plight of impoverished inner-city residents and victims of U.S. military actions. MIT intellectuals hid behind claims of impartiality, declaring themselves "apolitical" or "values-free," he complained, all the while building MIRV systems and publishing defenses of U.S. foreign policy in political science journals. Perhaps referring to the opening lines of the Port Huron statement, he warned that given this hypocrisy, his generation "cannot live comfortably in a place such as MIT."

Weisskopf, representing the older Manhattan Project generation, offered a general reflection on the worth of scientific research: despite dangerous applications in the near-term, in

[43] Jonathan Kabat, "Proposals for Further Action," in Allen, 125.

the long term, "it is good to know more."[44] Nevertheless, he deemed scientists to bear special obligations due to their knowledge of potential risks. Scientists should not work on weapons projects *solely* out of curiosity and interest, he explained, and they ought to prevent the misuse of their research through active political engagement, either within the system, through government and military advising roles, or without, through conducting independent studies, organizing protests, or educating the public. Trying to play the role of peacemaker, Weisskopf warned that though both insider and outsider scientists would likely distrust each other, both groups were working toward the same goal: the responsible application of technology to world problems.

Overshadowing both Feigenbaum and Weisskopf, however, were the presentations by Chomsky and McMillan. For Chomsky, "The Responsibility of Intellectuals" was a popular topic. A revised version of a talk he had given at Harvard on the subject had been published in *The New York Review of Books* in 1967 and reprinted elsewhere, including in his influential collection *American Power and the New Mandarins.* Chomsky had noted that intellectuals were "in a position to expose the lies of governments, to analyze actions according to their causes and motives and often hidden intentions." Because of Western intellectuals' privileged position and access to information and training, he explained, their responsibilities ran deeper than those of other people.[45]

But Chomsky argued that rather than using their advantages to analyze causes and consequences, most American intellectuals assumed that government motives were pure, and that their own contributions therefore ought to consist of technical critiques. The development of this attitude went hand-in-hand with the professionalization of the social and behavioral sciences,

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[44] Weisskopf, "Intellectuals in Government," in Allen, 29.

[45] Noam Chomsky, *American Power and the New Mandarins* (New York: Pantheon Books, 1969), 324.

and their "desperate attempt… to imitate the surface features of sciences that really have significant intellectual content." Citing Herman Kahn as an example, Chomsky observed that much social science could not actually be tested empirically, but that the trappings of empirical analysis allowed social scientists to promote themselves as rational, trained practitioners with "sophisticated" analyses, who could then dismiss the moral arguments of non-social scientists—arguments that lacked the prized technical veneer—as "emotional" and "irrational."[46] The result was a class of entrenched intellectual experts whose own professional success created a vested interested in the maintenance of the status quo.

Chomsky's criticisms of intellectuals targeted mostly social and behavioral scientists, for whom "the cult of the expert is both self-serving… and fraudulent." His analysis of the obligations and legitimacy of natural scientists, prior to March 4, had been less explicit. Now, however, he tackled the issue head on. He opened with the general observation that "The path of least resistance is always to accept the distribution of power as it exists," then launched into a damning critique of the war in Vietnam. Just as he had earlier described the reasons why few social scientists challenged deeper problems of morality and justification, he now sketched out the ways in which government and military engineering projects created a kind of "'socialism' for the rich and powerful and for segments of the technical intelligentsia." As with social science, engineering experts who weighed in with technical evaluations were considered "responsible," while those who criticized deeper motivations were dismissed by policymakers. As an example, Chomsky cited debates over the ABM. The technical arguments over whether it could work were red herrings, he explained, since the program would be dangerous whether it functioned effectively or not. But too many contracts, and too many millions of dollars, were at

[46] Ibid, 339.

stake. Despite these ominous tendencies, Chomsky reiterated his belief in the potential of human action. He told the student audience: "A good deal depends on our conscious choices. The scientists who are called upon to construct the ABM need not do so… They can organize and encourage others to join them in this refusal. They can also help to create the mass politics that provides the only real hope for restraining and ultimately dispelling the nightmare that they are now helping to create." The universities, in turn, ought to halt their acceptance of defense contracts and their complicity in the "war machine."[47]

McMillan, fresh from his two-year stint in Vietnam as science advisor to Gen. Westmoreland, perhaps embodied the type of 'captured intellectual' targeted by Chomsky, and he offered a spirited defense of his background and political positions. He opened his lecture, "The Scientist in Military Affairs," by presenting his qualifications as "a relatively independent member of the 'Establishment'" and listing his various governmental and military advisory roles, as well as his academic training. He urged students: "while you are busy subtracting out my bias, I hope you will remember to add in my exposure and experience."

McMillan took issue with several key aspects of the March 4th protest. First, he pinpointed SACC's complaints of "the present overemphasis on military technology" as a false target. Where, in the actual realm of MIT research, did this overemphasis exist? Compared to the skewed military research agenda of World War II, MIT in the Vietnam era hardly emphasized military work, let alone overemphasized it. McMillan characterized the general contributions of the academic community as "either nonexistent or actually negative." He regretted that while "there are literally hundreds of smaller developments that collectively could contribute enormously to shortening that unhappy conflict [in Vietnam] and reducing casualties," academic

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[47] Noam Chomsky, "Responsibility," in Allen, 8-14.

researchers remained largely uninterested. After detailing an extensive list of projects he wished universities would take on, including mine detection, intelligence collection, and improved communications techniques, he observed:

> In the face of such clear opportunities to turn technology toward constructive applications in the Vietnam conflict, one can only ask, *where are the Bushes, the Conants, the Ureys, the Fermis, the Oppenheimers, the DuBridges of this generation*? Where today is any organized effort on the part of the academic community to contribute? The answer is, there is no such organization. The university has been satisfied to sit back, accept the lopsided view of Vietnam presented by the news media, and on that unscientific basis decide to have nothing to do with Vietnam.[48]

McMillan also defended university research on weapons systems, including the ABM, on the grounds that the aggressive stances of Soviet and Chinese leaders mandated U.S. defensive research. He admonished the students in Manichean terms: “Let’s not kid ourselves: the sheep of the West and the communist leopard are arraigned on opposite sides of the arena—and the leopard is not about to change its spots.”[49] On the topic of chemical weapons, McMillan emphasized the nonlethality of the types of teargas used in Vietnam, which he also lauded as a means “to lessen battle casualties to both sides.” He did not address broader definitions of chemical warfare that might include napalm or defoliant use.

At the heart of McMillan’s presentation, though, was a fervent defense of the good motives and responsible efforts of the nation’s expert military scientists and planners. He chastised the MIT protesters for what he considered facile and ill-informed criticisms: “It is altogether too easy and too tempting to believe that the decision makers are stupid,” he explained. “They are not. Whether military or civilian, they are among the most capable, thoughtful, dedicated, and responsible people we have. Moreover, by contrast with most lay

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[48] W.G. McMillan, “The Scientist in Military Affairs,” in Allen, 17-18, emphasis added.

[49] McMillan in Allen, 19.

intellectuals, they are exceptionally well-informed."[50] The "lay intellectuals," in McMillan's view, refused to trust the expertise of the decision-makers but failed to conduct sufficient research themselves—to acquire their own expertise. In a final damning analysis, he upheld—and defended—Chomsky's description of the implacability of the status quo, assuring his audience that "*things are the way they are because it is exceedingly difficult for them to be otherwise.*"[51]

Beyond this defense of military experts, McMillan bypassed the question of "whether we should or should not be in Vietnam" as "not fruitful," identifying the main political problem facing American planners as ways to honor the "strong national commitments to the South Vietnamese people" and, more generally, "fulfilling our commitments with honor." Concern about national honor and a strong conviction that U.S. involvement reflected the popular will in South Vietnam were both largely absent among the critical groups organizing the March 4th events. And yet McMillan nevertheless pushed his audience to consider the logical consequences of their criticisms: were the pacific analyses of academia "tantamount to renouncing the capability to defend our country and its institutions"? Were *any* kinds of weapons-related research acceptable? For many of the activists, the answer was no.

The second key panel concerned the topic of "reconversion," the institutional effort to shift funding sources and research emphases away from military projects and towards more socially useful areas. Panelists included Ronald F. Probstein, a key researcher at MIT's Fluid Mechanics Lab; industrial consultant Mario C. Grignetti; David S. Dayton of Technical

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[50] *Ibid*, 20.

[51] *Ibid,* 24.

Development corporation; and engineering professor and Los Alamos consultant Leonard W. Gruenberg.

Both Grignetti and Dayton urged MIT scientists to volunteer to promote reconversion. Grignetti called for "a flood of unsolicited proposals" addressing urgent civilian problems. Dayton, like many others during the course of the day, emphasized scientists' "special responsibility," which he saw as rooted in the scientific quest for truth and unique ability to predict future applications and risks of new technologies. He observed that in the corporate world, many engineers were appropriately concerned with 'the application of their own resources'—their time, methodology, research areas—but not concerned enough with "the application of other peoples' resources," i.e., larger systemic trends and practices. It was easy for engineers to become financially tied to military projects, which often held genuine intellectual appeal beyond patriotism, rooted in exciting new areas of research and the inspiring nature of "problems…that haven't been solved." He nevertheless implored his audience, "Don't sink into the system. Don't sink in. But don't drop out either. … If you can't join a big company, form one. … The creative process is here, in universities like this. It's in your hands." [52]

Gruenberg discussed the topic of reconversion in the context of the previous remarks by Weisskopf and Chomsky. He echoed Weisskopf's endorsement of working 'within the system,' noting that a lab employee worried about losing his job would be unlikely to urge reform, but "If he can be educated, and if the administrators can be educated, into recognizing that by reorientation of their research they'll be in a less vulnerable position, I think that it's possible…that very great changes can be made with Defense-Department-supported research itself." Gruenberg was skeptical of the effectiveness of outsiders organizing mass actions against

[52] D.S. Dayton, "Problems and Possibilities in Reconversion," in Allen, 42-48.

defense work, since they possessed little leverage. From inside the system, however, even small but well-organized efforts might have far-reaching effects. Gruenberg's speech was thus a gentle call to action among weapons scientists themselves.[53]

Offering the most specific and detailed blueprint for how reconversion could work, R.F. Probstein discussed his experiences at the engineering department's Fluid Mechanics Laboratory. In 1966, the lab had held $300,000 in defense-sponsored research contracts, all focusing on research relevant to "missile re-entry and space exploration." Though this research was entirely unclassified, the lab's faculty and graduate staff, during months of soul-searching meetings, worried that the emphasis on military applications and money constituted an "imbalance." To compensate, they brainstormed how they could "reconvert" the lab's work to incorporate more socially palatable projects. Fluid mechanics specialists turned to new topics in water and air pollution, desalinization, and biomedical applications. By 1969, the lab's outside research contracts had doubled in size, but drew only 35% of funds from defense sources. Instead, money flowed in from the National Institute of Health, the Public Health Service, and Edison Electric. Researchers worked on problems related to the particulate dispersal from smoke stacks, smog from internal combustion engines, the spread of bladder infections, and ice crystallization as a means of desalinization. Probstein observed that although these topics might seem "vastly different… from the types of problems encountered in nuclear explosions or missile re-entry," in fact "They all involve fluid mechanical and chemical kinetic concepts, so that the real efforts were in reconverting our own thinking from one area of research to another…" Professors at MIT offered students rigorous studies in basic science "that you can apply in an infinite number of directions," but, he noted candidly, "once we professors get on one track, it is

[53] L.W. Gruenberg, "Reconversion Within Government Laboratories," in Allen, 49-50.

rare that we ourselves are willing to carry out what we teach."[54] Probstein acknowledged that many of the lab's grant proposals and applications for funds had been rejected, but he nevertheless pushed MIT to take a leadership position in defining and pursuing new areas of scientific research unconnected with defense objectives.

Reactions to the reconversion panel ranged from enthusiastic to skeptical. During the closing discussion session, older faculty and audience members eagerly expressed interest in pursuing concrete political steps toward reconversion. Physicist Bruno Rossi was particularly interested in the feasibility of creating national information clearing houses; another participant acknowledged that "older people with gray hair like myself get into a rut and we have to be shaken out of it after a while." But other commenters doubted the radical potential of trying to reform funding and research systems from within. As one questioner put it, "*You* built the atomic bomb, the H-bomb, the missile systems, and so on. How do you expect that, if you stay in a system, you're going to be able to do any different in the future than what they tell you?" To this, Gruenberg answered defensively that "to stand on the outside and act morally superior and moralistic is also not of use…just standing on the corner and rabble-rousing is just as useless as going ahead and working on missiles." But the fears of what Ralph Nader called 'captivity' ran too deep for Gruenstein's glib dismissals. One commenter, noting the "elitism, the arrogance, the presence of intellectual detachment that my colleagues, perhaps myself, sometimes pretend to," questioned "the inference that one can subscribe to the system and do his job well enough to maintain it and yet not be influenced by the system that surrounds him every day." Concerns

[54] R.F. Probstein, "Reconversion and Academic Research," in Allen, 34.

about "the system"—and the limits of individual agency within it—permeated every question and comment posed during the discussion.[55]

In the months following the strike, SACC continued to exert pressure on both university researchers and the administration. They demanded an end to the Instrumentation Lab's work on the Poseidon program and Army helicopter research.[56] They targeted researchers working on J.C.R. Licklider's Project CAM, a program to use the computer time-sharing system at MIT, known as Project MAC, to develop a "behavior science date management system" linked to resources at SRI, RAND, and ARPA. SACC members feared it would be used to enable studies of peasant groups and student radicals so as to be "more effective in suppressing popular movements around the world." The group drafted memos to MAC workers, citing a popular Tom Lehrer song of the period:

> Memo to: the members of Project MAC
> From: a friend at SACC
> Hi. While visiting your nice labs over in Tech Square the other day, I was disturbed to learn how little most of you know about the politics of your project. Who's funding, and why? How could the military use this stuff, and do you approve? Do you *really* think the Defense Department is the disinterested, benevolent patron of 'pure research'? Without moralizing, I just want to say I think it's your duty to study these questions and discuss them with your bosses. **Many people who worked on the atom bomb had no idea what they were contributing to**. We feel your efforts may be contributing to the manipulations of millions of unsuspecting people. Do you care? Or would you say, in the immortal words of Werner Von Braun, 'that's not my department'!"[57]

[55] "Discussion," in Allen, 51-56.

[56] *Thursday* no. 2 (24 April 1969), Box 4, Folder 8, BSP.

[57] SACC pamphlet, undated, Box 5, Folder 2, BSP.

In May, the university hosted "Agenda Days"—two days of cancelled classes, replaced by discussion groups and panel sessions addressing topics such as Pentagon research, Project CAM, the arms race, Poseidon and other weapons systems, the morality of defense research, MIT's place in the military-industrial complex, and insiders' views of Lincoln Lab. Participating speakers included Henry Kendall, J.C.R. Licklider, George Rathjens, and others, and portions were broadcast on Boston public television.

More importantly, in late April, the university administration agreed to reevaluate the activities and funding of MIT's special laboratories. Headed by William Pounds, dean of the Sloan School of Management, a new panel was formed: the MIT Review Panel on Special Laboratories. The initial roster of members included professors of engineering, group leaders from the special labs, an undergraduate business student, graduate students from engineering and biology, physics professor Victor Weisskopf, former MIT president Julius Stratton, Yale historian Elting Morison, and Frank Press, professor of earth and planetary sciences and future science advisor to Jimmy Carter. Within a few weeks, two additional staffers from the special labs joined, as did Noam Chomsky, though his participation required weekly flights from England, where he was lecturing at Oxford.[58] While the panel met throughout that spring, MIT imposed a temporary ban on classified research.

The Pounds Panel

Throughout May and June, 1969, the committee met in both private and public sessions, mulling over the ethical issues at stake. They heard testimony from researchers, administrators, politicians, and activists, and convened to discuss among themselves the most appropriate

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[58] Nelkin, 67.

recommendations to make. The committee's ethical debates and their evaluation of lab practices offer a snapshot into attitudes about science and morality at arguably the nation's premier technical research university.

In its early private sessions, committee members discussed the broader contours and history of university research. Why was it necessary for university labs to conduct military research? Peter Gray, of the MIT Alumni Advisory Council, observed to his fellow panelists that government research had traditionally taken place in three kinds of sites: government labs, university labs, and industrial labs. Recently, in-house Defense Department labs, such as those run by the Army and the Navy, had increasingly subcontracted out work to industry. Even MITRE over the previous decade had siphoned off much of its experimental work to focus on problems of management. In this analysis, university labs comprised the last, best haven for truly inventive, creative, independent people to work. Moreover, the intellectual atmosphere and prestige of university affiliation would draw top researchers. As Chomsky put it, "whenever there is an opportunity, financial and otherwise, the genius will go to the universities."[59] Nevertheless, Chomsky and other panelists objected to MIT's ties to the special labs on moral as well as logistical grounds—Chomsky worried about their size and the risk of 'the tail wagging the dog.' Lincoln Lab's budget was twice that of MIT, for example. [60]

The committee members also used their early meetings to articulate, with startling candor, their own "personal philosophies" on topics ranging from the proper role of student participation and campus democracy to the appropriateness of defense research at MIT. Marvin Sirbu, a graduate student in the electrical engineering department, opened the discussion by

[59] Transcript and minutes, 27 April 1969, Box 2, Folder 16, MIT.

[60] Ibid.

pondering the inherent difficulty of trying to identify universal contracting principles in a changing moral environment, noting that the original moral atmosphere of 1949, when the Lincoln Lab had been established, had changed dramatically by 1969, as had the lab's original function. He was unsure how to address this problem—he wondered, tentatively, if writing a university constitution that allowed for student representation in setting policy might be effective.

Jonathan Kabat, an outspoken biology graduate student and SACC member, concurred with many of Sirbu's points, but, surprisingly, downplayed the importance of participatory democracy, noting that SACC was not really "clamoring for student power" or institute-wide referenda. He went on to explain, in a lengthy speech to the committee, that:

> …one of the reasons we are not is because I for one do not feel that the state of society as it exists today and the microcosmic state of peoples' awareness of society and the issues that have to be dealt with inside of MIT is such that there is at this particular time enough competence on the part of the majority of the people, either technical competence in understanding the issues or intellectual competence in terms of putting these issues in any sort of higher perspective, and what the essential job both inside and outside the universities is going to have to be is a reconsideration of a problem that goes a lot deeper than the process by which we make decisions, but in fact deals with our very lives and the values that underlie our lives and the day to day workings and the axioms and assumptions that we live by.[61]

Kabat argued that MIT students and staffers were too entrenched in their environments to be capable of proposing viable alternatives. To do that would entail realizing and explaining the huge failures of American capitalism, the concentration of power in a tiny fraction of the country' population, the justification for people at Lincoln Lab losing their jobs, and other unpalatable truths. Unlike Sirbu, who had volunteered to support democratically-enacted policies with which he disagreed personally, Kabat fervently advocated the responsibility of individual

[61] Transcript, 29 April 1969, Box 2, Folder 18, MIT.

scientists to follow their own consciences, unwieldy as this might be, in terms of policy. What if the MIT student body voted to build a gas chamber, for example? Kabat's views epitomized the New Left struggle to reconcile the ethical obligations of both individuals and institutions. Individual acts of resistance would be necessary to counteract dangerous institution-wide policies, even as Kabat's ultimate goal was a remaking of society such that its institutions promoted more benign ends.

Like the graduate student Sirbu, most panel members expressed moral unease but uncertainty as to the proper policy course. Victor Weisskopf accepted defense research, so long as attention was focused not just on the "gadgets" themselves, but "the deeper use and consequences of use of these gadgets." Eugene Skolnikoff of the political science department echoed Sirbu's concern, worrying about "committing to making major decisions in changing the institution" in the context of "changing morality." But he wasn't sure how these concerns would translate into policy for the special labs. MIT Dean of Humanities and Social Science Robert Bishop found his Quaker beliefs hard to reconcile with his pride at the labs' accomplishments; Peter Gray agreed that weapons work was immoral, but doubted reconversion would be feasible; Sloan School undergraduate George Katsiaficas supported MIT's war-related research, but also endorsed a review panel to ensure the school did not become a "yes man" to the government; David Hoag of the Instrumentation Lab's Apollo Group agreed with many of Kabat's concerns, but also suggested that nuclear weapons might be like taking out the garbage—a dirty job that had to be done; Victor Weisskopf worried that talented MIT students who wanted to participate in large, cutting edge projects had no choice but to head to the special labs; chemical engineering professor Edwin Gilliland condoned defense research in general, but with certain controls; and graduate student Jerry Lerman lamented that MIT had no coherent moral policy, and the committee was only addressing the symptoms of living in a war-obsessed society, not the cause.

Of the representatives from the labs themselves, computer specialist and Lincoln assistant director Gerald Dinneen wholeheartedly supported the strong ties among MIT, industry, and the Defense Department, noting proudly that he was a self-aware researcher who considered his work with the lab important and beneficial to the country. Irwin Lebow, a group leader at the Lincoln Lab, marveled at the pluralism of the committee: "each one of us in this room, while we share the good wishes and the good intentions and the intelligence and the fair-mindedness to approach a problem, each one of us approaches it from our own point of view, and we can't help that, it is our background and built into us." He defended the work of his lab as both intellectually stimulating and worthwhile, and rejected arguments that "mission-oriented" work somehow violated university priorities. He observed: "I look upon the mission part of this work very much as the dissemination of knowledge, the dissemination of university-obtained knowledge toward the practical problems…of government." These government problems are "too important to leave to the generals," he argued. "It is too important for the university not to get involved."[62]

As the discussion wound down, Frank Press offered a clear synopsis of the problems identified by a large majority of the group, who simultaneously wanted MIT to distance itself from weapons research while exercising new political influence. Press opposed classified research on campus, but nevertheless shied away from proposals that would dismantle the labs entirely, on the grounds that their technical work was important. The problem, as he saw it, was the war in Vietnam. "The emotionalism of a very bad war completely cut off American

[62] Transcript, 29 April 1969, Box 2, Folder 18, MIT.

universities from rendering advice … in problems of defense," he observed. At the same time, he saw the ABM debate as "a revolution… the Senate Armed Forces Committee is being challenged for the first time on a major strategic system. The vote may go against the Department of Defense in Congress." In other words, the unpopularity of Vietnam had called into question MIT's ties to the Defense Department, even as its labs' technical input fueled the unprecedented political resistance to an ABM system. Was there any way to distance the school from Vietnam while preserving its contributions to arms control? The pervasiveness of Defense Department funding across multiple fields and laboratory research programs suggested otherwise.[63]

The View from the Labs

One of the panel's first tasks was to visit the special labs to hear presentations on current projects, tour the facilities, and solicit statements from researchers and administrators. At both labs, the panel's questions and lab staffers' responses gravitated toward a series of key issues: the ratio of basic to applied research, the autonomy of the labs, the importance of the labs' work to MIT's prestige, and ethical dilemmas concerning the inevitability of weapons development and the morality of demonstrating the feasibility of specific weapons systems.

The panel began its investigation at the Instrumentation Lab, where deputy director Roger Woodbury offered his audience a brief history of Draper's work on gunsights, tracking technologies, and autopilot systems. He emphasized that the lab's work on inertial guidance stemmed from fundamental research questions, including the basic task of "establishing geometry and measuring quantities with respect to inertial space." Moreover, even the development of specific devices, such as new kinds of gyros and accelerometers, depended on

[63] Transcript, 2 May 1969, Box 2, Folder 22, MIT; Review Panel Abstracts, Box 3, Folder 13, MIT.

basic advances in materials research and metallurgy. Woodbury and a stream of additional presenters hammered home this point—that basic research underlay much of the lab's work.

Woodbury also argued that the I-Lab differed fundamentally from other industrial labs because it had no profit motive, and because its military contracts supported versatile research that enabled advancements in non-military fields—for example, I-Lab researchers had recently developed a blood viscometer from technologies developed for other projects. This portrayal elicited critical questions from the committee members until Woodbury conceded that the lab did not receive any funds for truly basic, "unrestricted research." Nevertheless, Woodbury focused on convincing panel members of the lab's autonomy from both military demands and industry competition. He noted proudly that "I think what our charter is, is to really maintain a technology that is sufficiently advanced that we are not competing with industry, we are providing information to lead the way." He chose his words carefully; when asked about the Air Force Titan contract, he self-consciously explained: "we were instructed to work on an inertial, not instructed, we *agreed* to work on an inertial guidance system for Titan…" [emphasis added]

Woodbury also distinguished between the proprietary restrictions of industrial research and the more open atmosphere of government-sponsored work. He explained that at industrial labs, researchers were often hamstrung by requirements that they draw on their own patented techniques, building outward from limited, earlier technologies. He offered the example of Sperry Gyroscope, which lost its government contract for a civilian inertial navigation system due to the inefficient reliance on "proprietary information" rather than "available technology." In the case of the Instrumentation Lab, researchers had much more freedom to be creative and innovative. Woodbury described the development of one inertial guidance prototype: "…we built a tremendous thing which was impractical, unsaleable, and we knew it, but it demonstrated the principle, the principle was feasible, and then we went on to develop the state of the art and get it down to size." He estimated that without the innovation of the Instrumentation Lab, private industry might have taken another two decades to make the same advances in inertial guidance.[64]

Other lab administrators echoed this sentiment while addressing specific controversial projects, including the Poseidon missile—a powerful improvement over Polaris, capable of launching multiple warheads toward separate targets. The men explained that the lab had been the "design agent" for the guidance system, with industrial work farmed out to GE, Raytheon, Bendix, and Honeywell, among others. The lab's task was to construct a prototype, "to show it can be built," as program manager Samuel Forter put it. Conveniently, the lab was simultaneously at work on guidance for MIRV—multiple independently-targetable reentry vehicles—and although not part of the initial Poseidon missile design, flight tests for the new 'MIRVed' missile began in 1968. Experts told the committee that even if MIT were to halt its Poseidon work immediately, the MIRV research would continue, albeit at a higher cost, at Raytheon and GE.[65]

Here again was the question that had haunted the Manhattan Project scientists—what were the ethical implications of 'demonstrating the feasibility' of a new weapon system? The lab workers protested that if they didn't do it, other industry or military labs probably would, but less well. And at stake was the reputation and prestige of MIT. As Edwin Porter, Jr., Associate Director of the Instrumentation Lab, would argue in a later memo to panel: so long as the United States existed in an imperfect world, there would be a need for defense research. The

[64] Woodbury testimony transcript, 30 April 1969, Box 2, Folder 19, MIT.

[65] Forter and Houston testimony transcripts, 30 April 1969, Box 2, Folder 19, MIT.

government could find other researchers besides those at MIT, but MIT's decision to abandon the labs would excommunicate it from the adult world of real responsibility: "MIT… would be like the little boy who took his ball and went home," he wrote. "MIT would no longer be a part of the defense effort, and thus would have little or no chance to influence it."[66]

Though several years would pass before the final decision to sever ties with the lab, the seeds of disunion had been planted in these early discussions between panel members and lab researchers. Woodbury's criticism of inefficient corporate research implied support for the continuance of the Instrumentation Laboratory as a non-profit entity, though not necessarily one affiliated with MIT. When the charismatic Draper himself appeared before the committee the following week, he offered a rousing defense of the lab, replete with jokes and larger-than-life anecdotes, but his joviality failed to placate the critical minds of his interviewers. Victor Weisskopf worried that research was a zero-sum game, and that defense research crowded out other options. Most notably, Chomsky criticized lab requirements that staffers gain security clearance, and argued that, in a sense, accepting U.S. national security decisions had become "a necessary condition for being associated with the Instrumentation Lab." Lab researchers might have chosen their particular interests and projects honestly, but their presence in the lab indicated acceptance of the 'the political ideology the labs represent.' This acceptance was not neutral, Chomsky argued. It supported the status quo, and thus fundamentally undermined any lab claims to meaningful autonomy.[67]

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[66] Porter to Panel, 8 May 1969, Box 1, Folder 19, MIT.

[67] Review Panel Abstract, 3 May 1969, Box 3, Folder 13, MIT.

Confronted by the Pounds Panel, scientists at the Lincoln Lab made a kind of reverse argument—their lab could demonstrate feasibility of weapons systems, true, but they could also demonstrate *unfeasibility*. This theme was emphasized during a presentation by Joel Resnick, an assistant group leader at Lincoln, who explained the lab's work on system analysis and the "technical evaluation" of missile programs. In his view, this work consisted of "guidance" and advice as to how best to improve maintenance techniques and further research and development, without any specific recommendations concerning deployment. The Lincoln Lab was uniquely positioned for this task, he explained, because researchers were well versed in the technological details and data necessary, but also had the benefit of MIT affiliation to give them "an objectivity and independence here which will allow us to do the analysis without regard to the sponsor's reaction to the conclusion," to "let the chips fall where they may." As an example, he cited the lab's review of the Sentinel program—an ABM system of radar-monitored perimeters triggering Spartan interceptor missile—for the Defense Department and PSAC. Lincoln analysts reported several potential threats to the effectiveness of the system, and, as Resnick summarized, "when we finished the briefings there was no question about whether the Sentinel system would be effective. I think it was generally realized that it would not be." Resnick emphasized the differences between objective technical evaluation and the politics of deployment decisions: "we believe it is important to have an understanding of the implications and limitations of ballistic missile defense. This is true whether the country will ever deploy it or not."[68]

But members of the panel did not immediately accept this justification. As he had during an earlier exchange with Woodbury of the Instrumentation Lab, Jonathan Kabat wondered whether any kind of technical research into weapons systems exacerbated the arms race simply

[68] Transcript, 1 May 1969, Lincoln Laboratory, Box 2, Folder 21, MIT.

by being conducted. After presentations by lab director Milton Clauser and William Lemnos, a Lincoln group leader working on developing decoys, jammers, and other "penetration aids" to enable American missiles to breach a potential Soviet ABM system, Kabat finally broke in:

> Kabat: …sitting here and hearing this stuff over and over and over again, the offense and defense, and the penetration, and do any people feel after a while that it all just completely insane? How do you feel about this stuff personally? I am addressing this to anyone who cares to answer it. It is depressing the life out of me.
>
> Clauser: I can only refer you to the New York Times story about the Russian missile launches. Do you believe they are unreal?
>
> Kabat: No. I am willing to consider their reality.
>
> [Unidentified speaker]: I can answer that this way: when the Kwajalein [missile-testing] site was first set up I went down there and lived on Kwajalein for a year. When I saw my first re-entry it made me sick and the reason I am here, as far as I am concerned, is trying to prevent that from happening in reality. You may or may not agree this is the way to do this, but I believe that only a laboratory like this connected to MIT can allow us to make an input which is an independent and free input and that is what we have been doing as individuals and groups. Some of us have been sick for a long time about this, I am sure. It is a nasty world that we live in.
>
> Kabat: The question is, what is the most effective way to change that?
>
> Clauser: This is something we all have to think about. It is to be noted that a number of us operate privately. We haven't given up our citizenship. We operate privately and make our voices heard, but we don't confuse this role with the Laboratory role.
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> Kabat: I don't think it is a technological problem; I think it is a sociological problem.[69]

In an open hearing the following day, Clauser and the panelists also clashed over the nature of Lincoln's Vietnam-related research. Under stiff questioning, Clauser acknowledged that the lab had done some work on "moving target" radar, after he had received "a personal appeal from Johnny Foster… to help some of the people in Vietnam." A Lincoln-designed mobile radar unit, designed to detect person-sized movements, had been sent to protect a U.S. camp in Vietnam. Lincoln researchers were also at work on developing an antenna system to detect underground anomalies, such as the presence of tunnels. [70] In other presentations, Lincoln

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[69] Transcript, 1 May 1969, Lincoln Laboratory, Box 2, Folder 21, MIT.

[70] Transcript, 2 May 1969, Box 2, Folder 23, MIT.

staffers had taken pains to distance themselves from any Vietnam applications, suggesting that while most lab workers considered ABM work valuable, Vietnam perhaps presented more complex ethical challenges.[71]

Some of this ambivalence emerged in a joint statement to the panel submitted by staff members from Lincoln on May 15, agreeing that "research on defense problems is presently overemphasized at the Special Laboratories" but imploring the panel not to sever ties to the lab. The staffers carefully defended the lab's acceptance of classified research and of its military advisory role. Most of the classified material at the lab consisted of "externally generated" background government documents used to "guide" specific projects, not actual results from ongoing lab work. (This assertion was refuted by a "Fact Sheet on Lincoln Laboratory" produced by MIT's Office of Public Relations, which reported that 40% of Lincoln's research was classified.[72]) More importantly, the lab's role as a source of candid advice—an "honest broker" who could "help in the settlement of disputed technical questions"—could not be replicated by any other group of experts, including "an Ad hoc review board… a JASON group, or…a PSAC subcommittee." These other, well-intentioned but part-time groups simply could not bring the same amount of time and experience to the task. For example, who better to assess ABM system efficacy than Lincoln scientists, with their extensive firsthand understanding of the kinds of radar and optical data available to make discrimination decisions?

As their colleagues at the Instrumentation Lab had, Lincoln staffers also invoked the potential blow to MIT prestige. How could MIT *not* weigh in on these crucial decisions? Should the university sever its ties to the lab, they'd be giving up their access to key technologies and

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[71] Transcript, 2 May 1969, Box 2, Folder 23, MIT. For example, researchers assured Kabat that Lincoln's satellite technology, purchased by the Defense Department, was not being used in connection with the war in Vietnam.

[72] "Fact Sheet on Lincoln Laboratory," Box 1, Folder 18, MIT.

their authority to offer meaningful, influential assessments. MIT's reputation "would be tarnished for having turned its back on an active, progressive laboratory. It would be said that instead of continuing to serve society through the lab on the large scale necessary for today's problems, MIT turned down the challenge." [73]

The committee also met with Jack Ruina, former ARPA head and MIT Vice-President for Special Labs since 1966, and Paul Cusick, the MIT comptroller. Ruina clarified aspects of the process by which lab contracts were approved. A small group of university administrators—including the president—oversaw lab contracting decisions. Their determinations took into account several key factors, he explained, including: ensuring that the labs pursued broad programs of national importance (as opposed to small, specialized applications); whether the work constituted a "public service" of some kind; campus attitudes; and whether the labs actually had the technical capability to do a good job. The labs did not accept contracts blindly, and had previously refused certain projects, including chemical and biological weapons research and intelligence work. Cusick discussed the finances of the labs, and the possible economic consequences of severing them from the universities. He worried that although MIT was a non-profit institution, losing the labs could potentially result in an extra $12 million in overhead expenditures. He also warned that the character of labor performed at the labs would also be altered if they were severed from MIT, due to changes in contracting. Whereas MIT maintained its own grant-like contracting system, the spun-off lab would be subject to more traditional fee arrangements, such as those at MITRE, where employees submitted time sheets and were

[73] "Lincoln Laboratory Staff submission to The Panel on Special Laboratories," 15 May 1969, Box 1, Folder 18, MIT.

monitored like industrial workers. Cusick called such a system “unacceptable” for a university lab, but the norm in industry.[74]

Among the politicians, university administrators, and laboratory group leaders, few voices from the middle tiers of research staffers were heard. Only Paul Easton, a former Lincoln staff physicist from 1967-1968, addressed the panel at any length, and his convoluted narrative and obvious disenchantment irritated Pounds, who cut his testimony short. In the short time he spoke, however, he offered a detailed glimpse into a world that was nowhere as intellectually stimulating or ideology-free as others had portrayed it. Easton had applied for a plasma physics job at Lincoln after a graduate school stint working for the Air Force. As part of Lincoln’s Project PRESS, Group 35, he analyzed data collected at Kwajelein in order to develop techniques for discriminating between re-entry vehicles and decoys. The job paid well and his boss was laid-back, but he found the work itself to be easy and ‘low-level,’ despite the group’s reputation for brilliance. He recalled his coworkers spending hours playing the stock market.

Though opposed to the war in Vietnam, Easton initially justified his classified military research on the grounds that an anti-ballistic missile system was a defensive, deterrent technology. Over time, however, he began to wonder if the ABM wasn’t simply a deterrent, and if other groups at the lab—those working on “penetration aids,” for example—used his group’s data for offensive purposes. Even then, the nature of his research didn’t worry him excessively, for the simple reason that he, like most of the other members of his group, did not believe the ABM system would work. The more Lyndon Johnson and Republican politicians publicly supported the ABM, the more his colleagues laughed “at how dumb those congressmen were.”

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[74] Review Panel Abstract, 4 May 1969, Box 3, Folder 13, MIT.

The real problem, for Easton, was his anger about Vietnam, and his slow realization that if he traced the chain of command high enough, he was working for Robert McNamara. He offered the panel a kind of psychological analysis of the lab culture that mirrored accounts of wartime troop cohesiveness:

> Essentially, what was motivating people to work there was not interest in the work, because it was not interesting work; it was not idealism because it was obviously not useful work, but it was the kind of team spirit that people who work, you know, people like one another and they were working for the approval and respect of the other people on the team. And then, somehow… you have to have some sort of positive feeling about the other people on the team, and you also have to have positive feelings about the guy your boss reports to and the guy he reports to, and eventually you report to Secretary McNamara or some guy who I would consider a maniac… And the necessity to consider yourself a member of a group which includes Secretary McNamara and his friends in the administration… does something to one's thinking. … if one doesn't want to get into all kinds of angry reactions and all kind of stomach disturbances, he has to not resent the Defense Department too much.

Easton closed by warning that the lab staff was not "a neutral and disinterested group," because the experience of working there promoted a kind of loyalty to government leaders. As he put it, in language that surely failed to convince the skeptical Pounds, working at the lab "does something to your head."[75]

Meanwhile, outside the committee room, public pressures were mounting. UCS demanded that Poseidon research be "terminated as soon as possible," including MIRV testing, on the grounds that successful MIRV flight tests would be "irreversible" and would "raise arms control to a new level of difficulty." To UCS, the Instrumentation Lab housed the most objectionable classified research, and therefore required urgent intervention. Lincoln, in contrast, could be more gradually converted to other projects. But equally urgent for many were the

[75] Transcript,15 May 1969, Box 3, Folder 5, MIT.

employment consequences of any such immediate changes: Local 254 of the Building Service International Union, representing 860 lab employees, threatened to seek an injunction if MIT curtailed its research support. The group's business leader, Edward Sullivan, complained to *Electronic News* that "it's a group of pseudo-intellectual vagabonds who are stirring up the trouble at MIT. It's no longer a question of campus fun and games. It is the economic security of thousands of people which is at stake."[76]

Hundreds, if not thousands, of letters poured in, from alumni, current students, concerned citizens, and MIT staff, overwhelmingly defending the labs.[77] Two hundred and thirty-six Instrumentation Laboratory employees signed a petition to Howard Johnson, supporting the Poseidon program and their contributions to its guidance system. Lab workers also submitted dozens of personal position papers to the panel. Some, like John Allen, an Associate Division Head at Lincoln, emphasized their antiwar credentials while defending the nature of their research; he wrote that while he considered the war in Vietnam "one of the greatest mistakes in the history of the U.S.," he saw nothing "irreconcilable" in working to develop technologies to protect the lives of American soldiers while politically agitating for withdrawal. Others, such as Allen's colleague Thomas Casey, offered more philosophical assessments of the nature of scientists' obligations. He denounced claims that scientists bore special responsibility for the applications of their work. "I see no reason to believe that the man who designs the guidance system for a missile is necessarily <u>more</u> qualified to control its use than is a man who designs television sets," he wrote. "He is not more qualified than the average citizen to dictate detailed

[76] *Electronic News* 5 May 1969, in Box 1, Folder 25, MIT.

[77] Poll results, Box 3, Folder 32, MIT. Committee staff sorted response letters into separate position categories, from strong defense of labs to strong opposition to weapons-related research. By my count, reactions supporting the labs or status quo numbered 173 individual letters and 254 petition signatories; opponents 25 letters plus over a hundred signatories of the UCS statement. About 28 letters complained of other, indirectly related matters, such as the tactics of student protesters.

government policies. And refusing to build a missile that is considered necessary by the government is attempting to do just that." A few paragraphs later, however, he acknowledged that MIT's enormous influence would be lessened if it gave up control over its labs. Therein lay the tension. As an individual, Casey wanted no extra moral burden due to his work at the Instrumentation Lab, but he recognized—and endorsed—MIT's institutional power: "When MIT speaks," he wrote proudly, "people listen."[78]

Charles Broxmeyer of the Instrumentation Lab further explored this tension, between individual guilt and institutional responsibility, in a deeply emotional statement read to the members of the committee. Broxmeyer, an electrical engineer, had joined the Instrumentation Lab in 1955, after receiving degrees from Drexel and Penn and serving a stint at the Naval Air Development Center. At the I-Lab he specialized in inertial navigation and guidance, literally writing the book on the subject in 1964, and spent two years on loan to Raytheon's Space and Information Systems Division. Now a "technical consultant" for the I-Lab's Deep Submergence Systems Group, he defended the work of the special laboratories to the Pounds Panel, complaining that due to the sudden shift in campus opinion, "magically the work which was always so important, useful, aesthetically satisfying and necessary has now become trivial, harmful, ugly and definitely superfluous." More powerfully, he accused the faculty members on the panel of acting out of guilt and cowardice, projecting their own regrets about Manhattan Project contributions and their frustration at their own inability to stop the war in Vietnam on Broxmeyer's innocent colleagues—an ill-conceived and dangerous act of scapegoating. In a particularly evocative section, he observed:

[78] Allen to Pounds, 7 May 1969; Broxmeyer statement, 8 May 1969; Casey statement, 6 May 1969; Position Papers; Box 3, Folder 33, MIT.

> I have been sitting here listening to the various speakers—to the laboratory members explaining their work of half a lifetime with quiet voices, hoping it will be liked. Maybe the committee won't like it. And somehow the work in which they always took such pride has, suddenly, become shameful. …
>
> And I have listened to other distinguished gentlemen who with finely woven arguments and subtle intellectual logic have been standing at this podium, proving by the hour with geometric precision that the integrity of the university requires that the special laboratories must be destroyed, their staffs dispersed, for the good of everyone. It is a matter of historical necessity.
>
> Their logic is beautiful… However, none of these intellectual gentlemen ring true to me. They all seem to be concerned really with something else. …
>
> The MIT community, symbol of the rational conduct of human affairs, symbol of the power of science and technology, is indulging itself in the ancient rite of purification known as scapegoating.
>
> **The agony of the Vietnam War has induced intolerable feelings of anger, guilt, frustration and self-hatred in the American people. Further, we are even deprived of an object for our hatred. We can't hate the government because the government is reported to be trying to stop the war. We can't hate President Johnson because he isn't there anymore. We desperately need objects on which to deposit out hatred.**
>
> On the campus, the feelings of guilt for what is going on reside mainly in the faculty. What the students feel is fear. They are frightened, of being drafted, and being shipped to some jungle in Vietnam like livestock, and forced to kill, and then finally, slaughtered themselves for no reason. … **The special laboratories, which every shred of evidence indicates have nothing whatever to do with the Vietnam War, have been selected to be destroyed. Only then will the collective guilt of the MIT community—that part of it which remains—be expiated. Only then will the students and professors feel cleansed and purified. …**
>
> The men of the physics department have additional guilt—the blood of Hiroshima still unatoned for. Right now the students demand that you do not work on weapons, and you do not work on weapons. You beat up Draper and the students are with you. But what will happen when there are no more Drapers to beat up and the students demand not only that you do not work on weapons but that you must never have worked on weapons. We all know that such arguments are possible…[79]

There were more than just shreds of evidence linking the labs' work to the war in Vietnam; testimony to the committee had established as much. But with startling precision, Broxmeyer had tapped into the guilt, anxiety, and disillusionment of many MIT scientists: guilt for choosing a field with applications to weapons and war, deep disillusionment with government policy, and anxiety that recourse for changing the state of affairs no longer existed.

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[79] Instrumentation Laboratory documentation, April 1968, Box 1, Folder 13, MIT; Broxmeyer position statement, Box 1, Folder 3, MIT, emphasis added.

Perhaps most influential for panel members were the statements and testimonies of the towering former presidential science advisors and PSAC members, veterans of the Sputnik push for greater scientific research funding, now called to account for the backlash. On May 8, Jerome Wiesner, then provost of the university, appeared before the committee. Though generally opposed to classified research on campus, he distilled the committee's problems to two key questions: first, did the labs add or detract from MIT's educational mission; and second, did they contribute to the national welfare? On the first question, Wiesner answered yes; the labs provided access to sophisticated technologies, ideas, and equipment. The moral risks of weapons research itself was trickier. He observed that unilateral disarmament was impractical, and there was some value in specific types of weapons evaluations, as a means to "prevent panic" about hostile foreign systems. Nevertheless, he distinguished between general research on weapons systems and actual development of operational technologies. He explained, "We should try not to be engaged in the development of things that kill people. We can be involved in understanding of fundamental technology that might be useful in the development of… military weapons, make a deep contribution to the defense field."[80]

Caltech president Lee DuBridge, soon to be appointed science advisor to Nixon, weighed in with an oft-reprinted article in the *Bulletin of the Atomic Scientists*, agreeing that "it is not appropriate for secret military research to be carried on within university campuses. …but I do not agree that universities should not accept any research support from the Department of Defense." He endorsed military-sponsored basic research and lauded the "wholesome influence" of university professors and other independent advisors on the military establishment.[81]

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[80] Review Panel Abstract, 8 May 1969, Box 3, Folder 13, MIT.

[81] Lee Dubridge, *Bulletin of the Atomic Scientists*, May 1969, reprinted in the *Boston Globe*, 11 May 1969.

George Kistiakowsky, like the members of the panel themselves, agreed that "scientists and engineers have a very major social responsibility to society" but had difficulty enumerating the exact policy obligations such responsibility entailed, beyond individual activism. He confessed that he could not "conceive as bad" the "acquisition of scientific knowledge," and that he did not believe that "the scientific technological community" was "competent to make decisions on what's good and what is bad in the way of applied projects—good and bad in a social sense." Nevertheless, the same community ought to be "urging and pushing people…into making better decisions." He described his own actions in opposition to the escalation of the war in Vietnam, including his resignation from his Pentagon posts: "When it became clear to me that the military had sabotaged—and I use now the word deliberately—the project… I resigned." But despite his disassociation from the Defense Department, Kistiakowsky still represented an older, more individualized ethical stance. In part, this view stemmed from his conviction that military-academic ties were nowhere as strong as protesters had suggested. Dismantling the Instrumentation Lab would not halt the arms race; likely it would result in somewhat "inferior guidance for Poseidon." The military was not utterly dependent on MIT, and MIT's decisions would not drastically alter the course of U.S. foreign policy.

Kistiakowsky's focus on individual decision-making influenced his views of appropriate MIT policy. Though he considered MIT's blanket rejection of military funds to be "silly," he did not endorse accepting any offer "without any qualifications." "The money is not tainted," he observed, and MIT policy ought to take into account the different individual views of the researchers involved in order to find a "golden median." An MIT panel should review contracts and strive for imperfect moral decisions that would be "maximally satisfactory" to the MIT community. And in this process, Kistiakowsky saw little place for students. They could be consulted, of course, but in general he thought "students should be seen and not heard very much. By and large they are too emotional, they don't have time to look into issues in sufficient depth."[82] Judgments on projects were best left to individual, thoughtful experts.

Also influential was a report to the panel from a meeting of Eugene Skolnikoff and a group of high-powered physicists including Wolfgang Panofsky, Herbert York, and Harold Brown. Neither Panofsky, York, nor Brown supported classified research on campus, in general, but they balked at its abolition, recommending instead that MIT strive to minimize it or move it off campus. Skolnikoff also reported on the status of other similar university-laboratory relationships: Stanford and SRI, the University of California and Livermore and Los Alamos, and Caltech and the Jet Propulsion Lab.[83]

Chomsky and the Pounds Panel Reports

As the Pounds Panel members began to digest the hours of testimony and thousands of pages of documentation and personal statements of lab researchers in order to draft an interim report, the issue of personal and institutional responsibilities loomed large. In a long, eloquent internal memo to Pounds, Chomsky reviewed many of the philosophical and practical concerns facing the committee. In his view, the panel's work—and their eventual public report—could be enormously influential for two audiences: the nation as a whole, and MIT policymakers. For the national audience, he wanted a strong statement that government priorities "must be reordered, with war-related expenditures dropping sharply and national energies turned to other pressing social needs." In promoting this vision, he hoped the committee would "maximize" its impact by

[82] Transcript,13 May 1969, Box 3, Folder 2, MIT.

[83] Skolnikoff to Pounds, Box 1, Folder 19, MIT.

"being even more optimistic… than we may privately feel." In this context, Chomsky emphasized the special position of scientists, and the special position of MIT as their institutional home, arguing that a certain "form of politicization" would be welcome. He wrote:

> Science and technology have a powerful impact on society and on history. Those who develop science and technology have in their hands a powerful weapon of destruction, and a major instrument for overcoming the problems of contemporary society. They must be aware of this fact, and conscious of their responsibilities in regard to the use of science and technology. To exercise this responsibility, they must, continually, make political and historical judgments. This is true of the work of an individual. It is far more important when the university makes an institutional commitment to the support of organization of research.

Chomsky hoped these commitments would be subject to democratic evaluation by the entire university community, and ideally would result in serious and "dispassionate deliberation." Though he wrote that all people in democratic societies ought to exercise these responsibilities, Chomsky felt it was "particularly important in the case of scientists and engineers because the consequences of their acts—their research, study, and teaching—are potentially so immense." This type of politicization, Chomsky wrote, ought to be encouraged, and should take place in democratic committees composed of faculty, staff, and students.

The report's second, related, goal, Chomsky wrote to Pounds, should be to provide realistic plans and policies for MIT's immediate future. Here he reiterated earlier ideas about the dangerous kind of politicization of the university, particularly the adoption of "a consensus set elsewhere," as exemplified by the stipulation that lab employees agree to submit to security clearance and to receive defense money as funding. Most importantly, Chomsky urged that MIT not sever its ties to the labs, but rather set clear guidelines as to acceptable research, including bans on any work that "contributes to offensive military action," any "involvement in any form of counter-insurgency operations, whether in the hard or soft sciences," any "actual development of weapons systems," and, in general, any contributions to the "unilateral escalation of the arms race" (though research into defense and deterrence might be acceptable). Chomsky estimated that these prohibitions would "rule out" CBW, MIRV, and any "steps toward deployment" of ABM. He also called for an end to Project CAM, which he didn't think would "contribute to science in any serious sense" other than software development, and would "be used, primarily, for repression of popular movements and interference in the internal affairs of other nations, and perhaps for domestic repression as well."

The better course of action, then, would be conversion. He envisioned the I-Lab as "an interdepartmental laboratory… sharing some of the characteristics of RLE or the Magnet Lab," with no classified research, no MIRV work, and the promotion of "socially useful technology" and basic research. Lincoln presented a more complicated scenario. Chomsky expressed deep skepticism of lab administrators' claims that they provided "objective and independent evaluation of weapons systems." He noted that he "did not doubt the integrity of the laboratory staff, or their competence," but nevertheless felt that they represented "a limited range of opinion," working in a lab entirely funded by the Defense Department, where employment was subject to security clearance and tacit acceptance of the funding terms of the lab. MIT ought to apply the same standards to Lincoln as it did the I-Lab, but he acknowledged that if the restrictions proved too "sharp," the DoD might "simply take over the laboratory." Nevertheless, MIT should try for restriction and conversion, especially since it would send a strong national message. Even better might be a system in which Lincoln received funding from a more neutral government body—for example, Congress—and became a truly independent facility not subject to clearance requirements.[84]

Most of Chomsky's ideas found expression in the panel's interim and final reports, issued that spring and fall, accompanied by appendices of "Additional Statements by Members of the Panel." In the main reports, the panel endorsed retention of the laboratories coupled with explicit efforts to shift research to more civilian-oriented missions and provide better educational opportunities to undergraduates. The statement did not reject defense work entirely, nor did it propose permanently halting classified research (which had been temporarily banned while the panel met). In the strongest language of the report, panel members recommended an immediate end to Poseidon research, on the grounds that MIT labs should not produce prototypes and deployment-ready weapons systems. They also deeply criticized Lincoln's people-detector research, suggesting that the lab should have transferred this work to the military once it reached the testing phase. But in far weaker language elsewhere, the panel stated that these concerns were separate from any "collective judgments about military and strategic national policies," judgments which the majority of the panel disavowed as inappropriate. To manage future contracting decisions, the panel recommended creating a "Standing Committee" with faculty, student, administrative, and lab representation to advise MIT's president on approvals.[85]

It was this weak language that Chomsky attacked in his appended statement, in which he reiterated much of his earlier correspondence with Pounds and restated the authority of MIT to make judgments on military strategy and policy, given that in his view accepting defense contracts already constituted a form of political judgment. He eloquently harkened back to his

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[84] Chomsky to Pounds, Box 1, Folder 26, MIT.

[85] MIT Office of Public Relations press release, 2 June 1969, Box 1, Folder 19, MIT; "First Report of the Review Panel on Special Laboratories," 31 May 1969, Box 4, Folder 14, MIT.

reflections on responsibility, calling for scientists and engineers not to "remain blind to the question of how their contributions are likely to be put to use," or to act as if phrases like "national interest" or "public service" meant ideological neutrality. He rejected the majority claim that MIT ought not make "collective judgments on military and strategic power," arguing instead that "such judgments are entirely appropriate, indeed, inescapable." He wrote: "In an institution largely devoted to science and technology, we do not enjoy the luxury of refusing to take a stand on the essentially political question of how science and technology will be put to use, and we have a responsibility to take our stand with consideration and care. Those who find this burden intolerable are simply complaining of the difficulties of a civilized life."[86]

He also questioned the labs' independence and called for an end to all war-related research not strictly confined to defensive or deterrent work, thus preventing projects connected to chemical and biological weapons, MIRV, ABM deployment, and counter-insurgency (including social science research). He reiterated his preference for the creation of a student-faculty-staff committee with greater authority than the proposed advisory body of the main report, including authority to review all research contracts over $50,000.

Reaction and Implementation

Issued after the end of the spring semester, after the departure of most undergraduates, reaction to the interim report garnered largely favorable reaction. A *New York Times* editorial lauded MIT's decision not to sever ties to the labs or 'spin them off' in some way, noting that:

> The sole effect of such a spinoff, in the absence of basic redirection of Government research activities, would be to force the armed services to set up more laboratory complexes of their own or to give huge new contracts to

[86] Chomsky statement, Appendix II, "First Report on the Review Panel on Special Laboratories," 31 May 1969, Box 4, Folder 14, MIT.

> aerospace and other corporations. The result would be precisely opposite to the one desired by student and faculty dissenters—a strengthening of the military-industrial complex and a diminution in the capacity of university scientists to exert any useful influence in the shaping of public policy on military matters.[87]

MIT administrators took the Pounds panel's reports seriously and convened a series of meetings of the Academic Council, which included every dean and vice president, to plan implementation of the recommendations and reiterate support for ending MIT contributions to research related to "operational deployment of weapons systems."[88] Jack Ruina's old position as vice president for special laboratories was remade as vice president for research, and he was replaced by physicist Albert Hill. The Academic Council also approved the creation of the standing committee called for by the interim report, initially headed by chemistry professor John Sheehan and including two student representatives and two lab staffers among its ten members. The group was to meet in private sessions to evaluate potential lab contracts. In her 1971 account of the turmoil at MIT, sociologist Dorothy Nelkin summarized the nine key criteria used to judge contracts:

> The potential for favorable interaction with MIT's educational operations
> The uniqueness of the MIT contributions
> The degree to which projects would evoke favorable MIT attitudes
> The intellectual challenge
> The national importance of the problem
> The degree of basic research represented as opposed to production, field test, and deployment
> The absence of immediate identification with a weapons system
> The adequacy of existing national review of the problem
> The potential for civil application[89]

Nelkin observed that the function of this committee challenged notions of "academic freedom," and fostered resentment among lab staffers, some of whom reportedly referred to it as the

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[87] *New York Times*, 4 June 1969.

[88] Nelkin, 86-87.

[89] Nelkin, 90-91.

"morals committee." Meanwhile, SACC complained about the lack of procedural transparency and small student representation.[90] But by and large, student support for halting MIRV research and genuine efforts at reconversion remained strong.[91]

Far more unpopular than the panel's report and creation of the standing committee, however, was the subsequent administrative decision to replace Draper with Charles Miller, chair of MIT's civil engineering department, with the blow softened by officially renaming the Instrumentation Lab after its founder. Draper angrily told reporters that he had been fired, and his bitterness captured national attention. Joseph Alsop churned out column after column that fall criticizing MIT's treatment of Draper, which he called "Oppenheimer in reverse." He consistently referred to the New Leftists and other lab critics as "storm troopers" and "neo-McCarthyites."[92] Other industry publications echoed Alsop's language. *Air-Force/Space Digest* labeled the "self-appointed zealots" responsible for Draper's exit "latter-day Savonarolas," and quoted Draper's description of the Pounds Panel as "an inquisition." The same article referred to "professors still torn with guilt over Hiroshima and Nagasaki" and closed with extended quotations from Broxmeyer's emotional statement on the dangers of scapegoating.[93] Nelkin reported the presence of "Doc Draper forever, SDS Never!" signs posted throughout the Draper lab.[94]

Clashes on the polarized MIT campus continued throughout the fall. In September, MIT's Executive Committee of the Corporation announced that the university would no longer "incur

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[90] Nelkin, 92.

[91] Nelkin, 120.

[92] Alsop clippings, Box 2, Folder 2, MIT.

[93] William Leavitt, "A Triumph of Reverse McCarthyism," *Air Force/Space Digest*, December 1969, 46.

[94] In Nelkin, 96 and *The Tech* (MIT), 24 October 1969.

new obligations in the design and development of systems that are intended for operational deployment as military weapons," though the status of Poseidon research was left unaffected.[95] In October, the Pounds Panels' final report appeared, with initial support from president Howard Johnson and a majority of faculty members. Even Draper eventually endorsed a trial period during which the special labs would be subject to the Pounds recommendations, with the final decision on divestment contingent on the success of the trial.[96]

But after several months of continued student protests (including the arrest of radicalized Pounds Panel member George Katsiaficas), escalating threats and rumors of violence, lab resistance, and gridlocked standing committee meetings, faculty support for total divestment began to swell. At a March 1970 faculty meeting, Ascher Shapiro of the Fluid Mechanics Lab, the model for reconversion, offered a surprising endorsement of divestment, arguing that national priorities and budgeting decisions lay at the heart of the militarization of university research, and that attempts to convert MIT labs would only lead to fiercer competition for scant resources, not major shifts in defense contracting.[97] After a well-guarded meeting of MIT's Executive Committee, the university announced in May 1970 that attempts to reconvert the Draper Lab had ended, and MIT would sever its administrative and financial ties to the facility. President Howard Johnson explained, "I do not believe that we have the right to hurt the capability of the laboratory by continuing to impose a restriction that neither the laboratory nor its contractors are willing to accept. Were we to force that situation, we would be wrong, and it would not work."[98]

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[95] *The Tech* (MIT), 26 September 1969.

[96] *The Tech* (MIT), 24 October 1969.

[97] Leslie, 239.

[98] "Statement by President Howard Johnson on the Special Laboratories," 20 May 1970, in Appendix II, Nelkin, 181.

Outside observers speculated that the administration had failed to locate sufficient funds for the nonmilitary projects necessary to keep the lab open and in accordance with the Pounds Panel's recommendations.[99]

Though the MIT *Tech* published a supportive editorial and reported that faculty reaction was "difficult to gauge," Nelkin observed that the policy met with "ambivalent" reactions at the lab, and general dissatisfaction among student activists.[100] She wrote, "The administrative decision, intended to accommodate as many interests as possible, pleased few." Within a year, the unpopular Johnson had been replaced by Wiesner as MIT president, and a new interim Draper board of directors was created with Draper, former Kennedy advisor Carl Kaysen, and IBM scientist Emanuel Piore among its members. In the summer of 1973 the Draper Lab officially remade itself as an independent nonprofit corporation, still housed in lab buildings adjacent to the MIT campus, but no longer a part of the university.

Lincoln Lab, in the meantime, had tried desperately to emphasize its relevance to arms control efforts and to make itself more available to students, including opening the Lincoln Laboratory Ballistic Range—containing a large chamber in which a light gas gun could fire spheres and cones at speeds approaching 26,000 feet per second—and providing free shuttle service from Lexington to the MIT campus.[101] In the end, two factors most likely saved Lincoln's relationship with MIT. First was its status as a source of criticism for the ABM, rather than a producer of hugely unpopular weapons technology (as was the case with the

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[99] For example, see Victor McElheny, "MIT Administration Makes Public Its Intentions on Disposition of Draper and Lincoln Laboratories," *Science* 168 (29 May 1970), 1074-1075.

[100] *The Tech* (MIT), 22 May 1970; Nelkin, 145.

[101] Clauser to Ruina, 17 September 1969, Box 1, Folder 18, MIT.

Instrumentation Lab and MIRV). Second, the lab's off-campus location meant that MIT could consolidate and relocate any remaining classified research projects to the suburban lab site, thus easing some of the political tensions in Kenmore Square without fully severing its valuable contractual links to the Defense Department. For the duration of the Cold War, Lincoln would follow the Livermore model: a site for classified, cutting edge defense research that was geographically—but not financially or administratively—separate from a top tier undergraduate campus.

In the short term, severing ties with the Draper Lab coincided with a difficult period of federal funding cuts and other economic woes facing universities in the 1970s, and MIT scrambled to secure funds for graduate students, researchers, and additional programs.[102] After some initial turbulence and staff reductions, however, Draper Lab fared well. Leslie observes that the lab "prospered in its new role," and "maintained its reputation as a leading center for inertial guidance technology." MIT graduate students still worked at the lab as "Draper Fellows," undergraduates attended seminars there, and the facility's resources brought together academic researchers, corporate scientists, and military personnel. Draper researchers continued work on the high profile Apollo program (including assisting in the rescue of Apollo 13) throughout the 1970s, the Trident and Minuteman missile technologies of the same decade, the space shuttle in the 1980s, and robotics and advanced guidance systems throughout the 1990s, including GPS. As Leslie sums up, "In every way that mattered, nothing had changed except on paper."[103]

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[102] *The Tech* (MIT), 20 March 1970.

[103] Leslie, 249-250

After a brief period during the 1970s, when its research program broadened to include topics related to health and energy, Lincoln Lab followed a similar path. Rather than allow for easier reconversion transitions, Lincoln's status as an FFRDC led to research that was "tilted even further toward applied research and direct military applications." By the early 1980s, "about a quarter of Lincoln's budget [came] from the Strategic Defense Initiative."[104] In 1986, a second MIT review panel investigated the work of the lab, determining that it had not followed the Pounds Panel stipulations to pursue non-defense research and educational goals. John Deutch, a chemist and MIT provost at the time, acknowledged that "The 1969 strategy to broaden nondefense work at Lincoln has just not worked out, and frankly, is not likely to work out in the near term given the priorities in Washington."[105] No steps were taken towards divestment, however, and a decade later Deutch himself would take top positions at the Pentagon and CIA during the Clinton Administration.

Leslie also observes that the late Cold War success of the Draper Lab and the many Route 128 spinoff companies that likewise relied almost exclusively on Defense Department money represented heavy investment outside the "civilian economy." Economically, they did "nothing to reverse America's decline" in terms of deindustrialization and globalization.[106] Leslie mourns the passing of 1970s efforts to 'civilianize' engineering and hard science research, noting instead the triumph of increased militarization of campuses in the 1980s, in the form of the DOD-University Forum, heavy defense funding, and the rise of new military-university powerhouses: Georgia Tech, Carnegie-Mellon, Penn State, and others.

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[104] Leslie, 250.

[105] *Boston Globe,* 24 April 1986.

[106] Leslie, 254.

Despite these trends, the March 4 movement and the actions of the Pounds Panel at MIT were not simply weak roadblocks in a larger long-term race to build a ‘Massachusetts Miracle’ out of the growing defense industry. MIT’s failure to reconvert its labs contributed to this process, as did as the related trend of locating defense firms in suburban areas away from campus radicalism and urban activist groups. But the protests at MIT also pushed the ethical dilemmas of weapons research, university-military relations, and institutional responsibility into a broader national political discussion. Like Livermore employees in the 1980s, researchers at MIT’s special labs were forced, often with hostility and anger, to confront the nature and applications of their research. This process was painful for some and irritating for many, but nevertheless constituted the kind of individualized intervention that many Manhattan Project veterans later wished had occurred during World War II, even if it did not result in any change in their decisions to work on the bomb.

From an institutional standpoint, MIT set a precedent for establishing some limits on university-managed defense research. As Nelkin wrote in 1971,“MIT, more than many other universities, lives with the recognition of its relationship to the ‘real world’; indeed, it was the scope of its involvement outside the university that led activists to argue that the claim of political neutrality was a myth; that even the acceptance of given research priorities is an act in support of a particular political system.” The protests and debates at MIT inspired similar actions at universities across the country and the globe, extending even to professional organizations with deep-rooted claims to neutrality, such as the American Physical Society and the American Association for the Advancement of Science. All of these institutions would face the difficulty of creating universal moral standards in a postmodern world of contentious politics and shifting values. Nelkin observed this at MIT: “conflict concerning the direction of technological research is inevitable… For societal needs are themselves in conflict, and with no accepted system of social ideals that would define an appropriate use of technology, universities are necessarily exposed to the politics of competing interests and discordant demands."[107]

Jerome Wiesner also reflected on these trends, filtered through his personal experience, in his final commencement address to MIT students in the spring of 1980. "As science advisor to Presidents Kennedy and Johnson, and now as president of MIT, I have for years been under heavy pressure to defend science and technology," he told his audience. "At first, I did this with some of the same trepidation one would have in defending one's naughty child, but with each new challenge… I found that I was entering a new culture—or at least subculture—that saw the problem (and the world) though an intellectual filter quite different from mine." As he described his struggles to reconcile differing worldviews and to consider the balance of moral and technical concerns, Wiesner invoked the name of one of the leading scholars in the history of science, Princeton professor Thomas Kuhn, and Kuhn's account of "the difficulty of changing scientific structures" and the inherent challenges of overturning deeply held, paradigmatic assumptions. Wiesner noted ruefully that what Kuhn had written about science applied equally to assumptions about social priorities, including the social priorities of science and technology, but paradigm shifts were "even more difficult in the societal setting." [108] At Princeton in the turbulent early 1970s, Kuhn himself was learning this difficult lesson as well.

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[107] Nelkin, 155-56.

[108] Jerome Wiesner, "Commencement Address," 2 June 1980, in Walter A. Rosenblith, ed., *Jerry Wiesner: Scientist, Statesman, Humanist* (Cambridge: MIT Press, 2003), 365-366.

Chapter Five:
The Problem of Neutrality for Academia and Professional Societies

Neutrality and the American Physical Society

Founded in 1899 at Columbia University, the American Physical Society (APS) by the late 1960s was arguably the most prestigious professional scientists' society in the United States. Its membership approached 30,000, ranging from the rising stars of graduate schools to Nobelists and grizzled Manhattan Project veterans, and it constituted one-seventh of the umbrella American Institute of Physics, which published sixteen scholarly journals, including the *Physical Review*, *Physical Review Letters*, and *Physics Today*. The purpose of the society, and its parent AIP, was publicly affirmed in the opening pages of these journals: "the advancement and diffusion of the knowledge of physics and its applications to human welfare."

Behind the façade of scientific star power and technical prestige, however, lay deep political, economic, and generational divides. An early 1970s informal study of the makeup of the APS executive committee revealed a predictable homogeneity of background and position: of the eleven members, all but two were over forty years old, all held tenured university professorships or similarly secure laboratory positions, all but two had some association with Columbia, Cornell, Harvard, or MIT, and all but one had been educated in the northeastern United States. As one critic remarked, this "alarming pattern of conformity" skewed heavily toward "large institutions, PhD mills, with fat research contracts…"[1] The comfort and placidity of the organization's leadership alienated many younger physicists, particularly those who faced

[1] Krane to Schwartz; 12 June 1973; Box 2, Folder 2; Papers of Brian Schwartz, 1966-1977 (hereafter BSP), Niels Bohr Library, American Institute of Physics (hereafter AIP), College Park, MD.

precarious employment futures or who found military-funded university research opportunities ethically unpalatable in the context of the war in Vietnam.

The presidents of the APS from 1966 through 1972—John Wheeler, Charles Townes, John Bardeen, Luis Alvarez, Edward Purcell, Robert Serber, and Philip Morse—were brilliant men who had all worked on military projects during World War II: on the atomic bomb, radar, mine detection, and even nascent operations research. But the implicit Manichean moral terms that had facilitated the total research mobilization of those days no longer applied. From the late 1960s into the early 1970s, the APS leadership confronted a youth-driven, politically radical reform movement determined to shift the priorities of scientific research away from military applications and toward ending U.S. involvement in Vietnam.

Among the leaders of this effort was Berkeley physicist Charles Schwartz. Schwartz, like many of his physics peers, had been born in Brooklyn in the 1930s to parents with strong ties to Eastern European Jewry. His father, a Russian immigrant with little formal education, found enough work as a photographer and amateur inventor to support the family and eventually relocate to Connecticut. From there the younger Schwartz made his way to MIT, where he received first his BS and then his PhD in physics. As a graduate student, he worked in the field of nuclear structure with his advisor, Victor Weisskopf, whom he idolized (even going as far as to briefly affect a German accent). Despite MIT's strong military connections, he adopted the department's general disdain for applied work and, upon graduating, headed off to a short stint as an assistant professor at Stanford before landing at Berkeley in 1960.

Professionally, Schwartz dutifully continued his physics research, gradually specializing in complicated problems of particle systems and field theory. He held an Air Force research grant that paid his summer salary and supported his graduate students, and it was through this grant that Kenneth Watson identified him as a promising young candidate to work with IDA's Jason group, inviting him to the 1962 summer session. Schwartz absorbed the glamour and "heady" sense of his potential influence as a government advisor, and even after IDA rejected him for future participation, he maintained a kind of Washington "world view," deeply rooted in principles of gradual escalation and a technical, problem-solving approach to world conflicts.

In the summer of 1966, Schwartz's brother was killed in an airplane crash. He was devastated—emotionally and intellectually—and, as he put it, the experience "provided a deep psychological emotional opening" through which new philosophical and political attitudes suddenly flooded. His mild criticism of the war hardened into opposition: he suddenly noticed and began signing the anti-Vietnam petitions circulating among his colleagues; he withheld taxes designated for war funding; he wrote letters, joined peace groups, protested outside the Oakland induction center, and immersed himself in grassroots political organizing.[2]

The following year, Schwartz submitted a letter to the editor of *Physics Today,* recruiting physicists to express opposition to the war. Citing its failure to address "physics as physics and physicists as physicists," the editors rejected the letter for publication.[3] Schwartz interpreted the action as a kind of political censorship. Drawing on his newfound political skills, he responded by circulating a petition to amend the Physical Society's constitution, so that with the support of at least 1% of the membership, any "matter of concern to the society" could be brought before the entire organization for a vote. The results of the vote, if publicized, would constitute an

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[2] Interview of Charles Schwartz by Finn Aaserud on 5 May 1987, Niels Bohr Library & Archives, AIP, http://www.aip.org/history/ohilist/5053.html (hereafter Schwartz interview transcript).

[3] *Physics Today* clipping, December 1967; Box 3, Folder 1; BSP.

official organizational statement—potentially concerning matters of politics or U.S. foreign policy.

Though Schwartz gathered only 248 signatures in an organization of over 24,000, his actions provoked a firestorm of arguments and angry letters sent to the APS's top officers, a tempest in the APS teapot that drew outside criticism and even press coverage. As the letters poured in, the editors who had initially turned down Schwartz's Vietnam request found themselves devoting unprecedented time and space to debates over the political implications of the proposed amendment. Responses tended to fall into one of three categories. As the editors themselves summarized, most advocates of the reform believed that "Physics is already deeply involved in the lives of nations, and its discoveries have broad technological and social implications in both peace and war. If physicists as a group can clarify a problem or suggest a solution not clear to others, they should have the means to do so."[4] As Victor Paschkis and others had a decade earlier, these scientists sometimes invoked the specter of Nazi complicity: "German science remained pure and unpolitical during the 1930s. It has not yet recovered," wrote C.H. Blanchard of the University of Wisconsin.[5]

In other letters, however, opponents worried about "the danger of ineptitude and arrogance." The *Physics Today* editors summed up the views of these critics:

> [They argue that] the essential nature of physics is that it deals with simplified models of simple systems (point masses, hydrogen atoms, the compound nucleus) and seeks solutions where such solutions are available. People trained to deal with such models may not have any special skill in dealing with more complex systems like the human mind and international politics. They might offer a lot of bad advice. Public assertion that physicists have special competence might be an

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[4] "Physicists and Public Policy," *Physics Today,* December 1967, 128; Box 3, Folder 1; BSP.

[5] C.H. Blanchard, "Letters to the Editor," *Physics Today,* April 1968.

> arrogance that would bring discredit to a community in place of the respect it now enjoys.[6]

Rather than focus on the special *moral obligation* of physicists to address the social consequences of their work, these criticisms emphasized the potential *intellectual unsuitability* of physicists for just such a task.

Finally, a third group worried about the administrative consequences of a policy shift: for example, would the publicity of physicists' political views affect federal funding decisions for major projects? Would politicization sabotage physicists' precious permission to travel and convene with counterparts in the Soviet Union and other nations? Could the APS lose its tax-exempt status? Would the addition of political issues detract from the organization's ability to promote scientific objectives? After distilling and summarizing these arguments, the editors sided with the third group and endorsed alternate modes for physicists' political expression: for example, through advocacy work with the Federation of American Scientists or publication in the *Bulletin of the Atomic Scientists*. The editors warned that members who wanted to change the function of the APS "should look carefully at what they stand to lose while they are seeking routes to what they hope to gain."

After this introductory synthesis, the editors opened the letters page of the journal for reactions, vowing to print all or part of every submission. From January to April, 1968, screeds and diatribes and lamentations filled the section, from the most prominent physicists in the nation to concerned non-scientist readers. At a rate of more than four to one, they vehemently opposed the amendment. Eugene Wigner of Princeton wrote that changing the APS would be a "corruption of democracy."[7] Frederick Seitz, former head of the National Academy of Sciences,

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[7] Eugene Wigner, "Letters to the Editor," *Physics Today,* December 1967, 69.

worried that adding political discussion to the organization's journals would damage their "essentially professional character."[8] Edward Teller responded to the problem of physicists' suitability for political pronouncements by arguing that the APS ought to maintain "a proper division of authority," preserving the relegation of "public issues" to "traditional channels." Teller acknowledged one exception, however: a case in which "a question of fact (rather than of opinion) on which all reputable physicists agree on a professional basis and concerning which there is a public misunderstanding." (Just such a scenario would later arise in the form of the Strategic Defense Initiative debate, although in that case, the great majority of "reputable physicists" would take positions opposite from Teller.) Teller himself had advised military and government leaders about weapons technology and policy, but he didn't believe that the APS as an organization ought to act in a comparable manner.[9]

Most opponents, however, followed Seitz's lead, warning of the damaging effects of politics on the purity and professionalism of their organization. They worried that change would "jeopardize the purely scientific nature of APS and the harmony between its members," and urged their peers not to "dilute our professional efforts by becoming a debating society" or "contaminate physics with politics." David McCall of Bell Labs summed up this criticism in a simple declarative sentence: "The APS is not a political instrument."[10]

Nearly two decades later, Charles Schwartz would dismiss the critics who called for the preservation of political neutrality by noting that:

> Physicists know which side their bread is buttered on. … even those who have… liberal views on these things understand we're all part of that arrangement, and

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[8] Frederick Seitz, "Letters to the Editor," *Physics Today,* January 1968, 17.

[9] Edward Teller, "Letters to the Editor," *Physics Today,* January 1968, 17-18.

[10] Goetz Oertel and others, "Letters to the Editor," *Physics Today,* February 1968.

> you don't rock the boat. You don't do things that might offend the powers that be. So you claim to be neutral and apolitical and resist any attempts that might put you in a position where you might encourage the disfavor of important people…[11]

Not all of Schwartz's critics were Pentagon beneficiaries or timid liberals, however. Eugene Saletan, a young Northeastern University professor, took pains to note in his letter to the journal that he was as radical as Schwartz when it came to political activism, but he nevertheless saw the APS as a poor forum for outside issues. He wrote:

> …To establish my credentials, I am an adamant "extremist" on the war in Vietnam. I have demonstrated against the administration's policy, have spoken at public meetings against it, have signed resist petitions, am faculty advisor to SDS, etc. etc. Nevertheless I believe that APS should remain pure. There should be an organization of physicists whose purpose involves only physics. We have many opportunities to make public statements as physicists, and we can organize groups for such purposes. I would strongly favor such organization. But as physicists we also need a purely professional society, one to which politics and questions of power are irrelevant. Let us join together, those of us who want, to put an end to madness, but let us not do this within APS.
> Eugene J. Saletan
> Northeastern University[12]

Saletan's entreaty that the APS "should remain pure" assumed that the society's reluctance to issue political statements constituted a kind of neutrality, one which member-driven efforts to force votes on "matters of concern" would undermine. But was the APS "pure"? And what exactly were the "matters of concern" that might be raised? The amendment itself had yet to be passed and interpreted through practice, and the debate among physicists suggested a wide range of fears and assumptions. Jay Orear, the 42-year-old president of the Federation of American Scientists, considered the "matter of concern" clause to be a "safeguard," noting in a May, 1968 interview that "If 1% of the membership proposed a resolution outright condemning the Vietnam war, the APS council would have to rule that out of order as inconsistent with the

[11] Schwartz interview transcript.

[12] Eugene Saletan, "Letters to the Editor," *Physics Today,* February 1968.

purpose of the society. I consider that a quite adequate safeguard…"[13] But Schwartz himself disagreed. He was convinced that the APS was *not* a neutral organization, and his concern that a double standard existed in its political activities had fueled his amendment drive. As an example, he cited APS's invitation to Lyndon Johnson to attend an annual meeting and the subsequent publication of a photograph of the president with APS head Charles Townes. That, in his view, constituted "a legitimization of his Vietnam policies" by the APS membership.[14] If such legitimization was allowed, then surely APS members should be able to register their opposition as well.

Future Nobel Prize winner Martin Perl of Stanford offered a related analysis in the March issue, tackling the problem of Vietnam specifically:

> The great problem of those who are opposed to the war in Vietnam is to obtain adequate expression of this view through the *regular* and *established* methods of communications. This includes the platforms of political parties, the public speeches or remarks of people in politics and government, the publications and meetings of labor unions, professional societies and learned societies, and radio and television. Thus while almost half the people in this country are opposed to the war, the expression of this opposition is comparatively small.
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> The reasons for this problem are multiple. Certainly one of the reasons is that the almost automatic reaction of most *regular* and *established* methods of communication to issues like the war is to try to ignore these issues if possible. … The crucial thing about that silence is that it is *not* nonpartisan. No matter how one justifies or defends silence and no matter what sound reasons there are for silence, the result of silence is clear. Silence supports the war, and that is a sad silence.[15]

In the final tally, APS members voted against the amendment by a margin of three to one, and such a sound defeat mooted the question of practical interpretation. But perhaps more tellingly, out of an organization with over 24,000 members, not known for its political activity,

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[13] Clipping, *Physics Today*, May 1968; Box 4, Folder 7; BSP.

[14] Schwartz interview transcript.

[15] Martin Perl, "Letters to the Editor," *Physics Today*, March 1968, 9.

roughly half had participated in the amendment vote. The question posed by F. Jona of IBM's Watson Research Center was left unanswered: "Schwartz and his friends… would like to see APS officially oppose the Administration policy in Vietnam. Did it ever occur to Schwartz that APS might decide officially to *support* that policy?"[16]

Several years after the vote, Carleton College physicist Barry Casper, newly elected chair of the newly created APS Forum on Physics and Society, reflected on the legacy of the failed effort: "the Schwartz amendment had an effect that endured long beyond its defeat. The debate over the amendment had raised broader questions about the responsibilities of the APS to its membership and to society."[17] Schwartz's efforts had had dramatic short term effects as well. At the 1969 APS annual meeting, an inspired coterie of young physicists called for the establishment of a new internal division devoted to the discussion of physics and society, an effort that would culminate in the creation, three years later, of Casper's Forum. Meanwhile, Schwartz and Martin Perl, stymied in their internal reform efforts, proposed the creation of a radical new organization, Scientists for Social and Political Action (SSPA), wholly independent from the APS. Schwartz's failed amendment thus sparked two significant political drives: one to create an internal mechanism for the airing of political issues within the APS, and one to establish an outside body devoted to explicit political activism.

The impetus for the Forum sprang from the minds of two young physicists from MIT's Francis Bitter Laboratory, Brian Schwartz and Emanuel Maxwell, who led the drive to create an

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[16] F. Jona, *ibid.*

[17] Barry Casper, "Physicists and Social Responsibility: A New Role for the APS," 1973-1974; Box 5, Folder 3; BSP (hereafter Casper).

APS division concerned with "the problems of physics and society." At the 1969 annual meeting, held in the shadow of the war in Vietnam and Richard Nixon's inauguration, in which large contingents of attendees wore anti-ABM pins, and where Kurt Vonnegut warned the American Association of Physics Teachers that "The virtuous physicist is one who does not work on weapons," the pair found a ready audience of supporters.[18] Their petition quickly accumulated over five hundred signatures, enough to alert the APS's Executive Council and force a response.

Unlike the amendment proposed by Charles Schwartz, the new division would not require APS to issue any formal political pronouncements or subject the entire membership to referenda. Rather, the internal group would not be political in any active sense, focusing instead on helping concerned physicists "clarify their own ideas."[19] This moderate mission found grudging support in Edward Purcell and the APS Council, who eventually accepted the establishment of a Committee on Problems of Physics and Society, precursor to the APS Forum on Physics and Society.

But while the Forum's approval process dragged on over the next three years, Charles Schwartz and Martin Perl charted a quicker and more activist course with SSPA. Announcing their inaugural meeting at the APS conference, they joined with Michael Goldhaber of Rockefeller University (son of Brookhaven head Maurice Goldhaber) and Marc Ross of the University of Michigan to publicize a new "independent body of socially aware scientists free from the inhibitions which abound in the established institutions." They charged that the APS, and scientists' professional societies in general, had "deliberately remained aloof from the desperate problems facing mankind" by promoting the simplistic mantra that "research means

[18] Vonnegut quoted in *Electronic News* clipping; 2 October 1969; Box 4, Folder 7; BSP.

[19] Brian Schwartz and Emanuel Maxwell, "Letter to the Editor," *Scientific Research*, 12 March 1969; Box 1, Folder 1; BSP.

progress and progress is good." In SSPA's view, such an attitude encouraged young scientists to pursue weapons research without contemplating the moral and social consequences of their work.[20] The time was ripe for a new organization.

In response to the call, three hundred scientists showed up for a meeting held in a Hilton hotel room, "a broad coalition," in Casper's words, "that included arms controllers and environmentalists, liberals and radicals." An elated Martin Perl proclaimed that the youthful new organization would shake up the elderly APS Executive Council, and would force the old guard to "to come out of their ivory towers and bomb shelters." But exactly how, and to what end, remained unclear. As Charles Schwartz later recalled, "Marty Perl [gave] the first speech in which he made it very clear that this was not going to be a radical organization. And then I gave the second speech in which I said in my opinion this was going to be a radical organization. … It was designed as very much an unorganized organization." The first meeting covered a wide range of topics, from opposition to the anti-ballistic missile system to the possibility of a research strike at MIT. [21]

Though critics such as Lee DuBridge, Nixon's science advisor, and John Bardeen, two-time Nobelist and APS president, dismissed the protesters as extremists with little influence, the SSPA faction managed to attract coverage in a broad spectrum of print media, from *Electronic News* to the *New York Times.* In its first two years of existence, SSPA opened also its ranks to biologists, chemists, mathematicians, and engineers, eventually changing its name to the more inclusive Scientists and Engineers for Social and Political Action (SESPA). Members led the charge against Jason scientists employed on their campuses; campaigned to prevent future APS

[20] SSPA Flyer; Box 4, Folder 7; BSP.

[21] Casper; AP clipping; Box 4, Folder 7; BSP; Schwartz interview transcript.

meetings from taking place in Chicago, home of the violent 1968 Democratic Convention; lobbied against the anti-ballistic missile system; called for a boycott of Los Alamos and Livermore scientists; organized a non-participation pledge for war research; promoted a Hippocratic oath for scientists; and oversaw another unsuccessful amendment attempt, this time to include stronger moral language in the APS constitution. Specifically, they demanded that:

> The object of the Society shall be the advancement and diffusion of the knowledge of physics in order to increase man's understanding of nature and to contribute to the enhancement of the quality of life for all people. The Society shall assist its members in the pursuit of these humane goals and it shall shun those activities which are judged to contribute harmfully to the welfare of mankind.[22]

With the narrow defeat of this measure, Charles Schwartz and his supporters had once again demanded an accounting of the social consequences of research and failed. And with their failure, the broad coalition within SESPA began to fracture. In Boston, the local SESPA chapter volunteered to publish the organization's newsletter, previously the province of Martin Perl. As Charles Schwartz observed, the Boston group was "quite active," and the change in the newsletter "signified a transformation to a much more radical perspective and posture."[23] The SESPA newsletter became the radical magazine *Science for the People*, whose very logo—a red, clenched fist and a test tube—proved sufficiently radical to alienate Perl and other liberal group members. Early editions published exposes of Polaroid's involvement in South Africa, the work of Jason members at Columbia University, and cases of discrimination at the University of Massachusetts. Typical article titles included "Engineers in the Working Class," "Fighting the Police Computer System," "The Social Impact of Modern Biology," and "People's Science

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[22] Casper. Late in his life, Hans Bethe would publicly endorse the idea of a Hippocratic oath as well. See S.S. Schweber, *In the Shadow of the Bomb: Oppenheimer, Bethe, and the Moral Responsibility of the Scientists* (Princeton, NJ: Princeton University Press, 2000), 171.

[23] Schwartz interview transcript.

Projects for Vietnam," which called for medical and agronomic assistance to heal the ravages wrought by the U.S. military.

Frustrated by the radical turn of SESPA, some liberal APS members, such as Martin Perl and Brian Schwartz, turned their attention to the nascent Forum. Though Charles Schwartz and others complained that the Forum amounted to "cooptation" and "a way to get all those people who are concerned…under the control of the establishment,"[24] disgruntled moderates nevertheless found it a more appealing alternative to SESPA. By 1972, the Forum had roughly 2000 members and an annual budget of $4,200.

The aims of the Forum were indeed more limited than SESPA's mass boycotts and deep institutional reform efforts. Forum-sponsored panels and discussions nevertheless brought the problems of anti-ballistic missile systems, energy policy, international conditions for science, and even the work of the Jasons to a wider APS audience. The Forum also sponsored a Congressional Fellowship program, allowing two physicists a year to work on a Congressional staff. But throughout, the executive committee of the APS exercised subtle oversight, applying behind-the-scenes pressures and limits on Forum activity.

One notable example of the tensions between the executive committee and the Forum occurred during preparations for a Forum symposium on Physicists and Public Affairs, to be held in April of 1972 and moderated by Jay Orear, president of the Federation of American Scientists. Planned speakers included Cornell high energy physicist Raphael Littauer, discussing the Cornell Air War Study and physicists' potential contributions to political science; Congressional aide Leonard Rodberg, describing his work with Sen. Mike Gravel in the publication of the Pentagon

[24] Schwartz interview transcript.

Papers; Stanford physicist Pierre Noyes, detailing ways to use the legal system to oppose war; and, most controversially, William Davidon, a pacifist mathematician and physicist from Haverford College, speaking on the social responsibility of scientists.

Davidon had begun his career as the director of research at the Nuclear Instrument and Chemical Corporation in Chicago after World War II, before enrolling in the graduate physics program at the University of Chicago, where he was a star student. After a short stint at the Argonne National Laboratory, he settled into a long career at Haverford College in 1961. The Quaker-influenced school was a near-perfect fit. Davidon had long been active in peace-seeking scientists' organizations: he supported the Pugwash Conference, the Federation of American Scientists, and the Society for Social Responsibility in Science. During the 1960s, however, his opposition to the war in Vietnam spurred him to more drastic acts of protest and disobedience, particularly after an eye-opening trip to South Vietnam in 1966, sponsored by the Committee for Nonviolent Action. Described by one ally as "someone with a knowledge of the scene, a keen sense for tactic & detail & little fear of risk for himself," he refused to pay taxes earmarked for war funding, joined students in destroying records at the Georgetown draft board, and, in 1971, assisted Daniel and Philip Berrigan and a small cadre of other activists "in a conspiracy to blow up heating systems in Federal buildings and kidnap [Presidential Advisor] Henry Kissinger."[25] Davidon ultimately escaped prosecution for this last act as an "unindicted co-conspirator," and the remaining defendants in the "Harrisburg" group were never convicted. (In an unusual meeting during the spring of 1971, however, Davidon and two fellow co-conspirators, antiwar activist Thomas Davidson and Notre Dame Sister Beverly Bell, actually spoke with Kissinger in the Situation Room of the White House.)

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[25] *New York Times* 1 May 1971; *New York Times* 13 March 1971.

A year after the conspiracy, Davidon's scheduled Forum talk focused on the logic and ethics that had propelled his initial commitment to sabotage and property destruction. In Davidon's view, destroying draft records had been a means both to protest the war and "protect and build respect for life." In the abstract of his talk submitted to the APS *Bulletin*, Davidon argued that U.S. reliance on "technologically advanced weapons systems" in southeast Asia meant that "Scientific and technical workers are replacing the foot soldier." This left "scientists, engineers, technicians, and those who teach them" with the obligation to prevent the misuse of technology and, most crucially, to resist and impede the war effort. This resistance might take the form of publicizing the effects of deploying new weapons systems or, more radically, "Inactivating equipment intended for killing or harming people."[26]

Davidon's call for sabotage deeply distressed the executive council of the APS, who refused to print the abstract of the talk in the APS *Bulletin*, as was customary. American University's Earl Callen, president of the Forum, warned that suppressing the abstract would prove more harmful to the APS's reputation than publishing it. As he emphasized, Davidon was only *advocating* sabotage, after all, not *inciting* it. Callen believed that Davidon should be allowed to present his talk, and APS members could let the "Darwinian process of natural selection—of ideas, of speakers, of session chairmen, and APS Divisions" operate.[27] Jay Orear agreed. Though the panel itself hardly reflected a broad spectrum of viewpoints, the council's action still constituted "a clear case of censorship": the Forum had an obligation to present

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[26] William Davidon, "Significance of the Harrisburg Trial for Scientific Workers" conference abstract; Box 1, Folder 7; BSP.

[27] Callen to Havens; 22 February 1972; Box 1, Folder 6; BSP.

Davidon's view of the misuse of science, even if advocates of the opposing viewpoint (such as John Foster) had turned down invitations to participate in the Forum panel in the first place.[28]

The decision to withhold an abstract from the APS *Bulletin* might seem a trivial affair in the grand scheme of militarized science, the war in Vietnam, and campus protests, but, like Charles Schwartz's amendment attempt, it required another APS self-evaluation, and offered another opportunity for critics to voice their dissatisfaction. In the case of Davidon's abstract, the most vociferous critic of APS policy was Stanford physicist and fellow panel member Pierre Noyes, who personally took it upon himself to photocopy scores of abstracts to distribute at the meeting, and who further declared his opposition in a press release the day of the conference.

Noyes himself had traveled a great ideological distance to reach the day's panel discussion: after receiving physics degrees from Harvard and Berkeley he had spent much of the Eisenhower years as a group leader at the Lawrence Livermore Laboratory, as a consultant for Project Orion, and as an AVCO visiting professor at Cornell. But he had been outraged at the conduct of the war in southeast Asia. In 1970, he had burned his bridges to military science and brought a class-action lawsuit against the Nixon administration, citing the use of taxpayer dollars for actions violating international war crimes agreements and the U.S. Constitution. Now, in his talk and his press conference, Noyes drew heavily on the example of Nazi collaboration and the lessons of the Nuremburg trials. In his panel presentation, he warned his audience:

> Once it is clear that our Government is committing war crimes, our responsibility under the Nuremberg precedents is also clear: we must refuse all cooperation with any organization engaged in these criminal activities whenever we have a reasonable moral choice open to us. Those of us in policy-making positions within government have the further obligation either to force a change in policy or publicly resign in protest.
>
> Failure to do so is a war crime. This was the ruling in the case of the Japanese cabinet, even for department heads who had nothing to do with the war.

[28] Orear to Callen; 28 March 1972; Box 1, folder 6; BSP

> Nazi judges who enforced racial laws legally passed within the German system were convicted at Nuremberg. Those of us who receive rather than give orders are still under the legal obligation not to carry out criminal acts. **A soldier with a gun held to his head who obeys an order to kill or torture a prisoner is still guilty of a criminal act. The fact that he had no reasonable moral choice but to obey may be properly argued in mitigation of sentence, but cannot alter the fact of his guilt. In contrast, the extent of the guilt of the citizen in a war-related job who fails to search diligently for alternative employment is not an easy *legal* question to answer, but the moral imperative of the Nuremberg precedents is unambiguous.** …"[29] [emphasis added]

Noyes reiterated this analysis in his press conference, now including the APS and its claims of neutrality in his distributed text:

> It is sometimes objected that, as physicists, we have no business acting on these issues, or even discussing them. That was presumably the rationale behind the arbitrary decision of the Executive Committee of our Society to refuse to publish the abstracts for these four talks, in violation of precedent. But as physicists we train in our universities the men who make the illegal and inhumane weapons used in Southeast Asia, and the technology which insures their delivery. We perpetuate the myth that science and technology are neutral, and only people evil, rather than training our students to humane traditions. We welcome to our professional meetings representatives of military laboratories to discuss the unclassified spinoff from their labors, and to recruit for their illicit activities. We have come down heavily on the wrong side, and it is past time to redress the balance. …[30]

A year earlier, Noyes had offered a similarly-themed speech at the annual Banquet of the American Physical Society in Washington, D.C., an event covered by the *Washington Post.* Warning that "the moral imperative is unambiguous," he exhorted his audience—which included Edward David, Nixon's science advisor—to "use every means at our disposal, not only to end this war, but also to end those aspects of our institutions which made it possible." He demanded David's public resignation. The *Post* noted that the "conservatively-dressed" Noyes spoke "with cold heat" and intellectual rigor, in a "style [that] goes far past the shouting, slogan-rich radical

[29] Pierre Noyes, APS paper; Box 1, Folder 7; BSP.

[30] Pierre Noyes, press conference text; 25 April 1972; Box 1, Folder 7; BSP.

style of the late 1960s." Unlike the public disdain heaped upon an angry young microbiologist who had earlier interrupted the proceedings, Noyes "was listened to by all and was applauded by many." Noyes's status as a "shirt-and-tie" radical, with impeccable physics credentials, warded off trivialization and easy dismissal.[31]

Physicists were not the only scientists grappling with problems of professional neutrality. New organizations and accompanying publications were springing up everywhere among science and technology professionals. The Computer People for Peace exhorted fellow programmers to "Join with other workers to make computers serve the people!" and, in addition to their journal *Interrupt*, published pamphlets on data banks and repression, computer technology and warfare, and other controversial topics. Alan McConnell, a University of Illinois math professor, founded *MAG,* a periodical designed to "make the American Mathematical Society, and math community in general, more socially aware and responsible."[32] Even the scientists at Brookhaven Laboratory set up an underground "free press" and radical newsletter.

In New York, area engineers from academia and industry joined together to form the Committee for Social Responsibility in Engineering (CSRE), proclaiming that "Thousands of engineers feel that their engineering talents are misused in both civilian and military projects, and believe that the constant development of weapons technology spells ultimate disaster for mankind."[33] CSRE members marched on Washington in April of 1971 (under an "Engineers for Peace" banner) and held a "counter-conference" during the 1971 IEEE national convention in

[31] Victor Cohn, "Scientists Face Their Frankensteins," *Washington Post,* 27 June 1971

[32] Advertisement, *Science for the People,* May 1971.

[33] "CSRE Statement of Purpose," *Spark* 1, no. 2 (Fall 1971); Box 5, Folder 3; BSP. For more on CSRE see Matthew Wisnioski, "Engineers and the Intellectual Crisis of Technology, 1957-1973," (Ph.D. diss., Princeton University, 2005).

New York City, where they protested the invitation of Assistant Secretary of Defense David Packard, whom they called "Mr. Military Industrial Complex." The counter-conference itself drew high-profile attendees and speakers. Ed Koch, then a New York Congressman, attended the opening press conference; Seymour Melman lectured on conversion to a peacetime economy; Jeremy Stone of the Federation of American Scientists discussed the importance of expert testimony before Congress; and Victor Paschkis offered an environmentalism-tinged lecture on predicting "secondary effects" of new technologies.[34] Haverford's controversial William Davidon also made an appearance.

From 1969 through 1972, protests and confrontations disrupted the national meetings of nearly all the major scientific professional societies, taking a wide variety of forms: from impassioned interruptions of speeches to carefully calculated invocations of Robert's Rules of Order. In some cases, concerns about job security, dire employment conditions, and other labor problems comprised the major motivations for reform efforts, as with the American Chemical Society, but mostly, it was anger at U.S. foreign policy and the perceived complicity of scientists and engineers that fueled scientists' protests.[35] As had been true of the discontented physicists in the APS, manifestations of that anger ranged from moderate calls for reform in traditional venues to choreographed disruptions and street theater.

Many of these tensions came to a head during the notorious 1970 national meeting of the American Association for the Advancement of Science (AAAS), the largest professional society for scientists and the publisher of *Science.* The most detailed account of the meeting's

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[34] *Spark* 1, no. 2 (Fall 1971); Box 5, Folder 3; BSP.

[35] Robert Gillette, "ACS: Disgruntled Chemists Seek New Activist from Old Society," *Science* 173, no. 4003 (24 September 1971), 1218-1220.

proceedings appeared in the January 8, 1971 issue of *Nature,* in the tellingly titled "Dissent Blooms at AAAS Circus," under the byline "by our Washington Correspondent." The author, identified only as "Washington Correspondent," described the meeting as a blend of responsible, methodical science activism, embodied by the efforts of Matthew Meselson and Stewart Udall, and "noisy," "theatrical" stunts choreographed by SESPA and other groups. In the category of responsible activism, he was particularly impressed that mere days before Meselson's AAAS group was to present its final report on the consequences of herbicide use in Vietnam, Nixon's Defense Department had agreed to stop the spraying. The "Washington Correspondent" compared Meselson to Ralph Nader, noting that he, too, had "forced a giant corporation to change its policy."

Also praised was the heartfelt speech on the political and moral obligations of the scientific community delivered by Udall, the former Secretary of the Interior. Udall's words were harsh, but his delivery sober and respectful. He deeply criticized the attitude of organizations like the APS, which sought only to offer technical, but not political advice. To Udall, this made scientists into "political eunuchs—mere technicians detached from a value system and its attendant 'political' judgments." The National Academy of Sciences was no better:

> …by confining itself to a clientele almost exclusively made up of government agencies, and by permitting its clients to phrase the questions it will study, has all too often become a mere adjunct of established institutions. … There are good reasons if some segments of youth are disillusioned with science in the 1970s. While courageous individuals in the scientific community were raising the alarm about the lethal threat of chemical and biological warfare, what was the academy doing? It was working under contract to the Defense Department to select bright young scientists to work in the Defense Department's Chemical and Biological Weapons Centre.

Nature characterized Udall's speech as more serious, and more seriously delivered, than the boisterous SESPA theatrics, which came largely in reaction to a volatile session entitled: "Is There a Generation Gap in Science?" featuring presentations from "two venerable Hungarians": physicist Edward Teller and physiologist Albert Szent-Gyorgyi. Moderated by Margaret Mead, the panel also included Richard Novick of the Public Health Research Institute, *New York Times* science writer Nancy Hicks, and Harvard freshman Stuart Newman. To a packed room filled with students, activists, and television crews, Gyorgyi opened by acknowledging that the application of scientific research to weapons development justified popular antipathy for science: "Because science is used for war, we have lost the respect for the people and there is a revulsion against scientists." Referring to the deadly destruction of the science laboratory building at the University of Wisconsin, he noted, "The bombs dropped in Vietnam make bombs go off in Wisconsin." Gyorgi's critical assessment was easily overshadowed by the appearance of Teller, however, whose mere presence seemed to offer an unapologetic defense of weapons science, and who provoked outrage by arriving flanked by a bevy of bodyguards. As SESPA members waved signs labeling him a war criminal, Teller denounced the protesters as thoughtless and unreasonable, likening them to the Nazi thugs who had persecuted him in his youth. According to *Nature*, Teller's invocation of Hitler temporarily quieted much of the audience. Nevertheless, disruptions began anew when Richard Novick, the third speaker, offered Teller the "Dr. Strangelove Award," a Nazi-evoking trophy displaying a police officer in the act of shooting, captioned "I am just following orders." Novick sided with Gyorgi and called for scientists to create a new form of labor union in order to exert control over the applications of their research. He explained:

> It seems to me that if we as scientific workers were to organize ourselves along classical lines we could be an effective social force for the public good—could

> use our knowledge to support demands that the fruits of our research be used to serve society as a whole… By such actions it may still be possible without a bloody revolution to radically reorganize this society so that it serves the people and is therefore unable to misuse our science.[36]

Of the coverage of the conference in the mainstream press, *Nature* offered by far the most sympathetic account, painting SESPA as a cheerfully disorganized group whose outbursts may have alienated many conference attendees, but who succeeded in making the general point that "science is political." In *Science*, for example, Philip Abelson complained that the SESPA radicals had succeeded in disrupting enough meeting activities "to tarnish the image of the AAAS," but had failed to win over the vast majority of actual attendees. Abelson, like Teller, invoked the specter of Nazi stormtroopers, quoting a similarly themed *Washington Post* complaint.[37]

SESPA itself offered some self-critical reflections in a post-mortem published two months later in *Science for the People*. In retrospect, the heckling of Teller had been "in good fun" and effective as "ridicule," but

> the moralistic tone of the Strangelove award helps us not at all to understand Teller as a product of society, as an exaggerated example of what so many of us and our colleagues are in part or might be. It provides no basis for scientists to immunize themselves against the appeal of Teller's attractive personality or his obvious capability as a physicist…[38]

SESPA concluded that the attack on Teller, like Charles Schwartz's attempt to promote a Hippocratic oath for scientists at Berkeley, did not address the potential of researchers to become unknowing or even unwilling collaborators in military projects. Charles Schwartz himself had related a cautionary tale to the *Washington Post* in 1970, in the aftermath of the implementation

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[36] "Dissent Blooms at AAAS Circus," *Nature* 229 (8 January 1971), 81-82; Box 5, Folder 1; BSP.

[37] Philip H. Abelson, "The Chicago Meeting," *Science*, 22 January 1971.

[38] "1970 Chicago AAAS Actions: Review and Critique," *Science for the People* (February 1971), 8-11.

of the Mansfield Amendment, which required that all military-sponsored research have clear defense applications. Schwartz, whose work had been supported by the Air Force, inquired as to the justification for funding his ‘entirely theoretical, non-secret work on the structure of the atom.’ He had been told, he reported to the *Post*, that he could “rest assured” that his work was “vital to the aerospace mission,” but his funders could not be more specific for security reasons.[39] He promptly terminated his contract. SESPA’s analysis—that it was insufficient to judge an individual scientist’s personal actions instead of the larger system in which he operated—echoed the arguments taking place on university campuses across the country. Students chastised professors for working on war-related scientific research, and administrations for accepting millions of dollars of funding from the Defense Department and other military sources. As the leaders of the APS had been made to do, moderate and liberal faculties and administrations found themselves confronting the meaning of neutrality, academic freedom, and autonomy in the context of anti-war anger and deeply unpopular government sponsorship.

Neutrality, Thomas Kuhn, and Research at Princeton University

On a Thursday night in late April, 1970, President Nixon announced on television what pundits and reporters had long been speculating—that the United States planned to expand the war into Cambodia, and in fact would be sending in combat troops within the week. Clandestine aerial bombing of the region had been U.S. policy for several years, culminating in the famed “Operation Menu” secret bombings begun in the spring of 1969, but this night offered the first official announcement of U.S. incursions into Cambodia. Nixon openly acknowledged the imminent attacks designed “to clear out major enemy sanctuaries on the Cambodian-Vietnam

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[39] Victor Cohn, “Scientists Face Their Frankensteins,” *Washington Post,* 27 June 1971

border," but assured the viewing public that this was "not an invasion" and was part of his plan to deescalate and conclude the war.[40]

Reaction on the nation's campuses was swift and angry, most tragically on the Ohio campus of Kent State, a protest which left four dead and more injured. Among science students in particular, response varied greatly. *Chemical and Engineering News* reported that chemistry classes had gone on as usual on the campuses of Colorado State and the University of Nevada, while students in other departments demonstrated and engaged in property destruction. California was a different matter—Berkeley was closed by order of Ronald Reagan, a quarter of chemistry students at USC boycotted classes, and at Stanford, the overwhelming majority of members of the chemistry department—professors, researchers, and students alike—signed a resolution opposing Nixon's actions. The chair of Stanford's chemistry department, Harry Mosher, noted that such political activity among his peers had "never happened before." And at Caltech, science faculty and students publicly wrote antiwar letters to politicians and took out newspaper advertisements. Caltech chemist George Hammond, who vehemently condemned the Cambodian invasion at a campus rally, saw his recent appointment as the new deputy director of NSF revoked by the Nixon administration.[41]

At Princeton, thousands of students and faculty members reacted to the speech by gathering at the Princeton chapel for a "hastily called protest meeting" and, late in the night, voting to boycott classes. During the following week, SDS and other groups demanded an end to what they perceived as Princeton's complicity in the war effort, emphasizing weapons research

[40] *New York Times*, 1 May 1970

[41] Clippings, *Chemical &Engineering News* 1 June 1970 and 18 May 1970; Box 3, Folder 3; BSP.

on campus and school ties to the Institute for Defense Analyses, which was housed on Princeton property. As one campus leaflet summarized:

> Last Thursday night President Nixon announced the invasion of Cambodia, while the large-scale bombing of North Vietnam was secretly resumed the previous day by the Defense Department. In response to this, the Princeton community voted overwhelmingly to strike at a meeting Thursday night. Princeton University is not only a community of individuals, but also an institution. As an institution Princeton aids and abets the ever-expanding war in Southeast Asia by allowing ROTC, IDA, military recruiting, and war-related research to operate on campus. Therefore to remain consistent with the demand that Princeton University take a stand as an institution against the war in Southeast Asia the resolution that was passed Thursday night also demanded that the University sever its ties with the Department of Defense. ...[42]

Responding to student criticisms and their own concerns, on May 6 the Princeton faculty voted in favor of a systemic reckoning of war-related research at Princeton. Within a week, the Council of the Princeton University Community had taken up the faculty's resolution and formally called for a "special committee" to report back on Princeton's relationship with the Defense Department and the school's sponsored research policies. In particular, the Council set out two proposals for the new committee to evaluate:

> 1) That the University refuse to accept any outside funds for research on campus which is directly and specifically related to weapons and weapons systems.
> 2) That Congress be asked to channel all funds in support of research to universities through civilian departments and organizations such as H.E.W. and the N.S.F. rather than the Department of Defense.[43]

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[42] Leaflet "Why Strike? What Princeton Can Do as an Institution"; Special Committee on Sponsored Research Records (hereafter Kuhn Papers); Box 2 Folder 4; University Archives, Department of Rare Books and Special Collections, Princeton University Library.

[43] Council Resolution; 12 May 1970; Box 2 Folder 3; Kuhn Papers.

To accomplish this evaluation, the committee was to assess the cost, financial and otherwise, of purging Defense Department contracts; find ways to offset these losses; and reach out to other universities in order to promote new pathways of funding.

As plans for the new committee worked their way through the university's governing bodies in late May, the political climate at Princeton eased from tense to pragmatic. Students organized themselves into bands of political volunteers, most notably for the antiwar Congressional candidate Nicholas Lamont in Philadelphia, thus opting to work within, rather than challenge the legitimacy of, existing electoral channels. Noting the trend, the *New York Times* reported that for Princeton, 1970 was not to be "a year of revolution as 1969 had been Harvard's and 1968 Columbia's." The school had been "politicized by the Cambodian actions, not radicalized."[44]

The makeup of the committee reflected this critical moderation. Members included undergraduate and graduate students in both the sciences and the humanities, faculty members from multiple departments, and voteless representatives from the university administration. But the committee's most prominent public face was that of its Chairman, Thomas Kuhn, the well-regarded professor of Philosophy and History of Science. Kuhn had earned a PhD in theoretical physics from Harvard in 1949, then authored the influential *Structure of Scientific Revolutions* in 1962. He embodied the committee's mix of scientific expertise and social criticism.

As the committee began meeting in late May, it suddenly confronted the enormous complexity of its mission, which reached beyond mere financial analysis to deeper questions of ethics and Princeton's institutional philosophy. Should the committee worry only about research sponsored by the Defense Department, or any research funded by an outside source that might

[44] *New York Times*, 19 May 1970.

have military applications? What about basic research that might be useful in a particular field, but also spawn destructive new technologies? (Their debates echoed the question posed by hydrogen bomb theorist Stanislaw Ulam: "Even the simplest calculation in the purest mathematics can have terrible consequences. Without the invention of the infinitesimal calculus most of our technology would have been impossible. Should we say therefore that calculus is bad?"[45]). More fundamentally, was it absolutely wrong for weapons-related research to take place on campus? Did restrictions on weapons research restrict academic freedom? What was the purpose of a university in 1970s America?

Kuhn himself worried deeply about this last question. In June, he wrote a long personal letter to Charles Hitch, president of UC-Berkeley, requesting information about Berkeley's policies, and Hitch's general advice. He described the committee's work as "an attempt to make certain that the University has not, as an institution, involved itself so deeply with the federal government as to surrender an essential part of its traditional independence and assume functions inappropriate to its nature."[46] To acting Caltech president R.F. Christy, he reiterated this fear, wondering if "current modes of federal funding" might cause "distortions of academic research."[47] Such questions assumed an established role for a university: an impartial, independent institution providing fertile grounds for the advancement of knowledge, free from outside agendas and pressures.

But was that a valid assumption? Not everyone agreed. The critical view was probably best articulated by Martin Summerfield, a professor in the Aerospace and Mechanical

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[45] Stanislaw Ulam, *Adventures of a Mathematician* (New York: Scribner, 1976).

[46] Kuhn to Hitch; 12 June 1970; Box 5 Folder 3; Kuhn Papers.

[47] Kuhn to Christy; 2 July 1970; Box 5 Folder 4, Kuhn Papers.

Engineering Department, who wrote to committee members after an open meeting during the following fall. Irritated by assertions that Princeton's "institutional neutrality" precluded certain faculty activities, such as weapons-related research, government advising, and the maintenance and use of the classified library, he invoked the old concept of "Princeton in the Nation's Service." Was such a slogan still meaningful, he asked, and if so, did it not "imply the availability of [Princeton's] resources and talents" for government work?[48] Speaking for the engineering departments, he contended that "Most of us, as loyal citizens, will continue to work on defense problems." He reminded the committee of Princeton engineers' contributions during World War II, for which the department had received "the gratitude of our government and the public."

Summerfield also complained to committee members that research models touted by scholars in "contemplative" fields such as English or Math lacked relevance for engineering, an "activist field." An engineer, he argued, is "like a physician—he responds to visible needs," often in areas deemed important by government agencies and the private sector. "In your field of contemplative scholarship," he wrote to Kuhn, "you are privileged as individuals to set your own private goals. An engineer who tries to secure such privileges for himself is a misplaced individual; if he persists he will find himself on public welfare."[49] This oddly passive view of engineers—as heavily influenced by the priorities of outside funders—was echoed within the committee by Robert Jahn, a NASA-funded professor of Aerospace Science, who informed his colleagues that "the selection of research topics is influenced by the sponsor" in "any good

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[48] Summerfield to Kuhn; 13 November 1970; Box 1 Folder 2; Kuhn Papers.

[49] Summerfield to Fleming; 27 October 1970; Box 1 Folder 2; Kuhn Papers.

engineering school."[50]

As Kuhn and his counterparts at other universities weighed these types of deeper issues, the committee's research staff spent their first summer collecting information on every funded research project on campus, with special attention to projects flagged by critical students and those conducted by research groups in the Department of Aerospace and Mechanical Sciences (AMS), which seemed at first glance to have the most questionable projects under way. The staff also evaluated funding statistics, solicited information from dozens of other universities, arranged for an economic study of the financial consequences of severing Princeton's ties to the Defense Department, and reviewed dozens of statements from faculty members about their own research and their views on appropriate research policy.

The results nearly paralyzed the committee. Setting aside the issue of the Institute for Defense Analyses' presence in a Princeton-owned building and Princeton's maintenance of a classified library, the remaining problem of sponsored research proved far more ambiguous and complex than expected. For example, Professor William P. Jacob's botany research had been noisily targeted by campus critics, who had earlier distributed notices arguing that "the concept of 'neutral research' is a fallacy. One must question, for example, what future applications the Army has in mind when it allocates some $26,000 to study the 'Physiological Mechanism of Leaf Abscission and Senescence.'"[51] To critics, such a project could mean only one thing: defoliant research applicable to the war in Southeast Asia. Jacobs gamely defended himself in the *Daily Princetonian*, acknowledging Army support of his work, but characterizing it as "basic

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[50] Minutes; 22 September 1970; Box 2, Folder 7; Kuhn Papers. For more on the particularities of the problems for engineers posed by the political crises of the late 1960s, see Matt Wisnioski, "Inside 'The System': Engineers, Scientists, and the Boundaries of Social Protest in the Long 1960s," *History and Technology* 19, no. 4 (2003): 313-333 and Matthew Wisnioski, "Engineers and the Intellectual Crisis of Technology, 1957-1973," (Ph.D. diss., Princeton University, 2005).

[51] "Researching Research"; Box 4 Folder 2; Kuhn Papers.

research on the relations of hormones to ageing in leaves." The same projects had earlier been funded by the National Science Foundation, he argued, and "research support from the Army did not change the topics or course of the research in any way. … None of it was concerned with the practical problems of defoliation."[52]

The issue was not quite so straightforward, however. Earlier in the spring, Martin Summerfield had been informed by one of his own sponsors that Jacobs's work carried a "NOFORN restriction"—i.e., it was not to be released to foreign nationals. The irritated Summerfield accused Jacobs of damaging the fate of Defense Department research at Princeton, and demanded to know: "How can you defend to the Princeton community the motives of your research when you are willing to limit its distribution… Is this not self-evidence proof that your research is not really as open as you recently claimed in the Daily Prince?" His concern was not Jacobs's "motives," but rather the risk of "jeopardizing your defense in the recent public argument over your contract." Summerfield, himself a recipient of defense dollars, was "concerned about my own position as it might be affected by any defeat you suffer."[53] The military-funded Princeton researchers had to prove that their sponsored research was innocent, and Jacobs was sabotaging their case. A week later the beleaguered Jacobs sent copies of his correspondence with Summerfield to the Kuhn committee, noting simply that he hoped "that our D[efense Department] contracts cease, Congress be urged to give corresponding money to NSF, and that the University adapt to the much lower level of funding that will probably result."[54]

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[52] *Daily Princetonian* letter; 15 April 1970, in Kuhn Papers, Box 2 Folder 4

[53] Summerfield to Jacobs; 13 May 1970; Box 2 Folder 4; Kuhn Papers.

[54] Jacobs to Kuhn; 21 May 1970; Box 2 Folder 4; Kuhn Papers.

Princeton biologist A.J. Levine finally clarified the nature of Jacobs's research at a fall committee meeting. Sponsored most recently by the Army's Fort Detrick, Levine explained, Jacobs had merely been pursuing his fifteen-year interest in leaf senescence, which naturally included an analysis of a compound, 2-4-D, that had military defoliant applications. Levine assured the committee that Jacob's intrinsic interest had come first—and given that interest, use of 2-4-D was a scientifically appropriate area of exploration, no matter what the Army's priorities were. Besides, the compound was a "tool" rather than the "subject" of Levine's work.[55] This characterization added yet another difficult scenario for the committee members searching for an ethical policy: in one instance, they had engineers who argued that their field, by definition, relied on outside influences to set agendas; and on the other, they had the apparent case of a conveniently coincident agenda addressing one researcher's intrinsic interest and the needs of the U.S. Army. Apparently even the category of "basic research" contained gray areas. As Robert Frosch, Assistant Secretary of the Navy for Research and Development, had argued in a 1960 speech read by committee members, "pure" research and "basic" research were not indistinguishable. Pure research was defined *psychologically* as work that was just intrinsically interesting, while basic research implied work that advanced large areas of knowledge. More critically, Frosch approached the ethical dimensions of the research controversy by opining that whether work was "pure" or "impure" depended solely on the motivation of the researcher, and the only party responsible for immoral applications was the applier himself.[56] For Frosch, ethics was a matter solely for individuals. But the Kuhn Committee was searching for ethics on an institutional level.

[55] Minutes; 15 September 1970; Box 2 Folder 7; Kuhn Papers.

[56] Frosch speech; 29 September 1960; Box 5 Folder 1; Kuhn Papers.

As the committee members continued to review specific projects, they found that most of the projects flagged as possibly weapons-related were just that—*possibly* weapons related. There was no explicit weapons research at Princeton, and almost no classified research. No one was perfecting a neutron bomb or tinkering with weaponized napalm. Instead plausibly basic research projects with multiple applications abounded: from Jacobs' leaf senescence work to engineering studies of air flows with convenient relevance to the design of bomb-dropping airplanes. Faced with such ambiguity, committee researchers tried to sort projects by sponsor, but once again found that Jacobs' experience—receiving grants from both the Defense Department and the National Science Foundation—described many other campus researchers as well. The National Science Foundation, despite its reputation for funding only basic research, sometimes also funded research with military applications, while the U.S. military routinely sponsored basic research. As Physics Department Chair Marvin Goldberger told the *Daily Princetonian*, "The [Department of Defense] finances a lot of basic research because they think they're going to get another bomb out of us, and there is nothing we can do to dissuade them from this delusion. We have an Air Force contract on fundamental concepts in theoretical physics."[57]

The committee compiled data on the key outside funders for Princeton's science and engineering departments:

Dept.	DOD	NASA	AEC	NSF	Total
Astrophys.	$383,000	$2,599,000		$680,000	$3,663,000
Biology	$34,000	$112,000		$329,000	$994,000
Chemistry	$371,000		$297,000	$372,000	$1,700,000
Geology	$42,000	$51,000		$162,000	$289,000
Math	$160,000			$158,000	$340,000
Physics	$671,000		$2,738,000	$317,000	$3,776,000
Psychology	$69,000			$56,000	$649,000

[57] *Daily Princetonian*; *23* September 1970; Box 2 Folder 12; Kuhn Papers.

Statistics	$67,000			$83,000	$150,000
AMS	$1,688,000	$974,000		$230,000	$3,320,000
Chem. Eng.		$33,000	$54,000	$65,000	$184,000
Civil Eng.				$58,000	$70,000
Electr. Eng.	$88,000	$16,000	$15,000	$73,000	$246,000
Economics	$20,000			$11,000	$160,000

The results reflected some of the funding overlaps, and raised additional questions about another set of sponsors, those that were neither the Department of Defense nor the National Science Foundation. What was the committee to make of NASA, for example, or the Atomic Energy Commission? Comparing projects in the Aerospace and Mechanical Engineering Department, the committee noted that NASA-sponsored research looked "no different" from Defense-sponsored projects. As the committee's winter deadline approached, fewer and fewer policy recommendations emerged as adequate solutions to the problem of war-related sponsored research.

At a loss as to how to resolve these issues, the committee finally settled on a stronger reiteration of the school's anti-classified research policy and formal opposition to the existence of a classified library (a recommendation that would be overturned by the Princeton faculty). The proposal to sever ties with the Defense Department completely had long since been quietly jettisoned. These recommendations failed to satisfy many committee members, however, most notably Kuhn himself. The committee could use the classified barrier to deter explicit, exclusively war-related research campus, but were there ways to detect and prevent subtler changes in Princeton research caused by the influx of so much outside money? French literature professor and committee member English Showalter articulated this fear when he noted in a meeting that "It is striking how unaware researchers are of the extent to which they are influenced by outside agencies."[58] Did their choice of research topics, course curricula, and graduate students betray the influence of defense contracting?

Kuhn called the phenomenon "drift," and he had alluded to it in his early fears of "distortions of academic research." In his standard letter soliciting information about sponsored research at other universities, Kuhn had written to Cornell administrators of his concern about the dangers of heavy funding from "mission-oriented" sources such as the Department of Defense and NASA. He wrote, "If, as many members of the Committee are currently inclined to presuppose, that mode of funding does somehow *distort the normal evolution of academic research as a whole*, then we must recommend ways in which Princeton, hopefully in conjunction with other institutions, can help to effect a substantial change."[59] [emphasis added]

Such a view of academic research was odd for Kuhn, given his reputation as an advocate for the theory of paradigm shifts in science, which held that scientific theories did not, in fact, follow an independent, linear progress. His correspondents were quick to point out his error. W.D. Cooke, Vice President for Research at Cornell, admonished Kuhn for his naivete. Every source of funding influences the course of university research, he wrote, unless it is completely unrestricted—a rarity. Why single out the Department of Defense, NASA, the Atomic Energy Commission, and others as potentially distorting, and not, say, the Ford Foundation or the National Science Foundation? He noted wryly that "NSF is often accused of protecting the entrenched scientific establishment which generally dominates their policy boards."[60] Berkeley president Charles Hitch, responding to Kuhn's request for advice, noted that Berkeley had been

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[58] Minutes; 16 October 1970; Box 2 Folder 7; Kuhn Papers.

[59] Kuhn to Hitch; 12 June 1970; Box 5 Folder 3; Kuhn Papers.

[60] Cooke to Kuhn; 30 July 1970; Box 5 Folder 9; Kuhn Papers.

undergoing a similar review of sponsored research, though its mission, as a public university, perhaps differed from that of Princeton. He criticized Kuhn's "pejorative" category of "mission-oriented" agencies, noting that "the university research they sponsor is probably no more mission-oriented than, say, that of [Housing, Education, and Welfare] and [the National Science Foundation]." Moreover, this relatively decentralized "multiplicity of government agencies," originally envisioned by Kuhn's mentor, James Conant, probably worked better than the kind of "large-scale, government-run research institutions outside of universities" that operated in other countries.[61]

Preparing the final report, committee members went through draft after draft of recommendations regarding the problem of drift. In fact, they could not even declare that drift was authoritatively a problem. As Kuhn wrote in one draft, "one may categorically deny 'co-option' and still recognize the possibility that the differential availability of external funds for different university activities may gradually and subtly alter the direction of institutional development, producing over time a quite decisive transformation." The trend might be difficult to detect, and its effects might be negative or positive. Thus, there was no policy prescription other than "continuing vigilance."[62]

Kuhn, Drift, and the Historiography of Cold War Science

The travails of the Kuhn Committee, particularly its concern with the potential problem of drift, offer some new insight into the historiographical debates concerning agency and Cold War science. In the late 1980s, Paul Forman famously argued that scientists were in a sense

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[61] Hitch to Kuhn; 23 August 1970; Box 5 Folder 3; Kuhn Papers.

[62] Kuhn to committee; 17 May 1971; Box 3 Folder 6; Kuhn Papers.

duped by the postwar push for greater research and development funding.[63] Heavily influenced by the work of technology historian David Noble, he contended that while scientists had gained immense funding, they had lost *control.* He asked, "What direction of the advance of science, and thus what kind of science, resulted from military sponsorship?"—an echo of Kuhn's concern that "the differential availability of external funds for different university activities may gradually and subtly alter the direction of institutional development"—and determined that in the 1960s, certain fields, such as solid state physics, had flourished at the expense of less militarily relevant areas. When Forman complained that Cold War scientists suffered from "false consciousness," and were "focused so narrowly on immediate cognitive goals of their work as to miss its instrumental significance…to their military patrons," he might have been channeling the spirit of English Showalter, and his observations regarding "unaware researchers." Like Showalter, he thought that many scientists justified their research pursuits by 'pretending a fundamental character to their work that it scarcely had.' Levine's defense of Jacob's leaf senescence research—and his claim that it was based on Jacobs's intrinsic curiosity rather than military needs—would seem to illustrate this point. But does the experience of the Kuhn Committee really validate Forman's description of Cold War scientists as "more used by than using American society"?

In the annals of historiography, Forman's view was quickly contested. Daniel Kevles, for example, complained that Forman portrayed defense funding as having "seduced American physicists from, so to speak, a 'true basic physics,' encouraging them to the self-delusion that they were engaged in basic research of intrinsic interest while in reality they were merely doing

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[63] Paul Forman, "Behind Quantum Electronics: National Security as Basis for Physical Research in the United States, 1949-1960." *Historical Studies in the Physical and Biological Sciences*, 18.1 (1987): 149-229.

the military's bidding."[64] The truth, Kevles argued, was that there existed no single guiding federal agency setting research goals—rather, the funding system was varied and not monolithically organized, and scientists themselves often helped set "military technology policy." Thus, Kevles sided with Hitch and his preference for the "multiplicity of government agencies."

Though Forman hadn't posited the existence of a centralized conspiracy of military planners out to co-opt scientists, he worried, as Kuhn had, about the effects of universities' growing dependence on defense dollars. Forman argued that as almost all of physicists' labs and equipment began to depend on government and military funding for their existence, scientists' freedom to research could not but be subtly influenced and unconsciously redirected. The broader phenomenon described here—the influence of funding decisions and experimental successes on the paths of scientific research—had been documented by many other historians of science, perhaps most notably beginning in the late 1970s and early 1980s in works by Bruno Latour, Donald Mackenzie, Peter Galison, and Ian Hacking. All of these scholars were heavily influenced by Kuhn himself and his scholarly work on the creation of scientific paradigms.[65] As Hacking wrote eloquently in 1986, during the Reagan defense boom, "when so much knowledge

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[64] Daniel Kevles, "Cold War and Hot Physics: Science, Security, and the American State, 1945-56." *Historical Studies in the Physical and Biological Sciences,* 20.2 (1990).

[65] For an insightful review of this literature, see Ian Hacking, "Weapons Research and the Form of Scientific Knowledge," *Canadian Journal of* Philosophy; Supplementary Volume 12, (1986). He discusses Bruno Latour, *Laboratory Life: The Social Construction of Scientific Fact* (1977); Donald Mackenzie, *Inventing Accuracy*; and Peter Galison, "Bubble Chambers and the Experimental Workplace" in P. Achinstein and O. Hannaway, *Observation, Experiment and Hypothesis in Modern Physical Science* (Cambridge MA: Bradford Books (1985), among others. For more recent literature reviews and assessments, see David Hounshell, "The Cold War, RAND, and the Generation of Knowledge," *HSPS* 27:2 (1997); Rebecca Lowen, *Creating the Cold War University: The Transformation of Stanford;* and Zuoye Wang, "The Politics of Big Science in the Cold War: PSAC and the Funding of SLAC," *Historical Studies in the Physical and Biological* Sciences 25:2 (1995). A relevant personal account of how these trends played out at MIT in the 1960s and 1970s can be found in Raymond Siever, "Doing Earth Science Research During the Cold War" in Chomsky et al., eds., *The Cold War and the University: Toward an Intellectual History of the Postwar Years* (New York: The New Press, 1998).

is created by and for weaponry, it is not only our actual facts, the content of knowledge, that are affected. The possible facts, the nature of the (ideal) world in which we live becomes determined. Weapons are making our world, even if they are never exploded."[66]

Kevles also criticized Forman's failure to acknowledge the gray areas between basic and applied research. While some topics might follow naturally from one another, and others were clearly valued for their potential application value, a third kind of research defied these categories, or seemed to exist simultaneously in both camps—for example, fluid dynamics, a prominent area of research for Princeton's Department of Aerospace and Mechanical Engineering. Further complicating the matter, Kevles pointed out that something as fundamental and intellectually "pure" as particle physics simply could not have been pursued without government support; particle colliders are expensive, after all. The result was that Cold War military funding actually helped physics diversify. Nevertheless, he also observed:

> What the diversification fundamentally signified was not the seduction of American physics from some true path but its increased integration as both a research and advisory enterprise into the national security system. The more that the programs and installations of defense-related science came to find opportunities and enrichment in national security, the greater their propensity—and ability—to call for and create new weapons systems. The closer civilian scientists came to the center of executive power, the better positioned they were to influence overall defense policy.

Such a description, while different from that of Forman, nevertheless echoes Kuhn's fears as well. The provision of "opportunities and enrichment" in fields of defense science, with the promise of eventual political influence, could surely be considered a recipe for drift. All three of these men, however, posited the existence of an alternate, counterfactual, 'pure' path—the progressive journey that physics and other sciences would take in the absence of military funding. In the reality of the Cold War United States, however, no such path existed. The hopes

[66] Hacking, "Weapons Research…"

of Kuhn and others—that the increased sponsorship of basic research by the National Science Foundation could herald a return to the pure path—had been thoughtfully criticized by Kuhn's correspondents Cooke and Hitch.

Even if one were to combine the views of Cooke and Kuhn and assume the NSF-sponsored path to be more benign (from the perspective of a weapons science critic), in this scenario scientists still would not be *in control,* as Forman might wish. That is, they would not be able to pursue any topic they chose, relying still on the granting of access to expensive equipment, or money to construct equipment (from a Marxist perspective—the means of production). Kevles's argument that one logical progression of the field of nuclear physics required pricy investments in particle accelerators thus can be interpreted to support Forman's contention: no matter how benign the ends might seem, Big Science had diminished the autonomy and ***control*** of scientists, even as scientists followed what they believed was a natural path of inquiry.

What then, was a university to do? MIT had opted to sever official university ties with one of its war-related labs, while Princeton had established stronger barriers to classified research and made arrangements to phase out its lease to the Institute for Defense Analyses. But the same research continued at the Draper Lab, similar groups of men and women studied weapons efficacy and advised the Defense Department, and classified research that might have taken place in a Princeton laboratory now surely occurred elsewhere, in a government facility or, more likely, a private defense science firm. If not a Princeton scientist, then who? If not a laboratory with university oversight, then where? Had the academic critics of weapons science now pushed war research farther away from their means to restrain it?

Beyond MIT and Princeton

MIT and Princeton were not isolated cases—at campuses across the country, students, faculty, and administrators reviewed contracts and research projects and debated their schools' missions and ethical obligations. Cornell University sold its Aeronautical Laboratory, Caltech opened access and promoted more basic research at its Jet Propulsion Lab, and Stanford toyed with drafting mortgage documents that would sever ties to the Stanford Research Institute while imposing research restrictions as loan conditions. (Stanford eventually abandoned this plan, but severed ties to SRI nonetheless.) Michigan's Elderfield Committee, tasked with setting the school's classified research policy, ultimately chose to ban research whose "specific purpose" was "to destroy human life or incapacitate human beings," but to allow other classified research to continue.[67] At the other extreme, protesters occupied buildings across the country, including MIT's special labs, and in some cases committed acts of sabotage, as at the University of Wisconsin's Army Math Research Center.

The problems of "drift," the authenticity of scientific curiosity, and university-mandated research codes also revived the specter of academic freedom, interpreted in widely divergent ways by campus opponents. Michigan's Elderfield Committee argued that the preservation of academic freedom was a necessary barrier to the imposition of moral codes, noting that "One of the main values which distinguishes a university from other communities is its respect for and devotion to the principle of individual diversity—academic, political and moral. To restrict arbitrarily the activities of its faculty members on the basis of some concept of what a university 'ought to do' is to do violence to one of the main principles which a university should uphold."[68]

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[67] "Sponsored Research: Do You Control It or Does It Control You?" *College Management* April 1969, in MIT 2.3.

[68] "Sponsored Research: Do You Control It or Does It Control You?" *College Management* April 1969, in MIT 2.3

Princeton botanist William Jacobs and his defenders had espoused similar views, simultaneously lauding the importance of individual curiosity guiding one's research while also accepting the existing system of defense contracting and classified projects. Wolfgang Panofsky took a more skeptical, but similar position, telling Stanford students in 1969 that "…the initiative for wishing to do a given piece of research work should come from the professor and his students. It should not come from the sponsors who pay for it if special money is required."[69] But to a panel of Trustees he qualified his views, observing that classified research itself was not inherently "immoral," and, in the absence of "unilateral disarmament," necessary to some degree.[70]

To Chomsky, academic freedom meant just the opposite, a constant monitoring of the inevitable politicization that accompanied government and military outside contracts. As he had warned the MIT community in the Pounds report, "The idea that a university preserves its neutrality and remains 'value free' when it simply responds to requests that originate from without is an absurdity." Requiring employees at the special labs to gain security clearance or, at the very least, accept Defense Department funding, constituted a kind of institutional politicization that undermined any claims of individual academic freedom and academic neutrality.[71] Moreover, many research contracts at MIT were not simply requests from outside funders but were the products of ongoing negotiations between the university and defense agencies.

But two key questions remained: was any kind of politicization—or any kind of Defense-sponsored research—acceptable? Would weapons work have proceeded without disruption had

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[69] Panofsky speech to students, 18 April 1969, MIT 2.9.

[70] Panofksy, "Statement concerning SRI to Trustees Panel on April 30, 1969," in MIT 1.19.

[71] Also quoted in John Walsh, "MIT: Panel on Special Labs Asks More Nondefense Research," *Science* 164 (13 June 1969), 1264-1265.

the war in Vietnam been less unpopular? At the University of Michigan, scientists debated this very question: whether campus discontent was rooted in commitment to universal moral truths or frustration and anger at the war in Vietnam. No doubt the two were related; the Union of Concerned Scientists had emphasized their disillusionment in their founding statement, writing that "Through its actions in Vietnam our government has shaken our confidence in its ability to make wise and humane decisions." This same problem had motivated Charles Broxmeyer's emotional invective before the Pounds panel, as he accused its members of translating their frustration at Vietnam and guilt about the atomic bomb into inappropriately expansive moral language.

But in this regard scientists were no different from the rest of the country, and the rest of the world. Across the globe the language of a new generation of theorists and analysts, including Kuhn and Chomsky, was gaining ascendancy. The military-industrial complex, American imperialism, cultural hegemony, the dehumanizing effects of weaponry and automation—all were popular targets that could be addressed in the sweeping terms of theory and illustrated through the horrors of the U.S. war in Vietnam, French involvement in Algeria, or a host of other brutal world conflicts. As anger mounted, scientists whose World War II-era work had made them heroes now found themselves condemned for their monetary connections to tainted government agencies. For physicists and engineers especially, there were few ways out of this predicament; as the example of MIT showed, research conversion was difficult and funding rejections risky. In a sense, the possibility for individual choice was collapsing, but opportunities for significant institutional reform remained severely limited.

Back on the local level, few college administrations translated these radical institutional criticisms into significant policy reforms, but the analyses of Chomsky and others influenced unexpected audiences outside their own elite campuses, namely the entrepreneurs and managers of new spinoff defense firms and the administrators of large second-tier universities hoping to cash in on whatever defense moneys might be made available. The intellectual debates among elite academic scientists were not the major factors contributing to changes in the defense industry in the coming decades; nevertheless, the perceived risks of investing research funds in radicalized campus facilities or locating labs in hotbeds of student unrest helped create a new kind of social geography of defense research. The Epilogue of this dissertation explores some aspects of the development of this new social geography in Massachusetts, including the expansion of Northeastern University and its co-op program in the 1970s, the proliferation of suburban “spin-off” companies devoted to computer command-and-control technologies, the Reagan defense boom, and the new kinds of debates about politics, weapons, and ethics that emerged in the shadow of the Strategic Defense Initiative.

Epilogue: Science, Politics, and Ethics after Vietnam

In June 1985, MIT president Paul Gray delivered the university's annual commencement address to graduating seniors, in which he discussed science and the politics of research. He focused specifically on the current debates over the Strategic Defense Initiative (SDI), known popularly as "Star Wars," a new kind of anti-ballistic missile system proposed by the Reagan administration. Gray warned that the Pentagon was trying to use lucrative government contracts to universities to create an "implicit institutional endorsement for SDI." It was a "manipulative effort," Gray observed, given "the controversial nature and the unresolved public policy aspects of SDI." In stark terms, he announced that "This university will not be so used."[1]

Four years later, at an MIT teach-in held on the twentieth anniversary of the March 4, 1969 protests, engineering professor and self-described conservative James Melcher recalled that in his youth, he had actually felt alienated from the radical student movements of the 1960s. The antiwar protests had left him unmoved. In his case, he explained, his rebellion had come a decade and a half later, with Reagan's Star Wars speech in 1983.[2]

SDI created another turning point in the history of scientists' activism, in large part because so many scientists—including many who worked on nuclear weapons research—came to oppose it, or at the very least deeply and publicly doubt its chances for success. Star Wars reopened the old wounds of the lopsided arms control debate, once again pitting the elderly veterans of the Manhattan Project and the majority of academic physicists against the indefatigable Edward Teller and a handful of other SDI cheerleaders. It led thousands of

[1] Text of commencement address given by Paul Gray, 3 June 1985, Box 1, Folder 2, American Physical Society, Records of Directed Energy Weapons (DEW) Study, 1983-1988, American Institute of Physics, Niels Bohr Library, College Park, MD 20740, USA (hereafter APS DEW).

[2] March 4 Teach-In, Recording, 1989 (AC212), Institute Archives and Special Collections, MIT Libraries, Cambridge, Massachusetts.

scientists to sign statements refusing SDI contracts, and resulted in the resignations of key government advisors. Unlike the agonizing debates during the war in Vietnam, SDI elicited clear public condemnations from professional organizations, university presidents, and even Livermore employees.

The scientific and political landscape of the Star Wars debates—of the 1980s—was not the same as that of the test ban treaty and Vietnam controversies. Circumstances had changed, in large measure as a result of the earlier moments of upheaval described in previous chapters. In the 1970s, in the aftermath of the turbulent Vietnam years and in the face of a renewed debate over development of an antiballistic missile system, federal advisory mechanisms were dismantled and reconstituted in much weaker forms, scientists' compunctions about political activity shifted, and the social geography of weapons research itself stretched and expanded to include new regions, neighborhoods, and institutions. These changes, described briefly below, contributed to the particular form of the Star Wars debate, as well as the character and strategies of its participants. They also offer some insight into the contentious science politics of the twenty-first century, broadening beyond Cold War weapons disputes to debates over climate change, public health, and national science priorities.

The ABM and the Death of PSAC

In 1974, physicist-turned-activist Frank von Hippel reflected that the war in Vietnam had revealed the "bitter lessons about governmental limitations and fallibility," which eventually emboldened scientists and other experts to take strong public positions against projects they felt were inappropriate or wasteful. In the waning years of the war, this took the form of opposition to the development of an anti-ballistic missile system (ABM) and the supersonic transport

(SST).[3] The problem of the ABM had been the subject of a long-simmering debate that reached a full boil at the end of the Johnson administration, coinciding with the frustration of the Jasons and the resignation of George Kistiakowsky. The SST, though not as crucial a subject for national security officials, nevertheless tapped into the old philosophical questions about the avoidance of knowledge and the inevitability of technical development that had played such a key role in earlier nuclear debates.

At the center of these controversies was Richard Garwin, PSAC member, advisor to the Pentagon, CIA, and AEC, as well as star scientist at IBM. In January 1969, Garwin's extensive contributions to national security matters were lauded by top science advisor Donald Hornig in a letter to President Johnson. Hornig wrote that "The tragedy is that most of his labor has been so highly classified that public recognition of the usual sort has been out of the question." To compensate, Hornig drafted a personal thank-you letter for LBJ to sign, referring to Garwin's 'immense personal effort" on behalf of the government and lauding him as an "unsung hero."[4] But despite this gesture, the writing was on the wall; in March 1968, Garwin and Hans Bethe had published an enormously influential article on the problems facing an ABM system in *Scientific American*, arguing that in addition to threatening the relatively stable deterrence of mutually-assured destruction (MAD), the ABM system proposed by the Johnson administration could be easily overwhelmed by the use of decoy missiles and other inexpensive tactics.[5] Within a year, Garwin was engaging in the unthinkable. He took a highly visible, public anti-ABM stance, personally appealing to members of the US Senate to vote against the project. In 1970, after the

[3] Joel Primack and Frank von Hippel, *Advice and Dissent: Scientists in the Political Arena* (New York: Basic Books, 1974), 25.

[4] Memo, Hornig to LBJ, 16 January 1969, "Donald Hornig Chronological File: January 1969" Folder, Papers of Donald Hornig, Box 6, LBJL.

[5] Richard Garwin and Hans Bethe, "Anti-Ballistic Missile Systems," *Scientific American* 218, No. 3 (March 1968).

election of Richard Nixon, he likewise provided Congress with scathing testimony opposing the SST on the grounds that it was wasteful and potentially an environmental risk. He also revealed that PSAC had reviewed the project and reached conclusions at odds with the Nixon administration's current supportive stance. (The ensuing lawsuits by various groups attempting to gain access to the PSAC SST report would eventually contribute to the expansion of the Freedom of Information Act.)[6]

James Killian, the first presidential science advisor during the Eisenhower administration, wrote in his memoirs that he believed that the job of PSAC members was to serve the president, and if they did not believe in his policies, they ought to resign. He could not accept Garwin's behavior, writing in 1982 that while "I respected his conclusions and his right to them, I still cannot defend this act."[7] Lee DuBridge, former Caltech president and Nixon's top science advisor, would offer a similar view of PSAC's role, at one point telling Congress that, unlike Garwin, he considered himself "a soldier" who was bound "to support the President's decision" on the SST.[8]

In truth, the realities of the PSAC system had altered considerably from the days of Eisenhower to those of Nixon. Whereas Killian, Kistiakowsky, and Wiesner had been outspoken, respected advisors who were intimately involved in major national security debates (and who maintained excellent personal relationships with the presidents they served), DuBridge and his successor, Edward David of Bell Labs, were milquetoast science advisors, rarely consulted, lingering on the fringes of key decisions. A 1970 *New York Times* profile of DuBridge described

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[6] Thomas Halstead, "Lobbying Against the ABM, 1967-1970," *Bulletin of the Atomic Scientists,* 1 April 1971.

[7] James Killian, *Sputnik, Scientists, and Eisenhower: A Memoir of the First Special Assistant to the President for Science and Technology* (Cambridge, MA: MIT Press, 1977), 23.

[8] Quoted in Primack and von Hippel, 21.

him as a “gentle, grandfatherly science adviser” with apparently “little influence” and “no direct responsibility.”[9] He was followed by Edward David, whose access to the president was limited and who resigned after two and a half years.

John Baldeschwieler, the chemist who had worked on the barrier and other Vietnam projects and who was, briefly, deputy director of Nixon’s Office of Science and Technology, described some of the internal politics of science advising in a 2003 oral history for the Chemical Heritage Foundation. He recalled that under President Johnson, PSAC would typically produce reports with several approaches to a particular issue—one option to do nothing, one to spend an exorbitant amount, and one somewhere in the middle that seemed most reasonable, and which would usually be referred to the president. Under Nixon, however, the process changed: “[Nixon and Kissinger] requested a presidential-decision memoranda, employing a specific format to present a set of options, of which they got to choose one. Our recommendations were sent back to us for reworking if what the president wanted … wasn’t on the list.” Though Baldeschwieler credited the PSAC with a few notable instances of influence—for example, pushing Nixon to create the Environmental Protection Agency—he acknowledged that the bitter acrimony that developed between Nixon officials and Garwin constituted “a revelation that the argument was usually cast to fit the objective, rather than to fit the facts.”[10]

It was in this political atmosphere that scientists who opposed Nixon’s preferred policies suffered a swift backlash. Nixon revoked the appointment of Cornell chemist and ABM critic Franklin Long to head the National Science Foundation, a public humiliation that aroused an

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[9] James M. Naughton, “DuBridge, a Quiet Man at White House, Stirs Worry Among Scientists That His Views Are Not Heard,” *New York Times,* 1 March 1970.

[10] John D. Baldeschwieler, interview by David C. Brock and Arthur Daemmrich at Chemical Heritage Foundation, Philadelphia, Pennsylvania, 13 June 2003 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0280).

immediate outcry among scientists. As Mary McGrory noted in the *New York Times* at the time, "it touched the brotherhood to the quick."[11] The Federation of American Scientists warned:

> An aspect of the affair that is of even greater concern…is that the choice of scientists for important and influential positions in government, even those which are not directly related to the military, is being made on political grounds rather than on the basis of scientific and administrative competence. If this continues, the Nixon Administration will have so-called scientific advisors who are trained seals rather than intelligent men of independent scientific judgment.[12]

Nixon's move actually solidified scientific opposition to the ABM, McGrory argued, noting that "scientists have a strong sense of fraternity and an equally powerful sense of guilt for their silence about the moral consequences of splitting the atom. Now, goaded by the President they are probably in the anti-ABM fight to stay."[13] Whether Nixon was to blame or not, scientists overwhelmingly opposed the ABM. A SESPA poll conducted at the 1969 annual meeting of the American Physical Society revealed that 76% of the 1216 physicists who voted opposed Nixon's proposed Safeguard ABM program. During Congressional hearings in 1970, all four former top science advisors (Killian, Kistiakowsky, Wiesner, Hornig) testified against Safeguard, as did Hans Bethe, Herbert York, and Jason members Marvin Goldberger and Sidney Drell.[14] It was the unity and largely technical arguments of the arms control scientists that ultimately swayed Congress to oppose the program, and set the stage for Nixon's eventual endorsement of the moderate limitations imposed by the Anti-Ballistic Missile Treaty, signed by the US and USSR in 1972.

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[11] Mary McGrory, undated clipping (1969), in Box 4, Folder 8, Papers of Brian Schwartz, 1966-1977 (hereafter BSP), Niels Bohr Library, American Institute of Physics, College Park, MD.

[12] *FAS Newsletter,* April 1969.

[13] Mary McGrory, undated clipping (1969), in Box 4, Folder 8, BSP.

[14] Thomas Halstead, "Lobbying Against the ABM, 1967-1970," *Bulletin of the Atomic Scientists,* 1 April 1971.

Of course, it wasn't only the ABM and SST issues that deepened the antagonism between scientists and the Nixon administration. The war in Vietnam and the actions of President Johnson and his advisors had already created a dangerous gulf, and Nixon's invasion of Cambodia further fueled the disillusionment and opposition of many scientists. Though scientists' public pressure eventually forced Nixon to reoffer the NSF position to Long, he promptly refused it the second time around. A year later, the president would pull a nearly identical stunt, revoking the appointment of Caltech's George Hammond to head NSF. Hammond had participated in a Caltech protest against the invasion of Cambodia, part of the same wave of protests that led to the shootings at Kent State and the formation of the Kuhn Committee at Princeton.

It was not surprising, then, that in 1973, Nixon opted to dismantle the entire presidential science advising system, eliminating both the Presidential Science Advisory Committee and the Office of Science and Technology in favor of a much weaker White House Science and Technology Policy Office.[15] Joel Primack and Frank von Hippel assessed the decision along the same lines as Baldeschwieler and McGrory, noting that "The abuses of the advising system arise out of its political exploitation, and the White House appears to have abolished PSAC precisely because it was not exploitable enough."[16] They also concluded that "frustration with science advisors like Dr. Garwin" was at least "partly responsible" for the decision, especially considering that the SST and ABM debates had constituted the first times that Congress had "really challenged the administration on a major high-technology program."[17] If, as von Hippel

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[15] For more in-depth analyses of the dismantling of PSAC, see Gregg Herken, *Cardinal Choices: Presidential Science Advising from the Atomic Bomb to SDI* (New York: Oxford University Press, 1992); Interview of Richard Lawrence Garwin by Finn Aaserud on 23 October 1986 in Yorktown Heights, NY, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA.

[16] Primack and von Hippel. 37.

[17] Primack and von Hippel, 36, 25.

would later argue, the Johnson and Nixon administration had increasingly only drawn on science advisors “to give political decisions the appearance of objectivity,” and the science advisors—or at least an influential handful—had now refused to play along, it made sense that Nixon would no longer see a need to maintain the existing system.[18] Jerome Wiesner bluntly summed up this analysis in a 1973 statement to *Time Magazine*, observing that the dismantling of PSAC was, in the end, a more honest result: “The reorganization simply recognizes the situation as it has existed throughout the Nixon Administration.”[19]

Federal science advising would nevertheless limp along through the 1970s. In 1975, President Gerald Ford oversaw the establishment of short-term advisory panels, designed partly as a means to reach out to the science community and repair some of the Nixon-era damage, and partly to investigate actual topics in science and technology. A year later, Congress would create the Office of Science and Technology Policy, which included a presidential advisory committee. President Jimmy Carter dismantled this committee at the start of his administration, but the OSTP remained. Much of the science advising during the Carter administration took place at the departmental level, however, particularly in the Departments of Agriculture, Energy, and Defense.[20]

The Vietnam Bust in Massachusetts: From MIT to Northeastern

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[18] Frank von Hippel, *Citizen Scientist* (New York: American Institute of Physics, 1991), vii.

[19] Quoted in “Nixon v. the Scientists,” *Time Magazine*, 26 February 1973, http://www.time.com/time/magazine/article/0,9171,910590,00.html (accessed 5 May 2011).

[20] For further assessment of science advising during the 1970s, see Gregg Herken, *Cardinal Choices: Presidential Science Advising from the Atomic Bomb to SDI* (New York: Oxford University Press, 1992) and Gerhard Sonnert, *Ivory Bridges: Connecting Science and Society* (Cambridge: MIT Press, 2002).

The new advisory systems of the 1970s enabled and expanded the trend that had been in place since the mid-1960s: the elevation of a new class of experts to replace the aging Manhattan Project veterans and theoretical physicists borrowed from the elite institutions of academia. The new class had much stronger roots in military research and defense engineering. As James Katz observes in *Presidential Politics and Science Policy,* during the Johnson administration, "PSAC…began recruiting its members from industrial, engineering, and even social-science backgrounds."[21] The Air Force's Science Advisory Board had already shifted in that direction by 1964, and Air Force insider Thomas Reed later described a concerted effort in the 1970s to elevate the status of "R&D program managers, information systems people," and other technical experts in the Air Force hierarchy.[22] The Advanced Research Projects Agency in the 1970s likewise shifted its focus from the free-ranging promotion of cutting-edge new technologies to short-term projects with clear defense applications; in 1972 it was renamed the Defense Advanced Research Projects Agency, or DARPA.[23]

A similar transformation was occurring in the competition among universities for increasingly precious government research dollars. Whereas federal contracts had surged in Sputnik boom years, they began to level off and decline in the early 1970s, during the Vietnam bust.

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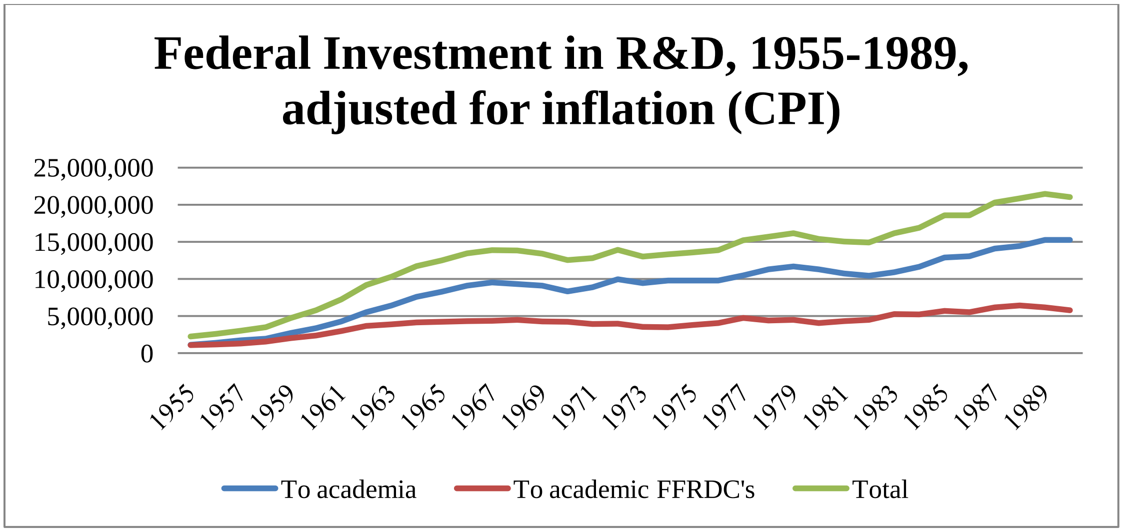
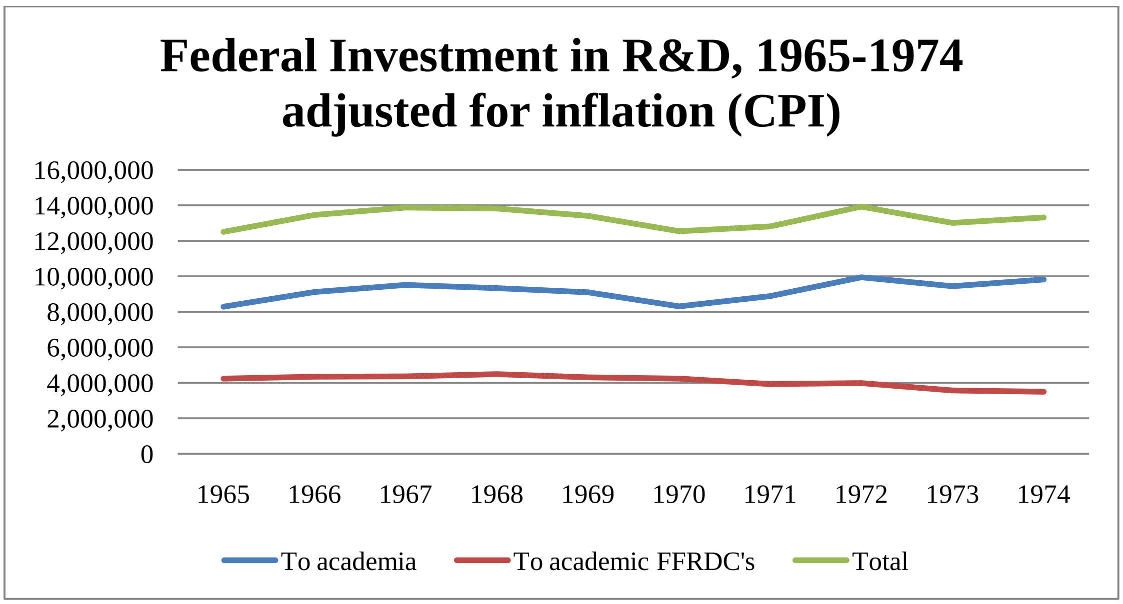
[21] Katz, 156.

[22] Thomas A. Sturm, *The USAF Scientific Advisory Board: Its First Twenty Years, 1944-1964* (Washington, D.C.: Office of Air Force History, 1986); Thomas C. Reed, *At the Abyss: An Insider's History of the Cold War* (New York: Ballantine, 2004), 208.

[23] Arthur L. Norberg, "Changing Computing: The Computing Community and DARPA," *IEEE Annals of the History of Computing,* Vol. 18, No. 2 (1996).





Source: National Science Foundation, Federal Funds for Research and Development, Detailed Historical Tables: Fiscal Years 1951-2002, http://www.nsf.gov/statistics/nsf03325/ (accessed 30 May 2008).

In Massachusetts, where the labs of MIT and Harvard had "spun off" dozens of small entrepreneurial defense firms in the 1960s, the dip in defense contracting—to both universities and their spinoffs—coincided with the dramatic student protests demanding an end to weapons-related campus research and the beginning of a wave of layoffs at both major defense firms and smaller technology startups. Rather than spending precious financial resources on retaining the top scientists with PhDs from elite universities, many companies axed their more expensive employees and recruited younger, more specialized engineers who could be lured at a cheaper price.[24] In 1970, Berkeley Rice published a depressing portrait in the *New York Times* of out-of-work scientists and engineers laid off from their cushy jobs along the “Golden Horseshoe” of Rte. 128, the “East Coast center of the electronics industry.” PhDs from Harvard and MIT complained that they had been priced out of their own companies, which were downsizing and scaling back research as federal funding for defense and space-related technology dried up.[25] Deborah Shapley reported a similar trend on the pages of *Science* in June 1971, conducting dozens of interviews in the Boston area and noting the common complaint of “de facto age discrimination,” in which labs and research firms discarded older experts in favor of “fresh graduates,” in order to “promot[e] economy in the company payroll.”[26]

Both Berkeley and Shapley noted widespread bitterness toward the Nixon administration among the unemployed scientists and engineers. Shapley reported that in the thirty interviews conducted by *Science,* every subject had “stated adamantly that they wouldn’t vote for Nixon in 1972,” though the reasons cited had little to do with foreign policy decisions and everything to do with cutbacks in aerospace and other defense contracts. Berkeley cited several engineers who theorized that Nixon’s cuts were a form of retaliation against the Massachusetts senators who had opposed the ABM. At the other end of the political spectrum, in 1970 Science for the People

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[24] For a useful overview of the rise and fall of Rte. 128, see AnnaLee Saxenian, *Regional Advantage: Culture and Competition in Silicon Valle and Route 128,* Cambridge (Harvard University Press, 1994) and David Lampe, ed., *The Massachusetts Miracle: High Technology and Economic Revitalization* (Cambridge: MIT Press, 1988).

[25] Berkeley Rice, “Down and Out along Route 128,” *New York Times,* 1 November 1970.

[26] Deborah Shapley, “Route 128: Jobless in a Dilemma about Politics, Their Professions,” *Science* 127, No. 3988 (June 11, 1971), 1116-1118.

complained of the “planned unemployment” orchestrated by the Nixon administration, designed to force recalcitrant engineers to go “begging for DOD and Space make-work.”[27]

While Nixon’s antagonism toward the scientific community is well-established, the conspiracy theories of the Rte. 128 engineers and the *Science for the People* radicals are difficult to prove. But since the mid-1960s there had been government efforts to redistribute research funding to promote geographical diversity; in other words, to redirect funding away from the established academic centers in California, Massachusetts, and New York, toward new regions and institutions. During the Johnson administration, a proposal by Donald Hornig to the Federal Council for Science and Technology was expanded by the president in late 1965 into a national policy to help “additional institutions to become more effective centers for teaching and research.” One outgrowth was the Pentagon’s Project THEMIS, which began as a $25 million a year program “deliberately seeking out departments in universities which have not been engaged significantly in defense related research, and supporting research in those departments which appear to have the capacity to contribute to the solution of problems of interest to DOD.”[28] During the three years of its existence, the program distributed $88 million to universities in 42 states. Though little has been written about the politics behind the launch of Project THEMIS and the results of its implementation, at least one scholar, James Katz, has concluded that campus activism at key research centers was an important precipitating factor: “Of major concern to the DOD was that the traditional performers of university research (e.g., Harvard,

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[27] Science for the People flyer, 25 February 1970, Box 5, Folder 1, Papers of Brian Schwartz, 1966-1977, Niels Bohr Library, American Institute of Physics, College Park, MD.

[28] Report, “Presidential Policy on Strengthening Academic Capability for Science Throughout the Nation,” undated, “Volume II Documentary Supplement [2 of 3]” Folder, Box 1, Administrative History, Office of Science and Technology, LBJ Library.

Massachusetts Institute of Technology, Princeton, Berkeley) were increasingly shying away from military-related R&D because of pressure from the rising anti-war movement."[29]

These trends—rising antiwar radicalism at elite research universities; private sector preferences for younger, cheaper labor; and federal contracts in need of new recipients—encouraged the expansion of existing second-tier research universities into powerhouses of defense research. The rapid growth of Northeastern University in Boston during this period exemplifies this process. In her comparative study of the technology firms of Rte. 128 in Massachusetts and Silicon Valley in California, AnnaLee Saxenian notes that in the 1960s, "the community and state colleges in Massachusetts were small, underfunded, and lacking in status, particularly compared with California's community and state college system." She cites the perception of some local technology employers that despite key spinoff genealogies, Harvard and MIT seemed standoffish and distant from the growing electronics and computing sector when compared to Stanford or Berkeley in California. As a consequence, some of the New England's large defense technology firms began to offer employees training and education themselves."[30] They also reached out to Northeastern, a large urban commuter-school with an ambitious president, Asa Knowles, and an innovative co-op program that blended classroom instruction and local work experience.

Through the 1960s and early 1970s, Knowles set out to remake Northeastern into a prominent research university. During his tenure, the school's undergraduate enrollment more than doubled and a host of new graduate programs offered professional degrees. Knowles was himself a canny surveyor of the political landscape. In the early 1970s, he pushed hard to bring

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[29] James Everett Katz, *Presidential Politics and Science Policy* (New York: Praeger, 1978), 155.

[30] Saxenian, 67-68.

defense contracts and research money to his school and to expand the co-op program as a feeder to big defense companies. He explicitly promoted his university as a safer alternative for weapons research than the more radicalized elite campuses of MIT and Harvard. In 1970, Knowles addressed an audience of representatives from some of the nation's most prominent defense contractors, including Westinghouse, Monsanto, Dow Chemical, and Du Pont. Hoping to secure new institutional ties, Knowles advertised the typical Northeastern co-op student as likely to be "more business and industry oriented and … likely to be a more understanding sympathizer of the establishment." He promised that companies hiring NU co-op students could "avoid many of the extremist attitudes and difficult problems which may arise."[31] Northeastern students were not apathetic, he explained, but their working-class backgrounds and interest in industry experience precluded antiwar radicalism. Along with Knowles's similar efforts to attract classified and military research to campus—the types of research that were so vilified elsewhere in the early 1970s—Northeastern succeeded in dramatically expanding its research and development contracts, as well as its student co-op program, during the 1970s.

Scientists, the Nuclear Freeze, and Star Wars

By the time of President Ronald Reagan's 1983 Star Wars speech introducing the Strategic Defense Initiative, the Massachusetts defense and technology industries were on the rise once again, as key centers for computing and software research. But also ascendant was the nuclear freeze movement, an outgrowth of the arms control efforts of the 1950s, now bolstered by the growth of environmentalism, the disaster at Three Mile Island, and significant attention

[31] Asa Knowles, "Cooperative Education—A Timely Concept" presented to the Council of Executives on Company Contributions, 4 June 1970, in Folder 1963, Box 44/49, Northeastern University Office of the President (Knowles), Records 1939-1983, Northeastern University.

from popular media through news reports, films, and literature.[32] The election of Reagan in 1980 further fueled nuclear fears and consequent political organization, as well as the publication of a spate of books by politicians, activists, and scientists warning of the terrifying risks of nuclear disaster.[33] The new wave of nuclear activists called for an immediate and complete halt to weapons development and production, an echo of Jerome Wiesner's proposals during the test ban negotiations of 1963. Graduate students at MIT publicly supported the freeze in 1980, and two years later, a New York City antinuclear protest drew a million participants. Even the 65,000-member International Chemical Workers Union, which included DuPont and Monsanto employees, voted in favor of a nuclear freeze resolution at their 1982 convention.[34]

While a full history of the nuclear freeze movement is beyond the scope of this dissertation, two aspects of its development are important to note. First, the movement incorporated many of the scientist-activist organizations that had flowered in opposition to the war in Vietnam, and included members of the Physicians for Social Responsibility, the Union of Concerned Scientists, and many other groups. Second, the movement was particularly strong in Massachusetts, where Senator Ted Kennedy pushed for a nuclear freeze in Congress, and in California, where protesters held daily vigils at the gates of the Livermore lab, confronting the

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[32] Some films include: *On the Beach* (1957 novel, 1959 film), *Fail-Safe* (1964), *Dr. Strangelove* (1964), *Planet of the Apes* (1968), *The China Syndrome* (1979), *The Atomic Café* (1982). For a thorough bibliography of nuclear-themed literature through 1984, see Paul Brians, *Nuclear Holocausts: Atomic War in Fiction, 1895-1984,* (Ohio: Kent University Press, 1987).

[33] For a history of the nuclear freeze movement, see David S. Meyer, *A Winter of Discontent: The Nuclear Freeze and American Politics* (New York, Praeger, 1990). For books published in early 1980s, see Edward Kennedy and Mark O. Hatfield, *Freeze! How You Can Help Prevent Nuclear War* (New York: Bantam Books, 1982); Jonathan Schell, *The Fate of the Earth* (New York: Knopf, 1982); Solly Zuckerman, *Nuclear Illusion and Reality* (New York: Viking, 1982); Robert Scheer, *With Enough Shovels: Reagan, Bush & Nuclear War* (New York: Random House, 1982).

[34] PCNAC/CANW pamphlet list, "Merger with Citizens Against Nuclear War" Folder, Box 2, Series C: Administrative/Programmatic Efforts, in the Professionals' Coalition for Nuclear Arms Control Records (DC 164), Swarthmore College Peace Collection.

scientists and engineers who worked within and demanding justifications for weapons contributions. Hugh Gusterson, an anthropologist who studied nuclear weapons scientists at Livermore in the 1980s and 1990s for his 1996 book *Nuclear Rites*, questioned his subjects about their reactions to the protesters outside. Many had been opposed to the Vietnam War and sympathized with the environmental movement, but believed strongly that their own nuclear weapons work promoted peace through deterrence. Gusterson concluded that for many, the protests actually strengthened in Livermore employees a kind of individualized ethics rooted in "the central axiom that, given the nature of the international system, nuclear weapons offer the best hope of preventing war and saving lives."[35] In many ways, the ethical assessments of the Livermore employees of the 1980s resembled those of the Manhattan Project scientists during the early years of World War II: their weapons work was a necessary contribution supporting national security goals with which they agreed.

Reagan's proposed Strategic Defense Initiative, offering a revamped ABM system with space-based interceptors, exactly undermined the MAD-based deterrence held dear by Livermore scientists. It called for defensive measures that could, in theory, allow for a new kind of nuclear war that was both survivable and winnable. The origins of this shift lay in the resurgent conservatism that had swept Reagan to power and in the elevation of Edward Teller to a level of advisory influence he had not held since the first Eisenhower administration. Since the late 1960s, Teller had been lobbying then-Governor Reagan in favor of missile defense, and immediately after Reagan's inauguration in1981, Teller was hard at work with colleagues from the Heritage Foundation developing strategic defense recommendations and meeting repeatedly

[35] Hugh Gusterson, *Nuclear Rites: A Weapons Laboratory at the End of the Cold War* (Berkeley: University of California Press, 1996), 67.

with top administration officials to present the new technologies being developed at Livermore that could contribute to such a system.[36]

But technology was not really at the heart of the decision to pursue a new kind of nuclear defense, as evidenced by the lack of consultation with the administration's own White House Science Council in the period preceding the announcement of the new policy. John Bardeen, inventor of the transistor and a double Nobel laureate in physics, resigned from the Council shortly after Reagan's speech, complaining that "there was no point in being on a committee which is supposed to give advice when such a truly major scientific and technical decision is made without consulting the body."[37] (Or as Herbert York complained, Teller had exploited his "privileged" advisory position to sell "exceedingly expensive technological exuberance" to "an uninformed leadership," while knowledgeable science advisors had been left unconsulted.[38]) Teller's preferential treatment was made possible by the weakness of the regular science advisory channels, and by the compatibility of his hawkish nuclear views and the strategic priorities of Reagan's cabinet, which represented a dramatic break from the arms control liberalism of Robert McNamara and the détente of Nixon and Kissinger. A 1983 article on nuclear policy in *Scientific American* dissected this shift, noting that whereas McNamara had reportedly advised both Kennedy and Johnson never to use nuclear weapons, Caspar Weinberger, Reagan's Secretary of Defense and a former Bechtel vice-president, believed in the

[36] Gregg Herken, "The Earthly Origins of Star Wars," *Bulletin of the Atomic Scientists,* 1 October 1987; Edward Teller with Judith Shoolery, *Memoirs: A Twentieth-Century Journey in Science and Politics* (Cambridge: Perseus Publishing, 2001), 509.

[37] Lillian Hoddeson and Vicki Daitch, *True Genius: The Life and Science of John Bardeen* (Washington, DC: Joseph Henry Press, 2002), 269.

[38] Quoted in Herken, "The Earthly Origins of Star Wars," *Bulletin of the Atomic Scientists,* 1 October 1987.

possibility of "waging a wide range of 'limited' or 'protracted' nuclear wars and the concomitant undesirability of a 'no first use' policy."[39]

If the scientific community had been divided over the war in Vietnam, the ethics of government service, and the legitimacy of New Left criticisms, unified opposition to the Strategic Defense Initiative began to heal some old wounds. A 1986 poll conducted by the National Academy of Sciences found that of over 450 members who were employed in fields "relevant to the Strategic Defense Initiative," over 80% opposed it.[40] After Reagan's announcement, the American Physical Society, which had been so reticent to take any public actions concerning the Vietnam War, immediately commissioned a Directed Energy Weapons (DEW) Study, an "unclassified study of the scientific and technological basis of directed energy weapons—R&D status, prospects and implications."[41] When it was finally completed in the spring of 1987, it offered a damning assessment of the key SDI technologies. As one magazine summarized, the report declared that "death rays in space will be science fiction for a long, long time."[42] Even earlier than the publication of the APS report, an SDI evaluation by the Office of Technology Assessment at the request of the Senate offered similar criticisms. Its main author, MIT's Ashton Carter, concluded that "The prospect that emerging 'Star Wars' technologies, when further developed, will prove a perfect or near-perfect defense system, literally removing from the hands of the Soviet Union the ability to do socially mortal damage to the United States

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[39] "Science and the Citizen," *Scientific American* (December 1983), 76. For a history of the decision-making behind SDI, see Frances Fitzgerald, *Way Out There in the Blue* (New York: Simon and Schuster, 2000).

[40] Clipping, "Academy Members Skeptical on SDI," *Science* 234, No. 4778 (14 November 1986), 816, Box 1, Folder 8, APS DEW.

[41] DEW Study Proposal, 1983, Box 2, Folder 10, APS DEW.

[42] Clipping, *Discover,* July 1987, in Box 1, Folder 8, APS DEW.

with nuclear weapons, is so remote that it should not serve as the basis of public expectation or national policy about ballistic missile defense."[43] Meanwhile, former high-level government advisors and Jason members like Richard Garwin and Sidney Drell testified before Congress that SDI technologies would be ineffective and wasteful,[44] and in the summer of 1987 a report from the Defense Science Board and a study from Livermore itself criticized SDI technology as deeply vulnerable.[45]

In addition to the production of highly critical studies, thousands of scientists demonstrated their opposition to SDI through open petitions and letters and a highly publicized campaign to boycott SDI-related research contracts. By 1984, over 15,000 physicists from 44 countries has signed a nuclear freeze statement; as *Physics Today* noted proudly, the list of signatories included more than half of the living Nobel Prize winners.[46] The Union of Concerned Scientists published books and pamphlets arguing the case against Star Wars. And beginning in May 1985, science professors and researchers at Cornell and the University of Illinois began circulating a pledge to boycott SDI research. Within six months, University of Illinois physicist John Kogut was reporting that "At least 54% of the faculty at the nation's top 14 physics departments have pledged to reject [SDI] funds, and the number is rapidly growing." At Princeton, Cornell, Berkeley, Caltech, Columbia, Carnegie Mellon, Northeastern, and numerous other universities, 50% or more of the physics faculty had signed the pledge. As a SANE press release reported, many of these anti-SDI scientists were researchers who "ordinarily accept other

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[43] Ashton B. Carter, "OTA Background Paper," April 1984, Box 2, Folder 3, APS DEW.

[44] Clipping, undated, *New York Times*, Box 1, Folder 7, APS DEW.

[45] Union of Concerned Scientists "Issue Backgrounder," August 1988, Box 1, Folder 3, APS DEW.

[46] Clipping, *Physics Today*, January 1984, Box 1, Folder 6, APS DEW.

types of military funding." Their opposition to Star Wars was rooted not simply in skepticism about technology, but in the conviction that SDI "would be destabilizing and dangerous."[47]

By 1988, the harsh report of the American Physical Society had been published, and the American Mathematical Society had voted, in the words of a UCS report, "not to participate in any activities that could be interpreted as supporting the SDI."[48] The professional organizations that in 1969 had fought tooth and nail to avoid public political statements regarding Vietnam now leapt headfirst into the political controversies of Star Wars. In a similar shift, the administrations at both Caltech and MIT displayed particular sensitivity at the appearance of any links to Star Wars. As noted, MIT president Paul Gray denounced government attempts to cite MIT research projects as an institutional endorsement of the SDI's aims as a whole. In 1985, Marvin Goldberger, the Jason physicist now elevated to president of Caltech, complained to the head of the Strategic Defense Initiative Organization that an earlier Pentagon announcement of SDI contracting falsely suggested that Caltech was a site of major institutional SDI research, rather than merely the home of one electrical engineer with a six-month Pentagon contract for computing research.[49] Top government science advisors were similarly attuned to the political consequences of their positions than had been the case during the Vietnam era. When John Bardeen resigned from Reagan's White House Science Council in the aftermath of the SDI announcement, he complained that "he was being used for his name," and resented the implication that he supported the new program.[50]

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[47] SANE press releases, 17 October 1985 and 13 May 1986, Box 1, Folder 2, APS DEW.

[48] Union of Concerned Scientists "Issue Backgrounder," August 1988, Box 1, Folder 3, APS DEW; Clipping, *Science News*, 2 April 1988, Box 1, Folder 8, APS DEW.

[49] Clipping, 5 June 1985, Box 1, Folder 7, APS DEW.

[50] Hoddeson and Daitch, 269.

Bardeen observed in the fall of 1985 that "there are few scientists either within or without the administration" who believed Star Wars was "feasible," but "the national laboratories (Los Alamos and Livermore) would have an exciting new project with unlimited funding. The aerospace industry would have a lucrative new program. So, in spite of the doubts, there is no lack of supporters."[51] Major SDI contractors included Livermore, GM, GE, Lockheed, Los Alamos, MIT's Lincoln Labs, and a host of others.[52]

Given the recessionary economics of the late 1970s and early 1980s, the issue of defense contracting in the age of Star Wars was a difficult one. Hugh Gusterson has estimated that during the 1980s, 20-40% of all US scientists and engineers were working on military projects, generally speaking. Los Alamos and Livermore alone employed 6% of all US physicists. Though these figures demonstrate that the majority of scientists were not directly working on nuclear weapons or Star Wars, Pentagon funding was still ubiquitous, and the array of SDI contributing technologies was wide: for example, computing necessary for remote command-and-control systems or signal processing required for over-the-horizon radar and other techniques were both popular areas of study along Massachusetts's Rte 128. A young Livermore scientist told Gusterson, "You realize how big the military-industrial complex is when you graduate. If you get a degree in physics, there's almost nowhere to get a job where you're not part of the military-industrial complex. Even the universities are getting drawn in. It's too big."[53] Daniel Kevles offered a similar anecdote in his introduction to *The Physicists*, noting that the pervasiveness of

[51] Quoted in SANE press release, 13 September 1985, Box 1, Folder 2, APS DEW.

[52] *FAS Bulletin*, April 1987.

[53] Gusterson, 43.

defense funding and influence by the 1980s was such that one recent physics PhD lamented that it seemed “all roads lead to the Pentagon.”[54]

The ethical strangeness of the Star Wars debates was such that few scientists believed it would work, yet while many publicly protested its implementation on both technical and strategic grounds, many others simply accepted the available funding and went about their research. It was perhaps an echo of the scenario described by Lincoln Lab employee Paul Easton to the Pounds Panel at MIT: he and his colleagues laughed at the politicians who believed an ABM system would work, even as they conducted the relevant research requested of them. Nevertheless, the unprecedentedly large number of scientists and engineers who did not remain silent in their opposition to SDI in the 1980s is significant. It reflects the culmination of multiple Cold War trends in ethical thinking among scientists: the historic affinity of scientists, especially physicists, for arms control; an evolving view of appropriate political behavior by scientists, including among high-level government advisors; and a heightened sensitivity to the politics of institutions and contracts in the military-industrial-academic complex, even as defense contracting reached new highs during the Reagan boom.

The moral imperatives of World War II had left the vast majority of physicists convinced that their work on the Manhattan Project was ethically justified, but the actual decision to use the bomb in Japan spurred many of the scientists to political action, in the hopes of controlling the development and use of nuclear weapons. In the postwar years, scientists pushed for—and won—civilian control of nuclear energy, though they were unable to prevent the onset of an arms race with the Soviet Union.

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[54] Daniel Kevles, *The Physicists: The History of a Scientific Community in Modern America* (New York: Vintage Books, 1979).

The related debates over the hydrogen bomb raised deep philosophical questions about whether a society could consciously and safely avoid a certain path of technical development. On one side, men like Teller and Ulam argued that the development of thermonuclear weapons was inevitable; on the other, Hans Bethe declared that he wished to remain ignorant. Arms control scientists lost this debate as well. In the muddled nuclear policy of the Truman administration and through Eisenhower's first term, they saw few moments of hope. But the launch of Sputnik itself launched a new era for arms control, as the flood of new science advisors—many Manhattan Project veterans who deeply supported arms control—found a sympathetic ear in Eisenhower and a genuine ally in Kennedy. The tortuous path to the Partial Test Ban illuminates the efforts made by Jerome Wiesner and other scientists to push the federal government—including both the president and Congress—toward a desired end, by working from within. Although international relations precluded a more sweeping comprehensive treaty, it was a moment of impressive cooperation between elite scientists and the government with which they so strongly identified.

That cooperation became more problematic with the escalation of the war in Vietnam. In 1974, the British physicist E.H.S. Burhop wrote in the *Bulletin of the Atomic Scientists* that "the 'Jason group' has often—rightly or wrongly—been taken to symbolize social irresponsibility among scientists and an attitude of callous disregard for the consequences of their work." The Jasons' own correspondence reveals that far from disregard, many members thought deeply about the consequences of their work, but found that they could not control the applications of their own research in the ways that they had expected; echoing the fears of Norbert Wiener at the end of World War II**,** they felt their work had been *misused*.[55] George Kistiakowsky had

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[55] E.H.S. Burhop, "Scientists and Soldiers," *Bulletin of the Atomic Scientists,* 1 November 1974

sought—and believed he had received—assurances from Robert McNamara that the electronic barrier would be a tool for deescalating the war. When no such result ensured, Kistiakowsky removed himself from his advisory position.

Burhop's essay on the Jasons was written on behalf of the World Federation of Scientific Workers, which he explained "believes that all scientists have the obligation to examine the likely consequences of their scientific work and, as far as lies in their power, to prevent its use for evil anti-social and destructive ends." He had solicited reflections from the roster of Vietnam-era Jasons, and quoted several, anonymously, in the *Bulletin.* The range of views largely reflected those discussed in Chapter Three: the Jasons described feeling misused, ignored, disillusioned, patriotic, regretful, and unapologetic. Some wondered whether it was more effective to work inside or outside "the establishment." But Burhop zeroed in on one response in particular, which he felt identified "the most important ethical problem, namely, the purposes which the scientific advice given to one's government will serve." It was the unsettling opposite of Wiesner's patriotic and powerful alliance with Kennedy in pursuit of a test ban. Burhop quoted the anonymous Jason:

> When and where should one draw the line? Designing gas chambers for Auschwitz is clearly over the line, working on the Manhattan project in the same period, clear (to me at least) not. What about today? Almost all of American science is supported in one way or another by the US government, in many cases directly through the Defense Department…. To what extent should one stop cooperating with a government when one disagrees with its policies?[56]

That final question was the same one asked, in multiple forms, by the students and faculty on the campuses of MIT, Princeton, and countless other universities during the years of the Vietnam War, and by Charles Schwartz and his colleagues in SESPA and the American Physical Society. In the 1980s, vast numbers of American scientists answered this question in their refusal to

[56] E.H.S. Burhop, "Scientists and Soldiers."

accept SDI contracts. They endorsed Hans Bethe's side of the debate over that of Edward Teller. But thousands of other scientists and engineers stood up to receive those same contracts.

Burhop's question has no simple answer. In the absence of a radical remaking of the structures of American science, government, and military, the ethical burdens felt by scientists will persist, endemic pitfalls in the intertwined paths of American science and Cold War politics that have led us into the twenty-first century. This dissertation has not attempted to provide easy resolutions or a universal moral code to follow; rather, it has tried to present contextualized portraits of the myriad ways that scientists themselves have answered Burhop's question. The purpose of these portraits is to illuminate how scientists have shaped Cold War policy (and vice versa), and to provide insights into ethical and philosophical questions relevant not only to Cold War historians, but to a wide professional audience, including today's practicing scientists and engineers.

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