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Containing Science: The U.S. National Security State and Scientists' Challenge to Nuclear Weapons during the Cold War

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Containing Science: The U.S. National Security State and Scientists' Challenge to Nuclear Weapons during the Cold War

by

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Containing Science: The U.S. National Security State and Scientists' Challenge to Nuclear Weapons during the Cold War

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Throughout the Cold War, many publicly influential and socially committed scientists participated in a wide array of efforts to push U.S. foreign policy toward nuclear disarmament. Some of these scientists, such as Linus Pauling and Carl Sagan, relied on their credibility as respected public authorities to sway public opinion against nuclear weapons. Other scientists, such as Eugene Rabinowitch, quietly pursued informal, quasi-diplomatic methods. Still others, such as Hans Bethe, George Kistiakowsky, and Jerome Wiesner, worked within the government to restrain the arms race. Though rarely working in concert, all these scientists operated under the notion that their scientific expertise enabled them to articulate convincing and objective reasons for nuclear disarmament. But the U.S. government went to great lengths to neutralize these scientific arguments against nuclear weapons with a wide array of tactics all aimed at undermining their scientific credibility. Some scientists who offered moral reasons to end the arms race found their loyalty questioned by the state. When prodisarmament scientists offered strictly technical reasons to

oppose to nuclear weapons, the government responded by promoting the equally technical objections to disarmament held by pronuclear scientists. At still other times, the government attempted to co-opt the arguments of its scientific challengers. In addition, scientists' professional identity as objective and apolitical experts hampered scientific antinuclear activism. From the beginning of the Cold War to the 1980s, scientists continuously challenged nuclear weapons in a variety of ways; the government likewise continuously reshaped its responses to meet this challenge, and in so doing crafted a method of scientific containment. Thus the result of this incessant struggle was the consistent defeat of scientists' dissent. By the time the Cold War ended, it did so on terms unrelated to scientists and nuclear weapons.

Table of Contents

Introduction

More Questions than Answers

At 9 a.m. on October 25, 1956, a reporter for the *Tonopah Times* of Tonopah, Nevada, placed a call to Linus Pauling to inform the Nobel Prize-winning chemist of the death of seven-year-old Martin Bordoli. Although Pauling had no connection to the boy, the reporter felt that the circumstances surrounding his death were "interesting," and sought Pauling's opinion. According to the reporter, residents of Twin Springs, Nevada, lived under clouds of radioactive fallout downwind from the U.S. nuclear testing site. When residents of the tiny town developed "sore eyes," the reporter told Pauling, "they were examined by [Atomic Energy Commission] doctors, who said there was nothing for them to worry about." The Bordoli family had also been "troubled by these sore eyes," and in spring of 1956 Martin developed leukemia; he passed away on October 24. Pauling admitted that "the circumstances were suspicious," and told the reporter that there was a chance that the leukemia had been caused by nuclear fallout. Almost as troubling to Pauling was the reporter's admission that her newspaper had been asked, "as a patriotic duty, not to publish anything about it."[1]

The reporter's decision to ask Pauling about young Martin Bordoli was not made randomly. During the 1950s Pauling had become a public authority on the dangers of nuclear testing. His iconoclastic pacifism distanced him from the U.S. government, and boosted his reputation as a source for information that challenged official policies on nuclear weapons. By the time of Martin Bordoli's death, Pauling had already begun a campaign for a

[1] LP to G. W. Beadle, Oct. 25, 1956, LP Peace 6: Other Peace Activism, 6.012: Pauling Peace Research Notes, 12.2: Assorted Pauling Peace Research Notes, 1950s, Ava Helen and Linus Pauling Papers, Oregon State University (hereafter LP Papers).

nuclear test ban to prevent the spread of fallout. But while journalists and other members of the public turned to Pauling for information, the U.S. government worked to ensure that the antinuclear messages of Pauling and other influential scientists were never heard, or at least diluted.

The government adopted many tactics to silence Pauling, but all of them involved questioning his credibility in some way. After he distributed an antinuclear petition, the Senate Internal Security Subcommittee brought Pauling to Congress. There, the assembled Senators asked him not about Martin Bordoli or the dangers of fallout, but rather who had helped him distribute his petition. "Will you tell us who did that work, the mimeographing?" he was asked. Other questions included, "Do you know, sir, who supplied the addresses of the scientists to whom letters were sent on that evening of May 15?" "Dr. Pauling, can you tell us what was the cost of printing the copies of your appeal?" and "Did you have arrangements with any individual in any foreign country to centralize the circulation of the petition in that country?" More pointed questions lay at the heart of the hearing, the point of which, in subcommittee vice-chairman Thomas J. Dodd's words, was to determine "Was this petition, in any sense, Communist propaganda? . . . Was there substantial Communist participation in the organization of the petition? . . . In the preparation and submission of the petition, were Communist techniques used?"[2] Pauling refused to answer, not because had any communist sympathies, but because as a fierce civil libertarian he considered such questions to be unconstitutional.

[2] U.S. Senate, Internal Security Subcommittee of the Committee on the Judiciary. *Testimony of Dr. Linus Pauling.* Part II, 86th Cong., 2nd sess. Hearing, October 11, 1960 (Washington, D.C.: U.S. Government Printing Office, 1960), 366, 414, 416, 419.

Meanwhile, scientists aligned with the national security state took it upon themselves to dispute Pauling's scientific claims. In spring 1958 the physicist Edward Teller, the most vocal scientific proponent of nuclear weapons, appeared on television to debate Pauling, who had publicly alleged that radioactive fallout from nuclear weapons tests would result in thousands of birth defects. Teller directly questioned Pauling's scientific evidence. "[T]his alleged damage which the small radioactivity is causing—supposedly cancer and leukemia—has not been proved, to the best of my knowledge, by any kind of decent and clear statistics," he stated. "It is possible that there *is* damage. It is even possible, to my mind, that there is *no damage;* and there is the possibility, furthermore, that very small amounts of radioactivity are helpful."[3]

Later in the debate, Pauling shifted gears and argued that the issue of fallout transcended science and lay in the realm of morality. In the consideration of nuclear weapons, "morality is a factor of prime importance," he said, emphasizing that "we should obey the commandment 'Thou Shalt Not Kill.'" But Teller seamlessly shifted to counter Pauling's moral argument and attempted to reframe the question under debate. A naturalized Hungarian, Teller made his own moral argument in favor of nuclear weapons. "I am not talking about these things calmly," he said. "I have feelings, I have strong feelings. Many people were killed [by communists] in Hungary, from where I came, and all people in Hungary lost their freedom." To Teller, a head scientist at the Livermore nuclear weapons lab and advisor to hawkish Senators and AEC officials, the morality at the heart of their debate was not about nuclear weapons but rather the moral imperatives of freedom and anticommunism. "This question of freedom is the most important question in my mind,"

[3] Transcript, "Fallout and Disarmament: The Pauling-Teller Debate," 3, 6, Publications, 1920–1998, Boxes 1956p–1959p, 1958p2: Publications of Linus Pauling, 1958, 1958p.2.1, LP Papers.

Teller averred. "I don't want to kill anybody. I am passionately opposed to killing, but I'm even more passionately fond of freedom." After their debate, Teller felt he had so effectively rebutted Pauling's antinuclear arguments on all fronts that he distributed numerous copies of the debate transcript to the public at his own cost.[4]

Containing Science

Pauling's encounters with reporters, congressional committees, and Edward Teller capture in microcosm the broad array of tactics the state used to resist the antinuclear actions of elite U.S. scientists.[5] The following dissertation demonstrates how the state successfully neutralized the substantial threat that scientists' dissent posed to the U.S. nuclear arsenal during the Cold War. It describes how some of the nation's most influential scientists engaged in activism for and against nuclear weapons during the Cold War, how the state constrained their capabilities for activism, how scientists' professional identity hindered scientific activism, and how some iconoclastic scientists transcended these barriers. It explains how these elite scientists went from being actively consulted on nuclear weapons by the state to *personae non gratae* within policymaking circles, as well as the decline among the public's interest in the "great scientist"—the expert who took it for granted that the government and the public needed to hear his views on political, ethical, and social issues. While some scientists undoubtedly continue to see themselves as providing this role, the government and the public appear to be less interested in hearing their views.

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[4] "Pauling-Teller Debate," 12; see for example ET to Norman Caine, June 2, 1958, Folder 1: Reading File, June–Oct. 1958, Box 420: Chronological Correspondence, 1956–58, Edward Teller Papers, Hoover Institution.

[5] The term "the state" is used here not to describe specific parties in power because different parties and administrations all resulted in a similar containment of science. Rather, the term is used as shorthand for the elected and nonelected officials of the executive and legislative branches, as well as military officials—in short, the complex of institutions and officials that make up the national security state and its nuclear policy, regardless of the individuals who inhabit it.

Historians have addressed this tension between scientists and government primarily by examining the life of Los Alamos scientific director J. Robert Oppenheimer, who continues to be hailed as "the acknowledged father of the A-bomb."[6] In these works, Oppenheimer's career—through his rise to fame and subsequent downfall after a 1954 security clearance hearing—embodies the course of U.S. science, where enlightened, scientific liberalism was overtaken by rigid Cold War nationalism. In fact, however, the nearly 40 years between the Oppenheimer hearing and the end of the Cold War were full of conflict between scientists and the national security state, as scientists' attempts to influence nuclear weapons policy and the government's concurrent resistance continuously reshaped power relations between science and the state. For the duration of the Cold War, I argue, the national security state incessantly enforced different criteria to fend off the challenges of antinuclear scientists, drawing a fluid but clear line between unacceptable and acceptable roles for scientists in the Cold War United States.

During the Cold War, scientists and the state struggled over the notion of objectivity; at stake was the direction of U.S. nuclear weapons policy. Just as the U.S. policy of containment adapted to confront communism in different geopolitical moments and contexts—economic aid in one region, military force in another, diplomacy elsewhere—so the government used a type of scientific containment, adapting its strategies to confront different scientific challenges, dissenters, and situations. At various times, the state confronted antinuclear scientists by questioning their loyalties, redefining what constituted proper scientific advice, confronting them with their scientific opponents, marginalizing

[6] Gerard DeGroot, *The Bomb: A Life* (Cambridge: Harvard University Press, 2005), 4. Robert Norris has convincingly argued that Manhattan Project director Leslie Groves was even more "indispensable" than Oppenheimer. See *Racing for the Bomb: Leslie R. Groves, the Manhattan Project's Indispensable Man* (South Royalton: Steerforth Press, 2002).

them as uninformed, challenging their credibility, co-opting their concerns as their own, or employing several of these tactics all at once. Underlying these different tactics was the simple question of whether scientists upheld or disputed the strengthening and enhancement of the U.S. nuclear arsenal. Scientists who opposed the continual reshaping of nuclear energy into newer and bigger weapons generally failed to influence policy. This power dynamic has often been described as the state's demand that scientists be "on tap," not "on top." But the government framed the issue as one of morality, loyalty, or objectivity, rather than a simple adherence to policy. When scientists gained—or threatened to gain—a degree of influence based on their expertise, the state responded by questioning scientists' objectivity, even though to be objective effectively meant not standing in the way of the nuclear arms race. Scientists' professional identities, conservative in nature, further restricted the actions of antinuclear scientists by implicitly assimilating the government's conception of the proper role of scientists.

In her study of U.S. scientists and anticommunism during the early Cold War, historian Jessica Wang has shown how the government weeded out many low-level scientists who fell too far to the left of the national security state.[7] A mountain of books and articles on Oppenheimer and his ouster from government have described the culmination of these Red Scare loyalty purges on the era's most famous scientist. But historians have not looked at how these purges and redefinition of scientists' roles played out during the rest of the Cold War. The following dissertation explains how the state fended off the continuous antinuclear challenges of those prominent U.S. scientists whose public stature or political clout allowed them to challenge U.S. policy.

[7] *American Science in an Age of Anxiety: Scientists, Anticommunism, and the Cold War* (Chapel Hill: University of North Carolina Press, 1999).

Organization

This dissertation takes as its subjects the scientists at the top of the scientific hierarchy, concentrating on those individuals who stood out and spoke most effectively and influentially for and against nuclear weapons from both in the government and out. These men include Nobel laureates Linus Pauling and Hans Bethe, presidential advisors George Kistiakowsky and Jerome Wiesner, celebrity scientist Carl Sagan, antinuclear advocate Eugene Rabinowitch, and government insider Edward Teller. The reason for this approach is that these most famous scientists directly engaged the state and had the capacity to shape public perceptions of science and nuclear weapons. Influential with the public and other scientists, these men took it upon themselves to act and speak for "science"; they were the focus of government actions, and helped shape conceptions about the proper role of scientists in U.S. government and society. This dissertation does not, therefore, take up the study of a broad social movement of scientists, nor does it provide a social history of the rank and file of U.S. Cold War scientists. Instead it examines the notable scientists whose actions and beliefs directly shaped the relationship between scientists and the state by speaking to each other, the government, and the public about the role and responsibility of scientists in a geopolitical system ruled by nuclear weapons.

The structure of this dissertation reflects this focus. Several chapters are devoted to biographical studies of certain scientists to best explain the influence, ideologies, and constraints at work. By directly participating in and helping determining the outcome of the struggle over scientific authority and U.S. nuclear policy, individuals like Teller, Sagan, and Pauling merit extended analysis. At other times, the focus of the dissertation draws back

from individuals to look more broadly at the scientific and political contexts of public debates over nuclear weapons. Thus certain chapters look at specific moments such as the Oppenheimer hearing, the nuclear test ban debate, and the antinuclear uprising of the 1980s, when this power elite of science interacted with the public and the state, changing the relationship between the government, science, and society. Finally, portions of several chapters provide a cursory glance at equivalent relationships between scientists, the antinuclear movement, and the government in the United Kingdom. While not an exhaustive international history by any means, this comparison will allow the reader to get a feel for how similar power relations played out across the Atlantic.

Because scientists' activism between 1945 and 1947 is well documented and the Oppenheimer hearing so exhaustively studied, I summarize them both in the opening chapter. This first chapter goes on to discuss the aftermath of the Oppenheimer hearing, when the Atomic Energy Commission mandated that government science advisors must, above all else, uphold "the protection of the strongest offensive military interests of the country."[8] Whereas before 1954 scientists had debated the moral dimensions of nuclear weapons, the Oppenheimer hearing confirmed that scientists who advocated disarmament for moral reasons would be viewed with suspicion. If scientists wished to pursue disarmament through government policymaking, they could object to nuclear weapons only on narrow technical grounds. Technical arguments against nuclear weapons became the new language of dissent, in the process limiting the possible outcomes for scientists' opposition to the arms race. This shift from moral concerns to strictly technical advice was part of the contested but successful establishment of the deterrence consensus by the early 1960s.

[8] U.S. Atomic Energy Commission, *In the Matter of J. Robert Oppenheimer: Transcript of Hearing before Personnel Security Board and Texts of Principal Documents and Letters* (Cambridge: MIT Press, 1970), 1016–18.

Though the state bears primary responsibility for implementing this new regime, previous historians have failed to recognize that scientists themselves also bear some responsibility for accepting the government's restricted identity of them.

Much less studied than the period between 1945 and 1954 is scientists' activism during the remainder of the Cold War. Chapters Two and Three contrast the campaigns of chemist Linus Pauling and physicist Edward Teller against and for nuclear testing in the 1950s. Pauling's "morality of science" offered a powerful antinuclear message that inspired activists in the United States and overseas. Surprisingly, after the state's elaborate redefinition of morality during the Oppenheimer hearing, the state responded to Pauling's moral crusade not by contesting his claims on moral grounds, but with the use of Red Scare tactics that smeared the chemist as disloyal. Though many ordinary U.S. citizens looked to Pauling for guidance on nuclear weapons, the government acted to ensure that his efforts had as little influence as possible. Meanwhile, the mixed scientific reaction to Pauling's antinuclear efforts shows how many elite U.S. scientists embraced a professional identity that tacitly accepted the nuclear deterrent and explicitly refused to engage with the powerful political and social implications of science. In contrast, scientists' role in Britain's antinuclear movement was far less controversial, but also far less effective at mobilizing the public.

Physicist Edward Teller, the subject of Chapter Three, was close to a polar opposite of Pauling. A vocal proponent of nuclear weapons development, Teller aligned himself with the national security state during the 1950s. Relying on political influence and Red Scare tactics, he neutralized his scientific opponents, Pauling among them. With his avowed anticommunism, Teller portrayed nuclear weapons and deterrence as moral responses to atheistic and domineering communism, and saw nuclear weapons as a fundamentally moral

way to defend U.S. interests against the Soviets. With his efforts, he secured for himself a prized place within the state, winning funding for his weapons laboratory and other scientific interests. Teller shared the government's desire for the strongest offensive military posture of the nation and in some ways acted as a scientific enforcer of the government mandate of the Oppenheimer hearing, not least because Teller himself played a direct role in the hearing's outcome.

Chapter Four examines the collision of these and other scientists' ideologies and actions in the debate over a nuclear test ban treaty. The chapter begins by looking at scientists who attempted to sway nuclear policy in the United States and Britain in favor of a test ban from within the government as advisors, and from just outside the government as influential but unofficial diplomats at the Pugwash conferences of scientists. The chapter culminates in the Congressional debate over the 1963 Limited Test Ban Treaty, which reveals how most elite scientists in and out of government attempted to maneuver within the realms of acceptable dissent as defined by the state. Moral arguments for the test ban, especially those expressed by Pauling, were left outside the debate, even though such moral activism mobilized the public to a great extent. An analysis of the test ban's creation reveals the fairly ineffectual nature of the technical approach to antinuclear activism: Since achievement of a comprehensive test ban depended on scientists' technical ability to implement reliable detection techniques, the failure to achieve this technical breakthrough resulted in the test ban's degradation into a limited measure that did little to slow the arms race. Furthermore, public disputes between scientists over the test ban harmed scientists' image as objective experts with clear answers to the nuclear dilemma. Thus these first four chapters allow for a comparison of the technical arguments of government scientists with

Pauling's moral message and with Teller's political advocacy of nuclear weapons in the guise of a scientist.

Chapter Five examines the deteriorating relationship between scientists and U.S. and British policymakers during the 1960s and the ensuing shift in scientists' antinuclear efforts. After 1963, antinuclear scientists debated the best way to follow through on the test ban. Pugwash scientists, for example, continued to attempt to solve the nuclear dilemma through transnational cooperation. This loose coalition of scientists fractured over the Vietnam War, however, and when they attempted to retain their authority and influence by applying their expertise to ending the war, the results had the opposite effect. As the United States and Soviet Union began to pursue détente during the 1970s, this transnational scientists' movement became unnecessary. And as the 1970s wore on, the U.S. and British governments continued to put distance between scientists and policymakers.

Chapters Six and Seven examine the revived scientific activism against nuclear weapons in the 1980s, as the deterrence consensus forged in the 1960s began to break down. The first of these final two chapters approaches Carl Sagan's nuclear winter theory as a reprise of the Teller-Pauling clash of the 1950s. Sagan, like Pauling, offered scientific evidence of impending nuclear disaster; as before, Teller rushed to confront this scientific challenge to the arms race. Whereas Pauling had predicted widespread birth defects and even deaths, Sagan's nuclear winter theory went even further by treating the extinction of humanity as a possible outcome of a nuclear war. Nevertheless, Sagan's ability to connect to the greater public, combined with the decline of rabid anticommunism, allowed his theory to

be taken more seriously by the U.S. government, especially Congress.[9] But nuclear winter's power as an antinuclear argument was severely undermined by many scientists' concerns about what constituted the proper behavior of a scientist. Scientists who criticized the nuclear winter theory complained that it was political and not scientific in nature, but these complaints were themselves fundamentally political statements. Sagan's opponents objected to the antinuclear motivation behind nuclear winter more than the science itself, showing that the conservative professional identity of scientists acted as a barrier to activism that privileged scientists' image as objective experts over the social responsibilities of science.

The final chapter focuses on the scientific aspects of debate over the Nuclear Freeze and the Strategic Defense Initiative (SDI) in the 1980s. The "Star Wars" program, ceaselessly promoted by Teller, served as a conservative response intended to co-opt the nuclear fears and moral urgency of the antinuclear movement. Teller's continued influence over nuclear weapons policy shows that scientific advice was often heeded—as long as it advocated newer, bigger nuclear weapons. Meanwhile, the massive antinuclear movement of the 1980s was largely based on moral arguments against nuclear war and Cold War bipolarity, including those based in religion, feminism, and human rights. Scientists who insisted on clinging to their nuanced technical objections to nuclear weapons found themselves left behind by the mainstream movement. Many scientists did contest SDI development and others even shifted to a moral critique of nuclear weapons, but for the most part, scientists' professional identity reinforced, rather than challenged, the national security state. When the Cold War

[9] My conclusions regarding Carl Sagan must be taken tentatively. Sagan's personal papers are not open to the public, and his estate refused direct access to them.

ended, the state had fended off scientific challenges long enough; for the United States, the Cold War concluded on terms unrelated to science and scientists.[10]

Historiography

In the long history of interactions between modern science, states, and society, two traditions can be roughly perceived. The most common is that of science and scientists serving the state, either willingly or under duress. The Manhattan Project is only the most famous example, when scientists rushed during World War II to build the first atomic weapon before Nazi Germany did. Although nuclear weapons scientists receive most of the attention, scientists of other disciplines have also lent their minds to destructive purposes. As U.S. scientists raced to build an atomic bomb, Japanese biological experts across the Pacific conducted research on biological weapons. Stationed in Japanese-occupied Manchuria, Shiro Ishii "experimented" with biological warfare techniques on thousands of Chinese civilians, tortures that included the injection of deadly viruses into captives, live dissections of humans, and the poisoning of entire villages, all dispassionately observed for research purposes.[11] The Japanese were not alone; as sociologist Jeanne Guillemin points out,

> The capacity of scientists to set aside moral scruples is abundantly illustrated in the history of biological weapons in the last century, when thousands of microbiologists were employed in secret state programs that defied international norms and laws protecting civilians in war. . . . Very few of these government-employed scientists

[10] In the Soviet Union, the human rights activism of scientists such as Andrei Sakharov did a great deal to inspire the internal events that dissolved the Soviet Union. See Sarah B. Snyder, "The Helsinki Process, American Foreign Policy, and the End of the Cold War," Ph.D. diss., Georgetown University, 2006.

[11] See Michael Bess, *Choices Under Fire: Moral Dimensions of World War II* (New York: A.A. Knopf, 2006), 273–75; as well as Daniel Barenblatt, *A Plague Upon Humanity: The Secret Genocide of Axis Japan's Germ Warfare Operation* (New York: HarperCollins, 2004); and Sheldon H. Harris, *Factories of Death: Japanese Biological Warfare, 1932–45, and the American Cover-up* (New York: Routledge, 1994).

> ever recanted their dedication to helping infect masses of civilians with anthrax, tularemia, plague, smallpox, and other diseases.[12]

Similarly, well before the Manhattan Project, chemists in Western nations rushed to serve their respective states at the outbreak of World War I. In the United States alone, 5,500 scientists and technicians worked on concocting deadly chemical weapons.[13]

Scientists in autocratic societies found themselves even more subject to the state's demands. Between the world wars, government authorities in the Soviet Union declared that science was the property of the state. For years after, the flawed biology of Tryfim Lysenko, conferred with the seal of state approval as properly socialist, perverted and stunted Soviet science.[14] But as scholars of science have pointed out, World War II culminated changes wrought by war and depression during the twentieth century by embedding science at the heart of the political process. Scientists and engineers came to be seen as a resource of all states, not just in the Soviet Union.[15]

Outside of totalitarian states, many scientists have resisted serving the state's military needs, establishing a second scientific tradition: that of serving society and the greater good from inside and outside the auspices of the state. Historian of science Peter Kuznick has shown that groups of U.S. scientists, shaken by the Great Depression, mobilized behind many progressive causes during the 1930s.[16] Much of the inspiration for this scientific

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[12] Jeanne Guillemin, "Seduced by the State," *Bulletin of the Atomic Scientists*, September/ October 2007, 14–16; and Guillemin, *Biological Weapons: From the Invention of State-sponsored Programs to Contemporary Bioterrorism* (New York: Columbia University Press, 2005).
[13] L.F. Haber, *The Poisonous Cloud: Chemical Warfare in the First World War* (Oxford: Oxford University Press, 1986), 106–07.
[14] See Ethan Pollock, *Stalin and the Soviet Science Wars* (Princeton: Princeton University Press, 2006). Douglas Wiener, *A Little Corner of Freedom: Russian Nature Protection from Stalin to Gorbachev* (Berkeley: University of California Press, 1999), argues convincingly that Soviet environmental scientists were able to resist state control of their discipline.
[15] John Krige and Kai-Henrik Barth, "Introduction: Science and Technology in International Affairs: New Approaches," in Krige and Barth, eds., *Global Power Knowledge: Science and Technology in International Affairs*, Osiris 21, 2.
[16] Peter J. Kuznick, *Beyond the Laboratory: Scientists as Political Activists in 1930's America* (Chicago: University of Chicago Press, 1988).

activism came from the United Kingdom in the years following the 1939 publication of crystallographer J.D. Bernal's *Social Function of Science*. Bernal's volume advocated the Marxist-inspired notion that, in the words of his biographer, "science should be centrally planned [by the state] to meet the needs of society." This call to action inspired the "scientists for social responsibility movement" in the United Kingdom, an activist undertaking that advocated using science to help society and instituting a mimicry of the Soviet state structure in the West. One of the most famous of the Bernalists was P.M.S. Blackett, who pursued science and socialism with equal enthusiasm.[17]

Other notable scientists embraced a commitment to society in different ways. Albert Einstein, perhaps the best-known scientist of all time, notably embraced pacifism. In 1929, Einstein famously remarked: "I would unconditionally refuse all war service, direct or indirect, and I would seek to persuade my friends to adopt the same position, regardless of how I might feel about the causes of any particular war" (though Einstein later made an exception for Nazi Germany).[18] And in 1945, even before the use of atomic bombs on Hiroshima and Nagasaki, some Manhattan Project scientists attempted to prevent use of the bombs. Despite their failure, this activism continued after the war as Manhattan Project veterans launched the Atomic Scientists' Movement, a flurry of mobilization aimed at preventing military control of atomic energy in the United States, as well as encouraging world government and international control of atomic weapons.[19]

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[17] J.D. Bernal, *The Social Function of Science* (London: G. Routledge and Sons, 1939); Andrew Brown, *J.D. Bernal: The Sage of Science* (New York: Oxford University Press, 2005), 436; Mary Jo Nye, *Blackett: Physics, War, and Politics in the Twentieth Century* (Cambridge: Harvard University Press, 2004); William McGuken, *Scientists, Society, and State: The Social Relations of Science Movement in Great Britain, 1931–1947* (Columbus: Ohio State University Press, 1984); and Gary Werskey, *The Visible College: The Collective Biography of British Scientific Socialists of the 1930s* (New York: Holt, Rinehart & Winston, 1979).
[18] Quoted in Banesh Hoffman, *Albert Einstein: Creator and Rebel* (New York: Plume, 1972), 155–57, 169.
[19] Alice Smith, *A Peril and a Hope: The Scientists' Movement in America, 1945–47* (Chicago: University of Chicago Press, 1965).

Regarding nuclear weapons in particular, numerous authors have written about the ways in which scientists confronted these swords of Damocles. In particular, a slew of recent books focus on Oppenheimer and his role in shaping nuclear policy.[20] By the time the government dismissed Oppenheimer in 1954, these authors argue, the Red Scare had effectively muted scientists' calls to hedge the arms race, and scientists had lost their place in policy circles. Oppenheimer's biographers assert that his hearing was the end of moral considerations and scientists' dissent, but they have supported this assertion with speculation rather than research. David Cassidy has written that the "thoughtful dissenter" all but disappeared from government after the Oppenheimer hearing; Priscilla McMillan has argued that because of the hearing, "the scientist is less and less likely to speak out against government policies." Finally, Kai Bird and Martin Sherwin have declared that "Scientists working within the system could not dissent from government policy."[21] Although Oppenheimer's ouster from government qualifies as sinister, it was not the end of scientists' dissent against nuclear weapons. I find this approach flawed, especially the exaggerated emphasis on Oppenheimer. Elite scientists who shared Oppenheimer's ambivalent (and often contradictory) views of nuclear weapons continued to attempt to influence nuclear

[20] Eleven books on Oppenheimer have been published since 2002, by Jeremy Bernstein, *Oppenheimer: Portrait of an Enigma* (New York: Ivan R. Dee, 2005); Kai Bird and Martin Sherwin, *American Prometheus: The Triumph and Tragedy of J. Robert Oppenheimer* (New York: Knopf, 2005); Cathryn Carson and David Hollinger (eds.), *Reappraising Oppenheimer: Centennial Studies and Reflections* (Berkeley: Office for History of Science and Technology, University of California at Berkeley, 2005); David Cassidy: *J. Robert Oppenheimer and the American Century* (New York: Pi Press, 2005); Jennet Conant, *109 East Palace: Robert Oppenheimer and the Secret City of Los Alamos* (New York: Simon and Schuster, 2005); Gregg Herken, *Brotherhood of the Bomb: The Tangled Lives and Loyalties of Robert Oppenheimer, Ernest Lawrence, and Edward Teller* (New York: Henry Holt, 2002); Priscilla McMillan, *The Ruin of J. Robert Oppenheimer and the Birth of the Modern Arms Race* (New York: Viking, 2005); Abraham Pais, *J. Robert Oppenheimer: A Life* (New York: Oxford University Press, 2006); Charles Thorpe, *Oppenheimer: The Tragic Intellect* (Chicago: University of Chicago Press, 2006); and S.S. Schweber's *In the Shadow of the Bomb: Oppenheimer, Bethe, and the Moral Responsibility of the Scientist* (Princeton: Princeton University Press, 2000), and *Einstein and Oppenheimer: The Meaning of Genius* (Cambridge: Harvard University Press, 2008). In addition, an opera by Peter Sellars and John Adams, *Dr. Atomic,* and a novel by Lydia Millet, *Oh Pure and Radiant Heart*, have brought fictionalized Oppenheimers into being. Sellars also announced his plans to work on a sequel to *Dr. Atomic*, stating that Oppenheimer is "an overwhelming figure of dramatic history who we will be arguing about as long as there is civilization." "The Bulletin Interview: Peter Sellars," *Bulletin of the Atomic Scientists*, March/ April 2008, Vol. 64, No. 1, 13.

[21] Cassidy, *J. Robert Oppenheimer and the American Century*, 334; McMillan, *Ruin of J. Robert Oppenheimer*, 264; Bird and Sherwin, *American Prometheus*, 549.

policy throughout the Cold War. As early as 1957, Dwight Eisenhower began to listen to scientists other than AEC hawks like Edward Teller and Lewis Strauss by creating a presidential science advisory committee to help him pursue nuclear disarmament. In fact, scientists had to choose whether they would accept the government's mandate to strengthen the nuclear deterrent. Scientists' acceptance of the government's terms was not a foregone conclusion, and it faced challenges by scientists such as Linus Pauling. Finally, scholars wish to endow Oppenheimer with qualities he simply did not possess, including opposition to nuclear weapons and a concern for society grounded in the Ethical Culture credo. Charles Thorpe, in an otherwise excellent biography of Oppenheimer, credits the physicist with being the "last" public-scientific intellectual. What then, are we to make of Carl Sagan, who explained the joys and skepticism of science to millions of people and defended science's claim to truth against legions of pseudo-scientists, UFO fanatics, and a generation of Americans wary of science and technocracy?[22] In addition, during the recent developments in biology and genetics as well as the heated debates between scientists and creationists, a number of biologists have played a large public role, including Richard Dawkins, the late Steven Jay Gould, and James Watson.

In contrast to the Oppenheimer-centric approach, the following work examines a wide spectrum of elite scientists in and out of government, both against and for nuclear weapons, providing a more nuanced look at the way scientists interacted with society, government, and each other during the Cold War. Other works fail to explore the issue in adequate perspective, either flippantly dismissing scientists' efforts for disarmament or

[22] Thorpe, *Oppenheimer: The Tragic Intellect*, Chicago: University of Chicago Press, 2006, Chapter Eight, 243–89. Thorpe does qualify his label with a question mark: "The Last Intellectual?"

ignoring the role of scientists outside the government.[23] Analyzing presidential science advisors, for example, makes little sense without an examination of government outsiders like Pauling. Science advice took place in a continuum, not a vacuum, where the far left (Pauling) and the far right (Teller) made the claims of many government science advisors appear more reasonable. Finally, scholars who argue that scientists had little influence on policy focus exclusively on antinuclear scientists. In fact, Teller effectively drove U.S. policy in favor of nuclear weapons development for decades.

This study also addresses the historiography of U.S. foreign relations, as the elite scientists under investigation were attempting to drastically—some of them fundamentally—reshape U.S. Cold War policy. Scientists who resonated with grassroots antinuclear movements, such as Pauling and Sagan, were challenged by the government, while scientists inside government circles distanced themselves from protest movements. Despite scientists' established role as antinuclear experts, when the great upsurge of antinuclear activism began in the 1980s, most veteran antinuclear scientists were more likely to be followers than leaders of this movement, if they participated at all.

In recent years, scholars have begun to recognize the deep interplay between political protesters, elites, and foreign relations, in particular the ability of activists to influence foreign policy.[24] Of special concern to scholars have been activists who successfully altered U.S. policy by internationalizing their protest, in the process transcending borders and the nation-state system. Thomas Borstelman, Mary Dudziak, and Penny von Eschen have

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[23] See, for example, DeGroot, *The Bomb*; Gregg Herken's *Cardinal Choices: Presidential Science Advising from the Atomic Bomb to SDI* (Stanford: Stanford University Press, 2000); and Benjamin P. Greene, *Eisenhower, Science Advice, and the Nuclear Test-Ban Debate, 1945–1963* (Stanford: Stanford University Press, 2007).
[24] See Jeffrey Knopf, *Domestic Society and International Cooperation: The Impact of Protest on U.S. Arms Control Policy* (New York: Cambridge University Press, 1998), for a theoretical approach.

explained the interconnections between civil rights activism in the United States and the waging of the Cold War abroad. Civil rights activists contrasted the differences between the expressed values of the United States in waging the Cold War abroad with the Jim Crow treatment of African Americans at home in order to put pressure on policymakers for civil rights change. At the same time, politicians often achieved civil rights measures by justifying them in Cold War terms.[25] Meanwhile, Akira Iriye has argued that such efforts have not been limited to grassroots activists. According to Iriye, international non-government organizations (NGOs) have often bypassed the nation-state system in order to create a "global community" that has influenced geopolitical affairs in a positive way.[26] Other scholars have looked at how transnational protestors altered the foreign policies of both the United States and the Soviet Union. Matthew Evangelista, Sarah Snyder, and Lawrence Wittner have shown how a potent mix of transnational NGOs, diplomats, and social movements forced Cold War leaders toward arms control, human rights measures, and coexistence; these peaceful protest movements led the Cold War away from conflict, shook the foundations of the nation-state system, and spurred the tectonic shifts and grassroots uprisings that forced an end to the Cold War.[27]

In contrast to these works, the following study shows how the U.S. state managed to contain a related aspect of this turmoil: scientists' challenge to nuclear weapons. The state effectively resisted this challenge alternately by muting protest, or adapting only slightly in

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[25] Thomas Borstelmann, *The Cold War and the Color Line: American Race Relations in the Global Arena* (Cambridge: Harvard University Press, 2001); Mary L. Dudziak, *Cold War Civil Rights: Race and the Image of American Democracy* (Princeton: Princeton University Press, 2000); and Penny von Eschen, *Race against Empire: Black Americans and Anticolonialism, 1937–1957* (Ithaca: Cornell University Press, 1997).
[26] Akira Iriye, *Global Community: the Role of International Organizations in the Making of the Contemporary World* (Berkeley: University of California Press, 2002).
[27] Matthew Evangelista, *Unarmed Forces: The Transnational Movement to End the Cold War* (Ithaca: Cornell University Press, 1999); Snyder, "The Helsinki Process"; Lawrence Wittner, *The Struggle Against the Bomb* (Stanford: Stanford University Press, three volumes: 1993, 1997, 2003).

order to prevent truly dramatic change. This view of protest and state power harmonizes with historian Jeremi Suri's study of the 1968 revolutions that explains how international protest movements entrenched, rather than erased, the global dominance of the Cold War superpowers. According to Suri, world leaders from Washington to Paris to Berlin felt threatened by the momentous disruptions of 1968. But rather than create a new world order, the protests fulfilled the law of unintended consequences: Cold War leaders thwarted the attempted revolution by establishing détente, an ostensibly peaceful measure that in fact reinforced the existing geopolitical order and enabled states to avoid major change.[28] Similarly, scientists' attempts to oppose nuclear weapons from within the government managed not to overturn Cold War nuclearism, but only to strengthen it. Those scientists who attempted to work for disarmament outside the government found themselves policed by their peers who had subconsciously (and occasionally willingly) adopted the state's conception of power.

This dissertation also has much to say about the deep connections between science and foreign policy of concern to historians of the Cold War ever since Martin Sherwin detailed the clash between the atomic scientists and policymakers that forged the nuclear age in the crucible of World War II.[29] Foremost, this dissertation heeds the call of historian of science John Krige and political scientist Kai-Henrik Barth to "draw attention to the immense importance the history of science and technology has for our understanding of international affairs" and to "build a bridge between historians of science and technology and diplomatic and economic historians."[30] In his own work Krige has characterized science

[28] Jeremi Suri, *Power and Protest: Global Revolution and the Rise of Détente* (Cambridge: Harvard University Press, 2003).
[29] Martin Sherwin, *A World Destroyed: Hiroshima and its Legacies* (Stanford: Stanford University Press, 2003, third edition).
[30] Krige and Barth, "Science and Technology in International Affairs," 5.

as one of the tools used by the United States in the postwar world to institute U.S. hegemony overseas. Especially in Europe, Krige writes, science "became enrolled in a hegemonic postwar American project," one that reconfigured the European scientific landscape as part of its larger project of building the Atlantic community under U.S. leadership and values. Science was part of, and used for, the projection of American power in postwar Europe.[31] My work does not challenge this analysis, but builds on it: the ways in which science was used by U.S. scientists and the state resulted from the efforts of individuals. The state's ability to harness science to its interests occurred because men like Edward Teller acted to link science to national security, but not without a fight from scientists like Linus Pauling.

The analysis of the 1963 Limited Test Ban Treaty in Chapter Four increases understanding of this influential agreement and elaborates on its significance for science and society. The test ban debate took place as public opinion and the broad spectrum of scientific opinion, respectively led and inflamed by Pauling, raged over conflicting views of the threat of nuclear weapons tests. Numerous historians have analyzed the role of scientists during this crucial time period. In his influential work on the test ban, Robert Divine examined the Eisenhower administration during the public test ban debate, arguing that scientists and the public desired a test ban not because it would reduce fallout, but as a "magic talisman" that would subconsciously ease fears of nuclear war. According to Divine, support of a test ban was actually protest by proxy—a way to focus the generalized fear of nuclear war on a concrete achievable goal. Although incisive, Divine frames his work as a study of Eisenhower, concluding that "Dwight D. Eisenhower must be credited with making

[31] Krige, *American Hegemony and the Postwar Reconstruction of Science in Europe* (Cambridge: MIT Press, 2006), 2–14.

a sincere effort to use the test ban as a first step toward genuine disarmament."[32] Likewise, historian Benjamin Greene has studied the test ban in the context of Eisenhower's decision making. Greene argues that Eisenhower allowed his test ban policy "to become the captive of those who controlled the presentation of scientific advice within his administration." But while Greene's work skillfully examines Eisenhower's decision-making style, his conclusion—that Eisenhower "merits praise" for his determination to seek a test ban—remains limited to the tired debate over Eisenhower Revisionism.[33] By looking solely at scientists in the government and limiting their scope to a critique of Eisenhower's leadership style, Greene and others fail to see how science advice and political dissent were constrained and contested in the 1950s.

The biographical aspects of this dissertation also provide long-overdue and less-simplistic analyses of Linus Pauling, Hans Bethe, and Carl Sagan. In his study of public conceptions of nuclear weapons and energy, Spencer Weart describes Pauling as a crafter of "finely tuned sentences" and "an irresistible persuader" who "cast intellectual caution aside and presented his conclusions as obvious truths." More "publicist" than scientist, Weart argues, Pauling ventured well beyond mainstream scientific opinion and "boasted a precision that conventional scientists would never dare to claim."[34] In fact Pauling's activism was not hampered by its failure to adhere to some idealized scientific "objectivity," but rather by other scientists' fierce devotion to this same mythic notion as well as the anti-communism of

[32] Robert A. Divine, *Blowing on the Wind: The Nuclear Test Ban Debate, 1954–1960* (New York: Oxford University Press, 1978), 318.
[33] Greene, *Eisenhower*, 1, 256, 261; on Eisenhower revisionism, see also Martha Smith-Norris, "The Eisenhower Administration and the Nuclear Test Ban Talks, 1958–1960: Another Challenge to 'Revisionism,'" *Diplomatic History* 27 (September 2003): 503-41; Jeremi Suri, "America's Search for a Technological Solution to the Arms Race: The Surprise Attack Conference of 1958 and a Challenge for 'Eisenhower Revisionists,'" *Diplomatic History* 21 (Summer 1997): 417–51; and H.W. Brands, "The Age of Vulnerability: Eisenhower and the National Insecurity State," *American Historical Review* 94 (October 1989): 963–89, which all speak to Eisenhower's "hidden hand" leadership style first put forth by Fred Greenstein. See Stephen G. Rabe, "Eisenhower Revisionism: A Decade of Scholarship," *Diplomatic History* 17 (Winter 1993): 109–13.
[34] Spencer Weart, *Nuclear Fear: A History of Images* (Cambridge: Harvard University Press, 1988), 203, 212, 214.

the U.S. government. Treatments of Bethe, though few in number, have tended toward the celebratory. In particular, S.S. Schweber's short biography found that, unlike Oppenheimer, Bethe embodied the "moral responsibility" of the scientist.[35] But many scientists would benefit from comparison with the dislikable Oppenheimer. The placement of Bethe's beliefs and actions within a broad spectrum of scientists, as this dissertation achieves, illuminates a more nuanced portrait of Bethe. Though possessing a deep moral conviction against nuclear war, Bethe's decision to work for disarmament as a government insider often forced him to support policies dedicated to maximizing U.S. nuclear strengths. Finally, Sagan has been the subject of only limited academic scholarship, perhaps because his personal papers remain inaccessible to the public. Despite this handicap, I attempt to bring this influential public figure into the study of scientists and the Cold War.

The government's suppression of antinuclear arguments rooted in political beliefs among its science advisors had great consequences for the state, science, and society, not least because such a distinction was blind to the realities of the scientific discipline. In the early twentieth century, the loose coalition of reformers known as the Progressives made moral arguments for government policy on everything from sanitation to prohibition to suffrage. But scientific and social scientific expertise was always the basis of Progressive reform initiatives, as evidenced by arguments ranging from the statistical nature of the Brandeis Brief to the psychological evidence used in *Brown v. Board of Education*. Thus the U.S. government's 1954 demand for scientific advice of a strictly technical nature did not come out of thin air. At the same time, however, scientists' socially-formed inclinations to let political concerns guide their technical advice had to be eradicated, as a distinction between

[35] Schweber, *In the Shadow of the Bomb.*

the two, while clear, had never been explicitly enforced. And natural and physical scientists were not alone in this new restriction: social scientists also grappled with their connections to the Cold War and U.S. militarism, a conflict that reflected a deep tension between knowledge and social action that challenged their scientific approach to understanding and reforming social relations.[36]

Works in the history of science have shown how closely the supposedly objective U.S. natural sciences (physics in particular) became influenced by and dependent upon the economic, political, and military elements of U.S. society. For example, the Department of Defense became science's greatest single patron in the ten years after World War II, a relationship that changed little during the remainder of the Cold War. As historian of science Stuart Leslie has written, "the military-driven technologies of the Cold War defined the critical problems for the postwar generation of American scientists and engineers."[37] While the close association between the Pentagon and U.S. science is a theme of this dissertation, just as important is the analysis of scientists' own role in the militarization of science. Innovative works in the history, sociology, and anthropology of science have begun to explain how scientists themselves participated in or resisted the militarization of science. Anthropologist Hugh Gusterson has explained the mindset of weapons scientists: they willingly accepted the philosophy of deterrence, yet were also constrained by the laboratory-security complex in which they worked. Sociologist Kelly Moore has shown how scientists actually worked against their own self-interests and reduced their own public authority by

[36] See Dorothy Ross, *The Origins of American Social Science* (New York: Cambridge University Press, 1991); Mary Furner, *Advocacy and Objectivity: A Crisis in the Professionalization of American Social Science, 1865–1905* (Lexington: University Press of Kentucky, 1975); Edward A. Purcell Jr., *The Crisis of Democratic Theory: Scientific Naturalism and the Problem of Value* (Lexington: University Press of Kentucky, 1973); and Mark C. Smith, *Social Science in the Crucible: The American Debate over Objectivity and Purpose, 1918–1941* (Durham: Duke University Press, 1994).

[37] Leslie, *The Cold War and American Science*, 9. See also Daniel J. Kevles, *The Physicists: The History of a Scientific Community in Modern America* (New York: Knopf, 1977).

disrupting science in the 1960s and 1970s, when activists disillusioned with the use of science for violent purposes in Vietnam revealed the militaristic, misogynist, and elitist nature of science allied with the Pentagon. Finally, historian of social science Joy Rohde has detailed the skein of authority, objectivity, and Defense Department funding that entangled U.S. social science during the Vietnam War era.[38] In harmony with these works, I demonstrate how elite natural and physical scientists of all political beliefs (save apathy) reconfigured themselves within the intersections of science, politics, and the national security state.

Lukes and the Three-Dimensional Model of Power

In order to inform the discussion of the relationship between science and the state, the study undertaken here is framed by the three-dimensional model of power first proposed by sociologist Steven Lukes. In his landmark study *Power: A Radical View,* Lukes sought to explain how willing compliance to domination is secured by power holders. As an answer, Lukes proposed a three-dimensional model that takes into account the fact that power is not always reflected in concrete, observable decisions or actions.

The people and events of this dissertation reveal the three dimensions of Lukes's process at work. The first dimension of power is straightforward: observable political behavior is tallied, and a winner and a loser are determined. In Lukes's terms, the power-holder, A, can and does get B to do something B would not otherwise do, or that A simply

[38] Hugh Gusterson, *Nuclear Rites: A Weapons Laboratory at the End of the Cold War* (Berkeley: University of California Press, 1996); Kelly Moore, *Disrupting Science: Social Movements, American Scientists, and the Politics of the Military, 1945–1975* (Princeton: Princeton University Press, 2008); Joy Rohde, "Counterinsurgency on Contract: Project Camelot, Social Science, and American National Security in the Cold War," Ph.D. diss., University of Pennsylvania, 2007.

makes decisions that affect B.[39] This type of power is essentially force or coercion, and was at work very clearly in the Oppenheimer hearing (as well as earlier, less publicized security clearance hearings) and the conflicts between Pauling and Teller, when the state punished those who did not adhere to its rules and rewarded those who did. Oppenheimer biographer Charles Thorpe has characterized the Oppenheimer hearing as a "degradation ceremony," while more mundane language would simply say that the AEC made an example of Oppenheimer.[40] Either way, the hearing instituted the state's forcible exclusion of certain political views by refusing access to science advisors who did not pledge to uphold and strengthen the nuclear deterrent. Pauling, for example, found himself slandered as a Red for his antinuclear stand, while Teller attained easy access to government officials.

The second dimension of power takes into consideration the ways in which the use of power stifles potential conflicts. In addition to A making decisions that affect B (as in the first dimension), A can also create or reinforce barriers to the public airing of policy conflicts; Lukes calls this process "organization as the mobilization of bias." This dimension therefore explains decision making as well as *non*-decision making, when demands for change are smothered before they are even voiced. The result is that these demands are either kept covert, killed before they enter the decision making arena, or destroyed in the decision making process.[41] The relationship between Pauling and other scientists reflects this second dimension of power, as elite U.S. scientists considered his test ban campaign as inappropriate and damaging for scientists. Lukes's second dimension of power was also at work in the test

[39] Lukes, *Power: A Radical View* (Houndmills: Palgrave Macmillan, 2004, second edition), 16.
[40] Thorpe, *Oppenheimer*, xiv.
[41] Lukes, *Power*, 6, 16, 22–24.

ban treaty debates, where Pauling's demands for change were simply ignored and left outside the boundaries of the decision making arena.

In the context of this dissertation, the first and second dimensions served to remove moral arguments against nuclear weapons from the agenda of U.S. science advisors and other elite scientists. But also at work here is the third dimension of power: the state's ability to prevent its subjects from having grievances at all by shaping their perceptions, cognition, and preferences so that they accept their role in the existing order.[42] This supreme exercise of power is, essentially, a description of the institutionalization of power, when the state's subjects cannot even conceive of alternative or conflicting views. The third dimension of power involves the unconscious influencing of values and rituals; Lukes also describes a dominating power that constrains choices and impedes people from living as their own judgment dictates. "Domination occurs," he writes, "where the power of some affects the interests of others by restricting their capabilities for truly human functioning."[43] Such a process is evident in the instinctual hostility of many scientists to Sagan's nuclear winter theory, when scientists' apolitical professional identities naturally conformed to the demands of the national security state.

Lukes's third dimension of power can also be seen in individual scientists' adoption of the state's views, without even thinking about it, such as when government science advisors learned to formally distinguish between acceptable and unacceptable methods of dissent against nuclear weapons. As an example, a science advisors' report on nuclear testing from 1961 included some of the government's brightest scientific minds: Wolfgang

[42] Lukes, *Power*, 6–11.
[43] Lukes, *Power*, 6, 28, 37, 58, 86, 118.

Panofsky, Hans Bethe, Norris Bradbury, George Kistiakowsky, and Frank Press. The panel qualified their report as strictly technical:

> the Panel has considered the technical questions involved in a decision by the U.S. on the resumption of testing. . . . In submitting this report, the Panel hopes that it will clarify the technical issues involved in a decision on the resumption of testing. The Panel wishes to emphasize, however, that while these technical issues have an important bearing on this decision, the final decision on whether or not to resume testing also involves very important non-technical or military issues which have not been included in the Panel's considerations.[44]

By restricting themselves to technical issues, these scientists obscured the reality of nuclear weapons which earlier scientists in a similar advisory position had described as "weapon[s] of genocide." What were scientists doing in avoiding moral language but distancing themselves from Pauling and his ilk, who despised the thought of dispassionate, amoral language that normalized and tacitly approved of nuclear weapons?

The physicist Charles T. Schwartz saw this third dimension of power at work in himself, though he did not name it as such. In the early 1960s Schwartz was asked to participate in a summer study with the Institute for Defense Analysis. "There was a sense of glamour attached to it," Schwartz recalled in a 1987 interview. "I had to get a security clearance, go to Washington, get paid, have a bunch of briefings, encouraged to work on some problem or other, and the headiness—wow, you know, now we're really getting into it!" But in 1965, Schwartz attended a party where a guest denounced President Johnson's decision to begin bombing North Vietnam as "a terrible thing." Schwartz scolded the woman, "It's better that we have small wars like that than that we have a nuclear war." The woman was, Schwartz recalled, "aghast" at his "terrible" comment. "But I felt very smug and very well-informed," Schwartz explained, "And when I look back at that statement, I find

[44] Folder 3.5: Test Ban Policy Proposals, 3 of 4, Box 189, MC 420, Jerome B. Wiesner Papers, MIT Institute Archives.

that that's a disgusting thing to have said, and I understand where that came from. That's the residue of my summer in Washington." There he had taken on a mentality of, in his words, "I understand the world, I've been to Washington, I've heard from the people who know, this is what it's really all about." Years later he came to see that his experiences working for the government directly resulted in his adoption of a specific ideology that enabled him to endorse war. He explained that, "by going to Washington, you pick up a world view, and it's available, it's easy, it's comfortable. . . . It fits very well, so it must be right. And then you use that as a shield."[45]

Early in the Cold War, the state had redefined scientists as upholders of the nuclear deterrent and the demands of the national security state. But as Schwarz's testimony shows, it was up to scientists to accept and internalize this definition. Many of them willingly served the state, and acted as the state demanded. Though all the government's science advisors claimed to dislike nuclear weapons and hoped they would never be used, they saw no difference between making a moral argument against nuclear weapons on the one hand and arguing that disarmament would not harm U.S. interests and security on the other. In fact, the two are very different arguments that are fundamentally statements about core values and identity. Scientists like Pauling and Sagan who demanded that social concerns be at the heart of science within a democracy were essentially beyond the pale. By getting government

[45] Interview with Charles Schwartz, by Finn Aaserud, May 15, 1987, American Institute of Physics Oral History Collection, College Park, MD, 14–16. In the late 1960s Schwartz engaged in many anti-Vietnam War protests, publicly criticized scientists who consulted for the IDA, and participated in the direct-action group Science for the People. Schwartz's view that Washington had a seductive power over scientists was not unique or even new. Thorpe writes that after Hiroshima, Oppenheimer's "authority was increasingly defined by his advisory offices, rather than by his charisma in the eyes of the scientific community or the public," *Oppenheimer*, 201.

scientists to embrace this shift, the state eliminated dissent and proved Lukes's claim that "Power is at its most effective when least observable."[46]

[46] Lukes, *Power*, 1.

Chapter One: From "Highly Unreliable" to "Patriotic and Prompt": Scientists Confront the National Security State, 1945–1957

Carving a Niche

Observers of all types hailed the atomic bomb at the moment of its use as a watershed invention in human history. Harry Truman claimed that upon hearing of the use of the bomb against Hiroshima, he blurted out "This is the greatest thing in history."[1] From a distinctly different perspective, Japanese Emperor Hirohito, speaking directly to his subjects for the first time, declared "The enemy has for the first time used cruel bombs to kill and maim extremely large numbers of the innocent, and the heavy casualties are beyond measure. To continue the war further could lead in the end not only to the extermination of our race, but also to the destruction of all human civilization."[2] In his survey of immediate reactions to the atomic bomb, historian Paul Boyer found that Hirohito's perspective was not unique—that "in the earliest moments of the nuclear era," nuclear fears "had already found urgent expression." Boyer highlighted the reaction of journalist Edward R. Murrow, among many others, who stated, "Seldom, if ever, has a war ended leaving the victors with such a sense of uncertainty and fear, with such a realization that the future is obscure and that survival is not assured."[3]

The daunting nature of atomic weapons certainly did shape the postwar world. To title his landmark history of the diplomatic significance of the A-bomb, Martin Sherwin coined a phrase to describe the military and political effects of the weapon: "A World

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[1] Quoted in Richard Rhodes, *The Making of the Atomic Bomb* (New York: Simon and Schuster, 1986), 734.
[2] Quoted in John Dower, *Embracing Defeat: Japan in the Wake of World War II* (New York: Norton/ The New Press, 1999), 36.
[3] Quoted in Paul Boyer, *By the Bomb's Early Light: American Thought and Culture at the Dawn of the Atomic Age* (Chapel Hill: University of North Carolina Press, 1994, second edition), 5, 7, 8.

Destroyed."[4] In the long history of violence that has produced countless weapons, "the bomb" requires no other descriptors. But another reason the atomic bomb held such import for the postwar world was that its creators self-consciously endowed it with great significance long before its initial use. In 1939, hours after discovering that uranium would release enough neutrons during fission to create a nuclear chain reaction, physicist Leo Szilard felt that "there was very little doubt in my mind that the world was headed for grief." That same year in Germany, Carl von Weizsacker, working on the German A-bomb, reflected "that this discovery could not fail to radically change the political structure of the world." Niels Bohr, an idol to many Manhattan Project scientists, predicted that the weapon would be so terrible as to end forever the human race's propensity for war. "We are in a completely new situation that cannot be resolved by war," Bohr declared, adding that the harnessing of atomic energy was "a far deeper interference with the natural events than anything ever before attempted."[5] Bohr heavily influenced the thinking of wartime Los Alamos director J. Robert Oppenheimer, who not long after the war's end justified atomic weapons as "the best argument science could make . . . for a more reasonable and a new idea of the relations between nations."[6]

The select scientists responsible for creating the A-bomb directly shaped perceptions of the weapon, and in so doing carved a niche for themselves in the policymaking of the atomic age. When Los Alamos physicist Robert Wilson felt pangs of doubt while working on the Manhattan Project, he convened a meeting to discuss the consequences of creating such

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[4] Martin Sherwin, *A World Destroyed: Hiroshima and Its Legacies* (Stanford: Stanford University Press, 2003, third edition).
[5] Quoted in Rhodes, *The Making of the Atomic Bomb,* 292, 312, 532. On von Weizsacker, see Jeremy Bernstein, ed., *Hitler's Uranium Club: The Secret Recordings at Farm Hall* (New York: Copernicus Books, 2001, second edition). After World War II, von Weizsacker unconvincingly claimed that he sabotaged his own work on the German A-bomb.
[6] Quoted in Charles Thorpe, *Oppenheimer: The Tragic Intellect* (Chicago: University of Chicago Press, 2006), 180.

a powerful weapon. To emphasize the bomb's image as a world-changing force, Wilson titled his meeting "The Impact of the Gadget on Civilization." The fact that a mere "gadget"—a wartime nickname for the A-bomb—could have an effect on "civilization" succinctly expressed scientists' vision of the bomb's magnitude: straightforward to invent, difficult to control.[7] In a November 1945 speech to Los Alamos scientists, Oppenheimer defended the bomb as the culmination of the quantum revolution in physics of the 1920s. Oppenheimer (no longer the lab's director) told the audience, "when you come right down to it the reason that we did this job is because it was an organic necessity. If you are a scientist you cannot stop such a thing." In the same speech, Oppenheimer also described the bomb as a momentous and crucial turning point in the history of science itself: "the real impact of the creation of the atomic bomb and atomic weapons . . . has in common with the early days of physical science the fact that the very existence of science is threatened, and its value is threatened."[8] On a more upbeat note, Oppenheimer apparently told fellow Los Alamos physicist Edward Teller after the war, "We have done a wonderful job here. It will be many years before anyone can improve on our work in any way."[9] Harry Truman absorbed views such as these and later declared the Manhattan Project to be "the greatest achievement of organized science in history."[10]

Not everyone joined the chorus of voices proclaiming a new era. Many U.S. policymakers saw the bomb as a fairly conventional weapon, while those unfriendly to the United States simply scoffed at it. Unlike most of his fellow Manhattan Project engineers,

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[7] On Wilson's meeting, see Thorpe, *Oppenheimer,* 154–55, and Kai Bird and Martin Sherwin, *American Prometheus: The Triumph and Tragedy of J. Robert Oppenheimer* (New York: Alfred A. Knopf, 2005), 287–89. The implosion bomb was fairly difficult to design, but the gun bomb was so simple that U.S. officials did not even bother to test it before dropping it on Hiroshima.
[8] Alice K. Smith and Charles Weiner, eds., *Robert Oppenheimer: Letters and Recollections* (Cambridge: Harvard University Press, 1980), 316, 317.
[9] Edward Teller, *Memoirs: A Twentieth-Century Journey in Science and Politics* (Cambridge: Perseus Publishing, 2001), 222.
[10] Quoted in DeGroot, *The Bomb: A Life* (Cambridge: Harvard University Press, 2005), 104.

David Greenglass (who passed atomic information to Soviet agents) did not even bother to wake up early for the Alamogordo test. "You gotta understand something," he explained to journalist Sam Roberts. "I knew it went off." Similarly, according to legend, Stalin slept through the first Soviet A-bomb test. When awoken by a phone call to tell him of the successful explosion, the dictator muttered "I know," and hung up. Mao Zedong simply laughed at atomic weapons: "The atom bomb is a paper tiger. It looks terrible, but in fact it isn't."[11] Reactions to the bomb reflected what was at stake in the control of atomic energy. Just as enemies of the United States needed to be unimpressed with the bomb, it similarly served scientists' interests that the bomb appear to have created a sea change in the course of history. If the public saw the bomb as momentous, it would seem more natural for scientists, as the guardians of atomic knowledge, to claim a role in helping guide the nation on atomic energy policy.

Scientists and Early Atomic Policy

Such was the thinking of the atomic scientists who forged for themselves large roles in nuclear policymaking near the war's end and after. Quoting again from Oppenheimer's Los Alamos speech, the physicist commented on the new world created by the bomb. "It is a new field," he remarked, "in which the role of science has been so great that it is to my mind hardly thinkable that the international traditions of science, and the fraternity of scientists, should not play a constructive part."[12] Oppenheimer was hoping to reinforce what had already tentatively begun: scientists actively consulted by policymakers. Along with fellow

[11] Sherwin, *World Destroyed*, 5; Greenglass quoted in Roberts, *The Brother* (New York: Random House, 2001), 136; Stalin and Mao quoted in DeGroot, *The Bomb*, 145, 187.
[12] Smith and Weiner, eds., *Robert Oppenheimer,* 319.

scientists Enrico Fermi, Arthur Holly Compton, and Ernest Lawrence, Oppenheimer had been "invited" to serve as a scientific advisor to the 1945 wartime Interim Committee that decided the most effective way to use the bomb. Thus scientists rubbed shoulders with such establishment luminaries as Vannevar Bush, James Byrnes, James Conant, George Marshall, and Henry Stimson.[13] Other scientists endeavored to influence policy after the war with more civic-minded activism. As has been well documented, Albert Einstein, Linus Pauling, Eugene Rabinowitch, Szilard, Teller, and other scientists took part in what became known as the Atomic Scientists' Movement, an attempt by scientists to shape U.S. atomic energy legislation. These efforts helped defeat the May-Johnson bill, which would have ceded control over atomic energy policy to the military, and helped pass the McMahon Act, which established a civilian Atomic Energy Commission (AEC) to regulate, promote, and protect the U.S. atomic energy industry.[14]

The Atomic Scientists' Movement had loftier visions than the provincial goal of lobbying the U.S. Congress for legislation. Since the threat of atomic weapons was viewed as so grave, many scientists—including even the relatively conservative Oppenheimer—adamantly believed that only a world government with control over atomic energy could, in the long run, guarantee the survival of humanity. But as is well known, the fragile ship of world government crashed and sank on the rough shoals of the Cold War.[15] As tensions

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[13] See the "Interim Committee Minutes, May 31, 1945," in Robert C. Williams and Philip Cantelon, eds., *The American Atom: A Documentary History of Nuclear Policies from the Discovery of Fission to the Present* (Philadelphia: University of Pennsylvania Press, 1984), 58–64.

[14] The authoritative study of the Atomic Scientists' Movement is Alice K. Smith, *A Peril and a Hope: The Scientists' Movement in America, 1945–47* (Cambridge: MIT Press, 1971). The Atomic Scientists' Movement is also discussed in Boyer, *Bomb's Early Light,* 59–75; William Lanouette, *Genius in the Shadows: A Biography of Leo Szilard, the Man Behind the Bomb* (New York: C. Scribner's Sons, 1992); Sherwin, *World Destroyed,* 58–60; and Lawrence Wittner, *The Struggle Against the Bomb, Vol. 1: One World or None: A History of the World Nuclear Disarmament through 1953* (Stanford: Stanford University Press, 1993). On the creation of the AEC, see Richard G. Hewlett and Oscar E. Anderson, *A History of the United States Atomic Energy Commission, Vol. I: The New World, 1939–1946* (University Park: Penn State University Press, 1962).

[15] Wittner, *One World or None*, 162–63, 269–71, 314–17, 321–22, 326–28.

between the United States and Soviet Union increased, nationalism triumphed over internationalism. U.S. officials cynically transformed Oppenheimer's proposal for UN control of atomic energy into a vacant piece of Cold War propaganda.[16] The elite scientists who had hoped to guide atomic energy policy soon clashed with the *sang-froid* realism of U.S. policymakers and increasingly fell out of favor. Even more damaging was a growing public perception of theoretical physicists as the "weakest links" in the U.S. security system that painted scientists with a pink brush. In the words of historian of science David Kaiser, "theoretical physicists emerged as the most consistently named whipping-boys of McCarthyism."[17] Questions of character only became more troubling when atomic spy cases, the fall of China, and the Korean War encouraged a full-blown Red Scare well into the 1950s. In 1954, one scientist complained to the AEC that "Exaggerated reports in the public press have led to the impression that scientists as a class are highly unreliable and that many are disloyal."[18] Perhaps most troubling for scientists, some academic and government science institutions fell in line behind the Red Scare. The University of California at Berkeley instituted loyalty pledges for its faculty and fired many who refused to sign, while the AEC launched a new round of security clearance hearings to weed out subversives.[19]

Although historians have devoted a substantial amount of pages to scientists' political activism after World War II, a categorization of all scientists as liberal-leaning

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[16] Oppenheimer's proposal (officially named the Acheson-Lilienthal Plan) is discussed in Boyer, *Bomb's Early Light*, 53–58; Bird and Sherwin, *American Prometheus*, 342–45; and Wittner, *One World or None,* 250–53.
[17] David Kaiser, "The Atomic Secret in Red Hands? American Suspicions of Theoretical Physicists During the Early Cold War," in Cathryn Carson and David Hollinger, eds., *Reappraising Oppenheimer: Centennial Studies and Reflections* (Berkeley: Berkeley Papers in the History of Science, Vol. 21, 2005), 186.
[18] M. Stanley Livingston to W. Libby, Nov. 26, 1954, Folder: Libby, AEC Commissioner, Box 5 (SL), Hans Bethe Papers, Cornell University (hereafter HB Papers).
[19] Jessica Wang, *American Science in an Age of Anxiety: Scientists, Anticommunism, and the Cold War,* Chapel Hill: University of North Carolina Press, 1999; Kaiser, "The Atomic Secret," and Stephanie Young, "'Something Resembling Justice': John Francis Neylan and the AEC Personnel Security Hearings at Berkeley, 1948–49," in Carson and Hollinger, eds., *Reappraising Oppenheimer*. Oppenheimer underwent and passed a security clearance in 1947 when Lilienthal was head of the AEC.

activists would be inaccurate. The Manhattan Project's most famous scientists left Los Alamos after the end of the war, some of them to take up antinuclear causes. But plenty of scientists remained behind to staff the weapons lab and help build the nation's nuclear arsenal. Though many scientists worldwide hesitated to continue working at weapons labs after the war, in the words of historian Eric Hobsbawm, "There is no evidence that subsequently such establishments had any trouble in recruiting their staff."[20] Plenty of scientists accepted, explicitly or tacitly, the policy of deterrence as the best means for preventing a nuclear war. But though rank and file scientists may have powered big science forward in the years after World War II, the nation's elite scientists—mostly Manhattan Project veterans—wrestled over the direction of the ship. Men like Oppenheimer, Hans Bethe, and Edward Teller engaged with the state on the issue of nuclear weapons, and were present at the site where policy was made, at the moment in the 1950s when conflicts between scientists and the state reshaped power relations.

The Oppenheimer Hearing: Morality and Security

To express the tumultuousness of scientists' engagement with military, politics, and society between 1930 and 1954, historians often present their audience with J. Robert Oppenheimer, the head of the Los Alamos lab during the Manhattan Project. Though not really a participant in the Atomic Scientists' Movement, Oppenheimer had been politically radical during the Great Depression, when he supported a wide variety of far left causes and embraced communism as a fellow traveler.[21] Having purged himself of these leftist

[20] Eric Hobsbawm, *The Age of Extremes: A History of the World, 1914–1991* (New York: Vintage, 1994), 546–47.
[21] Bird and Sherwin, among others, dispute the claim that Oppenheimer belonged to the CPUSA. Gregg Herken conversely believes that Oppenheimer was a communist, and that his CP membership proves that a person could be both a communist

associations during the Manhattan Project, Oppenheimer emerged from the war as the premier government science advisor and perhaps the greatest beneficiary of scientists' newfound fame and cachet. To represent scientists' alleged fall from grace during the Red Scare, historians point to Oppenheimer's ouster from government service after a 1954 hearing when the AEC declared him a security risk and revoked his security clearance.

Despite eleven biographies of Oppenheimer appearing since 2002, not to mention one opera and a novel, historians most likely will never close the book on this complex, confounding, and influential person. Without questioning the value of the new subdiscipline of Oppenheimer studies, it bears noting that the insistence on using Oppenheimer as the lens through which to view the atomic age has inadvertently left the greater consequences of the Oppenheimer hearing severely under-examined. Obsessed with his tragedy (the FBI and AEC conspired to turn his naïve but well-intentioned activism into evidence of disloyalty) or with esoteric questions (was he *really* a member of the communist party?), historians have yet to directly address the ways in which the security clearance hearing ushered in a new relationship between the national security state and the elite scientists only slightly less influential than Oppenheimer.

In 1945, the destruction of Hiroshima and Nagasaki by atomic bombs elevated Oppenheimer to celebrity status. As the head of the Los Alamos lab that had produced the atomic bombs, the enigmatic and photogenic Oppenheimer expressed ambivalence about

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and a loyal American, thus showing the Red Scare loyalty purges to be even more egregious. Barton Bernstein, after years of consideration, falls somewhere in between. In addition to Bird and Sherwin, *American Prometheus*, and Gregg Herken, *Brotherhood of the Bomb: The Tangled Lives and Loyalties of Robert Oppenheimer, Ernest Lawrence, and Edward Teller* (New York: Henry Holt and Co., 2002), see Herken, "Was Robert Oppenheimer a 'Closet Communist'? The Debate and the Evidence," Bird and Sherwin, "Robert Oppenheimer and the Communist Party," and Bernstein, "The Puzzles of Interpreting J. Robert Oppenheimer, His Politics, and the Issues of His Possible Communist Party Membership," all in Carson and Hollinger, eds., *Reappraising Oppenheimer*. Despite these careful considerations, one might question the accuracy and usefulness of the FBI records upon which these authors base much of their research. After all, the point of FBI surveillance was to make Oppenheimer look bad and prove him a communist.

nuclear weapons and cultivated the mystique that ensured that later historians would interpret him as the human embodiment of the atomic age and all its attendant dilemmas and contradictions.[22] The slender physicist was essential to the successful crafting of the first weapons of mass destruction, but worried deeply about war. As one who prided himself on having pondered Eastern religion, world literature, and human nature, Oppenheimer gave the impression that he would not wield his influence lightly. Yet he could make boneheaded decisions—"I was an idiot," he later confessed to the AEC in an exasperated attempt to explain his concocted stories about espionage attempts during the Manhattan Project.[23] At times Oppenheimer appeared to be a sort of pacifist, worriedly prophesizing that "the time will come when mankind will curse the names of Los Alamos and Hiroshima," and denouncing the proposed Classical Super (an early version of the Hydrogen bomb) as a potential "weapon of genocide" that would "slaughter a vast number of civilians."[24] Yet Oppenheimer eagerly advised military policymakers, and in place of the terrifying Super, he comforted himself with the thought of many, many A-bombs, including an "acceleration of the [A-bomb] program" and "an intensification of efforts to make atomic weapons available for tactical purposes."[25] Respected by many, Oppenheimer was also consulted by the powerful: as head of the AEC's General Advisory Committee (GAC), he often met with congressional and other officials, including—rarely—the president.

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[22] Oppenheimer's notoriety was so great that the May 1948 issue of *Physics Today* dedicated to Oppenheimer simply showed a picture of his porkpie hat on the cover, with no accompanying text needed. See Thorpe, *Oppenheimer,* 15.
[23] Oppenheimer was specifically referring to his numerous and contradictory explanations of the moment during World War II when his friend Haakon Chevalier informed Oppenheimer that he had been approached by a Soviet agent interested in attaining A-bomb secrets. U.S. Atomic Energy Commission, *In the Matter of J. Robert Oppenheimer: Transcript of Hearing before Personnel Security Board and Texts of Principal Documents and Letters* (Cambridge: MIT Press, 1970), 137.
[24] Smith and Weiner, eds., *Robert Oppenheimer,* 311; Williams and Cantelon, eds., *The American Atom,* 125–6.
[25] Williams and Cantelon, eds., *The American Atom*, 122.

But in a stunning reversal of fortune, the AEC humiliated Oppenheimer in 1954 by revoking his security clearance (and thus his access to privileged government policymaking circles) after a now-infamous hearing that has been extensively studied.[26] Over a span of less than a decade, Oppenheimer went from the power elite of atomic energy circles to a martyred intellectual, exiled to the purgatory of Princeton's Institute for Advanced Study. Observers at the time, as well as scholars in years since, recognized that the hearing served as retaliation for a multitude of Oppenheimer's technical and political faux-pas, including his initial opposition to the Classical Super, his recommendations against Air Force atomic weapons and policy plans, and his alleged domination and intimidation of members of the GAC. Scholars have convincingly argued that the hearing also resulted from the desire of several of Oppenheimer's enemies to punish him for his personal arrogance and the perceived insults they had received from him over the years. These foibles included his bypassing of Edward Teller for Hans Bethe as head theoretical physicist of the Los Alamos lab in 1943, his public ridiculing of AEC commissioner Lewis Strauss in front of the Joint Committee on Atomic Energy in 1949, and his continued "association," in the words of the AEC Personnel Security Board, "on what could not be considered a casual basis" with known-communist Haakon Chevalier (sinister associations that consisted of a birthday card and a shared dinner).[27] In her account, Priscilla McMillan has shown that Strauss used intimidation, coercion, and bribery to orchestrate the hearings like a dark Wizard of Oz and

[26] The many studies include Richard Polenberg, ed., *In the Matter of J. Robert Oppenheimer: The Security Clearance Hearing* (Ithaca: Cornell University Press, 2002); Bird and Sherwin, *American Prometheus*; David Cassidy, *J. Robert Oppenheimer and the American Century* (New York: Pi Press, 2005); Thorpe, *Oppenheimer*; Jeremy Bernstein, *Oppenheimer: Portrait of an Enigma* (Chicago: Ivan R. Dee, 2004); Priscilla McMillan, *The Ruin of J. Robert Oppenheimer and the Birth of the Modern Arms Race* (New York: Viking, 2005); Rachel Holloway, *In the Matter of J. Robert Oppenheimer: Politics, Rhetoric, and Self-Defense* (Westport: Praeger, 1993); Barton Bernstein, "In the Matter of J. Robert Oppenheimer," *Historical Studies in the Physical Sciences* 12 (1982): 195–252; and Philip Stern, *The Oppenheimer Case: Security on Trial* (New York: Harper & Row, 1969).

[27] Bird and Sherwin, *American Prometheus,* 401; Polenberg, *The Security Clearance Hearing,* 361; U.S. AEC, *In the Matter of J. Robert Oppenheimer,* 1019. The Personnel Security Board consisted of Gordon Gray and Thomas Morgan, with Ward Evans in dissent.

ensure Oppenheimer's dismissal. Meanwhile Teller's testimony against Oppenheimer damned him with the words "I would like to see the vital interests of this country in hands which I understand better, and therefore trust more."[28] In fact, Teller had helped set the hearing in motion in 1952 by telling an FBI interviewer that the "H bomb would have been a reality at least one year ago if it had not been opposed by Oppenheimer," and that "he would do most anything to see subject [Oppenheimer] separated from General Advisory Committee because of his poor advice and policies regarding national preparedness and because of his delaying the development of H bomb."[29]

Naturally, the hearing served as a way of preventing influential advisors from giving the AEC advice it did not want to hear. But it was more than that, as the AEC chose to make an example out of Oppenheimer—certainly Oppenheimer was not the only scientist in the United States who could advise policymakers on nuclear weapons. Concerned about the elaborate lengths to which the AEC went to discredit Oppenheimer, I.I. Rabi drew attention to this very point during the hearing. Rabi stated, "he is a consultant, and if you don't want to consult the guy, you don't consult him, period."[30] Instead, the AEC went so far as to redefine what proper science advice was in the course of the hearing and its ensuing verdict. The AEC's insistence on making a spectacle out of Oppenheimer indicated that the agency wanted to do more than just make sure it got the advice it wanted. As Thorpe has pointed out, the AEC feared that scientists had attained too much influence—that they were "on top," not "on tap."[31]

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[28] McMillan, *Ruin*; Teller quoted in U.S. AEC, *In the Matter of J. Robert Oppenheimer,* 710.
[29] FBI teletype, June 12, 1952, Folder: "Re: Oppenheimer Hearing," Box 446: Topic File, Edward Teller Papers, Hoover Institution (hereafter ET Papers). Teller discusses the interview in *Memoirs*, 372, and denies that he told the agent that he wanted Oppenheimer removed from the GAC.
[30] Polenberg, *The Security Clearance Hearing*, 178.
[31] Thorpe, *Oppenheimer*, 200.

The details of the Oppenheimer hearing have been well covered elsewhere. Scholars have clearly proved the AEC's most notorious hearing a farce—an injustice to Oppenheimer personally as well as an insult to the Bill of Rights that epitomized the Red Scare era, when the trampling of civil liberties took on the fervor of a national pastime.[32] And while the hearing reflected and exhibited the Red Scare politics of paranoia, the hearing also shaped the future of science-government relations by allowing the state to eliminate dissent by excluding certain views on nuclear weapons. This process ultimately restricted scientists' role in government and served as one way the state contained scientists' antinuclear challenges for the duration of the Cold War.

The hearing defined the rules for future input and advice from scientists, and demonstrated the costs of dissent from the state's militarism.[33] Moral qualms such as reluctance to work on the H-bomb, the AEC believed, had hindered the pursuit of nuclear weapons development, and thus deserved to be expelled from the science advisor's intellectual repertoire. Not that morality itself was strictly forbidden, but key aspects of the AEC's Oppenheimer verdict, presented in the Personnel Security Board's "Findings and Recommendation," expressed a deliberate intent to redefine morality to such an extent that it could not challenge the nation's commitment to increasingly powerful nuclear weapons. "We must make it clear," the board wrote,

> that we do not question Dr. Oppenheimer's right to the opinions he held with respect to the development of this weapon [the H-bomb]. They were shared by other competent and devoted individuals, both in and out of Government. We are willing to assume that they were motivated by deep moral conviction. We are concerned,

[32] See, for example, Stephen J. Whitfield, *The Culture of the Cold War* (Baltimore: Johns Hopkins University Press, 1996, second edition); Richard M. Fried, *Nightmare in Red: The McCarthy Era in Perspective* (New York: Oxford University Press, 1990); and David Oshinsky, *A Conspiracy So Immense: The World of Joe McCarthy* (New York: The Free Press, 1983).
[33] Steven Lukes's first dimension of power was at work here, where the state forcibly excludes certain views from governmental consideration. Lukes, *Power: A Radical View* (Houndmills: Palgrave Macmillan, 2004, second edition), 16.

> however, that he may have departed his role as scientific adviser to exercise highly persuasive influence in matters in which his convictions were not necessarily a reflection of technical judgment, and also not necessarily related to the protection of the strongest offensive military interests of the country.[34]

"Any man," the verdict continued, "whether specialist or layman, of course, must have the right to express his deep moral convictions . . . [but] emotional involvement in the current crisis, like all other things, must yield to the security of the nation." The document elaborated:

> It is vitally important that Government and scientists alike understand the need for and value of competent technicians. This need is a present and continuing one. Yet, those officials in Government who are responsible for the security of the country must be certain that the advice which they seriously seek appropriately reflects special competence on one hand, and soundly based conviction on the other, uncolored and uninfluenced by considerations of an emotional character.
>
> In evaluating advice from a scientist which departs from the arena of his specialty, Government officials charged with the military posture of our country must also be certain that underlying any advice is a genuine conviction that this country cannot in the interest of security have less than the strongest possible offensive capabilities in a time of national danger.[35]

As Thorpe has pointed out, the AEC here defined the state's security interests in explicitly military terms.[36] During his testimony, Oppenheimer appeared to realize the state's preference for strictly technical advice. He explained his opposition to the Super not as a

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[34] The wording of the Oppenheimer verdict was not unique. In 1957, the American Legion asked a Pennsylvania judge to refuse the incorporation charter of the Society for Social Responsibility in Science, arguing that the SSRS's activities "might not necessarily [be] related to the protection of the strongest offensive military interests of this country." Quoted in Kelly Moore, *Disrupting Science: Social Movements, American Scientists, and the Politics of the Military, 1945–1975* (Princeton: Princeton University Press, 2008), 80.

[35] Polenberg, *The Security Clearance Hearing,* 359–360; Cassidy, *Oppenheimer and the American Century,* 323; U.S. AEC, *In the Matter of J. Robert Oppenheimer,* 1016–18. Polenberg argues in contrast to Thorpe and Cassidy that the hearing had little to do with morality. According to Polenberg, AEC counsel Roger Robb had attempted during his aggressive examination to get Oppenheimer to admit that his opposition to the H-bomb had been based on loyalty to communism rather than moral objections. Polenberg, *The Security Clearance Hearing,* xxiv. If this was the case, Oppenheimer misunderstood his escape clause because during the hearing, he denied being influenced by communism but also noticeably avoided discussing morals. Cassidy, in contrast to Polenberg, argues that the entire H-bomb debate was a moral debate, the type of which was declared out of bounds by the Oppenheimer hearing. Cassidy, *Oppenheimer and the American Century,* 298, 323.

[36] Thorpe, *Oppenheimer,* 230.

moral objection but a technical matter—once Teller and his colleagues at Los Alamos came up with a "technically sweet" design, Oppenheimer claimed he had turned in favor of the weapon.[37] Oppenheimer may have hoped that this paean to amorality would play well with the AEC, but to no avail. While the AEC was busy destroying Oppenheimer's reputation, the agency was also establishing new guidelines for its science advisors. In the process, the balance of power shifted away from H-bomb skeptics to nuclear advocates like Teller. As Teller's opinion mattered in the hearing, so it would also matter in the future. Most importantly, the hearing was, according to Oppenheimer biographer David Cassidy, the government's way of excluding morality from official discussions about nuclear weapons policy, including arms control and disarmament. Scientists, the state demanded, should recognize the threat to national security and give only technical advice that upheld the nuclear foundations of the national security state.[38]

The AEC verdict directly addressed morality for a specific reason: the high-level debate over the H-bomb had been less about *how* to build an H-bomb and more about *whether* to build one. In his 1952 FBI interview, Teller had explicitly blamed Oppenheimer's moral concerns for thwarting his push for thermonuclear weapons. "Teller claims subject [Oppenheimer] delayed or hindered development of H bomb from 1945 to 1950 by opposing it on moral grounds," FBI agent McCabe noted.[39] Many influential scientists besides Oppenheimer had objected to the Classical Super, and they had, like Oppenheimer, often done so on moral grounds. The oft-cited majority GAC report that recommended

[37] Polenberg, *The Security Clearance Hearing,* 46; U.S. AEC, *In the Matter of J. Robert Oppenheimer,* 81. Not that Oppenheimer was dissembling. The new H-bomb design was of limited explosive potential—unlike the Classical Super, which theoretically had no limits to its explosive power.
[38] Cassidy, *Oppenheimer and the American Century,* 323.
[39] FBI teletype, 1, June 12, 1952, Folder: "Re: Oppenheimer Hearing," Box 446: Topic File, ET Papers.

against a crash program, signed by James B. Conant, Hartley Rowe, Cyril Stanley Smith, Lee DuBridge, Oliver Buckley, as well as Oppenheimer, cast the Super as "a weapon of genocide," while the minority report, signed by Enrico Fermi and I. I. Rabi, went even further, describing the weapon as "necessarily an evil thing considered in any light," adding that "the use of such a weapon cannot be justified on any ethical ground which gives a human being a certain individuality and dignity even if he happens to be a resident of an enemy country. . . . Its use would put the United States in a bad moral position relative to the peoples of the world."[40]

But moral qualms about the Super were not limited to the GAC. In particular, the respected physicist and future government advisor Hans Bethe made clear in his personal correspondence how the H-bomb troubled his conscience. Indeed, historian of science S.S. Schweber has presented Bethe as an example of a scientist who upheld his "moral responsibility" during the Cold War.[41] After President Truman disregarded the GAC recommendation and announced a crash H-bomb program in 1950, Bethe wrote to Norris Bradbury, the head of the Los Alamos lab, with the weapon clearly at the forefront of his mind. "The announcement of the President has not changed my feelings in this matter," he wrote to Bradbury in February 1950;

> I still believe that it is morally wrong and unwise for our National security to develop this weapon. In most respects I agree with the opinions of the General Advisory Committee although I have not seen their report itself. So much has been said about the reasons on both sides that I do not need to go into them here. The main point is that I can not in good conscience work on this weapon. . . . In case of war I would obviously reconsider my position.[42]

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[40] Williams and Cantelon, eds., *The American Atom,* 126–27. Fermi and Rabi ultimately said, however, that if the Soviet Union went forward with the H-bomb, the United States should follow suit.
[41] S.S. Schweber, *In the Shadow of the Bomb: Bethe, Oppenheimer, and the Moral Responsibility of the Scientist* (Princeton: Princeton University Press, 2000).
[42] HB to Norris Bradbury, Folder: Bethe, Hans A., Box 273: Correspondence, Personal, ET Papers. [Also in: HB to Bradbury, Feb. 14, 1950, Folder: Correspondence on H-bomb with Bradbury, Box 21 (SL), HB Papers.]

Less than two months later he wrote to AEC member Robert Bacher. Referring to a Bacher speech on the Super and its "hard-headed argument" about national security, Bethe commented:

> However I still think that the moral argument is important and that one must point out on this occasion that one should not accept bigger and more powerful weapons of destruction without question. You are right that the moral argument is again a relative one—a point which most non-scientists and especially the theologians don't seem to be able to see. But thinking of the destruction a hydrogen bomb war would cause I really question whether victory in such a war would mean anything and would still be preferable to submission. It seemed to me not only necessary to try to avoid war in which we may or may not succeed, but also to try at least as far as possible to minimize the probability that hydrogen bombs would be used in the war. At present I think nothing has been done on this matter.[43]

Bethe still agreed to work part-time at Los Alamos, though not on the Super. And when the Korean War began on June 25, 1950, true to his word he began to participate in H-bomb research. But Bethe's agreement to work on the weapon did not mean he had resolved his moral dilemma. Teller, who had exerted much effort in order to get Bethe to Los Alamos, was still unhappy with Bethe's lack of enthusiasm for the Super when he wrote to him in late November 1950. At first, Teller expressed pleasure "that our difference of opinion about general questions is becoming smaller and will eventually vanish completely. By now, I really have a strong component in my wishing that this whole question should be settled in the negative, if necessary. On the other hand," Teller continued, "I certainly feel that it would be awfully good if you might somehow rid yourself of this strong bias in the negative direction.

[43] HB to Robert F. Bacher, April 4, 1950, Folder 10.5: Bacher, R.F., Box 8, HB Papers.

If we could get together on our basic opinion on this subject I believe this would be of the highest importance and would even get practical results with much greater rapidity."[44]

Bethe responded to Teller just over a week later. "I am sorry I can't change my negative attitude; I just don't like the gadget," he began.

> But for the progress of the work the main thing is that I work on it, and do it honestly, not suppressing any results, which might be 'favorable' or 'unfavorable'. This I believe I have done. Perhaps it is even a good thing that we take opposite points of view because then there is a motive for you to have new ideas and for me to investigate them in detail. In this way we are both doing what we can do best.[45]

In the years before the Oppenheimer hearing, Bethe struggled to reconcile his moral sensibilities with weapons work, and did not shy away from bringing his moral conscience to bear on his work as government science advisor and weapon scientist. The Oppenheimer hearing told scientists to set aside questions of morality when giving the government advice on nuclear weapons. Of course, it was not morality per se that troubled the state—it was a morality that questioned the reliance on thermonuclear weapons that gave the government pause. As long scientists accepted nuclear weapons as the basis of defense, they could advise the state.

But if the hearing decreed that science advice had to conform to the AEC's demands, did elite U.S. scientists—who, aside from Oppenheimer, still had the power to choose or reject such work—accept this censoring? Of all people, the guardians of nuclear knowledge were not passive subalterns. They had power and could react to the state's decree. And, though many of them initially doubted that they would serve the state, many eventually did.

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[44] ET to HB, Nov. 29, 1950, Folder: Bethe-Teller Correspondence, 1950–57, 1982–85, Box 21 (SL), HB Papers.
[45] HB to ET, Dec. 8, 1950, Folder: Bethe, Hans A., Box 273: Correspondence, Personal, ET Papers.

Scientists React to the Oppenheimer Verdict

Scientists were quick to recognize the significance of the Oppenheimer ruling. Of all the emotions evoked by the Oppenheimer hearing, ambivalence was not one of them. Scientists who commented on the event generally reacted in two ways: they predicted the government would suffer by alienating quality scientific advisors, and they feared that the scientific community would split in two over the treatment of Oppenheimer. Over the years Oppenheimer had endeared himself to many influential political figures, including David Lilienthal, James B. Conant, and George Kennan. Oppenheimer's leadership of the Manhattan Project, his thoughtful consideration of atomic policy, and his advocacy of world government had engendered among many physicists a feeling of respect, if not always affection, for the man. Even many of those scientists who found Oppenheimer "very difficult to understand" (in the words of chemist Wendell Latimer) owed their careers and prestige in some part to Oppenheimer's successful development of atomic weapons and large-scale physics during the Manhattan Project. Oppenheimer himself said that "the men who worked with me during those years [of the 1920s and 1930s at Caltech and UC Berkeley] hold chairs in many of the great centers of physics in this country."[46]

Not surprisingly, many scientists rushed to his defense as soon as the hearing began. Sensing that the tribunal could have consequences beyond mere damage to Oppenheimer's reputation, several notable scientists predicted a major backlash on the government. In the

[46] Quoted in U.S. AEC, *In the Matter of J. Robert Oppenheimer,* 660, 7–8. Latimer made this comment during the security clearance hearing when he testified against Oppenheimer. Note the similarity between his and Teller's comments about Oppenheimer.

Bulletin of the Atomic Scientists, physicist F.W. Loomis worried that the treatment of Oppenheimer would

> make [government] advisory positions so precarious that they will become unacceptable to men of ability and integrity. Equally important, the penalty that goes with advocating views that could become unpopular will prevent honest expression of opinion by men who do accept such positions. Objective and fearless advice to those responsible for high policy will be seriously inhibited with grave consequence to the future of the country.

Other observers echoed the fear that only unprincipled scientists would dare serve the government in the future. Sociologist Edward Shils, a close friend of *Bulletin* founders Hyman Goldsmith and Eugene Rabinowitch, predicted that because of the hearing, the government

> will alienate the best scientists from the service of the government and will make sure that only the mediocre and characterless among scientists will make themselves available for the purposes of an arbitrary government of demagogues and bureaucrats. Every honorable man will hesitate, after the suspension of Dr. Oppenheimer, to offer advice to a government which destroys those who turn out to be on the losing side of an argument in an advisory committee.

Farrington Daniels, the chemist who had been director of the Manhattan Project's Met Lab in summer 1945, similarly worried that "men of high caliber" would no longer serve the government, while Hugh C. Wolfe, physicist and participant in the Atomic Scientists' Movement, mourned in advance the disappearance of "the kind of responsible and able citizen who would be capable of making decisions on major questions."[47]

Scientists expressed such fears in private as well as in public; they also worried that, because of the conflict between Teller and Oppenheimer, the hearing—no matter the verdict—would cause a division in the scientific community. Hans Bethe, slightly less

[47] "Scientists Affirm Faith in Oppenheimer," *Bulletin of the Atomic Scientists,* May 1954, Vol. X, No. 5, 189.

prestigious an advisor than Oppenheimer but more admired by his peers, denied such a rift and consciously tried to prevent one. During the hearing (at which he testified in Oppenheimer's defense) Bethe wrote to one of Oppenheimer's lawyers. "I had a long conversation about this matter with Dr. Von Neumann of the Institute for Advanced Studies," Bethe noted. "To my great pleasure, I found that we agreed on almost every point. This is particularly important because Dr. Von Neumann has been very close to Dr. Teller and has taken entirely Dr. Teller's point of view in the controversies [over the H-bomb]. His testimony would therefore be of particular value."[48]

A letter from NYU mathematician Richard Courant to Bethe, written just after the hearing concluded, showed that not all observers shared Bethe's confidence that scientists remained undivided. "Probably we will not quite agree in Robert's case," Courant began, "but although I have been entirely on the side lines and unaware of any details, I am quite fearful that this case may lead to internal tensions and even to an internal split among the scientific fraternity, and I am very much concerned about this danger."[49]

Bethe quickly responded to calm Courant. "Concerning the Oppenheimer case in particular," he wrote,

> I do not believe there is any division, at least among physicists. As far as I can make out, there is an opinion different from mine only at the University of California and its Physics Department [where Teller was a member of the faculty] is about evenly divided. Regardless of any individual's opinion about Oppenheimer's advice to the government, it seems to me essential that a strong stand be made in his case. Even if this does not save Robert it may prevent indiscriminate extension of such procedures to others.[50]

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[48] HB to Samuel J. Silverman, March 18, 1954, Folder 12.4: Oppenheimer Case (2), Box 12, HB Papers. Von Neumann did testify, arguing that Oppenheimer had been wrong about the H-bomb but was a loyal American nevertheless.
[49] R. Courant to HB, May 12, 1954, Folder 10: R. Courant, Box 2, ET Papers. [Also in Folder 12.4: Oppenheimer Case (2), Box 12, HB Papers]. On Courant, see John Krige, *American Hegemony and the Postwar Reconstruction of Science in Europe* (Cambridge: MIT Press, 2006), 50.
[50] HB to Courant, May 18, 1954, Folder 10.13: (Correspondence) –C– (3), Box 10, HB Papers.

In contrast to claims of a split between scientists and the AEC, Bethe in fact undertook efforts to take "a strong stand" and reform the AEC's security clearance procedures as best his influence would allow, in the process attempting to rehabilitate the relationship between scientists and the agency. In November 1954 Bethe wrote to AEC commissioner Willard Libby and asked him to seek out the views of at least three individuals on "personnel security problems." Following Bethe's advice, Libby contacted the recommended scientists: Gregory Breit, M. Stanley Livingston, and Joseph Platt. First to respond to Libby's inquiry was Platt, an occasional consultant to the AEC from the University of Rochester. Platt told Libby that the AEC had made it "less easy" for the state to obtain scientific help, "and I feel this is a disservice to the nation which is not offset by any corresponding gain in personnel security." Platt doubted that "responsible physicists" would refuse to serve the government "because of petulance about the Oppenheimer case," but admitted that the "climate of cooperation has nevertheless deteriorated." Of concern to the AEC, Platt believed, was that a scientist approached by the AEC might hesitate to serve because of the "substantial stigma attached to the denial of clearance." Potential AEC scientists could expect the AEC to apply to them the same criteria used to discredit Oppenheimer, causing them to fret that they had been on the "wrong" side of a scientific decision, made contact with an uncleared person, or befriended "other nationals or political mavericks." In addition, the AEC's attempt to sever Oppenheimer's influence over other government scientists was "a security criterion not generally understood or accepted, and which would give pause to a scientist requested for help." Platt recommended "as unambiguous as possible a statement from the AEC of the goals of the personnel security

program and of the grounds which will, and which will not, be considered in a determination of clearance."[51]

Similarly, M. Stanley Livingston of MIT described to Libby the "very disturbing" and "widening areas of misunderstanding and distrust between the Commission and scientists in this country." Although "scientists will not refuse their responsibilities" to the nation, Livingston assured Libby, "many of them have become disheartened and disillusioned by practices within the security system which seemed to them arbitrary and unjust." Livingston warned Libby of "a noticeable decrease in enthusiasm for work requiring security clearance." The conflict could be traced to "a lack of appreciation of the purpose of the security system and overzealous application of the regulations." Livingston went a bit further in his recommendations than Platt, recommending a "thorough overhaul of the security system," provided that the commission was willing to "clarify the philosophy and purpose of the security program." Livingston then outlined seven philosophical suggestions as well as seven procedural recommendations to establish a security system that encouraged science and valued traditional U.S. freedoms.[52]

A final letter from Yale scientist Gregory Breit urged Libby to reinstate Oppenheimer's clearance in order to "improve general morale." Otherwise, Breit feared a situation where "only those agreeing with official policies and those exhibiting a high degree of obedience in modifying their technical views will be acceptable for clearance."[53] Oppenheimer was of course never reinstated, but nor was there a mass exodus or vocal

[51] Platt to Libby, Nov. 24, 1954, Folder: Libby, AEC Commissioner, Box 5 (SL), HB Papers.
[52] Livingston to Libby, Nov. 26, 1954, Folder: Libby, AEC Commissioner, Box 5 (SL), HB Papers.
[53] Breit to Libby, Dec. 8, 1954, Folder: Libby, AEC Commissioner, Box 5 (SL), HB Papers.

break between scientists and the AEC. The official history of the AEC labels the effect of the Oppenheimer case on the commission as "permanent and damaging." Its authors assert:

> How the Oppenheimer case affected the career decisions of individual scientists has never been determined in any systematic way. Some saw the Commission's action as outrageous and sickening; in the minds of others, Oppenheimer got what he deserved. There was, however, a subtle but permanent shift in many scientists' perception of the Commission. Eight years earlier the scientists had seen the Commission as their agency, a new and enlightened institution that could, among other things, free the scientist from the restraints and indignities of military control. The Commission had justified that faith, but the Oppenheimer case had planted seeds of doubt. It was not likely that an agency that had destroyed the career of a leader like Oppenheimer could ever again enjoy the full confidence of the nation's scientists.[54]

Though Bethe denied a split to some, he admitted that a divide existed in his correspondence with Teller. At the same time as he was organizing his attempt to reconcile scientists' concerns with the AEC's security system, Bethe wrote to Teller about recent press coverage of the H-bomb debate. Transcripts of the Oppenheimer hearing had revealed to the nation for the first time the debate over thermonuclear weapons of four years earlier, and Bethe had become particularly upset about a *Time* magazine article by James Shepley and Clay Blair that essentially credited Teller with the single-handed invention of the H-bomb at Livermore. As such the article implicitly endorsed the AEC's judgment that Oppenheimer had slowed the development of the H-bomb, and explicitly denied the efforts of many scientists at Los Alamos who had worked to develop the weapon. Bethe implored Teller to set the record straight, and admitted that the scientific community had indeed split. "Only you can give such an answer in a manner which will unify the scientific community again," he wrote.

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[54] Richard G. Hewlett and Jack M. Holl, *Atoms for Peace and War: Eisenhower and the Atomic Energy Commission, 1958–1961* (Berkeley: University of California Press, 1989), 111–12.

> An answer from me must necessarily be divisive, and in fact must be hard on you. I should very much like to avoid publishing this answer, just because it is divisive. However, if an answer from you is to be useful, it must be very clear. It must say in effect that there were no essential delays in the Los Alamos work after the President's decision in 1950, except those caused by our technical ignorance.

In closing, Bethe expressed regret for the damage nuclear weapons had done to his personal relationship with Teller, who he had known since 1928 as a physics student in Munich and with whom he had set sail for the United States in 1935. Bethe wrote that he hoped someday they could "again talk about the things we used to talk about meaning the things talked about before 1942."[55]

And the scientist who eventually replaced Oppenheimer as the AEC and military establishment's premier science advisor certainly saw a split—after all, he could take some credit for having caused it. Teller explained his actions many times after the hearing. "I want you to know that whatever I did in the Oppenheimer matter was done after a considerable amount of worry," he confided to one correspondent. "It was done not with the feeling of confidence that my action was correct but simply with the unhappy feeling that I did not know in what way to behave that would be better." Feeling the heat from other scientists, Teller confessed that "I hope that the present strong feelings will quiet down and that there may be perhaps some realization that, in an unfortunate case like that of Oppenheimer, it is unlikely that anyone should be quite right or quite wrong." A sobered Teller reflected that

[55] HB to ET, Nov. 30, 1954, Folder 12.36: (Correspondence –T–) (2), Box 12, HB Papers, emphasis in original. Teller himself called the Shepley and Blair article "disastrous." But Teller's attempt to credit the many Los Alamos scientists, called "The Work of Many People," only exacerbated the situation since, as Teller admitted, secrecy restrictions prevented him from using actual names in the article. Teller, *Memoirs*, 367–68; Peter Goodchild, *Edward Teller: The Real Dr. Strangelove* (Cambridge: Harvard University Press, 2004), 212–13.

"For me personally, there is one real gain in all this. I had an opportunity which does not come to many people, namely to find out what people really think about me."[56]

As years went by, Teller would continue to justify his actions during the hearing. In 1961, Strauss asked Teller to read the draft of a chapter on the Oppenheimer hearing he had written for his memoirs. Teller praised the chapter, and then admitted to Strauss that he had also written on the same subject. "Perhaps it would be wisest if I would refrain from writing about Oppenheimer," Teller suggested. "But I am inclined to believe that being silent about the issues might not be right and might cause as much trouble as to speak out." In the course of his letter to Strauss, Teller managed to craft an explanation that placed the blame for the hearing's verdict squarely on Oppenheimer himself. "I never have told you," Teller wrote Strauss,

> about my visit to Lloyd Garrison [Oppenheimer's lawyer]. I made this visit on the insistent request of Oppenheimer, a request which I felt I should not refuse. When I arrived in Garrison's office Garrison asked me whether I knew the accusations. Since I desired to make the interview as short as possible, I said that I did know them. This was a mistake as I later found out. At that time I had no idea of the most serious nature of the accusations. The following discussion was actually limited to a statement by me which was quite similar to what I later said on the witness stand. All this may have been a mistake but at any rate it has given me the certainty that my statements were not unfair to Oppenheimer. He had many weeks in which he and his attorneys could answer them.[57]

Other influential scientists debated the split for years after the hearing. In 1962, T.F. Walkowicz, a member of the Kennedy administration's Presidential Science Advisory Committee (PSAC), sent a letter to presidential science advisor Jerome Wiesner. Among other subjects, Walkowicz spent a few paragraphs discussing "the political and philosophical

[56] ET to George Stewart, Sept. 23, 1954, Folder: Re: Oppenheimer, J.R., Box 446: Topic Files, ET Papers.
[57] ET to Lewis Strauss, May 26, 1961, Folder 4: Reading File, April–June 1961, Box 422: Chronological Correspondence, 1959–61, ET Papers.

basis of PSAC advice." Walkowicz attempted to convince Wiesner that "PSAC is and has been predominantly represented by . . . 'the Oppenheimer point of view,' and that it has not taken proper account . . . of 'the Teller point of view.'" He doubted that "the all but invisible representation of the Teller 'camp' in PSAC is to be explained by the failure of that camp to produce scientists of appropriate intellectual credentials." Walkowicz closed by writing that "American science is sharply divided," and beseeching Wiesner to do something to repair "this dangerous crack in their own community."[58]

The Oppenheimer hearing undoubtedly changed the relationship between government and scientists, and indeed caused some acrimonious divisions. Oppenheimer's subsequent retreat into martyrdom amid the Siberian emptiness of Princeton is well known. Teller, the primary villain of the hearing, would refuse to discuss the hearing in interviews for years afterward. But predictions that scientists would abstain from government work were well off the mark, although previous scholars have assumed that this was indeed the case. Cassidy, for example, argues that "The Oppenheimer case had all but destroyed a meaningful role for the scientific adviser beyond offering technical possibilities; and it all but eliminated a place in government circles for the thoughtful dissenter from official policies."[59] But Cassidy and other Oppenheimer biographers do not acknowledge the role scientists themselves played in this elimination of dissent. It was scientists' responsibility to accept or reject the state's restriction of their role. Linus Pauling, admittedly an iconoclast, refused a vision of amoral science, and attempted to maintain what he perceived as the dignity and

[58] Walkowicz to Wiesner, June 5, 1962, 3–4, Folder: PSAC 1971, Members and Consultants, Box 5/5, George B. Kistiakowsky Papers, Harvard University.
[59] Cassidy, *Oppenheimer and the American Century,* 333.

independence of science. But plenty of scientists resumed working for disarmament from within the government not long after the Oppenheimer hearing.

PSAC: Scientists Return

After weathering the storm of the Red Scare, scientists continued to participate in government debates over nuclear weapons during the late 1950s, but the state had altered their terms of service. The outcome of the Oppenheimer hearing had changed how government scientists who opposed nuclear weapons expressed themselves in the realm of politics. Dissenting from the AEC's 4–1 decision to deny Oppenheimer security clearance, commissioner Henry DeWolf Smyth had written that the security "system itself is nothing to worship. It is a necessary means to an end."[60] Despite Smyth's plea, the system was worshipped, rather than the values the system was supposed to protect. To maintain access to the government, a scientist had to approach opposition to nuclear weapons from a technical angle. Such an approach may sound easy and even logical, as it did to many scientists who prided themselves on their dispassionate, objective dedication to knowledge. In practice, however, opposing weapons on technical grounds was easier said than done. Scientists troubled by nuclear weapons—as Bethe had been—had to reconcile themselves to their presence at the heart of U.S. Cold War policy. Scientists who wished to work for nuclear disarmament and arms control from within the government had to make a case that limiting nuclear weapons would not harm the security interests of the nation. Scientists outside the government (like Linus Pauling) could simply say that nations should not have nuclear weapons. On the inside of government, exceptions had to be made. Some nuclear

[60] U.S. AEC, *In the Matter of J. Robert Oppenheimer,* 1065.

weapons were necessary—the Soviets had to be deterred. And if some nuclear weapons were necessary, why not more? The logic of the arms race put arms control scientists on the defensive. Scientists who expressed objections to nuclear weapons were at a disadvantage since the arguments they could make were restricted; meanwhile, those who voiced for nuclear weapons could freely use morality to support their arguments.

Far from being "all but eliminated," as Cassidy argues, thoughtful individuals such as Bethe willingly continued trying to shape U.S. nuclear policy. Just as the First World War drove chemists to the poison gas laboratories, and Pearl Harbor and the blitzkrieg sent physicists running to Los Alamos, the combined threat of H-bombs and *Sputnik* sparked a resurgence of scientists' influence in the government during the late 1950s. Far from the dire prediction of former Manhattan Project administrator Harold C. Urey that, after the Oppenheimer case, "Scientists will be reluctant to serve the government in sensitive areas," scientists endeavored to influence policy just three years after the Oppenheimer hearing, eagerly serving on Eisenhower's newly-formed President's Science Advisory Committee (PSAC).[61] It is as though these top scientists endeavored to show that their own predictions would not come true, and that scientists of high quality would not fail to serve their country when called. Rather than force the state to atone for its treatment of Oppenheimer or renounce nuclear weapons, many scientists quickly adapted themselves to the state's new conception of scientists. It seemed all the bitterness of the Oppenheimer fiasco had been forgiven. In a 1959 PSAC meeting with Eisenhower, the man who had given Strauss approval to initiate the Oppenheimer hearing, scientists seemed star-struck as they fawned over the President, telling him they were "grateful to [the] President for making science as

[61] "Scientists Affirm Faith in Oppenheimer," *Bulletin of the Atomic Scientists,* May 1954, Vol. X, No. 5, 188. Urey had joined Szilard's ill-fated attempt to meet with Secretary of State James Byrnes in 1945.

important as baseball. [You] Have entirely changed [the] attitude of [the] country towards science. Progress is speedy [all] over [the] country," an occurrence that "seems almost too good to be true."[62]

Perhaps these scientists thought things might be different by the late 1950s, as the nation came to agree that scientists and science education were once again essential resources for the nation's security. The National Defense Education Act poured millions of dollars into science and engineering education after the *Sputnik* launch exposed a gap between U.S. and Soviet technical abilities. Though never forthrightly stated, scientists may have seen PSAC as a government body capable of balancing the relative hawkishness of the AEC, dominated as the commission was by men like Strauss and Teller. Eisenhower himself appeared to recognize this impulse of scientists. In a television address titled "Science in National Security" delivered on the night of November 7, 1957, Eisenhower set forth his views on the role of science in society. He announced to the American people his intention "to put current scientific discovery at the service of your defense." An obvious reaction to the *Sputnik* launch, Eisenhower's speech explicitly linked science to U.S. military capabilities. The president listed numerous scientific accomplishments, including missiles, "atomic depth bombs" used by the Navy, and nuclear-powered submarines. Science, then, would be a vital element in protecting and promoting U.S. anticommunism, as Eisenhower reminded the nation of the Soviet invasions of Finland, Poland, and "their ruthless suppression of Hungarian freedom."[63]

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[62] "Transcript of Meeting with the President, May 19, 1959," 2, U.S. PSAC, Records 1957–61, Box 1, Meeting Notes: May 1959, Dwight D. Eisenhower Library (hereafter DDEL), quote is from Land.
[63] "Science in National Security," 2–4, transcript of presidential address delivered Nov. 7, 1957, Folder 1.2: General Security Policy, Box 187, Jerome B. Wiesner Papers, MIT (hereafter JBW Papers).

But the President was not only interested bombs and missiles, as he revealed some ambivalence about science. Eisenhower changed gears and stated that "my scientific friends" had told him to begin by addressing "the failure of us in this country to give high enough priority to scientific education and to the place of science in our national life." The President promised the "time, incentive, and skilled teachers" necessary for science education. He also announced his willingness to allow scientific sharing and cooperation with allied nations.[64]

Finally, the President announced the creation of the cabinet-level office of Special Assistant to the President for Science and Technology, a post soon filled by James Killian of MIT. In addition, Eisenhower continued, the science advisor would lead the PSAC, "a strong Advisory Group of outstanding experts." The President then declared that our scientists and engineers, in offering their services to the government in this field, have been generous, patriotic and prompt." As the speech ended, Eisenhower expressed a desire to transcend militarism, hailing (in oddly violent language) "The peaceful contributions of science—to healing, to enriching life, to freeing the spirit" as "the most important products of the conquest of nature's spirit." And more than science, "the spiritual powers of a nation—its underlying religious faith, its self-reliance, its capacity for intelligent sacrifice" were "the most important stones in any defense structure."[65]

At one 1958 meeting, Eisenhower elaborated on the role of science in U.S. society, telling PSAC, "if science is to be used [to fulfill the American dream], [the] individual citizen will have to become closer to and part of the scientific community."[66] He added that because of the rapid growth of science and technology, people "must, in a sense, be a part of the

[64] "Science in National Security," 5, JBW papers.
[65] "Science in National Security," 6–7, JBW papers.
[66] "Transcript of Meeting with the President, 6/17/58," 10, US PSAC 1957–61, Box 1, Meeting Notes: June 1958, DDEL.

scientific community themselves," and that "we must find ways to disperse scientific power to the individual."[67] In a 1959 meeting with PSAC, Eisenhower showed a willingness to embrace the scientists' visions, especially for arms control and disarmament. The meeting memorandum records that PSAC requested

> a systematic and rigorous feeding in of technical factors involved in arms control. The President asked if Dr. Killian felt that any additional charter or directive from him was needed to carry this out, expressing agreement with the principle. He added that just as military men succeed as they work themselves out of a job, the scientists should be working themselves out of the job of devoting their talents to military weapons systems.

Edwin Land then told Eisenhower that "he was grateful for the President's support of scientific activity since the group first met with him . . . . The whole attitude of the country has been turned around . . . and the President's personal part in this has been the decisive factor." He then asked the president if "something could be done to dramatize scientific efforts in behalf of arms control." Scientists who discuss arms control with the military, Land stated, "are immediately suspect as being soft or indifferent to security," even if they happened to be scientists who helped develop nuclear weapons in the first place. Eisenhower responded that "if the scientists can help to show concrete ways to make progress on arms control, he will be most grateful and glad to join in the process."[68]

But as scientists attempted to change U.S. nuclear policy, scientific advice continued to be framed within the state's concept of the appropriate role and rhetoric of scientists. Starting in 1954 and throughout the Cold War, science advisors had to play by the rules set

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[67] Memorandum of Conference with the President, June 18, 1958, Folder 3.5: Test Ban Policy Proposals, 1 of 4, Box 189, JBW Papers. Eisenhower's comments were in response to Land's claim that "we are now coming to a time in which it is very hard to maintain private initiative and private property because so much of what we do—including science—is done by large groups or by the state itself. More and more we tend to resemble the Soviets, however much we disclaim this."
[68] "Memorandum of Conference with the President, May 19, 1959," May 20, 1959, Folder: 3.5: Test Ban Policy Proposals, 1 of 4, Box 189, JBW Papers.

during the Oppenheimer hearing: to hold paramount in their concerns the ceaseless strengthening of the U.S. nuclear arsenal.

A more typical meeting between Eisenhower, his science advisor Killian, and PSAC contained such restrained advice as the following:

> The purpose of the study was to consider technical aspects of our ballistic missile program, including the warning factor. A first point is that Nike-Zeus cannot become a factor in Defense against missiles before 1964 or 1965. Accordingly, such measures as dispersal, hardening and improved warning and reaction all seem more promising than active defense, at least for the near future. He thought that passive tactics were cheaper than active, and should be a basic element in the protection of our retaliatory force.[69]

Under the new regime, political or moral advocacy had no place in scientific advice. When Bethe became a member of PSAC he resigned as vice-president of the Federation of American Scientists. "It would be difficult to reconcile advocating a certain policy from the outside, as the Federation has to do," he reasoned, "and at the same time being an advisor so much on the inside of government. In many matters the recommendations of the Scientific Advisory Committee may be similar to those of the Federation but it would seriously detract from my effectiveness, and from that of the Federation as a whole, if I were to combine those two functions."[70]

By the late 1950s, government science advisors who wished to maintain their influence had dramatically changed their rhetoric since the H-bomb debate; now technical opposition to nuclear weapons replaced moral objections. Continuing to look at Bethe, a 1959 letter finds him arguing for a nuclear test ban not on moral grounds, but because it would lock in U.S. nuclear weapons superiority. "Our development of nuclear weapons has

[69] U.S. State Department, *Foreign Relations of the United States, 1958–1960* (Washington, D.C.: U.S. Government Printing Office, 1996), Vol. III: 184.
[70] Bethe to Paul Doty, Dec. 3, 1957, Folder 23.13: 1956–65, F including FAS, Box 23, HB Papers.

been so successful that not much more remains to be done," he wrote to Nelson Rockefeller, the Republican governor of New York. He continued:

> It is true that always something more can be done, but these additional developments of the future will only contribute in a relatively minor way to our military strength. . . . If nuclear tests are resumed, the Russians are likely to benefit more from such a resumption than we are, simply because their nuclear weapons technology is not quite as far advanced as ours. Ever since 1954 the Russian weapons technology has been gaining relative to ours. On the political side, I am deeply convinced that U.S. security would be far better served by a well considered, reasonably monitored agreement on limitation of armaments to a low level than by an uncontrolled arms race.[71]

In 1962 Bethe considered the same factors when he declared himself no longer in favor of a nuclear test ban. In a speech at Cornell University, where he was a member of the physics faculty, Bethe explained that his previous reasons for supporting a test ban had included a wish "to stabilize the technical advantages which the United States had in 1958 in nuclear weapons." Since the United States no longer had an advantage, Bethe no longer saw any reason to support a test ban.[72] Bethe had been willing to support a test ban on the grounds that it would not harm U.S. national security. But if new circumstances meant that arms control measures hindered "the strongest possible offensive capabilities" of the U.S. military, he could no longer advocate such agreements. Bethe's changing rhetoric from 1950 to 1962 shows how a moral argument against nuclear weapons differed from a technical argument in defense of arms control or disarmament.

In her study of the Oppenheimer hearing, McMillan argues that the verdict solidified—in secret—the U.S. commitment to an arms race. But the government's decision

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[71] Bethe to Rockefeller, Nov. 4, 1959, Folder 4.23: Test Ban: Correspondence, Talks (Outlines), Other People's Articles, Box 4, HB Papers.
[72] Bethe, "Cornell University Lecture, January 5, 1962," 16, Folder: Strategy and Disarmament/ Bulletin, Box 15 (SL) unprocessed collection, HB Papers.

to dedicate the nation to an arms race does not mean that scientists stopped trying to prevent one. Essentially unwilling to learn the lesson from the hearing—that the government wanted scientists to support the nuclear deterrent, not challenge it—many U.S. scientists continued to try and influence government policy in favor of disarmament. The Oppenheimer hearing was hardly the end of science-government interaction, even though Oppenheimer's biographers and other scholars of the nuclear age treat it this way.[73] The hearing had changed the nature of the scientist-government relationship; as late as 1983, Teller still complained that "The controversy over the Oppenheimer hearings is, unfortunately, still having its effects on national security."[74] As a high-profile witness against Oppenheimer, Teller had only himself to blame. For the Oppenheimer hearing and Teller's role in it had shaped the way that those scientists who wished to influence policy (i.e. those at whom the Oppenheimer verdict was aimed) would interact with the government, the public, and other scientists for the remainder of the Cold War.

Not all scientists agreed to exchange moral arguments in return for government influence. The dramatic nuclear scares of the 1950s, including the advent of the H-bomb and the Korean War, mobilized the chemist Linus Pauling in particular. Where Oppenheimer was opacity, Pauling was clarity. He embraced the type of moral-based pacifism that the AEC

[73] Although Oppenheimer is an obvious choice to represent scientists' collision with the atomic age, he should by no means be the only one. Several authors have emphasized Leo Szilard though Szilard has not attained the same level of cultural fame as has Oppenheimer. See Lanouette, *Genius in the Shadows*, and Michael Bess, *Realism, Utopia and the Mushroom Cloud: Four Activist Intellectuals and Their Strategies for Peace, 1945–89* (Chicago: University of Chicago Press, 1993). Linus Pauling, though not a Manhattan Project veteran, could also contend as a figure to represent the scientist during the Cold War. Bethe might also offer historians a different interpretation, and scholars are eagerly anticipating Schweber's long-awaited biography of him.

[74] ET to Nancy Weil, Nov. 4, 1983, Folder 6: Oct.–Dec. 1983, Box 433: Chronological Correspondence, 1981–1991, ET Papers.

had objected to. But where Pauling had the freedom to discuss the ethics and morality of nuclear weapons clarity, government scientists like Bethe had influence.

At the same time, the heightened Cold War tensions mobilized some scientists in the opposite direction in favor of nuclear weapons, specifically Edward Teller. The Oppenheimer hearing had only changed the relationship of scientists to government—it had not yet altered the public perception of science or irrevocably altered science's role in society. Many scientists and politicians still believed that science and scientists had a role to play in solving the nuclear dilemma, even if they faced new constraints. The following two chapters investigate this collision between scientists, social commitment, and nuclear weapons by looking at Pauling and Teller. The outcome of this clash would have consequences not only on science-government relations, but on science's role in society as well.

Chapter Two: Linus Pauling's "Science of Morality"

Mobilizing Science

Just as the U.S. military successfully tested the first thermonuclear weapon in 1954, many U.S. scientists began to reassess their role in the Cold War. As the guardians of nuclear knowledge, scientists were essential to national security. Consequently, U.S. Cold War science was not an objective, isolated endeavor, but rather a contested form of expertise that was mobilized in the name of national security. Two of the main protagonists in this battle were Linus Pauling, the Nobel Prize-winning chemist and leader of a scientific campaign for nuclear disarmament, and physicist Edward Teller, the so-called Father of the H-bomb, who exploited his personal connections and influence to secure for himself and nuclear weapons a prized place within the national security state. As this chapter and the one that follows show, Pauling and Teller took it upon themselves to mobilize science and scientists behind their opposing points of view. The rivalry between these two men played out as a battle for public opinion, but on a deeper level the two waged a war that, in the end, influenced the direction of U.S. nuclear policy and helped redefine the place of science within a Cold War context.

Scientists did not confront the thermonuclear age in any single way. Activism embodied different—often diametrically opposed—political views, as well as different tactics. Pauling's peace campaign provides strong evidence that an overtly moral stand against nuclear weapons was an effective way to mobilize antinuclear sentiment, though not necessarily achieve concrete changes in policy. As a government outsider, Pauling practiced a

brand of peace activism that embraced a scientific, humanistic approach to politics and society, which led him to a life-affirming, moral ideology that opposed nuclear weapons.

Morality was central to Pauling's beliefs about science and nuclear weapons. When the state confronted Pauling's antinuclear challenge, however, it did not do so on moral grounds. In the case of Oppenheimer and other H-bomb skeptics, the state had excluded their views in part by making morality subordinate to anticommunism and faith in the nuclear deterrent. But as this chapter on Pauling shows, the state had more than one way to exclude an antinuclear scientist. While the AEC had primarily relied on a formal process to break with Oppenheimer, suspicions, insinuations, and upfront allegations of communist sympathies had also been effective against the physicist. In Pauling's case, the state relied on similar Red Scare tactics, linking him (falsely) to communism and restricting his travel. Since Pauling was not an establishment figure with powerful allies, the state could easily afford to deal with him in this manner. While Pauling presented himself to the public as an expert who had the scientific authority to challenge the government's nuclear weapons policy, the state responded by questioning his loyalty. At the same time, other influential scientists questioned Pauling's objectivity and helped contest his scientific authority.

Pauling's activism has to this point been frequently dismissed by scholars. Spencer Weart, Robert Divine, and other historians have criticized the scientific claims Pauling made about radioactive fallout in his campaign against nuclear testing as subjective and patently unscientific.[1] But the mantle of objectivity was not the exclusive possession of any one

[1] Weart, *Nuclear Fear: A History of Images* (Cambridge: Harvard University Press, 1988), 203, 212, 214; Robert Divine, in *Blowing on the Wind: The Nuclear Test Ban Debate, 1954–1960* (New York: Oxford University Press, 1978), refers to Pauling's claims as "exaggerated and dubious," 321; Harold Karan Jacobson and Eric Stein, *Diplomats, Scientists, and Politicians: The United States and the Nuclear Test Ban Negotiations* (Ann Arbor: University of Michigan Press, 1966), 479–80. Lawrence Wittner is more polite: "Although some scientific critics of radioactive fallout . . . considered Pauling's claims—as well as Teller's—to be exaggerated, they acknowledged that their movement benefited greatly from his enormous energy and popular

scientist, as claims about fallout on either side of the debate could not be separated from their political implications.[2] In fact, Pauling's campaign was a struggle against the state to determine who in U.S. society would hold the authoritative view on nuclear weapons. To Pauling, nuclear tests were blatantly illogical, as they aimed at enhancing national security but because of fallout resulted in physically harming those the weapons were meant to protect. The dangers of fallout and weapons tests brought into question the entire concept of deterrence, while the scientific aspects of fallout (lethal radiation, dangerous elements, and biological effects) offered an opportunity for scientists like Pauling to play a public role. To dismiss Pauling and his test ban campaign as "unscientific" only reprises the debate over nuclear testing in the 1950s and does little to elevate the understanding of it.

Linus Pauling: The Roots of Activism

Like many of the influential scientists of the nuclear age, Pauling came of age scientifically during the quantum revolution in physics of the 1920s. Though a chemist, Pauling worked with physicists and studied under Arnold Sommerfeld in Munich, one of the epicenters of the revolution in quantum physics.[3] According to one biographer, Pauling quickly grasped the significance of quantum theory, and neatly applied its principles to the field of chemistry in the late 1920s. Pauling soon revolutionized the understanding of the chemical bond, for which he eventually received the 1954 Nobel Prize in Chemistry. Thus like many politically

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appeal." *The Struggle Against the Bomb, Vol. 2: Resisting the Bomb, 1954–1970* (Stanford: Stanford University Press, 1997), 37–39.

[2] See sociologist Kelly Moore's discussion of the Committee on Nuclear Information (CNI). The CNI attempted to maintain political neutrality while making accessible information about nuclear testing and fallout. But this neutrality was actually an act of political expediency rather than personal conviction, as the CNI hoped to avoid charges of disloyalty. Moore argues that, neutrality notwithstanding, the CNI's challenge of government information about fallout served to undermine the more general authority of science in society partly because "neutrality" was often seen by the public as a cover for communism. *Disrupting Science: Social Movements, American Scientists, and the Politics of the Military, 1945–1975* (Princeton: Princeton University Press, 2008), 96–98, 108–117.

[3] Thomas Hager, *Force of Nature: The Life of Linus Pauling* (New York: Simon and Schuster, 1995), 113.

active U.S. scientists, Pauling was by the 1950s an accomplished scientist who had achieved a great reputation before World War II, and was in some ways in danger of having passed his scientific prime. As with Bethe, Oppenheimer, Szilard, and Teller, Pauling could devote much of his time to causes outside the laboratory because he had already made his name as a scientist. Unlike these other scientists, however, Pauling was not a veteran of the Manhattan Project. At Caltech in the late 1920s, Pauling and Oppenheimer had become close friends, and when the awkward physicist became involved with the bomb project, he had invited his gregarious and boisterous chemist friend to join him at Los Alamos. Pauling turned Oppenheimer down, however, because he suspected that physicists would dominate the project; in addition, the friendship had frayed when Oppenheimer made a pass at Pauling's wife, Ava Helen. Notably, at this point no moral considerations prevented Pauling from working on atomic weapons, as he ended up assisting the military with explosives research during the war.[4]

Although Ava Helen may have indirectly kept Pauling away from Los Alamos, she played a much more direct role in shaping the social conscience that drove Pauling after the war. During the Great Depression Ava Helen had encouraged Pauling to take a greater interest in social problems. As fascist movements took hold in Europe and the world approached war, Pauling began to see his life and science in a greater context. A social commitment was further solidified when Pauling read J.D. Bernal's *Social Function of Science,* a Marxist polemic advocating the engagement of scientists in determining the social uses of

[4] Hager, *Force of Nature*, 151, 258, 259.

their knowledge. Bernal, a scientist himself, also predicted that unfettered science could help achieve a social utopia on Earth.[5]

Also pivotal for Pauling was an event closer to home. Living in Southern California during World War II, the Paulings witnessed Japanese internment firsthand, an event so enraging to Ava Helen that she joined the American Civil Liberties Union in order to oppose it. Pauling himself remained relatively aloof from the issue until March 1945, when, without giving it much thought, he hired a Japanese-American gardener, a decision that sparked outrage in the Paulings' Pasadena neighborhood. A vandal scribbled racist, anti-Japanese graffiti on the Paulings' house, and the family also received threatening phone calls. Pauling immediately became infuriated over the racist behavior of his neighbors, and later said he had been politicized by the incident.[6]

After the war, Pauling exhibited a much greater awareness of political events, though Ava Helen still spurred him on. While Pauling often joked that he engaged in social causes merely to impress his wife, her influence went much deeper than that. Pauling recalled that "The humanistic concern she had was very great. I'm sure that if I had not married her, I would not have had this aspect of my career—working for world peace. It was her influence on me and her strong support that caused me to continue."[7] Furthermore, Pauling's subsequent dedication to political activism—to the detriment of his career and reputation—evinced a commitment far beyond the desire to please his wife.

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[5] Hager, *Force of Nature*, 246–247; Andrew Brown, *J.D. Bernal: The Sage of Science* (New York: Oxford University Press, 2005); Gary Werskey, *The Visible College: The Collective Biography of British Scientific Socialists of the 1930s* (New York: Holt, Rinehart & Winston, 1979).

[6] Hager, *Force of Nature*, 296–98.

[7] Linus Pauling, *Linus Pauling on Peace: A Scientist Speaks out on Humanism and World Survival*, Barbara Marinacci and Ramesh Krishnamurthy, eds. (Los Altos: Rising Star Press, 1998, 89–90); Linus Pauling, *Linus Pauling: Scientist and Peacemaker*, Clifford Mead and Thomas Hager, eds. (Corvallis: Oregon State University Press, 2001), 6; Hager, *Force of Nature*, 174, 247.

Like many scientists, Pauling found himself struck by the danger of atomic weapons, and though he had played no role in the atomic bomb's construction and could not in any sense be considered responsible for it, the bomb's magnitude of destruction triggered in him a sense of urgency and responsibility. In addition to his fear of atomic war, Pauling possessed a touch of hubris—a natural self-assuredness that told him scientists could and should lead society to a better future. Endowed with these characteristics, which many Manhattan Project veterans shared at the time, Pauling joined the Atomic Scientists' Movement of 1945–47. Pauling initially gave talks to lay audiences on the atomic bomb itself, early efforts that soon blossomed into an embrace of the atomic scientists' campaign for world government and the abolition of war. Pauling joined the Emergency Committee of Atomic Scientists in 1946 where he met Albert Einstein, whose politically-engaged pacifism Pauling tried to mimic.[8] In these ways Pauling shared a set of beliefs and a sense of purpose with other activist scientists, yet he also stood slightly out of place since he had not taken part in the Manhattan Project. He consequently lacked connections to government circles and political institutions, which enabled him to take an approach different from his scientific peers' dissent against nuclear weapons.

More than anything, Pauling's overwhelming dedication to (some might say obsession with) social and political causes set him apart from other scientists and even more outside the mainstream. In a consequential decision (which Pauling would later recount casually), Pauling announced to Ava Helen that he would devote half his time to science, and half to peace.[9] This simple mathematical formula launched a career in peace and social

[8] Linus Pauling, *Linus Pauling in His Own Words: Selections from his Writings, Speeches, and Interviews*, Barbara Marinacci, ed. (New York: Touchstone Press, 1995), 147.
[9] Pauling, *Linus Pauling in His Own Words*, 150.

activism as eventful as his impressive scientific career. During the late 1940s and early 1950s, as other scientists shied away from activism, Pauling only became more vocal. He first opposed the 1947 National Security Act, then the Rosenberg execution, and later the development of the H-bomb. Pauling achieved his greatest visibility during the fallout scare of the mid to late 1950s, engaging in a high-profile push for a ban on nuclear testing. Science provided hard evidence for his arguments, offered a model of rationality for the world, and allowed him to speak with a voice of authority and a veneer of intellectualism in an age when the average American stood in awe of science. Most importantly, Pauling contested the government's decision, expressed publicly in the Oppenheimer hearing, to forbid moral considerations from hindering the continuous development of a stronger nuclear arsenal.

Pauling's Science-Based Morality

In the years following World War II, Pauling presented his opposition to war, nuclear weapons, weapons tests, and the arms race as equal parts moral and scientific. His two Nobel Prizes, one for chemistry (1954) and one for peace (1963), manifested this duality, as his scientific view of the world told him that peace simply made logical sense. Pauling formulated a science-based morality, or in his words a "science of morality," that relied on science to shape a moral code for society. "The more I studied issues about health, the more I looked around me at a world populated by humans who were in perpetual deadly conflict with one another, for no good reason," he recalled. This moral ideology offered a clear approach to the nuclear dilemma, yet one that risked coming into conflict with the objectivity usually associated with science since it so clearly grappled with contemporary

political concerns. At the same time, Pauling's world view made scientific geniuses such as himself central actors in the nuclear age. He explained: "I thought that people trained to think as scientists might make valuable contributions in searching for solutions to problems in society and in shaping the human future in a positive way. I came to believe that a science of morality is possible."[10]

This science of morality led Pauling to oppose nuclear weapons and war in general, and in particular nuclear testing that spread radioactive fallout. Pauling's crusade harmonized with the mainstream antinuclear activists of the 1950s such as the Committee for a SANE Nuclear Policy (SANE), who also mobilized to protest nuclear fallout. After the U.S. nuclear testing program moved from remote Pacific isles to the Nevada desert, the radioactive byproducts of nuclear explosions scattered into the atmosphere during nuclear weapons tests menaced residents of Nevada and the Southwest. When fallout drifted downwind and began to show up in milk and other foods, many Americans grew concerned that young children were at great risk, since the byproducts strontium 90 (Sr90) and carbon 14 (C14) proved highly likely to bond to human bones. Divine has argued that criticizing fallout was a way for Americans to politely express their more primal fear of nuclear weapons in general.[11] But for Pauling, the link between fallout and the arms race was both concrete and symbolic; the fallout problem deeply offended his scientific and moral principles. In the mid-1950s Pauling, along with scientists Ralph Lapp and Barry Commoner, began to take the lead in bringing scientific expertise to bear on the issue of nuclear testing.[12]

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[10] Hager, *Force of Nature*, 547; Pauling, *Linus Pauling in His Own Words*, 11.
[11] Divine writes, "The test ban became a kind of magic talisman, a way that the nation could confront a real and present danger without coming to grips with the true reality of the 1950s—the possibility of total destruction." *Blowing on the Wind*, 323. Weart endorses this view in *Nuclear Fear*, 212.
[12] For a discussion of Barry Commoner, see Moore, *Disrupting Science*, 96–129.

In his polemic *No More War!*, written over the course of two long weekends and published in 1958, Pauling outlined his science of morality. Although Pauling became a popular public figure during the 1950s, his science of morality clearly fell far to the left of the U.S. political mainstream. He wrote:

> Man has developed admirable principles of morality, which in large part govern the actions of individual human beings. And yet, we are murderers, mass murderers. Almost all of us, even many of our religious leaders, accept with equanimity a world policy of devoting a large part of our world income, our world resources—one hundred billion dollars a year—to the cold-blooded readying of nuclear weapons to kill hundreds of millions of people, to damage the pool of human germ plasm in such a way that after a great nuclear war our descendants might be hardly recognizable as human beings. . . .
>
> I am an American, deeply interested in the welfare of my fellow Americans, of our great Nation. But I am first of all a human being. I believe in *morality*. . . . I believe that there is a greater power in the world than the evil power of military force, of nuclear bombs—there is the power of *good,* of *morality*, of *humanitarianism.*[13]

Pauling continually strived to get scientists from around the world to endorse his views on science. At one point he convened a gathering of scientists in Oslo, Norway, to coincide with a NATO meeting. Over 60 natural and social scientists, including several from the Soviet Union, gathered with Pauling to discuss disarmament.[14] At the end of the conference, the participants released a statement on the "Moral Responsibility" of scientists and scholars that elaborated on Pauling's science of morality and linked scientists directly to the nuclear dilemma. The existence of nuclear weapons, Pauling wrote in the statement, "demands a new understanding of moral responsibility." Nationalism was "obsolete, and loyalty to the whole of mankind is now a necessity." Scientists in particular bore a special responsibility, he wrote, "to make plain the full significance of the revolutionary weapons

[13] Pauling, *No More War! 25th Anniversary Edition* (New York: Dodd, Mead & Company, 1983), 215–17, emphasis in original.
[14] Hager, *Force of Nature*, 528, 531–32.

development of the past decade," to give "factual information," and to provide "constructive proposals" in search of peace.[15]

This growing sense of morality and responsibility became an obsession with Pauling. While preparing articles and speeches, Pauling would jot his thoughts down on paper. Some of his notes read: "Great progress can be made following and built upon moral principles"; "Transformation of evil into good—i.e., possibility of decreasing suffering in the world"; "My ideas—Consistency not necessary in theory of values of morality or ethics (any more than in chemistry)"; and "The prime ethical postulate—We should take such actions as to preserve and increase the wonder of the universe & to decrease human (and other) suffering."[16] In another unguarded moment, Pauling scribbled "I love this world. I believe that we can prevent this insanity" below a series of megaton calculations.[17] Pauling often exhibited utopian tendencies, as when he wrote in *I.F. Stone's Weekly* that: "I believe that war is immoral, and must be eliminated. I believe that there will never be a great nuclear war."[18]

On a fundamental level, Pauling's campaign against nuclear testing embodied his mix of science and morality by relying on scientific data that supported moral arguments against nuclear weapons, as manifested in his 1958 article in *Science,* "Genetic and Somatic Effects of Carbon-14." Buttressing his arguments against nuclear weapons testing was scientific proof of the effects of radiation on young people and children yet unborn, even "thousands of years" into the future. The half-life and radioactivity of the elements scattered by a nuclear

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[15] Pauling, *Linus Pauling on Peace*, 127.
[16] Pauling, *Linus Pauling on Peace*, 164–65.
[17] Undated notes, "Threat of destruction—Terrible danger," LP Peace, 6.0: Other Peace Activism, 6.012: Pauling Peace Research Notes, 12.1: Assorted Pauling Peace Research Notes, 1930s–1940s, Linus Pauling Papers, Oregon State University (hereafter LP Papers). [The quoted document is clearly from the 1960s, not the 1930s or 1940s, as it mentions the H-bomb and MIRVs.]
[18] "An Exclusive: Nobel Prize Winner Linus Pauling Answers Edward Teller's Recent Life Magazine Article on 'The Compelling Need for Nuclear Tests,'" *I.F. Stone's Weekly,* February 24, 1958, LP Peace 6.0: Other Peace Activism, 6.001: The Fallout Suits, 1958–62, 1.3: Publication: "An Exclusive . . . Nuclear Tests," LP Papers.

test ensured, Pauling argued, that the Earth would suffer the consequences of these effects for decades to come. By using children as a form of scientific data, Pauling's argument supported a moral interpretation of the nuclear weapons dilemma—otherwise, the death or deformation of millions of children simply would not have been a problem. But the argument also cloaked itself in the dispassionate language of science. Pauling rationally calculated the danger of fallout as "the predicted number of children born with defects caused by the mutations induced by the radioactivity."[19]

Pauling thus culled information released by the AEC on bomb tests, and used his own knowledge to interpret the numbers. His calculations published in "Genetic and Somatic Effects" convinced him that the AEC had grossly underestimated the threat posed by fallout. "My estimate," he wrote, "is that the fission products in radioactive fallout from the bomb tests to date will in the course of generations cause the birth of 80,000 children with gross physical or mental defects, 300,000 to die at birth or in early childhood, plus 700,000 embryonic deaths. About one tenth of the defective children will appear in the first generation, and the others in later generations."[20] In Pauling's interpretation, science revealed the absurd cruelty of the arms race by objectively proving the government's willingness to sacrifice children in the name of national security.

Overall, Pauling dedicated more of his career to science than peace (he was a scientist long before he became an activist, and he eventually drifted from peace activism). But for the rest of his life after World War II, Pauling's professional interests reflected a concern for the well-being of humanity, including research into the causes of sickle-cell

[19] Pauling, "Genetic and Somatic Effects of Carbon-14," *Science,* November 14, 1958, Vol. 128, No. 3333, 1183.
[20] LP to Robert Gilmore, undated, LP Peace 4: Peace Groups, 4.003: SANE, 1958–1966, 3.1: Correspondence: SANE, Local Chapters, 1958, LP Papers.

anemia as well as his vitamin-C crusade of the 1980s, which trumpeted the vitamin as a guarantor of perfect health and a cure for cancer. And at times during the 1950s and 1960s, peace appeared to become more of a priority than science. According to some accounts, Pauling's science seemed far less solid than his courage; several of his contemporaries accused Pauling of being more loyal to his political objectives than his scientific evidence. Pauling himself admitted that his scientific work at the time was not "wholehearted," and he willingly accepted that his activism harmed his scientific credibility. "I have not regretted my peace activism," he wrote late in life, "although this has damaged my reputation as a scientist among certain people and institutions."[21]

Pauling's wielding of scientific knowledge gave him the confidence to challenge convention and agitate for disarmament. Yet his abrasive and smug personality may have worked against him at times, as he believed scientists—and only scientists—were smart enough to guide the world away from nuclear war. In the late 1950s, Pauling wrote to a *Newsday* reporter who had written an article on fallout. "The article seems to me to be thoroughly unsatisfactory, in that it repeats the meaningless statements made by AEC spokesmen and others," Pauling scolded the journalist, adding, "I think that you should have given some numbers." He concluded, "I know that the problem is a difficult one, and that it is too much to expect that it should be treated accurately by people other than scientists."[22]

Pauling's activities reflected the belief that scientists had special knowledge to bestow upon the world in the hopes of a better life, a concept that shared much with the Atomic Scientists' Movement. Leo Szilard, who proved the most energetic activist-scientist of the

[21] Pauling, *Linus Pauling in His Own Words*, 12, 150; see also Pauling, *Linus Pauling on Peace*, 114.
[22] LP to Richard Wyse, May 15, 1958, Folder: Pauling-Teller Debate, Box 437: Topic Files, Edward Teller Papers, Hoover Institution (hereafter ET Papers).

1940s, also believed that scientists had a great deal to add to policy debates.[23] The Atomic Scientists' Movement accepted without question the notion that scientists had to educate Congress and the public. In certain terms, Pauling's career in social causes can be seen as a direct continuation of this earlier movement. On the other hand, Pauling differed greatly from the atomic scientists in his tactics and iconoclastic political behavior.

Pauling as Government Outsider

Pauling consistently held an oppositional stance toward the government regarding nuclear weapons policy and civil liberties. He publicly opposed the Rosenberg execution in 1953 and vociferously denigrated the Oppenheimer hearing a year later. With many others, Pauling joined the chorus in the *Bulletin of the Atomic Scientists* voicing support for his former friend Oppenheimer, adding his opinion that "Many thoughtful scientists will conclude that it is dangerous to make an important contribution to the national welfare, and that they should not accept employment in government agencies, or, if they do, that they should be careful that their contributions are not important."[24]

By the mid 1950s, of course, Pauling had no illusions about being a government advisor, since the Oppenheimer hearing had announced that the government wanted science advisors to uphold the nuclear deterrent. Because the state distinguished between appropriate and inappropriate types of scientists and arguments, Pauling's moral arguments

[23] Szilard could be alternately humble and pompous about this. In his story "The Voice of the Dolphins," Szilard wrote, "Political issues are often complex, but they are rarely anywhere as deep as the scientific problems which were solved in the first half of the century. These scientific problems were solved with amazing rapidity because they were constantly exposed to discussion among scientists, and thus it appears reasonable to expect that the solution of political problems could be greatly speeded up . . . if they were subjected to the same kind of discussion." But elsewhere he said, "I am a scientist and science, which has created the bomb and confronted the world with a problem, has no solution to offer to this problem. Yet a scientist may perhaps to be permitted to speak on the problem of peace, not because he knows more about it than other people do, but rather because no one seems to know very much about it." Quoted in Michael Bess, *Realism, Utopia and the Mushroom Cloud: Four Activist Intellectuals and Their Strategies for Peace, 1945–89* (Chicago: University of Chicago Press, 1993), 53, 84.

[24] "Scientists Affirm Faith in Oppenheimer," *Bulletin of the Atomic Scientists,* May 1954, Vol. X, No. 5, 188–89.

and maverick behavior had consequences for his activism both negative and positive. Freedom from government restrictions allowed him to pursue objectives and ideals beyond those that official science advisors could consider. Pauling was able to rely far more heavily on moral arguments against nuclear weapons, though he of course based these arguments in scientific reasoning. On the other hand, however, Pauling had no access to classified government sources—his attacks on government policy simply consisted of AEC statements and other public information subjected to his own scientific analysis and criticism.[25]

The most well-known example of this method was his "Genetic and Somatic Effects" paper in *Science*. The article acted as a scientific challenge to and refutation of a 1956 paper by AEC commissioner Willard F. Libby (also published in *Science*) that had argued that fallout posed little risk to humans. "Local precautions," Libby had concluded, "should be entirely adequate and the worldwide health hazards from the present rate of testing are insignificant."[26] "Genetic and Somatic Effects" was a vibrant warning about the dangers of thermonuclear weapons tests, and it offered a distinct and original challenge to national security policy and the arms race. Pauling began the paper by explaining that Libby had previously claimed that the problem of fallout came from the "fission products" of a thermonuclear explosion, thus implying that as fusion weapons evolved, their risks would disappear.[27] But according to Pauling, a Russian paper provided evidence of the dangers of Carbon 14, a byproduct produced during the fusion segment of thermonuclear explosions. The heart of Pauling's paper consisted of calculations of the danger posed by C14 based on the amount of testing being conducted. The results, Pauling concluded, offered great cause

[25] Pauling, *Linus Pauling on Peace*, 113.
[26] W.F. Libby, "Radioactive Fallout and Radioactive Strontium," *Science,* April 20, 1956, New Series, Vol. 123, No. 3199, 657–60.
[27] Early fusion weapons used a fission bomb to trigger thermonuclear fusion.

for concern: "It is concluded that 1 year of testing (30 megatons of fission plus fusion) is expected to cause in the world (estimated future number of births per year 5 times the present number) an estimated total of about 55,000 children with gross physical or mental defects, 170,000 stillbirths and childhood deaths, and 425,000 embryonic and neonatal deaths. . . . These numbers are about 17 times the numbers usually estimated."[28] Because information about how the H-bomb worked was at this point still highly classified, Pauling was in a literal sense making an educated guess.

Pauling later defended his paper, claiming that the editor of *Science* "kept sending new memos to people in the atomic energy business, who wrote back saying that there was something wrong." Pauling continued to frame the paper as the result of objective and scientific reasoning, and later explained that his findings inspired in him an urgency to speak out about the figures. "I was sort of stuck with it," Pauling recalled. "I felt that it was my duty to continue to work along these lines." Pauling crafted an image of himself as a noble crusader for justice supported by undeniable scientific evidence. "I gave over 500 lectures about radioactive fallout and nuclear war and the need for stopping the bomb tests in the atmosphere and the need for eliminating war ultimately. I didn't enjoy giving these lectures especially," he said.[29] Such an image contains some truth but also false modesty, as Pauling had consciously decided to dedicate half of his time to social causes.

Though he may have yearned to return to purely scientific work, his moral code and self-importance compelled him to speak out. "I had a strong feeling about the morality of carrying out these problems," Pauling offered as an explanation of his *Science* article.[30] The

[28] Pauling, "Genetic and Somatic Effects," 1186.
[29] Pauling, *Linus Pauling on Peace*, 114.
[30] Pauling, *Linus Pauling on Peace*, 113.

article did create a minor stir in political circles. In 1959 the Subcommittee on Radiation of the Congressional Joint Committee on Atomic Energy had taken Pauling's claims about fallout seriously enough to ask its advisors for an analysis and opinion of his *Science* article.[31] But as an outsider, Pauling generally had no access to sources except those publicly available; his archives contain numerous newspaper articles, glued to writing paper, with notes and calculations scribbled all over them. Like any other ordinary citizen, Pauling had to get his official information from sources such as the *New York Times*.[32] This restriction did not mean that his views were invalid, but it did mean that government officials could dismiss his claims as uninformed. He had the freedom to say what he wanted, but lacked the authority of being able to base an argument on "official" sources. In this way, Pauling could be fairly influential with the public, but not with the government, where his claims were easily marginalized.

His adamant opposition to the nuclear deterrent and the arms race obstructed him from government influence, but the government did not attack Pauling's moral message. Instead, the state used Red Scare methods to dilute his influence; Pauling claimed that the FBI began keeping tabs on him as early as 1950.[33] This oppositional relationship certainly suited Pauling's temperament, as he and the government wanted nothing to do with each other. Pauling's constant grappling with the moral dimensions of U.S. policy led him to question the entire Cold War and the nation-state system, including the Red Scare blacklist, loyalty oaths, the Smith Act, and the McCarran Act. Such outspoken criticism from such a

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[31] U.S. Congress, Special Subcommittee on Radiation of the Joint Committee on Atomic Energy, *The Nature of Radioactive Fallout and its Effects on Man*, 85th Cong., 1st sess. Hearings, May 27–29 and June 3–7, three volumes (Washington, D.C.: U.S. Government Printing Office, 1957), 19, 691, 1989, 1991; and U.S. Congress, Special Subcommittee on Radiation of the Joint Committee on Atomic Energy, *Fallout from Nuclear Weapons Tests*, 86th Cong., 1st sess. Hearings, May 5–8, four volumes (Washington, D.C.: U.S. Government Printing Office, 1959), 60, 2347–62, 2455–60, 2462–65.
[32] See, for example, "Test ban," March 3, 1961, LP Peace 6: Other Peace activism, 6.012: Pauling Peace Research Notes, 12.3: Assorted Pauling Peace Research Notes, 1960s, LP Papers.
[33] Pauling, *Linus Pauling on Peace*, 63; Hager, *Force of Nature*, 339, 343.

notable individual spurred the Red Scare apparatus into action. As with many critics of the Cold War, Pauling found himself slandered in the press as a Red.[34] In 1952, the State Department began to restrict Pauling's travel abroad, telling him that "your proposed travel would not be in the best interests of the United States," and refusing to grant him a passport, essentially punishing the scientist for his leftist stand and making his scientific career more difficult. Only when Pauling won the Nobel Prize for Chemistry in 1954 was the State Department shamed into issuing him a passport so that he could accept the award.[35]

The Nobel Prize gave Pauling more than just the highest scientific honor in his discipline. Pauling now had instant fame, credibility, and job security. But having reached the highest peaks of science, Pauling's subsequent behavior indicated a yearning for a role in the world beyond science. As Pauling increased his activism, the government maintained its efforts to stifle the scientist. Though the worst excesses of McCarthyism had peaked in 1954, the Red Scare continued into the late 1950s and early 1960s. And despite historians who see the Oppenheimer hearing as the final act in the government's prosecution of leftist scientists, such practices continued.[36] Senator Thomas J. Dodd (D–CT), head of the Internal Security subcommittee, had built a reputation as a Democratic counterpart to Joseph McCarthy, as he conducted a series of hearings in 1960 alleging that communists had infiltrated SANE and other antinuclear organizations. On June 21, Dodd hauled Pauling in

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[34] Hager, *Force of Nature*, 356.

[35] Hager, *Force of Nature*, 400. When he was not protesting nuclear weapons in the early 1950s, Pauling was participating in the race to uncover the structure of DNA. The State Department had refused his passport request after he had accepted an invitation to address the British Royal Society about protein structures. But learning the most recent British advances in DNA research was also on his agenda. Looking back on his travails, Pauling claimed that had he traveled to Britain, he would have seen Rosalind Franklin's x-ray photos of DNA and, in his words, "well, who knows?" Pauling, *Pauling on Peace*, 67. Franklin's photos eventually led James Watson and Francis Crick to recognize DNA's double-helical structure, possibly the greatest scientific discovery of the twentieth century.

[36] Jessica Wang argues in *American Science in an Age of Anxiety: Scientists, Anticommunism, and the Cold War* (Chapel Hill: University of North Carolina Press, 1999), that though the Oppenheimer hearing was the highest profile case, it was only the culmination of the government's purge of leftist scientists.

front of the subcommittee to answer questions about his petition against nuclear testing that had gained over 11,000 signatures—evidence, in Dodd's mind, that the petition had been organized by communists. The *Bulletin of the Atomic Scientists* described the hearings as "in effect a loyalty screening of Mr. Pauling."[37] Harry Kalven, the University of Chicago law professor who wrote about Pauling's congressional appearance for the *Bulletin*, drew comparisons to the Oppenheimer hearing of six years earlier. Although both scientists suffered harassment for their leftist views, Pauling fared much better than Oppenheimer. Kalven wrote that Pauling "is so witty and effective in defending himself that one cannot down the impression, despite the content of some of the questions he was asked, that he was having a very good time."[38]

In 1961 Dodd's subcommittee published *The Pugwash Conferences: A Staff Analysis*, hoping to discredit Pauling by virtue of his association with a suspicious group of scientists. Most of the document consisted of material on the Pugwash conferences, a series of meetings that brought together scientists from both sides of the Iron Curtain interested in disarmament. Dodd's document first attempted to discredit the conferences as tools of Soviet propaganda and U.S. participants as unwitting dupes. "In most of the contacts that have thus far taken place," the tract began, "the free world scientists, although they have sometimes argued strongly, have not been able to compete with their Communist counterparts. The extensive use which the Communist propaganda apparatus has made of the Pugwash conferences is proof enough of this."[39] The Soviets, the document explained,

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[37] Harry Kalven Jr., "Congressional Testing of Linus Pauling, Part I: The Legal Framework," *Bulletin of the Atomic Scientists*, December 1960, Vol. XVI, No. 10, 383–90; Harry Kalven Jr., "Congressional Testing of Linus Pauling, Part II: Sourwine in an Old Bottle," *Bulletin of the Atomic Scientists*, January 1961, Vol. XVII, No. 1, 12–19, quote is from 15.
[38] Kalven, "Sourwine in an Old Bottle," 13.
[39] U.S. Senate, Internal Security Subcommittee of the Committee on the Judiciary, *The Pugwash Conferences: A Staff Analysis*, 87th Cong., 1st sess. (Washington, D.C.: U.S. Government Printing Office, 1961), 1.

came to the Pugwash conferences with an imperialist agenda, and hoped to exploit the conferences for subversive purposes.

The Dodd subcommittee analysis explicitly highlighted Pauling's participation: "The enlistment of Linus Carl Pauling in support of the Pugwash Conferences added a certain amount of academic prestige to these conclaves." Failing to provide any direct evidence of Pauling's alleged communist sympathies, the Dodd document listed numerous instances in which Pauling was mentioned or praised by socialist and communist rags like the *Daily Worker*, intimating that such mention provided evidence of a "predisposing framework"—i.e. communist sympathies—on Pauling's part. "During the past decade," the report concluded, "Pauling has appeared in the public eye with noticeable frequency as a crusader for distinctly political aims, and has shown a marked bias for Communist causes and a willingness to aline [sic] himself with Communist-held views having no scientific bearing or interest."[40]

Despite dozens of pages of allegations, the Dodd committee had no crimes with which to charge Pauling, and although the document linked Pauling to Pugwash, the chemist had taken part in only two conferences by the time the Dodd report appeared.[41] Furthermore, linking Pauling to Pugwash was especially misguided, since at the time Pauling and Pugwash scientists wanted little to do with each other. Dodd's published smears can only be seen as an attempt to make sure that those in power would not take Pauling (or anyone even loosely affiliated with Pugwash) seriously. The direct influence of the Dodd document is unclear, but the government undeniably avoided Pauling and excluded his

[40] *The Pugwash Conferences*, 54–55.
[41] Pauling went to four Pugwash conferences in all: the second Pugwash conference in April 1958, the third in September 1958, the seventh in September 1961, and the tenth in September 1962. See Joseph Rotblat, *Scientists in the Quest for Peace: A History of the Pugwash Conferences* (Cambridge: MIT Press, 1972), 105.

influence from the privileged circle of scientists dealing with nuclear weapons and disarmament policy. The scientists who did shape U.S. nuclear policy at the time clearly did not value Pauling's claims about fallout. Discussing improvements in seismic detection methods to enforce a test ban during a meeting with the State Department, the CIA, and the Department of Defense, the presidential science advisor James Killian "pointed out that it would be dangerous to propose an agreement to ban atmospheric tests, since that would imply that the fallout hazard was real."[42]

Thus Pauling stood as a public figure with the credibility of a scientist and the renown of a Nobel Prize-winner, but also the stigma of a leftist in the eyes of the government. Pauling returned the government's contempt in a noticeably bipartisan manner. Pauling sued the Eisenhower administration for risking his life by exposing him (and the rest of the population) to fallout from nuclear tests; when John F. Kennedy replaced Eisenhower, Pauling filed an identical suit against the Kennedy administration.[43] In 1963, after Pauling won the Nobel Peace Prize, the White House arranged a meeting between the President and the scientist. Just before his meeting with Kennedy, Pauling picketed the White House with antinuclear activists angered by Kennedy's rush to complete nuclear tests before the Limited Test Ban Treaty took effect. Determined to belittle politics, Pauling refused to ask for access to government circles; nor was the government willing to grant influence to such a leftist. In this way Pauling saw himself as following in the tradition of the first modern celebrity scientist to embrace pacifism: Albert Einstein, who personally admired

[42] U.S. State Department, *Foreign Relations of the United States, 1958–1960* (Washington, D.C.: U.S. Government Printing Office, 1996), Vol. III: 694.
[43] See LP Peace 6: Other Peace Activism, 6.001: The Fallout Suits, 1958–62, 1.9: Correspondence re: the Fallout Suits, 1958–62, LP Papers.

and encouraged Pauling's activism.[44] Pauling saw himself as his own entity beyond government, and helped generate substantial antinuclear sentiment in the 1950s and 1960s based not on access to government but on his own moral and scientific authority, a challenge the government contested. But the government was not the only factor limiting Pauling's influence. His vision of science was also contested by those he saw as indispensable for his efforts to lead scientists away from militarism and toward morality: scientists themselves.

Pauling and His Peers

Pauling saw the superpowers as reprehensible for risking human annihilation during the Cold War. Since states would bear the brunt of his criticism, Pauling placed his faith in the public by embracing activism that encouraged grassroots dissent. Pauling's peace activism proved popular among ordinary protesters as well as more refined activists including the Nobel committee, who awarded Pauling the 1963 Peace Prize. Such acclaim shows that Pauling's message of social commitment effectively moved people during the Cold War, and that a scientific critique of nuclear weapons resonated with many people.

But Pauling's insistence on basing his crusade in a particular vision of "science" proved particularly galling to many elite U.S. scientists. Pauling steadfastly maintained a belief that science and the scientific method formed the basis of a peaceful and moral society, and although he spoke to the public at large, scientists remained an essential

[44] Einstein wrote to Pauling: "It is very meritorious of you to fight for the right to travel. The attitude of the government corresponds, of course, to the state of transition toward a kind of totalitarian state in which we find ourselves. The simple means to make this possible is the uninterrupted repetition of the lie that the country is in mortal danger. Behind this attitude is the intention to crush the Soviet Union by war. As long as this tendency prevails the fight for the conservation of the democratic rights seems to me not promising. But not to fight is still worse. . . . The fact that independent minds like you are being rebuked equally by official America and official Russia is significant and to a certain degree also amusing." Albert Einstein to LP, May 21, 1952 [2.32], LP Safe Contents, Drawer 2, Folder 2.002, LP Papers.

constituency for his movement. This high profile mixing of science with activism asked scientists to take a side: peace and morality, or militarism and Cold War. Though many aligned with or against him, a number of scientists refused these options, and opted instead for a third way of non-alignment.

The Pauling Appeal

Even at the very start of his activism, Pauling saw himself as speaking for all scientists. This grandiose tendency was especially risky because Pauling hoped to spark outrage and encourage activism among scientists, essentially enlisting the entire discipline of science behind his political movement. Delivering a sermon at a Unitarian church in August 1951, Pauling claimed that scientists had failed their moral and social obligations: "I deny that scientists have been guilty in making their discoveries. They have, however, failed in some part to do their duty as citizens"[45] Pauling spent much of his time in 1957 attempting to enlist scientists around the world to sign his petition to stop nuclear testing. Pauling's appeal claimed an authority based on the idea that science revealed truth. When confronted with the objective assertions of scientists, Pauling hoped world leaders would halt the arms race.

The appeal urged that "an international agreement to stop the testing of nuclear bombs be made now." As with all of Pauling's activism, the petition drew its moral authority from scientific evidence, reading: "Each nuclear bomb test spreads an added burden of radioactive elements over every part of the world. Each added amount of radiation causes damage to the health of human beings all over the world and causes damage to the pool of

[45] Pauling, "Science in the Modern World," 1, Aug. 12, 1951, LP Peace 4: Peace Groups, 4.001: First Unitarian Church, [Rev. Stephen H. Fritchman] Correspondence, 1948–1980, Folder 1.1: Correspondence: Rev. Stephen H. Fritchman, 1948–1980, LP Papers.

human germ plasm such as to lead to an increase in the number of seriously defective children that will be born in future generations." The third paragraph argued that continued testing will increase the likelihood of nuclear war, while the fourth claimed that a test ban would act as a "first step" toward disarmament. The final paragraph made a declaration about the social responsibility of all scientists: "We have in common with our fellow men a deep concern for the welfare of all human beings. As scientists we have knowledge of the dangers involved and therefore a special responsibility to make those dangers known. We deem it imperative that immediate action be taken to effect an international agreement to stop the testing of all nuclear weapons."[46] The petition traveled first to U.S. scientists and then around the world; eventually over 11,000 scientists added their names to the petition, which Pauling presented to the United Nations on January 13, 1958.[47]

Pauling's presumption to speak for all of science apparently rubbed many U.S. scientists the wrong way, feelings that came out in private and public. Helen C. Allison addressed Pauling's campaign in the *Bulletin of the Atomic Scientists*, beginning with some perfunctory compliments. "As a speaker," she wrote, "Pauling is humorous, personal, and very, very self-confident. He seeks to arouse the idealism of people, especially young people, to work for ends all sane men agree with—a test ban, disarmament, peace, freedom, survival." But Pauling was, it seemed to Allison, more of a threat to the hallowed name of science than to the proponents of the arms race. "To the more mature knowledgeable person," Allison continued, "his oversimplifications and unqualified absolutes tend to seem irresponsible. On the question of justification for the Hiroshima and Nagasaki bombings—

[46] Pauling, *No More War,* 180–81.
[47] LP to Dag Hammarskjöld, Jan. 13, 1958, LP Peace 5: Nuclear Bomb Test and Proliferation Petition, 5.002: *An Appeal by Scientists to the Governments and People of the World*, 1957–58, 2.4: Correspondence: Dag Hammarskjöld, United Nations, 1957–58, LP Papers. Pauling specifies that 11,021 scientists signed the document in *No More War!*, 187.

he sees none whatsoever—he uses hindsight to distort the realities of a nation at war." Allison sarcastically blamed "the gap between the crusader attacking large evils from the outside and the man in [a] responsible position having to make difficult decisions" for the "certain caution with which many of Pauling's scientific colleagues regard him." She noted that "very few outstanding scientists" deigned to sign his petition, and concluded that although Pauling was to be admired for his "independence, courage, and fighting qualities," he nevertheless "cannot be regarded as the spokesman for the scientific community."[48]

Even late in life, Pauling brazenly continued to claim the exact opposite. In a 1977 PBS interview, Pauling stated that "Some scientists said that they considered me a spokesman for science in these matters [the study of fallout], as I was to some extent with respect to the bomb test treaty. There were many other scientists working on this problem. . . . I happened to be the one who stirred up feeling about it."[49] Undoubtedly he had scientists who supported him, notably Barry Commoner of Washington University in St. Louis, an activist-scientist in his own right. But plenty of scientists refused to embrace Pauling's scientific vision. Instead of halting the arms race, Pauling's petition revealed that many scientists' professional identity resisted connecting science to specific social and political concerns.

At first, Pauling offered his appeal exclusively to U.S. scientists and received about 2,000 signatures which he submitted to the White House in the summer of 1957.[50] Pleased with his success and cognizant of the internationalism of science, he then expanded the

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[48] Helen C. Allison, "Outspoken Scientist," *Bulletin of the Atomic Scientists*, December 1960, Vol. XVI, No. 10, 382, 390.
[49] Pauling, *Linus Pauling On Peace*, 114, elided text in original.
[50] See LP to Eisenhower, June 4, 1957, LP Peace 5: Nuclear Bomb Test and Proliferation Petitions, Section 5.002: *An Appeal by Scientists to the Governments and People of the World,* 1957–1958, 2.3: Correspondence: Dwight D. Eisenhower, June 4, 1957, LP Papers.

petition to scientists worldwide. As always, Pauling mixed the moral with the scientific. To Princeton scientist James Arnold, Pauling explained: "I want tests stopped for two reasons. First, as a step toward disarmament, and second, because of the moral issues involved in injuring the nationals of other countries to any degree whatever."[51]

But in fact the scientific community failed to reflect the unity asserted by Pauling's petition. Although over 11,000 scientists did sign, many refused to do so. Remarkably, Pauling kept 103 notable letters of refusal in a safe in his office which reveal a significant amount of division among scientists.[52] In fact, Pauling himself compiled a list of nine types of rejections. For the purposes of this analysis, three kinds of disagreement stand out: scientific objections, activist objections, and complete opposition to Pauling's crusade. (Of course a few letters defy such strict categorization.) Such sources provide the opportunity to witness how scientists debated the role of science in politics and activism, the proper behavior of scientists, and the credibility, authority, and responsibility of scientists in the nuclear age.

Scientific Disagreement

Some scientists begged off signing the petition but maintained that their refusal to sign had nothing to do with either a test ban or the politics of the statement; instead, it was simply a matter of lacking the right expertise. Referring to the Pauling petition as "a diadactic [sic] statement regarding knowledge which I do not possess," Milton Burbon at Notre Dame told Pauling that "It is hard to get unprejudiced information. I know that political as well as scientific matters are involved in the decision. Scientists disagree on expressed

[51] LP to James R. Arnold, Dec. 10, 1957, [1.100] LP Safe Contents, Drawer 2, Folder 2.001, LP Papers.
[52] In *Resisting the Bomb*, Wittner writes that most biologists approved of the petition, while physicists were "more standoffish," 38.

interpretations of the data and on conclusions." He concluded that "I do not feel sufficiently well informed to arrive at a reliable scientific conclusion." Burbon assured Pauling that he would make his decisions about political issues "more on the basis of moral and ethical principles than on any scientific basis," essentially telling Pauling that science and politics should not mix.[53] David Inglis of Argonne National Laboratory told Pauling "I agree completely that nuclear tests should be stopped." Inglis did not sign, however, explaining "I find it hard to sign a statement implying that I have special knowledge about germ plasm, which I regret I don't have."[54] Similarly, Robert Brode of UC Berkeley felt he could not sign because the statement "bears essentially on the analysis of the genetic damage that will come to the pool of human germ plasm." Brode explained that "I am not a specialist in this particular area and . . . a signature to this Appeal would inevitably be considered an attempt on my part to assume scientific competence in an area in which I am not competent." Brode closed by expressing hope that Pauling would seek responses "from those who are competent to express an opinion on this matter."[55]

Felix Bloch claimed that a lack of expertise prevented him from signing, yet his letter contained an implicit rebuke to Pauling and his mixture of science and politics. Then at Stanford, Bloch wrote: "The trouble is that, while I am afraid of the possible damage done by nuclear tests, I really do <u>not</u> have the factual knowledge which would permit me to positively affirm the existence of such a danger." Bloch added, perhaps in a dig at Pauling, that "I would feel rather irresponsible in making a statement without such knowledge."[56]

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[53] Burbon to LP, May 27, 1957, LP Peace 5: Nuclear Bomb Test and Proliferation Petitions, 5.002: *An Appeal by Scientists to the Governments and People of the World*, 1957–68, 2.1: Correspondence, Notes re: circulation and signing of *An Appeal by Scientists to the Governments and People of the World*, 1957–69, LP Papers.
[54] Inglis to LP, May 17, 1957, [1.6] LP Safe Contents, Drawer 2, Folder 2.001, LP Papers.
[55] Brode to LP, May 23, 1957, [1.9] LP Safe Contents, Drawer 2, Folder 2.001, LP Papers.
[56] Bloch to LP, May 20, 1957, [1.7] LP Safe Contents, Drawer 2, Folder 2.001, LP Papers, emphasis in original.

Other refusals voiced more complex concerns about the credibility of science and how Pauling claimed the mantle of science. This group overtly worried about applying science to support a political point of view and saw Pauling as behaving unscientifically. In this way, Pauling's social commitment prevented him from getting the support of some scientists. In addition, these letters revealed that many scientists' professional identity supported the government's view that opinions on nuclear weapons based in morality were inappropriate for scientists, and that fundamental opposition to nuclear weapons was not within the range of acceptable technical advice.

Percy Bridgman of Harvard refused to sign the appeal because, he argued, "the danger of genetic damage from the tests alone is negligible." Although Bridgman admitted he would sign a statement on the need to abolish war, "I would be willing to sign only in my capacity as a citizen and not as a physicist."[57] The Danish scientist P. Brandt Rehberg excused himself from signing the petition because

> it is necessary that scientists as a group confine themselves to scientific matters and as a group speak only on matters where the individual members of the group have sufficient knowledge to enable them to form a real scientifically founded opinion on the matter. . . . I doubt, however, that all 2000 scientists who have signed your appeal have such a background knowledge that they may justly be called authorities on the subject.

Such a "false authority," Rehberg wrote, could "weaken the confidence in general scientific statements. The number of signatures alone is not enough in such a case, and may be even harmful." Scientists, Rehberg concluded, "overstep their competence" when they mix science and politics, even though they have "the right" to express their opinions.[58]

[57] Bridgman to LP, May 17, 1957, [1.92] LP Safe Contents, Drawer 2, Folder 2.001, LP Papers.
[58] Rehberg to LP, Oct. 12, 1957, [1.96] LP Safe Contents, Drawer 2, Folder 2.001, LP Papers.

Sam Goudsmit of Brookhaven National Laboratory told Pauling that the "scientific statements" were "the weakest portions" of the appeal. "Though I fully agree," Goudsmit wrote, "that atomic war and really all destructive conflicts should be abolished, I see no reason to base this aim on scientific statements which lack objectivity." Goudsmit questioned Pauling's analysis of how much Sr90 enters the bone, and accused Pauling of relying on "unproven assumptions" and "scare data." He predicted that an exaggeration of the dangers would cause the public to lose confidence in scientists in general. Goudsmit did admit that "the other side" also promoted data favorable to their political interpretation. Nevertheless, Goudsmit deemed Pauling's appeal "thoroughly unscientific," and called for "arguments based on moral, economic, and historical foundations."[59]

Another scathing response came from an unidentified Cornell scientist who opposed bomb tests "as a man, not as a scientist." This scientist criticized Pauling's appeal

> because it purports to be scientific advice, from scientists, about a scientific matter. The statement is anything but scientific because it is not quantitative. The assertions could equally apply, I suppose, to food preservatives, noxious gases from factories or refineries, even to toothpaste. . . . [S]cientists of all people have the duty to make precise statements, especially in a warning about such a grave matter.

The author argued that nuclear war was a far greater threat than tests, and thus "I fear that your appeal will waste a significant scientific impact on what is today a peripheral subject."[60] Similarly, Edward Purcell of Harvard told Pauling that he could not agree with the scientific aspects of the statement (i.e. that radiation from testing will be harmful to people) "unless I abandon quantitative judgment entirely, as your second paragraph very nearly does." Purcell

[59] Goudsmit to LP, June 18, 1957, [1.58] LP Safe Contents, Drawer 2, Folder 2.001, LP Papers.
[60] Unknown to LP, Dec. 2, 1957, [1.140] LP Safe Contents, Drawer 2, Folder 2.001, LP Papers.

wrote that Pauling's statement was not "measured" enough, and "As scientists we ought to be the last to be drawn by our zeal into that form of excess."[61]

Finally, Pauling received a negative response from James Franck at the University of Chicago. Franck, also known as Papa Franck for his mentoring of atomic activists such as Szilard and Eugene Rabinowitch during the Manhattan Project, wrote Pauling that "in spite of the fact that I agree entirely with you, I can not make up my mind to sign your appeal." Franck objected to appeals that, like Pauling's, "are written by scientists wearing the cloak of science. . . . Moreover, I believe that the task of science—to inform the public that danger is ahead—has been done. I doubt that we can help at this moment to see that the tests will be stopped internationally."[62] In December, Franck made up his mind not to sign because he feared that the petition might weaken the U.S. government in its negotiations with the Soviets.[63] The neutral stance taken by many scientists actually reinforced the notion that the state held authority over legitimate expertise. Just as the government insinuated that Pauling was disloyal to the United States, elite scientists told Pauling he was disloyal to objectivity.

Activist Disagreement

Some scientists who refused to sign voiced no objections to the scientific aspects of Pauling's appeal. Rather, they objected to the petition itself as a form of protest. Some of these activist scientists complained that fallout was not the best reason to oppose nuclear weapons tests. A few refused to sign because of their positions, even when these positions would seem to be in harmony with Pauling's appeal, such as when Paul Doty of the

[61] Purcell to LP, May 17, 1957, [1.84] LP Safe Contents, Drawer 2, Folder 2.001, LP Papers.
[62] Franck to LP, May 20, 1957, [1.108] LP Safe Contents, Drawer 2, Folder 2.001, LP Papers.
[63] Franck to LP, Dec. 12, 1957, [1.107] LP Safe Contents, Drawer 2, Folder 2.001, LP Papers.

Federation of American Scientists (FAS) declined to sign.[64] The British scientist P.M.S. Blackett, at one time the president of the British Association of Scientific Workers, explained that he would not sign in order to remain "uncommitted." Instead, Blackett told Pauling that he hoped to "bridge the gap" between military realists and activists like Pauling by learning to "talk the language of the strategico-military people."[65]

The esteemed Cornell physicist Hans Bethe, who would soon become an integral member of the U.S. team dispatched to Geneva to negotiate a nuclear test ban with the Soviet Union, turned down Pauling's petition in May 1957. Bethe told Pauling that he had listened to Pauling's nemesis Willard Libby of the AEC, who "convinced me that he and his collaborators have studied the problem of radioactivity very seriously, and that their conclusions are essentially sound." He told Pauling that it was "quite wrong to put further emphasis on the radioactivity argument," and suggested that the sections of the petition about the dangers of proliferation might hold more appeal.[66]

Leo Szilard, one of the more committed activists among scientists, refused to sign, he wrote, "for what seem to me to be good and valid reasons"—reasons which he failed to divulge.[67] At any rate, Szilard's refusal to sign petitions is well known, and was apparently not uncommon for esteemed scientists.[68] The future Nobel Prize winner George Wald, like Szilard a deeply committed activist, wrote Pauling that "after a number of unsatisfactory experiences, I decided once and for all to sign no more petitions, but to speak for myself

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[64] Doty to LP, May 24, 1957, LP Peace 5: Nuclear Bomb Test and Proliferation Petitions, 5.002: *An Appeal by Scientists to the Governments and People of the World*, 1957–58, 2.1: Correspondence, Notes re: circulation and signing of *An Appeal by Scientists to the Governments and People of the World*, 1957–59, LP Papers.
[65] Blackett to LP, Nov. 11, 1957, [1.124] LP Safe Contents, Drawer 2, Folder 2.001, LP Papers.
[66] Bethe to LP, May 17, 1957, [1.80] LP Safe Contents, Drawer 2, Folder 2.001, LP Papers.
[67] Szilard to LP, May 27, 1957, [1.50] LP Safe Contents, Drawer 2, Folder 2.001, LP Papers.
[68] Rotblat wrote that Szilard "often abstained from conference statements, although substantially helping in their preparation." *Scientists in the Quest for Peace*, 5.

whenever I felt impelled." As for the appeal itself, he wrote: "I am of course against war, atom bomb testing, and so on; but I think there are circumstances under which I would be willing to put up with war, with or without atom bombs. I have lived a whole life joining in rejections of this or that; it is the particular disease of our time. I am about ready to try to find a few affirmations."[69] Vannevar Bush, the influential government figure and prophet of Big Science, stated that "I do not join in petitions and the like. . . . I have not had the feeling that this was the very important way to get heard." Bush confessed that "I made a vigorous personal attempt way back in the early days when it would have made a real difference," but he collided "against a stonewall at the time."[70] Samuel K. Allison of the University of Chicago dismissed Pauling's appeal as "utopian," and feared it would "do more harm than good." Allison wrote, "Nobody pays much attention to round robin statements any more. On glancing at the (was it two thousand?) names on the appeal I recognized a mere handful as 'scientists (who) have knowledge of the dangers involved.' Enemies of the appeal can always, in such a mass of names, pluck out some dubious persons and claim that they are characteristic of the signers." Allison echoed Szilard, Wald, and Bush, when he declared a preference for "direct, personally worded, and individual statements."[71] Finally, Wolfgang Panofsky, a physicist at Stanford University and advocate of arms control, questioned the political impact of the statement. Although Panofsky admitted that "no serious disagreement exists" about the biological effects of fallout, he suspected that a test cessation "would certainly not decrease the mutual suspicion and would increase the common uncertainty."[72]

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[69] Wald to LP, Dec. 6, 1957, [1.74] LP Safe Contents, Drawer 2, Folder 2.001, LP Papers. Wald won the Nobel Prize in Physiology/ Medicine in 1967.
[70] Bush to LP, Nov. 21, 1957, [1.76] LP Safe Contents, Drawer 2, Folder 2.001, LP Papers.
[71] Allison to LP, Nov. 20, 1957, [1.110] LP Safe Contents, Drawer 2, Folder 2.001, LP Papers.
[72] Panofsky to LP, Dec. 3, 1957, [1.28] LP Safe Contents, Drawer 2, Folder 2.001, LP Papers.

Some refusals took issue with the specific focus of Pauling's petition. Harvard University's John T. Edsall admitted "complete agreement" with the statement's objectives "and nearly all of its wording." Nevertheless, he wrote Pauling, "I believe the major reason for trying to stop bomb tests now is to help check the atomic armament race, serving as a first step to real disarmament. This seems to me of overwhelming importance, while the biological effects of nuclear testing—although a very grave matter in principle—are quantitatively less important right now than medical x-rays and other problems." Instead Edsall worried about convincing people that ending testing would not harm U.S. national security.[73]

Norman Goldberg of the FAS also disagreed with the emphasis of the proposal. He worried that the wording "permits and perhaps encourages exaggerated and even incorrect interpretation of the danger." If the statement rested on the danger of fallout, Goldberg reasoned, some people might conclude that fallout is worth the health risk of defending national security.[74] Theodore Puck of the University of Colorado worried that Pauling's focus on fallout would "divert attention from the real issue of the necessity to give up warfare entirely."[75] George Beadle expressed his feelings in a public statement, saying he wanted to "abolish all unnecessary radiation," whether it came from weapons tests, nuclear power, or medical uses. He argued further that the arms race would increase chances of nuclear war. "In these respects I agree with the Pauling Appeal. I did not sign it because certain important issues raised in it go beyond science. In these I have no special knowledge

[73] Edsall to LP, May 20, 1957, [1.137] LP Safe Contents, Drawer 2, Folder 2.001, LP Papers.
[74] Goldberg to LP, May 29, 1957, [1.120] LP Safe Contents, Drawer 2, Folder 2.001, LP Papers.
[75] Puck to LP, Oct. 1, 1957, [1.12] LP Safe Contents, Drawer 2, Folder 2.001, LP Papers.

or competence."[76] Beadle thus declined the appeal, and in a personal letter warned Ava Helen Pauling about people such as Libby of the AEC and the pacifist Albert Schweitzer who used "arguments that sound scientific but are not good science." Beadle, a geneticist, privately admitted to Pauling in a handwritten letter that "I have special competence and responsibility" to express views on fallout. And, he added, "Fallout is bad—no doubt about it." Yet Beadle could not sign the appeal, he wrote, because he did not feel that nuclear fallout was the primary reason to stop tests, as Pauling's appeal indicated. Beadle assured Pauling that he was dedicated to stopping tests and could be more effective without signing.[77] Thus the appeal revealed to Pauling division even among scientists who agreed that science supported antinuclear policies.

Total Disagreement

Finally, a number of scientists simply disagreed fundamentally with Pauling's science, politics, and activism. James B. Conant, the legendary establishment figure and president of Harvard University, simply wrote that "I couldn't disagree with you more heartily. I, therefore, have high hopes that you will fail completely in your undertaking!!!"[78] Others sided more directly against Pauling and with nuclear deterrence. Frederick Seitz, the president of the National Academy of Sciences, told Pauling that "the dictatorships" were to blame for the arms race, and that "the evil represented by the combination of Soviet imperialism and

[76] Beadle "Statement on the Pauling Petition," LP Peace 5: Nuclear Bomb Test and Proliferation Petition, Section 5.001: *An Appeal by Scientists to the Governments and People of the World,* 1957–1958, 1.3: Typescript, Correspondence: "Statement on the Pauling Petition," by G. W. Beadle, 1957, LP Papers.
[77] Beadle to LP, May 20, 1957, [1.105] LP Safe Contents, Drawer 2, Folder 2.001, LP Papers.
[78] Conant to LP, Nov. 19, 1957, [1.3] LP Safe Contents, Drawer 2, Folder 2.001, LP Papers. Conant had earlier opposed the H-bomb "as strongly as anybody," quoted in James Hershberg, *James Conant: Harvard to Hiroshima and the Making of the Nuclear Age* (New York: Knopf, 1993), 679–82.

brutality far overwhelms, in my own mind, the evil associated with the tests."[79] John Wheeler at the Institute for Advanced Study at Princeton, after saying he had "no special competence," continued to argue that although he would prefer peace, he believed that nuclear weapons protected freedom that was threatened by the Soviets. David Griggs, a geophysics professor who worked with Edward Teller, opposed the unilateral nature of the appeal because he feared such a move would halt U.S. nuclear weapons development. Other nations would meanwhile continue to develop weapons, which "might well tempt a nation bent on dominating the world to subjugate us with their newer and better nuclear weapons." Griggs then assured Pauling that it was possible to conduct tests underground without any radiation escaping.[80]

One of the more virulent refusals came from Kenneth S. Pitzer of UC Berkeley's chemistry department. Calling Pauling's appeal "a most unfortunate action," Pitzer wrote that the risks of fallout were "very small," and that "any military defense involves some risks." Without tests, the quality of nuclear weapons would decline, threatening "human freedom in the world today." In closing, Pitzer tagged Pauling as an appeaser: "Just as the peace movements of the middle 1930's may have increased the probability of World War II, I am afraid that appeals such as yours may increase the risk of a future war."[81]

Pauling and Antinuclear Scientists in the United States and Britain

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[79] Seitz to LP, Nov. 21, 1957, [1.44] LP Safe Contents, Drawer 2, Folder 2.001, LP Papers.
[80] Griggs to LP, Nov. 25, 1957, [1.61] LP Safe Contents, Drawer 2, Folder 2.001, LP Papers. Griggs had been one of Oppenheimer's strongest accusers at the security clearance hearing and believed that he was a communist. Richard Polenberg, ed., *In the Matter of J. Robert Oppenheimer: The Security Clearance Hearing* (Ithaca: Cornell University Press, 2002), 271–74.
[81] Pitzer to LP, May 27, 1957, [1.32] LP Safe Contents, Drawer 2, Folder 2.001, LP Papers. Pitzer had testified against Oppenheimer in 1954, Polenberg, *The Security Clearance Hearing*, 249.

Undeterred by his conflicts with leading scientists, Pauling presented his petition to Dag Hammarskjöld of the UN on January 13, 1958. And Pauling presented a second petition—this one on nuclear proliferation and signed by a far less impressive 720 scientists—to the UN in February 1961.[82] After the United States and Soviet Union signed the Limited Nuclear Test Ban Treaty in 1963, the Nobel committee rewarded Pauling's years of activism with the Peace Prize, giving the embattled scientist a large amount of support, credibility, and publicity. Despite this recognition from the Nobel Prize committee, Pauling continued to drive a wedge between leading scientific arms control advocates, as his personality led to or exacerbated clashes with scientists affiliated with Pugwash and the *Bulletin of the Atomic Scientists*.

The biophysicist Eugene Rabinowitch had left his native Russia after the revolution of 1917, and arrived in the United States in 1938. Five years later he joined the Manhattan Project's Met Lab, where, working with Papa Franck, Rabinowitch did a great deal to draw scientists into the debate over nuclear weapons.[83] Just before the use of atomic weapons against Japan in August 1945, Rabinowitch had helped author the Franck Report, the Met Lab document that protested the use of the atomic bomb against Japan, arguing that doing so would make future international control of atomic weapons impossible in addition to provoking an atomic arms race.[84] After the war, Rabinowitch continued his activism by co-founding the *Bulletin of the Atomic Scientists* as a forum for informed, scientific debate over nuclear weapons and foreign policy.

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[82] LP to Dag Hammarskjöld, Feb. 16, 1961. LP Peace 5: Nuclear Bomb Test and Proliferation Petitions, 5.011: *An Appeal to Stop the Spread of Nuclear Weapons, 1961*, 11.10: Correspondence: Dag Hammarskjöld, LP Papers.
[83] Alice K. Smith, *A Peril and a Hope: The Scientists' Movement in America, 1945–47* (Chicago: University of Chicago Press, 1965), 22–23.
[84] Smith, *A Peril and a Hope*, 41–48.

Although the *Bulletin* included Pauling's name on its Board of Sponsors, Rabinowitch had refused to sign Pauling's petition in May 1957. Rabinowitch echoed other scientists when he told Pauling: "I do not feel that I can sign it as a scientist . . . . I am not convinced that a strong quantitative case for the cessation of testing can be made simply on the basis of the threat to health or genetic endowment of mankind. I have no reason to believe that Dr. Libby's figures are not approximately correct." Rabinowitch also feared that the appeal might accidentally convince Americans to willingly risk fallout in order to protect national security. Then Rabinowitch stated that "I am personally sceptical [sic] . . . that the cessation of tests is likely to become a first step towards controlled nuclear disarmament."[85]

As editor-in-chief, Rabinowitch ran the *Bulletin* as a forum where scientists' role in the nuclear age could be debated, arms control proposals examined, and science policy explored. In general, its contributors showed a preference for technical debate rather than impassioned rhetoric, and endorsed the quiet and indirect influencing of policymakers' views instead of mobilization of the masses. Although the *Bulletin* had published a favorable account of Pauling's appearance before the Dodd committee in 1960, by 1962 Pauling had begun to take issue with the *Bulletin*'s treatment of him and his peace campaign. In addition to the Allison article mentioned above, a 1962 Bentley Glass article titled "Scientists in Politics" criticized Pauling. Tracing the history of scientists' interaction with politics, Glass focused mainly on the fallout and nuclear testing debates between Pauling and Edward Teller. Glass wrote that both men "are carried away by the conviction of the rightness of

[85] Rabinowitch to LP, May 18, 1957, LP Peace 5: Nuclear Bomb Test and Proliferation Petitions, 5.002: *An Appeal by Scientists to the Governments and People of the World*, 1957–58, 2.1: Correspondence, Notes re: circulation and signing of *An Appeal by Scientists to the Governments and People of the World*, 1957–59, LP Papers. Rabinowitch's comment seems odd given that in 1963, he hailed the LTBT as a "first step" toward disarmament. Rabinowitch, "Editorial: First Step—To Where?" *Bulletin of the Atomic Scientists*, October 1963, Vol. XIX, No. 8, 2–3.

their opinions, and make extrapolations from the existing facts that are scarcely trustworthy, or are even lured into flat misstatements." He added that neither of the two had any particular scientific expertise on the matter of fallout, though most of Glass's article was devoted to refuting at length Teller's pro-nuclear arguments. Glass concluded by stating that "often scientists engaged in such [political] efforts have forsaken the scientific approach to the problem at issue." Instead, Glass suggested, the Pugwash conferences could serve as a "more hopeful" attempt at international cooperation, one imbued with "scientific integrity."[86] Highly offended, Pauling wrote to the *Bulletin*, which published his response in the December 1962 issue wherein he decried the "untrue and damaging statements" in the Glass article.[87]

Various demands and explanations went back and forth between June and September 1963, as the pages of the *Bulletin* filled with Rabinowitch's response, a comment from Glass, and then another response from Pauling. Such activity was not abnormal for Pauling, who was defiant about bad publicity and often filed successful libel suits. Indeed, Pauling bragged as much to Rabinowitch at one point, perhaps making a veiled threat.[88] During the melodrama Pauling wrote to Hans Bethe, the chairman of the *Bulletin's* Board of Sponsors, to announce his resignation from the Board. Pauling cited Rabinowitch's "half-hearted apology" and "damage done to me" during the Glass controversy as his reasons for leaving, concluding that "I am not willing to continue to sponsor Eugene Rabinowitch as Editor of the Bulletin."[89] Pauling had essentially been ousted—or rather ousted himself—

[86] Glass, "Scientists in Politics, *Bulletin of the Atomic Scientists*, May 1962, Vol. XVIII, No. 5, 2–7.
[87] Pauling, "Scientists in Politics," *Bulletin of the Atomic Scientists*, December 1962, Vol. XVIII, No. 10, 32.
[88] See, for example, LP to Harry Kalven Jr., Aug. 19, 1962, Folder 5.43: Correspondence related to Linus Pauling, Box 5, Hans Bethe Papers, Cornell University.
[89] LP to Bethe, Sept. 26, 1963, [2.4] LP Safe Contents, Drawer 2, Folder 2.002, LP Papers.

from the *Bulletin*, further divorcing him from the scientific constituency who would seem to be his natural allies in the pursuit of disarmament. In 1966 Pauling wrote to Bethe again, asking if he could "withdraw my resignation," feeling that "it would be proper for me to be on the Board of Sponsors."[90]

Pauling's break with the *Bulletin* probably evolved out of a clash with the U.S. branch of the Pugwash organization, where Glass and Rabinowitch also ran the show. The Pugwash conferences began in 1957 as informal meetings between scientists from both sides of the Iron Curtain; in this unofficial atmosphere, scientists could exchange information that would encourage disarmament. Pauling had willingly participated in the second Pugwash conference in 1958 and continued to take heat for it from Senator Dodd years later. Nevertheless, as with the *Bulletin*, he found himself out of place as an iconoclast addicted to making public statements among reserved insider types fond of thinking of themselves as quietly—almost secretly—influencing geopolitics. On May 10, 1962, Pauling wrote to Joseph Rotblat, the head of British Pugwash, turning down an invitation to a Pugwash conference. Sensing a conspiracy to exclude him, Pauling wrote Rotblat: "I judge that I am not going to be invited to the more important conferences, and I feel that for this reason, too, there is no need for me to attend the less important ones."[91] That same day, Pauling wrote to Bertrand Russell, the British philosopher who had initiated the Pugwash movement with his 1955 manifesto calling for scientists to work for disarmament which had been endorsed by a dying Albert Einstein.[92] Pauling told Russell that he would not participate in Pugwash as long as

[90] LP to Bethe, May 18, 1966, Folder: Bulletin of Atomic Scientists Board of Sponsors Correspondence, Box 443: Topic Files, ET papers.
[91] LP to Rotblat, May 10, 1962, LP Peace 1: Pugwash Conferences, 1.005: Materials re: Tenth through Thirteenth Pugwash Conferences; Pugwash Symposia; International Student Pugwash, 5.1 (tenth conference), LP Papers.
[92] See Rotblat, *Scientists in the Quest for Peace*, 2, 137–40.

the U.S. representatives included Rabinowitch and Glass, who he described as "uncooperative." Furthermore, Pauling believed that "the calibre of American scientists . . . participating in the Conferences should be raised." Finally, Pauling complained about Glass's "serious and grossly damaging attack upon me" that had appeared in the *Bulletin*.[93] Pauling probably found a sympathetic ear in Russell, who was already drifting from Pugwash toward more grassroots efforts.[94]

The passage of time did little to heal Pauling's wounded ego. In 1967 he wrote to the editor of *Ramparts Magazine*, alleging that "the C.I.A. or the State Department might have infiltrated the Pugwash movement." In 1959, one year after his first visit to a Pugwash conference, Pauling claimed to have noticed a "change in policy," as U.S. participants "turned out to be less well-known scientists connected in some way with the government, especially with the military." Also of great significance to Pauling was the damning evidence that "I have not been allowed to attend most of the Pugwash Conferences since 1959." Finally, Pauling argued that the U.S. Pugwash group's ability to raise $100,000 in 1961 "with a major contribution made by the Ford Foundation" was evidence of secret CIA influence over the organization.[95] In fact, such suspicions were not entirely baseless, as the CIA did direct the Ford Foundation's support of the Congress of Cultural Freedom.[96] But it is unknown if Pugwash's Ford money was similarly tainted.

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[93] LP to Russell, May 10, 1962, LP Peace 1: Pugwash Conferences, 1.005: Materials re: Tenth through Thirteenth Pugwash Conferences; Pugwash Symposia; International Student Pugwash, 5.1 (tenth conference), LP Papers.
[94] Russell to LP, June 30, 1962, [2.28] LP Safe Contents, Drawer 2, Folder 2.002, LP Papers.
[95] LP to Robert Scheer, Feb. 23, 1967, LP Peace 1: Pugwash Conferences, 1.001: Materials re: First through Eighth Pugwash Conferences, 1.1 (first conference), LP Papers.
[96] John Krige, *American Hegemony and the Postwar Reconstruction of Science in Europe* (Cambridge: MIT Press, 2006), Chapter Six, 155–189, discusses collaboration between the Ford Foundation and the CIA, especially regarding the Congress of Cultural Freedom. Oppenheimer joined the Board of Directors of the CCF in the 1960s in order to reestablish his anticommunist credentials and had no problem with the agency's influence over the foundation. See Thorpe, *Oppenheimer*, 247, 276, 279–80.

Pauling also broke with SANE, the mainstream antinuclear organization that had helped carry the torch of disarmament during the 1950s. After SANE conducted a purge of its left-leaning members following the Dodd hearings in May 1960 (the same hearings at which Pauling testified about his petition), Pauling made his disgust official. When SANE promised Dodd that it would question any of its left-leaning members and drop them from the organization if they gave "unsatisfactory" answers, Pauling told SANE's leader Norman Cousins that he objected to the organization's "recourse to McCarthyism." Pauling also refused an invitation to join the board of SANE and prophesied that "SANE will be destroyed," an essentially accurate prediction.[97] Pauling's break with SANE also had a personal aspect, as Pauling made clear in November 1960. Writing to SANE in order to distance himself from the organization, Pauling emphasized that he had "never been a member" of SANE, and was dropping his connection to the organization because they refused to support him during the Dodd hearings.[98]

In contrast to his uneasy relationships with some U.S. scientists and activists, the fact that some 9,000 scientists overseas signed his petition, as compared to 2,000 in the United States, suggests that scientific opposition to Pauling's efforts might have been at least partly regional in nature.[99] While British scientists participating in the antinuclear movement played a conventional role by providing it with essentially technical ammunition, advocacy of a

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[97] LP to Norman Cousins, Aug. 11, 1960, LP Peace 4: Peace Groups, 4.003: SANE, 1958–1966, 1982, 3.9: National Chapter, Correspondence and Miscellaneous, 1960, LP Papers; LP to Clarence Pickett, May 29, 1961, LP Peace 4: Peace Groups, 4.003: SANE, 1958–1966, 1982, 3.10: National Chapter: Correspondence, 1961–1964, LP Papers. On SANE, see Milton Katz, *Ban the Bomb: A History of SANE, the Committee for a Sane Nuclear Policy, 1957–85* (New York: Greenwood Press, 1986); and Wittner, *Resisting the Bomb*, 56–60, 247–63, 453–57.

[98] LP to Nolan Kerschner, Nov. 8, 1960, LP Peace 4: Peace Groups, 4.003: SANE, 1958–1966, 1982, 3.3: Correspondence: SANE, Local Chapters, 1960, LP Papers.

[99] Krige notes in *Hegemony* that European (particularly French) scientists were not nearly as suspicious of scientists linked to communism, as the political philosophy had a genuine tradition on the continent, 117. Not that Pauling had any ties to communism. The Soviet Union in fact denounced his scientific work as bourgeois and non-Socialist, though the Soviet government did approve of his antinuclear activism. Pauling, *Scientist and Peacemaker*, 52.

policy position was not as controversial as it was in the United States. Yet, the scientific aspect of the movement did not command as much attention as Pauling did in the United States.

The Campaign for Nuclear Disarmament (CND), Britain's largest antinuclear group, established a Scientists' Group in the early 1960s that informed and participated in CND activities. These efforts included a letter urging the U.S. government to abandon plans for a high altitude thermonuclear test. "The test has been heralded as a great scientific experiment," they wrote,

> and vulgarised as a "rainbow" shot with dramatic atmospheric effects. In fact, it is a military rather than a scientific experiment, and its execution violates a resolution of the International Astronomical Union. The possible effects of the experiment on the earth's environment are not well understood; qualified scientists in this and other countries have not even had adequate opportunity to try to predict them. In these circumstances, we think it regrettable that the experiment has been publicised as a harmless firework display. Its consequences could be serious.[100]

Less dramatic efforts included a CND scientists' bulletin that reached a circulation of roughly 1,500, while a "Handbook on Nuclear War" covered "Effects of Blast on People" and "Protection from Fire."[101] Most often the Scientists' Group provided information to other activists, such as when Bertrand Russell's direct action group Committee of 100 wrote to the group "enquiring whether Plutonium was being used at Capenhurst." The CND scientists confirmed that "fissile material was being dealt with there." The group also received an "enquiry from the Bradford Group about radioactivity in water as their local authority was reluctant to release the figures, and they feared this was due to an increase in

[100] Mass letter, Peggy Duff, June 7, 1962, 2–3, 1/62: Scientist Group, 1962, Section 1: CND Central Constitution, Minutes of Committees, Subject Files, 1958–1971, Campaign for Nuclear Disarmament Papers, London School of Economics and Political Science (hereafter CND Papers).
[101] "Minutes of the A.G.M. of the Scientist's Group of C.N.D.," May 28, 1962, 4–6, 1/62: Scientist Group, 1962, Section 1: CND Central Constitution, Minutes of Committees, Subject Files, 1958–1971, CND Papers.

the level." In response, a Birmingham scientist "agreed to inform them that the levels were not dangerous and to urge that pressure be applied for the figures to be publicised." Other activists asked for a study on the potential effects of nuclear war on Britain, while "A letter from the East Bay Women for Peace asked for figures for radioactive contamination of food."[102] The scientists also served to inform the public about radiation threats, including Sr90 contamination of milk and on the rate of leukemia relative to the amount of nuclear fallout.[103] In these efforts, the Scientists' Group was roughly equivalent to the St. Louis Committee on Nuclear Information (CNI), which, according to sociologist Kelly Moore, promoted itself as politically neutral by simply providing objective technical information to the public.[104]

But opinions on policy flowed naturally from the Scientists' Group's efforts, similar to Pauling but unlike many of his U.S. counterparts, including CNI. One scientist from Edinburgh, according to the minutes of a 1962 meeting, "proposed that the Group organise a petition to Parliament about the dangers of nuclear tests to be signed by biologists. . . . It was agreed that it should be limited to biologists as it would deal with the biological uncertainty of fallout effects which were being soft-pedaled at the moment."[105] The scientists' group also plotted to have particular questions brought up in Parliament. In late

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[102] "Minutes of a Committee Meeting of the Scientist's Group following the A.G.M. on Monday, May 28, 1962," 1/62: Scientist Group, 1962, Section 1: CND Central Constitution, Minutes of Committees, Subject Files, 1958–1971, CND Papers.

[103] "C.N.D. Scientists' Group," minutes, Jan. 2, 1964, 10, 1/62: Scientist Group, 1962, Section 1: CND Central Constitution, Minutes of Committees, Subject Files, 1958–1971, CND Papers; Jill Harrington to Peggy Duff, Jan. 11, 1960; Peggy Duff to Jill Harrington, Jan. 15, 1960; Peggy Duff to Anna Steele, CND, Jan. 27, 1960; Anna Steele, London CND, to Peggy Duff, Feb. 2, 1960; Peggy Duff to Anna Steele, Feb. 12, 1960; 5/ 11: Policy (2); Structure, Strategy and Relations with other organisations: Correspondence and circulars, CND policy, resignation of Bertrand Russell from CND, Section 5: London Region Minutes, Correspondence, CND Papers.

[104] As Moore points out, neutrality was a politically expedient image for the group, not a fundamental philosophy. *Disrupting Science*, 96–129.

[105] "Minutes of the A.G.M. of the Scientist's Group of C.N.D.," May 28, 1962, 4–6, 1/62: Scientist Group, 1962, Section 1: CND Central Constitution, Minutes of Committees, Subject Files, 1958–1971, CND Papers.

1962 a "Dr. Berenbaum" planned to "frame a question . . . to draw attention to the absurdity of a dispersal plan to move people from cities to areas near bases," while "Dr. Pirani agreed to suggest a question about planes carrying nuclear heads over Britain" as well as "a question about the frequency of nuclear accidents."[106]

A petition to Prime Minister Harold MacMillan against nuclear testing provoked little outrage among British scientists, though it mingled scientific expertise with policy pronouncements. In Pauling-esque tones, it began: "As professional biologists, we are gravely concerned about the harmful biological consequences of testing nuclear weapons." It expressed concern that

> we know that even a small increase in the level of radiation will have harmful consequences. The burden of suffering and death which must inevitably result will chiefly affect future generations, and its extent will not be known for many years; but these facts cannot absolve us from moral responsibility.
>
> Further, we feel that continued nuclear tests increase the danger of war by poisoning the political atmosphere, as well as by conditions people to regard nuclear war as a feasibly possibility rather than as a social and biological disaster of unprecedented and awesome magnitude.
>
> We therefore urge you to use all your influence to stop further testing of nuclear weapons by any nation, and to withold [sic] British facilities and co-operation form any future tests of this nature.[107]

As the CND scientists' campaign shows, Pauling was not alone in believing that contesting the nuclear arms race was a scientist's duty. But Pauling's use of science as a method of activism had not guaranteed the allegiance of all scientists, particularly in the United States. Nor did being a scientist create enough solidarity to allow other activist scientists to join

[106] "Minutes of a meeting of the Scientists Group of CND," Dec. 14, 1962, 9, 1/62: Scientist Group, 1962, Section 1: CND Central Constitution, Minutes of Committees, Subject Files, 1958–1971, CND Papers.
[107] "To the Rt. Hon. Harold Macmillan, M.P., F.R.S.," 12, 1/62: Scientist Group, 1962, Section 1: CND Central Constitution, Minutes of Committees, Subject Files, 1958–1971, CND Papers.

Pauling without question. In the United States after the Oppenheimer hearing, there was indeed a split between scientists. But too much emphasis on the divisions between an "Oppenheimer camp" and a "Teller faction" can distract historians from noticing other influential splits. Antinuclear scientists had fierce divisions and found plenty to disagree upon. In addition, the U.S. government had done a great deal to challenge Pauling and harm his public image.

Still, the split over the Oppenheimer hearing and the role of Edward Teller remained important. Pauling was not the only scientist on a crusade; Teller successfully led his own campaign in favor of nuclear weapons throughout the entire Cold War. Pauling and Teller were both iconoclastic, polarizing figures who frequently clashed during the 1950s and 1960s. An extended look at Teller further demonstrates how the U.S. government enrolled scientists against antinuclear scientists, and that the use of science in political movements resulted in the division, not the triumph, of science.

Chapter Three: Edward Teller's Flexible Response

The Case for Doomsday

The escalation of the arms race initiated by the successful development of the hydrogen bomb in the 1950s saw a quick increase in the number of thermonuclear oracles heralding the new world order. This group included physicist Edward Teller as well as the eccentric thinker Herman Kahn. One of the earliest nuclear strategists at RAND, Kahn made a name for himself by pursuing the unthinkable logic of the arms race. "Kahn's most notorious idea," according to his biographer Sharon Ghamari-Tabrizi, was the purposefully absurd Doomsday Machine that he described in his most famous tome, *On Thermonuclear War*. As the ultimate deterrent, the machine would, upon sensing a nuclear attack, detonate enough fissile material to destroy human life on the planet. The Doomsday Machine was merely a thought experiment of Kahn's with which he could critique the policy of massive retaliation, since destroying all of humanity would presumably be judged unacceptable by the citizens and leaders of any nation. But since nations depended on nuclear deterrents only somewhat less lethal than the Doomsday Machine, Kahn asked, how many people *was* it acceptable to kill?[1]

In May 1960, Linus Pauling sat in attendance at the Bridge Laboratory on the Caltech campus as Kahn attempted to answer this question. As Kahn gave estimates ranging from 20 to 160 million, Pauling scribbled his criticisms in a notebook. Some were petty: Pauling disapproved that Kahn "Mispronounces words . . . crises for crisis." At other times, Pauling bristled with moral indignation. After Kahn told the audience that theorists avoid

[1] Herman Kahn, *On Thermonuclear War* (Princeton: Princeton University Press, 1961, third edition), 145-49; Sharon Ghamari-Tabrizi, *The Worlds of Herman Kahn: The Intuitive Science of Thermonuclear War* (Cambridge: Harvard University Press, 2005), 41, 212.

the study of nuclear war because "people don't like to face it," Pauling noted "But I say there is even less study of peace—less than of war—why?" Naturally some of Pauling's criticism was scientific, while some was simply accurate: "He's a good . . . comedian," Pauling noted.[2] When Kahn attempted to take the fear out of nuclear war by asking, "will [the] survivors envy the dead?" and responding "No," Pauling fumed.[3] He summarized his feelings about Kahn when, after receiving a copy of *On Thermonuclear War* from Princeton University press, he replied, "I have been interested to read a good bit of it, and I have referred to it a number of times. Each time I have felt like throwing it out of the window."[4]

After the success of *On Thermonuclear War,* Kahn left RAND in 1961 to form his own think tank, the Hudson Institute.[5] Ensconced at the Hudson Institute, Kahn sought out other oracles of nuclear war, including, naturally, Edward Teller. In August 1964, Kahn apparently remained enamored with the idea of the Doomsday Machine because he and D.G. Brennan (also of the Hudson Institute) wrote to Teller and asked for his thoughts on the machine. After consideration, Teller wrote to Brennan and Kahn to tell them that Doomsday Machines suffered from, in rare understatement, "impracticalities." Kahn and Brennan rushed to assure Teller that the machines were, in fact, practical. Brennan wrote Teller that "rough calculations" suggested that "perhaps a million tons of U238" would be enough to create a Doomsday Machine. "Obviously," Brennan continued, "the political decision to undertake an effort of that magnitude is not likely to be forthcoming, but the

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[2] See Ghamari-Tabrizi, *The Worlds of Herman Kahn,* 43–45, 69–70, 80–83, 236–80. Ghamari-Tabrizi places great emphasis on Kahn's comedic style.
[3] Handwritten notes, LP Peace 6: Other Peace Activism, 6.011: Materials re: Herman Kahn, 1960–1961, 11.2: Notebook kept by Pauling on a speech by Herman Kahn, May 27, 1960, Ava Helen and Linus Pauling Papers, Oregon State University (hereafter LP Papers).
[4] LP to Herbert Bailey Jr., Feb. 24, 1961, LP Peace 6: Other Peace Activism, 6.011: Materials re: Herman Kahn, 1960–1961, 11.4: Correspondence, Notes re: *On Thermonuclear War,* by Herman Kahn, 1961, LP Papers.
[5] Ghamari-Tabrizi, *The Worlds of Herman Kahn*, 36–37.

point is that a society capable of undertaking the moon program could undertake a Doomsday Machine if it felt strongly about it."[6] Still uncharacteristically playing the skeptic, Teller responded to Brennan:

> I am quite willing to accept that enough material could be produced for a Doomsday Machine. But do you really believe that this effort would be kept secret for the requisite length of time? I readily agree that the Russians could keep such a thing secret from us. But how many people in Russia or China or anywhere else could be persuaded to work hard and consistently on a project with a clear-cut aim that includes inescapable self-destruction? I would like to plea for some realism in evaluating the dangers with which we are faced.[7]

Kahn and Brennan would be forgiven for expressing surprise at Teller's "plea" for "realism." After all, Teller was no stranger to outlandish ideas. Along with praising the mutative effects of nuclear radiation in the 1950s, Teller had also started a nuclear excavation program and showed a flair for the fanciful by brandishing a business card that read, "If your mountain is not in the right place, drop us a card."[8]

Just a few months before the Teller-Kahn-Brennan correspondence, Stanley Kubrick's film *Dr. Strangelove* premiered; Kubrick used the Doomsday Machine as the comedic denouement in his satirical spoof of the arms race. The title character is a composite of Kahn, Teller, Wernher von Braun, and Henry Kissinger: at times the doctor's dialogue contains verbatim snippets of *On Thermonuclear War*, Strangelove is an ex-Nazi (like von Braun), and he exhibits a passion for nuclear weapons (like Teller). Although Ghamari-Tabrizi has provided a thoughtful and nuanced portrait of Kahn, the treatment of Teller in

[6] D.G. Brennan to ET, Aug. 14, 1964, Folder: Hudson Institute, Box 443: Topic File, Edward Teller Papers, Hoover Institution (hereafter ET Papers), emphasis in original.
[7] ET to D.G. Brennan, Aug. 25, 1964, Folder: Hudson Institute, Box 443: Topic File, ET Papers.
[8] Quoted in Peter Goodchild, *Edward Teller: The Real Dr. Strangelove* (Cambridge: Harvard University Press, 2004), 287.

the academic literature does not stray very much from a simplistic path. Peter Goodchild, for example, titled his 2004 biography *Edward Teller: The Real Dr. Strangelove.*[9]

Portraying Teller as Dr. Strangelove is not by itself a mere intellectual shortcut. At times, Teller does seem to be the caricature portrayed by Kubrick. But the Doomsday exchange between Teller, Kahn, and Brennan reveals much about Teller. Yes, he was a Strangelovian scientist who preached the gospel of the arms race, often in nefarious and misleading terms. But Teller was also rational and smart, and knew how to pitch an idea. He knew how to get a viewpoint across in politics, and could make exaggerated claims sound reasonable to certain audiences. This ability allowed him to cast doubt on the scientific claims of others. Teller may have been a suitable subject for satire, but in fact he had great influence over U.S. nuclear policy. Thus his claims—no matter how extreme—carried more weight with influential people because Teller expertly framed them. He shrewdly chose his audience, and provided a modicum of logic, consistency, and scientific reasoning that fell somewhere within the bounds of contemporary political thought. Kahn may have been outrageous, but Teller was outrageous and influential.

As an established theoretical physicist at UC Berkeley, the driving force behind the Livermore Nuclear Weapons Lab during the 1950s and 1960s, advisor to all sorts of government offices (including the AEC, PSAC, the armed forces, and the occasional congressman), and the public face of the H-bomb, Teller challenged Pauling's antinuclear crusade.[10] While Pauling attempted to rescue science from militarization, Teller spent his

[9] In *The Worlds of Herman Kahn,* Ghamari-Tabrizi titles one of her chapters on Kahn, "The Real Dr. Strangelove." In *The Wizards of Armageddon*, Fred Kaplan titles his chapter on Kahn, "Dr. Strangelove." Fred Kaplan, *The Wizards of Armageddon* (New York: Simon and Schuster, 1983), 220.

[10] Herbert York states that Teller acted as Livermore's de facto director even when not officially in that position: "Edward Teller's main base of operations has remained at the Lawrence Livermore Laboratory ever since its founding in 1952. He served briefly as the laboratory director (1958–1960), but his considerable influence over the laboratory's life and scientific

time working to link science to U.S. national security and anticommunism, in the process growing increasingly closer to resurgent conservative politicians. From this position, Teller played a unique and essential role in the state's containment of antinuclear scientists. To borrow a term from the Kennedy administration's nuclear strategy, Teller offered a flexible response to antinuclear challengers. When the occasion called for it, Teller could be subtle; when polemics were in order, Teller provided them. When scientific evidence appeared to challenge the nuclear deterrent, Teller refuted it in dry, technical terms. When scientific experts offered strategic reasons for arms control, Teller marginalized them as uninformed. While antinuclear scientists deemed the arms race a path to oblivion, Teller offered new ideas and innovations in nuclear weaponry that would lead to clear victory in the arms race and the Cold War. Teller thus actively helped the U.S. government confront the scientific challenge to the arms race.

As the previous chapter focused on Pauling's campaign to mobilize science for peace, this chapter focuses on Teller's campaign to mobilize science on behalf of the national security state. It not only provides a look at the counterpoint to Pauling's movement, but also addresses a gap in the current historiography. Recent scholarship on scientists and U.S. nuclear policy has emphasized those scientists who opposed or at least questioned the development of nuclear weapons during the Cold War, in contrast to earlier works which portrayed a Strangelovian elite.[11] In addition, the recent outpouring of books about J. Robert Oppenheimer has shifted the focus away from conservatives like Teller.[12] As

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programs has always derived more from his scientific charisma than from his formal positions." *The Advisors: Oppenheimer, Teller, and the Superbomb* (Stanford: Stanford University Press, 1989), 146.

[11] See for example, Kaplan, *Wizards of Armageddon.*

[12] William J. Broad, *Teller's War: The Top-Secret Story behind the Star Wars Deception* (New York: Simon and Schuster, 1992); Goodchild, *The Real Dr. Strangelove*; and Teller's own *Memoirs: A Twentieth-Century Journey in Science and Politics* (Cambridge: Perseus Publishing, 2001), are the only recent works on Teller. Gregg Herken's, *Brotherhood of the Bomb: The Tangled Lives and*

a consequence, most of these works conclude that scientists essentially lost their influence over nuclear policy after the Oppenheimer hearing. In fact, scientists who hoped to achieve arms control and disarmament did see their influence diminish by the mid 1950s, and this was in large part because of Teller's efforts. But somehow in these works, Teller does not "count" as a scientist because he was for—rather than against—nuclear weapons. In fact, Teller gained a great deal of influence over U.S. nuclear weapons policy, and his career challenges the notion that scientists had no role in nuclear policy after the Oppenheimer hearing.

After a brief biographical sketch exploring the origins of his anticommunism and his vision of the scientist's role in society (which were central to his pronuclear sentiments), this chapter explores the methods that allowed Teller to influence U.S. nuclear weapons policy. First, Teller acted as a divisive figure, working hard to discredit his opponents through relatively sophisticated scientific arguments as well as what can only be termed a name-calling campaign. Second, Teller (like Pauling) expressed an interpretation of nuclear weapons grounded in scientific arguments. But because he favored nuclear weapons development these arguments were the direct opposite of Pauling's, designed to counter the chemist's peace campaign as well as other scientists' arguments for arms control and disarmament. Finally, Teller relentlessly sought, acquired, and used political influence to shape the scientific community's relationship with the national security state, particularly through awards, appointments, lobbying, and personal correspondence.

An Anticommunist Scientist

Loyalties of Robert Oppenheimer, Ernest Lawrence, and Edward Teller (New York: Henry Holt, 2002), is a triple biography of Teller, Oppenheimer, and E.O. Lawrence.

Like many of the Manhattan Project's most famous scientists, Edward Teller grew up in Europe, was educated in Germany, and eventually relocated to the United States. Teller was born in 1908 and raised a Jew in Hungary, where the maelstrom of World War I exposed him to several torrents of political upheaval. In his memoirs, Teller recounted the fear unleashed when communists under Béla Kun took over a defeated Hungary after the war. Kun's brief reign, Teller indicated in his memoirs, left him with such emotional scars that a profound fear of communists remained with him for the rest of his life. The communist takeover started at home:

> The communists overturned every aspect of society and the economy. My father could no longer practice law. In fact we became social outcasts. A lawyer was clearly a capitalist; and, unlike a doctor, who provided a service, a lawyer was a thoroughly worthless person in a "good" society. Two soldiers moved into our "extra space," the rooms that had been my father's office in our home.

Outside the home, Teller found no relief:

> Of this time, I remember more clearly the multitude of posters that appeared in the streets and subways. On one of them, a stern man with his arm extended and his fingertip as large as if it were half an inch from my nose, said: "You, hiding in the shadows, spreading horror stories, you counterrevolutionary, TREMBLE." The finger seemed to follow me wherever I went.

When the communist regime collapsed and the Jewish Kun was deposed, a wave of anti-Semitism accompanied the next regime's ascent to power. "During my first eleven years," Teller summarized in his memoirs, "I had known war, patriotism, communism, revolution, anti-Semitism, fascism, and peace."[13]

Despite all the turmoil, Teller did well in school. In 1926 he left Hungary in order to study chemistry and mathematics at Karlsruhe Technical Institute in Germany. In April 1928

[13] Teller, *Memoirs*, 13–14, 16.

he enrolled at the University of Munich to study physics under Arnold Sommerfeld (who was also Pauling's mentor), though Teller and the elder physicist never got along. He eventually finished his studies in Leipzig with Werner Heisenberg, where he relished living at the heart of the quantum mechanics revolution. In contrast to the joys of studying physics, the rise of Nazism made evident to Teller the need to leave Germany. Thus Teller spent the 1933–34 academic year in Copenhagen, the home of Niels Bohr and "the first assembly point of the Diaspora of the German physicists," in Teller's words.[14] Teller and his wife Mici then ended up in London in September 1934; one year later they arrived in Washington.

Just a few years after his arrival, Teller found himself working with his fellow Hungarian refugee physicists Leo Szilard and Eugene Wigner, who had decided to petition the U.S. government to begin development of an atomic bomb. Teller's efforts were at first fairly mundane. In August 1939 Szilard had composed a letter to Franklin Roosevelt alerting the president to the possibilities of atomic weapons; Albert Einstein (who had fled Germany to Princeton) had promised to sign his name to the letter. But because Szilard did not know how to drive, it fell to Teller to chauffer the spoiled Szilard to Einstein's summer home.[15]

As the Manhattan Project came to life, Teller managed to play a more significant role. In the summer of 1942 J. Robert Oppenheimer brought Teller and many of the nation's other elite physicists to Berkeley to probe the possibilities of an atomic bomb. At this meeting, Teller first became captivated by Oppenheimer; he also conceived of using a fission explosion as a trigger for an even larger explosion. When the military decided to establish a lab for assembling the atomic bombs at Los Alamos, New Mexico, Teller began to recruit other scientists to work there. Teller was himself one of the earliest arrivals at the lab in

[14] Teller *Memoirs*, 95.
[15] Teller *Memoirs*, 145–46.

March 1943. Despite this early enthusiasm for the bomb, historians agree that Teller did not have a particularly active role at Los Alamos. Nearly every account shows that Teller spent most of his time theorizing about a thermonuclear weapon (known then as the Super and later the Hydrogen bomb) that would dwarf the power of the A-bomb.[16] Oppenheimer's refusal to give priority to the Super, combined with his selection of Hans Bethe over Teller as head of the lab's theoretical division, have caused historians to conclude that a future showdown between Teller and Oppenheimer was all but inevitable. Teller himself lends credibility to this characterization. "When he [Oppenheimer] told Bethe and me that he had named Hans to head the [theoretical] division," Teller remembered, "I was a little hurt. I had worked on the atom bomb project longer than Bethe. I had worked hard and fairly effectively on recruiting, and on helping Oppie organize the lab during the first chaotic weeks."[17]

But an obsession with the Teller-Oppenheimer rivalry obscures even bigger conflicts. Teller's memoirs make it clear that he had a dispute with Bethe equal to or greater than his conflict with Oppenheimer. "Although I appreciated and enjoyed the differences between Hans and me," Teller wrote, "sometimes I suspected that Hans thought I spent too much time in impractical pursuits, a failing that I could correct if someone disciplined me. I was not happy about having him as my boss." Thus Teller had already started to distance himself from the men who would become the U.S. scientific elite after the war. In his memoirs, Teller described a specific incident where Bethe had asked him to work on some implosion equations:

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[16] These accounts include Richard Rhodes, *The Making of the Atomic Bomb* (New York: Simon and Schuster, 1986), 545–46, as well as Kai Bird and Martin Sherwin, *American Prometheus: The Triumph and Tragedy of J. Robert Oppenheimer* (New York: Knopf, 2005), 282–83, Goodchild, *Edward Teller*, 79–91, and Herken, *Brotherhood of the Bomb*, 85–86.
[17] Teller, *Memoirs*, 177.

> The task Bethe was discussing seemed far too difficult. Not only were other people more capable than I of providing such work, but I also suspected that a job that formidable might not be completed in time to have any influence on a bomb that could be used during this war. Although I began explaining all those reasons to Bethe, he was convinced that I needed to tackle the job; I was just as convinced that if I did, I would make no contribution to the war effort. . . . Although I hoped that he would come to understand my position, he never did, and the incident marked the beginning of the end of our friendship.[18]

Contrary to his comments, however, Teller had little interest in playing a role in the war effort. He eventually worked out an arrangement with Oppenheimer that allowed him to pursue theoretical work on the Super, which stood no chance of being a factor in the war.

When the war ended, Teller anticipated that Los Alamos would begin concerted effort on the Super, though most scientists simply left to resume their prewar lives. Without a core of quality scientists around him, Teller reluctantly left Los Alamos (though he remained a consultant to the lab). In his memoirs, Teller blamed Oppenheimer for scuttling work on the Super. Following the use of the bombs, Teller claimed, "Oppenheimer had seemed to lose his sense of balance, his perspective. After seeing the pictures from Hiroshima, he appeared determined that Los Alamos, the unique and outstanding laboratory he had created, should vanish. When asked about its future, he responded, 'Give it back to the Indians.'"[19]

Several of Teller's peers resisted further military work—an attitude that Teller declared "thoughtless." Late in life, Teller suggested that he simply held different views from other scientists about weapons work and communism. At the war's end, Teller wrote, "I slowly came to realize that in two important respects, my views differed from those held by the majority. Perhaps because I had spent the previous four years working for and on

[18] Teller, *Memoirs*, 177–78.
[19] Teller, *Memoirs*, 219.

weapons research, I saw it as continuing to have great importance for the future. Unlike many in the scientific community at Los Alamos, I had become convinced that the problems connected with Communist Russia were very great." Teller, influenced by Arthur Koestler's novel *Darkness at Noon*, as well as his interactions with communist scientists, concluded "that Stalin, who could never catch up with Hitler, was still a close runner-up."[20] As the Red Scare began, Teller increasingly attempted to smear his opponents as naïve about Stalin.

Like many Manhattan Project veterans, Teller advocated international control of atomic energy, supported the development of nuclear power, and also did his part to defeat the May-Johnson bill that would have given the U.S. military control of the nation's atomic energy policy. Teller also advocated a strong atomic defense—not unlike Oppenheimer himself.[21] But Teller, who in his memoirs admitted to being a "monomaniac . . . who had several manias," saw communism as perhaps the greatest threat among equally urgent problems. Thus when the Soviet Union tested an atomic weapon in 1949, Teller focused his energies on getting government support for the Super, and in the process further developed an anticommunism quite distinct from those he saw as his rivals, including Oppenheimer and Bethe. Historians have portrayed Teller as quite active in pushing for the Super at a time when many scientists were ready to move on from nuclear weapons; he clashed with Oppenheimer, who cautiously pursued international control of atomic energy, and raged in frustration when Oppenheimer told him to "keep your shirt on."[22] Instead Teller began to find common cause with other scientists who shared his conservative, anticommunist beliefs

[20] Teller, *Memoirs*, 183.
[21] See Charles Thorpe, *Oppenheimer: The Tragic Intellect* (Chicago: University of Chicago Press, 2006), for an insightful discussion of Oppenheimer's easy transition to anticommunism after the failure of his Lilienthal (later Baruch) Plan. Oppenheimer had also, in contrast to most Manhattan Project veterans including Teller, supported the May-Johnson bill.
[22] Teller, *Memoirs*, 279.

such as Berkeley physicist Ernest Lawrence. In short, during the late 1940s and early 1950s, Teller underwent a political awakening. At the time, he described himself as "like most immigrants, a Democrat." But soon, Teller wryly recalled, Lawrence's secretary "introduced me to the idea that there were two political parties in the United States," thus improving "my grasp of political realities."[23] Teller's personal political commitments were drawing him away from the postwar nuclear establishment centered around Oppenheimer who, as the chair of the AEC's General Advisory Committee (GAC), was recognized as the leading science advisor in the nation.

During the 1954 Oppenheimer hearing, Teller himself made this split permanent. As discussed in Chapter One, Teller had blamed Oppenheimer for the failure to develop the Super. As the most prominent and outspoken (but by no means only) critic of Oppenheimer, Teller later became notorious for his testimony against Oppenheimer in the AEC security clearance hearing. Late in life, Teller recounted how his actions separated him from the scientific community that had provided him with meaning and friendship for his entire life:

> The sequela that most occupied my mind during this time was my own pain. It may be hard for someone who has not left his homeland, language, and culture behind to appreciate the profound impact the events that followed had on me. Twice before, I had been forced to relinquish the familiar—first my homeland, then Germany, a country where I could speak my second language and was familiar with the culture; and then even the continent of my birth. In my new land, everything had been unfamiliar except for the community of theoretical physicists. Nineteen years later, I was passably acquainted with my adopted country's customs, language, and attitudes. But the community of my fellow scientists was the only place that afforded me complete comfort, and had done so since my arrival. Now, at forty-seven, I was again forced into exile.[24]

[23] Teller, *Memoirs*, 404, FN.
[24] Teller, *Memoirs*, 397.

Teller's first meeting with his friend Robert Christy at Los Alamos after the hearings left a lasting memory: "I hurried over, reaching out to greet him. He looked me coldly in the eye, refused my hand, and turned away. I was so stunned that for a moment I couldn't react. Then I realized that my life as I had known it was over. I took Mici by the arm, and we returned to our room upstairs. Our last exile had begun."[25]

These somewhat melodramatic recollections indicate that the division in the scientific community became the defining feature of the rest of Teller's life; biographers have largely concurred. But Teller's papers reveal that he omitted from his memoirs the fact that he encouraged this division and used it to build his own faction of scientists in favor of nuclear weapons development and the arms race. Although Teller liked to act as though he was the victim of the Oppenheimer hearings (and indeed his personal reputation was in shambles), his reputation only suffered in the eyes of those he had already repudiated. At the same time, his testimony proved a boon to his career as it impressed the powerbrokers in the AEC and military who had maneuvered to remove Oppenheimer from power. Teller eventually emerged from the Oppenheimer hearing as one of the government's foremost science advisors. With Oppenheimer discredited, Teller embodied the zeal for weapons work demanded by the AEC's Personnel Security Board as expressed in the majority opinion against Oppenheimer:

> Government officials charged with the military posture of our country must also be certain that underlying any advice is a genuine conviction that this country cannot in the interest of security have less than the strongest possible offensive capabilities in a time of national danger.[26]

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[25] Teller, *Memoirs*, 401.

[26] U.S. Atomic Energy Commission, *In the Matter of J. Robert Oppenheimer: Transcript of Hearing before Personnel Security Board and Texts of Principal Documents and Letters* (Cambridge: MIT Press, 1970), 1016–18.

Teller would spend the rest of his life working to meet this demand for ever stronger offensive—and even defensive—nuclear capabilities.

Teller recognized how his actions against Oppenheimer had left science riven, but rather than suffer in silence or help heal the divide, he quickly mobilized to win scientists to his side. Physicist Luis Alvarez, who had also testified against Oppenheimer, wrote Teller a revealing letter showing how the Oppenheimer hearing was just one step in Teller's campaign to get U.S. scientists on his side. Just after the hearing, Alvarez went to Los Alamos, where he tried to convince other scientists that Teller had done the right thing by testifying. There he first called on Berkeley professor and former Manhattan Project physicist Robert Brode, "and I talked two hours, with the 'Bible' in my hand"—an obvious reference to the hearing transcript. Alvarez showed Brode sections he had marked, "and he [Brode] said several times to me that it was abundantly clear to him that Robert was a security risk, and that it was clear, without any evidence from the H-bomb business." Alvarez told Teller that Brode "agrees with me now, that you are an innocent bystander, and in no way a Judas." Alvarez urged Teller to get more copies of the transcript to scientists at Los Alamos in order to show that Teller had been right and Oppenheimer had been wrong.[27] Thus began an active campaign to perpetuate and exploit the schism dividing U.S. science.

Although Teller would craft a narrative that he had been ostracized from many of his peers, the reality was that he stood on one side of a divide in U.S. science, unpopular with the opposing side, but with plenty of allies of his own. Inside and outside the scientific community, many Americans supported Teller's actions, including a resident of Toledo, Ohio, who sent the physicist a telegram in October 1954. Signed "A human being," the

[27] Luis Alvarez to ET, undated, Folder 1: General, Box 2, ET papers.

telegram read: "Thank Heaven for a person with your integrity. You know your [subordinate] position, and do not attempt to decide the future of this U.S. With all the subversives, who have wormed their way into gov't. projects as 'experts' it is a joy to read of you. All the American people owe you their futures, and hopes. Bless you for your honesty."[28] Like this letter, Teller's own views came to echo those of Senator Joseph McCarthy's anticommunist crusade as the 1950s and 1960s progressed.

The Peaceful Uses of Nuclear Weapons

In a way, Teller had yoked his career, political beliefs, and even his own identity to the development of nuclear weapons. Thus the momentum that gathered for a nuclear test ban in the late 1950s threatened to quash more than just Teller's ideas for science and national security. In response Teller sought to expand the nuclear weapons establishment, as well as give those weapons a more positive reputation. Teller solidified his commitment to the militarization of science by successfully pushing for the establishment of a second nuclear weapons lab, which ended up in Livermore, California (later called Lawrence-Livermore Nuclear Laboratory). One of his pet projects at Livermore was a nuclear excavation business named Project Plowshare, so he had his own interests in mind when opposing a nuclear weapons test ban.[29] During the 1950s, Teller was eager to show that nuclear explosives also had "peaceful uses." His list of "suitable projects" for Plowshare included using thermonuclear explosions to create "harbors, channels and canals." Expecting opposition on

[28] Telegram to ET, Oct. 1954, Folder 5: Oppenheimer, J. Robert, Miscellaneous, Box 30, ET Papers.
[29] Teller, *Memoirs*; Goodchild, *Edward Teller*, and Herken, *Brotherhood of the Bomb*, cover Plowshare; Daniel T. O'Neill, *The Firecracker Boys* (New York: St. Martin's Press, 1994), details the campaign against Plowshare in Alaska. Scott Kirsch offers a geographer's perspective in *Proving Grounds: Project Plowshare and the Unrealized Dream of Nuclear Earthmoving* (New Brunswick: Rutgers University Press, 2005).

account of a fear of fallout, Teller argued that the Plowshare program ensured that it would be "possible to release large energies in a safe way even rather near to populated areas."[30]

Teller knew that to prove Plowshare's feasibility, he had to provide a concrete demonstration of its viability. This meant finding some place on Earth willing to host a nuclear explosion—one that would also benefit from the changed landscape. Teller soon decided that Alaska would be the perfect place to reveal Plowshare's potential. He drafted a memo and sent it to Washington's Democratic Senator Henry Jackson, explaining that the Plowshare team was ready to show that excavation was possible, and could do so in the United States. "Alaska has impressed all who have looked at the problem," he wrote, though he urged discretion since "the [Atomic Energy] Commission seems to be quite anxious not to influence real estate values or upset anybody by speculation."[31]

The memo outlined grand uses for Plowshare, including "shattering water impermeable layers" of earth to "influence the distribution of water underground and thereby make more water available in semi-arid regions." Teller also saw "extensive possibilities of using nuclear explosives for mining purposes" that "might increase our oil reserves very greatly." With even more grandeur, the memo explained how the Plowshare explosion in Alaska (codenamed Project Chariot) would even help atone for use of the A-bomb during World War II:

> For the relatively near future the most valuable natural resources in the neighborhood of the proposed harbor is the coking coal deposits to the immediate north of it. There is a market for such coal on the West Coast and particularly in

[30] Memo, "Plans for the Peaceful Use of Thermonuclear Explosives (Plowshare) in Alaska and Elsewhere"; ET to Jackson, Sept. 22, 1958, Folder 1: Reading File, June–Oct. 1958, Box 420: Chronological Correspondence, 1956–58, ET Papers.
[31] "Plans for the Peaceful Use of Thermonuclear Explosives (Plowshare) in Alaska and Elsewhere"; ET to Jackson, Sept. 22, 1958, ET Papers.

> Japan. It would be satisfying if the first peaceful application of nuclear explosives would benefit the same Country which had first suffered from the atomic bomb.[32]

Teller then began a prolonged attempt to seduce Alaska into hosting a thermonuclear blast. One week after President Eisenhower signed the Alaska Statehood Act on July 7, 1958, Teller left for the state "with the hope that the elation about statehood will help along other reasonable plans," a clear reference to Plowshare.[33] One year later, after returning from another trip, Teller told the president of the University of Alaska: "You may have heard about the fact that I am practically on my way back to Alaska. I just cannot stay away from your state for any length of time."[34] Invited to speak at Canada's McGill University on the subject of nuclear-earth moving, Teller replied that "An objective lecture could, I believe, be arranged. In talking about Plowshare we shall try, of course, to avoid making propaganda. At the same time you will naturally expect us to be thoroughly interested in our work and to show our interest."[35] All these efforts in the forty-ninth state came to nothing; Daniel O'Neill explains in *Firecracker Boys* how groups of local activists and University of Alaska scientists defeated Project Chariot.

Although no nuclear excavations took place, Teller's dream of a nuclear-powered earth-moving business lasted a long time. A January 1967 PSAC meeting agenda discussed the "Plowshare Excavation Program," and in the 1980s Teller went to Australia to propose a

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[32] "Plans for the Peaceful Use of Thermonuclear Explosives (Plowshare) in Alaska and Elsewhere"; ET to Jackson, Sept. 22, 1958, ET Papers.
[33] ET to John H. Morse Jr., July 14, 1958, Folder 1: Reading File, June–Oct. 1958, Box 420: Chronological Correspondence, 1956–58, ET Papers.
[34] ET to Ernest N. Patty, June 9, 1959, Folder 3: Reading File, June–Sept. 1959, Box 423: Chronological Correspondence, 1959–1961, ET Papers.
[35] ET to Stuart Foster, Oct. 29, 1959, Folder 4: Reading File, Oct.–Dec. 1959, Box 423: Chronological Correspondence, 1959–1961, ET Papers.

Plowshare-esque project aimed at unearthing uranium deposits in Darwin.[36] Teller never indicated that the program's lack of profit bothered him in the least, since Plowshare's great value lay in providing a way to continue weapons experiments and subvert the arms control measures being pursued by the Eisenhower administration in the 1950s. After representatives at the 1958 Geneva test ban negotiations proposed a test cessation, Teller told NASA missile engineer Hans Mark that "I am beginning to think quite seriously of putting under Plowshare a shot with the exclusive purpose of scientific experiments" that would provide data against a nuclear test ban.[37] In a letter to the chair of the AEC's GAC, Teller directly linked Plowshare to the prevention of a test ban, writing that "the demonstration of the peaceful use of nuclear explosives will help to stop the threat toward ban of all nuclear explosives."[38]

"Making the Facts Known": The Role of Scientists

Teller linked himself, his science, and his scientific allies to nuclear weapons and the national security state. In this way, Teller was not totally unlike Pauling, who in opposing nuclear weapons hoped to create a society based on the vision of scientists like himself. But in contrast to Pauling's "science of morality"—a pompous but consistent personal credo—Teller shifted between viewpoints on the proper role of a scientist in society. Just as Pauling invoked his moral philosophy to justify his activism, Teller articulated a position on the role of scientists in society that could alternately boost his own image (as "objective" and

[36] PSAC Jan. 23, 24, 1967 meeting agenda, Box 5, Folder: PSAC, 1967—All Correspondence, HUG (FP) 94.14, George B. Kistiakowsky Papers, Harvard University (hereafter GBK Papers); "Presenting the Real Life Dr. Strangelove," *Gamut,* May 24, 1977, in Folder 4, Box 1, ET Papers.
[37] ET to Hans Mark, Sept. 10, 1958, Folder 1: Reading File, June–Oct. 1958, Box 420: Chronological Correspondence 1956–58, ET Papers.
[38] ET to Warren C. Johnson, Feb. 24, 1958, Folder: GAC: Opinions on GAC issues, Box 439: Topic Files, ET Papers.

"apolitical") or criticize his opponents (as "subjective" and "political"). Most often he advocated that scientists should be apolitical, but at other times he admitted to being active in politics, and sought to justify his actions.

Teller's science always matched his politics. As the A-bomb was originally an anti-Nazi endeavor, for Teller the H-bomb directly countered the aggressive tendencies of the Soviet Union, matching his anticommunism. But his conflicts with other scientists demanded he hide any connection between science and political positions in order to claim that he held the objective high ground. Since the late 1930s, Teller's scientific work had been rooted in his conscience: he helped Szilard get the U.S. government interested in a building an A-bomb, and at Los Alamos, Teller again felt compelled to act on his conscience when Szilard began circulating a petition urging a demonstration of the bomb as an alternative to military use against Japan. Szilard's petition emphasized the role of scientists in determining how the bomb would be used and argued for a demonstration on the grounds that it would avoid bloodshed and uphold moral responsibility. In his memoirs, Teller wrote "Those reasons made good sense to me, and I could think of no reason that those of us at Los Alamos who agreed shouldn't sign it." Oppenheimer told him not to sign, however, counseling instead that he place his faith in the leaders of the nation.[39]

Curiously, instead of simply notifying Szilard of Oppenheimer's refusal, Teller fashioned a different reasoning in his response to Szilard: "The accident that we worked out this dreadful thing should not give us the responsibility of having a voice in how it is to be used. This responsibility must in the end be shifted to the people as a whole and that can be

[39] Teller, *Memoirs*, 205. See William Lanouette, *Genius in the Shadows: A Biography of Leo Szilard, the Man Behind the Bomb* (New York: C. Scribner's Sons, 1992), 269–75, and Michael Bess, *Realism, Utopia and the Mushroom Cloud: Four Activist Intellectuals and Their Strategies for Peace, 1945–89* (Chicago: University of Chicago Press, 1993), 48–49, for discussions of Szilard's petition.

done only by making the facts known."[40] Teller thus constructed a new vision of scientists somewhere between passive bystanders and political activists. In his memoirs, Teller explained further. "That was our job as scientists," he wrote, "a point that became clearer when I became aware of the great progress the Soviet Union had made on a nuclear explosive. The responsibility of scientists is to describe and demonstrate what is possible, to disseminate that knowledge as fully as possible, and, with everyone else in our democracy, to share the decisions that are necessarily connected with knowledge." Time and again Teller would argue that, in contrast to Pauling's "morality of science," scientists' "special responsibility" extended only as far as informing the public and those in power of the technical and scientific possibilities of nuclear weapons.[41] Voters and government officials would decide how to act on these possibilities.

Predictably, Teller framed his thoughts as a contrast to Oppenheimer's. Teller wrote that he felt betrayed when he learned that Oppenheimer had been an advisor on the wartime committee to examine the use of the bomb. Teller was especially peeved because Oppenheimer had told him that scientists should leave the decision-making to men like Truman, Stimson, and Marshall: "In the course of time, I became aware that when Oppenheimer had advised me that, as scientists, 'we' should not participate in the decisionmaking, he had already acted contrary to his advice. The result was that I became convinced that, as scientists, we have a clear responsibility to participate in decisions by making new knowledge and new possibilities available for discussion."[42] Teller repeated his thinking in a 1960 letter to a Fordham University official about a talk he proposed on "The

[40] Teller, *Memoirs*, 207. Szilard had asked Teller not to Oppenheimer's permission to circulate the petition. Teller's reasoning resembles Oppenheimer's contention that using the bomb will alert the world to the necessity of avoiding war in the future.
[41] Teller, *Memoirs*, 208.
[42] Teller, *Memoirs*, 209.

Role of a Scientist." He explained, "I frequently have been faced with the argument that a scientist is responsible for the way in which the result of his work is used. I disagree with this. I do not believe that the scientist has such a responsibility. However, I do believe that the scientist has many other responsibilities," namely, seeing that new facts and discoveries become public knowledge.[43]

These carefully chosen words sound straightforward, but Teller would use them (or some variation of them) again and again during the Cold War to hide or deny his great influence. In fact, Teller spent much of the Cold War trying to shape policy and dictate the end uses of science by dreaming up new weapons systems, imagining ways that the Soviets might attack, proffering reasons not to trust the Soviets, and enumerating the flaws of arms control proposals.

Teller's stance was, essentially, pronuclear. Historians have noted that some Manhattan Project physicists expressed a sense of responsibility—often bordering on guilt—for Hiroshima. Robert Wilson recounted that "The news of the tremendous suffering and damage and loss of lives . . . was an epiphany that has changed my life ever since." Szilard embarked on a restless crusade for world peace, while Pauling (not a Manhattan Project scientist) wanted scientists to dedicate themselves to life-affirming work—though Oppenheimer biographer Charles Thorpe rightly points out that "the idea that the Manhattan Project's scientists were collectively wracked with guilt over Hiroshima and Nagasaki is a misconception."[44] Teller sought to appeal to those scientists who saw Los Alamos as a patriotic and impressive accomplishment by ignoring the bomb's negative image

[43] ET to Thomas E. Paradine, Jan. 13, 1960, Folder 5: Reading File: Jan–April 1960, Box 423: Chronological Correspondence, 1959–1961, ET Papers.
[44] Wilson quoted in Thorpe, *Oppenheimer*, 165–66. See Bess, *Utopia*, for a discussion of Szilard's strategies for peace.

and arguing that scientists should let the proper authorities decide on the end results of scientific knowledge, a passive action that would undoubtedly result in military applications of science. At the Oppenheimer hearing, the Personnel Security Board had demanded that scientists devote themselves to the strongest military capabilities of the United States; the national interest was defined in explicitly military terms. The state had thus made it clear how it would use the knowledge its scientists produced. To ignore the end result may have sounded like objectivity, but it was in reality the tacit acceptance of the militarization of science, especially because Teller was completely aware of what the state would do with scientific knowledge.[45]

Teller competed with other scientists during the 1950s and 1960s. While Pauling (and other scientists) had cultivated an image of thoughtful, responsible, scientific leadership, Teller had to counter their claims while still retaining the same scientific credibility. As a scientist, Teller counted on and believed in the image of science as truth just as Pauling did. Also like Pauling, Teller imagined and promoted his own distinct vision of the scientist's role in society. At the same time, he needed to simultaneously attack the political agenda of his liberal opponents.

But as a vocal and politically active scientist, Teller was often accused of violating his claim to scientific objectivity. He accordingly formulated a philosophy that justified his actions, and revealed his vision in a letter to Robert Gilpin, the author of *American Scientists and Nuclear Weapons Policy,* who had interviewed Teller for the book. Teller began by contesting the book's conclusions, but then stated:

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[45] See Thorpe, "Disciplining Experts: Scientific Authority and Liberal Democracy in the Oppenheimer Case." *Social Studies of Science*, August 2002, Vol. 32, No. 4, 545.

> I am happy to see you quote my statement that a scientist should work and clarify and that beyond that point he has no further duties. . . . To put it another way: I do not insist that a scientist should not voice and defend his political opinion, I do feel, however, that he should not support his purely political opinion with his scientific authority, that he should separate his functions as a scientist and as a citizen, and that he should refrain from generating the false impression that scientific competence is a necessary prerequisite of finding one's way among the political consequences of technical developments. In other words, I feel that scientists should not pose as the high priests and augurs on whom society must depend if wise decisions are to be reached.[46]

Despite his claims to the contrary, Teller constantly relied on his scientific expertise to give credence to his political activities and opinions. During the 1950s, Teller spent little time on work—scientific or otherwise—not explicitly connected to national defense. A letter from physicist Freeman Dyson to his parents, written after a visit in 1958 to Livermore, gives a glimpse of the scientific work of the lab. In the days before the nuclear test moratorium, "Livermore was wildly exciting," Dyson wrote. The Livermore scientists

> were throwing together everything they possibly could to give it a try before the guillotine came down. Everyone was desperate and also exhilarated. Edward Teller, who is head of the Lab talked to me quite a lot about his plans. He was in very good spirits and pressed me with invitations to come and work for him. . . . A lot of the talk at Livermore was about cheating the test-ban. We found a lot of ways to cheat which would be quite impossible for any instruments to detect. The point of this is not that the Livermore people themselves intend to cheat, but we are convinced the Russians can cheat as much as they want any time they want, without being found out.[47]

In crafting an image of scientists divorced from responsibility for the social and political uses of their knowledge, Teller simply wished to distance himself from the scientists associated with disarmament and arms control. In 1957, Szilard attempted to get his friend Teller involved in the first Pugwash conference, but Teller turned him down. "I am not confident

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[46] ET to Gilpin, Nov. 24, 1961, Folder 6: Reading File, Oct.–Dec. 1961, Box 422: Chronological Correspondence, 1959–61, ET Papers. Gilpin, *American Scientists and Nuclear Weapons Policy* (Princeton: Princeton University Press, 1962).
[47] Quoted in Teller, *Memoirs*, 436.

that scientists are in any better position than other people to make reasonable recommendations about questions like the stability of the world," he wrote Szilard. "The facts are really available to all people and to the extent they are not I feel that we should make efforts to make them more available."[48] Teller consistently opposed excessive secrecy requirements during the Cold War, though this viewpoint, if acted upon, could have conflicted with the security system. Teller was of course well aware of the consequences of crossing the security system and thus he ostentatiously obeyed secrecy requirements while steadfastly bemoaning them.

Although Teller's vision sounded quite neutral, it was anything but. In contrast to his comments above, Teller admitted on several occasions that science did have a role to play in politics, but only because his scientific views were truly objective. In a letter to Frederic Nagy, Teller told the former Hungarian Prime Minister that he was not disturbed by claims of mixing science with politics. He explained "that people will pay close attention to what I say only as long as my statements are connected in some way with my scientific and technical knowledge. I am afraid that if I deviated from this course in the near future I would lose what little effectiveness I have." Teller added, "There is one point about which you may share the misconception of some other people. My influence within the Administration is practically nonexistent. Some people in Congress do listen to me, but this is almost entirely due to the fact that they respect my scientific objectivity."[49] Like Pauling, Teller claimed the mantle of scientific objectivity and attempted to apply it to his interpretation of the dilemmas of the nuclear age.

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[48] ET to Szilard, Dec. 15, 1957, and ET to Szilard, Sept. 15, 1957, Folder 2: Reading File, May–Dec. 1957, Box 421: Chronological Correspondence, 1956–58, ET Papers.
[49] ET to Nagy, May 13, 1960, Folder 1: Reading File, May–June 1960, Box 422: Chronological Correspondence, 1959–61, ET Papers.

Meanwhile, Teller accused his critics of lacking objectivity. After Walter Selove trashed Teller's *Our Nuclear Future* in a 1958 *Science* book review, Teller wrote to Selove: "You seem to regret that I am going beyond the scientific points and that I am dabbling in politics. I should like to remind you that a few years ago, shortly after you came to Livermore, you encouraged me to become more vocal in politics. Do I have to conclude that you want only those people who agree with you to express their views?"[50]

And while Teller's vision for scientists conveniently matched his own behavior (again not unlike Pauling), Teller also crafted an image of what a scientist should *not* be. In another letter to Nagy, Teller told him that "The majority of the scientists are indifferent to politics. Of the minority which are engaged in politics, practically all are dedicated to a line of appeasement."[51] Such a stance implied that a political scientist was a seditious scientist. Playing the role of the reluctant but loyal patriot, Teller deigned to enter politics, only doing so for the sake of national security.

Teller's thoughts about scientists' role in society went deeper than his purely political concerns. Despite his pronuclear stance, his views on other aspects of the scientific vocation were in some ways similar to those of his rival scientists who pursued arms control, including Szilard and Pauling, who both believed that scientists deserved an influential and high status role in society.[52] In 1956 Teller unburdened himself of his views on science, scientists, and society in a letter to C.L. Brown, an Esso scientist in New Jersey with whom Teller had had a conversation about scientific education. In his letter, Teller wrote: "I feel

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[50] ET to Selove, May 24, 1958, Folder 2: Reading File: Jan–May 1958, Box 420: Chronological Correspondence, 1956–58, ET Papers.
[51] ET to Nagy, Jan. 12, 1962, Folder 5: Reading File, Jan–March 1962, Box 426: Chronological Correspondence, 1962–66, ET Papers.
[52] On Szilard and the *Bund*, see Bess, *Utopia*, 49–52.

that there is a definite need in this country for a kind of re-orientation in values. It is right and wholesome that we should love football and respect those who can play it well, but we need to be made aware that considerable contributions to our welfare are coming from sources which are not only ignored, but often scoffed at." Teller bemoaned that those responsible for creating the high standard of living in the United States were largely dismissed by the public as "at best . . . harmless eccentrics." Despite the public's belief in a scientific community, Teller saw science as a lonely pursuit, perhaps rooted in his own self-image as scientific pariah. "The scientist must either not give a hoot what people think of him," he wrote, "or he must conceal his interest and work in solitude; and naturally this atmosphere does little to encourage entrance into the field." The letter then took a personal turn for Teller. "My son, Paul, is thirteen," he wrote.

> Some of my interest in science has rubbed off on him, and with me he is enthusiastic in his curiosity. But he conceals this curiosity from his teachers and his classmates. This is pretty clever of him because he knows that people would not understand his interest in such a boring subject and would consider him a kind of anomaly. . . . But, if my son were a really good quarterback, would he be ashamed to let his classmates know about it? The result of all this is that we are flooding the market for quarterbacks while the field of science goes begging.

Teller compared the status of scientists in the United States to their peers in Europe and even the Soviet Union, where "a scientist is very nearly the most revered man in the community. It is not at all inconceivable that at a point not too far in the future, Russia could surpass us in scientific achievement, and this is just about the most dangerous situation I can imagine." He proposed to fix the problem through education. As a start, "we must pay teachers better." Science teachers must also improve, he argued, and interactions between

scientists and budding students must increase. He also hoped for efforts to popularize science as well as industry support for lectures and books.[53]

Teller wrote this letter before the Soviet launch of *Sputnik*, meaning that the National Defense Education Act had yet to flood schools and colleges with funding for math and science education. At the time, the "manpower gap" worried some congressmen that the Soviets had surpassed the United States in the production of scientists and engineers.[54] Teller revealed in this letter similarities with Pauling, who also argued that scientists deserved a social status that reflected their importance. Teller's letter reflects how the public often switched its views on scientists after World War II, alternating between awe and suspicion.[55] At Livermore, Teller attempted to mobilize U.S. science in a way that would validate his political beliefs and create for himself a more conservatively aligned scientific community.[56]

Other scientists attempted mobilization as well. For Szilard, individuals and nation states needed to soberly confront the dangers of nuclear war. For Pauling, science and scientists needed to lead the way to a peaceful, humanistic society. And for Teller, science needed to protect U.S. society from the communist threat. Despite each man's belief that science transcended politics, science was in fact inexorably wedded to politics and society. According to Thorpe, science had been inextricably bound to the state during the Manhattan

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[53] ET to Brown, April 3, 1956, Folder 3: Reading File 1956, Box 420: Chronological Correspondence, 1956–58, ET Papers. Teller's son Paul did not become a quarterback, but a philosopher of science.
[54] John Krige, *American Hegemony and the Postwar Reconstruction of Science in Europe* (Cambridge: MIT Press, 2006), 193–96.
[55] Paul Boyer, in *By the Bomb's Early Light: American Thought and Culture at the Dawn of the Atomic Age* (Chapel Hill: University of North Carolina Press, 1994, second edition), describes an adulation of scientists after Hiroshima. And Kelly Moore notes that scientists were *Time* magazine's Men of the Year in 1960, in *Disrupting Science: Social Movements, American Scientists, and the Politics of the Military, 1945–1975* (Princeton: Princeton University Press, 2008), 1. But as David Kaiser points out, in the intervening years, scientists—and theoretical physicists especially—were seen as a suspect group prone to communist sympathies. "The Atomic Secret in Red Hands? American Suspicions of Theoretical Physicists During the Early Cold War," in Cathryn Carson and David Hollinger, eds., *Reappraising Oppenheimer: Centennial Studies and Reflections* (Berkeley: Office for History of Science and Technology, University of California at Berkeley, 2005).
[56] See S.S. Schweber, *In the Shadow of the Bomb: Bethe, Oppenheimer, and the Moral Responsibility of the Scientist* (Princeton: Princeton University Press, 2000), for a discussion of Bethe, who cultivated and thrived in a different type of scientific community.

Project and in the immediate postwar era, when, after the failure of the Baruch Plan, Oppenheimer (as the de facto leader of the scientific establishment) adopted the anticommunist position of the Truman administration. The H-bomb mandate and finally the Oppenheimer hearing only restricted scientists' behavior; they had long ago been drawn into the national security state.[57]

But individual scientists could still decide whether they would submit to the state's will. Pauling, for example, tried to recast science as a peaceful endeavor and resisted the militaristic vision of scientists proffered by the government. Additionally, at the time of the Oppenheimer hearing, many scientists predicted that their colleagues might refuse government work altogether.[58] Teller's actions were not those of a man calmly in control of policy, but of a man constantly struggling to maintain policy in the direction of his interests. The state continued to rely on scientists to improve and expand the nuclear arsenal. And despite the new rigidity enforced by the Oppenheimer hearing, the H-bomb had in fact created a whole new awareness of the nuclear threat among scientists and grassroots activists.[59]

Teller thus constantly worried about opposition from politicians, scientists, and the public, waging a scientific and political campaign in order to maintain his privileged space in the state. Like McCarthy and other partisans of the era, Teller divided the world in two: on the one side stood the patriotic advocates of a strong nuclear defense, and on the other side

[57] See Thorpe, *Oppenheimer*, 247–53, on Oppenheimer's anticommunism.
[58] See "Scientists Affirm Faith in Oppenheimer," *Bulletin of the Atomic Scientists,* May 1954, Vol. X, No. 5, 189. Some scientists did give up government work or weapons work.
[59] Lawrence Wittner, *The Struggle Against the Bomb: Vol. 2. Resisting the Bomb: A History of the World Nuclear Disarmament Movement, 1954–1970* (Stanford: Stanford University Press, 1997), 1, 7, 11, 14, 27, 33–39, 51–52, 134–40, 256–61, 292–95.

stood those who opposed weapons development. Though political arguments were at the heart of his activism, Teller attracted a broad audience because of his scientific credibility.

Calming Reassurance: Teller's Scientific Arguments

Much antinuclear activism in the 1950s and early 1960s centered on the harmful effects of nuclear fallout released into the air during nuclear weapons tests. This fear of fallout first gripped many Americans when radioactive elements began to appear in milk, threatening to increase genetic mutations in newborn children. Scientists like Pauling and Ralph Lapp took the lead in criticizing nuclear tests for needlessly polluting the planet with Sr90 and C14. This sentiment eventually coalesced around support for a nuclear test ban, even becoming a significant issue during the 1956 presidential election.[60] Ever vigilant against criticism of nuclear weapons, Teller and his fellow physicist Albert Latter wrote and published *Our Nuclear Future* in 1958 in order to counter the "fallout propaganda" which was making Teller "more and more unhappy."[61]

On a very public level, Teller and his rivals differed over the role and content of expertise and the interpretation of complex scientific data. In 1958, for example, Warren Weaver criticized some of the data from *Our Nuclear Future*. Teller responded:

> I find that it is hard for me to agree with some of the details of your criticism. For instance, we state in the article that 100r will, as a general rule, not cause permanent damage and then proceed to make a comparison with the 0.0003r from the fallout. You say that this gives the misleading impression that 0.0003r is negligible. Having looked into the matter with a little care we have come to the conclusion that as far as somatic effects are concerned, no harm from 0.0003r has been proved. I know that I

[60] See Robert Divine, *Blowing on the Wind: The Nuclear Test Ban Debate, 1954-1960* (New York: Oxford University Press, 1978), and Benjamin P. Greene, *Eisenhower, Science Advice, and the Nuclear Test-Ban Debate, 1945–1963* (Stanford: Stanford University Press, 2007), on the test ban and the 1956 election. Greene argues that public opinion against fallout was less severe in the United States than overseas, *Eisenhower*, 7, 59, 90, 142.
[61] ET to Al Latter, May 29, 1957, Folder 2: Reading File, May–Dec. 1957, Box 421: Chronological Correspondence, 1956–58, ET Papers.

> am talking here about your proper field and your implied opinion that such little amounts of radiation have certainly a harmful effect is most significant. However, having talked, in some detail, with quite a few people who are among the best experts in the field, you must allow me to keep an open mind on the issue.[62]

In his writings, talks, and correspondence, Teller reassured the public that fallout was almost entirely safe, and that nuclear tests would benefit humanity in two ways. First, testing would protect innocents by helping develop a clean, fallout-free bomb; and second, testing would enhance the nuclear weapons that protected the United States from Russia through deterrence.

Teller's scientific claims can be seen in many places, including a standard letter that Teller sent in response to the queries of ordinary people: "Fallout from tests is not dangerous; [a] foolproof inspection system is impossible, as it would be very difficult to distinguish between an earthquake and an underground kiloton explosion; and nuclear weapons are essential in order to defend ourselves and the free world against Communist aggression." Teller offered two further ways in which science negated the need for a nuclear weapons test ban: that civil defense worked, and that the science supporting a test ban was unsound.[63]

Facing the public's fears of fallout, Teller calmly answered them by swearing on his own scientific credibility that fallout was safe. For example, he simply dismissed nuclear fears, claiming that nuclear fallout was as safe as a common chest x-ray.[64] These claims did, at least in one instance, cause some confusion. Teller's comparison between fallout and chest x-rays alarmed a Dr. A.H. Schwichtenberg, who wrote to Teller and explained that he had

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[62] ET to W. Weaver, Feb. 24, 1958, Folder 2: Reading File: Jan–May 1958, Box 420: Chronological Correspondence, 1956–58, ET Papers. Weaver criticized a *Life* magazine reprint of material from Teller's book.
[63] See for example ET to Norman Caine, June 2, 1958, Folder 1: Reading File, June–Oct. 1958, Box 420: Chronological Correspondence, 1956–58, ET papers.
[64] Greene, *Eisenhower*, tabs Willard Libby as the source of this comparison, 65.

become wary of submitting his patients to chest x-rays. Teller responded that he meant "to point out how unimportant the present fallout hazards are and not in order to state how damaging chest x-rays can become. I am firmly convinced the good which comes from chest x-rays greatly outweighs any possible and as yet unproven damage." Always the polymath, he went on to offer some ideas about how "to reduce the intensity of chest x-rays."[65]

To promote the idea of safe fallout, Teller wrote to the Deputy Assistant Secretary (Health and Medical) of Defense. He argued that Pauling's and Lapp's views were "distorted," and contradicted an article printed on the subject in the *Washington Post*: "The estimate of 200,000 to a million leukemia victims is a pure guess," he wrote. "There is no concrete evidence that these low-radiation levels have any effect on leukemia. . . . That there are some mutations induced by low-level radiation is undoubtedly correct," but "[t]he estimates given are unreliable." Teller may have been on relatively safe ground when he noted that not all mutations are harmful (though most are). But in misleading overstatement he made the case that fallout might help the human race evolve. "The implied assumption that all mutations are harmful is incorrect. Without mutations adaptability and evolution would be impossible." Teller advised the secretary to concentrate on three other facts: that radiation from fallout was "considerably less" than other sources of radiation, that "different types of radiation have entirely similar effects," and that "many avoidable sources of radiation, such as high altitude radioactivity, wrist watches, etc., are not being considered by

[65] ET to A.H. Schwichtenberg, undated, Folder 1: Reading File, June–Oct. 1958, Box 420: Chronological Correspondence, 1956–58, ET Papers, emphasis in original.

people like Lapp and Pauling and that therefore their bias against nuclear explosions rather than radiation is obvious."[66]

In 1960, Teller gave his frequent correspondent Henry Kissinger a quick lesson on fallout. He described C14's radiation as "so weak that its biological effects need not be taken at all seriously. . . . The harmful nature of radiation as weak as this is completely unproven. It is likely to induce some mutations but the increase in the mutation rate over normal mutations is quite infinitesimal."[67] In yet another variation on the safe fallout theme, Teller told the Senate Committee on Foreign Relations that because the city of Denver was situated at a high altitude and consequently had a thinner atmospheric shield against the sun, "People in Denver are in greater danger" of exposure to radiation than those exposed to fallout from nuclear tests.[68]

During the test ban debate of the 1950s, Teller proposed to many people (including Lewis Strauss, at the time chair of the AEC) that instead of a comprehensive test ban or moratorium, the United States should restrict the amount of radioactivity released into the atmosphere.[69] Clearly, Teller also had other interests at stake: "The main problem which we have to put before the public is the local fallout because this is the only worry and the only real worry, and because it is closely connected with Plowshare."[70] A comprehensive test ban

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[66] ET to Frank. B. Berry, Oct. 23, 1961, Folder 6: Oct.–Dec. 1961, Box 422: Chronological Correspondence, 1959–61, ET Papers.
[67] ET to Kissinger, May 7, 1960, Folder 1: Reading File, May–June 1960, Box 422: Chronological Correspondence, 1959–61, ET Papers. C14 and Sr90 are incorporated into human tissue and decay immediately, making them vastly more damaging than elements that decay outside the body, where damaging effects can be blocked by the skin.
[68] U.S. Congress, Senate Committee on Foreign Relations, *Nuclear Test Ban Treaty: Hearings before the Committee on Foreign Relations, United States Senate*, 88th Cong., 1st sess., August 12–15, 19–23, 26–27, 1963 (Washington, D.C.: U.S. Government Printing Office), 455.
[69] ET to Lewis Strauss, March 20, 1958, Folder 2: Reading File, Jan–May 1958, Box 420: Chronological Correspondence, 1956–58, ET Papers.
[70] ET to Stuart Foster, Oct. 29, 1959, Folder 4: Reading File, Oct.–Dec. 1959, Box 423: Chronological Correspondence, 1959–1961, ET Papers.

would ruin Teller's plan for a nuclear excavation business, whereas a ban that limited the amount of radiation released would keep the program alive.

A limit on released radiation would also allow work to proceed on a so-called clean bomb, an idea Teller proposed to rehabilitate the image of nuclear weapons. With typical optimism, Teller counterintuitively claimed that the problem of fallout could be solved by more testing and new weapons. As progress on a test ban gained steam, Teller began to make promises of clean bombs—thermonuclear weapons of heightened efficiency that would release little or no radioactive fallout. Teller wrote to Kissinger: "You probably know already that in general terms I am in favor of clean weapons. The essential reason is that in the case of dirty weapons we get more of an effect which is unintentional or uncontrollable."[71]

Such weapons would help protect innocent civilians whether used in testing or in war. In a letter to Stuart Symington (D-MO), Teller gave his pitch for clean weapons to the influential Senator who had been the first Secretary of the Air Force in the late 1940s. "In a limited war," Teller wrote, "radioactivity from a dirty bomb is likely to be carried over countries which are not participating in the conflict. There would be every moral reason and considerable political reason to assure the neutral countries that we at any rate are not hurting them." Teller told Symington that while radioactivity from testing "is negligible," numerous weapons used in war could create a hazardous amount of fallout. He therefore wanted to "limit the harmful effects of nuclear weapons to the enemy's fighting forces. I feel most unhappy about hurting anyone who is not directly participating in the conflict. In case of a war with Russia it is in my opinion right not only to save all American lives that can be

[71] ET to Kissinger, May 7, 1960, Folder 1: Reading File, May–June 1960, Box 422: Chronological Correspondence, 1959–61, ET Papers.

saved, but to save all Russian lives which can be reasonably saved." Because the clean bomb had not been developed yet, of course, further testing was necessary—and thus a test ban was in fact immoral.[72]

Teller (along with his UC Berkeley colleagues Ernest Lawrence and Mark Mills) made this exact claim in a meeting with President Eisenhower. After the meeting, an NSC staff member wrote to Eisenhower's special assistant for National Security Affairs. The staffer claimed that Teller's argument

> reopens the issue of nuclear testing, in my opinion, from both a technical and moral point of view. . . . The scientists raised the moral issue of the use of nuclear weapons in the following way: a. If we know how to make . . . weapons, but fail to do so and to convert existing weapons into . . . ones, then the use of dirty weapons in war would be a "crime against humanity." b. If we live up to an agreement to stop our tests and the Soviets continued (secretly and illegally) testing and develop . . . bombs, we may face a situation in the future in which world opinion would inhibit us from using our . . . weapons, while the Soviet Union would not be restrained from using their . . . weapons.[73]

Elsewhere, Teller argued that "Clean bombs must be developed to save the innocent bystander," though nuclear weapons' fundamental value was as a deterrent: "Without atomic weapons there would not be any means by which to stop Russian aggression."[74] In a letter to AEC commissioner Thomas Murray, Teller expanded upon the theme: "I agree that our present disarmament policy is ill-conceived and that a distinction must be drawn between violence and force. I fully agree that force is justified, but violence is not." He offered Murray a new way beyond "the immoral doctrine of massive retaliation." The nation, he argued, should retain enough nuclear weapons "for the exclusive purpose of deterring

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[72] ET to Symington, May 23, 1958, Folder 2: Reading File, Jan.–May 1958, Box 420: Chronological Correspondence, 1956–58, ET Papers. As an added bonus, an H-bomb triggered by pure fusion would derive its power completely from Teller's work, and would not be defiled by using Oppenheimer's fission bomb as a trigger.

[73] U.S. State Department, *Foreign Relations of the United States, 1955–1957* (Washington, D.C.: U.S. Government Printing Office, 1990), Volume XX: 640–41, elided text in original.

[74] Genevieve Greteman, ET's secretary, to Donald Robinson, June 27, 1957, Folder 2: Reading File, 1957, Box 421: Chronological Correspondence, 1956–58, ET Papers.

massive attack on our own Country. . . . At the same time we must announce that an all-out attack on Russia will not be under taken unless we suffer such an attack ourselves at their hand. This means that we must develop our retaliatory force to the point where it cannot be wiped out by an all-out Soviet attack." Nuclear defense of the NATO alliance could be achieved with "small nuclear weapons and not by the threat of massive retaliation." Finally, a renunciation of would contribute "to a considerable extent to the stability of the world. I do not believe that the Russians will choose the path of violence."[75] In short, Teller advocated a no first-use policy that relied on nuclear weapons as deterrents.

As yet another reason to avoid a test ban, Teller offered the concept of civil defense. Teller contacted Senator Henry Jackson (D-WA) in 1956 with what he described as "another maverick idea." He argued that civil defense could work, and "we can look into the future with a considerable amount of optimism." He assured the Senator that

> It is possible to construct deep shelters, readily available to all the endangered part of our population for a fraction of the cost of our present yearly defense budget. Such shelters could be reached in a few minutes by anyone in our densely populated areas. They would stand up under anything except a direct hit by an H-bomb. They could weather the worst things that can follow atomic bombing, such as the heaviest contamination or fire storms, and could see our people through the critical days in discomfort, but in safety. The number of casualties would, of course, be great; but the nation as a whole could survive the attack.[76]

Finally, Teller frequently challenged the science behind detection and verification in order to damage the test ban under discussion in the Eisenhower administration. During his second term, Eisenhower desired a test ban but refused to consider proposals that were not

[75] ET to Thomas E. Murray, Dec. 9, 1959, Folder 4: Reading File, Oct.–Dec. 1959, Box 423: Chronological Correspondence, 1959–1961, ET Papers.
[76] Here, Teller sounds like Herman Kahn. ET to Jackson, May 15, 1956, Folder 3: Reading File 1956, Box 420: Chronological Correspondence, 1956–58, ET Papers.

enforced by completely reliable verification systems.[77] Teller exploited Eisenhower's demands, and easily sabotaged test ban proposals by questioning their verification measures. He claimed, for example, that Democrat scientists "are so confident about the soundness of their seismological judgment and about the effectiveness of our proposed network that they seem to expect to detect whenever a fly lands on the Ural Mountains."[78]

Confronting the Appeasers: Teller and the Scientific Divide

Despite Teller's wide array of scientific arguments, politics were clearly at the heart of the scientific divide over nuclear weapons, as Teller spent much more time criticizing his opponents' politics than their science. In his correspondence, Teller saw nuclear politics in personal terms and simplistically reduced the opposing sides to patriots and appeasers, reflecting his own nuclear identity. This split fell along the same lines that formed when he exiled himself from much of the scientific community after the Oppenheimer hearing. Well into the 1960s, Teller often complained about an omnipotent "Oppenheimer group" that controlled U.S. foreign policy.

The Oppenheimer hearing had created a massive division among scientists, with undeniably personal consequences for Teller. In 1962 he told Richard Nixon that in "the Oppenheimer conflict I had found myself accused and even slandered by many of the people whom I used to count among my friends."[79] That same year, when a CBS News producer offered Teller $3,000 to participate in an interview which would discuss, among

[77] See Greene, *Eisenhower*, 5, 12–13, 21, 187, 196.
[78] ET to David Griggs, undated, Folder 5: Reading File, Jan.–April 1966, Box 423: Chronological Correspondence, 1959–1961, ET Papers.
[79] ET to Nixon, Nov. 16, 1962, Folder 2: Reading File, Oct.–Dec. 1962, Box 426: Chronological Correspondence, ET Papers.

other subjects, the Oppenheimer hearing, Teller replied that "even for $3,000,000 I would not participate in an interview in which we planned to include the Oppenheimer case."[80]

In his explanation to the show's producer, Teller linked the Oppenheimer schism to the arms race and the Cold War. Teller first explained that during the hearing he "could not conscientiously avoid giving testimony," and that he testified "reluctantly and unhappily . . . caus[ing] me more personal unhappiness than I can easily explain to you." Teller continued to endow the hearings with personal and international significance:

> On the other hand, the public debate that resulted from the hearings introduced a deep split in the scientific community, impeded work on national defense, and has contributed powerfully to the present situation where—according to my opinion—Soviet developments have outstripped our own. Any further discussion of the hearings is likely to contribute to this unfortunate situation both in the personal sense and in the public domain. A repetition of questions about Oppenheimer's security clearance will force me to say either what is not true or else what is harmful to both the public and my personal interests.[81]

Teller again typically downplayed his own role, absolving himself of responsibility. And as seen with Pauling, Teller was not the only scientist with a divisive view of science's role in society. But more than anyone else, Teller encouraged the scientific divide that defined nuclear policy debates during the 1950s and early 1960s. Throughout the decade, as Pauling tried to mobilize science for a test ban, Teller worked just as hard to keep science committed to nuclear weapons. His rhetoric in personal correspondence reflects his divisiveness; he became especially fond of the right-wing tactic of branding his political opponents as appeasers, a phrase loaded with connotations of weakness, femininity, naivety, and even disloyalty. He stated in 1958 that test ban talks in Geneva "remind me of the procedures of

[80] ET to John Beck, March 21, 1962, Folder: CBS—RE Cancelled Talk, Box 436: Topic Files, ET Papers.
[81] ET to Jack (John F.) Beck, March 5, 1962, Folder: CBS—RE Cancelled Talk, Box 436: Topic Files, ET Papers.

Munich in 1938," and that the country was "giving in."[82] In a letter to Senator Clinton P. Anderson (D-NM) in 1960, Teller described arms control talks as "a trend which may lead to an agreement similar to that of Munich in 1938. As Munich has led to World War II, so a new surrender may lead to a third world war."[83]

Such language described general political trends as well as specific individuals. In addition, Teller frequently waged his campaign on a personal level. After Teller discussed nuclear weapons with Carl von Weizsacker, the German physicist who had worked with Niels Bohr and later joined Werner Heisenberg on the Nazi A-bomb project, he wrote to Eugene Wigner, who sympathized with Teller's views. "He [Weizsacker] also continues to feel that there are strong arguments for stopping nuclear arms in one way or another," Teller told Wigner. "I could not convince him that all these plans are completely unrealistic and that one should not look in this direction when one tries to insure peace."[84]

In 1958 Soviet Premier Nikita Khrushchev challenged President Eisenhower to begin a nuclear testing moratorium. Although fearful that the Soviets would cheat a moratorium, Eisenhower also felt that a testing pause could serve as a sort of practice test ban allowing PSAC and other officials time to work on a formal test ban agreement. During the testing pause, Teller struggled to stave off a test ban. Writing to Los Alamos director Norris Bradbury after learning of the moratorium agreement, Teller lamented: "I now learn with the greatest of disappointment that apparently neither of us will be able to execute the experiment. You know how greatly I am worried about the consequences of the recent

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[82] ET to Floyd Odlum, August 9, 1958, Folder 1: Reading File, June–Oct. 1958, Box 420: Chronological Correspondence, 1956–58, ET Papers

[83] ET to Anderson, March 25, 1960, Folder 5: Reading File, Jan.–April 1960, Box 423: Chronological Correspondence, 1959–1961, ET Papers.

[84] ET to Wigner, April 21, 1958, Folder 2: Reading File: Jan–May 1958, Box 420: Chronological Correspondence, 1956–58, ET Papers.

policy decisions and I am anxious to do everything to put our developments in the strongest possible position because I am afraid even that will not be strong enough."[85] The United States began its moratorium at the end of October, and the Soviet Union followed suit just a few days later; the moratorium continued into the Kennedy administration until the Soviets ended it in September 1961.

As the director of the Livermore Lab between late spring 1958 and June 1960, Teller advocated a lasting and influential pronuclear standpoint that embraced deterrence for the present and development of new nuclear weapons for the future, claiming that nuclear weapons not only kept the peace, but also promoted essential U.S. values.[86] He wrote to AEC chairman John McCone as the moratorium continued: "You know that I am deeply worried about the issue of the test moratorium. The reason for my worry is the fate of our Nation and the fate of the Free World."[87] Months later Teller wrote to McCone again. "If weapons work is discontinued in Livermore many of the best people in the Laboratory will leave. They will feel that the Laboratory is no longer fulfilling the unique mission to which it had been dedicated," Teller argued. "Our group here is firmly convinced that in maintaining our preparedness we are working for peace and serving a purpose of the greatest importance."[88] Since the culture of Livermore was built around nuclear tests, a prohibition

[85] ET to Bradbury, Aug. 28, 1958, Folder 1: Reading File, June–Oct. 1958, Box 420: Chronological Correspondence, 1956–58, ET Papers.
[86] Hugh Gusterson's study, *Nuclear Rites: A Weapons Laboratory at the End of the Cold War* (Berkeley: University of California Press, 1996), found that scientists at Livermore decades later had the same feelings.
[87] ET to McCone, March 8, 1960, Folder 5: Reading File, Jan–April 1960, Box 423: Chronological Correspondence, 1959–1961, ET Papers.
[88] ET to McCone, Nov. 25, 1960, Folder 4: Reading File, Oct.–Dec. 1959, Box 423: Chronological Correspondence, 1959–1961, ET Papers.

on these essential exercises meant that new weapons scientists would not be properly trained.[89]

Most often, however, instead of casting his own efforts in a positive light, Teller preferred to attack his opponents, in particular Linus Pauling, the scientist most associated with a test ban. Teller often opposed Pauling on a scientific level by trying to counter Pauling's data with data of his own, rebutting rational argument with rational argument. The embodiment of this approach came during their 1958 debate that aired on the Bay Area's PBS affiliate, KQED. For years after, Teller considered himself the victor and constantly sent a transcript of the program to people who wrote to him, and implored others to seek it out when he ran out of copies.[90]

Teller, who had more of a sense of humor than Pauling, recognized a shared iconoclasm between the two. After Pauling endured a night stuck on a cliff at his ranch that overlooked the Pacific Ocean, necessitating emergency rescue, Teller dashed off a brief letter to the chemist. "Dear Linus: I was happy to read in the paper that you are safe and that the Pacific Ocean has been now properly instructed in the theory of the chemical bond," Teller chuckled. "The particular purpose of this letter is to welcome you to the most exclusive club of controversial scientists out on a limb."[91]

Such humor was most likely wasted on Pauling, probably because of Teller's otherwise relentless and occasionally mean-spirited attacks on Pauling. When Teller and Lawrence managed to meet with President Eisenhower on June 24, 1957, Teller immediately

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[89] Gusterson, *Nuclear Rites*, 131–64, and *People of the Bomb: Portraits of America's Nuclear Complex* (Minneapolis: University of Minnesota Press, 2004), 147–64.
[90] See for example ET to Norman Caine, June 2, 1958, Folder 1: Reading File, June–Oct. 1958, Box 420: Chronological Correspondence, 1956–58, ET Papers.
[91] ET to LP, Feb. 1, 1960, Folder 5: Reading File, Jan.–April 1960, Box 423: Chronological Correspondence, 1959–1961, ET Papers.

brought up Pauling's petition against nuclear testing. "Dr. Teller commented, in reference to discussion of Dr. Pauling's recent statement, that there are 5,000 scientists on the Berkeley Campus and only 27 of them signed his statement. This 27 included no biologists and no physicists engaged in atomic studies," the record of the conversation states. Apparently Eisenhower recognized "that the Pauling comment may be quite invalid but he said that so many nations and people are reading in the press these fearsome and horrible reports that they are having a substantial result." Lawrence ended by saying "there should be no implication that the testing that has been, and is now being, conducted will have any appreciable adverse effect."[92]

In other circumstances, Teller preferred to paint Pauling as an appeaser. "I deeply regret the immoderate way in which Dr. Pauling is expressing himself, the more so because he is a very good scientist," Teller wrote to one correspondent. "I believe that disarmament at this time is a mistake. The logic which Dr. Pauling is using in connection with the Communists is the same that was taken at the time of Munich in 1938. His point of view will lead to either surrender or delay in our resistance. In order to avoid another war it is important that our Country remain strong."[93] In another letter, Teller railed: "I am not the only one who is worried, but it seems that only Pauling and his friends talk and those of us who believe that the days of Munich are being repeated seem to shut up."[94]

Pauling apparently did not even deserve the right to voice his views. When the American Chemical Society wanted Pauling to give a talk, Teller appealed to the organization

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[92] *FRUS, 1955–57*, Vol. XX: 639.
[93] ET to Mrs. William McKeeman, June 30, 1960, Folder 1: Reading File, May–June 1960, Box 422: Chronological Correspondence, 1959–61, ET Papers.
[94] ET to Jimmy Allen, Sept. 13, 1958, Folder 1: Reading File, June–Oct. 1958, Box 420: Chronological Correspondence 1956–58, ET Papers.

as a scientist: "It certainly is inappropriate to give Linus Pauling a free chance to make propaganda in the Chemical Society," he wrote. "The knowledge of Linus Pauling on that subject is not that of a scientist because his scientific work had nothing to do with the topic in question. His knowledge is not even that of an informed intelligent layman. His knowledge is that of a propagandist representing an extreme point of view." Teller explained that although he had previously respected Pauling's right to free speech, "the Chemical Society is not the right forum and I also find that Dr. Pauling has gone in his pronouncements beyond limits of really good taste."[95]

Teller continued to feel threatened by Pauling's peace campaign and insisted on discrediting Pauling's views while portraying himself as the rational, credible authority. When Teller was invited to a debate with Pauling, geophysicist Harrison Brown, and scientist-writer C.P. Snow, he complained that his would-be opponents were "those people whose main object seems to be to scare everybody and to discourage any other methods toward a reasonable solution for peace." Teller described himself as one of the "vocal advocates for positive measures of defense."[96]

An important aspect of Teller's efforts involved making common cause with politicians who opposed the antinuclear movement. Teller heartily endorsed the efforts of Thomas J. Dodd, the Democratic Senator from Connecticut who had taken on SANE, Pugwash, and Pauling in an attempt to discredit the antinuclear movement. He wrote to the Senator's office: "I finished reading Senator Dodd's statement on the Sane Nuclear Policy group. I think that it is a wonderful statement." Meeting Dodd, Teller added, "was a great

[95] ET to Arthur Adamson, undated, Folder 1: Reading File, April–June 1963, Box 425: Chronological Correspondence, 1962–66, ET Papers.
[96] ET to Allen Clarke, Feb. 8, 1961, Folder 3: Reading File, Jan.–March 1961, Box 422: Chronological Correspondence, 1959–61, ET Papers.

privilege." To his many contacts, Teller echoed the line of Dodd, who many considered a Democratic McCarthy.[97] Teller sent Dodd's speech to a friend in an attempt to give the speech wider distribution; the speech had alleged that "the Communists have unquestionably succeeded in infiltrating this [test-ban] movement . . . providing it with much of its guiding philosophy and stock of arguments."[98] Teller deemed the speech "excellent," and wrote to yet another correspondent, "The propaganda of the Sane Nuclear Policy people is to a considerable extent financed by the Communists. . . . If in a capitalist country the capitalists do not find the money to give a small amount to refute the false Communist statements, then I believe we shall deserve what we are going to get." He then described his plans for "a small organization" that would "counteract the program of appeasement" in which Dodd "might take a very active part."[99] Though the organization never materialized, Teller continued to recruit help in getting Dodd's speeches distributed. One such approach explained: "I feel that the issue of testing is becoming sharper every week. On the other hand, our disadvantage due to the moratorium is mounting; yet Russian actions make it more and more clear that appeasement does not pay. Senator Dodd's statement is most helpful in this situation."[100]

Teller took particular interest in Dodd's congressional investigation of Pauling. When Pauling failed to crack under Dodd's inquiry, Teller wrote to the Senator's office: "I continue to worry about the Pauling hearings. I would very greatly appreciate it if you could

[97] ET to David Martin, May 31, 1960, Folder 1: Reading File, May–June 1960, Box 422: Chronological Correspondence, 1959–61, ET Papers; "Ex-Senator Dodd Is Dead at 64," *New York Times*, May 25, 1971.
[98] Thomas J. Dodd, "The Summit and the Test Ban Fallacy," *Congressional Record—Senate*, May 12, 1960, 10137. The Soviet-run World Peace Congress did advocate specifically for a test ban and in general for arms control that limited U.S. (and not Soviet) strength. Pauling was one of the very few non-aligned activists who refused to dismiss communist-aligned antinuclear activism out of hand. See Wittner, *Resisting the Bomb*, 92.
[99] ET to T.F. Walkowicz, May 27, 1960, Folder 1: Reading File, May–June 1960, Box 422: Chronological Correspondence, 1959–61, ET Papers.
[100] ET to Floyd Odlum, June 29, 1960, Folder 1: Reading File, May–June 1960, Box 422: Chronological Correspondence, 1959–61, ET Papers.

send me a transcript of these hearings."[101] That same day Teller also helped promote Dodd's anti-Pauling and anti-SANE campaign by "arranging to have 25,000 reprints of Senator Dodd's speech distributed" by a member of the Air Force Association.[102]

Teller had plenty of other scientists to confront with the same name-calling tactics he used against Pauling. Like other activist-scientists, Teller expressed a desire for peace, but he differentiated himself from his disarmament-oriented peers with his fierce anticommunism. In an exchange between him and I.I. Rabi, the men used crocodiles as a metaphor for communists. Teller told Rabi: "I think that one of the main differences between us is that I happen to know personally a greater number of people who were eventually eaten by crocodiles."[103] Though based in his childhood experiences, Teller's anticommunism effortlessly transformed into a badge of status among the conservatives of an era still in the shadow of McCarthyism. From behind his shield of anticommunism, Teller attacked prodisarmament scientists in his correspondence with influential people. In a way, Teller waged the battle as a public relations campaign aimed at scientists and policymakers who he saw as his constituency.

To a scientist at Oak Ridge during the 1960 presidential campaign, Teller wrote: "there are a number of indications which make me apprehensive and lead me to believe that Kennedy is for appeasement. For this reason I have made up my mind to vote for Nixon." That same year he corresponded with Hungary's former Prime Minister: "I am fully aware of the enormous dangers to which our policy of appeasement exposes all of us. It is also clear

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[101] ET to David Martin, June 29, 1960, Folder 1: Reading File, May–June 1960, Box 422: Chronological Correspondence, 1959–61, ET Papers.
[102] ET to James Straubel, June 29, 1960, Folder 1: Reading File, May–June 1960, Box 422: Chronological Correspondence, 1959–61, ET Papers.
[103] ET to Rabi, March 31, 1959, Folder 2: Reading File, Jan.–May 1959, Box 423: Chronological Correspondence, 1959–1961, ET Papers. Other prominent Hungarian physicists included Eugene Wigner and Johnny von Neumann, both staunch anticommunists. Their fellow physicist and countryman Leo Szilard, on the other hand, was far more tolerant.

that the first and decisive mistake was committed when we allowed the Russians to move into eastern Europe."[104] To Harry Truman Teller alleged: "Many people in this Country are doing their utmost to repeat all the mistakes of Munich." Teller found encouragement in the former president's reply, and invited him to visit Livermore. In 1964, even as the U.S.-Soviet confrontation began to ease, Teller complained to Herman Kahn: "I do worry somewhat about increased emphasis throughout the Country on moves that are politely termed 'détente' but could be impolitely called 'appeasement.'"[105]

Teller did display some principles. As with the Oppenheimer hearing, Teller did not accuse his opponents of disloyalty, but rather the lesser crime of naiveté. He expressed as much in a letter to Congressman Francis E. Walter: "I am sure that you and I are in complete agreement about the dangers of Communism. It seems to me, however, that the primary danger does not arise from the internal subversion, but rather from the military might of the Communists and from their spreading influence outside the United States." Teller recommended "that we should counteract our opponents within the United States with the greatest possible patience."[106]

A zeal for anticommunism often overcame his own advice, however, as he continued to attack his opponents with virulent denunciations. After *Sputnik*, the Eisenhower administration brought scientists (via PSAC and the presidential science advisor, James Killian) into the cabinet circle to advise on arms control, science policy, and—most

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[104] ET to Robert H. Messier, Oct. 12, 1960, Folder 1: Reading File, Oct.–Dec. 1960, Box 423: Chronological Correspondence, 1959–1961; ET to Ferenc Nagy, May 13, 1960, Folder 1: Reading File, May–June 1960, Box 422: Chronological Correspondence, 1959–61, ET Papers. Nagy had been deposed after a communist coup in the 1940s.
[105] ET to Truman, May 11, 1960, Folder 1: Reading File, May–June 1960, Box 422: Chronological Correspondence, 1959–61; ET to Kahn, Feb. 28, 1964, Folder 1: Reading File, Jan.–March 1964, Box 424: Chronological Correspondence, 1962–66, ET Papers.
[106] ET to Francis E. Walter, April 7, 1961, Folder 4: Reading File, April–June 1961, Box 422: Chronological Correspondence, 1959–61, ET Papers.

importantly—the technical aspects of a nuclear test ban. Though Teller was invited at least once by Killian to participate on a PSAC panel, Teller mostly sought to counter the influence of these advisors.[107] Hans Bethe, an influential PSAC advisor on arms control and disarmament and Teller's old rival from the Manhattan Project, suffered much of Teller's abuse. Although in Teller's words the two "used to be really good friends"—indeed Bethe had signed a 1941 letter to Teller, "Love, H. Bethe"—by 1962 Bethe's "methodical and unimaginative approach and . . . his passion for working with the appeasers has caused confusion and is going to do still more damage."[108] Teller confessed to Lewis Strauss that Bethe "has been misled by his wishes," and he hoped "that this mistaken advice will not lead us into a really terrible situation," an implication that PSAC scientists would render the United States impotent against the Soviet threat.[109] Teller wrote directly to Bethe in late 1960 about nuclear testing in outer space, which Teller saw as a method the Soviets could use to evade detection. "I feel extremely strongly about your refusal to discuss these matters with me. In particular, your statement that you won't discuss them in private I feel is completely unjustified," he complained. "To the extent that your personal feelings are involved I am both regretful and also greatly disappointed about your reaction," he continued. "But there is a more serious point: I have no doubt that you, like I, feel strongly about the National

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[107] U.S. State Department, *Foreign Relations of the United States, 1958–1960* (Washington, D.C.: U.S. Government Printing Office, 1996), Vol. III: 696.
[108] ET to Charter Heslep, Aug. 2, 1962, Folder: AEC/ Public Info Office Heslep Re Bethe Meeting in 1962, Box 444: Topic Files; ET to Walter Selove, June 14, 1958, Folder 1: Reading File, June–Oct. 1958, Box 420: Chronological Correspondence, 1956–58, ET Papers. Bethe to ET, Sept. 9, 1941, Folder 8.53 (untitled), Box 8, Hans Bethe Papers, Cornell University.
[109] ET to Strauss, April 25, 1958, Folder 2: Reading File: Jan.–May 1958, Box 420: Chronological Correspondence, 1956–58, ET Papers.

importance of the question under discussion. Your refusal of a face-to-face discussion strikes me as an irresponsible position."[110]

Teller also attacked James Killian, Eisenhower's first science advisor. He told Air Force General James Doolittle that "It is an unpleasant fact that the Killian Committee is dominated on the scientific side by men who have worked most effectively on the national defense up to 1945, but have tended to oppose the development of nuclear weapons after 1945." Although Teller thought Killian and his scientists acted "in good faith," these good intentions did not "detract from the dangers of the situation which they are creating."[111] To Hans Mark, Teller worried that a Plowshare shot might be cancelled because of the impending moratorium: "I am afraid that a lot will depend on Killian who so far has been extremely unfriendly."[112] Teller confessed that his disagreement with Killian "is by no means a scientific one" because Killian surely agreed with Teller's scientific assessment: "I do not think Killian believes that a test ban can be really effectively enforced."[113]

This name-calling campaign was consciously waged in order to influence the national security priorities of the state. In the late 1950s and early 1960s, Teller began to link himself more and more to the fortunes of rising Republican politicians, especially Nelson Rockefeller. He initiated fairly regular correspondence with Rockefeller (and Rockefeller's associate Henry Kissinger) to advise the aspiring presidential candidate on science and national security issues. Teller wrote to Kissinger in 1962 providing a list of scientists "to

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[110] ET to Bethe, Oct. 29, 1960, Folder 1: Reading File, Oct.–Dec. 1960, Box 423: Chronological Correspondence, 1959–1961, ET Papers.
[111] ET to James Doolittle, Nov. 5, 1958, Folder 1: Reading File, Nov.–Dec. 1958, Box 421: Chronological Correspondence, 1956–58, ET Papers.
[112] ET to Mark, Sept. 10, 1958, Folder 1: Reading File, June–Oct. 1958, Box 420: Chronological Correspondence 1956–58, ET Papers.
[113] ET to T. Keith Glennan, May 23, 1958, Folder: GAC: Opinions on GAC issues, Box 439: Topic Files, ET Papers. Killian did, in fact, later reverse course on a comprehensive test ban. See Greene, *Eisenhower*, 166.

whom Nelson might want to talk about questions of scientific development." Predictably, Teller began with a scathing denunciation of Oppenheimer, despite the latter's removal from power eight years earlier: "Without any doubt he is by far the most clever on the side of the opposition," Teller railed. "Nelson might want to talk with him because in this way he gets to the source of many of the opinions. On the other hand, Oppenheimer is also the least sincere and this might cancel his usefulness." Teller clearly endorsed and reinforced the idea of an Oppenheimer divide in the scientific community, while also maintaining his conspiracy theory, first expressed during the H-bomb debate, that Oppenheimer controlled scientists' opinions.[114]

In the same letter, Bethe also suffered Teller's acid critique. "He is the scientific mainstay of the opposition," Teller told Kissinger. "He talks with an appearance of considerable authority. Actually he is relatively easily influenced and his opinions are none too stable." I.I. Rabi, a vocal scientist-critic of Teller, was dismissed as "a complete opportunist." About Killian, he wrote: "Actually his first-hand scientific information is nil and he is strongly biased. I would be hopeful that he would not be asked." Revealing that he sought to influence scientists just as much as he claimed Oppenheimer did, Teller said of Edward Purcell: "He is eminently fair-minded and in fact could be won over to our side, but actually is at present under the influence of those with whom you and I disagree." George Kistiakowsky, Killian's successor as Eisenhower's science advisor, was "a rather frivolous individual." Wolfgang Panofsky "is thoroughly convinced of the general line of Oppenheimer and Bethe" and "is one of the strongest among our opponents," while Freeman Dyson at Princeton's Institute of Advanced Study "is under the influence of our

[114] ET to Kissinger, Aug. 8, 1962, Folder 3: Reading File, July-Sept. 1962, Box 426: Chronological Correspondence, 1962–66, ET Papers.

opponents but he has withstood this influence remarkably well." UCLA geophysicist David Griggs, who had testified against Oppenheimer and questioned the technical reliability of a test ban, "is the strongest proponent of the anti-Oppenheimer group. He is apt to make rather extreme statements which turn out to be correct in a great number of cases."[115]

Such claims had credibility because of Teller's access to individuals as well as classified information. To his credit, Teller consistently opposed secrecy restrictions all his life, even though as head of Livermore he had access to plenty of restricted information. But while the information remained secret, Teller used his access to it to his advantage. He made a show of telling people that he had access to secret information, but that in the interests of national security he could not reveal it. For example, when a member of the American Chemical Society suggested that Teller debate Pauling, Teller pondered the challenge. "It so happens that I have some relevant specialized knowledge in the field," he wrote. However, he added, "I cannot use this knowledge in any complete manner because of security restrictions."[116] In an interview given during his campaign against the 1963 Limited Test Ban Treaty, Teller claimed that his biggest reservations about the treaty were classified.[117] Even when he could not reveal information, he allowed his access to such information to appear as a mark of credibility. Thus he dismissed many rivals who lacked equivalent access, such as Bethe, who "does not have complete information," he wrote.[118] To Bethe himself, he argued: "I would be very happy if I could share your opinion that we are ahead of the Russians in tactical weapons. From what I know I have not the slightest reason to conclude that they

[115] ET to Kissinger, Aug. 8, 1962, ET Papers.
[116] ET to Arthur Adamson, undated, Folder 1: Reading File, April–June 1963, Box 425: Chronological Correspondence, 1962–66, ET Papers.
[117] Interview with Hahn, July 31, 1963, Folder: Test Ban: 1963 Press Coverage, Box 448: Topic Files, ET Papers.
[118] ET to Walter Selove, June 14, 1958, Folder 1: Reading File, June–Oct. 1958, Box 420: Chronological Correspondence, 1956–58, ET Papers.

are. I am most eager to hear from you why you are so optimistic on this score."[119] To Eugene Rabinowitch, editor of the *Bulletin of the Atomic Scientists*, Teller complained about a paper by Jay Orear written in support of a test ban: "My difficulty in discussing it is: security. I never know precisely what has been declassified and what has not. . . . It seems clear to me that I should not discuss methods of evasions openly."[120] Teller then wrote to Orear to convince him that under a test ban, the Soviets could still hide tests and evade detection. "I have said repeatedly," he thundered, "that I cannot tell you how tests can be hidden without violating security regulations. I can assure you that I have studied the question carefully." He then encouraged Orear and Rabinowitch to obtain security clearances and participate in government research.[121]

The above exchange was hardly Teller's only clash with the *Bulletin.* Although Teller had been among the journal's earliest members, he, like Pauling, split from the *Bulletin.* The same Bentley Glass article from the *Bulletin* that so angered Pauling had actually saved most of its venom for Teller.[122] Both Teller and Pauling were thus seen as divisive figures by other scientists. In the eyes of its editors, the *Bulletin* offered a diverse sounding board for opinions on the nuclear age. In Teller's view, however, they pushed a purely political line. When *Bulletin* editors changed the wording in an article of his, Teller saw it as a personal affront. "I hope you will excuse me for taking this a little bit to heart. I wonder whether the Bulletin is practicing these editorial changes with all its authors. I should feel that the

[119] ET to Bethe, Oct 3, 1960, Folder 1: Reading File, Oct.–Dec. 1960, Box 423: Chronological Correspondence, 1959–1961, ET Papers.
[120] ET to Rabinowitch, Jan. 20, 1958, Folder 1: Reading File, June–Oct. 1958, Box 420: Chronological Correspondence, 1956–58, ET Papers.
[121] ET to Jay Orear, March 8, 1958, Folder 2: Reading File: Jan.–May 1958, Box 420: Chronological Correspondence, 1956–58, ET Papers.
[122] Bentley Glass, "Scientists in Politics," *Bulletin of the Atomic Scientists*, May 1962, Vol. XVIII, No. 5, 2–7.

Bulletin is not properly serving its purpose in providing a means of expression for its varied contributors."[123]

Teller continued to battle with Orear into April 1958. After Orear testified about a test ban in front of the Committee on Foreign Relations, Senator Hubert Humphrey (D-MN) had asked Teller to comment on Orear's testimony. When Teller sent his comments to Humphrey, he also told to Orear that he was "most deeply disturbed" by his claims, including "greatly exaggerated accuracy for seismic data." He then challenged Orear's use of science to support a policy position:

> Your standing as a professor of physics gives your words authority which should not be misused in technical matters. You are suggesting repeatedly that the weight of plutonium or deuterium determines the weight of nuclear bombs. In actual fact you have no knowledge of the way nuclear bombs are constructed and you are fully aware of the fact that you have no such knowledge. May I suggest that you read over your testimony on these points. You might even find it useful to make a strong and clear statement about your lack of competence in certain areas. This may increase the weight of your word in the future when you talk about matters in which you are competent. . . . I think that by your exaggeration you have not helped your case and have done damage to the profession which, I hope, is equally important to both of us.[124]

In late 1959, after reading the *Bulletin*'s annual report, a "deeply disturbed" Teller wrote to Rabinowitch in order to complain about the *Bulletin*'s mission statement, which described the journal's "primary purpose" as "explor[ing] and expos[ing] the danger of nuclear war, the consequences of testing nuclear weapons, and other dangerous implications of science in human affairs; and to discuss methods to meet and control these dangers, and to approach a stable peace." The reference to the consequences of testing especially peeved Teller, who wrote to Rabinowitch:

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[123] ET to Rabinowitch, April 18, 1957, Folder 2: Reading File, 1957, Box 421: Chronological Correspondence, 1956–58, ET Papers.
[124] ET to Orear, April 25, 1958, Folder 2: Reading File, Jan.–May 1958, Box 420: Chronological Correspondence, 1956–58, ET Papers.

> Do you mean that nuclear testing is dangerous in itself because of fallout or some other reason? . . . Or do you mean to say that nuclear testing is indirectly dangerous by causing international tension? If so, could nuclear testing not also be beneficial by creating greater stability due to power in the hands of free nations? Whether we agree or not on this last point, would you not be forced to state that the answer to this question is not one of scientific discussion, but one of politics? Is it proper that a scientific publication with the prestige of science behind it should prejudge an issue of this kind to the extent that it states its primary purpose is to expose a danger whose very existence is only an assumption.[125]

To Eugene Wigner in early 1960, Teller confided, "I am greatly worried about the Bulletin. They have a really harmful influence," and added that its editorial standards were "one-sided." Later that year Teller bluntly told Rabinowitch that his *Bulletin* was "a partisan publication."[126]

A Well-Armed Lobby: Teller's Insider Influence

Although he often refused the title, Teller's public image as the "father of the H-bomb" demonstrated to many his unwavering anticommunist credentials. Teller thus had access to important government figures, and did not shy from approaching them. While both he and Pauling had attempted to sway many minds to their respective points of view, Teller cultivated connections to those already more inclined to support his pronuclear views. Because Teller was making a case *for* nuclear weapons, his arguments were not received by government officials with as much skepticism as some other scientists' views. Since his anticommunist, pronuclear scientific arguments fit in nicely with the militaristic drift of U.S.

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[125] ET to Rabinowitch, Dec. 14, 1959, Folder 4: Reading File, Oct.–Dec. 1959, Box 423: Chronological Correspondence, 1959–1961, ET Papers.

[126] ET to Wigner, Jan. 19, 1960, Folder 5: Reading File, Jan.–April 1960, Box 423: Chronological Correspondence, 1959–1961; ET to Rabinowitch, July 13, 1960, Folder 2: Reading File, July–Sept. 1960, Box 422: Chronological Correspondence, 1959–61, ET Papers.

policy during the 1950s, Teller had easy access to political officials; he also attempted to bend specific aspects of the rewards and education system to reflect his own views.

The armed forces warmly received Teller. At one point he wrote to the Air Force commander-in-chief about weapons development. "I would be very happy to join in advocating any system, with practically no regard to its expense," he stated, "provided that the Russians cannot counter this defense promptly by a much less expensive development." Teller was especially taken with missile defense, including both a U.S. system and ways of overcoming a Soviet system. He expressed the "great danger of the Russians developing an anti-ICBM" as a reason for U.S. anti-ICBM research.[127] Teller discussed planning requirements for long range nuclear weapons development with Admiral Arleigh A. Burke of the Navy, and served on the Science Advisory Board of the Air Force, which he described as "the only group of scientists who are giving effective and sane advice to our Government."[128]

Teller not only approached military men but also government lawmakers, concentrating his efforts on those who occupied important committee seats. In Congress, he advised Senator Henry "Scoop" Jackson (D-WA), a member of the Preparedness Subcommittee and the Joint Committee on Atomic Energy (JCAE), and served on Jackson's NATO Committee on Scientific and Technical Personnel.[129] During 1957 JCAE hearings, Jackson paraphrased Teller's "Denver" argument as a question for witness Dr. Shields Warren.

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[127] ET to Laurence S. Kuter, Folder 1: Reading File, Oct.–Dec. 1960, Box 423: Chronological Correspondence, 1959–1961, ET Papers.
[128] ET to Burke, Sept. 26, 1957, Folder 2: Reading File, May–Dec. 1957, Box 421: Chronological Correspondence, 1956–58; ET to James Doolittle, Nov. 5, 1958, Folder 1: Reading File, Nov.–Dec. 1958, Box 421: Chronological Correspondence, 1956–58, ET Papers.
[129] ET to Bethe, Jan. 8, 1958, Folder 2: Reading File: Jan.–May 1958, Box 420: Chronological Correspondence, 1956–58, ET Papers.

> Dr. WARREN. . . . as a scientist I am firmly convinced that radiation will produce mutations. The estimates that have been made by the majority of geneticists appeal to me as reasonable and sound estimates.
>
> Senator JACKSON. In that connection, Dr. Warren, have there been any studies made of the situation as in Denver where people live at 5,000 feet as distinguished from people living at sea level? I was told that these mutations do not occur as anticipated.
>
> Dr. WARREN. One would have to get a much higher level than occurs in Denver to reach the doubling dose that we have spoken of.[130]

In response to the Senator's 1958 request, Teller sent Jackson a memo on Plowshare and also denounced current test ban proposals, particularly the lack of a "rascal-proof plan for policing."[131]

In 1962, Jackson asked Teller for a list of people to recommend for service on the PSAC, and Teller again took the opportunity to castigate his opponents and promote his allies. John Wheeler, Teller wrote, "would be my first choice. I doubt that he would be strong enough to balance the present overwhelming opinion on the PSAC, but he would be a good influence." Ford Researcher Montgomery Johnson "is generally respected, but of course is not considered as a real first runner in those circles in which you have to be of the opinion of [Kennedy Science Advisor Jerome] Wiesner and Bethe in order to be taken seriously." And Griggs "has been one of the strongest opponents of Oppenheimer in 1954. For all the above reasons he is thoroughly unpopular with the present members of PSAC. In

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130 U.S. Congress, Subcommittee on Radiation of the Joint Committee on Atomic Energy, *The Nature of Radioactive Fallout and its Effects on Man*, 85th Congress, 1st Sess., Hearings, May 27–29 and June 3–7, three volumes (Washington, D.C.: U.S. Government Printing Office, 1957), 983.

131 ET to Jackson, Sept. 22, 1958, Folder 1: Reading File, June–Oct. 1958, Box 420: Chronological Correspondence 1956–58, ET Papers.

fact, I suspect he is the only one who is more unpopular than I am. For this very reason he may be the best candidate."[132]

Access to Congress also came through his position on a JCAE Advisory Panel. In 1958 Teller wrote to Rep. Carl T. Durham (D-NC), the chair of the JCAE, to propose limiting the amount of radiation released in testing every year instead of a test ban. Because anti-ballistic missiles (ABMs) needed high altitude testing and peaceful uses of nuclear explosives needed atmospheric testing, he reasoned, a test ban might present an obstacle to future innovations in nuclear weaponry and defense.[133]

Teller continued to devote much of his time lobbying Symington, a member of the Preparedness Committee whom he once described as "our only hope."[134] These efforts sometimes involved straightforward scientific education, as in early February 1958, when Teller sent Symington a primer (evidently requested) on the scientific concept of parity as well as the "screw-nature of the neutrino."[135] But even basic science could be essentially political, as when he wrote a long letter to Symington in March 1958 with a detailed explanation of the dangers and non-dangers of radiation.[136] At other times, Teller attempted to influence Symington more directly. In April 1958, Teller directly asked Symington for a chance to testify in a closed session. "I believe that the analysis of Bethe, for which I have

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[132] ET to Jackson, Aug. 8, 1962, Folder 3: Reading File, July–Sept. 1962, Box 426: Chronological Correspondence, 1962–66, ET Papers. Naturally, making nominations and suggestions was perfectly reasonable and normal for scientists. Alvin Weinberg wrote to Emanuel Piore to name his possible replacements on the PSAC: Wigner, McMillan, and Graves. Though Weinberg gave reasons for each, none of them mentioned specific political views. Weinberg to Piore, April 2, 1962, Box 2, Folder: PSAC, 1961—All Correspondence, HUG (FP) 94.14, GBK Papers.

[133] ET to Henry M. Jackson, undated, Folder 2: Reading File, Jan.–May 1958, Box 420: Chronological Correspondence, 1956–58; ET to Carl Durham, June 4, 1958, Folder 1: Reading File, June–Oct. 1958, Box 420: Chronological Correspondence, 1956–58, ET Papers.

[134] ET to T.G. Lanphier Jr., Feb. 16, 1960, Folder 5: Reading File, Jan.–April 1960, Box 423: Chronological Correspondence, 1959–1961, ET Papers.

[135] ET to Symington, Feb. 5, 1958, Folder 2: Reading File, Jan.–May 1958, Box 420: Chronological Correspondence, 1956–58, ET Papers.

[136] ET to Symington, March 10, 1958, Folder 2: Reading File, Jan.–May 1958, Box 420: Chronological Correspondence, 1956–58, ET Papers.

considerable technical respect, is nevertheless incomplete," he explained. "It is important that the weak points of this analysis be clearly brought out."[137] Not all of Teller's attempts to lobby against the test ban worked, of course. In 1959, he wrote to a correspondent, "I was just in Washington and did my best to see Senator Symington. This unfortunately did not work out. I am quite unhappy about it because the Geneva discussions are in a critical state."[138]

Again lobbying against a test ban, Teller wrote to Symington about the "clear and increasing evidence of the enormous difficulties of enforcing a test ban. The technical facts which lead to this conclusion have been agreed to by all, including the original proponents of the test ban." But because this information had not been released to the public, "the American public is allowed to retain the illusion that enforcement of test cessation is technically feasible."[139] Most Senators who had any pull on nuclear policy could expect to hear from Teller, though occasionally they sought him out. As noted above, when Orear testified to Humphrey's Committee on Foreign Relations, the Senator requested Teller's comments. Teller described Orear's testimony to Humphrey as "wild guesses," and also hoped for "an opportunity to counter Bethe's arguments."[140] Teller sent a long, detailed criticism of Orear's testimony to Humphrey on April 25, 1958, and also wrote to Symington, describing Orear's testimony as "the will-o'-the wisps of Dr. Orear's imagination."[141]

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[137] ET to Symington, April 25, 1958, Folder 2: Reading File, Jan.–May 1958, Box 420: Chronological Correspondence, 1956–58, ET Papers.
[138] ET to Thomas Lanphier Jr., May 7, 1959, Folder 2: Reading File, Jan.–May 1959, Box 423: Chronological Correspondence, 1959–1961, ET Papers.
[139] ET to Symington, June 5, 1959, Folder 3: Reading File, June–Sept. 1959, Box 423: Chronological Correspondence, 1959–1961, ET Papers.
[140] ET to Lewis Strauss, April 25, 1958, Folder 2: Reading File: Jan.–May 1958, Box 420: Chronological Correspondence, 1956–58, ET Papers.
[141] ET to Humphrey, April 25, 1958, Folder 2: Reading File: Jan.–May 1958, Box 420: Chronological Correspondence, 1956–58; ET to Symington, April 30, 1958, Folder 2: Reading File: Jan.–May 1958, Box 420: Chronological Correspondence, 1956–58, ET Papers.

Teller meanwhile grew closer to Republican figures, providing them with scientific information and ensuring that they took a hard nuclear line. Teller told William F. Buckley: "It is wonderful to know that there are people like you and publications like the National Review defending my viewpoint."[142] Teller also buttered up Republican Nelson Rockefeller during his run for governor of New York. "If it would be technically feasible I would almost be tempted to move to New York to deliver a vote on the right side," he flattered the candidate. In early 1960, Teller and Kissinger discussed Rockefeller's decision not to run for president, with Teller lamenting, "I am most unhappy about it."[143]

As a Republican, Teller hoped to impart to the party his sense of urgency on nuclear weapons. Critiquing a report of Kissinger's in 1957, Teller complained that the party was "underplaying the immediate danger which arises from the fact that the Russians have caught up with us in military strength and are moving at the present time at the rate undoubtedly much superior to our own rate of development" and that "we are obviously losing the arms race." He even warned Kissinger that the nation should prepare "our own people and our allies for the necessary use of nuclear weapons in limited engagements."[144]

Teller's views also had influence inside the AEC, and these views could sometimes make their way to the highest levels. At a meeting between Eisenhower and the NSC, Strauss drew upon Teller's arguments when he critiqued a disarmament proposal suggested by Harold Stassen, Eisenhower's disarmament advisor. Strauss vigorously denounced Stassen

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[142] ET to William F. Buckley Jr., May 9, 1958, Folder 2: Reading File: Jan.–May 1958, Box 420: Chronological Correspondence, 1956–58, ET Papers. On the rise of conservatives in the 1950s, see Sam Tanenhaus, *Whittaker Chambers: A Biography* (New York: Random House, 1997).
[143] ET to Nelson A. Rockefeller, July 5, 1958, Folder 1: Reading File, June–Oct. 1958, Box 420: Chronological Correspondence, 1956–58; ET to Kissinger, Jan. 11, 1960, Folder 5: Reading File, Jan.–April 1960, Box 423: Chronological Correspondence, 1959–61, ET Papers.
[144] ET to Kissinger, Sept. 18, 1957, Folder 2: Reading File, May–Dec. 1957, Box 421: Chronological Correspondence, 1956–58, ET Papers.

for lowering the required number of annual, on-site inspections in the Soviet Union, and mentioned that "both Dr. Teller and Dr. Lawrence felt that several score of inspection stations would be required to monitor testing in the Soviet Union, rather than the eight or twelve which Governor Stassen proposed as requisite to detect clandestine nuclear testing within the Soviet Union."[145]

While U.S. diplomats and scientists negotiated in Geneva, Teller wrote a letter to the AEC's GAC chairman Warren C. Johnson to poke holes in the ongoing test ban negotiations. First, he argued, the conference did not consider explosions under one kiloton, which could not be distinguished from earthquakes. Second, the proposed detection method was "dubious." Third, no detection proposal was made for shots more than 30 miles up in the atmosphere. Finally, the experts had not even considered testing in "interplanetary space," making "evasion indeed quite possible." Such an experiment would involve sending a weapon and an observation "object" not less than 10 million miles into space. Naturally, "the Russians could apply this method sooner than the other side."[146]

In a different letter, Teller offered to resign from the AEC's GAC because he felt his position as director of Livermore created a conflict of interest.[147](He later resigned his Livermore directorship as well, in order to "oppose without hindrance the test-ban treaty that was soon to be pursued by President John Kennedy," in historian Gregg Herken's words.[148]) Teller nevertheless often coordinated his work with the AEC. When writing *Our Nuclear Future* with Albert Latter, Teller had a draft of the manuscript vetted by AEC

[145] *FRUS*, *1958–1960*, Vol. III: 537. Not that Strauss was just Teller's mouthpiece. In fact, Greene, in *Eisenhower*, argues that early on, Strauss often had to push Teller to provide arguments against the test ban, 72, 97.
[146] ET to Johnson, Oct. 18, 1958, Folder 1: Reading File, June–Oct. 1958, Box 420: Chronological Correspondence, 1956–58, ET Papers.
[147] ET to Johnson, June 14, 1958, Folder 1: Reading File, June–Oct. 1958, Box 420: Chronological Correspondence, 1956–58, ET Papers.
[148] Herken, *Brotherhood of the Bomb*, 330.

commissioner Willard Libby, who advised the duo on fallout-related matters including "rain scavenging," how fallout "almost exclusively" comes down in rain, the calcium-to-radioactive-strontium ratio, the chemical similarity of active and nonactive strontium, "chemical aging," and the "plowing factor." Teller then instructed Latter to "be a good 'student' and read all of his [Libby's] papers, perhaps even read his testimony which is very long." Since Teller deemed Libby's comments "really justified," he considered thanking Libby in the book's acknowledgements.[149] Teller also continued to supply Strauss with ammunition to use against test ban advocates, advising the AEC chair that "a test moratorium would have disastrous consequences. The only military field in which we still possess superiority is that of nuclear explosives. A test moratorium would be a powerful tool in the hands of the Russians to deprive us of this last advantage."[150] Historian Benjamin P. Greene has convincingly shown that Strauss used such information to weaken Eisenhower's confidence in test ban verification methods and trust of the Soviet Union.[151]

Teller also attempted to shape Republicans' views on the militarization of space. Apparently in response to Kissinger's request, Teller elaborated on the possible military uses of satellites. He described "the possibility of using satellites as platforms from which to launch weapons," and was especially interested in how "satellites might conceivably be well adapted to shoot down other satellites."[152] Though nothing came of his proposals at the

[149] ET to Albert Latter, Aug. 28, 1957, Folder 2: Reading File, May–Dec. 1957, Box 421: Chronological Correspondence, 1956–58, ET Papers.
[150] ET to Strauss, March 20, 1958, Folder 2: Reading File, Jan–May 1958, Box 420: Chronological Correspondence, 1956–58, ET Papers.
[151] On the role of Strauss within the Eisenhower administration, see Greene, *Eisenhower*, 5, 24, 28–33, 39–49, 59–66, 69–82, 92–100, 112–33.
[152] ET to Kissinger, Jan. 11, 1960, Folder 5: Reading File, Jan.–April 1960, Box 423: Chronological Correspondence, 1959–61, ET Papers.

time, the concepts described in this exchange closely resemble the Strategic Defense Initiative and Brilliant Pebbles programs that Teller pushed for during the 1980s.

In 1963, Teller thanked Kissinger for setting up a meeting between him and Rockefeller, adding that "I hope to be helpful toward the nomination of Nelson" in 1964 as the Republican presidential candidate. Teller then ventured beyond science advice when he urged Kissinger to prod Rockefeller to attack the right wing of the Republican Party for its opposition to civil rights and for its isolationist foreign policy. The Kennedy administration, on the other hand, he denounced as "wrong in its policy of appeasement."[153] During congressional debate over the 1963 test ban, Teller sent Kissinger some statements against the treaty to pass on to Rockefeller.[154]

A loyal Republican by this time, Teller also reached out to Richard Nixon, supporting the former presidential candidate in his 1962 run for California governor, and pressing Kissinger to enlist Rockefeller's support for him as well.[155] After Nixon lost, Teller wrote to console him. "The 1962 campaign has not been in vain," he started. "It has certainly stiffened the Administration's attitude on Cuba and this was indeed badly needed. You and others who have been on the firing lines have rendered a real service which is not diminished by the fact that in a personal sense the situation turned out to your disadvantage."[156] In 1964, Teller was invited to serve as Republican presidential candidate Barry Goldwater's science advisor and found himself tempted to take the position despite

[153] ET to Kissinger, Sept. 6, 1963, Folder 2: Reading File, July–Sept. 1963, Box 425: Chronological Correspondence, 1962–66, ET Papers.
[154] ET to Kissinger, Aug. 26, 1963, Folder 2: Reading File, July–Sept. 1963, Box 425: Chronological Correspondence, 1962–66, ET Papers.
[155] ET to Kissinger, May 2, 1962, Folder 4: Reading File, April–June 1962, Box 426: Chronological Correspondence, 1962–66, ET Papers.
[156] ET to Nixon, Nov. 16, 1962, Folder 2: Reading File: Oct.–Dec. 1962, Box 426: Chronological Correspondence, 1962–66, ET Papers.

his previous alliance with Rockefeller. But with Rockefeller and Nixon out of the running for the 1964 nomination, he signed on with Goldwater after Rockefeller gave him his blessing.[157] Teller's position amounted to naught, however, after Lyndon Johnson easily trounced Goldwater in the general election.

Teller's status as an esteemed scientist also allowed him to participate in the distribution of awards representing the authority and imprimatur of the scientific and government establishment. Teller also tried to use his ability to influence awards to craft an image of himself as nobly atoning for his role in the Oppenheimer hearing. As a winner of the AEC's Enrico Fermi Award in 1962, Teller was invited to suggest nominees for the next recipient. In his memoirs, Teller explained his choice:

> I, like countless other scientists, was saddened by the onus that the security hearing had cast over Robert Oppenheimer, who had indeed provided unparalleled leadership at Los Alamos during the war years. I also hoped that government acknowledgement of Oppenheimer's great service would help to heal the schism that had developed in the scientific community. Therefore, I used my newly acquired position as nominator to submit Oppenheimer's name for the 1963 Fermi Prize, which, to my delight, he received that year. Unfortunately, the schism persisted without change.[158]

Oppenheimer and Teller biographers echo this explanation; Goodchild, for example, writes that Teller "was not alone in thinking of Robert Oppenheimer; but while others saw the award as a timely opportunity to rehabilitate him, after nine years of political exile following the security hearing, Teller also saw things in more personal terms. He hoped it would end

[157] ET to Rockefeller, Aug. 13, 1964, and ET to Kissinger, Aug. 17, 1964, Folder 5: Reading File, July–Sept. 1964, Box 425: Chronological Correspondence, 1962–66, ET Papers.
[158] Teller, *Memoirs*, 465.

his differences with Oppenheimer and lead to an easing of his own miseries of the past decade."[159]

Evidence from Teller's papers reveals a fairly different story. Teller in fact nominated three men for the 1963 prize: Oppenheimer, Hyman Rickover, and Leo Szilard. Teller may have even wanted Szilard to win the award more than Oppenheimer, since he had been trying to get the prize to Szilard since 1960. That year he wrote to Wigner:

> In talking with Szilard and also with Trude [Szilard's wife]I got the very definite impression that it would be of special value to Szilard to receive the Fermi prize. I do not believe that a financial problem exists. Szilard never wanted recognition. Right now he seems to want it and he appears to have his heart set on this special thing. I may be wrong, but this is my impression.[160]

Szilard, of course, was an eccentric and inspired arms control advocate, showing that Teller did not as a rule avoid or scorn those who held opposite political views. In addition, while Teller may have wished to appear as though he had influence over the distribution of awards, the reward system was too dispersed for him to control it. In fact, according to Oppenheimer biographers Kai Bird and Martin Sherwin, liberal establishment figures McGeorge Bundy and Arthur Schlesinger Jr. "were no doubt responsible" for the award, since many Democrats saw Oppenheimer as an unfairly martyred man. At the award ceremony, Teller and Oppenheimer genially shook hands, though Oppenheimer's wife stood "stonefaced" during this encounter.[161]

Teller took a more direct approach in his efforts to shape the education of scientists and engineers by helping establish a school of applied sciences affiliated with UC Davis and

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[159] See Goodchild, *Edward Teller*, 306, and David Cassidy, *J. Robert Oppenheimer and the American Century* (New York: Pi Press, 2005), 349. Bird and Sherwin, *American Prometheus*, 681FN, sound more skeptical.
[160] ET to Wigner, Jan. 19, 1960, Folder 5: Reading File, Jan.–April 1960, Box 423: Chronological Correspondence, 1959–1961; and ET to Manson Benedict, Feb. 18, 1963, Folder 3, Reading File, Jan–March 1963, Box 425: Chronological Correspondence, 1962–66, ET Papers. Rickover had helped develop the nuclear submarine. Szilard never won the award.
[161] Bird and Sherwin, *American Prometheus*, 574–76.

located on the grounds of the Livermore lab. In 1964 Teller helped create the Hertz Fellowship for graduate students in the applied sciences. Money for the fellowship came from the Hertz Foundation, and rewarded graduate students at MIT, University of Chicago, Caltech, and the new department of applied sciences at UC Davis-Livermore so that, in Teller's words, "we can stay in closer touch with them." At first, Teller personally interviewed each candidate, and continued to conduct interviews through the 1960s, 70s, and 80s.[162] He also helped select the recipients of the Hertz Prize, the first of which went in 1966 to the men who developed lasers. In 1977 the Hertz Building at Livermore was dedicated with an address by Teller's old friend Nelson Rockefeller. That same year, Greg Canavan won the second Hertz Prize for his "outstanding work on shielding missiles against laser radiation while he was in the air force." After winning the prize Canavan joined Los Alamos. Teller himself pointed out that of 1,200 Ph.D. scientists at Livermore, 35 of them were former Hertz Fellows. In his memoirs, however, Teller disputed the idea that Hertz fellows were only weapons workers; rather, he argued, they were bright scientists who worked in all sorts of settings.[163] He was undoubtedly correct about this, but in 1960, when Teller began to set up the school for applied science at Livermore, he directly linked its mission to national security. "If this plan should turn out to be feasible," he wrote to AEC chair John McCone, "it will be an important contribution to the continued vigor of the Livermore Laboratory."[164]

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[162] ET to John A. Wheeler, Jan. 21, 1964, and ET to Raymond Mar, Feb. 20, 1964, Folder 1: Reading File, Jan.–March 1964, Box 424: Chronological Correspondence, 1962–66, ET Papers.
[163] Teller, *Memoirs*, 485–91. The questions apparently did not have any connection to the military or national security.
[164] ET to McCone, March 8, 1960, Folder 5: Reading File, Jan.–April 1960, Box 423: Chronological Correspondence, 1959–1961, ET Papers.

While the state mobilized to control scientists (particularly during the Oppenheimer hearing), the same can be said about scientists during the Cold War, when too much was at stake for them to sit back and be neutral. Pauling's grassroots campaign, which imagined a consensus identity for scientists, mobilized many scientists and others outside of the government. But his movement failed to achieve disarmament in part because many scientists were divided over their identity. Teller's counterattack appealed to anticommunism and fell more in line with the demands of the national security state.

This choice of audience was crucial. While Pauling aimed at the vast constituency of some imagined scientific community, Teller aimed at the political and military elites most inclined to favor his views. In 1958, after Pauling and Teller participated in the KQED debate, the Air Force distributed a film of the debate to each of its air divisions. The film arrived accompanied by a memo explaining the Air Force's reasons for showing the debate. The memo's author acknowledged the Air Force's "great stake in Dr. Teller's position," but expressed the view that hearing both sides would help "all audiences in making up their minds." Each viewing was required to be open to the public, "must be shown in its entirety . . . [and] must be presented by a military man, in uniform, who makes the attached preamble and summing-up remarks, as indicated." This required preamble began with some mundane remarks, but quickly got to the point:

> We believe history has shown us the fallacy of entrusting that security entirely to the wishful thinker, the idealist, and the theorist until we have worked out international agreements which are as binding and just to all parties as the current threat of retaliatory power is arresting to a potential aggressor.

The preamble then justified the screening by stating that "There is nothing so helpful in making up your own mind about nuclear weapons and their bearing on the preservation of

peace as hearing from Dr. Teller and Dr. Pauling on the same program, airing both sides of the case."[165]

In case anyone in the audience still had not made up their mind in the right way, a set of "summing-up remarks," also required to be read aloud, followed the film. Though no names were used, the comments made a distinction between one of the scientists who logically "accepts the possibility of wars," while "the other, apparently, rejects it."

> One accepts the world as we know it to be and the demonstrated characteristics of people who live in it; the other believes we can leap whole and clean into a new order, and is so sure of it, he would abandon some of our security guarantees which in themselves pave the way to a better hearing before our possible enemies. One of these scientists worries about potential mutilations of the unborn, but ignores the $5,000,000,000 we spend annually on war veterans for their mutilations because we did not keep up our military power in relation to enemy efforts, which drew us into wars.

Still without naming Pauling, the comments asserted that a "good" scientist "does not discard those [facts] which only fail to appeal to him, or search only for the items which tend to prop up a pre-conceived notion or whim." Then came an analogy that might have made Teller envious:

> Not long ago people avoided eating a vegetable called the "love apple" because it was said flatly to be poison. Today, we eat it all the time and know it as the common tomato. Somebody tested and proved the poison claim false. . . . Yet in this debate, we have one side pleading for cessation of testing, testing which could lead to possible discoveries of staggering import beneficial to the human race. This is paying homage to panic rather than progress, which is hardly scientific.

After comparing nuclear weapons to tomatoes, the remarks went on to say that Teller, on the other hand, "makes the point that you should think out carefully what has been said, and

[165] July 15, 1958, Memo from Barney Oldfield and attached preamble and summing-up remarks, Folder: USAF Re Pauling Teller Debate, Box 457: Topic file/ Alphabetical file, ET Papers, emphasis in original.

shown here, and we, in the military service, subscribe absolutely to this." It went on: "We are still trying to arrive at answers, and we may live better by what we find from continued testing, live safer by the protections these findings give us, or we might die miserably from surrendering search and initiative to our possible enemies."[166]

This document shows how Pauling's use of science proved, in one sense, self-defeating. While science gave Pauling credibility, it also left him open to scientific counter attacks, from the Air Force's cartoonish analogies to Teller's relatively more sophisticated discussion of radiation effects. And many scientists rejected Pauling's claims because as scientists, they did not have all the scientific evidence their professional standards demanded.

Pauling's campaign shows how the deck was stacked against an outsider: the armed forces naturally embraced Teller's message, while government officials in Congress and the State Department dismissed Pauling as a communist. Teller had the benefit of being an insider, with all the attendant credibility and connections. Because the state was ready to believe Teller, Teller could contradict Pauling with science and the imprimatur of classified sources. Pauling could answer Teller's claims, though he could not back it up with official information. Teller could claim access to all sorts of classified information, from the labs to the AEC; Pauling, meanwhile, got his data from the *New York Times*.

As the conflict between Teller and Pauling shows, scientists, politicians, and the military have long sought to use "science" to support a specific point of view. Scientists were not just the victims of a Cold War consensus. Instead, they actively struggled over the meaning of science. Teller's role was to support U.S. nuclear policy, and cast doubt on those

[166] July 15, 1958, Memo from Barney Oldfield and attached preamble and summing-up remarks, Folder: USAF Re Pauling Teller Debate, Box 457: Topic file/ Alphabetical file, ET Papers, emphasis in original.

scientists who opposed it. The debate over the nuclear test ban treaty, discussed in the following chapter, brought this conflict into public view.

Chapter Four: "Crucified on a Cross of Atoms": Scientists, Politics, and the Test Ban Treaty, 1957–1963

The Significance of the Test Ban

Diplomatic historians of the Cold War never fail to place great importance on the 1963 Limited Test Ban Treaty; many scholars see the treaty as a catalyst for the transition from aggressive confrontation to détente between the United States and Soviet Union.[1] But the test ban's significance extends beyond even this momentous shift. The test ban hinged on scientific expertise, and allowed scientists in favor of disarmament an opportunity to influence both U.S. and British policy. These efforts did not go unchallenged, and so the debate over the test ban reveals the contested role of science in the making of foreign policy, as well as how the efforts of prodisarmament scientists were neutralized. Moral arguments for a test ban, such as Pauling's, were left outside the boundaries of debate, while government science advisors argued over the test ban in terms of national security. But technical arguments in favor of a test ban were countered by other scientists' equally technical arguments against a test ban.

The test ban debate took place in an era when the public and the government looked to scientists for truth and guidance. Surprisingly, only a handful of works explicitly study the role of scientists in the test ban debate and the resulting treaty. The earliest of these works examines scientists who participated in the official Geneva test ban negotiations, arguing that scientists made for ineffective diplomats as the result of their inability to distinguish

[1] See for example John Lewis Gaddis, *We Now Know: Rethinking Cold War History* (New York: Oxford University Press, 1997), and Marc Trachtenberg, *A Constructed Peace: The Making of the European Settlement, 1945–1963* (Princeton: Princeton University Press, 1999).

technical issues from political questions, as well as having received poor guidance from the Eisenhower administration.[2] In 1981 Glenn Seaborg published a memoir of the test ban as seen from his position as head of the Atomic Energy Commission (AEC) during the John F. Kennedy administration. But although Seaborg was a scientist, his account is not actually about scientists; as he makes clear, his interest is in highlighting Kennedy's role in the test ban, and, to a lesser extent, Khrushchev's.[3]

Other historians who have looked at scientists and the test ban debate, most recently Benjamin Greene, have approached the subject as a means with which to analyze and critique the concept of Eisenhower revisionism.[4] But the issues at stake in the test ban transcend the scholarly boundaries of presidential decision-making assessment. The test ban era was a time when individual activists and international movements endeavored to influence U.S. foreign policy. In contrast to previous works, this chapter and those that precede it have expanded the boundaries of test ban scholarship to encompass the full range of scientists' efforts for a test ban activism, including government science advisors, transnational scientific activists, test ban opponents, and scientists outside policymaking circles. In addition, this analysis looks at Congress as a target of those scientists who hoped

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[2] Harold Karan Jacobson and Eric Stein, *Diplomats, Scientists, and Politicians: The United States and the Nuclear Test Ban Negotiations* (Ann Arbor: University of Michigan Press, 1966).

[3] Seaborg's emphasis is evident in his title: Glenn T. Seaborg, with Benjamin S. Loeb, *Kennedy, Khrushchev, and the Test Ban* (Berkeley, 1981). See also xiii.

[4] Benjamin P. Greene's recent *Eisenhower, Science Advice, and the Nuclear Test-Ban Debate, 1945–1963,* (Stanford: Stanford University Press, 2007) discusses scientists but only in the context of an analysis of Eisenhower's leadership style. Robert Divine, in *Blowing on the Wind: The Nuclear Test Ban Debate, 1954–1960* (New York: Oxford University Press, 1978), examines scientists' participation in the public test ban debate. His insightful volume was written before access to Kennedy archives was available, however, and thus covers only the Eisenhower years. In addition, Martha Smith-Norris, "The Eisenhower Administration and the Nuclear Test Ban Talks, 1958–1960: Another Challenge to 'Revisionism,'" *Diplomatic History* 27 (September 2003): 503-41; Jeremi Suri, "America's Search for a Technological Solution to the Arms Race: The Surprise Attack Conference of 1958 and a Challenge for 'Eisenhower Revisionists,'" *Diplomatic History* 21 (Summer 1997): 417–51; and H.W. Brands, "The Age of Vulnerability: Eisenhower and the National Insecurity State," *American Historical Review* 94 (October 1989): 963–89, all speak to the debate over Eisenhower's "hidden hand" leadership style first put forth by Fred Greenstein. See Stephen G. Rabe, "Eisenhower Revisionism: A Decade of Scholarship," *Diplomatic History* 17 (Winter 1993): 109–13, for an analysis.

to influence nuclear weapons policy, as well as a site where scientists and politicians debated the proper role of scientists in politics.[5] Full understanding of the test ban demands an exploration of the range of conflicts between scientists, the state, and society, including scientists who advocated a test ban but did not participate in governmental science advising. Accordingly, the following chapter shows how conflicting ideas about the relationship between science and politics affected the ultimate shape of the test ban treaty.

In the late 1950s and early 1960s, prodisarmament scientists combined a rejuvenated activism with newfound access to the White House, as both Eisenhower and Kennedy made a point of maintaining the cabinet-level position of presidential science advisor. In this chapter I argue that, although restricted in their arguments, pro-test ban scientists in the United States helped achieve their goal of a test ban largely because of their technical expertise. This test ban, however, was not the comprehensive prohibition of nuclear tests that most scientists had hoped for. The Limited Test Ban Treaty that emerged after a grueling six-year debate reflected scientists' efforts, yet was also truly "limited" in every sense of the word.[6] The differences between a comprehensive ban, which might have put a brake on the arms race, and the limited ban that actually emerged, which only moved the arms race underground, show how the very public technical debates in which scientists engaged

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[5] Historians have approached the test ban from many different perspectives but none have covered the entire range of scientific activism involved. The scientists involved in the test ban point to the treaty as evidence of their influence on foreign affairs, although most scholarly accounts from over the years have not supported this interpretation. Divine, *Blowing on the Wind,* and Spencer R. Weart, *Nuclear Fear: A History of Images* (Cambridge: Harvard University Press, 1988), state that scientists essentially abandoned their objectivity in order to influence the test ban debate. In *Eisenhower,* Greene argues that scientists had influence because Eisenhower's test ban decisions changed as he began to listen to different science advisors. Greene's otherwise solid account does not closely examine the broader scientific environment in which the test ban debate took place, focusing largely on those scientists with access to the Eisenhower administration. Although Greene has convincingly argued that Eisenhower based his test ban decisions on the advice of his science advisors, the broader context of advice-giving (as distinct from the specific technical content of advice) has not been rigorously analyzed. For example, Gregg Herken, *Cardinal Choices: Presidential Science Advising from the Atomic Bomb to SDI* (Stanford: Stanford University Press, 2000) examines only scientists within the government.

[6] In *Kennedy, Khrushchev, and the Test Ban,* Seaborg makes clear his and other scientists' deep regret that the test ban was not comprehensive, xiv, 242, and Chapter Twenty-Two, 293–99.

directly—and unintentionally—resulted in the test ban being a limited one. This mild version of success, then, came at a great price: the very nature of their influence had opened scientists up to challenge. Technical arguments for arms control were countered by other scientists, while the transnational basis of the movement was easy to negate by questioning Soviet credibility. Furthermore, the debate exacerbated existing divisions in the scientific community and severely damaged scientists' status as authoritative and objective experts in society. Famous scientists split over the test ban issue and publicly confronted each other in congressional hearings. This conflict reflected deep political divisions in the scientific community, as well as the gap between scientists' personal ambitions and their status as objective authorities. Society relied heavily on scientists for objective truths, but government officials expected science to offer a consensus view on nuclear weapons rather than the continual argument, counterargument, and refinement that characterizes the scientific discipline. Simply put, at the beginning of the test ban debate, the government turned to scientists for answers; by the end of the debate, the government (and the public, to a certain extent) had begun to look elsewhere for answers to the dilemmas of the nuclear age.

Different Paths to Protest

While Linus Pauling campaigned for a test ban from outside the government, two other groups of elite scientists took a more conventional approach and tried to influence U.S. policy from within. These scientists, many of them unconcerned with the fallout threat, believed that a test ban would help slow the arms race and prevent a thermonuclear war. The first of these ventures consisted of transnational activism that promised to use scientists' elite status to bring about disarmament.

On April 13, 1955, philosopher Bertrand Russell wrote to Albert Einstein just days before the legendary physicist's death. Russell insisted on showing Einstein an urgent manifesto he had composed that called attention to the increased menace of thermonuclear weapons. Einstein, in what was apparently one of his last acts on Earth, added his name to the document. The Russell-Einstein manifesto implored scientists to take action to end the arms race. "Scientists should assemble," the manifesto read, "not as members of this or that nation, continent, or creed, but as human beings, members of the species Man, whose continued existence is in doubt."[7] Two years later, twenty-two scientists from both sides of the Iron Curtain heeded Russell and Einstein's call and met in Pugwash, Nova Scotia, to discuss disarmament. The Pugwash movement became a leading transnational organization of scientists, mobilizing international connections and technical expertise and bringing together politically prominent Soviet, British, and U.S. scientists to discuss urgent issues in an unofficial atmosphere.[8] Some of Pugwash's key members included the British physicist Joseph Rotblat, the only scientist to quit the Manhattan Project for moral reasons, and Eugene Rabinowitch, a Manhattan Project veteran and editor of the *Bulletin of the Atomic Scientists*. Soviet scientist Lev Artsimovich, a Pugwash participant, declared "Diplomacy is an antiquated vehicle. Our role is to remove obstacles from the path of this antique chariot."[9] While official U.S.-Soviet relations remained fraught with tension in the late 1950s over incidents in the Suez, Hungary, Berlin, and even outer space, the Pugwash conferences

[7] Joseph Rotblat, *Pugwash—the First Ten Years: History of the Conferences of Science and World Affairs* (New York: Humanities Press, 1968), 77; Lawrence Wittner, *The Struggle Against the Bomb: Vol. 2. Resisting the Bomb: A History of the World Nuclear Disarmament Movement, 1954–1970* (Stanford: Stanford University Press, 1997), 6.
[8] See Wittner, *Resisting the Bomb*, 292–96. Matthew Evangelista, in *Unarmed Forces: The Transnational Movement to End the Cold War* (Ithaca: Cornell University Press, 1999), examines Pugwash's influence on the Soviet Union. Scientists' enthusiasm for Pugwash and the Russell-Einstein manifesto can most likely be explained by a combination of admiration for Einstein, genuine concern for nuclear war, and the implicit flattery in a manifesto that called upon them to save the world.
[9] "Private Meeting in London to Discuss the Nuclear Test Ban Deadlock," March 16–17, 1963, part 1, 1, Box 35: Correspondence, 1961–1965, Folder: Nuclear Test Ban, HUG (FP)–94.8, George B. Kistiakowsky Papers, Harvard University (hereafter cited as GBK Papers).

helped develop trust across the Iron Curtain, bypassing suspicious officials and connecting scientists who shared common understandings.

Although inspired by the moral urgency of the Russell-Einstein manifesto, Pugwash scientists decided at the outset that their influence on disarmament would be technical—specifically by providing scientific analysis of inspection and verification systems for a nuclear test ban treaty. As early as their second meeting in 1958, Pugwash scientists discussed technical aspects of inspection under a test ban, and the fourth conference in 1959 aimed to jumpstart the stalled official test ban negotiations underway in Geneva.[10] Pugwash scientists felt comfortable working on a test ban because the Russell-Einstein manifesto encouraged international agreements. "Any agreement between East and West is to the good in so far as it tends to diminish tension," Russell had written.[11] But the elusive test ban proved to be a fateful choice since, in order to come to fruition, any treaty would require Senatorial approval in the United States, opening the agreement up to political—as well as scientific—debate.

Pugwash activism occurred alongside a parallel effort by government scientists. After *Sputnik*, the Eisenhower administration began to incorporate scientists into the presidential cabinet. Scientists suddenly found themselves with access to the highest echelons of U.S. foreign policymaking, while policymakers were again concerned with the opinions of the country's most esteemed scientists.[12] The nature of protest had changed since the mid 1940s, however. When scientists in the late 1950s mobilized to take advantage of this new access, they no longer carried with them the heroic aura of atomic wizards. Instead, a new

[10] Rotblat, *Pugwash,* 19, 23.
[11] Rotblat, *Pugwash,* 78.
[12] Wittner, *Resisting,* 33–39; Divine, *Blowing,* 171.

generation of scientists had entered the labs—a type of organization men, as characterized by one historian of science.[13] In the late 1950s, scientists brought technical expertise and transnational connections to the test ban debate. But the Oppenheimer hearing had demonstrated that if government scientists opposed nuclear weapons, they had to make the case that neither arms control nor disarmament would harm the nation's offensive capabilities. These government scientists who favored arms control could not help but be very aware of how Oppenheimer had been punished for framing his opposition to the Super in moral terms.[14] At the very moment that nuclear weapons increased dramatically in destructive power, government scientists' vocabulary of dissent became much smaller. Despite these limitations, a number of scientists energetically took to advising the U.S. government on nuclear weapons policy in the late 1950s. Though fear of thermonuclear war spurred them, the Eisenhower administration's newfound sentiments in favor of scientists and a test ban encouraged these scientists to hope that they might influence U.S. nuclear policy.[15]

The Eisenhower administration's relationship with scientists had gotten off to a rocky start, and this uneasiness persisted through the President's first term. Eisenhower's tacit approval of the Oppenheimer hearing had offended many scientists, especially since Lewis Strauss, a friend of the President, chaired the AEC from 1953 to 1958 and had

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[13] Cassidy, *Oppenheimer,* 330.

[14] Cassidy, *Oppenheimer,* 323, 335; and Thorpe, *Oppenheimer,* 229–30. Not that scientists' moral arguments had been particularly effective. Oppenheimer's General Advisory Committee for the AEC advised against pursuing aggressive development of the Super; although the GAC believed that H-bomb development would hinder the fission weapons program, they also cited the H-bomb's potential for "genocide." See "USAEC General Advisory Committee Report on the 'Super,' October 30, 1949," in Robert C. Williams and Philip L. Cantelon, eds., *The American Atom: A Documentary History of Nuclear Policies from the Discovery of Fission to the Present, 1939–1984* (Philadelphia: University of Pennsylvania Press, 1984), 126. Even James Conant declared that the H-bomb would be built "over my dead body," Richard Polenberg, ed., *In the Matter of J. Robert Oppenheimer: The Security Clearance Hearing* (Ithaca: Cornell University Press, 2002), 258. Despite these heartfelt expressions of moral qualms, President Truman nevertheless ordered a crash program to build the Super.

[15] The emphasis in this chapter is on U.S. and British policy. Evangelista, *Unarmed Forces,* examines Pugwash's influence with the Soviet government.

colluded with the FBI to ensure Oppenheimer's humiliation.[16] Later in his administration, Eisenhower would express a commitment to a nuclear test ban, but demonstrated ignorance, disdain, or a dark sense of humor when he retained Strauss as his primary atomic energy advisor.[17]

Judging by the distinct pronuclear slant of Eisenhower's first term, Strauss did seem an appropriate nuclear advisor. The New Look, Massive Retaliation, and brinkmanship strategies all relied on the nuclear arsenal as the basis of U.S. military and diplomatic policy. As late as January 1956, UN Representative Henry Cabot Lodge and Secretary of State John Foster Dulles linked U.S. nuclear weapons to world peace, telling the president that "The greatest single factor in the world today for peace is our atomic superiority."[18]

International events and public opinion eventually convinced the Eisenhower administration of the wisdom of pursuing a test ban.[19] Although government officials doubted the dangers of fallout, many expressed hope that a test ban would lead to more concrete disarmament efforts. The menacing bleeps of *Sputnik* in 1957 initially spurred fears of Soviet ICBMs sporting thermonuclear warheads, but scientists, grassroots activists, and Eisenhower himself all hoped to turn this threat into an opportunity by increasing their efforts for a test ban. Scientists' status as the essential guardians of nuclear knowledge meant that when nuclear weapons were involved, government officials still instinctively turned to

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[16] See Priscilla McMillan, *The Ruin of J. Robert Oppenheimer and the Birth of the Modern Arms Race* (New York: Viking, 2005), for an incisive and powerful indictment of Strauss and the AEC's cutthroat tactics.
[17] Greene, *Eisenhower,* 24. Greene argues that Eisenhower made a test ban one of his goals earlier than previously recognized. Eisenhower was so out of touch with scientists that, just after the Oppenheimer decision, he wrote Strauss "Why do we not get Dr. Oppenheimer interested in desalting sea water? I can think of no scientific success of all time that would equal this in its boon to mankind." Eisenhower to Strauss, June 16, 1954, Ann Whitman File, Administration Series, Box 4, Atomic Energy Commission, 1953–54 (4), Dwight D. Eisenhower Library, (hereafter DDEL).
[18] U.S. State Department, *Foreign Relations of the United States, 1955–1957* (Washington, D.C.: U.S. Government Printing Office, 1990), Vol. XX: 273.
[19] *FRUS, 1955–1957*, Vol. XX: 419–21. Wittner, *Resisting the Bomb*, cites a January 1958 public opinion poll showing 49 percent of Americans supported a test ban, with 36 percent opposed, 59.

them for advice.[20] When Eisenhower reached out to scientists beyond those vetted by Strauss and the AEC, he found them surprisingly eager to rejoin the ranks of government. In late 1957, partly in response to *Sputnik,* Eisenhower created the President's Science Advisory Committee (PSAC), and appointed MIT president James Killian as his personal science advisor.[21] As one PSAC member put it, "Eisenhower now had a group of scientists who had a loyalty only to him." Hans Bethe, an early member of PSAC, excitedly wrote to Rabinowitch at the *Bulletin of the Atomic Scientists*: "we now have a mechanism to get the ideas of scientists directly to the government."[22]

The PSAC instantly put science in the service of the President. Initially, PSAC advised Eisenhower on space exploration and defense matters. Although the test ban debate increasingly intruded on his work, Killian remained reluctant to enter the disarmament field. But "inevitably," he wrote, PSAC was "drawn into the debate about nuclear tests and their detection."[23] The debate over a test ban hinged not on the moral implications of a thermonuclear war, however, but on the narrow technical issues of nuclear test detection. Specifically, the enforcement of a comprehensive test ban relied on the ability to detect nuclear explosions hidden underground. Thus at a 1958 NSC meeting, Killian was told that "The President expressed the hope that we could advance rapidly in our discovery of

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[20] For example, in 1955 government leaders in the United States and Britain suggested "that a committee of scientists be appointed to study the matter of inspection and control of nuclear weapons" to assist Eisenhower's 1955 Open Skies proposal. *FRUS, 1955–1957*, Vol. XX: 201.
[21] Killian's official title was Special Assistant for Science and Technology. See James R. Killian Jr., *Sputnik, Scientists, and Eisenhower: A Memoir of the First Special Assistant to the President for Science and Technology* (Cambridge: Harvard University Press, 1977).
[22] Walter A. Rosenblith, ed., *Jerry Wiesner: Scientist, Statesman, Humanist: Memories and Memoirs* (Cambridge: MIT Press, 2003), 257; Bethe to Rabinowitch, Feb. 12, 1958, Box 8, Folder 16, Eugene I. Rabinowitch Papers, University of Chicago (hereafter ER Papers).
[23] Killian, *Sputnik*, 150.

detection devices."[24] Meanwhile, the Soviet Union announced a unilateral test moratorium in March of that year. Eisenhower, emboldened by world opinion against testing as well as the counsel of PSAC and Dulles, but also eager to neutralize the Soviet propaganda advantage from their moratorium, subsequently announced that the United States would begin a unilateral nuclear test suspension on October 31, 1958. The Soviet Union, claiming the right to catch up to Western advantages attained during their own moratorium, actually exploded a few nuclear weapons just before starting another moratorium on November 4.[25] When nuclear testing finally paused, Eisenhower faced continuous pressure from the AEC and the Department of Defense (DoD) to resume tests while Killian and the PSAC raced to hammer out a viable detection system.

Killian and his successor George Kistiakowsky provided the respected pro-test ban views of scientists to counter test ban opponents in the AEC and DoD. Bethe recalled, "Eisenhower had confidence in Killian and later in Kistiakowsky."[26] The science advisory committee had the president's confidence, but advisors did not argue against the H-bomb as a possible "weapon of genocide" as the previous generation of science advisors had done. Government scientists could not oppose nuclear weapons on the grounds of the harm the weapons might do to others; instead, they had to prove that arms control measures would not harm the United States. The restriction to technical advice was often made explicit; in PSAC's reports, Killian wrote that the scientists "limited themselves to technical factors."[27]

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[24] U.S. State Department, *Foreign Relations of the United States, 1958–1960* (Washington, D.C.: U.S. Government Printing Office, 1996), Vol. III: 544.
[25] Greene, *Eisenhower,* 134–64. Jacobson and Stein, *Diplomats, Scientists, and Politicians,* 119.
[26] See Greene, *Eisenhower*, for a discussion of the gradual process that broadened the spectrum of Eisenhower's science advice, especially Chapter Seven, 134–64; Hans Bethe Oral History, Nov. 3, 1977, 22, DDEL.
[27] Killian to Eisenhower, April 3, 1958, White House Office of the Special Assistant for Science and Technology (James R. Killian and George B. Kistiakowsky): Records, 1957–1961, Alphabetical File, Disarmament—Nuclear [March–April 1958] (1) Box 1, DDEL.

During a meeting with Eisenhower, Killian demonstrated that science advisors had adopted the limitations mandated by the Oppenheimer hearing by emphasizing "this [test ban] is a controversial subject on which the observations of his group are limited to technical aspects only and must of course be balanced against other considerations." At an NSC meeting, after Bethe reported on detection, Eisenhower questioned whether the Soviets could disguise tests as earthquakes. According to the memorandum of discussion, "Dr. Bethe replied that we really knew too little about this problem to provide the President with a clear and categorical answer." After Eisenhower twice insisted, Bethe offered the president a 90% certainty.[28] Bethe's reluctance to put forward a clear and unambiguous answer reflects the very nature of the scientific method, in which rigorous experimentation and careful analysis precedes any conclusion. As Greene has shown, the President genuinely wanted a test ban and his science advisors tried to provide him with one.[29] But restricted to technical interpretations, Eisenhower's advisors could only give him the rough estimates of an ongoing scientific analysis, not moral boldness. Purely scientific advice thus provided no quick or easy answers to the dilemmas of the nuclear age.

As U.S. science advisors limited themselves to technical arguments, Pugwash scientists attempted to use PSAC as a conduit to influence U.S. nuclear weapons policy. Thus they too adopted technical arguments, despite the moral urgency of the Russell-Einstein Manifesto that had inspired their movement. Sources indicate that Pugwash had access to PSAC in the late 1950s, but the group's influence on policy appears only

[28] *FRUS, 1958–1960*, Vol. III: 585–86, 603.
[29] Greene, *Eisenhower,* 2–3. Greene's assertion that Eisenhower wanted a test ban counters previous assessments by Smith-Norris, "The Eisenhower Administration," Jacobson and Stein, *Diplomats, Scientists, and Politicians*, and, to an extent, Divine, *Blowing*. These earlier works give the president poor marks in pursuing a test ban and are considered counter-revisionist, in contrast to the Eisenhower revisionism of Stephen Ambrose, *Eisenhower, Vol. II: The President* (New York: Simon and Schuster, 1984). Greene in effect splits the difference in *Eisenhower*, arguing that Eisenhower indeed wanted a test ban, but his consensus-based leadership style hampered his efforts.

sporadically. Jerome Wiesner, a member of PSAC, went to a Pugwash meeting in 1958 and on his return immediately reported to PSAC and the President. Wiesner's Pugwash experience reaffirmed his belief—and helped him convince PSAC—that a test ban would not risk the nation's security. Wiesner tellingly wrote "I thought my best arguments were technical, that no amount of Soviet testing would reduce the value of the American weapons."[30] For the most part, however, the Pugwash movement had a hard time getting through to the administration. In late March 1960, Eugene Rabinowitch visited Kistiakowsky "with a hard sell of the Pugwash conferences." Kistiakowsky told him "I am not at all sure about the value of these conferences." Six weeks later Wiesner also urged Kistiakowsky to support Pugwash, but "I reserved judgment," he noted.[31] Nevertheless, the organization alerted its contacts within the U.S. administration to the genuine desire among Soviet scientists for a test ban, as well as the fact that the Soviet government was divided over the issue. Later in 1958, U.S. scientist and Pugwash participant Victor Weisskopf wrote to Killian conveying information from the third Pugwash conference, including the Soviet delegation's assertion that they were "very much devoted to the cause of the atom bomb test stop," and "that there is a bitter fight within the USSR government between the test supporters and the test stoppers, and that the stoppers have a[t] present a slim lead which might break at any time."[32]

Pugwash and the British Government, 1957–1958

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[30] Rosenblith, *Wiesner,* 263.
[31] George B. Kistiakowsky, *A Scientist at the White House: The Private Diary of President Eisenhower's Special Assistant for Science and Technology* (Cambridge: Harvard University Press, 1976), 292, 322.
[32] Weisskopf to Killian, Sept. 27, 1958, White House Office of the Special Assistant for Science and Technology (James R. Killian and George B. Kistiakowsky): Records, 1957–1961, Alphabetical File, Nuclear Test Policy [May 1958–October 1960] (1) Box 8, DDEL.

The U.S. government was not Pugwash's only target; British scientists attempted to push their leaders toward a test ban as well. Their initial efforts received only skepticism from British government officials, who later became determined to control Pugwash if they could not thwart it. Officials in the Foreign Office who discussed Pugwash in 1957 were convinced that communists would exploit the conference for propaganda purposes. One analyst saw two options for the government: "treat such a Conference as unnecessary . . . and as something with which we should have nothing to do, or . . . accept that it is likely to take place and try and inject scientists into it who would take a sensible line." A second official agreed that "we should try and influence the conference if we cannot prevent it taking place."[33] In late 1957 a Foreign Office memo repeated the view that the conferences would merely serve as vehicles of communist propaganda and could undermine Western defense policies. The author added that he had been "disturbed" by the Canadian government's support of Pugwash.[34]

The following year, the British government was more open to the upcoming conference, perhaps because Rotblat, the head of British Pugwash, had consulted the British Atomic Energy Authority (AEA) on which scientists should attend.[35] The Foreign Office picked up on this discussion of possible British delegates but wished to follow the lead of the U.S. government. "We should be interested to learn as soon as possible what line they [U.S. officials] are taking and what Americans are likely to be going to this further Pugwash,"

[33] "Reports on a statement signed by 196 Soviet scientists supporting a suggestion made at the Pugwash Conference in July, 1957 that a conference of scientists be called to consider the perils of atomic energy," FO 371 129250, Z E112/ 333 UK National Archives.
[34] H.C. Hainworth to J.B. Johnston, Dec. 3, 1957, FO 371/ 129250; Z E112/ 333: Nuclear Tests and Radiation Hazards, 1957, UK National Archives.
[35] J.D. Cockcroft to W. Strath, Feb. 10, 1958, FO 371/ 135525, Z E112/ 35, UK National Archives.

a Foreign Office representative wrote.[36] A British diplomat at the embassy in Washington reported back to the Foreign Office that the U.S. government shared the British view that "if one [a second Pugwash] is to be held, some of those who participate in it ought to be adequately briefed to argue the Western viewpoint." The scientist Charles Darwin (a Pugwash delegate and relation to the author of *The Origin of Species*) later informed the Foreign Office about the agenda for the second Pugwash and received a briefing on British policy.[37] As the second conference approached, some British government officials remained wary. The Canadian government had changed their way of thinking and came to agree that the Western governments should treat the conferences with "skepticism," but although Canadian officials agreed that it would be desirable "if there were in attendance some scientists who are favourable to Western policy and who have been briefed upon it," they doubted whether they could so easily influence the Canadian delegation.[38]

Following the 1958 conference, the Foreign Office was satisfied that their gentle interference had paid off. After Darwin sent papers to D.S. Cape at the Foreign Office, Cape wrote that "If these articles give an accurate picture, the discussions at the Conference were quite satisfactory from our point of view . . . . Our briefing of Sir Charles Darwin was not therefore wasted."[39] But as Pugwash scientists looked forward to the third conference in 1958 in Austria, the British government again grew apprehensive. An officer at the British embassy in Vienna took the initiative of discussing ways to combat "Communist influence at

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[36] D.S. Cape to Foreign Office, Feb 24, 1958, FO 371/ 135525, Z E112/ 35, UK National Archives.
[37] J.C.A Roper to H.C. Hainworth, Feb. 27, 1958; Foreign Office Memo, March 11, 1958, FO 371/ 135527; Z E112/ 58: UK Policy on Tests of Nuclear Weapons, 1958, UK National Archives.
[38] Telegram to Commonwealth Relations Office, March 5, 1958, FO 371/ 135527; Z E112/ 66: UK Policy on Tests of Nuclear Weapons, 1958, UK National Archives.
[39] D.S. Cape, "Second Pugwash Conference," May 1, 1958, FO 371/ 135530, Z E112/ 137, UK National Archives; C.G. Darwin to D.S. Cape, undated, and D.S. Cape, "2nd Pugwash Conference," Minutes, May 18, 1958, FO 371/ 135530, Z E112/ 146, UK National Archives.

the Conference" with a Professor Karlik at the Austrian Institute for Radium Research. Karlik promised to do what she could to see that "level-headed scientists" participated, while members of the Foreign Office tried to coordinate efforts with their U.S. counterparts. But a suggestion that the State Department brief U.S. Pugwash scientists was rejected, in part because Eugene Rabinowitch, the head of U.S. Pugwash, was "quite unbriefable." A Foreign Office memo subsequently expressed disappointment at the "rather too complacent" U.S. attitude, since members of the Foreign Office suspected that Soviet scientists would stage an attempt at the conference to form a worldwide organization of prodisarmament scientists funded by the communist World Federation of Scientific Workers. British officials were thus anxious to find Western scientists who would thwart this move. Again privy to the workings of the British Pugwash delegation, the Foreign Office was pleased that "Sir George Thomson has agreed to go so that the left wing will not have it all their own way."[40] Officials in the AEA also considered it "desirable" to have "reliable" scientists at the Austria conference, and suggested to Darwin that British Pugwash delegates should consider a government briefing. When Darwin mentioned this suggestion to Rotblat, he was not completely averse to the idea.[41] Meanwhile, U.S. officials had finally agreed to coordinate Pugwash policy with Britain. The Foreign Office received a memo from William Henson, an atomic energy advisor with the U.S. Information Agency (USIA), that listed probable U.S. Pugwash delegates and labeled them according to their political beliefs, such as "liberal leftish" (Harrison Brown), "not left wing but very independent" (Herman Muller), or

[40] R. Hanbury-Tenison to H.C. Hainworth, May 16, 1958; P.L. Carter to H.C. Hainworth, May 28, 1958; D.S. Cape to P.L. Carter, July 2, 1958, FO 371/ 135532; Z E112/ 161: UK Policy on Tests of Nuclear Weapons, 1958, UK National Archives.
[41] Minutes, 1; Darwin to D.S. Cape, June 16, 1958, FO 371/ 135532; Z E112/ 174: UK Policy on Tests of Nuclear Weapons, 1958, UK National Archives.

"politics unknown" (Leo Szilard). Eventually the USIA also adopted the British practice of offering a government briefing to Pugwash delegates.[42]

Upon returning from the Austria conference, Thomson reported back on his attempts to influence the proceedings. "It is difficult to persuade a meeting to committ [sic] suicide," Thomson wrote, "and I did not try very hard." Without going into detail, he passed on the information that two "conservative" U.S. representatives would be chosen by Alvin Weinberg, Frederick Seitz, and Harrison Brown for the Pugwash Continuing Committee.[43] Overall, the British government was pleased with the result of its efforts. R.A. Burroughs wrote that "our worst fears were not realized," and expressed satisfaction that the U.S. delegates had disliked the "wickedness" of the Soviet delegation, while a Pugwash press conference had been "innocuous" and failed to impress journalists. Nor had anyone attempted to organize a communist-backed propaganda organization. One official credited the Foreign Office's steps to influence the conference, including the briefing of scientists, with causing the positive outcome.[44]

As the U.S., British, and Soviet governments took tentative steps toward a test ban in Geneva, public pressure increased. Much of the international sentiment in favor of a test ban consisted of opposition to radioactive fallout. Instead of concentrating on fallout, however, U.S. government and Pugwash scientists followed three arguments for a comprehensive test ban: a test ban would maintain U.S. nuclear superiority; most tests could be detected; and the Soviets could be counted on to adhere to the treaty. None of these three arguments

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[42] P.L. Carter to D.S. Cape, Aug. 15, 1958, FO 371/ 135533; Z E112/ 193: UK Policy on Tests of Nuclear Weapons, 1958; D.S. Cape to P.L. Carter, Oct. 15, 1958, FO 371/ 135533; Z E112/ 217: UK Policy on Tests of Nuclear Weapons, 1958, UK National Archives.
[43] Thomson to Miss Brown, Sept. 20, 1958, FO 371/ 135533; Z E112/ 215: UK Policy on Tests of Nuclear Weapons, 1958, UK National Archives.
[44] R.A. Burroughs to D.S. Cape, Sept. 9, 1956 and D.S. Cape to P.L. Carter, Oct. 15, 1958, FO 371/ 135533; Z E112/ 217: UK Policy on Tests of Nuclear Weapons, 1958, UK National Archives.

contained the moral sentiment implicit in the fallout issue. But prodisarmament scientists saw their claims and influence contested nevertheless. This challenge directly threatened a comprehensive test ban, which depended on the U.S. government's confidence in scientists' claims about detection reliability. Arms control opponents began to challenge the scientific basis of a test ban by using science to challenge science, mirroring the traditional manner in which unresolved scientific debates progress. Kistiakowsky recalled that "there was a continuous running battle" between PSAC and the AEC, and the DoD also opposed a test ban.[45]

Teller Mobilizes against a Test Ban

By far, however, Edward Teller took the form of the primary nemesis of the test ban. In Kistiakowsky's words, he was "the most dangerous scientist in the U.S."[46] Teller, an esteemed physicist in his own right as well as an ardent anticommunist, provided scientific ammunition for test ban opponents and tirelessly waged his own war against the ban to protect the nuclear weapons establishment around which he had constructed his professional career.

Teller naturally opposed a test ban on scientific grounds. Although he was not the only scientist opposed to a test ban, he was by far the most effective. He often dismissed fears of fallout, attempting to make increased radiation seem normal. Teller declared that living in a brick house—a perfectly safe choice—exposed a person to more radiation than fallout from a nuclear test. Relying on his own scientific expertise and credibility, Teller claimed that "our knowledge is sufficient to state that the fallout effect is below the statistically observable

[45] George B. Kistiakowsky Oral History, Nov. 3, 1977, 16, DDEL.
[46] Kistiakowsky, *Scientist,* 228.

limit. It is also considerably less than the effect produced by moving from sea level to an elevated location like Denver, where cosmic radiation has a greater intensity. It is also less than having a chest x-ray every year." Never one to shy away from bold statements, Teller even suggested that radiation might hold some benefits. "A living being is a most complex thing," he wrote. "Damage to a small fraction of the cells might be beneficial to the whole organism. Some experiments on mice seem to show that exposure to a little radiation increases the life expectancy of the animals." Most effectively, he played on U.S. fears of the Soviets by telling Congress about methods the Soviets might use to cheat a test ban. "We already see strong possibilities, effective possibilities of hiding tests," he menacingly claimed.[47] In March 1960 Kistiakowsky spoke of the United States and Soviet Union when he told PSAC that the "real stumbling block is the mutual distrust of the two sides,"[48] though the same could be said about pro-test ban scientists and Teller. Teller's prophecies of Soviet evasions undermined the trust between U.S. and Soviet scientists that had been built up by Pugwash as well as official Geneva conferences on the test ban, and drove the superpowers further apart, all in the name of further testing, anticommunism, and an increased arms race.

Teller often opposed a test ban on scientific grounds by arguing that the Soviets could merely move testing underground, where it would be nearly impossible for U.S. seismologists to distinguish low-yield nuclear explosions from earthquakes. But Teller also made it his mission to devise more theoretical ways in which the Soviets could evade a test

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[47] U.S. Senate, Subcommittee on Disarmament, Committee on Foreign Relations, *Controlling the Further Development of Nuclear Weapons: A Collection of Excerpts and a Bibliography* 85th Cong., 2nd sess. (Washington, D.C.: U.S. Government Printing Office, 1958), 29, 39–40.

[48] "Meeting on Arms Limitations," 2, March 1960, U.S. President's Science Advisory Committee: Records 1957–1961, Box 2, DDEL.

ban. By far his biggest success was with the decoupling theory that hypothesized that the Soviets could muffle explosions in extremely large, deep holes (called Latter Holes after the scientist who conceived of them, Albert Latter, a close colleague of Teller's). Teller's motives can hardly be called genuine; when Latter won the AEC's Ernest O. Lawrence award in 1964 (which apparently came with a cash prize), Teller wrote to congratulate him, and chuckled, "Would it be proper to say that you found a pound of gold in the 'big hole'?"[49] Tactics like this frustrated proponents of a test ban on both sides of the Iron Curtain. Soviet Pugwash scientist J. Riznechenko later regretted how the "U.S. delegation dragged in the big hole to disrupt the [Geneva] conference."[50] At a meeting in England, Killian recalled: "Dr. Latter said to me in casual conversation that whatever advances might be made in detection technology, the West Coast group led by Teller would find a technical way to circumvent or discredit them."[51]

All of Teller's machinations made concrete progress on arms control more difficult to achieve. His claims increased pressure from Congress and the Pentagon against a comprehensive test ban, where the technical basis of test detection was vulnerable to a scientific onslaught. Furthermore, the inability of the United States to be consistent about verification confused the Soviet Union.[52] Eisenhower, predisposed not to trust the Soviets, vacillated as well. In a June 1957 meeting with Eisenhower, Teller and his UC Berkeley colleagues Ernest Lawrence and Mark Mills impressed the President with visions of clean nuclear weapons that would produce no fallout, as well as promises that Plowshare would

[49] Teller to Albert Latter, March 20, 1964, Folder 1: Reading File, Jan-March 1964, Box 424: Chronological Correspondence, 1962-66, ET Papers.
[50] "Private Meeting," part 2, 14, Folder: Nuclear Test Ban, Box 35: Correspondence, 1961–1965, HUG (FP) 94.8, GBK Papers. See also: Kistiakowsky to Thomas Morgan, Aug. 25, 1961, Folder: ACDA (Arms Control and Disarmament Agency), 1961–64, Box 31: Correspondence, 1961–1965, HUG (FP) 94.8, GBK Papers.
[51] Killian, *Sputnik*, 168.
[52] Kistiakowsky, *Scientist,* xlvii, 49, 73, 198.

effortlessly unearth natural resources. Eisenhower, according to the conference memorandum, responded that "no one could oppose the development program they had described. We are, however, up against an extremely difficult world opinion situation and he did not think that the United States could permit itself to be 'crucified on a cross of atoms,' so to speak." Teller responded by bringing the scientific reliability of verification into question, and voiced "a great deal of concern as to the situation that might exist if the Soviets secretly were to continue testing and developing . . . bombs and peaceful uses of . . . atomic explosions while we, having stopped our tests, are left only with . . . weapons which we are inhibited through world opinion from using."[53]

Eisenhower eventually began to resist Teller and the AEC to some extent.[54] In 1957, Eisenhower wrote to Strauss: "Because I did have in Dr. Lawrence, Dr. Teller and Dr. Mills, I rather think it might be a good idea for me to pick two or three scientists who represent a contrary view and ask them in for a visit." Strauss advised the President to resist the urge.[55] Nevertheless, Eisenhower sought out his physicist friend and test ban advocate I.I. Rabi, who gave him advice that conflicted completely with Teller's. In an October 1957 diary entry, Eisenhower wrote, "the Rabi Committee believes that with a half dozen or so properly equipped inspectional posts inside of Russia, any significant explosion could be detected."

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[53] *FRUS, 1955–1957*, Vol. XX: 638–40, elided text in original. Eisenhower questioned Teller if fallout was harmless. The physicist replied, "this is essentially correct, although there may some miniscule effects—extremely low in relation, for example, to the difference in radioactive exposure of people at sea level as against people at the elevation of Denver, Colorado." The meeting with Teller, Mills, and Lawrence changed some minds in the Eisenhower administration. An NSC memo described the scientists' comments as "very disturbing." The memo continued: "Their judgment that no policing of nuclear tests can be relied upon must be accepted. The risks involved in illegal and undetected Soviet testing were considered by the scientists to be very serious. The scientists obviously would wish to continue testing for professional reasons. In addition, there may be an unconscious desire to reduce the horror of nuclear weapons which they are responsible in large part for creating," *FRUS, 1955–1957*, Vol. XX: 642.

[54] See Greene, *Eisenhower,* for an extensive analysis of Eisenhower's genuine desire for a test ban. Greene's interpretation agrees with that offered by Eisenhower biographer Stephen Ambrose, and breaks from scholars including Smith-Norris who question the seriousness with which the Eisenhower administration pursued at test ban. See Greene's introduction, 1–8, for an extended discussion.

[55] Eisenhower to Strauss, July 2, 1957, and Strauss to Eisenhower, July 3, 1957, Ann Whitman File, Administrative Series, Box 5, Atomic Energy Commission, 1957 (2), DDEL. See also Hans Bethe Oral History, Nov. 3, 1977, 8, DDEL.

His entry then revealed that he had begun to grasp the notion that scientists could dislike each other. "Incidentally," he wrote, "I learned that some of the mutual antagonisms among the scientists are so bitter as to make their working together almost an impossibility. I was told that Dr. Rabi and some of his group are so antagonistic to Drs. Lawrence and Teller that communication between them is practically nil."[56] Eisenhower later told Killian straightforwardly that "he had never been too much impressed, or completely convinced by the views expressed by Drs. Teller, Lawrence and Mills that we must continue testing of nuclear weapons."[57]

But as the Geneva talks went on, Teller continued to do what he could to upset them. Representing the AEC, Teller attended a meeting with the State Department and Killian's office where he quickly poked holes in the ongoing negotiations. The memorandum of conversation recorded Teller's argument: "Dr. Teller said he felt [that] thought should be given to the wisdom of prohibiting events which cannot be detected by a control system." In particular, Teller pointed out many "loopholes," including that explosions below one kiloton would be indistinguishable from earthquakes, and that no method had been discussed for detecting nuclear tests at extremely high altitudes or in outer space.[58] Meanwhile, Strauss continued to play upon Eisenhower's suspicions of the Soviets, telling him to "refus[e] to be victimized by Russian duplicity."[59]

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[56] *FRUS, 1955–1957*, Vol. XX: 754–55; Hans Bethe Oral History, Nov. 3, 1977, 8, DDEL. Bethe noted how Eisenhower was friends with I.I. Rabi since his Columbia days. Rabi once called Teller "an enemy of humanity," quoted in Goodchild, *Teller,* 282.
[57] *FRUS, 1958–1960,* Vol. III: 604.
[58] Ibid., 662–65.
[59] *FRUS, 1955–1957*, Vol. XX: 478–79, 754–55.

Such pressure worked; Eisenhower could not bring himself to ignore Teller's and Strauss's claims.[60] Even more damaging to the test ban, Eisenhower felt obligated to share these doubts with the Soviets. Wiesner wrote that Teller's visions of clean bombs convinced Eisenhower in 1960 to tell the Soviets that the United States could end the test moratorium at any time. Pugwash scientists attempted to negate Teller's efforts at their annual conferences. In selecting scientists to invite, Bernard Feld, a U.S. Pugwash member, suggested one scientist for his "unofficial, Anti-Teller, more 'radical' technical view."[61] Nevertheless, Teller's doubts had turned several of Eisenhower's advisors against a test ban, and Eisenhower himself worried about trusting the Soviets. Without the scientific unanimity of his experts, Eisenhower did not push as hard as he could have for a test ban. Feld later lamented that the test ban "opposition . . . was more effective than the test ban proponents."[62]

The superpowers came tantalizingly close to a test ban treaty, albeit an atmospheric one. What had begun as tentative talks had resulted in scientific conferences, moratoria, and, by 1960, an agreement in principle to ban all tests in the atmosphere as well as those below ground that registered over 4.75 on the Richter scale. This proposed threshold ban was

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[60] Rosenblith, *Wiesner,* 441; Eisenhower's vacillation over a test ban, Greene argues in *Eisenhower*, complicates the notion of Eisenhower's "hidden hand" leadership style. Though Eisenhower genuinely desired a test ban, his reliance on the counsel of Strauss reflects a weak leadership style. But, Greene continues, once Eisenhower opened himself up to a greater range of science advice, the president assertively pursued a test ban. By taking a less Eisenhower-centric view, this paper seeks to look beyond the issue of Eisenhower's leadership style. In fact, the president's decision-making was restricted by many factors, including the state of the NATO alliance, the machinations of scientists, the feelings of his cabinet, and his own distrust of the Soviet Union.

[61] Rosenblith, *Wiesner,* 409; Feld to Rabinowitch, March 17, 1960, Box 30: Committee on Pugwash Conferences on Science and World Affairs (P-COSWA), Folder 304: 1959–1965, Bernard T. Feld Papers, Massachusetts Institute of Technology (hereafter cited as BTF Papers).

[62] Feld, unpublished autobiography, "VIII: Nuclear Politics in the U.S.A." Doc. 11, March 8, 1988, Box 65: Writings, Folder unnumbered: Autobiographical, BTF Papers; Hans Bethe Oral History, Nov. 3, 1977, 18, DDEL. Greene, *Eisenhower,* argues that Eisenhower "showed understandable confusion with the complex technical issues" of a test ban and was thus dependent on his science advisors, including Strauss as well as Rabi and Bethe, 3. A different interpretation might suggest, however, that Eisenhower struggled to reconcile the conflicting advice he received, rather than failed to understand the science behind a test ban.

actually a way to sidestep the technical disagreements over inspection and detection that confounded a comprehensive ban. Most government scientists and officials in the United States, United Kingdom, and the Soviet Union nevertheless endorsed the threshold ban, until the U-2 affair in May 1960 caused Khrushchev to withdraw from test ban talks.[63] His test ban aspirations thwarted by Teller at home and fateful events abroad, the President turned bitter. His frustration bubbled over in his famous farewell address, when, after warning about the military-industrial complex, he said: "Yet, in holding scientific research and discovery in respect, as we should, we must also be alert to the equal and opposite danger that public policy could itself become the captive of a scientific-technological elite." With the U.S. scientific community divided over a test ban, the President's ambiguous words accurately reflected the state of U.S. science.[64]

The day after the address, Kistiakowsky approached Eisenhower at a farewell party and asked him to elaborate on his farewell comments. In a memo sent to PSAC members, Kistiakowsky recounted the conversation. The President had expressed to Kistiakowsky "concern and forebodings" over the state of U.S. science. Especially worrisome to Eisenhower were colleges and universities, institutions that, while supposedly dedicated to "free intellectual inquiry . . . were influencing research people on their staff to abandon basic science for the sake of higher monetary rewards." Eisenhower also expressed a fear that scientists were "being hoarded by the military-industrial combine." Kistiakowsky noted in

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[63] See Chapter Seven of Jacobson and Stein, *Diplomats, Scientists, and Politicians,* 231–61.
[64] "Farewell Radio and Television Address to the American People, January 17, 1961," *Public Papers of the Presidents of the United States, Dwight D. Eisenhower, 1960–61* (Washington, D.C.: U.S. Government Printing Office, 1961), 1035–40. An online transcript is available at John T. Woolley and Gerhard Peters, *The American Presidency Project* [online]. Santa Barbara, CA: University of California (hosted), Gerhard Peters (database). Available from World Wide Web: http://www.presidency.ucsb.edu/ws/?pid=12086. Killian, *Sputnik,* 224.

his memo that Eisenhower's sentiments resembled those of many PSAC members.[65] Years later, PSAC member Herbert York asked Eisenhower who had inspired these words. Eisenhower, in York's words, "answered without hesitation: . . . 'Teller.'"[66]

Scientists and the Limited Test Ban Treaty

The debate over a nuclear test ban did not end with the Eisenhower administration, though historians have often treated it that way.[67] In September 1961 the Soviet Union ended its voluntary nuclear test moratorium with a series of high-yield nuclear explosions; the United States responded two weeks later by ending its own moratorium. The resumption of tests, however, only served to reincarnate the test ban debate for the Kennedy administration. The clash of scientists that had occurred under Eisenhower had failed to quell the greater public's belief that science would provide needed truths about nuclear weapons. Ruth Adams, the managing editor of the *Bulletin of the Atomic Scientists*, wrote to Bernard Feld, saying that after the Soviet Union resumed nuclear tests, "our mail has tripled from readers seeking advice and what they call nonpartisan facts."[68] Sharing the era's faith in science, Kennedy relied, as Eisenhower had, on science advisors for test ban advice. Even before his inauguration, Kennedy sent his security advisor Walt Rostow, along with Jerome Wiesner, his choice for science advisor, to Moscow for a Pugwash meeting in late 1960. An early Pugwash veteran,

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[65] Kistiakowsky, Memo to Members, PSAC, Jan. 19, 1961, Box 5: PSAC, All Correspondence, 1968–1969, Folder: PSAC, 1968, HUG (FP) 94.14, GBK Papers.
[66] Herbert F. York, *Arms and the Physicists* (Woodbury: American Institute of Physics, 1995), 147. Eisenhower also mentioned Wernher von Braun, the U.S. missile scientist. Greene, however, claims that no evidence concretely links the farewell address to Teller, *Eisenhower,* 232.
[67] Such is one consequence of looking at the test ban in the context of Eisenhower decision-making. In *Blowing on the Wind,* Divine restricted his analysis to the Eisenhower years as many sources had not yet been declassified; Greene, in *Eisenhower,* only briefly discusses the test ban debate during the Kennedy years. Seaborg, *Kennedy, Khrushchev, and the Test Ban,* on the other hand, covers the Kennedy years with only a cursory summary of the test ban debate in the Eisenhower administration. Jacobson and Stein, *Diplomats, Scientists, and Politicians,* is more inclusive.
[68] Ruth Adams to Feld, Nov. 8, 1961, Box 5: Arms Control, Folder 42: 1960–61, BTF Papers.

Wiesner allowed Pugwash the opportunity to play a more prominent role in U.S. disarmament policy. With the scientists' movement well represented, the Kennedy administration signed a test ban treaty in August 1963. But the test ban opposition again mobilized, and used science to turn the test ban from what many considered a first step toward disarmament into an "arms control disaster."[69]

Much like Kistiakowsky had for Eisenhower, Wiesner acted as Kennedy's counter to the anti-test ban hawks. According to one Kennedy administration official, Wiesner "was a driving force to do something about the test ban negotiations."[70] With a friend of Pugwash as the President's science advisor, the organization's views could finally be brought to bear directly on U.S. policy. After attending the 1960 Moscow Pugwash meeting, Rostow and Wiesner reported directly to the State Department about Soviet willingness to work with Kennedy for a test ban. But with Wiesner's direct access to the White House, Pugwash's influence appears to have gone even further. Wiesner's informal contacts with the Soviets provided him a great deal of otherwise inaccessible information. Wiesner himself wrote in a 1987 letter: "I developed very good relationships and a good rapport with Soviet scientists [at Pugwash meetings] . . . . While working as science advisor to President Kennedy, these contacts were very important to me in my effort to achieve a nuclear test-ban."[71]

Kennedy and Wiesner also appeared to share a moral concern about nuclear weapons. One rainy day, the two sat in the Oval Office, when Kennedy asked Wiesner to explain how fallout returned to earth. "And I told him," said Wiesner, "that it was washed out of the clouds by the rain, that it would be brought to earth by rain, and he said, looking

[69] Feld, unpublished autobiography, "VIII: Nuclear Politics in the U.S.A.," Doc. 11, March 8, 1988, Box 65: Writings, Folder unnumbered: Autobiographical, BTF Papers.
[70] Interview with Carl Kaysen, MIT, March 16, 2005.
[71] Rosenblith, *Wiesner,* xiii–xiv, 40, 509.

out the window, 'You mean, it's in the rain out there?'—and I said, 'Yes'; and he looked out the window, looked very sad, and didn't say a word for several minutes."[72]

Despite the apparently moral concerns about fallout shared by Wiesner and the president, reasons for the test ban remained purely technical. One Wiesner letter to Kennedy reviewed the need for atmospheric testing, and justified a test ban in national security terms: "my assessment of the proposed tests leads me to the conclusion that, while these tests would certainly contribute to our military strength, they are not critical or even very important to our over-all military posture. I believe that our most important task is to maintain an extremely effective deterrent and that we have the capability to do so without conducting atmospheric tests." Instead of atmospheric testing, he suggested "aggressive use of underground testing, imaginative laboratory work, and possibly even space testing if it should prove necessary."[73] Wiesner thus gave Kennedy what was essentially advice on military capabilities—a far cry from standing by the window and worrying about rain. Though technical views were a necessity in such a debate, scientists could hope for at best limited arms control measures, since deterrence and the build-up of weapons could not be questioned.

Still, Pugwash could have some influence. When Khrushchev wrote to Kennedy in 1962 about a test ban treaty, he mentioned the scientists' movement by name, praising Pugwash's suggestion of using "black boxes" to monitor seismic activities in order to eliminate the need for manned observation within the Soviet Union. Kennedy, however, failed to see Pugwash scientists in the same light as Khrushchev. "None [of the scientists]

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[72] Theodore C. Sorensen, *Kennedy* (New York: Harper and Row, 1965), 621–22; Rosenblith, *Wiesner,* 58; Kaysen interview.
[73] U.S. State Department, *Foreign Relations of the United States, 1961–1963* (Washington, D.C.: U.S. Government Printing Office, 1995), Vol. VII: 268, 271.

represented the United States Government or had discussed the matter with responsible officials," Kennedy replied to Khrushchev. "All were speaking as individuals and none were seismologists. Their agreement does not signify anything other than that this area was an area which justified further study."[74]

Meanwhile, the test ban negotiations at Geneva continued to idle. Pugwash held an elite meeting in 1963 in London specifically to discuss "the nuclear test ban deadlock." The meeting included Eisenhower's former science advisor Kistiakowsky, who had at one time doubted the value of the conferences. Kistiakowsky had remained on PSAC after the change in administration but had joined Pugwash when he grew frustrated with "the ineffectiveness of diplomatic efforts in the field of disarmament."[75] The Pugwash scientists discussed the need to convince people in the United States and Soviet Union about the uselessness of further testing, as well as the potential problem of the U.S. Senate's ratification of a treaty. The scientists also shared a great deal of information about methods of detection and inspection, including the results of various seismic experiments and the number of inspections each side desired. Lev Artsimovich, who had regular contact with Khrushchev, told Kistiakowsky that the Soviets would accept five annual inspections even after the United States had suggested just three. This information was passed on to Kennedy administration officials, though with no discernable result. Perhaps more significantly, the exchange foreshadowed the upcoming battle in the United States over the test ban, which emphasized suspicions of the Soviet Union's motives. At one point Soviet Pugwash scientist

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[74] *FRUS, 1961–1963*, Vol. VII: 576–77, 585, 623–24. This suggests support for several historians' claims that Soviet scientists had more influence on Soviet leaders because in an unfree society, groups with access to leaders have less interference to overcome. See Evangelista, *Unarmed Forces,* and Kai-Henrik Barth, "Catalysts of Change: Scientists as Transnational Arms Control Advocates in the 1980s," in John Krige and Kai-Henrik Barth, eds., *Global Power Knowledge: Science and Technology in International Affairs. Osiris,* vol. 21, 2006.
[75] Kistiakowsky, *Scientist*, 292.

J. Riznechenko said to the U.S. scientists: "We are not working on anti-spoofing [counter-intelligence]. Why are American seismologists and those close to them work[ing] so much on spoofing? . . . Latter and Teller for instance. Why are US [scientists] suspecting USSR seismologists? We are honest!"[76]

Pugwash and the British Government, 1960–1963

The 1960 Pugwash conference in Moscow proved a turning point in official British opinion about the organization. With Kennedy's personal science advisor Jerome Wiesner at the conference, British government observers liked what they saw. Reports from the conference sent to Atomic Energy and Disarmament officials also convinced British officials that the Soviets were "not simply using the subject as a propaganda trap for the West."[77] When one representative of the Atomic Energy Authority received an inquiry from a British foundation about providing monetary support for the conferences, he replied that before the 1960 conference, he would have adamantly recommended against it. But because of the "very powerful and politically uncommitted American delegation" that was at the 1960 Moscow conference, Pugwash merited support. The conferences, A.D. Wilson gushed, provided "a valuable means of non-official contact with the Russians, and of making ourselves acquainted with their thinking without committing ourselves too far."

But esteem for Pugwash depended on progress at the official test ban talks in Geneva. When the Geneva talks faltered in June, the British government refused to consider supporting Pugwash financially, since any chances of progressing on a test ban at Pugwash

[76] Kistiakowsky, *Scientist*, 423–24; "Private Meeting," part 1, 14, Folder: Nuclear Test Ban, Box 35: Correspondence, 1961–1965, HUG (FP) 94.8, GBK Papers.
[77] "Pugwash Conferences," FO 371/ 149403; IA D41/ 221: 6th International Pugwash Conference, 1960, UK National Archives.

"were likely to be small." But because some members of the Foreign Office believed that the Soviets would try and make an issue of the Geneva deadlock at Pugwash, it was even "more desirable than before" that the British Pugwash delegation have scientists of the "highest calibre" who adamantly supported the West's nuclear policies.[78]

The British government still found Pugwash useful—for example, in 1961 the Foreign Office was pleased to receive details about Soviet fears of German nuclearization. P. de Zuleta wrote to Prime Minister MacMillan about "one or two fascinating speculations" raised in a Pugwash report from the 1960 Moscow conference. "I wonder if this Soviet fear of Germany could not be used to overcome the possible danger that if we help the French with their nuclear the Russians will feel compelled to help the Chinese," de Zuleta wrote, offering that "I have a feeling that there may be elements of an arrangement here." The Prime Minister, apparently having looked at the Pugwash papers himself, concurred. "I rather agree with you that Soviet fear of Germany and the spread of nuclear weapons might come in useful in the enterprise on which we are now engaged," he replied.[79] Of course, using information from Pugwash conferences to aid nuclear proliferation was exactly the opposite type of influence the scientists had hoped to achieve.

In 1962 Pugwash continued to stand in high esteem in British policy circles. A Foreign Office memo stated that the organization was taken "quite seriously" and was "very respectable." British officials enjoyed dealing with Rotblat after learning that he was not the radical they had feared he was, and they even expressed interest in hosting a Pugwash

[78] A.D. Wilson to Harold Redman, Feb. 10, 1961, and H.S. Stephensen to John Cockcroft, June 27, 1961, FO 1110/ 1476; PR 1411/ 12: General, 1961–1962, UK National Archives.
[79] Note from P. de Zuleta, Jan. 19, 1961, 73–74, and note from the Prime Minister, Jan. 24, 1961, 72, PREM 11 5124, UK National Archives.

conference in England.[80] When that conference took place in Cambridge in 1962, Atomic Energy and Disarmament officials kept in close contact with the British scientists there, and the Nobel Prize-winning physicist John Cockcroft, a part-time consultant for the AEA, even presented a paper there.[81]

The British government found Pugwash so useful that diplomatic officers helped Pugwash scientists and Geneva negotiators exchange papers during test ban negotiations. A note that preceded Pugwash papers sent to British diplomats at Geneva read: "A report of the unofficial Pugwash meeting will reach you in the next bag. The main interest from our point of view is that the Russian scientists admitted that there would remain a number of seismic events which could not be identified without inspection."[82] Scientist Edward Bullard telephoned the Foreign Office from the Cambridge conference to report that "the Soviet scientists attending the Pugwash Conference there had confidently expected their government would accept the new draft treaty on atmospheric tests."[83] But while Pugwash scientists cooperated with governments in hopes of achieving a test ban, the scientific nemesis of arms control advocates tirelessly worked to undermine these efforts.

Teller's Test Ban Assault

After Kennedy's election, Teller continued his assault on a test ban begun under the Eisenhower administration. Wiesner wrote to Kennedy in October 1962 to notify him that a

[80] FO 371/ 163160 IA D49/ 1: 9th Pugwash Conference on Science and World Affairs, 1962; B.T. Price to D.M. Cleary, Jan. 26, 1962, FO 371/ 163160 IA D49/ 2: 9th Pugwash Conference on Science and World Affairs, 1962, UK National Archives.
[81] Minutes, FO 371/ 163161 IA D49/ 21: 9th Pugwash Conference on Science and World Affairs, 1962; minutes, FO 371/ 163161 IA D49/ 22: 9th Pugwash Conference on Science and World Affairs, 1962, UK National Archives.
[82] FO 371 163162, IA D49/44, 1962, UK National Archives; Telegram to Atomic Energy and Disarmament Department, April 3, 1963, FO 371 IA D1092/ 23G, UK National Archives.
[83] Lord Norwich to Mr. Pemberton-Pigott (Disarmament Department), undated, FO 371 IA D49/28, UK National Archives.

group chaired by Bethe had determined that the United States had "significantly over-estimated Soviet capabilities," referring to nuclear weapon capabilities as evidenced by a 1961–62 Soviet nuclear test series. "These conclusions," Wiesner continued, "which have been agreed to by senior scientists from the Livermore and Los Alamos Laboratories and the RAND Corporation are considerably at odds with the views of Dr. Teller who . . . has undertaken a program of public education on the status of U.S. and Soviet weapon capabilities. He has access to all classified weapons data, presumably the analysis on which this report was based." Teller had also told a reporter, "It is my guess, and it is only a guess, that the Russians are ahead of us in the nuclear race today."[84]

While scientific debate continued, events outside the sphere of science drove the United States and Soviet Union toward a test ban. In particular, the near-disaster of the Cuban Missile Crisis spurred mutual desires on both sides of the Iron Curtain for an arms control agreement.[85] Yet this newfound urgency resulted in an atmospheric, not a comprehensive, test ban. The scientific approach to the test ban debate directly affected this shift from a comprehensive ban to a limited one, primarily because of Teller's ability to provide Congress with technical claims that a comprehensive test ban was scientifically shaky and that the Soviets could not be trusted.[86] In August 1963, the United States and Soviet Union signed the Limited Test Ban Treaty (LTBT) in Moscow. As the conference began, the Soviets made it clear that they were not willing to discuss a comprehensive test ban.[87] The treaty that was signed prohibited testing in the atmosphere, outer space, and underwater,

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[84] *FRUS, 1961–63*, Vol. VII: 584.
[85] See, for example, Seaborg, *Kennedy, Khrushchev, and the Test Ban,* 172–76, and Gerard DeGroot, *The Bomb: A Life* (Cambridge: Harvard University Press, 2004), 264–65.
[86] Seaborg's discussion of Teller and Congress supports this interpretation, *Kennedy, Khrushchev, and the Test Ban,* 227, 242. See also Jacobson and Stein, *Diplomats, Scientists, and Politicians,* 437, 445, 448.
[87] See for example Seaborg, *Kennedy, Khrushchev, and the Test Ban,* 241; Jacobson and Stein, *Diplomats, Scientists, and Politicians,* 455; and Greene *Eisenhower,* 233–53; Kaysen interview.

moving tests underground. Pro-test ban scientists still hoped the treaty might lead to future disarmament and a slowdown in the arms race. But the treaty had to leap one final hurdle—the U.S. Senate, where the test ban's opponents would make their final stand.

The test ban eventually passed, but to the chagrin of many scientists who wanted genuine arms control, Senate ratification required the U.S. government to pursue an increased weapons development program. This hitch negated the arms control aspects of the treaty, leaving scientists to praise only the ban's reduction of fallout, even though many of them had originally doubted the dangers of fallout. The treaty's final form upset the many scientists who had hoped it would temper the arms race. Three factors worked against the pro-test ban scientists in the Senate: scientific opposition to the treaty, nationalist suspicions of the Soviet Union, and scientific divisions that harmed scientists' reputation as objective authorities. In July, the Kennedy brain trust held a meeting to discuss the "pros and cons of the test ban" after Averill Harriman had worked out an atmospheric agreement in Moscow. A memo drafted by CIA chief John McCone argued that "we must recognize that the Chiefs and, to a lesser extent the laboratory directors and such men as Edward Teller, would protest the treaty, [and] that I did not think the protest would be particularly well received by either the public or the Congress."[88] As the following section shows, McCone's prediction about Congress proved drastically off base.

As he had during the Eisenhower administration, Teller flexed Herculean might in opposing the Kennedy administration's test ban. As the hearings began, Teller declared to his frequent correspondent Henry Kissinger: "I hope to amend or to defeat the test ban

[88] *FRUS, 1961–1963*, Vol. VII: 821.

treaty."[89] At the start of his campaign, Teller continued to voice his distrust of the Soviets, and argued that only a strong nuclear deterrent would stop them.

In 1961 Teller had written to Glenn Seaborg, chairman of the AEC, essentially previewing the arguments he would later use against a test ban. Most importantly, Teller emphasized, "there is in my opinion serious reason to believe that the Russians are ahead of us." Teller then wrote, "The argument that secret testing can proceed even during such a treaty has been made most explicitly. In its main points this argument remains valid. Only atmospheric tests can be policed." If a treaty is signed, "we must count on continued Russian secret tests for at least a number of years."[90] Teller also worried—with reason—that a test ban would thwart his dream of Project Plowshare. At a meeting of the principal test ban officials, discussion turned to the consequences of a test ban on Teller's pet project. McCone declared that "we must choose between a test ban or our Plowshare program and that we cannot have both." The conversation record notes that "This was agreed by most present although Seaborg took some exception."[91]

Teller had, in fact, turned his attention to the Senate long before a treaty had been signed. Teller maintained consistent correspondence with the Senators who would likely decide the fate of a test ban, including those on the Committee on Foreign Relations and the Preparedness Subcommittee of the Senate Committee on Armed Services. In the summer of 1962, Teller visited Democratic Senator Stuart Symington (MO) of the Preparedness Subcommittee in Washington, and Symington took him to see his fellow subcommittee

[89] ET to Kissinger, Sept. 6, 1963, Folder 2: Reading File, July–Sept. 1963, Box 425: Chronological Correspondence, 1962–1966, ET Papers.
[90] ET to Seaborg, Nov. 2, 1961, Folder 6: Reading File, Oct.–Dec. 1961, Box 422: Chronological Correspondence, 1959–1961, ET Papers.
[91] *FRUS, 1961–1963*, Vol. VII: 680-81.

member Democrat John Stennis (MS). After the visit, Symington asked Teller to compose "a statement on the need for continued testing."[92] A short time later, Teller wrote to Stennis to tell him "I stand ready at any time to appear before your committee and make the required statements on the need for further nuclear testing." He then listed for Stennis people who "may be of assistance to you in organizing hearings on this question," several of whom came from Livermore Laboratory.[93] In a typical letter, Teller told Symington: "Even with further improvements our present proposals commit us to the acceptance of a system whereby cheating under twenty kilotons will be possible at some expense and where cheating under two kilotons will be certainly easy and simple."[94] When the hearings finally began, Teller wrote to Senator Henry Jackson (D-WA), who also served on the Preparedness Subcommittee, telling him of the "possibilities" and "also extremely serious difficulties" in improving U.S. defense under the test ban.[95]

As Teller lobbied behind the scenes, he also played a public role in the hearings. Although he was adept at the use of media and other outlets, his ability to nearly dominate the hearings—even taking into account the eventual passing of the LTBT—demonstrated most clearly the great effects of his rhetorical and demagogic skills, as well as the prodisarmament scientists' logical but fateful decision to seek disarmament through a treaty.

Whereas science advisors had influenced Eisenhower and Kennedy, Teller had a comparable influence on certain members of Congress and the military. The Senate

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[92] ET to Symington, Aug. 8, 1962, Folder 3: Reading File, July–Sept. 1962, Box 426: Chronological Correspondence, 1962–66, ET Papers.
[93] ET to Stennis, Aug. 8, 1962, Folder 3: Reading File, July–Sept. 1962, Box 426: Chronological Correspondence, 1962–66, ET Papers.
[94] ET to Symington, Sept. 11, 1962, Folder 3: Reading File, July–Sept. 1962, Box 426: Chronological Correspondence, 1962–66, ET Papers.
[95] ET to Jackson, Aug. 29, 1963, Folder 2: Reading File, July–Sept. 1963, Box 425: Chronological Correspondence, 1962–1966, ET Papers.

Preparedness Subcommittee held hearings on the test ban treaty, as did the Senate Committee on Foreign Relations (CFR). Teller played three parts in the hearings: an advisor to test ban opponents, a willing warrior-witness against the test ban, and an omnipresent force that framed many of the questions to which other witnesses had to respond. Many witnesses in the hearings reflected Teller's influence either openly, as when General Maxwell Taylor acknowledged Teller as one of his advisors, or tacitly, as when witnesses regurgitated Teller's arguments as their own.[96] When attempting to dismiss the significance of the treaty's fallout reduction, Air Force General Curtis LeMay, Los Alamos director Norris Bradbury, and Navy Admiral Arleigh Burke all recycled Teller's "Denver argument" that downplayed fear of fallout. Burke, for example, argued that "the difference [in natural radiation] of living here [in D.C.] and in the Rocky Mountain area, is a greater difference than an increase of contamination in the air caused by a reasonable amount of nuclear explosives"—without mentioning Teller.[97] During the CFR hearings, Senator Bourke Hickenlooper (R-IA) echoed Teller's claims about radiation when he questioned AEC chair Glenn Seaborg. "We could take any number of examples," Hickenlooper began, "where a little bit is not necessarily proved to be harmful and a little bit of some things are actually beneficial, and where a tremendous over dose of one kind or another is harmful. . . . it runs in my mind that we do have evidence in some kinds of life that a little stepped up radiation has actually increased the strength of the recipient."[98]

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[96] U.S. Senate, Preparedness Investigating Subcommittee of the Senate Committee on Armed Services, *Military Aspects and Implications of Nuclear Test Ban Proposals and Related Matters*, 88th Cong., 1st sess. Hearings, Pt. I: May 7, 15, 28, June 5, 25–27, August 1, 2, 9, 1963; Pt. II: Aug. 12, 14, 16, 19, 20, 22, 23, 27, 1963 (Washington, D.C.: U.S. Government Printing Office, 1964; hereafter Preparedness Hearings), 594.

[97] Preparedness Hearings, 356, 471, 944–45.

[98] U.S. Senate, Committee on Foreign Relations, *Nuclear Test Ban Treaty*, 88th Cong., 1st sess. Hearings, August 12–15, 19–23, 26–27, 1963 (Washington, D.C.: U.S. Government Printing Office, hereafter Committee on Foreign Relations Hearings), 227. Seaborg told the Senator that "some of that evidence is not, of course, agreed upon."

Teller himself spent many hours in front of both committees passionately testifying against the test ban. Echoing the words of government scientists, Teller asserted that he was limiting himself to "technical factors." As before, Teller used his own scientific expertise to poke holes in the scientific arguments in favor of a test ban. Teller claimed that the treaty would prevent the United States from achieving an antiballistic missile (ABM) system, and added that, in his judgment, the Soviets already had one. He cast doubt on U.S. scientists' ability to detect Soviet tests, calling existing detection methods "castles of sand." He thundered that the Soviets were ahead in weapons development, and promised that the treaty would hinder U.S. weapons development as well as bury useful Plowshare projects. He narrated scenarios in which the Soviets would hide tests: "the Russians can explore effects upon our Minuteman sites by small clandestine experiments in the atmosphere and by bigger shallowly buried literally legal underground experiments. . . .The above [example] shows that Russian violation of the treaty in the subkiloton range may endanger U.S. security."[99] Teller even predicted that the treaty would leave the United States powerless to use its nuclear arsenal in times of war. Certainly not every Senator believed Teller, but both committees gladly listened to him. William Fulbright (D-AR), chairman of the CFR, told Teller "I had no idea you would be such an interesting witness as to keep us in session all day. . . . this is unprecedented, as far as the length of the session is concerned."[100]

Most importantly, the Senators found Teller's arguments plausible enough that they posed his claims as questions to other witnesses who testified. Policymakers like Robert

[99] Preparedness Hearings, 543, 546, 549.
[100] Committee on Foreign Relations Hearings, 506. Teller's ability to maintain a strong opposition to the treaty during the hearings is especially impressive as CFR Democrats Humphrey and Fulbright supported the treaty so much that they journeyed to Moscow to participate in its signing. The Democratic members of the Preparedness Subcommittee had refused invitations to do the same, Seaborg, *Kennedy, Khrushchev, and the Test Ban,* 259.

McNamara and scientists including Harold Brown, director of the DoD's Defense, Research, and Engineering agency; Los Alamos director Norris Bradbury; former PSAC member Herbert York; AEC chairman Glenn Seaborg; and Kistiakowsky found themselves having to answer questions about Teller's objections to the treaty, including ABM development, Soviet advantages in high yield weapons, detection methods, and the Plowshare program.[101] For example, in his testimony, Kistiakowsky was asked by Fulbright, "Dr. Teller stated categorically the Soviets are ahead of us in their anti-ballistic missile defense because of their 1961–62 test series. Would you care to comment on that?"[102] By adopting Teller's concerns as their own (even if not endorsing them), the Senators essentially allowed Teller to ask the questions during the hearings, forcing the discourse to focus on his objections and casting enough doubt on the test ban to limit it severely in several ways.[103]

Test ban opposition also came in the form of fears of Soviet mendacity, something that the Pugwash movement had worked hard to overcome. Senator Symington cast doubt on the transnational exchange of Pugwash by stating "at the time of the Pugwash Conferences, in 1960, when they assured some of our people, including Dr. Wiesner, that they were very glad of the change in administration because they felt they could work out a test agreement, they were planning [tests] all the time, lying when they were talking to these people."[104] Most Senators and witnesses throughout the hearings took it for granted that the Soviets would cheat the test ban, and Teller encouraged the idea that the Soviets only wanted a test ban because they held an advantage in weapons development. The CFR hearings

[101] Committee on Foreign Relations Hearings, 137 (McNamara), 557 (Brown), 584–85 (Bradbury), 763 (York), 267 (Seaborg); Kistiakowsky, *Scientist,* 859.
[102] Committee on Foreign Relations Hearings, 859
[103] Seaborg, *Kennedy, Khrushchev, and the Test Ban,* 272, claims that the CFR Senators posed Teller's questions to other scientists so that his objections could be refuted by objective experts. But such a strategy also gave credibility to Teller's claims.
[104] Preparedness Hearings, 206.

began with numerous references to the number of treaties the Soviet Union had violated, leaving Secretary of State Dean Rusk to resort to the argument that the "treaty is not standing upon the foundation of trust."[105] Instead, pro-test ban witnesses argued, any Soviet violation of the treaty would not threaten U.S. security, since underground testing, permitted by the treaty, would ensure preparedness.[106] The scientists who had reached across to their Soviet counterparts quickly jettisoned their arguments about transnational trust in favor of more pragmatic claims about the treaty's ability to promote weapons development, essentially the opposite reason they had originally wanted the treaty.

Finally, the conflicting views of Teller and other scientists shattered long held beliefs about the nature of "science." Teller made a convincing enough case against the test ban that the Senate believed that scientific opinion was divided over the ban, despite science advisor Herbert York's testimony that "the majority [of scientists] agrees with me rather than Dr. Teller . . . they are in favor of proceeding with the test ban."[107] Scientists were so worried about Teller's influence that they publicly challenged his knowledge and questioned his personal motivations. Kistiakowsky, worried that Teller would raise a "red herring," testified in order to refute his predictions of clandestine Soviet outer space tests.[108] Still more scientists testified or wrote to Fulbright in order to question Teller's qualifications. The Federation of American Scientists accused Teller of making "misleading" statements, and Hans Bethe directly attacked Teller's competence, writing to Fulbright: "I should like to

[105] Committee on Foreign Relations Hearings, 81, 165, 541; Kaysen interview.
[106] See, for example, Kistiakowsky to Eisenhower, Aug. 18, 1963, Folder: General Eisenhower, Box 32: Correspondence, 1961–1965, HUG (FP) 94.8, GBK Papers.
[107] Committee on Foreign Relations Hearings, 767.
[108] Kistiakowsky to Eisenhower, Aug. 18, 1963, Folder: General Eisenhower, Box 32: Correspondence, 1961–1965, HUG (FP) 94.8, GBK Papers.

point out that to my knowledge Dr. Teller is not an expert in this subject [of antimissile defense]."[109]

This dispute within the scientific community baffled the Senators. During the Preparedness hearings, Symington, referring to a disagreement between Livermore Nuclear Laboratory director John Foster and Teller, commented: "Now what is a mystery to me is how two people who have worked together and been so close together in this field can nevertheless be so far apart in conclusions. I am not asking this in humorous fashion. I think in a sense it is the crux of this whole problem." Henry Jackson, also confused, asked Los Alamos director Norris Bradbury about the division of scientific opinion:

> What I am getting at is, do we have enough back of us to come to a conclusion? All science is a matter of prediction, isn't it? I mean, you try to predict out of a given number of cases or situations, based on certain things happening, that in the future certain things are apt to happen. And it will vary by the degree of the experience back of you. Now I realize you do not have too much experience back of you in all of these elements we are talking about. But do you have enough to come to a reasonably certain conclusion that a certain thing will happen in a given set of circumstances?

Bradbury's answer could not have satisfied Jackson: "I think the answer to the question is 'Yes.' Dr. Teller's answer to your question would be 'No.' Dr. Foster can make his own answer. . . . We all look at the same set of facts, and we come to different opinions about their relevance, interpretation, and importance." Scientists were accused of being subjective, not the objective embodiments of reason and empiricism that they were in their laboratories. West Virginia Senator Robert Byrd (D) joked with Curtis LeMay that nuclear weapons tests were more reliable than the scientists who devised them: "While you have to depend upon these . . . people in the scientific community," he said, "this, again, is not as sure and as

[109] Committee on Foreign Relations Hearings, 996, 1007.

dependable as would be the results that we might get from the testing of high-yield weapons." LeMay concurred.[110] As the hearings dragged on, Fulbright grew increasingly peeved by scientists' differing views. "One of the most unexpected developments, at least from my point of view," he told former AEC commissioner Willard Libby, "has been the wide divergence of views among the acknowledged experts, particularly in the basic scientific areas in which you operate. I thought that these intellectual giants would arrive at some sort of common conclusions. But I find they differ, just as much as politicians do, and this has been very puzzling to me." Near the end of the hearings Fulbright became downright irritable. "It is good to have someone who isn't a scientist occasionally discuss these matters," he complained to one witness, "not that I didn't like the scientists."[111]

Teller's campaign did not defeat or amend the test ban treaty as he had hoped. But it is clear that his influence in military and government circles greatly weakened the test ban that eventually emerged. Teller's scientific claims, his influential connections, and the mistrust of Soviets he encouraged had their biggest influence on the JCS. The JCS made their acceptance of the LTBT contingent upon government acceptance of their four "safeguards": "comprehensive, aggressive, and continuing underground nuclear test programs"; financial and experimental upkeep of the U.S. nuclear laboratories; a well-funded readiness program to restart atmospheric testing at a moment's notice in the event of Soviet treaty violations; and drastic improvements in verification and intelligence. The safeguards bore the distinct imprint of Teller, and indeed the JCS admitted that Teller heavily influenced their views.[112] Other observers noted the closeness first-hand. In a letter to the

[110] Preparedness Hearings, 536, 750, 772.
[111] Committee on Foreign Relations Hearings, 639, 836.
[112] Preparedness Hearings, 590, 594.

Prime Minister, British government science advisor Solly Zuckerman reported that at a June 1963 NATO speech, Teller's anti-test ban fury was "clearly music to the ears of the American Chiefs of Staff."[113] Although some Senators disputed Teller's testimony, none dared oppose the JCS and happily pledged allegiance to the safeguards. By the time the LTBT easily passed the Senate (80–19) in September 1963, the Kennedy administration had also accepted the JCS safeguards.[114] The safeguards ensured that nuclear testing actually increased after the test ban, adding fuel to the arms race fire; Curtis LeMay had assured the Senators that the U.S. military budget would increase after the treaty because of the safeguards.[115] Pugwash scientist Bernard Feld had these developments in mind when he called the treaty an "arms control disaster."[116]

Teller's efforts must not be overestimated, of course. He had campaigned tirelessly against a test ban since the mid 1950s, efforts that rewarded him with the hostility of his former friends and colleagues. Teller failed to prevent the ratification of the LTBT, and the survival of his Plowshare project was far from guaranteed; he appeared to have been defeated. Even his longtime foe Linus Pauling was honored with the Nobel Peace Prize in recognition of his efforts for a test ban. According to one biographer, Teller's secretary cried when she heard Kennedy praise the test ban treaty's ratification.[117] Perhaps Teller consoled

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[113] Solly Zuckerman to the PM, June 24, 1963, PREM 11/4159, UK National Archives.
[114] Seaborg, *Kennedy, Khrushchev, and the Test Ban,* 269–71; Kaysen interview.
[115] Committee on Foreign Relations Hearings, 405. Of 638 U.S. tests between 1945 and 1980, 345 (54%) took place after the LTBT. In the Soviet Union, of the 298 tests between 1949 and 1980, 168 (56%) occurred after the LTBT, Seaborg, *Kennedy, Khrushchev, and the Test Ban,* 288. On the test ban's psychological effect on the public, see Paul Boyer, "From Activism to Apathy: The American People and Nuclear Weapons, 1963–1980," *Journal of American History* 70 (Mar. 1984): 821–44.
[116] Feld, unpublished autobiography, "VIII: Nuclear Politics in the U.S.A." Doc. 11, March 8, 1988, Box 65: Writings, Folder unnumbered: Autobiographical, BTF Papers.
[117] Judged by his own goal of amending or defeating the treaty, Teller suffered a significant failure. But at the same time, characterizing the treaty as a major defeat for Teller is also inaccurate. Although the Senate passed the treaty with no significant amendments, such a view ignores the JCS safeguards and Teller's role in them. While Peter Goodchild writes that Teller had to "admit defeat" after ratification, he adds that the LTBT was "a temporary, even hollow victory for the President. Teller, on the other hand, had, by playing his part in ensuring the continuation of underground testing,

himself by realizing that his far-right arguments had enabled the intense nuclear development mandated by the JCS safeguards. Just as Pauling's extremism made the moderate goals of government test ban advocates more acceptable to the state, Teller seems to have played a similar role on the opposite end of the spectrum.[118]

Although the test ban struggle brought many scientists together in common cause, the debate revealed science to be a house divided. In the Eisenhower and Kennedy years, when genius scientists opposed each other in public, politicians ended up believing those who supported their political point of view. At one point in the hearings, Symington demonstrated this change when he brought up the disagreement among scientists. He then told Teller: "I am more inclined to go along with you and Dr. Johnny Foster than I am with Dr. Brown." And Symington was perhaps even more baffled than he let on (or perhaps simply made his decision on reasons other than science), as he ended up voting *for* the treaty.[119]

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contributed in a major way to maintaining a future both for his laboratory and for his anti-Soviet crusade," *Edward Teller: The Real Dr. Strangelove* (Cambridge: Harvard University Press, 2004), 305–07. Time appears to have healed any lingering resentment Teller had over the treaty; his memoirs dedicate only one simple sentence to the treaty's ratification: "As [Dr. James R.] Maxfield had predicted, the Limited Test Ban Treaty was ratified and went into effect in late 1963," Teller, *Memoirs*, 469. In *The Advisors: Oppenheimer, Teller, and the Superbomb* (Stanford: Stanford University Press, 1989), Herbert York writes: "In my opinion, he [Teller] deserves much of the credit (or blame), probably more than any other single person, for the failure of the 1963 Nuclear Test Ban Treaty to prohibit underground tests along with those in all other environments," 146.

[118] For example, the radical activists of the U.S. civil rights movement made the peaceful activism of Martin Luther King more palatable to mainstream society. King points this out in his "Letter from Birmingham City Jail" when he argues that were it not for his philosophy of nonviolence, "by now many streets of the South would, I am convinced, be flowing with blood. And I am further convinced that if our white brothers dismiss as 'rabble rousers' and 'outside agitators' those of us who employ nonviolent direct action, and if they refuse to support our nonviolent efforts, millions of Negroes will, out of frustration and despair, seek solace and security in black nationalist ideologies—a development that would inevitably lead to a frightening racial nightmare." "Letter from Birmingham City Jail," in *A Testament of Hope: The Essential Writings and Speeches of Martin Luther King, Jr,* James Melvin Washington, ed. (New York: Harper San Francisco, 1991), 289–302. An online transcript is available at http://www.stanford.edu/group/King/popular_requests/frequentdocs/birmingham.pdf

[119] Preparedness Hearings, 772. For voting results, see "Role Call Vote in Senate on the Test Ban Treaty," *New York Times* 25 September, 1963. Jackson and Hickenlooper also voted for the treaty, while Stennis voted against it. Symington endorsed the Preparedness Subcommittee's report that recommended rejection of the treaty, but voiced his intent to vote for ratification in the Senate nonetheless. Seaborg, *Kennedy, Khrushchev, and the Test Ban*, 278.

The *New York Times*, observing the confusion caused by the scientists' role in the Senate debate, described the LTBT as "coming into the world like an unwanted child." Yet scientists still spoke with authority. In expressing their support for the treaty, the *Times* editorial staff quoted Norris Bradbury: "If now is not the time to take this chance, to count on this hope . . . what combination of circumstances will ever produce a better time?"[120] In late 1963, British Prime Minister MacMillan also looked to scientists for guidance in the nuclear age. A telegram of greeting from the Prime Minister to the Pugwash conference underway in Yugoslavia read,

> I am glad that the Pugwash Conference has been meeting against at this time. The Nuclear Test Ban Treaty which you have long discussed in the past has been a notable step towards relaxation of tension. But it is only a first step and now we need others. The ideas of the scientists can help the politicians as they have done before. And in their contacts between East and West the scientists can help each other. I am sure your discussions will prove as fruitful as in the past.[121]

But as the 1960s progressed, science's image began to deflate, making scientists' voice just one among many. The New Left, although originally rooted in early antinuclear movements, eventually grew to scorn such traditional activism as conformist. Emblematic of this old guard, Rabinowitch dismissed the New Left, calling student movements "The Aimless Revolution."[122] Increasingly during the 1960s and 70s, Americans turned toward religion and spirituality. At times, scientists who advised the government on nuclear policy even found themselves the object of student protest.[123]

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[120] "War and Peace," *New York Times*, 23 August 1963.
[121] Harold MacMillan to Secretary General, Pugwash, Dubrovnik, Yugoslavia 1963, FO 371 IA D1092/ 30, UK National Archives.
[122] Paul Boyer, *Fallout: A Historian Reflects on America's Half-Century Encounter with Nuclear Weapons* (Columbus: Ohio State University Press, 1998), 111–127.
[123] See Maurice Isserman and Michael Kazin, *America Divided: The Civil War of the 1960s* (New York: Oxford University Press, 2000), 241–259; Bruce J. Schulman, *The Seventies: The Great Shift in American Culture, Society, and Politics* (Cambridge: Da Capo, 2002), 78–101; and Ann Finkbeiner, *The Jasons: The Secret History of Science's Postwar Elite* (New York: Viking, 2006), 102-06.

The test ban treaty, long fought for by scientists, eventually came to be seen as a missed opportunity. The LTBT did little to slow the arms race, but still provided many Americans with the calming illusion that the superpowers were taking steps to make nuclear war less likely.[124] Near the end of his life, Bernard Feld wrote that "the treaty was fatally flawed."[125] Attending a Pugwash meeting in Czechoslovakia just after the treaty's ratification, scientists noticed that the achievement of a test ban threatened to divide the organization. Many U.S. Pugwash scientists complained that their organization had become too official, risking its free and open exchange, while others hoped to broaden the movement's goals beyond "symbolic" arms control agreements like the test ban.[126] Frank Long, Assistant Director for Science and Technology with the Arms Control and Disarmament Agency, told Feld that Pugwash was "attaining an increasingly quasi official status," a path that could only lead to irrelevance.[127]

The test ban years also marked the zenith of the PSAC's influence on policy. Kistiakowsky remarked that PSAC's decline began under Kennedy, when "PSAC and the special assistant [for science and technology] largely lost their influence in matters of national security." "Still later when President Johnson came in," Hans Bethe added, "the whole of PSAC deteriorated and there was not very much contact between the science adviser and President Johnson. And still later even less between the science adviser and President Nixon, until Nixon abolished the whole committee."[128] Edward Teller, on the other hand, continued

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[124] Boyer, "From Activism to Apathy," 829–33.
[125] Feld, unpublished autobiography, "VIII: Nuclear Politics in the U.S.A." Doc. 11, March 8, 1988. BTF Papers, MIT. MC 167, Box 65: Writings, Folder unnumbered: Autobiographical, BTF Papers.
[126] Rabinowitch, "Editorial: The Test Ban." *Bulletin of the Atomic Scientists*, September 1963, Vol. XIX, No. 7, 2.
[127] F. Long to Feld, Oct. 2, 1963, Box 33: Eleventh Pugwash Conference, Dubrovnik, Folder 329: Correspondence, 1963, BTF Papers.
[128] George B. Kistiakowsky Oral History, Nov. 3, 1977, 21, DDEL; Hans Bethe Oral History, Nov. 3, 1977, 23–24, DDEL. See also Herken, *Cardinal Choices*.

to make bold assertions about nuclear weapons and retained influence in the Reagan administration, where he promoted the Strategic Defense Initiative.

After the test ban, scientists continued their efforts to influence politics, though they could no longer claim to be apolitical. In 1962 Leo Szilard, the physicist who first conceived of an atomic bomb's chain reaction, founded the Council for a Livable World, which raised money for Senate candidates who endorsed nuclear disarmament. In 1964, much to the chagrin of his old Republican friend Eisenhower, Kistiakowsky helped found Scientists and Engineers for LBJ where he ended up debating Teller, who was campaigning for Barry Goldwater. Kistiakowsky later grimaced, "My name was so frequently used in this campaign that I have clearly lost the status of a pure scientist."[129]

In their analysis of scientists' role in the test ban debate, Jacobson and Stein argue that scientists allowed their hopes for international cooperation to interfere with their technical expertise and objectivity, hampering, rather than helping, U.S. diplomacy.[130] Such a claim applies to those scientists who acted as diplomats at the Geneva conferences. But for those scientists elsewhere in the policymaking process, their strict observation of the technical boundaries of their positions hindered the antinuclear arguments they could make. Limited to technical advice and confronted by nationalist fears, government disarmament advisors failed to develop arguments effective enough to challenge the supporters of thermonuclear weapons. Just as transnational resources opened different doors for antinuclear protestors, moral messages effectively swayed public support in favor of the disarmament movement. In the 1950s and early 1960s, Pauling mobilized a significant

[129] Kistiakowsky to David Tunick, Sept. 4, 1964; Kistiakowsky to M. Kasha, Nov. 19, 1964, Folder: Scientists and Engineers for LBJ, 1 of 2, Box 36: Correspondence 1961–1965, HUG (FP) 94.8, GBK Papers.
[130] Jacobson and Stein, *Diplomats, Scientists, and Politicians,* 481.

number of Americans against fallout, arguing that nuclear testing was immoral because it harmed the people, including children, it was meant to protect. Even Teller at one point told Congress that it would be immoral to stop testing because he was close to developing a clean bomb—one that would produce no fallout at all. Such an invention, he claimed, would save innocent people at home and abroad.[131]

In contrast, in order to retain influence in the government, science advisors and Pugwash participants had the burden of arguing that fewer nuclear weapons made the nation safer. As Eric Hobsbawm has written, science "was too big, too powerful, too indispensable to society in general and its paymasters in particular to be left to its own devices."[132] Since the Oppenheimer hearing, the state had explicitly demanded a brand of science advice that spoke only the language of deterrence, where only nuclear weapons promised salvation. While the test ban proved a dramatic turning point in scientists' relationship with the state, the following chapter shows that scientists remained determined to influence policy. This goal proved difficult, however, as few policymakers were interested in listening to them.

[131] Goodchild, *Teller,* 268.
[132] Hobsbawm, *Age of Extremes*, 556.

Chapter Five: To "Sail Before the Wind of Time": Scientists and Disarmament after the Test Ban Treaty

The successful conclusion of the test ban debate, initially cause for celebration, soon turned into a crisis for Pugwash and government scientists because the debate over the test ban had challenged, rather than established, scientists' role in policymaking. After 1963 the rise of détente threatened to make the transnational connections of Pugwash scientists superfluous. At the same time, increased hostility from the Johnson and Nixon administrations lessened scientists' influence in government. Government scientists attempted to remain relevant to U.S. policymakers, in particular by trying to end the Vietnam War, though these efforts abetted the waging of war more than they hindered it. Meanwhile, rather than disband, Pugwash scientists attempted to transform their movement by broadening their goals to include settlement of the German question, non-proliferation, international cooperation, aid to developing countries, and ending the Vietnam War. This transformation, however, would stretch Pugwash rather thin. Meanwhile, the U.S. and British governments contained Pugwash scientists by emulating them—taking over the provenance of international cooperation in the form of détente.

Fallout from the Test Ban

On September 24, 1963, the U.S. Senate ratified the Limited Test Ban Treaty (LTBT), pledging to restrict nuclear tests to below the Earth's surface. When he heard the news, biophysicist Eugene Rabinowitch had mixed reactions. As editor of the *Bulletin of the Atomic Scientists* and one of the leaders of Pugwash, the implementation of the treaty validated his

years of antinuclear activism. He was pleased that after nearly two decades of nuclear tension, the United States and the Soviet Union had finally agreed to a small check on the arms race. At the same time, Rabinowitch recognized that the treaty might very well put an end to the antinuclear movement without eliminating nuclear weapons at all.

Rabinowitch hailed the signing of the LTBT, even moving the *Bulletin's* symbolic clock back a few minutes from nuclear midnight. "Not that the treaty is a significant step toward disarmament," he cautioned; "it is not." Although the Test Ban was "welcome news" and "an encouraging event," Rabinowitch believed that its value was entirely symbolic. The treaty did not outlaw underground tests, and allowed the arms race actually to accelerate after 1963. Rabinowitch believed that such partial, almost token gestures fell far short of the ultimate goal of disarmament.[1]

On a deeper level, Rabinowitch also worried that arms control agreements like the LTBT might render much of his life's work irrelevant if they distracted people from crucial problems yet to be resolved. If the test ban relieved people that the air would no longer be poisoned by fallout, they might forget that the superpowers continued to stockpile enough nuclear weapons to kill everyone on Earth. Rabinowitch essentially wondered how to sustain his movement once one of its primary goals had been met. Five years before the LTBT, Rabinowitch predicted that a test ban would "become a first step to nowhere," leaving the world "where it was before—a house divided, with nations jealously maintaining their

[1] Eugene Rabinowitch, "Editorial: The Test Ban," *Bulletin of the Atomic Scientists,* September 1963, Vol. XIX, No. 7, 2; Rabinowitch, "Editorial: First Step—To Where?" *Bulletin of the Atomic Scientists,* October 1963, Vol. XIX, No. 8, 2–3.

capacity for mutual destruction." The antinuclear movement, he feared, "will find itself deflated without a new worthwhile, realistic goal."[2]

Cold War historians have long recognized the significance of the year 1963. Noted historians peg 1963 and the LTBT as the moment when East and West backed away from the "eyeball to eyeball" confrontation of the Cold War. Soon after, a stabilizing détente arose.[3] Historians of the antinuclear movement also place special emphasis on the year 1963. The years after the treaty, according to Paul Boyer, saw "nuclear apathy" grow in the United States.[4] Moving tests underground made the nuclear threat harder to perceive, while protest against the Vietnam War drew away many protesters from the antinuclear movement, leaving only "a dwindling band of older activists" to carry a torch for nuclear disarmament.[5]

Many such stalwarts could be found at Pugwash conferences during the 1960s. Stoic though these activists may have been, previous accounts have argued that the LTBT was particularly damaging for Pugwash and its adherents. Lawrence Wittner has described the group of scientists as "languishing" during the late 1960s and early 1970s.[6] But this languishing was the result of an anxious paralysis rather than apathy, as Pugwash spent the 1960s actively pursuing scientific internationalism while attempting to reformulate its goals and maintain influence over policy. Most importantly, Pugwash lacked its former access to U.S. officials. In a contentious and difficult process, Pugwash members pushed and pulled

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[2] Rabinowitch, "Nuclear Bomb Tests: A Skeptical View," in *The Atomic Age: Scientists in National and World Affairs* (New York: Basic Books, 1963), 341.
[3] See Marc Trachtenberg, *A Constructed Peace: The Making of the European Settlement, 1945–1963* (Princeton: Princeton University Press, 1999), and John Lewis Gaddis, *We Now Know: Rethinking Cold War History* (New York: Oxford University Press), 1997.
[4] Paul Boyer, "From Activism to Apathy: The American People and Nuclear Weapons, 1963–1980." *Journal of American History* 70 (Mar. 1984): 821.
[5] See Lawrence Wittner, *The Struggle Against the Bomb: Vol. 2. Resisting the Bomb: A History of the World Nuclear Disarmament Movement, 1954–1970* (Stanford: Stanford University Press, 1997), and Boyer, "From Activism to Apathy," 837.
[6] Wittner, *The Struggle Against the Bomb: Vol. 3. Toward Nuclear Abolition: A History of the World Nuclear Disarmament Movement, 1971 to the Present* (Stanford: Stanford University Press, 2003), 14.

the movement in different directions, toward ideas as disparate as disbanding or broadly increasing its aims. After 1963, Pugwash divided its attention between disarmament, nonproliferation, solutions to the Vietnam War, scientific cooperation between nations, and aid to developing countries. Geir Lundestad has written that détente between the United States and the Soviet Union caused a schism within the Atlantic Community, even as it brought the end of the Cold War in sight.[7] I argue here that détente affected the scientists' movement in a similar way, by causing a schism in the movement and forcing a redefinition of Pugwash scientists' relationship to the Cold War. When the U.S. and Soviet governments appeared to be making progress on arms control, the transnational resources the movement could provide naturally became less important.

Rabinowitch and Pugwash, 1957–1963

During World War II, Rabinowitch had worked on the Manhattan Project at the University of Chicago's Metallurgical Laboratory where, after the defeat of Germany, he began to reconsider the need for the atomic bomb. He consequently joined fellow Manhattan Project scientists Leo Szilard and James Franck in opposing the use of atomic bombs on Japan without a demonstration. Rabinowitch penned most of the pro-demonstration "Franck Report" intended for Secretary of War Henry Stimson, only to watch the report get "lost somewhere in the higher echelons in Washington." Despite this frustration, Szilard and Rabinowitch's work on the Franck Report helped launch the Atomic Scientists' Movement.[8]

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[7] Lundestad, *The United States and Western Europe Since 1945: From "Empire" by Invitation to Transatlantic Drift* (Oxford: Oxford University Press, 2003), 128.
[8] Rabinowitch, *Dawn of a New Age,* 99; see also Alice K. Smith, *A Peril and a Hope: The Scientists' Movement in America, 1945–47* (Chicago: University of Chicago Press, 1965).

The Atomic Scientists' Movement and antinuclear sentiment in general quelled with the rise of the Cold War in the late 1940s. But amid the tumultuous waters of the Red Scare, Rabinowitch maintained a steady ship. After his work on the Franck Report, Rabinowitch devoted much of his professional life to mobilizing scientific opinion to prevent nuclear war. Months after World War II ended, Rabinowitch co-founded the *Bulletin of the Atomic Scientists* and became its leading editorial voice for nearly 30 years. Through the *Bulletin*, Rabinowitch invited scientists to contribute to the diffusion of informed ideas about the role of nuclear energy and science in general throughout the world. Under Rabinowitch's guidance, the *Bulletin* became an influential sounding board for scientifically based antinuclear sentiment. Convinced that humans would use nuclear technology only to destroy themselves, Rabinowitch hoped that the *Bulletin* would bring the rational and informed opinions of scientists to bear on the dilemmas of the nuclear age. Under his leadership, the *Bulletin* spoke out against McCarthyism, the Oppenheimer hearing, the H-bomb, and the arms race in general, often with an "evangelical fervor" for peace.[9]

Not content with merely editing the *Bulletin* and being "undoubtedly the most distinguished encyclopedic authority in Photosynthesis" research, Rabinowitch embarked in 1957 on another venture for nuclear disarmament.[10] With Joseph Rotblat, the only scientist to quit the Manhattan Project for moral reasons, and the financial support of the left-leaning railroad magnate Cyrus Eaton, Rabinowitch arranged for a meeting in Pugwash, Nova Scotia, among influential scientists from 10 countries, including the United States, Britain, Japan, and the Soviet Union. The goal of this meeting was to assess, from a scientific

[9] Ralph Lapp, "Toward Nuclear Education," *Bulletin of the Atomic Scientists,* June 1973, Vol. XXIX, No. 6, 6.
[10] M. Losada to ER, March 11, 1964, Series IV, Addenda II, Box 6, Folder 13: General Correspondence, Eugene Rabinowitch Papers, University of Chicago (hereafter ER Papers).

perspective, "the perils to humanity which have arisen as a result of the development of weapons of mass destruction." Rabinowitch thought scientists were ideally suited to confront the challenges of the future because "the detachment to which the study of science has accustomed one helps to avoid errors into which partisanship often leads the most astute political leaders and observers."[11]

At Pugwash conferences the assembled scientists discussed "the scientific and technical implications of atomic energy," paying special attention to "the political problems which are the background to international negotiations."[12] The relatively unstructured and informal private meetings attempted to create a cooperative atmosphere for the enlightening exchange of views from each side of the Iron Curtain. The scientists at Pugwash considered their first meeting in 1957 so successful that they initiated annual meetings and even took to describing their efforts as a "movement."[13] Pugwash participants hoped to encourage disarmament in three ways: by influencing governments, forming a channel of communication between scientists, and educating public opinion. Dialogue between East and West, Rabinowitch believed, could only encourage trust and cooperation between the two hostile powers. Immediately, Pugwash members engaged in discussions about adequate

[11] Joseph Rotblat, *Scientists in the Quest for Peace: A History of the Pugwash Conferences* (Cambridge: MIT Press, 1972), 141; Rabinowitch, *Dawn of a New Age*, 5.
[12] Rotblat, *Quest for Peace*, 143.
[13] Characterizing Pugwash as a social movement is open to debate. Rabinowitch was prone to refer to Pugwash in such terms, writing: "the word 'movement' cannot be avoided here. . . . The Pugwash *conferences* do evolve slowly into a Pugwash *movement*—a vague and groping movement, but nevertheless a movement, not insignificant for the future of humanity." Rabinowitch, "About Pugwash," *Bulletin of the Atomic Scientists,* April 1965, Vol. XXI, No. 4, 9, emphasis in original. By 1970 Feld was using the term "movement," though he surrounded it with noncommittal quotation marks. Feld to ER, Feb. 4, 1970, Series IV, Addenda II, Box 7, Folder 1: General Correspondence, ER Papers. Pugwash "members" (also a problematic term) were never arrested or attacked, never marched or took to the streets in the name of Pugwash. Although Pugwash may not resemble the stereotypical grass roots movement, it does fulfill one classic definition of social movement organizations: "a complex, or formal, organization that identifies its goals with the preferences of a social movement . . . and attempts to implement these goals." A social movement in this context is a set of opinions and beliefs representing preferences for change. John D. McCarthy and Meyer N. Zald, "Resource Mobilization and Social Movements: A Partial Theory," in Steven Buechler and F. Kurt Cylke Jr., eds., *Social Movements: Perspectives and Issues* (Mountain View: Mayfield Publishing Company, 1997), 153.

verification for a test ban. Nine of the first 11 Pugwash conferences, first held in the West and later in Warsaw Pact nations, dealt with aspects of a test ban in some measure.[14]

The Eisenhower administration had mostly resisted Pugwash efforts to influence its policy, but U.S. Pugwash scientists had managed to play a small role in negotiating a nuclear test ban during the Kennedy administration. When the Moscow Treaty was signed in 1963, Pugwash claimed for itself a fair share of the credit.[15] Rabinowitch and his fellow Pugwashites expected an avalanche of more meaningful disarmament measures to follow but instead, the Test Ban Treaty threatened to bury their movement.

Pugwash and Disarmament after the Test Ban, 1963–1966

Pugwash scientists greeted the test ban with "exhilaration." The *New York Times* reported that the eleventh Pugwash Conference in Dubrovnik, Yugoslavia, opened with an "air of amity" due to the recent signing of the treaty.[16] A Brazilian scientist at the conference described participants as "full of optimism" and "euphoria" because of the test ban, while others "expressed unanimous satisfaction" with the treaty and "expressed hope" that further measures "directed at the relaxation of international tension [and] paving the way towards complete and universal disarmament" would follow. Although the treaty was reason to

[14] Rotblat, *Pugwash—the First Ten Years: History of the Conferences of Science and World Affairs* (New York: Humanities Press, 1968), 17; Rotblat, *Quest for Peace*. Soviet and other Eastern Bloc participants were, of course, only allowed to attend Pugwash conferences with official state permission and thus held politically-approved positions on nuclear disarmament. See Wittner, *Resisting the Bomb*, 80–82.

[15] Kistiakowsky, *A Scientist at the White House: The Private Diary of President Eisenhower's Special Assistant for Science and Technology* (Cambridge: Harvard University Press, 1976), 423–24; See Rotblat, "Movements of Scientists," in Rotblat, ed., *Scientists, the Arms Race, and Disarmament* (London: Taylor & Francis Ltd, 1982), 139; Rotblat, *First Ten Years*, 42; Rotblat, *Quest for Peace*, 33.

[16] Rotblat, *First Ten Years*, 44. David Binder, "Scientists Begin East-West Talks," *New York Times,* 21 September 1963.

"rejoice," Pugwash scientists urged each other to "continue their research work in aid of reaching an agreement . . . to ban all underground explosions."[17]

After the euphoria subsided, however, Pugwash appeared to lose some focus. Freed from their test ban obsession, Pugwash scientists at Dubrovnik recommended everything from a U.S.-Soviet flight to the moon, proposals of nuclear safeguards, and seismic detection measures to prevent underground testing. Still another observer claimed that problem of the division of Germany "appeared to be at the heart" of much of the conference.[18]

The achievement of its goal of a test ban placed Pugwash at a crossroads, and the organization began to redefine itself. This introspection reached the Pugwash Continuing Committee—the body of elected scientists who organized the conferences including Rotblat, secretary-general of the committee; Bernard Feld, chair of the U.S. Pugwash Committee; and Rabinowitch, an member of the committee since its creation. These men found themselves questioning the meaning of their efforts on a fundamental level and confronted the worst fate for all social movement organizations: irrelevance. They feared that Pugwash was in danger of losing its autonomy, of becoming "unofficially official," rather than "officially unofficial," in the words of one Soviet scientist. Bertrand Russell, the philosopher and co-author, with Albert Einstein, of the manifesto that inspired Pugwash, began to leave scientists behind in order to participate in disarmament efforts based on civil disobedience. "I suspect," Rabinowitch wrote to Feld, "that these conferences have appeared to [Russell]

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[17] W.E. Kerr, "Impressions from Dubrovnik," *Pugwash Newsletter,* January 1964, Vol. 2, No. 3, 43, Series IV, Addenda II, Box 1, Folder 2: 13th Pugwash Conference on Science and World Affairs, ER Papers; "Preamble to Continuing Committee Press Release," 1963, Series I, Box 6, Folder 9: Press Releases, Internal Memoranda, Miscellaneous Notes, ER Papers; J.D. Cockcroft, "The Nuclear Test Ban," Paper XI.9, Box 6, Folder 11: Conference Papers, Aubert-Cockcroft, ER Papers. The Limited Test Ban Treaty only banned nuclear explosions in the atmosphere, allowing tests to move underground where they actually increased.

[18] "Scientists Discuss Germany's Division," *New York Times*, 23 September 1963; "World Scientists Hail U.S. and Soviet Peace Plans," *New York Times*, 24 September 1963; "Study of Quakes," *New York Times*, 26 September 1963.

for some time to be much too tame (and perhaps not sufficiently anti-American) to suit his impatience (and taste)!"[19] With their honorary chairman bolting for different ventures, Rotblat, Feld, and Rabinowitch knew that they had to recreate their movement in order to keep it alive.[20]

Proud of creating an innovative approach to confronting the Cold War, Pugwash feared becoming a part of the status quo in the quest for peace. In 1961 the social critic and author of *The Lonely Crowd* David Riesman commented on Pugwash. He wrote that while "talk of disarmament has become fashionable . . . an intellectual never wants to be fashionable, but rather to be ahead of fashion."[21] In 1962 Rabinowitch wrote that the U.S. government should "sail before the wind of time with a stronger will and a firmer hand at the tiller than it has now." Never one to count on the government, Rabinowitch fully expected scientists to navigate the world through the atomic age. After the Dubrovnik conference, Rabinowitch wrote to Feld, asking "Did we act ahead of official thinking, or merely endorse what is already agreed upon . . . on the official level? The purpose of the Pugwash Conferences," he declared, "is to stay far ahead of governmental thinking and to influence it, rather than to endorse it!"[22]

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[19] Rabinowitch, "Pugwash," from *Retort,* undated (mid-1965), Series IV, Addenda II, Box 6, Folder 7: Disarmament, Proposed Publication On, ER Papers; ER to Bernard Feld, Oct. 7, 1963, Series I, Box 8, Folder 20: Feld-Goldsmith, ER Papers.

[20] "Perceived lack of success in goal accomplishment by [a social movement organization, or SMO,] may lead an individual to switch to SMOs with alternative strategies or . . . to those with other target goals." McCarthy and Zald, "Resource Mobilization," in *Social Movements,* 161. Put simply, without any evidence of success (or even a viable goal), individuals will leave a movement organization.

[21] Riesman added that he could only hope that "conferences and newsletters on disarmament could become a fashion like yo-yos or even folk singing (although I'd guess that the audience for the latter overlaps that for disarmament)." David Riesman, "Report from Moscow," *Committees of Correspondence Newsletter*, April 1961, Series I, Box 10, Folder 11: Committees of Correspondence, National Headquarters, ER Papers.

[22] Rabinowitch, *The Dawn of a New Age*, 98; ER to Feld, Oct. 7, 1963, Series I, Box 8, Folder 20: Feld-Goldsmith, ER Papers.

But in which direction would the ship of Pugwash sail? All agreed that Pugwash should aim to end the Cold War and encourage peace. Opinions differed widely, however, over the best tactic to take. This division reached all the way to the leadership of Pugwash. Some, like Rotblat, wanted to retrench on disarmament. Others, particularly Rabinowitch, wanted to broaden the movement's goals and take Pugwash in new directions, especially toward expanding international cooperation and scientists' influence over world affairs.

In 1962, Rotblat wrote: "The urgency of the disarmament problem makes it, at present, a topic of priority."[23] In fact, that same year the Pugwash Continuing Committee "recommended that disarmament be the main topic of all future Pugwash conferences," since otherwise the Continuing Committee feared a "dissipation of interest" would dampen the "pioneering" nature of the conferences. Although Pugwash conferences continued to vary in their focus, Rotblat attempted to keep Pugwash on the track of disarmament after the test ban, as he understandably thought that the ban would make further disarmament even easier. In this spirit, the Continuing Committee devoted the 1964 conference to disarmament in order "to restore to Pugwash its role as a source of new ideas and mutual understanding in the disarmament field."[24]

Rotblat could not ignore the Pugwashites who wanted to address issues such as international cooperation in science, the problems of developing nations, the environment and population explosion, and the social responsibilities of scientists. But a "consensus" among Pugwash decreed that disarmament and world security demanded "the most urgent attention and should, therefore, occupy the bulk of the Pugwash effort." In practice this

[23] "Recommendations from the Continuing Committee to the Committee on Future Activities and Organization," 1962, Series I, Box 6, Folder 1: Cambridge Pugwash Conference, August 1962, ER Papers.
[24] Rabinowitch, Memo to possible U.S. Pugwash delegates, April 27, 1964, Series IV, Addenda II, Box 1, Folder 2: 13th Pugwash Conference on Science and World Affairs, ER Papers.

meant that at a typical Pugwash conference three of the five sessions concerned disarmament, while the other two addressed any of the various other issues. Rotblat apparently preferred to keep issues of development and cooperation separate from disarmament efforts "so that he would be free to proceed with Pugwash's historic focus of attention on disarmament."[25]

Despite this alleged consensus, Pugwash began to spread in different directions. Of the ten conferences between 1964 and 1971, only two focused exclusively on disarmament and world security. Six of the conferences framed disarmament as merely one part of the problems related to cooperation and development. Still, in the eyes of the media, at least, Pugwash remained a disarmament organization. In the years following the test ban, the (fairly scant) coverage of the movement highlighted Pugwash's recommended disarmament measures, including a ban on the sale of uranium, a non-aggression pact between NATO and the countries of the Warsaw Pact, the postponement of the deployment of NATO ships armed with nuclear weapons, and the creation of nuclear-free zones around the world. In 1966, a *New York Times* article highlighted Rotblat's assertion that Pugwash discussions would soon result in a comprehensive nuclear test ban.[26]

New Directions for Pugwash, 1963–1966

The anticipated comprehensive test ban never happened, nor did the expected avalanche of disarmament measures. For Rabinowitch, the wind of time blew in a different direction.

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[25] Rotblat, *Quest for Peace*, 26; Wilton S. Dillon, Memo to Harrison Brown and Murray Todd, Office Files of Bill Moyers: Pugwash, Box 74 (1390), Lyndon Baines Johnson Library (hereafter LBJL).
[26] Rotblat, *Quest for Peace*, viii; "Scientists at Parley Discuss Atom Issue," *New York Times*, 15 September 1964; "Scientists' Parley Asks Antiwar Pact," *New York Times*, 21 September 1964; "Atom Ban Called Possibility Soon," *New York Times*, 12 September 1966; "New Step Is Urged To Ban Atom Tests," *New York Times*, 18 September 1966.

Disarmament, almost within the world's grasp, seemed to be slipping away when China exploded a nuclear device in 1964. Rabinowitch began to argue that disarmament, strangely, could not be achieved by pursuing disarmament. By taking Pugwash in different directions, he hoped to eliminate the forces that drove nations to arm themselves. Disarmament would come as nations learned to cooperate and develop without recourse to war or ideology.

Back in 1958, in a letter to the social critic Gilbert Seldes, Rabinowitch complained that the idea of a test ban had "been permitted to assume much more than its proper share of importance." Because a test ban was a concrete, tangible goal, scientists rallied to the cause. But Rabinowitch had refused to go along with the crowd. At Pugwash conferences, Rabinowitch called for cooperation above all else, since disarmament would only soothe the fever of the "world's sickness" without attacking its causes. Two years later, Rabinowitch penned a draft memo in which he contemplated taking disarmament off the Pugwash agenda, ostensibly because the American Academy of Arts and Sciences had begun a bilateral disarmament study with the Soviet Union, but perhaps more because he grew weary of the "endless, frustrating, all-or nothing disarmament negotiations."[27]

By the end of 1963 this attitude had taken hold to some extent in Pugwash. U.S. scientist Matthew Meselson wrote that, after the Dubrovnik conference,

> I came away feeling that Pugwash . . . had lost considerable initiative in its attempts to find ways of securing peace. The substantial lessening of international tensions and especially the signing of the Moscow Treaty left Pugwash scientists not so far ahead of the general level of thinking in these matters as they used to be.

[27] ER to Seldes, Oct. 29, 1958, Series III, Addenda I, Box 12, Folder 30: Post Third Pugwash Conference Correspondence, ER Papers; Rabinowitch, *Dawn of a New Age*, 209, 47, 94; (Draft) Memorandum on Future Development of the COSWA [Pugwash] Conferences, Series I, Box 6, Folder 20: U.S. Pugwash Continuing Committee, ER Papers.

Meselson concluded that Pugwash needed to "alter the nature of its Conferences" in order to "maximize its leverage and relevance to the international problems which remain to be solved."[28]

With a test ban out of the way, Rabinowitch seized his opportunity to push Pugwash in new directions. The problems of the world in the 1960s, he argued, could not be solved by disarmament measures, policy decisions, or anything scientists or intellectuals could devise. Instead, the world needed "a change of mentality, for the replacement of obsolete attitudes." Such a revolutionary shift would be brought about once international cooperation—previously only a "side show" to disarmament—rose to the top of Pugwash's agenda. Rabinowitch's conception of cooperation had two components. First, the developed nations of the world would assist each other on prohibitively expensive scientific programs such as space exploration, which would encourage the Cold War powers to cooperate while simultaneously advancing scientific discovery. Second, he wanted the developed countries of the world—especially the United States and the Soviet Union—to launch joint programs to aid developing countries. This aspect of cooperation would produce two tangible results. All nations, and especially those in the process of industrializing, would see material results. Furthermore, by working together, the United States and Soviet Union would end the Cold War by learning to co-exist and thus trust each other. Only then could meaningful disarmament take place.[29]

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[28] M. Meselson, "Impressions from Dubrovnik," *Pugwash Newsletter*, January 1964, Vol. 2, No. 3, 43, Addenda II, Box 1, Folder 2: 13th Pugwash Conference on Science and World Affairs, ER Papers.
[29] ER to Harrison Brown, June 10, 1964, Series IV, Addenda II, Box 6, Folder 13: General Correspondence, ER Papers; Rabinowitch, "About Pugwash," *Bulletin of the Atomic Scientists*, April 1965, Vol. XXI, No. 4, 9; Rabinowitch, *The Atomic Age*, 500; Rabinowitch, *Dawn of a New Age*, 229.

Rabinowitch seemed to have a point. Rotblat found the pace of disarmament so slow in September 1966 that he considered disbanding Pugwash, and asked the Continuing Committee to explore why Pugwash should continue. He told the committee that he "recognized the undeniable successes, but there were also many shortcomings, such as insufficient influx of fresh blood, lack of public support for Pugwash, and inactivity of many of the national groups." Rabinowitch, on the other hand, considered Pugwash as a viable vessel for his mission of cooperation. The fourteenth Pugwash conference in April 1965, held in Venice, focused on international cooperation, while the fifteenth conference, held in Addis Ababa, Ethiopia, concentrated on "Science in Aid of Developing Countries." At the Addis Ababa conference, 86 scientists represented 31 countries, with over half coming from Africa, Asia, and Latin America. Conference proposals included a "massive" training program for science teachers funded by developed countries, the encouragement of scientists from developed countries to work in underdeveloped areas, and a call for international cooperation in scientific research and conservation programs in developing countries.[30] After the conference, Rabinowitch conducted an extensive survey of the participants, and despite some complaints about the over-representation of large, developed countries and political biases, most participants embraced the idea of further conferences on and in developing nations. In 1966 Rotblat encouraged the formation of a Pugwash committee on development in order to "absorb the inevitable frustrations growing out of

[30] "Minutes of Pugwash Continuing Committee," Sept. 7–8 and 16–17, 1966, Series IV, Addenda II, Box 1, Folder 6: 16th Pugwash Conference, ER Papers; Rotblat, *Quest for Peace*, 66, 246–55.

efforts at arms control" of which Rabinowitch became chair.[31] Rabinowitch eagerly looked toward the future, though another storm awaited.

Pugwash and Vietnam, 1965–1969

By the end of 1966 Pugwash, like much of the United States, could no longer ignore the worsening situation in Vietnam. The war threatened to overwhelm the Pugwash movement by creating tensions between U.S. and Soviet scientists, and while U.S. Pugwashites ultimately adapted their goals to include finding ways to end the Vietnam War, this abrupt shift drew attention and energy away from Rabinowitch's move toward cooperation.

The war first intruded on the 1965 Venice conference, where it cast a "heavy shadow" over the proceedings, taking up much of the participants' efforts despite not being on the agenda. After a great deal of debate over possible solutions, the conference issued a relatively tame statement that scored the use of gas by the United States in Vietnam and urged the United Nations to intervene to end the war. Rabinowitch later dismissed the statement as "hardly worth the effort."[32] The following year, Pugwash's reaction to the war had already begun to obscure other goals of the movement. Although Rabinowitch intended the Addis Ababa conference to begin a new era in cooperation, it was Vietnam and disarmament that occupied the bulk of a *New York Times* editorial about Pugwash. Nothing was made of the conference's discussions of developing nations. Unlike other issues, Vietnam produced bickering at Addis Ababa. Working group five on security problems in

[31] Wilton S. Dillon memo to Harrison Brown and Murray Todd, Office Files of Bill Moyers: Pugwash, Box 74 (1390), LBJL; Rotblat, *Quest for Peace*, 67; see Y. Peter and B. Winid to ER, May 31, 1966, Series IV, Addenda II, Box 1, Folder 4: 15th Pugwash Conference, ER Papers; Paul Freund to Harrison Brown, October 28, 1966, Series IV, Addenda II, Box 1, Folder 6: 16th Pugwash Conference, ER Papers.
[32] Rotblat, *First Ten Years*, 51–52; "U.N. Urged To Lead A Year For Science," *New York Times*, 17 April 1965; Rabinowitch, "Pugwash XVI," *Bulletin of the Atomic Scientists*, January 1967, Vol. XXIII, No. 1, 43.

developing countries quibbled endlessly over the wording of their statement, debating whether to advocate the withdrawal of troops from "Vietnam" or "North and South Vietnam."[33]

Vietnam was introduced as part of the agenda for the fall 1966 Pugwash conference in Sopot, Poland. One working group was devoted exclusively to the Vietnam conflict, and most conferees skipped a trip to the opera to attend a special session on the war. Despite missing the opera, the delegates witnessed plenty of *sturm und drang* at the special session, as Soviet speakers "bitterly attacked" U.S. involvement and expressed "horror and indignation" over the war. The heated debate produced little in the way of recommendations. Although the conference made news by condemning the war, in Rabinowitch's opinion the meeting offered only empty speeches, not tangible results.[34]

Vietnam threatened Pugwash like nothing else by risking the free exchange of opinions between scientists. The war challenged what Pugwash hoped to demonstrate: that through calm, scientific discussion, any problem could be solved, any nationality transcended. Instead, the conflict in Vietnam represented transnational relations at their worst, with U.S. and Soviet scientists sniping at each other's governments and approaching each other as subjects of rival states rather than citizen-scientists of the world.[35] This brought into question the objective nature of scientists that allowed the group to approach the problems of the world without bias. Pugwash participants believed their efforts had been

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[33] "Pugwash in Africa," *New York Times*, 28 January 1966; Richard Burling to ER, Aug. 24, 1966, Series IV, Addenda II, Box 1, Folder 9: 17th Pugwash Conference, ER Papers.
[34] Rotblat, *First Ten Years*, 56; Bernard Feld memo, Sept. 29, 1966, Series IV, Addenda II, Box 1, Folder 9: 17th Pugwash Conference, ER Papers; Rotblat, *Quest for Peace*, 68–69, 262; Rotblat, *First Ten Years*, 56; Rabinowitch, Agenda for Sopot Pugwash Meeting, September 1966, Series IV, Addenda II, Box 1, Folder 7: 16th Pugwash Conference, ER Papers; "Vietnam War Condemned by Pugwash Conference," *New York Times*, 5 January 1966; Rotblat, *First Ten Years*, 206.
[35] Rabinowitch, "Pugwash," from *Retort*, undated (mid-1965), Series IV, Addenda II, Box 6, Folder 7: Disarmament, Proposed Publication On, ER Papers.

successful because influential scientists had been freed, to a large extent, from the political restraints of diplomats. But Soviet attacks on U.S. policy at Pugwash put U.S. scientists in the awkward position of being asked to defend policies they did not necessarily support. At the same time, the Vietnam War tested Pugwash's claims to represent a broad peace movement concerned with more than disarmament. Scientists might have felt at home discussing nuclear weapons payloads and inspection measures, but when it came to Vietnam, they spoke with less authority.

1966 might be characterized as Pugwash's worst year. Along with the upheaval of Vietnam, Rotblat had questioned the reasons for continuing the conferences. Frederick Seitz, president of the National Academy of Sciences (and earlier a colleague of Rabinowitch's at the University of Illinois), wanted to withdraw the NAS from participation in Pugwash. Rabinowitch himself bemoaned "amateurism, [and a] lack of proper preparation and follow-up" at conferences. Though he praised the "informal and multilateral discussion," the debates over Vietnam proved how tenuous these benefits were.[36] Battered over Vietnam and divided over pursuing disarmament or cooperation, Pugwash began a slide into near-devolution. Rabinowitch noticed that although more countries than ever had a role in Pugwash, they had begun a shift toward "more regional problems." Instead of trying to end the Cold War confrontation, Pugwash participants wanted to focus more on their own respective corners of the globe. This reflected a "weakening of forces directed at international integration, and of a renewed trend toward international anarchy and the traditional interplay between a multitude of nationalistically oriented sovereign states."

[36] See Harrison Brown to ER, June 10, 1966, Series IV, Addenda II, Box 1, Folder 5: 16th Pugwash Conference, ER Papers; Frederick Seitz to ER, June 13, 1966, Series IV, Addenda II, Box 1, Folder 5: 16th Pugwash Conference, ER Papers; and ER to Harrison Brown, June 23, 1966, Series IV, Addenda II, Box 1, Folder 5: 16th Pugwash Conference, ER Papers.

Nationalist priorities would make cooperation all but impossible. "This central hope is becoming obscured," he lamented, "and the great vision of the world community is receding." Still, Rabinowitch contended that ending the Cold War "remains the basic challenge of our age; it is the life and death problem of the human race." Pugwash must continue despite its "disappointingly slow" progress.[37]

Other Pugwash scientists noticed this slow decline. David Inglis, a Pugwash veteran since 1960, noted the "frustrations" caused by Vietnam. "I believe Pugwash will make more progress when that's over," he wrote. But rather than wait for the storm of Vietnam to blow over, Pugwash confronted the problem of the war head on. At the 1967 conference, Pugwash delegates ceased attacking each other and managed to agree on specific proposals to end the war.[38] Other Pugwash efforts went even further. In 1967 two French Pugwash scientists became "unofficial negotiators" between the United States and North Vietnam. Encouraged by Henry Kissinger, himself a veteran of Pugwash, to exploit their personal acquaintance with Ho Chi Minh, Raymond Aubrac and Herbert Marcovich embarked on a secret mission for the U.S. State Department. Because Marcovich was organizing a Pugwash symposium in Cambodia, the scientists could journey there on "Pugwash business." Through Cambodia they reached Hanoi in July 1967. According to various accounts, the Frenchmen brought back assurances that the United States could assume that North Vietnam would not use a bombing pause to supply its combatants in the south. This so-called "San Antonio formula" apparently softened U.S. reluctance to begin negotiations.

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[37] Rabinowitch, "Pugwash XVI," *Bulletin of the Atomic Scientists*, January 1967, Vol. XXIII, No. 1, 43.
[38] David Inglis to John Voss, Oct. 12, 1967, Series IV, Addenda II, Box 6, Folder 14: General Correspondence, ER Papers; Rotblat, *Quest for Peace*, 72.

Although the San Antonio plan had little immediate impact, negotiations finally began outside of Paris less than a year later.[39]

Kissinger, in fact, began to use Pugwash meetings as a vehicle through which he could conduct informal diplomacy. In 1968, at a Pugwash symposium at Marienske Lazne, Czechoslovakia, Kissinger met with the Czech foreign minister and his advisor. The Czechs channeled to Kissinger North Vietnamese demands and offered to mediate any peace talks. A Soviet Pugwash delegate also filled Kissinger in on the Soviet leadership's assessment of the war situation.[40]

In an imitation of Aubrac and Marcovich's maneuver, Cyrus Eaton, who funded the early Pugwash meetings and often attended later ones, made his own trip to North Vietnam. In Hanoi he met with Xuan Thuy, head of the North Vietnamese negotiating committee in Paris. In a letter delivered to the 1969 Pugwash conference, Eaton listed Xuan's demands, hoping that perhaps the delegates could do something about them. As the war dragged on, even U.S. Pugwash scientists began to advocate more radical measures for ending the war. A 1968 Pugwash report by Paul Doty decried the effects of the Vietnam War (and the Soviet invasion of Czechoslovakia) on non-proliferation efforts, thundering "End the Vietnam War. Get out of Czechoslovakia." The statement from the 1969 Pugwash conference advocated the complete withdrawal of U.S. forces from Vietnam as a "necessary condition

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[39] "'Secret' Mission Described," "French Engineer Silent," *New York Times*, 5 April 1968; "Frenchman Took U.S. Plan To Hanoi," *New York Times*, 9 April 1968; Robin Clarke, "Science and Technology Comment," from *Science Journal,* undated (probably 1968), Series IV, Addenda II, Box 7, Folder 1: General Correspondence, ER Papers; "Minutes for the Pugwash Continuing Committee Meeting," December 10–11, 1967, Series IV, Addenda II, Box 7, Folder 5: General Correspondence, ER Papers; George Herring, *America's Longest War: The United States and Vietnam, 1950–1975* (New York: McGraw-Hill, 1996, third edition), 196.

[40] W.W. Rostow to LBJ, Monday, May 20, 1968, 68, Czechoslovakia Memos, Vol. II, 3 of 4, Jan. 1968–Aug. 1968, National Security File, Country File, Europe and USSR, Czechoslovakia, Box 179, LBJL; 68a, Czechoslovakia Memos, Vol. II, 3 of 4, Jan. 1968–Aug. 1968, National Security File, Country File, Europe and USSR, Czechoslovakia, Box 179, LBJL; Memorandum of conversation, Henry Kissinger and Millionshikov, 17, Czechoslovakia Cables, Vol. II, Jan. 1968–Aug. 1968, National Security File, Country File, Europe and USSR, Czechoslovakia, Box 179, LBJL.

for the establishment of peace."[41] Although Pugwash scientists had placed a premium on being an avant-garde force in the world, their approach to Vietnam placed them squarely in the mainstream.

In 1965 Rabinowitch devoted a great deal of space in the *Bulletin of the Atomic Scientists* to discussion of the war, including a "Focus on Vietnam" in June. On New Year's Day 1966, Rabinowitch proposed in the *Bulletin* that the United Nations or the World Court arbitrate an end to the war. Two years later, Rabinowitch again attacked the Vietnam War and the Johnson administration in a scathing editorial. The war, he argued, diverted world attention from the challenges of peace and developing nations, wasting precious energy and resources on "struggles for political and economic domination or ideological supremacy." Recognizing that the war risked further nuclear disaster, Rabinowitch angrily moved the *Bulletin*'s clock forward to seven minutes to midnight.[42] Even though he opposed the war, Rabinowitch preferred not to use Pugwash as a method of ending the conflict. The war distracted governments and the public from the possibilities of cooperation, Rabinowitch believed, and apparently it distracted Pugwash as well. Slowly, Rabinowitch began to grow frustrated with Pugwash. In 1968 he published no articles about Pugwash in the *Bulletin*.

Members of Pugwash concerned with disarmament, on the other hand, had reasons for optimism. Although as of 1967 at least one Pugwash scientist was complaining that conferences were stagnant and that "the pros have caught up with us," Bernard Feld was anticipating in 1968 "exciting and fruitful" advances in disarmament building upon the non-

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[41] Cyrus Eaton letter, 1969, Series IV, Addenda II, Box 2, Folder 4: 19th Pugwash Conference, ER Papers; Paul Doty, "On the Current Impasse," 1968, Series IV, Addenda II, Box 2, Folder 1: 18th Pugwash Conference, ER Papers. Rotblat, *Quest for Peace*, 320–21.

[42] *Bulletin of the Atomic Scientists*, June 1965, Vol. XXI, No. 6; "World Court or U.N. Decision To End Vietnam War Urged," *New York Times*, 2 January 1966; Rabinowitch, "New Year's Thoughts, 1968," *Bulletin of the Atomic Scientists*, January 1968, Vol. XXIV, No. 1, 2–4.

proliferation treaty (NPT) that pledged each signatory power to pursue realistic disarmament proposals "in good faith." Furthermore, nuclear weapons states vowed not to provide non-nuclear powers with nuclear weapons, while non-nuclear adherents promised not to pursue nuclear weapons development. The fight against proliferation re-energized Pugwash. After much debate, the movement issued a public statement in support of the NPT, and began a critique of antiballistic missiles.[43] Rabinowitch grudgingly acknowledged the NPT, even moving the *Bulletin* clock back to ten minutes to midnight. But he refused to be impressed by arms control measures that simply froze the status quo. "The importance of the treaty is mostly symbolic," he wrote, echoing his interpretation of the Limited Test Ban Treaty nearly six years earlier. The NPT, he explained, would not lower the number of nuclear weapons, nor would it prevent the current nuclear powers from increasing their arsenals. The treaty would also do little to stop a nation from acquiring nuclear weapons should it choose to do so. The NPT, like the LTBT before it, was nothing more than "a first step." The great powers "must proceed without delay to the next one—the dismantling, gradually, of their own oversized military establishments. Otherwise the hope raised by the treaty will prove futile."[44] Rabinowitch likely anticipated that a revival in symbolic yet tangible goals like the NPT would drive Pugwash away from cooperation and back toward the tired field of arms control. Again in 1969, Rabinowitch omitted any mention of the annual Pugwash conference from the *Bulletin*'s pages. The only Pugwash material published in the *Bulletin* that year consisted of two papers from a symposium: one on "Developing Nations and Scientific

[43] David Inglis to John Voss, Oct. 12, 1967, Series IV, Addenda II, Box 6, Folder 14: General Correspondence, ER Papers; B. Feld to J. Rotblat, July 2, 1968, Series IV, Addenda II, Box 6, Folder 6: Disarmament Studies, ER Papers; Rotblat, "Movements of Scientists," in *Scientists, the Arms Race, and Disarmament*, 139–40; Rotblat, *Quest for Peace*, 272; Matthew Evangelista, *Unarmed Forces: The Transnational Movement to End the Cold War* (Ithaca: Cornell University Press, 1999), 143. On the NPT, see Wittner, *Resisting the Bomb*, 433.

[44] Rabinowitch, *Bulletin of the Atomic Scientists*, April 1969, Vol. XXV, No. 4, 48.

Responsibility," and the other on "The Scientist in Public Affairs," both subjects dear to Rabinowitch's heart.[45]

As the 1960s drew to a close, Pugwash had only just survived the crisis of the test ban. Disarmament remained the focus, while issues of cooperation and development were of secondary importance. Pugwash extended its reach, but it did not completely transform. Most importantly, the movement's ultimate goal continued to be the influencing of governments. But during its crisis years, Pugwash had become estranged from the governments it hoped to influence.

Scientists and the Johnson Administration, 1963–1969

In the United States, Pugwash had played a small part in bringing the test ban debate to a successful conclusion under the Kennedy administration. Kennedy's science advisor Jerome Wiesner had been a veteran of Pugwash before accepting his cabinet post, and continued to attend conferences after his term expired. Thus the most fruitful avenue for influencing the administration had been through PSAC and the president's special advisor.[46] While Pugwash reformulated its goals, the government changed scientists' role in policy. Whereas the Eisenhower and Kennedy administrations had relied fairly heavily on the expertise of scientists in general and Pugwashites in particular, the Johnson administration slowly began to spurn the advice of its scientific experts regarding nuclear weapons. When Lyndon Johnson ascended to the presidency after Kennedy's assassination, it remained to be seen whether Pugwash could maintain this influence. In fact, the Johnson administration generally

[45] *Bulletin of the Atomic Scientists*, November 1969, Vol. XXV, No. 9, 27. Pugwash symposia were small-scale meetings on specific topics similar to traditional academic conferences.
[46] Knopf, *Domestic Society and International Cooperation: The Impact of Protest on U.S. Arms Control Policy* (Cambridge: Cambridge University Press, 1998), 67–69, 132, 137.

turned a cold shoulder to Pugwash. Although the Office of Science and Technology (OST) remained friendly to the movement, a gap grew over time between scientists in general and the Johnson White House, mostly over the president's Vietnam policy.

An October 11, 1964, *New York Times* editorial praised Pugwash, hailing its recommendations as "important because policymakers in Washington, Moscow, and other world capitals pay serious attention to the results of Pugwash meetings." While this was somewhat true during the test ban era, Lyndon Johnson's White House made a point of distancing itself from Pugwash. On September 13, 1964, Paul Doty and Wiesner cabled McGeorge Bundy from Karlovy Vary, Czechoslovakia, requesting a message of greeting to the conference from President Johnson. As president, Kennedy had sent greetings to the Pugwash conferences, a gesture that continued at an early 1964 conference in India. But by late 1964 the situation had changed. Refusing Doty and Wiesner's request, Bundy replied that Pugwash would "survive without [the] banality of [a] repeated official blessing from on high."[47]

The actual reasons for snubbing Pugwash lay in the interplay of domestic politics and the administration's fear of criticism. In a handwritten note to McGeorge Bundy, the acting director of the State Department's Office of International Scientific Affairs explained that a message to Pugwash might harm the president during an election year "because of congressional criticisms of Pugwashery." Despite his eventual landslide, Johnson pessimistically predicted a difficult challenge from Goldwater in the 1964 election, though

[47] "Peace Moves at Pugwash," *New York Times*, 11 October 1964; Telegram from Jerome Wiesner and Paul Doty to McGeorge Bundy, Department of State, Czechoslovakia Cables, Vol. I, Nov. 1963–Aug. 1967, 23, National Security File, Country File, Europe and USSR, Czechoslovakia, Box 179, LBJL; for Kennedy's messages to Pugwash, see *Pugwash Newsletter*, January 1964, Vol. 2, No. 3, Series IV, Addenda II, Box 1, Folder 2: 13th Pugwash Conference on Science and World Affairs, ER Papers; Rotblat, *Quest for Peace*, 59; Telegram from McGeorge Bundy, Department of State, to Jerome Wiesner and Paul Doty, Czechoslovakia Cables, Vol. I, Nov. 1963–Aug. 1967, 24, National Security File, Country File, Europe and USSR, Czechoslovakia, Box 179, LBJL.

there is little evidence to suggest that he could have seriously expected to be attacked for support of the little-known Pugwash conferences. The *Congressional Record* reveals no comments upon Pugwash during either 1963 or 1964, while in 1962 Jennings Randolph (D-WV) had praised Pugwash, emphasizing his endorsement of the 1962 conference's statement that "full disarmament is realistic and urgent." Randolph then entered into the *Record* excerpts from various accounts of the conference as well as an address from the opening session.[48]

In 1961, however, the Senate halls had echoed with attacks on Pugwash in particular and disarmament in general. Senator Thomas J. Dodd (D-CT) chaired the Internal Security Subcommittee, which he used as a "base for domestic and foreign forays in his personal anti-Communist crusade." Dodd had previously made news in 1960 by hounding the Committee for a Sane Nuclear Policy as well as chemist Linus Pauling for his international petition against nuclear testing. In 1961 he continued his campaign against arms control scientists. A staff analysis by the Internal Security Subcommittee alleged that the Pugwash conferences "have been exploited by the Communist apparatus as a propaganda devise [sic]." According to Dodd, Communist scientists arrived at Pugwash conferences as "captive[s] of an inflexible political dogma," and hoped "to shape and exploit the conference in a manner which will best serve the ends of Soviet imperialism." Dodd also ripped Pugwash for its connections to the unabashedly pro-Soviet Cyrus Eaton, the industrialist who had helped fund the conferences.[49]

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[48] J. Kretzman to McGeorge Bundy, undated (Sept. 1964), 82 and 82a, Czechoslovakia Memos, Vol. I, Nov. 1963–Aug. 1967, National Security File, Country File, Europe and USSR, Czechoslovakia, Box 179, LBJL; *Congressional Record,* 1963, Appendix, A7185.

[49] "Ex-Senator Dodd Is Dead at 64," *New York Times*, 25 May 1971; Wittner, *Resisting the Bomb*, 363–64; Press Release from Senate Internal Security Subcommittee, May 28, 1961, Series II, Box 8, Folder 10: The President and Foreign Policy–Publication, ER Papers; "Senate Staff Study Hits Science Talks Promoted by Eaton," *New York Times,* 28 May 1961;

For years, the U.S. Pugwash group had been distancing itself from Eaton's "moronic mentality" and his "particularly embarrassing" support of Pugwash. In fact, after the first conference, the U.S. Pugwash group attempted to rename the organization the Conferences on Scientific and World Affairs because of the "odious" connotations of the term "Pugwash." The name change never caught on, but the presence of Cyrus Eaton became more muted over time. At any rate, Dodd's charges appeared to cause little damage to public perceptions of the movement. Rabinowitch claimed that although one oppositional editorial appeared in a local paper during the 1961 Stowe, Vermont, conference, its author celebrated Pugwash by the conference's end.[50]

Nevertheless, Dodd's accusations might have made an impression on Lyndon Johnson, the vice-president at the time after a long and influential tenure in the Senate. In fact, Johnson and Dodd had encountered each other years before. As Senate majority leader in 1959, Johnson welcomed Dodd to the Senate and immediately enlisted his support against Clinton P. Anderson, who opposed Johnson's position on a filibuster rule. Returning the favor, Johnson placed Dodd in charge of the Internal Security Subcommittee. In 1964 Johnson again called upon Dodd for a favor. Hoping to "fabricate some mystery" around his choice for running mate, Johnson involved Dodd in one of his infamous "melodramatic maneuvers." Just before flying to the Democratic National Convention in Atlantic City, Johnson met with Dodd, hinting that he was considering him for his vice-presidential running-mate. Although Johnson knew that "you couldn't do it with [Dodd]," he believed that just by floating Dodd's name he might steal some of Goldwater's anticommunist

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Congressional Record, Vol. 107, Pt. 11, 15059; U.S. Senate, Internal Security Subcommittee of the Committee on the Judiciary. *The Pugwash Conferences: A Staff Analysis.* 87th Cong., 1st sess. (Washington, D.C.: U.S. Government Printing Office, 1961).
[50] S.M. Levitas to ER, May 7, 1958, Series II, Box 9, Folder 2: Magat–Phil, ER Papers; ER to Walt Whitman, Feb. 23, 1960, Series I, Box 6, Folder 19: U.S. Pugwash Continuing Committee, ER Papers; Rabinowitch, *The Atomic Age*, 570.

thunder. After a "sham" meeting with the president, Dodd "played out the . . . act with a straight face." Telling Johnson that he lacked "Presidential qualities," Dodd endorsed Hubert H. Humphrey. Although at first glance this incident seems tangential to Pugwash, it is unlikely that Johnson, who relished such manipulation, would risk ruining his melodrama by befriending Pugwash when in need of a favor from Dodd.[51]

More to the point was Johnson's fear of domestic anticommunism. In 1965 Jim Thomson, a China specialist from the State Department, mentioned Dodd by name in a memo to McGeorge Bundy, expressing fear of a McCarthy-esque reaction if the United States failed to stand up to communist aggression in South Vietnam. Perhaps the Johnson administration preferred to give up the minor benefits of Pugwash rather than risk the wrath of Dodd; Johnson himself expressed fear that he would be "destroyed" if Dodd ever accused him of being soft on communism. Congressional opinion of Pugwash nevertheless remained positive through the 1960s. In 1966 Senator Stephen M. Young (D–OH) hailed Pugwash as "an outstanding example of peaceful cooperation," and even praised Eaton without any apparent harm.[52]

Along with fears of anticommunist backlash, Johnson believed that science properly belonged not in the realm of nuclear weapons policy and geopolitics, but in the domestic sphere as part of the Great Society, where the progress of science would elevate the standard

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[51] "Ex-Senator Dodd Is Dead," *New York Times*, 25 May 1971; Michael Beschloss, ed., *Taking Charge: The Johnson White House Tapes, 1963–64* (New York: Simon and Schuster, 1997), 471, 539, 541n.

[52] Kai Bird, *The Color of Truth: McGeorge Bundy and William Bundy, Brothers in Arms* (New York: Touchstone, 1998), 309; LBJ phone conversation with Abe Fortas, May 19, 1965, in Beschloss, ed., *Reaching for Glory: Lyndon Johnson's Secret White House Tapes, 1965–65* (New York: Simon and Schuster, 2001), 332; *Congressional Record,* Vol. 112, Pt. 15, 1966, 19441. Perhaps the Ohio Senator hoped that Eaton, one of Cleveland's wealthiest sons, would hear his praise. Dodd was censured in 1968 for using campaign funds on personal expenditures. See "Ex-Senator Dodd Is Dead," *New York Times*, 25 May 1971.

of living of all U.S. citizens.[53] Donald F. Hornig, who had replaced Wiesner as Kennedy's science advisor and chair of PSAC in late 1963 and retained his position after Kennedy's assassination, pressed Johnson to support Pugwash. In 1965 he drafted a statement to send to the spring Pugwash meeting, but the White House turned down the idea. Later that year Hornig tried again with identical results. Hornig had at the very least casual interest in the Pugwash conferences, going so far as to request the reports of the individual working groups from the Addis Ababa conference.[54] Surprisingly, when the administration did pay attention to Pugwash, officials outside of OST were most interested not in disarmament or cooperation, but in what Pugwash had to say about Vietnam. When Hayes Redman, Bill Moyers's assistant, requested a memo from the NAS on the Addis Ababa conference, Wilton S. Dillon responded with an assessment of how the suggestion of a bombing pause affected the Soviet Pugwash delegation, and included the conference's statement on Vietnam. Dillon also attached a memo on Pugwash's goals for cooperation and development.[55]

Pugwash veterans continued to serve on PSAC, although their overall numbers declined during the Johnson presidency. But when they served, they appeared to advocate the ideas of Pugwash. Between 1964 and 1965, six scientists served on PSAC who had attended at least one Pugwash conference before Johnson became president. Although the individual retained much personal initiative, it appears that these Pugwash veterans brought Pugwash ideas to the PSAC table. In late September 1965 Pugwashites Frank Long and

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[53] GBK to Morris Marden, Dec. 6, 1965, Box 33: General Correspondence ca. 1928–1982, Folder: Don Hornig, HUG (FP) 94.8, George B. Kistiakowsky Papers, Harvard University (hereafter GBK Papers).
[54] Donald F. Hornig (DFH), Memo to the President, April 8, 1965, April–June 1965, Box 2, Chronological File, Papers of DFH, LBJL; DFH to McGeorge Bundy, Dec. 23, 1965, Oct.–Dec. 1965, Box 3, Chronological File, Papers of DFH, LBJL; DFH to Bernard Feld, March 15, 1966, Jan.–March, 1966, Box 3, Chronological File, Papers of DFH, LBJL.
[55] "Report of Working Group 5," Office Files of Bill Moyers: Pugwash, Box 74 (1390), LBJL; Wilton S. Dillon to Hayes Redman, April 15, 1966, Office Files of Bill Moyers: Pugwash, Box 74 (1390), LBJL; Wilton S. Dillon, Memo to Harrison Brown and Murray Todd (also sent to Hayes Redman), April 15, 1966, Office Files of Bill Moyers: Pugwash, Box 74 (1390), LBJL.

George B. Kistiakowsky wrote to Hornig discussing their meetings with Soviet and British scientists on "the South Vietnam problem." Hornig encouraged the scientists to continue such meetings and stressed the "value of such informal contacts."[56] Paul Doty, a regular Pugwash attendee, conducted a PSAC study of Western European affairs, while Long explored "new initiatives with Eastern Europe . . . in the area of applied science and technology." Wiesner, who still served PSAC as a consultant, and Long had begun to support Rabinowitch's initiatives in cooperation and development. These efforts harmonized with the concurrent shift toward cooperation in Pugwash. Long also suggested that Hornig create a panel on "International Science" that would "search for new directions for improvement of scientific relations between the U.S. and USSR with emphasis on the development and use of non-governmental channels."[57]

As the president's science advisor, Hornig's duties ultimately reflected the president's concerns rather than those of Pugwash (or any scientists). Hornig's papers reveal that although he pursued several policies reflective of Pugwash concerns, the demands of the Johnson administration gradually pulled Hornig and PSAC further from the influence of the organization. Hornig initially supported transnational communication between the United States and the Soviet Union in hopes of preventing the development of antiballistic missiles (ABMs), and also worried about the plight of developing countries. In 1966 Hornig traveled to London to give a talk on "The Role of Science and Technology in Aid to Developing Countries," a title nearly identical to the theme of the 1965 Addis Ababa Pugwash

[56] DFH to Frank Long, Sept. 27, 1965, July–Sept. 1965, Box 2, Chronological File, Papers of DFH, LBJL; DFH to George Kistiakowsky, Sept. 27, 1965, July–Sept. 1965, Box 2, Chronological File, Papers of DFH, LBJL.
[57] DFH to Herbert York, Jan. 10, 1966, Jan.–March 1966, Box 3, Chronological File, Papers of DFH, LBJL; ER to Todd Murray, Nov. 29, 1970, Series IV, Addenda II, Box 7, Folder 10: General Correspondence, ER Papers; Frank Long to Kenneth Pitzer, Oct. 19, 1966, Oct.–Dec. 1966, Box 4, Chronological File, Papers of DFH, LBJL.

conference.[58] Another problem Hornig attempted to solve was the so-called "brain drain," the migration of physicians from the Philippines to the United States that left the Filipinos with precious few doctors. Hornig proposed extending aid to the Philippines for development, and in 1968 the brain drain listed high on the agenda for the Nice Pugwash conference. As background to a PSAC meeting on industrial and agricultural uses of nuclear power, the committee received a copy of an Alvin Weinberg paper presented at a 1967 Pugwash conference.[59]

But on matters of military and strategic importance, Hornig and the Johnson administration diverged sharply from Pugwash. In a list prepared for Johnson describing the work of the OST, Hornig put "Military technology and its interaction with strategic problems" at the top of the list, ahead of disarmament and arms control. Hornig, like Pugwash, supported the idea of a comprehensive test ban, though he paradoxically supported conducting nuclear tests in order to develop verification techniques to enforce a test ban.[60] In 1968 Hornig prepared a memo describing PSAC's accomplishments in honor of its tenth anniversary, and although arms control made the list, "military technology" achievements outnumbered any other category.[61]

The biggest disconnect between the OST and Pugwash scientists involved the growing role of science in waging the Vietnam War. As late as December 1964, PSAC meetings did not address Vietnam, but the advisory committee's role changed as soon as

[58] DFH to Joshua Lederberg, Nov. 19, 1965, Oct.–Dec. 1965, Box 3, Chronological File, Papers of DFH, LBJL; June 7, 1966, April–June 1966, Box 4, Chronological File, Papers of DFH, LBJL.
[59] DFH, Memo to Walt Rostow, Sept. 9, 1966, July–Sept. 1966, Box 4, Chronological File, Papers of DFH, LBJL; J. Rotblat, Memo to Continuing Committee, "Nice Pugwash Conference Agenda (tentative)," Feb. 19, 1968, Series IV, Addenda II, Box 1, Folder 10: 18th Pugwash Conference, ER Papers; Memorandum for Members and Consultants-at-Large, President's Science Advisory Committee, Oct. 6, 1967, Box 5, Folder: PSAC Reports, 1967, HUG (FP) 94.14, GBK Papers.
[60] DFH, Memo for Jack Valenti, May 26, 1964, April–May 1964, Box 1, Chronological File, Papers of DFH, LBJL; Oct. 8, 1965, Oct.–Dec. 1965, Box 3, Chronological File, Papers of DFH, LBJL; DFH to Solly Zuckerman, July 24, 1967, July–Sept. 1967, Chronological File, Box 5, Papers of DFH, LBJL.
[61] DFH, Memo for Marvin Watson, Feb. 19, 1968, Jan.–March 1968, Box 6, Chronological File, Papers of DFH, LBJL.

ground troops were introduced into Southeast Asia. An April 1965 PSAC meeting considered "War in South Viet Nam," a discussion that continued at meetings on June 7 and 8. By February 1966, PSAC had moved on to "Scientific and Technical Support for Military Operations in Viet Nam," and scientists' role in the war continued to fill up PSAC's agenda at meetings in May 1966, May 1968, and June 1968.[62]

Hornig's tenth anniversary memo pledged PSAC to develop new weapons technology for use in Vietnam. But as the Johnson White House began to mobilize its scientific resources for the war effort, many U.S. scientists began to oppose the war. Although PSAC had always worked on defense and military issues, Vietnam was a different matter, and forced Hornig into an awkward position. As Pugwash and other scientists increased their criticisms of Johnson's Vietnam policy, Hornig found himself trying to keep the scientific community from revolting against the White House while simultaneously trying to enlist scientists for the war effort. A *New York Times* article in late May 1967 described Johnson's growing disconnect from the intellectual community. Hornig wrote to Johnson about the article, warning him not to "discount the interest and influence of the scientific community . . . . They are among the most worried and hard to deal with in connection with Viet Nam and we continue to need their support." Just a few weeks later an OST scientist terminated his government contract in protest of the war. Elsewhere Hornig had to defend himself against charges of denying a spot to William Taylor on PSAC's Innovation in Education panel because of the professor's opposition to the Vietnam War.[63]

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[62] PSAC December 14, 15, 1964 meeting agenda, Box 3, Folder: PSAC, 1964—All Correspondence; PSAC June 7, 8, 1965 meeting agenda, Box 3, Folder: PSAC, 1965—All Correspondence; PSAC Feb. 14, 15, 1966 meeting agenda, Box 4, Folder: PSAC, 1966—All Correspondence; PSAC May 16, 17, 1966 meeting agenda, PSAC May 20, 21, 1968 meeting agenda, PSAC June 17, 18, 1968 meeting agenda, Box 4, Folder: PSAC, 1966–1969, HUG (FP) 94.14, GBK Papers.
[63] Max Frankel, "Intellectuals to Johnson: War's the Rub," *New York Times*, 22 May 1967; DFH, Memo to the President, May 29, 1967, April–June 1967, Box 5, Chronological File, Papers of DFH, LBJL; Donald Greenspan to DFH, June 8,

In order to bridge the growing divide between the administration and scientists, Hornig tried one last time to convince Johnson to send a presidential greeting to Pugwash in 1967. Hornig assured Johnson that "Expressing a friendly interest in the work of an organization which represents a good cross-section of the international scientific and intellectual community interested in world affairs would help improve your image in the intellectual world." As before, no message was sent. But even as he tried to convince Johnson to reach out to scientists, Hornig played his own part in driving the two sides apart. The same year that Pugwash recommended an immediate and unconditional bombing halt followed by peace negotiations, Hornig formed an Ad Hoc PSAC Vietnam Group. This group's contribution to ending the "problem" of Vietnam would be through the "application of technology [capable of] improving our ability to secure and hold areas and hamlets."[64] Clearly by this point Pugwash and the OST were working at cross-purposes.

A letter from scientist Sidney Drell, the chair of PSAC's Ground Warfare Panel, to Hornig in 1967 reflects the active role government science advisors took in pursuing weapons technology during the Vietnam War, but also the Great Society mandate Johnson had given his science officials. Expressing a mindset that new weapons technology would end the war (not dissimilar to many scientists' feelings about the A-bomb and radar during World War II), Drell told Hornig that he was worried about "increased talk . . . of engaging in 'hot pursuit' across boundaries into Cambodia and Laos and across the DMZ." Before such tactics were used, Drell hoped "that technology be given a chance to show what it can

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1967, April–June 1967, Box 5, Chronological File, Papers of DFH, LBJL; see Aug. 7, 1967, July–Sept. 1967, Box 5, Chronological File, Papers of DFH, LBJL.

[64] DFH, Memo to Sec. Katzenbach, Aug. 16, 1967, July–Sept. 1967, Box 5, Chronological File, Papers of DFH, LBJL; DFH to W.W. Rostow, Aug. 26, 1967, July–Sept. 1967, Box 5, Chronological File, Papers of DFH, LBJL; Rotblat, *Quest for Peace*, 293–94; DFH, Memo to members of the PSAC Ad Hoc Vietnam Group, August 25, 1967, July–Sept. 1967, Box 5, Chronological File, Papers of DFH, LBJL.

offer for improved border security." At the time, Drell and other top U.S. physicists consulted for the Institute for Defense Analysis as part of a group known as the Jasons. Their attempts to solve the Vietnam War with technology came from their belief that the Rolling Thunder bombing campaign was ineffective and needlessly cruel. With new "sensor technology," Drell wrote,

> I would hazard the guess that those areas of the Cambodian border which front near the population centers could be better surveilled and large enemy troup [sic] movements across this border into Viet Nam could be identified and their likely retreat routes back across the border determined and blocked with a commitment of resources far less than would be required of us to extend the actual fighting base across the Cambodian border or across the DMZ.[65]

Drell also expressed concern about "a large introduction of American fighting forces into the Mekong Delta on search and destroy sweep operations." Drell thought that a more useful tactic would be to "uproot and erode the VC infrastructure economically" rather than use force. Echoing Johnson's rhetoric about providing a Great Society for South Vietnam, Drell asked, "What possibility is there of rallying the farmers by the AID type of programs directed at raising their standard of living so that they will want to commit themselves to the Saigon government?" Drell also mentioned "new secondary school vocational and technical programs which seem to be badly needed in a developing nation" as well as "a more vigorous program of refugee retraining" in Vietnamese refugee camps. Finally, Drell wondered about "a national TV communication network" that would provide the South Vietnamese with "entertainment as well as information and education to increase interest in, awareness of, and the bond to the government by the villagers." Turning to domestic issues,

[65] "Excerpt from Dr. Drell to Dr. Hornig, December 30, 1967," PSAC 1971: Members and Consultants, Box 5/5, HUG (FP) 94.14, GBK Papers. On the Jasons and the Vietnam War, see Ann Finkbeiner, *The Jasons: The Secret History of Science's Postwar Elite* (New York: Viking, 2006), 62–117.

Drell mentioned "urban renewal or slum clearance projects" and ways "science and technology can offer to assist crime prevention and control."[66]

As the war further devolved into a quagmire in 1968, Hornig found himself working more and more for the war effort rather than the peace effort. He defended the war, "regrettable as it is, [as] vital to the interest and security of this Nation and the World." He assigned Drell's Ground Warfare Panel to work on problems associated with "pacification/ revolutionary development." He then promised Johnson "an intensive effort on Viet Nam" which included improving war technology in Vietnam because "some brands of [the] latest technology make us look pretty silly." One of the OST's accomplishments during this period was the "Snoopy" helicopter that carried a television camera for use at Khe Sanh.[67]

Even though Hornig continued to work toward closing the division between the Johnson administration and scientists, the damage had been done. In March 1968 Kistiakowsky resigned his position as an advisor to the Pentagon in opposition to the war. Although Kistiakowsky remained a PSAC consultant, his move reflected heavy disenchantment among the academic community with Johnson. In spring 1968 PSAC counted just two Pugwash veterans among its members, and one of those, Frederick Seitz, happened to be president of the NAS, who automatically sat on the committee. Instead,

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[66] "Excerpt from Dr. Drell to Dr. Hornig, December 30, 1967," GBK Papers.
[67] DFH to J. James Moore, Jan. 19, 1968, Jan.–March 1968, Box 6, Chronological File, Papers of DFH, LBJL; DFH to Robert W. Komer, Jan. 31, 1968, Jan.–March 1968, Box 6, Chronological File, Papers of DFH, LBJL; DFH, Memo for the president, March 7, 1968, Jan.–March 1968, Box 6, Chronological File, Papers of DFH, LBJL; DFH to John F. Foster, March 15, 1968, Jan.–March 1968, Box 6, Chronological File, Papers of DFH, LBJL; DFH, Memo to the president, April 10, 1968, April–June 1968, Box 6, Chronological File, Papers of DFH, LBJL. Apparently "Snoopy" suffered from "reliability problems": DFH to John S. Foster, May 20, 1968, April–June 1968, Box 6, Chronological File, Papers of DFH, LBJL.

representatives of industry and business served on the committee, including an executive of Ford and two industrialists.[68]

With the Johnson administration reluctant to acknowledge Pugwash and growing ever more obsessed with the war in Vietnam, scientists' avenues for influencing U.S. Cold War policy narrowed during the 1960s. After the high point of Pugwash's influence in 1963, the movement and the government moved in fairly opposite directions regarding Vietnam. Hornig had sympathy for Pugwash and shared many of the organization's goals regarding development. In fact, at certain points Pugwash goals harmonized with OST policy, especially in development and cooperation. But Vietnam stifled this potential partnership. As for Pugwash's antinuclear goals, disarmament was gradually removed from the OST's agenda—just as Rabinowitch expected. In fact, PSAC's influence declined severely after Wiesner left the post of chief science advisor. According to Matthew Evangelista's analysis, Pugwash had more influence with Soviet officials than the U.S. government at this time.[69]

Pugwash and the British Government, 1963–1969

Its founders meant for Pugwash to be an international effort to influence world governments, and not just that of the United States. Accordingly, Pugwash consistently targeted British policy, though the government largely resisted its efforts. In fact, for most of the 1960s, the situation was practically reversed, as the British government expended a fair amount of effort trying to influence Pugwash. In July 1963, as Averill Harriman traveled to

[68] DFH, Memo for Marvin Watson, April 18, 1968, April–June 1968, Box 6, Chronological File, Papers of DFH, LBJL; Evert Clark, "Top Scientist Cuts All Links To War," *New York Times*, 1 March 1968; DFH to LBJ, Dec. 8, 1967, Oct.–Dec. 1967, Box 5, Chronological File, Papers of DFH, LBJL; DFH, Memo for Marvin Watson, February 19, 1968, Jan.–March 1968, Box 6, Chronological File, Papers of DFH, LBJL.
[69] Herbert York and Allen Greb, "Scientists as Advisers to Governments," in *Scientists, the Arms Race, and Disarmament*, 94; See Evangelista, *Unarmed Forces*, 8.

Moscow to negotiate what would become the Limited Test Ban Treaty, a member of the British Foreign Office expressed confidence that Pugwash conferences were "valuable," and that with an atmospheric ban looming, "the views of scientists on the remaining problems of underground identification will obviously be crucial."[70] For the 1963 Dubrovnik conference, the Foreign Office and Ministry of Defence (MOD) science advisor Solly Zuckerman asked the British Pugwash delegation to take along several MOD officials in order to encourage official progress in an unofficial atmosphere. Eager to influence policymakers but wary of appearing too official, Rotblat could only be "persuaded to accept" a single official. Determined to get Pugwash to promote official Western policy positions, the British government subsequently attempted to influence the selection (and mindset) of Pugwash participants, both in order to get more done and to keep British scientists from contradicting official diplomats, which handed the Soviets propaganda victories. But because of the "delicate" temperament and "strong and independent views" of British Pugwash scientists, the Foreign Office had been unable to influence the delegates, with the exception of John Cockcroft, a consultant to the Atomic Energy Authority who accepted "a confidential briefing."[71]

British government officials continued to look to their U.S. counterparts for guidance in making policy toward Pugwash. A 1963 communiqué from the Foreign Office requested the names of the U.S. Pugwash delegates, and asked if the government had planned any attempts to plant official views into the Pugwash conference.[72] A telegram from

[70] D.E. Tatham, "Pugwash," Minutes, July 8, 1963, FO 371/ 171190; IA D1092/ 31: 11th International Pugwash Conference Dubrovnik, restricted Pugwash Meetings, 1963, UK National Archives.
[71] A.D.F. Pemberton-Pigott, Sept. 10, 1963, 1, FCO 371/ 171190; IA D1092/ 30: 11th International Pugwash Conference Dubrovnik, restricted Pugwash Meetings, 1963, UK National Archives.
[72] A.D.F Pemberton-Pigott to J.E. Killick, Sept. 10, 1963, FO 371/ 171190; IA D1092/ 22: 11th International Pugwash Conference Dubrovnik, restricted Pugwash Meetings, 1963, UK National Archives.

the British embassy back to the Foreign Office stated that U.S. officials were taking a "very relaxed" attitude toward the Dubrovnik conference, but that the Kennedy administration did hope that scientists would drop disarmament and other political hot potatoes in favor of "non-controversial subjects" such as cooperation on development in the Third World. In addition, the U.S. Arms Control and Disarmament Agency had briefed Matthew Meselson before he left for the Dubrovnik conference.[73]

After Dubrovnik, British officials debated the value of the conferences, in the process revealing that their interest in Pugwash was based not on respect for the scientists, but in the potential intelligence and propaganda gains to be exploited. B.T. Price, the MOD official who had attended the conference, argued that the future inclusion of scientific delegates briefed on official Western policy would improve "the present deplorably low standard of discussion in formal sessions." But in contrast to the formal sessions, the "real virtue" of Pugwash, Price wrote, "is in providing a window on the communist world—not the one-way window of which we have heard so much, but a two-way window through which we can both observe the communists and allow them to sample life in the free world." Price described the conferences a type of "diplomatic pearl-fishing." While Western governments did little to influence Pugwash, "The Russians carefully interlard their own scientists with influential political, diplomatic and military observers. Perhaps if we followed their example we might in time manage to land more pearls," he suggested.[74] Another official told Rotblat after the Dubrovnik conference that "we wanted to keep the Pugwash Conferences alive as a means of contact with the Russians, although they might not be so

[73] Telegram to Foreign Office, Sept. 16, 1963, FO 371/ 171190; IA D1092/ 31: 11th International Pugwash Conference Dubrovnik, restricted Pugwash Meetings, 1963, UK National Archives.
[74] B.T. Price letter, "Pugwash," Oct. 16, 1963, FO 371/ 171190; IA D1092/ 39: 11th International Pugwash Conference Dubrovnik, restricted Pugwash Meetings, 1963, UK National Archives.

useful now." In response, Rotblat apparently responded that he would permit a government briefing on "the political dimensions of the disarmament problems which they intended to discuss" at the upcoming Pugwash conference in India. Finally, the official told Rotblat that he hoped Pugwash "might turn to more general scientific subjects and away from the disarmament field." Confronted with this view from both British and U.S. officials, Rotblat resisted.[75]

In an analysis of the 1964 Pugwash conference, one Atomic Energy and Disarmament official wrote that Pugwash had probably outlived its usefulness, though "On the other hand, any contacts between East and West have a value of their own; and I think it would be the general view that these meetings are worth continuing—not that we are consulted in any way." Two other officials argued that with proper briefing "of what political and propaganda advantages the Russians can be expected to try to get from each meeting" the conferences might be of value to the British and U.S. governments.[76]

In 1965, British officials continued to debate the merits of Pugwash. In January, members of the Foreign Office "agreed that we should do all we could to prevent the next Pugwash meeting from going off the rails." These efforts included attempting to persuade British Pugwash delegates "at least to resist political conclusions damaging to the West" and "to take a more sensible view on disarmament questions." Officials also hoped to "insinuate a Foreign Office representative into the British delegation."[77] One month later, U.S. opinions were taken into account. The Foreign Office learned that the U.S. State

[75] A.D. Wilson, "Pugwash Conferences: Briefing of British Delegation," Dec. 5, 1963, FO 371/ 171190; IA D1092/ 45: 11th International Pugwash Conference Dubrovnik, restricted Pugwash Meetings, 1963, UK National Archives.
[76] Minutes, "Pugwash Conference," Sept. 19, 1964, FO 371/ 176423/ IA D1092/ 18: Pugwash Conference on Science and World Affairs, 1964, UK National Archives.
[77] J.A. Thomson, Minutes, "Pugwash," Jan. 22, 1965, FO 371/ 181421; IA D1092/ 2: Pugwash Conferences on Science and World Affairs, 1965, UK National Archives.

Department did not send a representative to Pugwash so as to more easily "disown the conclusions of a particularly unsatisfactory meeting," though the State Department did "discreetly" brief U.S. participants. U.S. officials also hoped that Pugwash would become "more scientific in character and less political," a "change of direction" that the Foreign Office heralded as "overdue." Finally, the State Department had considered cutting off funds for Pugwash if the meetings continued to spout anti-Western statements.[78] One British proposal wondered that, if unable to suitably influence the proceedings, "whether it might be better to try to kill the organisation by indicating to the responsible British participants that we no longer favour it as a means of international discussion." An internal response to this suggestion argued that the Foreign Office would be unable to "kill" Pugwash "unless the Americans agreed," and, repression of Pugwash might backlash by further encouraging British scientists to attend, thereby handing an easy propaganda victory to the Soviets.[79] The MOD, on the other hand, thought Pugwash was "of value in providing continuing contacts with Communist scientists, and through them in influencing the Soviet government," though the Foreign Office considered the claim "doubtful."[80]

A Foreign Office report on the 1965 Venice Pugwash meeting stated that the conference "could have turned out to be a good deal more embarrassing to us," and was possibly even a little bit "helpful." Still, one official bemoaned the very idea of scientists serving as "political amateurs" as they failed to achieve an "intellectual, let alone scientific,

[78] D.L. Benest, Minutes, "Pugwash," Feb. 22, 1965, FO 371/ 181421; IA D1092/ 7: Pugwash Conferences on Science and World Affairs, 1965, UK National Archives.
[79] Staff memo, Feb. 15, 1965; F.W. Mottershead letter, February 24, 1965; FO 371/ 181421; IA D1092/ 8: Pugwash Conferences on Science and World Affairs, 1965, UK National Archives.
[80] H.B. Shepherd, "Pugwash Conferences," Minutes, March 2, 1965, FO 371/ 181421; IA D1092/ 21: Pugwash Conferences on Science and World Affairs, 1965, UK National Archives.

examination of the questions."[81] On May 7, 1965, Rotblat and Rudolf Peierls of British Pugwash met with MOD officials to discuss the proceedings of the Venice conference. (Peierls had been briefed before Venice.) The scientists told the MOD that they had sensed a "restive" feeling in the Soviet delegates, giving them hope that Soviet participants might speak more freely at future conferences. While Peierls was irritated by the immense amount of time spent over political propaganda and the lack of "really constructive work" at the meeting, Rotblat argued that "this was part of the price that had to be paid to get the Soviet Government to allow its scientists to participate." Apparently Rotblat and Peierls "both said that they would genuinely welcome suggestions from H.M. Government for subjects that might be seriously pursued" at Pugwash. One MOD official suggested that Pugwash could help with "studies of scientific aspects of disarmament proposals which the Western Powers are advocating in disarmament negotiations."[82]

As the 1966 Pugwash conference approached, talk in the British government turned from influencing the general direction of the organization to ensuring that participants accurately expressed the official line on specific issues. In particular, the British government worried that "the Rhodesia crisis may well become an important topic at the meeting . . . and views critical of British policy may be expressed." Rotblat and the Foreign Office hashed out a solution where a government expert essentially stood on call to answer any questions that Rotblat had about Rhodesia while the conference was going on.[83]

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[81] D.L. Benest, April 29, 1965, H.B. Shepherd, May 3, 1965, Minutes, FO 371/ 181422; IA D1092/ 23: Pugwash Conferences on Science and World Affairs, 1965, UK National Archives.
[82] H.B. Shepherd, "Review of the Pugwash Conference, Venice, April 1965," May 7, 1965, FO 371/ 181423; IA D1092/ 25A: Pugwash Conferences on Science and World Affairs, 1965, UK National Archives.
[83] Telegram, "From Foreign Office to Addis Ababa," Dec. 24, 1965; Tom Bridge to J.O. Wright, December 23, 1965; PREM 13/ 310: Foreign Policy, 1965, UK National Archives. See also Minutes, "Pugwash," FO 371/ 187510; IA D1092/ 1: Pugwash Meetings, 1966, UK National Archives.

After that small crisis was resolved, the Foreign Office found itself "back where we were last year" on the Pugwash conferences. One suggestion was to offer Rotblat a "friendly warning" that Pugwash scientists must take an "impartial and scientific approach" to world and European security at the upcoming Sopot, Poland, conference.[84] But what government officials really wanted was for Western Pugwash participants to voice a pro-Western and anti-Soviet line. Accordingly, the MOD thoroughly briefed British Pugwashers Rotblat and Hermann Bondi with unclassified information on British policy toward disarmament, Rhodesia, and other topics before the Sopot conference.[85] The Foreign Office expressed pleasure that Bondi and his fellow Pugwashers Baron Flowers and William Gutteridge had "close contacts with the Ministry of Defence," while "Peierls and Rotblat are well-known to us and well disposed." The Foreign Office had also briefed "some" of the U.K. delegates on official political and disarmament policies, though it hoped to keep this effort secret, since "the scientists who go to Pugwash strongly resent any suggestion of official guidance and our briefing activity should not therefore be revealed."[86]

Hedley Bull of the Foreign Office attended the Sopot meeting, and, after witnessing the proceedings, strongly doubted whether Pugwash was of any use to the government. He qualified his skepticism by noting that the "appalling . . . standard of accommodation and entertainment" at the conference "had an effect on my attitude." But the rest of the conference had fared no better in Bull's opinion. He had been especially disappointed in one Sovietologist "who seems to be inhibited from even being tough by a personal love of the

[84] P.L. O'Keefe, "Pugwash," Minute, March 2, 1966, FO 371/ 187510; IA D1092/ 3: Pugwash Meetings, 1966, UK National Archives.
[85] "Pugwash, 1966," July 8, 1966, FO 371/ 187510; IA D1092/ 14: Pugwash Meetings, 1966, UK National Archives.
[86] D.L. Benest to D. Tonkin, Aug. 24, 1966, FO 371/ 187511; IA D1092/ 25: Pugwash Meetings, 1966, UK National Archives.

Russians and anxiety lest they do not accept his application to study arms control at the Academy of Sciences." Later, Bull commented on Rotblat, who he described as "very genuinely impartial, and cares only about holding together the Pugwash movement of which he regards himself as the conscience, guardian and physical embodiment, as well as the Secretary-General. This makes him very tiresome, but he has no axe to grind other than that of Pugwash itself." In sum, Bull recommended "playing down our links with" Pugwash and ending the briefing and debriefing sessions, which only "have the effect of inflating their sense of their own importance without, so far as I can judge, causing their interventions in the Pugwash discussions to be any the wiser."[87] The Foreign Office decided to continue briefing Pugwash scientists in the hope of improving the conferences, but ended the debriefings so as not to give the impression that the government cared what went on. The Foreign Office wanted to express to Pugwash a "slackening" of interest in the movement, but did not fundamentally change its official position on the scientists: "restrained interest in, and encouragement for, the ideals for which Pugwash stands, tempered by some disappointment at the way in which the movement has so often allowed itself to be diverted from its true purposes."[88]

At a time when U.S. officials were distancing themselves from Pugwash, the British government attempted to be more accommodating to the scientists. While not overly impressed by the conferences, the British government tried gently prodding the organization's British participants toward taking the state's views. At first, British Pugwash scientists jealously guarded their independence, a stance which promised to prevent the

[87] Hedley Bull, "The Sixteenth Pugwash Conference on Science and World Affairs," Oct. 21, 1966, FO 371/ 187511; IA D1092/ 25: Pugwash Meetings, 1966, UK National Archives.
[88] J.E.D. Street to Rennie, Dec. 1, 1966, FO 371/ 187511; IA D1092/ 26A: Pugwash Meetings, 1966, UK National Archives.

scientists from meeting their professed goal of influencing the government. Perhaps realizing this, Pugwash scientists began to accept guidance from the British government. In the United States, no such accommodation was possible, as the Vietnam War and partisan politics drove scientists and government officials apart.

U.S. Scientists under Nixon

Despite the schism between scientists and the Johnson administration, many politically engaged scientists saw a Republican presidency in 1968 as an even greater evil. Thus many former science advisors exerted their influence in an attempt to defeat Richard Nixon's presidential bid. Jerome Wiesner agreed to head a scientific advisory panel for Democratic candidate Hubert Humphrey's campaign, and Eugene Rabinowitch wrote to the *New York Times* endorsing Humphrey and blasting Nixon for "stemming the flood of change." Bernard Feld went even further, framing the election as a "life-or-death" decision between Humphrey and Nixon, whose plans for a nuclear build-up "would place us squarely on the road to national and international disaster." Kistiakowsky also penned a letter to the *Times* endorsing Humphrey. Hans Bethe became a founding member of Scientists and Engineers for Humphrey in 1968, despite receiving a form letter from Republican and former AEC chair Lewis Strauss inviting him to join his organization for Nixon and Agnew.[89]

As the election drew near, Bethe penned a letter to Sidney Drell in October 1968 regarding a planned TV discussion between Drell and Humphrey. Possibly remembering

[89] "Humphrey Appoints Panel to Advise Him on Science," *New York Times*, 17 August 1968; Rabinowitch, "Humphrey's Assets," *New York Times*, 2 October 1968; Bernard Feld, "Life-and-Death Issue," *New York Times*, 2 November 1968; Detlev Bronk and George Kistiakowsky, "Humphrey Backed," *New York Times*, 3 November 1968; Peter Bing to Bethe, Oct. 15, 1968; Lewis Strauss to HB, Oct. 11, 1968; Folder 17.5: Scientists for Humphrey, 1968, Box 17, Hans Bethe Papers, Cornell University (hereafter HB Papers).

Humphrey's heavy involvement in the test ban debates, Bethe urged Drell to emphasize arms control in the discussion rather than such Johnson pet projects as "Technological contributions to the plight of the cities." Bethe's primary arms control interest was ratification of the NPT, but he also mentioned ABMs and MIRVs. Hoping to prevent these advances in weaponry, he told Drell, "The experience has been that it is very difficult to eliminate a weapon once it has been introduced, but that it may be possible to avoid introducing it." Outlawing a weapon before its creation was what Bethe and other scientists had hoped to do with the H-bomb in 1949. "So the time to act is now," he wrote, "and Humphrey is the only likely candidate to act constructively." On arms control as well as R&D money for science, Bethe concluded "we are likely to be better off with Humphrey than with Nixon."[90]

After Nixon won the election, scientists found themselves even more distant from the new administration. Nixon appointed Lee DuBridge of Caltech as his science advisor, and although the choice "delighted" Kistiakowsky, DuBridge failed to revive the role of scientists in the policymaking realm. Certainly DuBridge seemed less likely to do so: In 1958 he had written to Rabinowitch expressing a desire to stay out of political causes. "I am one of those," he wrote, "who feel that things would have been better if [scientists] had stayed out of the so-called civilian control of atomic energy fight in 1945–46, for I think a better bill . . . had been proposed than the one which was finally passed."[91] Eventually, DuBridge grew "disaffected" with Nixon and resigned amid complaints from the scientific community that

[90] HB to Drell, Oct. 17, 1968, Folder 7.42: [Correspondence] –D–, Box 7, HB Papers.
[91] R.W. Apple Jr., "DuBridge To Aid Nixon On Science," *New York Times*, 4 December 1968; Robert Reinhold, "Scholars Praise 2 Nixon Choices," *New York Times*, 4 December 1968; Lee DuBridge to ER, Jan. 31, 1958, Box 8, Folder 19: DuBridge–Einstein, ER Papers. DuBridge's comments had some truth in them. The MacMahon bill contained stricter penalties—including death—for revealing atomic secrets. Although Rabinowitch himself characterized the atomic scientists' movement as a "failure," he placed the blame not on scientists, but rather on the statesmen who failed to institute international control of atomic energy. Rabinowitch, *The Atomic Age*, 7.

the White House had failed to adequately fund U.S. science. Nixon replaced DuBridge with electrical engineer E.E. David, but this failed to placate the administration's scientific critics. In early 1972 *Physics Today* railed that no Nixon administration official was qualified to speak for the scientific community. Feeling "less than useful," David resigned in January 1973. Later that month Nixon abolished PSAC altogether, claiming budgetary restrictions. A *New York Times* editorial conjectured that "it seems plain that an element in the Nixon decision was the President's resentment at the large number of prominent scientists who opposed him on the Vietnam war and on many other issues," including the Supersonic Transport and ABMs.[92]

As the Vietnam War dragged on, increasing numbers of scientists were distancing themselves from work with military applications in general. A declaration by MIT scientists stated, "Through its actions in Vietnam our government has shaken our confidence in its ability to make wise and humane decisions. There is also disquieting evidence of an intention to enlarge further our immense destructive capability." Criticizing the "small group that helps to conceive these policies" and the "handful of eminent men who have tried but largely failed to stem the tide from within the government," the MIT scientists stated that "We feel that it is no longer possible to remain uninvolved." They proposed turning research away from military technology and "towards the solution of pressing environmental and social problems." They also hoped to "convey to our students the hope that they will devote themselves to bringing the benefits of science and technology to mankind, and to ask them

[92] Glenn Seaborg, *The Atomic Energy Commission Under Nixon* (New York: St. Martin's Press, 1993), 5–6; Herbert York and Allen Greb, "Scientists as Advisers to Governments," in *Scientists, the Arms Race, and Disarmament*, 94; Richard D. Lyons, "DuBridge Resigns as Nixon's Science Adviser; Computer Expert Named," *New York Times*, 20 August 1970; "Physics Journal Decries Role of Scientist in Capital," *New York Times*, 5 January 1972; Richard D. Lyons, "Science Adviser to Nixon Leaving for Industry Job," *New York Times*, 3 January 1973; "Downgrading Science," *New York Times*, 7 February 1973.

to scrutinize the issues raised here before participating in construction of destructive weapons systems," as well as organize scientists and engineers for political action, harnessing "their desire for a more humane and civilized world."[93] This cry came from radically inclined scientists who called for a research strike on March 4, 1969, and in the aftermath, formed activist groups including the radical Science for the People and the more moderate Union of Concerned Scientists. Calling for an end to exploitive science that fed upon social, racial, and sexual inequality, many scientists (often younger and not physicists) formulated an ethos that declared their independence from militarism, but also one that diminished their public authority as objective experts.[94]

During the Nixon years, scientists had almost no role within the administration, making it nearly impossible for Pugwash to influence the Nixon White House. Although Henry Kissinger and Nixon preferred to keep disarmament negotiations, and foreign policy in general, under their own control, Kissinger had attended seven Pugwash conferences as either a participant or an observer. But he never acknowledged the organization. In 1963, when Pugwash's "black box" proposal attempted to break the deadlocked test ban talks, Kissinger wrote to the *New York Times* in order to distance himself from the group.[95]

Pugwash carried on nevertheless and continued to make occasional news. Pugwash's plan for an end to the arms race made the front page of the *New York Times* in 1969. Far from a slow news day, the article on Pugwash shared the headlines with the Supreme Court's order to use busing to enforce school integration, and the placement of Bobby Seale in

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[93] "Against the Misuse of Science—An Appeal By M.I.T. Scientists," *Bulletin of the Atomic Scientists*, March 1969, Vol. XXV, No. 3, 8.
[94] See Kelly Moore, *Disrupting Science: Social Movements, American Scientists, and the Politics of the Military, 1945–1975* (Princeton: Princeton University Press, 2008), 158–89.
[95] See Seaborg, *The Atomic Energy Commission Under Nixon*; Joseph Rotblat, "'Black Box' Discussion," *New York Times*; 25 January 1963; Henry Kissinger, "Reference to 'Black Box,'" *New York Times*; 13 March 1963.

chains at the Chicago Eight trial. Meanwhile, in recognition of his many years of service, Eugene Rabinowitch was elected president of Pugwash in 1970. His address to the annual conference tried to steer Pugwash toward cooperation in the pursuit of world peace. The "danger" of nuclear war "had abated," he asserted, "even if it is by no means permanently eliminated." Instead, "Pugwash has a more creative role to play—that of exploring and implementing constructive cooperation between all parts of mankind." Pugwash, he declared, must work on "narrowing the gap between the rich and the poor nations," and "preservation and restoration of the healthy habitat, endangered by unthinking pursuit of industrialization." More than anything else, however, "development of the underdeveloped countries is the key to the future of mankind." As always, Rabinowitch closed on an optimistic note. "In approaching the end of my involvement in Pugwash," he announced, "I know that I will, at best, see some light at the end of the tunnel."[96]

Rabinowitch's optimism was misplaced. A cynical Canadian student at the conference asked the scientists if all their talk achieved anything at all. After the conference, Rabinowitch wrote to Rotblat, explaining that "My last ambition in connection with Pugwash is to help initiate a viable Pugwash program in development." He then warned Rotblat about the fate he had feared for Pugwash since 1963:

> Another danger to Pugwash which we have to avoid is its becoming utterly institutionalized and ritualized as a series of friendly annual social affairs with no pioneering in critical problems of man's future—and development is in my mind, the most important one of them.[97]

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[96] Walter Sullivan, "Pugwash Experts Offer Arms Plan," *New York Times*, 30 October 1969; Rabinowitch, "Speech for Lake Geneva Conference," 1970, Series IV, Addenda II, Box 3, Folder 10: 20th Pugwash Conference, ER Papers.
[97] *Chicago Tribune* article, September 20, 1970, Series IV, Addenda II, Box 3, Folder 11: 20th Pugwash Conference, Press Clippings, ER Papers; ER to J. Rotblat, Nov. 17, 1970, Series IV, Addenda II, Box 7, Folder 10: General Correspondence, ER Papers.

But Rabinowitch believed he was speaking to deaf ears. He wrote in late 1970 about the "recalcitrant attitudes when it comes to discussing development at Pugwash." Although some scientists agreed on the urgency of development (notably Wiesner and Long), the Soviet members of Pugwash stifled true advances. The "backwardness" of Soviet delegates, as well as disapproval from "their bosses in the Kremlin," prevented progress on a development program. The Soviet Union, he wrote, did not want to deal with development because doing so would imply that coexistence with capitalism was possible, and the Soviets are "sensitive about soiling their pure white Marxist clothes." After venting his frustrations, Rabinowitch announced that he was "rotating out" of the Continuing Committee in early 1971. Late that same year he suffered a heart attack and less than two years later he died.[98]

Pugwash and the British Government after 1969

By the late 1960s, the British government had arrived at a fairly neutral policy toward Pugwash. A member of the Foreign Office described the "current official attitude towards Pugwash . . . to be that, while keeping aloof from their organised activities, we are glad to maintain touch with the British group, both in anything they have to tell us and to give such advice as they may seek."[99] But officials worried that any public association with the organization would reflect poorly on the government. In April 1970, MP Lord Chalfont had agreed to participate in a Pugwash discussion on CS and other poison gases, where government officials expected him "likely to be very critical" of current policy. The Prime Minister's office asked him to reconsider his participation, stating that "during the pre-

[98] ER to Todd Murray, Nov. 29, 1970, Series IV, Addenda II, Box 7, Folder 10: General Correspondence, ER Papers; ER to Carl Djerassi, Jan. 22, 1971, Series IV, Addenda II, Box 7, Folder 10: General Correspondence, ER Papers; B. Feld, Memo to ER, Dec. 8, 1971, Series IV, Addenda II, Box 7, Folder 8: General Correspondence, ER Papers.
[99] "1969 Pugwash Conference, Sir T. Brimelow," FCO 66/40, DS 4/ 10, 4, UK National Archives.

election period, the prime minister would like ministers to avoid if possible any engagements that will be very controversial." As an excuse, the aide suggested "A possible pretext would be that this is the date for the prorogation of parliament." An ensuing telegram from Lord Chalfont's office voiced the MP's assent: "Lord Chalfont agrees that it would be better for him not to participate in the Pugwash meeting. Grateful if you would explain this to the organisers, expressing his great regret since the date of the meeting was changed specifically to enable him to participate, and citing the prorogation of parliament as the reason for this decision."[100]

In 1971, the British government continued to express regret that they were unable to directly influence Pugwash proceedings. A Foreign Office official noted that while the agency offered briefings to Pugwash on an ad hoc basis, "the British scientists who attend Pugwash are always suspicious of official attempts to influence their thinking." In fact, it had been "years" since the Foreign Office had briefed anyone. "It would be very useful if we could discreetly pass on to some of those attending our disappointment at the way in which recent meetings have been exploited for anti-Western propaganda," J.M. Hagy wrote. He hoped to "plant certain topics which could be raised to exploit Western positions viz a viz those of the Russians."[101] A 1973 government review of Pugwash activities described the conferences as "notorious for their banality and for mingling distortions, half-truths and platitudes." By 1976, arms control officials approved of "informal contact" with Pugwash, but did not wish to give "any kind of governmental blessing to its proceedings," and even

[100] "Pugwash Group Meeting on CS Gas," R.W. Renwick, Lord Chalfont's sec., to Benest, Disarmament Department, April 23, 1970; "Telegram to New York, UK Mission, May 21, 1970," 11; "Telegram to Foreign And Commonwealth Office, May 22, 1970," 12; FCO 66/ 178, DS 4/6, UK National Archives.
[101] J.M. Hagy, Nov. 23, 1971, FCO 66/ 264; DS 4/4: Disarmament: Conferences and Meetings, 1971, UK National Archives.

though one official wrote that "We continue to keep in touch with Pugwash thinking," he doubted that the conferences would influence policy at all.[102]

ABM: Test Ban Redux

As the importance of Pugwash diminished, developments in arms control briefly promised to return scientists to political relevance. The controversy over ABMs in the late 1960s and 1970s followed the routine for scientific debate over nuclear weapons and arms control established during the test ban. In the mid 1950s, new weapons (ICBMs) had driven some scientists toward disarmament efforts, while other scientists (notably Teller) had risen to defend these new weapons. This conflict had resulted in a weak arms control measure that failed to dampen the arms race (the LTBT). And so the ABM debate played out in similar fashion, with opponents and proponents of the new weapons system offering technical reasons for and against it, resulting in an arms control treaty that did little to change the course of the Cold War.

Less than two years after ratification of the LTBT, Teller had mobilized to counter criticism of ABMs. During the test ban debate, Teller had named ABMs as one reason not to sign a test ban, arguing that the treaty would hamper U.S. development of the defensive weapons. Teller offered several arguments in favor of developing missile defense. Worried that French President Charles de Gaulle was eroding the NATO alliance, he offered that "joint work on developing and installing missile defense in the NATO countries may help to restore the purpose and unity of the Atlantic Alliance." A second pitch for missile defense involved a plethora of imagined future threats coming from China, including "big nuclear

[102] M.F. Cullis, "Pugwash," Jan. 5, 1973; B. Richards to M.A. Fakenham, Jan. 2, 1976, FCO 66/ 721: Arms Control and Disarmament, 1976, UK National Archives.

explosives" delivered "from ocean-going vessels" that could be detonated in "any big city in the world." "If this situation should arise," he declared, "it will be possible for the Chinese to blackmail either us or our NATO allies." No technology could prevent "an all-out Russian attack," he said. "It seems, however, highly probably that a missile defense can be developed to insure all big cities in the Atlantic Alliance against the relatively small number of crude missiles that the Chinese are likely to possess. It seems to me reasonable, in fact almost necessary, to insure ourselves against independent Chinese blackmail."[103]

In 1967, Teller continued to reference the Chinese threat and NATO tensions in his defense of ABM development and deployment. But ABMs would also be of value against the Soviet Union. At a public discussion, Teller's audience applauded when he stated: "The Russians are not given to adventure. If we have an Anti-Ballistic Missile system which will call into question the efficacy of their attack, this in itself will greatly contribute to deterrence. I want retaliatory power but I claim that retaliatory power is not enough and that we need ballistic missile defense not instead, but in addition."[104] Elsewhere Teller presented a further argument for ABM deployment: "such a measure would be the best procedure to shield the United States from the consequences of nuclear proliferation."[105]

At congressional hearings in 1969, each presidential science advisor except for Nixon's testified against ABM: Killian, Kistiakowsky, Wiesner, and Hornig. Other expert witnesses included York, Bethe, Panofsky, and Drell. They argued that "ABM components were poorly designed to do the job for which they were assigned, and even if they performed

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[103] ET to McGeorge Bundy, May 10, 1965, Folder 3, Reading File, April–June 1965, Box 424: Chronological Correspondence, 1962–66, Edward Teller Papers, Hoover Institution (hereafter ET Papers).
[104] ET, "Needed Now: Transatlantic ABM Defense—And Union," *Freedom & Union*, November 1967, Vol. 22, No. 11, 4, Folder 5.3: Speeches and Writings, "Needed Now," Box 5, ET Papers.
[105] ET, Draft Statement on ABMs, undated, Folder: Anti-Ballistic Missiles, Statements, Box 443: Topic Files, ET Papers.

as intended, they would only provide protection over a very narrow range of threats over a very limited period of time."[106]

As he had during the test ban debate, Teller challenged the credibility of his ABM opponents by questioning their objectivity. In a letter to Sharon Weldon, a graduate student in political science who was apparently writing on arms control, Teller wrote,

> Those who are opposed to ballistic missile defense today are mostly the same people who have opposed United States defense measures in the past. One may mention as examples civil defense and the development of the hydrogen bomb. . . . It should be emphasized, however, that in the arguments considerable emphasis is placed on technical statements concerning lack of practicality of the defense system. . . . it is my opinion that these technical arguments happen to be fallacious and are to a great extent motivated by political, rather than by technical considerations.[107]

Hans Bethe, by now a member of the old guard of arms control scientists, also wrote to Weldon, explaining that his reasons for opposing ABMs "were both technological and strategic. . . . Technologically, we believe the system to be ineffective. Strategically we think it will greatly stimulate the arms race. . . . There are no moral reasons against ABM."[108]

But the absence of moral arguments against ABMs meant that when the political or strategic environments changed, those weapons became acceptable as alternatives to even newer, more destabilizing weapons. In 1978, Bethe wrote to Herbert York about the MX missile. "I am appalled by the concept of the MX," he wrote. "Surely if we go in this direction, the Soviets can and probably will do so as well." Bethe recognized the pattern of the arms race, where new systems only created new threats. But his technical opposition to nuclear weapons put Bethe in a pattern of basically accepting whatever happened to be the

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[106] Thomas A. Halsted, "Lobbying Against the ABM, 1967–1970," *Bulletin of the Atomic Scientists*, April 1971, Vol. XXVII, No. 4, 26.
[107] ET to Sharon Weldon, undated, Folder: ABM's, Box 444: Topic Files, ET Papers. See also ET to Harold M. Wynne, April 25, 1969, Folder: ABM's, Box 444: Topic Files, ET Papers.
[108] Bethe to Sharon Weldon, March 17, 1969, Folder 8.8: [Correspondence]–W– (2), Box 8, HB Papers.

previous generation of weapons. "I remember when the multiple warheads were first conceived and introduced," his letter to York continued. "From the beginning it seemed to me highly destabilizing, and in our Strategic Weapons Panel of PSAC we pretty well agreed that this was so. After the brief interval during which MIRV gave us superiority, we now must be afraid of it in the hands of the Russians. Do we want to repeat this, on an even larger scale with MX?" Given these facts, the formerly scorned ABM seemed like a good option. "If we believe that it is essential to preserve all three arms of the Triad, it looks to me that the ABM defense of selected missile sites is still very preferable to the MX," he wrote. "It may even be cheaper."[109]

The First Step is the Easiest: Scientists and Arms Control, 1956–1972

Thwarted in their own efforts, mainstream antinuclear scientists vicariously enjoyed conventional achievements on nuclear weapons, rooted in their technical approach to arms control. Scientists' papers, publications, and personal correspondence between the late 1950s and early 1970s exude the distinct belief that if the United States and the Soviet Union could just reach one cooperative agreement, take one first step, real disarmament would follow. Initially, when a nuclear weapons test ban became a political issue in the 1956 U.S. presidential election, many antinuclear scientists had jumped on the idea and predicted that the test ban would be the crucial first step toward disarmament.

During the presidential primary campaign in the late summer and fall of 1956, the Federation of American Scientists endorsed a nuclear test ban "[a]s a preliminary step

[109] HB to York, Oct. 9, 1978, Folder 22.18: Test Ban, Box 22, HB Papers.

toward complete and universal enforceable disarmament."[110] The eventual Democratic candidate, Adlai Stevenson, proposed a nuclear test moratorium during the general election as "a beginning, a starting point, a way to get off the dead center of disagreement" over the "means of taming the nuclear weapons."[111] Such language permeated the test ban debate. Rabinowitch, not personally disposed toward a test ban, nevertheless begrudgingly listed some justifications for a test ban, including "that the test ban could be a relatively easy first step toward controlled disarmament."[112] When the superpowers signed the Limited Test Ban Treaty in 1963, Rabinowitch echoed his earlier sentiment in an editorial titled "First Step—To Where?" In moving the *Bulletin's* famous Doomsday clock a few minutes back, he explained that "the limited test ban is a first step," hopefully, though not certainly, "toward progressive disarmament and readjustment of the threatening deployment of the two camps."[113]

As the years passed after the test ban treaty, disarmament, rather than appearing one step closer, seemed as far away as ever.[114] But hope sprang anew when the superpowers reached agreement on the NPT in 1968. A *Bulletin* article in June of that year by Indian physicist Indu Shekhar Mishra argued that "All well-meaning peoples in the world realize the dangers of a nuclear war and are sincerely in favor of a nonproliferation treaty. In fact," Mishra emphasized, "an effective treaty will be the first major step toward nuclear disarmament."[115] When the U.S. Senate finally ratified the NPT in March 1969, Rabinowitch

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[110] "The Nuclear Weapons Test Ban," *Bulletin of the Atomic Scientists*, September 1956, Vol. XII, No. 7, 268.
[111] "Test Ban Dialogue," *Bulletin of the Atomic Scientists*, November 1956, Vol. XII, No. 9, 324.
[112] Eugene Rabinowitch, "Nuclear Bomb Tests," *Bulletin of the Atomic Scientists*, October 1958, Vol. XIV, No. 8, 282–83.
[113] Rabinowitch, "Editorial: First Step—To Where?" *Bulletin of the Atomic Scientists*, October 1963, Vol. XIX, No. 8, 2–3.
[114] In "From Activism to Apathy," Paul Boyer explains how the treaty, by satisfying the public's desire to "see something done about the arms race," had actually ended antinuclear activism rather than bolstering it.
[115] Indu Shekhar Mishra, "The NPT is Not Enough for India," *Bulletin of the Atomic Scientists*, June 1968, Vol. XXIV, No. 6, 4.

hailed the treaty in these exact terms. Rabinowitch first announced that the Doomsday clock would be set back by 3 minutes, to 10 minutes to midnight. Then, perhaps forgetting what he had written about the test ban treaty, he declared: "The great powers have made a first step. They must proceed without delay to the next one—the dismantling, gradually, of their own oversize military establishments. Otherwise the hope raised by the treaty will prove futile."[116]

Apparently the hope did prove futile, as by the 1970s, scientists continued to search for that first step. A June 1972 *Bulletin* editorial by Feld praised the SALT agreements as "only the first step" in what would be further disarmament. Feld concluded "Bravo—all that warrants setting back the clock!" The Doomsday countdown, despite the many repeated attempts at a first step, now stood at a calming 12 minutes to apocalyptic midnight.[117] In comedic coincidence, Rabinowitch reprised his old metaphor in an article just a few pages later. He wrote: "movement toward disarmament and stable peace . . . must start with stabilization of both the arms race and the political conflicts between the two nuclear powers." Building up to the unintentional punch-line, he exclaimed: "And the chief importance of the Moscow agreements is that they represent first steps toward such a freeze."[118]

Along with arms control, détente also threatened to make Pugwash irrelevant. Rotblat resigned as secretary-general in 1973, reasoning that with détente in full swing, Pugwash was no longer the only East-West communication channel. A 1974 paper by the

[116] Rabinowitch, "NPT: Movement Toward a Viable World," *Bulletin of the Atomic Scientists*, April 1969, Vol. XXV, No. 3, 48.
[117] Feld, "Looking to SALT-II," *Bulletin of the Atomic Scientists*, June 1972, Vol. XXVIII, No. 6, 2.
[118] Rabinowitch, "The Moscow Summit," *Bulletin of the Atomic Scientists*, June 1972, Vol. XXVIII, No. 6, 50.

Polish Pugwash group declared that the Cold War "has been completed."[119] Historians have pointed out the oddity that as nuclear protest in general declined after 1963, many arms control measures were passed during the era of détente, including a non-proliferation treaty, the establishment of nuclear free zones, and limits on strategic arms. Lawrence Wittner explains this occurrence as public policy "catching up" with the antinuclear movement. Geir Lundestad, on the other hand, argues that the United States initiated arms control agreements during the 1970s as a way of slowing U.S. decline in world leadership.[120] Looking at the arms control agreements of the 1960s and 1970s through the eyes of Pugwash scientists makes it hard to credit Wittner's point of view. They found it especially painful that arms control agreements had failed to bring peace; a joint statement by the U.S. and Canadian Pugwash groups castigated the movement's impotence. Pugwash had been "foolishly complacent" in believing that détente would lead to disarmament. Instead, atmospheric tests by the Chinese and French, underground tests by the British, Soviets, and Americans, and India's successful nuclear test were "a rude awakening." They predicted that "a new cycle in the arms race is about to begin."[121]

The scientists of Pugwash perversely had to look forward to the end of détente to play an influential role again. In an interview, Bernard Feld, head of Pugwash after Rotblat, explained that during détente, Pugwash "seemed less and less interesting, because there were a lot of channels for discussion. Now that we're back in a period of freeze, this becomes important again." Herbert York, a science advisor for the Carter administration, began

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[119] J. Rotblat, "Report on Pugwash Activities During the Period 1967–72," *Pugwash Newsletter*, January 1973, Vol. 10, No. 3, 72–81, Swarthmore College Peace Collection (hereafter SCPC). Rotblat kept close ties to Pugwash, and he and Pugwash won the Nobel Peace Prize in 1995; "Background Paper by the Polish Group," *Pugwash Newsletter*, July 1974, Vol. 12, No. 1, 7. Great Britain, Collective Box, SCPC.

[120] See Boyer, "From Activism to Apathy"; Wittner, *Resisting the Bomb*, 415; Lundestad, *United States and Western Europe*, 193.

[121] "The Uncontrolled Atom: A Crisis of Complacency," by Canadian and US Pugwash groups. *Pugwash Newsletter*, July 1974, Vol. 12, No. 1, 30–32, SCPC.

negotiating a comprehensive test ban at 1979 and 1980 Pugwash meetings.[122] Pugwash scientists endorsed a nuclear weapons Freeze in the early 1980s, and the organization even held a workshop on using the media to prevent nuclear war. Even Mikhail Gorbachev noticed the revived scientists' movement, writing:

> I regard the various Pugwash initiatives as part of the arduous search for an end to the dangerous nuclear stalemate. I recall in particular their warning in the early 1980s against newly fashionable strategies of "limited nuclear war" and speculations about the "advantages" of a nuclear first strike. I can say today that the courageous and well substantiated statements of Western scientists and opinion leaders supplied me and my supporters in the Soviet Union with fresh arguments in our efforts to effectively reduce the level of military confrontation.[123]

But as the 1980s dawned, the Reagan administration dropped casual references to "winnable" nuclear wars and the grassroots antinuclear movement swelled and soon left scientists' technical approach to disarmament movement behind. Soon, East and West were meeting at the negotiating table and Cold War divisions began to break down. The scientists' movement faced obsolescence yet again. Attending his first Pugwash conference in 1988, the Soviet scientist and dissident Andrei Sakharov found the meetings "mediocre" and "self-absorbed." Recognizing that the conferences might be "worthwhile," Sakharov concluded: "let Pugwash do its work. But without me!"[124] But Pugwash's decline was not the end of scientific challenges to the arms race. The urgency of the Cold War in the 1980s called forth new scientists outside traditional realms such as Pugwash.

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[122] "LINK Interview: Prof. Bernard Feld," *LINK*, Sept. 21–Oct. 4, 1981, 6–7, Great Britain, Collective Box Pr–Ri: Pugwash Conferences on Science and World Affairs (org.), London 1957–1983, SCPC; York, *Making Weapons, Talking Peace: A Physicist's Odyssey from Hiroshima to Geneva* (New York: Basic Books, 1987), 248.
[123] "Second Pugwash Workshop on Averting Nuclear War: The Role of the Media," *Pugwash Newsletter*, January 1982, Vol. 19, No. 3, 127–32, SCPC; Gorbachev, "Working for a Humane Society," in Maxwell Bruce and Tom Milne, eds., *Ending War: The Force of Reason* (New York: St. Martin's Press, 1999), 17, 20.
[124] Sakharov, *Moscow and Beyond: 1986 to 1989* (New York: Alfred A. Knopf, 1991), 63–64.

Chapter Six: "An Elaborate Way of Committing National Suicide": Carl Sagan, Popularization, and Nuclear Winter

In the early 1980s, antinuclear activists swarmed across North America and Europe. Protesters and ordinary citizens in the United States rallied around calls for a Nuclear Freeze, an arms control proposal that would instantly halt the arms race. Scientists shared in the revived antinuclear movement, including some a generation removed from the old guard of the Manhattan Project. No one exemplified these new voices better than the planetary biologist Carl Sagan, who moved politicians and the public alike to oppose the escalating arms race. But as with other antinuclear iconoclasts, the state relied on new and old strategies of containment to silence this nuclear dissent.

Mission to Mars

Carl Sagan arrived at the Senate Caucus Room in Washington on December 8, 1983, to participate in a forum convened by Senators Edward Kennedy and Mark Hatfield. The exobiologist and public face of astronomy had come to speak on the severe disruption a nuclear war would inflict on the global climate, a disastrous effect he and his colleagues had dubbed "nuclear winter." In so doing, Sagan culminated a journey that had begun over a decade earlier and millions of miles away—on Mars. As part of the NASA team controlling the *Mariner 9* mission to the red planet in 1971, Sagan followed the spacecraft into Mars orbit. "We arrived at the planet—I say 'we', but it was an unmanned spacecraft," he explained at the forum; "the spacecraft arrived at the planet and permitted us to watch what

was happening on Mars."[1] Sagan vicariously returned to Mars in 1976 when the *Viking* spacecraft landed on the planet's surface to collect samples, run experiments, and take pictures. "I personally spent in a certain sense a year on Mars in the course of that mission," Sagan later recalled.[2] Although the experiments dashed Sagan's hopes by finding no signs of life, the photographs provided him a particular thrill. At one point, he fashioned a large panoramic photo of the Mars horizon into a cylinder with the photo facing inward, and placed it around his head. With this crude but effective virtual reality device, Sagan took himself to Mars. Coming along for the "ride" was *Rolling Stone* journalist Timothy Ferris; the two men took turns gazing at the Martian landscape, silent and awestruck.[3]

Sagan also spent a great deal of time analyzing the data sent back from Mars. During its orbit of the planet, *Mariner 9* had encountered a "global dust storm" and recorded the odd combination of a warm atmosphere but a cool planetary surface. With little else to pass the time as they waited out the dust storm, Sagan and his colleagues began to crunch numbers, hoping to explain the oddity. The *Viking* mission confirmed that during a dust storm "the temperature [on Mars] drops considerably," leading Sagan's group to conclude that "fine particles in a planetary atmosphere" cooled the surface temperature. Intrigued, the scientists wondered if a similar temperature drop could occur on Earth. Computer models soon showed that a volcano could theoretically cool the Earth's surface if a major eruption

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[1] "Washington Forum on the World-Wide Consequences of Nuclear War," *Disarmament: A Periodic Review by the United Nations*, Vol. VII, No. 3, Autumn 1984, 39; Philip Shabecoff, "U.S.-Soviet Panel Sees No Hope in an Atomic War," *New York Times*, 9 December 1983, A13.
[2] Carl Sagan, *The Varieties of Scientific Experience: A Personal View of the Search for God*, Ann Druyan, ed. (New York: Penguin, 2006), 195.
[3] Keay Davidson, *Carl Sagan: A Life* (New York: John Wiley and Sons, 1999), 281; William Poundstone, *Carl Sagan: A Life in the Cosmos* (New York: Henry Holt and Company, 1999), 205.

released enough particulate matter into the atmosphere.[4] Eventually, the scientists realized that the explosion of nuclear weapons could also cause a similar effect through the deposit of tremendous amounts of dust and, through the burning of cities and forests, smoke and soot into the atmosphere. After further calculations, the Sagan group estimated that nuclear war could trigger "a dense hemisphericwide pall of sooty smoke" over the northern half of the globe and "quick freezes" in the south, leaving the planet "in the cold and the dark."[5] Alarmed by the findings, Sagan journeyed to Washington where he told Congress, "there are no sanctuaries in a nuclear war." Indeed, the extinction of mankind was a possibility. During hearings on nuclear winter, when Sagan projected a slide onto the wall portraying the post-nuclear war United States and Soviet Union engulfed in flames and blanketed by soot, he intoned, "One cannot tell from this figure who started the war. And it hardly matters."[6]

Assessing Nuclear Winter

Whereas Linus Pauling's test ban movement stalled over government hostility and scientific skepticism, Sagan's nuclear winter campaign appears to have made a better impression with scientists and the U.S. government. Not that Pauling's campaign was a flop; the Nobel Prize-winning chemist mobilized public opinion and put great pressure on the Eisenhower and Kennedy administrations as well as governments around the globe. Unlike nuclear winter, Pauling's efforts bore fruit in the form of the 1963 Limited Test Ban Treaty which, although

[4] Indeed they postulated that such an event had already occurred when the Tambora volcano eruption in 1815 kept temperatures so low that 1816 came to be known as the "year without a summer." See Sagan and Richard P. Turco, *A Path Where No Man Thought: Nuclear Winter and the End of the Arms Race* (New York: Random House, 1990), 95–101.
[5] Paul Ehrlich et al, *The Cold and the Dark: The World after Nuclear War* (New York: Norton, 1984), 3–4, 88; Sagan and Turco, *Path*, 39.
[6] U.S. Congress, International Trade, Finance, and Security Economics Subcommittee of the Joint Economic Committee. *The Consequences of Nuclear War*. 98th Cong., 2nd sess. Hearings, July 11 and 12, 1984, (Washington, D.C.: U.S. Government Printing Office, 1986), 4–11.

it did little to slow the arms race, ended the fallout contamination which Pauling so despised. For his efforts, Pauling earned a Nobel Peace Prize, in the process becoming the first and only person to receive two unshared Nobels. But the politics of the Red Scare limited what Pauling could do. The U.S. government restricted his travel overseas in the 1950s, and Pauling's campaign was costly to the scientist himself; he later admitted that his activism reduced his scientific output. Furthermore, many of his peers accused Pauling of lacking objectivity, a claim that historians have repeated.

Sagan by contrast had the good fortune of being active at a less virulently anticommunist time in U.S. society, and his celebrity in a media age also helped his message spread. But just as important, Sagan made a priority of maintaining nuclear winter's scientific credibility by running the theory past many scientists, including those most skeptical of his theory and his politics. A scientist as well as a popularizer of science, Sagan used both approaches to fuel his political activism. While Pauling had alienated many prominent scientists with his test-ban petition, Sagan made a more concerted (though not always successful) effort to establish the scientific *bona fides* of nuclear winter.

Historians have had little to say about nuclear winter.[7] In his epic history of the world disarmament movement, Lawrence Wittner never mentions the theory or its charismatic proselytizer. Those who have analyzed the theory have done so in an incomplete manner. Historian of science Lawrence Badash argues that Sagan had to take nuclear winter to the public because the Reagan administration was hostile to the theory.[8] But while the executive branch did resist Sagan's efforts, the planetary astronomer received an enthusiastic reception from Congress, where the theory had substantial influence on debates over arms

[7] One reason for this might be that Carl Sagan's papers are not open to the public.
[8] Lawrence Badash, "Nuclear Winter: Scientists in the Political Arena," *Physics in Perspective*, Vol. 3, 2001, 76–105.

control, and provided scientific evidence for challengers of nuclear deterrence and the arms race. The concept of nuclear winter played a part in the breakdown of the deterrence consensus in the 1980s, as the bare facts of science repudiated the Cold War logic that had upheld deterrence since the 1960s.

Badash also argues that Edward Teller countered Sagan as a molder of public opinion. But he overstates Teller's role; while Teller was an obvious critic, his opposition was hardly surprising. Rather, it was the opposition of other, more anonymous scientists that truly restricted the reception of the nuclear winter theory. Badash argues that the 1985 Department of Defense (DoD) report recommending the Strategic Defense Initiative (SDI) as protection against nuclear winter resulted in an "ultimate null influence of the nuclear winter debate." "The Pentagon thus co-opted nuclear winter," Badash declares, concluding that scientists face great difficulties in trying to influence policy.[9] But Badash fails to recognize the role scientists played in the negation of nuclear winter. Taking his message to the public actually harmed Sagan's image among some of his peers. Because the nuclear winter theory was explicitly linked to contemporary politics, some scientists refused to endorse it as science, hindering its widespread acceptance. Despite earning the accolades of many scientists by subjecting his theory to the usual and rigorous standards of pure science, his popularization efforts alienated other scientists, resulting in a blunting of the theory's impact.

Some biographers and associates of Sagan have characterized his nuclear winter campaign as evidence of some extraordinary passion or fortitude. Richard P. Turco, who worked with Sagan on the theory, explained that Sagan had the "guts" to take on the defense

[9] Badash, "Nuclear Winter," 84, 100–01. Poundstone also argues that the Reagan administration co-opted nuclear winter, *Sagan* 349.

establishments of both the United States and Soviet Union. In Turco's words, Sagan went "wading into the bog of nuclear politics searching for dragons to slay."[10] Similarly, in an otherwise entertaining and informative biography, Keay Davidson tells a caricatured version of the nuclear winter debate, framing it as a Sagan vs. Goliath story. According to Davidson, nuclear winter "directly challenged" the Reagan administration's gospel of winnable nuclear wars. "It is for this reason," Davidson writes, "that the Reagan Administration, right-wingers and nuclear weaponeers and strategists . . . reacted so furiously to the nuclear winter hypothesis and its glamorous champion, Carl Sagan." But from a scientific perspective, Davidson feels compelled to admit that "The fate of the nuclear winter hypothesis remains contested."[11] Sagan, however, never set out to prove definitively that a nuclear winter would happen. Rather, his studies convinced him that a nuclear winter was a possible outcome of nuclear war. This possibility, in Sagan's mind, put the burden of proof on the Reagan administration to either prove that nuclear winter could not happen, or change U.S. nuclear policy to ensure that it did not. Instead, Sagan's political opponents took advantage of the contested nature of the theory in order to avoid both confronting the possibility of nuclear winter and changing nuclear policy.

William Poundstone similarly describes nuclear winter as an embarrassing misadventure in Sagan's complex scientific life. In his biography of Sagan, Poundstone introduces nuclear winter as "one of the most troubling demonstrations of the so-called relativity of scientific truth." Poundstone links nuclear winter to a "new mingling of science and politics," though a brief survey of modern science would suggest that such overlap is far from new. Furthermore, Poundstone responds to Sagan's 1990 assertion that time had borne

[10] Yervant Terzian and Elizabeth Bilson, eds., *Carl Sagan's Universe* (Cambridge: Cambridge University Press, 1997), 239.
[11] Davidson, *Sagan,* 359, 379.

out the theory by claiming that "By then, that statement flew in the face of most public opinion." Public opinion is of course fickle, but scientific opinion seems to have largely confirmed the nuclear winter theory by the 1990s. Poundstone writes that Sagan was part of just one "group of distinguished scientists" advocating the nuclear winter thesis, while an "equally distinguished group said almost the opposite."[12] But such a claim does not hold up to close examination—even Edward Teller, the longtime nemesis of the antinuclear movement, admitted that the hypothesis had at least some validity. Finally, Poundstone argues that the study of nuclear winter was too hasty to be any good, and quotes a NASA scientist to that effect: "I really think they [Sagan and his fellow scientists] decided beforehand what the conclusion should be, and then selected their assumptions to lead to this conclusion."[13] If this assessment is accurate, then Sagan fooled a tremendous number of scientists, scholars, and political experts. Nuclear winter endured intense scientific scrutiny, and while the theory may have been a scientific veneer for Sagan's antinuclear beliefs, valid science supported his arguments. Furthermore, working backward from a possible event (in this case nuclear winter) to determine ways to prevent it (by reducing nuclear arsenals) may not be the strictest application of the scientific method of observation and description, but it is a perfectly rational way to avoid a disaster. In fact, experts from engineers to national security strategists often take such an approach.

A significant segment of scientists attacked Sagan when the theory appeared. Many of these opponents cared less about Sagan's science than his presentation of the science to the public and his subsequent political stands. These critics believed that using his science to

[12] Poundstone, *Sagan*, 292–93.
[13] The scientist is Kathy Rages of NASA Ames. Poundstone also quotes Freeman J. Dyson as writing that nuclear winter was "sloppy." Poundstone, *Sagan*, 334–35.

make a political argument tainted his results and, by association, science itself. Sagan's failure to adhere to the strict and conservative mores of the scientific discipline cost him allies, and resulted in the theory being excessively muted and disputed. While Sagan argued that science spoke urgently to the world, many scientists asked him to keep quiet, stressing that science should not seek out public or political influence.

Carl Sagan: Science in the Counterculture

Sagan's activist roots ran deep and reflected his coming of age amidst the 1960s counterculture. According to his biographers, Sagan always held liberal political beliefs. His activism began early on, though the Democratic Party canvassing he did during graduate school was apparently required by his advisor, Subrahmanyan Chandrasekhar. Despite this early peonage, Sagan took quickly to activism. He traveled to Alabama to teach science seminars at the Tuskegee Institute during the civil rights era, and spoke out against the Vietnam War as the conflict escalated in the mid-1960s. Sagan also turned down projects funded by the military, stating, "I certainly don't want to be a party to this crime." As the anonymous "Mr. X" he praised the mind-expanding powers of marijuana in the early 1970s; Davidson describes Sagan's 1977 Pulitzer Prize-winning book *The Dragons of Eden* as "obviously written under the inspiration of marijuana." Despite these countercultural leanings, Sagan refrained from speaking out against nuclear weapons during the late 1960s and 1970s, even as a heated debate raged over ABMs. In Davidson's words, "The safest scientific foes of militarism were those who, like [Hans] Bethe, Linus Pauling, and others,

were already famous. Sagan probably didn't feel famous enough to take the risks they did. Not yet."[14]

According to Davidson, Pauling was an influence on Sagan as a scientist and an activist. On a trip to California at 19 years of age, Sagan whimsically decided to stop in "unannounced" on Pauling at Caltech, to run his theory of the generation of life by the great chemist.[15] Decades later, Sagan and his third wife, Ann Druyan, met with Pauling, widowed after a long and mutually empowering marriage to Ava Helen Pauling. When Druyan mentioned Ava Helen, Pauling apparently began to sob for his departed wife who had inspired his activism. Davidson suggests that Druyan played a similar role in Sagan's activism after their marriage in 1981. Scientists also noted the parallels with Pauling; Freeman Dyson said of Sagan, "He is as much a hero of our times as Pauling was a hero of the fifties."[16]

Observers of Sagan, from wives to biographers to former colleagues, discuss his reverence and love for science as almost religious in nature. Sagan himself described science as "informed worship." Though an outspoken atheist, Sagan looked to science to fulfill personal and philosophical aspects of life for which many other people turned to religion, including the search for meaning in and understanding of the world, as well as moral guidance.[17] As a planetary astronomer, Sagan knew the vastness of the universe, in addition

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[14] Davidson, *Sagan,* 76–77, 194, 234, 288; see also Poundstone, *Sagan*, 98–103.
[15] Davidson, *Sagan*, 64–65, 77. Such chutzpah was not unique to Sagan. In 1955 he and his future wife Lynn Margulis drove to New Jersey. While Sagan met his family in Rahway, Lynn made a solo trip to Princeton, where she looked up J. Robert Oppenheimer in the phone book, soon after showing up unannounced at the elder physicist's residence. What they discussed may have centered around Lynn's recent term paper, "Not 'Whether or Not' but 'How?': J.R. Oppenheimer and the Decision to Drop the Bomb." Poundstone, *Sagan*, 31. Margulis went on to have a very successful career as a biologist; her work focused on the Gaia hypothesis that described the Earth as a living being.
[16] Davidson, *Sagan*, 316, 372.
[17] Sagan, *Varieties*, 31; Davidson, *Sagan*, xii, 1, 237, 260, 336; and Poundstone, *Sagan*, 291, 388. Sagan addresses the connections and oppositions between science and religion in *Varieties*. Also see "Editor's Introduction," by Ann Druyan, in *Varieties,* ix–xiii. For example, Sagan thought scientific analysis yielded answers to moral and social problems such as abortion. See Sagan, *Billions and Billions: Thoughts on Life and Death at the Brink of the Millennium* (New York: Random House, 1997), 164–78.

to humanity's isolated and powerless place in it. Since "we're on our own . . . nothing can save us," he liked to say. In an existentialist rationale, Sagan suggested making life meaningful by accomplishing something valuable, such as ending poverty, hatred, or inequality.[18]

Sagan was equally intent on being scientific and encouraging a scientific rigor in other realms of life, including religion and politics. He told an interviewer in 1976 that science is "littered with dead theories," which he interpreted as a sign of progress, evidence that scientists have to correct their theories and admit their mistakes. "I'd like to see politicians willing to admit that their ideas have been wrong and now they'll adopt a new one which will work better," he explained.[19] Sagan passionately and aggressively defended the objective name of science by relentlessly attacking pseudoscience such as "evidence" of UFOs. Perhaps this conspicuous skepticism was necessary because at various times in his life he had entertained some fairly wacky ideas, including UFOs as a young man.[20] He had also pondered whether the moons of Mars were actually disguised alien vessels, theorized that "balloon animals" floated above Venus, and enthusiastically endorsed and promoted radioastronomer Frank Drake's equation predicting the number of advanced civilizations in the Milky Way galaxy, $N=R_*f_pn_eff_il_iL$.[21] When Sagan did enter the nuclear weapons debate, he was careful to appear as scientific as possible.

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[18] Terzian and Bilson, *Carl Sagan's Universe*, 147, 155, 160.
[19] Tom Head, ed., *Conversations with Carl Sagan* (Jackson: University of Mississippi Press, 2006), 37.
[20] On pseudoscience, see Davidson, *Sagan*, 120, 135, 226–32, 268–71; and Poundstone, *Sagan*, 155. On Sagan's interest in unconventional ideas, see Davidson, *Sagan*, 130, 151.
[21] See Davidson, *Sagan*, 126–30, 185; *L* stood for "the average lifespan of a technologically advanced civilization," for which there was only one example, human civilization on Earth. The estimated value of *L* rose and fell as the world drifted toward and away from thermonuclear conflict in the late 1950s and early 1960s. Thus the equation hinged on a single variable that was difficult to estimate. See also Poundstone, *Sagan*, 55–58, 144–46.

The public first became aware of Sagan as part of the team organizing the publicized NASA missions to other planets in the 1970s. A combination of lofty visions, media savvy, and non-nerd demeanor drew journalists to him, while his good looks and verbal flair ensured him a wide audience, including his role as essentially the in-house astronomer on Johnny Carson's *Tonight Show*. Though he published prolifically in the scientific literature—one biographer says he averaged a paper per month over his 40-year career—Sagan suffered criticism from some scientists who dismissed him as a mere popularizer. Undeterred, Sagan reached the crest of his fame as host and creator of the PBS series *Cosmos* in 1980. An estimated 150 million viewers soared across time and space with Sagan as their guide, making *Cosmos* the most-watched PBS series of all time up to that point. In 1996 Sagan claimed that half a billion people in over 60 countries had seen the series, and a 1999 article in *Nature* stated that the book based on the television series "was the best-selling science book ever published in the English language." Fans of Sagan described him as a "science missionary" who explained to the public the important intellectual and political relevance of science.[22]

Cosmos aired during the great reawakening of antinuclear movements across the United States and Europe. Sagan had in fact reached a deal with ABC for a series on nuclear war, though the network later canceled the project. As a popular celebrity with an intellectual's mindset and liberal politics, Sagan was poised to move into the antinuclear movement—a move that had hampered Pauling's scientific career and reputation. But unlike

[22] Christopher Chyba, "An exobiologist's life search," *Nature*, Vol. 401, Oct. 28, 1991, 857; Terzian and Bilson, *Sagan's Universe*, 3; Poundstone, *Sagan*, 288; *Conversations,* 57, 138. Ken Burns's *Civil War* later surpassed *Cosmos* in number of viewers.

Pauling, Sagan claimed he could not separate his science from his activism, seeing the two as indivisible.[23]

Thanks to *Cosmos*, Sagan had come to be seen as such an authority on science that the Jimmy Carter administration asked him to provide a passage for the President's farewell address in January 1981. Thus the president read, in words unmistakably Sagan,

> Nuclear weapons are an expression of one side of our human character. But there's another side. The same rocket technology that delivers nuclear warheads has also taken us peacefully into space. From that perspective, we see our Earth as it really is—a small and fragile and beautiful blue globe, the only home we have. We see no barriers of race or religion or country. We see the essential unity of our species and our planet, and with faith and common sense, that bright vision will ultimately prevail.[24]

Famous Enough

The success of *Cosmos* and the Carter endorsement may have convinced Sagan that he had finally become "famous enough" to take on U.S. militarism and nuclear weapons. When the Union of Concerned Scientists (UCS) organized scientists and students for a national teach-in on November 11, 1981, Sagan took part. A "Scientists' Declaration" by the UCS called upon "America's technical community" to drum up support for arms control recommendations such as a comprehensive test ban, a missile flight-test ban, reductions in arsenals, and efforts at nonproliferation. Other sponsors of the teach-in included the Federation of American Scientists, International Physicians for the Prevention of Nuclear War, the National Council of Churches, and Physicians for Social Responsibility. Former presidential science advisors George B. Kistiakowsky and Jerome Wiesner participated, as

[23] Poundstone, *Sagan*, 292; *Conversations,* 117.
[24] Quoted in Douglas Brinkley, *The Unfinished Presidency: Jimmy Carter's Journey Beyond the White House* (New York: Penguin, 1998), 32–33. During the *Voyager* mission in 1977, Sagan convinced the team to turn the spacecraft around as it passed Neptune in order to take a picture of Earth from the farthest distance possible. The resulting photograph showed the Earth to be nothing more than a "pale, blue dot," which "underscored the tinyness, the comparative insignificance of our world and ourselves." Interview with Ira Flatow, 1994, in *Conversations*, 82–83.

did Manhattan Project veteran Philip Morrison and antinuclear physician Helen Caldicott. Sagan spoke to an audience of 1,500 at Cornell University, where he served as professor of astronomy. "If you refuse to think about these issues because they are too difficult and agonizing," Sagan said in heavy bombast, "then you are making a contribution toward future holocaust."[25]

That line also appears in "To Preserve a World Graced by Life," a Sagan essay about nuclear war that mixes scientific knowledge, political advocacy, and an environmental ethos, all expressed with the popularizer's great gift for language. The essay contained themes Sagan would return to often, and established his voice as an opponent of nuclear weapons well before he began to promote the nuclear winter theory. "There is no issue more important that the avoidance of nuclear war," the essay began. Sagan then took the reader on a journey through the solar system, past "broiling, cratered Mercury; and Venus, a hellhole of a planet," as well as Mars. During a century of magnificent achievement, humans "have scrutinized the stunning rings of Saturn. We have looked at the moons of Jupiter and Saturn. Every one of these worlds is lovely and instructive. But, so far as we know, they are also desolate and lifeless." The Earth, it turned out, is the only planet with life, life that is threatened by nuclear weapons. Just a single nuclear weapon contained "the destructive force of the entire Second World War," while nuclear missiles were "genies of death patiently awaiting the rubbing of the lamps." The speed and destructive power of a nuclear war was the equivalent of "a World War II every second for the length of a lazy afternoon." Like scientist-activists before him, Sagan envisioned a role for scientists: "I believe that one of the most important jobs that scientists have in this dialogue, this polylogue, on the

[25] Paul F. Walker, "Teach-ins on American Campuses," *Bulletin of the Atomic Scientists*, February 1982, Vol. 38, No. 2, 10–11.

dangers of nuclear war is to state very clearly what the dangers are." As the essay ended, Sagan attempted to shame humans by describing an extraterrestrial's discovery of Earth. This theoretical alien, Sagan wrote, would be confused as to why the Earthlings spend so much money on weapons when they could use the money for far more noble causes, such as ending misery and hunger.[26]

Sagan's words made their way into the halls of Congress. In 1981, Rep. Stephen Neal (D-NC) entered Sagan's "Who Speaks for Earth?" into the *Congressional Record*. Neal hailed Sagan as "America's foremost scientist communicator," and echoed the astronomer when he wondered how humans could claim stewardship of Earth when they have created "a collective nuclear arsenal capable of murdering 100 billion human beings . . . particularly when the human population totals less than 4½ billion?" Neal urged his colleagues to read the piece "and take heed of its message of survival." The article itself echoed the "To Preserve a World" essay and Sagan's passage from Carter's farewell address. "National boundaries are not evident when we view the Earth from space," Sagan had written. "Fanatical ethnic or religious or national chauvinisms are a little difficult to maintain when we see our planet as a fragile blue crescent fading to become an inconspicuous point of light against the bastion and citadel of the stars. . . . If we do not speak for the Earth, who will? If we are not committed to our survival, who will be?" Sagan described war as murderous madness writ large on society, and questioned the concept of deterrence. "What is often

[26] "To Preserve a World Graced by Life," 1982, Folder 22.27: Nuclear Winter continued, Box 22, Hans Bethe Papers, Cornell University (hereafter HB Papers). Also appeared in shortened form in *Bulletin of the Atomic Scientists,* January 1983, Vol. 39, No. 1, 2–3. When the Council for a Livable World published the essay as a pamphlet, Sagan dedicated the essay to the former Manhattan Project alumnus and arms control advocate George B. Kistiakowsky.

called the strategy of nuclear deterrence is remarkable for its reliance on the behavior of our nonhuman ancestors," a relic of "reptilian passions."[27]

The Nuclear Winter Theory

Sagan's antinuclear essays and actions spoke to scientists, the public, and governments simultaneously, drawing from the logic of science and the humanism of the counterculture among other inspirations. He soon came to focus on a specific antinuclear argument: nuclear winter. But this theory did not come from thin air. In a way, many people in the 1980s already had Armageddon on the mind. Members of the Reagan administration casually voiced millennial musings, while Jonathan Schell's popular 1982 book *The Fate of the Earth* argued that nuclear war would result in a global holocaust.[28] And scientists had begun to study for the first time the long-term effects of a nuclear war. A 1982 Congressional Committee on Science and Technology hearing explored the concerns of "scientists worldwide" that a nuclear war could disrupt the global environment and the balance of life on Earth. Representative Al Gore (D-TN) saw the distinct possibility that the superpowers could "bomb all humanity back to the Stone Age" until they "destroy humanity."[29] The scientific statements during this hearing presaged some of the dire forecasts of nuclear winter, though Sagan and his team elaborated on these studies, and Sagan the popularizer brought such concerns to a new level of public consciousness.

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[27] Extensions of Remarks: "Who Speaks for Earth," Hon. Stephen L. Neal, *Congressional Record,* Nov. 4, 1981, 97th Cong., 1st sess., 26676–78.
[28] Paul Boyer, *Fallout: A Historian Reflects on America's Half-Century Encounter with Nuclear Weapons* (Columbus: Ohio State University Press, 1998), 150–55. Jonathan Schell, *The Fate of the Earth* (New York: Knopf, 1982).
[29] U.S. House of Representatives, Investigations and Oversight Subcommittee of the Committee on Science and Technology, *Consequences of Nuclear War on the Global Environment*, 97th Cong., 2nd sess. Hearing, September 15, 1982 (Washington, D.C.: U.S. Government Printing Office, 1983), 1–2, 177.

The concept of nuclear winter hatched from the confluence of three scientific endeavors. First, Sagan and his colleagues' analyses of dust storms on Mars and volcanic eruptions showed that particulates in the atmosphere could drastically cool the planet. Second, in 1980 Luis and Walter Alvarez presented evidence that a tremendous collision between an asteroid and the Earth resulted in an altered and hostile climate that drove the dinosaurs to extinction; this theory arguably proved that a severe climate change could cause mass extinctions on Earth.[30] Third, the 1982 article "The Atmosphere after a Nuclear War: Twilight at Noon" by Paul Crutzen and John Birks explained that smoke from fires caused by nuclear explosions could blot out sunlight for a significant period.[31] Realizing the political implications, Sagan and his colleagues Richard P. Turco, Owen B. Toon, Thomas P. Ackerman, James B. Pollack—a group soon abbreviated as TTAPS—used the one-dimensional model from their volcanic studies to calculate the specific temperature drops that would follow various levels of nuclear wars. Unique to their study was the analysis of the burning of cities, which contain enormous amounts of materials that, when burned, turned into thick, dense smoke. TTAPS found that airborne smoke, dust, and soot would absorb and reflect enough sunlight to cause temperatures on land to plummet to -15 or -20° C. Calculations for different nuclear war scenarios showed that the particles and fallout could spread to the Southern Hemisphere and "encircle the earth" in as few as one or two weeks. The cold, darkness, and radiation would threaten human survivors and other living species by halting photosynthesis and food production. Perhaps most alarming was the evidence that such a scenario could be triggered by a threshold as low as 100 megatons. At

[30] Luis W. Alvarez, Walter Alvarez, Frank Asaro, Helen V. Michel, "Extraterrestrial Cause for the Cretaceous-Tertiary Extinction," *Science*, New Series, Vol. 208, No. 4448. (June 6, 1980), 1095–1108.
[31] *Ambio*, 1982, 114–25.

the time of their study, the cumulative yield of the 17,000 nuclear warheads on Earth equaled 12,000 megatons.[32]

The Search for Peer Review

As a lifelong opponent of pseudoscience and self-appointed protector of the scientific method, Sagan knew that rigorous peer review of the nuclear winter hypothesis by his fellow scientists was essential. But as the urgency of an activist equally possessed Sagan, he was also anxious to present nuclear winter to the greater public. In fact, in early 1983 he had already begun to plan a conference at which he would announce the theory of nuclear winter. Cognizant of the potential for controversy, the TTAPS team made a conscious effort to balance their desire for publicity with attempts to boost the theory's scientific credibility by subjecting it to peer review, or at least something resembling it. Rather than go public immediately, Sagan put the conference on hold and sent a long paper summarizing their work to numerous colleagues. He also convened an unconventional meeting with scientists to review the theory in person, achieving something close to, yet in some ways still far from, conventional peer review.

Sagan arranged for between 70 and 100 physical and biological scientists to attend two meetings over five days in late April 1983 at the American Academy of Arts and Sciences in Cambridge, Massachusetts. One account of these meetings notes that "It was understood that only if the [nuclear winter] data held up after peer review would the proposed public Conference be scheduled." First, 40 physicists and ten biologists heard the

[32] TTAPS, "Nuclear Winter: Global Consequences of Multiple Nuclear Explosions," *Science*, New Series, Vol. 2222, No. 4630, (Dec. 23, 1983), 1283–92. The one-dimensional model treated debris as capable of moving up and/ or down in the atmosphere, but not in any other direction.

TTAPS presentation and commented on the paper draft. The scientists "generally agreed with the conclusions of the report" regarding reduced sunlight and severe climate changes, though they did suggest "minor adjustments."[33] One observer later explained that the physicists "had numerous questions about details but very little quarrel with the findings. Several of the scientists went home resolved to try the scenarios on *their* atmospheric models." Next, the remaining scientists examined the "the consensus results" of the first group and found "general agreement" upon the "devastating" and "previously unforeseen" effects, including possible "extermination" of humans and many wildlife species.[34] With this positive response, planning for the conference recommenced.

Though no doubt pleased with the approval at Cambridge, Sagan continued to seek out review from his peers. Especially influential was the respected scientist and arms controller Hans Bethe, Sagan's colleague at Cornell and a Manhattan Project veteran who had for many years advised the government on science and arms control policy. As a member of the Steering Committee of the upcoming nuclear winter conference, Sagan wrote to Bethe and invited him to be on the conference's Scientific Advisory Board; Bethe accepted the invitation.[35] In the intervening months, Bethe advised TTAPS during the development of their study and offered advice on how to present their results to the public. In the summer of 1983 Bethe gave a serious critique of the science behind nuclear winter. Bethe liked the theory, but he questioned their choice of model, suggesting that a different model "could reduce the results [of nuclear winter] substantially." Overall Bethe praised the

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[33] Ehrlich et al, *Cold and the Dark*, xv–xvi, 31; Anne Ehrlich, "About the Conference," in "Nuclear Winter: A Forecast of the Climatic and Biological Effects of Nuclear War," *Bulletin of the Atomic Scientists*, April 1984, Vol. 40, No. 4, 12S–13S.
[34] Ehrlich, "About the Conference," 12S–13S; Ehrlich et al, *Cold and the Dark*, xvi.
[35] Sagan to Bethe, March 2, 1983, Folder 22.28: Nuclear Winter continued, Box 22, HB Papers. See list of Scientific Advisory Board, *Cold and the Dark*, 218–21.

nuclear winter hypothesis as "very good indeed" and "a very important piece of work."[36] Bethe's enthusiasm, tempered by slight concerns about the one-dimensional model, foreshadowed the mixed scientific reception nuclear winter eventually received.

Elsewhere Bethe gave Sagan some straightforward suggestions, ranging from simple edits to more scientific advice, including the need to explain how "Natural winters [are] not necessarily the same [as nuclear winter] because air masses don't carry particles."[37] While listening to Sagan review his findings in November 1983, Bethe recorded in his notes, "Generally in agreement w/ Sagan. Have read report," and noted that the Livermore and Los Alamos laboratories were very interested in conducting three-dimensional calculations that would incorporate an analysis of data from forest fires. Bethe, who had decades of arms control advocacy behind him, counseled Sagan that he "Should emphasize most likely result, not worst case, if we want to persuade govm't." The "we" indicates that Bethe felt himself to be actively involved in the endeavor.[38] But Sagan chose to disregard Bethe's advice about not emphasizing the worst-case scenario, a decision which later earned him criticism. Sagan argued back at these critics that because nuclear winter presented such a cataclysmic threat, it should be taken seriously as long as the scientific evidence did not convincingly rule it out.

Although Sagan continued to handpick the reviewers, he did attempt to run the theory by some potentially hostile critics. And Sagan undoubtedly knew where to expect criticism; the physicist Edward Teller had recently reinforced his extensive credentials as an outspoken arms control opponent in the early 1980s. Teller's 1982 "Dangerous Myths about Nuclear Arms" in *Reader's Digest* had offered scientific rebuttal to the pre-nuclear winter

[36] Bethe, memo to TTAPS, Aug. 1, 1983, Folder: 22.8: Nuclear Winter continued, Box 22, HB Papers.
[37] Undated, handwritten notes by Bethe, Folder 22.25: Sagan, Box 22, HB Papers.
[38] Handwritten notes by Bethe: "11–11 Panel, 1983," Folder 22.27: Nuclear Winter continued, Box 22, HB papers, emphasis in original. Bethe would later become more comfortable discussing the worst-case scenario of nuclear winter.

predictions of long-term environmental consequences of nuclear war. One "myth" spread the misconception that "If a large number of nuclear weapons were exploded, fallout would pollute food and water supplies, making combatant countries uninhabitable. The spread of radioactive fallout throughout the world would end life on earth." Teller dismissed this fear by explaining that the decay of radioactive substances ensured that the "radioactivity of fallout declines rapidly." Another myth worried that "The explosion of nuclear weapons in the atmosphere will bring an end to life on the planet by damaging the ozone layer." Teller tut-tutted that "This new doomsday myth is gaining popularity." Although skin cancer rates could increase, "ultraviolet-sensitive species could be extinguished," and "some serious ecological changes might follow," he comforted his readers that "our survival can be considered certain." Yet another myth, phrased as "Civil defense is without value in saving lives and may actually increase the risk of war," he described as "perhaps the most dangerous myth of all."[39]

Sagan contacted Teller in the spring of 1983. Whether Sagan was offering a preemptive olive branch, initiating the inevitable confrontation, or aggressively challenging Teller's authority on nuclear weapons, he undoubtedly recognized that it would enhance the credibility of nuclear winter if it withstood a fierce critic's glare. At roughly the same time as the Cambridge peer review meeting, Sagan sent Teller "a 145-page précis" of the TTAPS team's work and requested "detailed criticisms." Sagan sent more material to Teller on June 24.[40]

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[39] *Reader's Digest,* November 1982, 34–37.

[40] CS to ET, April 25, 1983, and June 24, 1983, Folder: Sagan, Carl Dr., Box 283: Correspondence, Personal, Edward Teller Papers, Hoover Institution (hereafter ET Papers). CS to ET, Feb. 23, 1984, Folder: Sagan, Carl Dr., Box 283: Correspondence, Personal, ET Papers. [Also in Folder 22.27: Nuclear Winter, continued, Box 22, HB Papers.]

Later that summer, Teller sent Sagan a not entirely hostile response. "There is no question that the effects of smoke and dust are of great importance," he wrote. But because the TAPPS one-dimensional model was an "incomplete step in evaluation," he preferred calculations on a two-dimensional model performed at his own Livermore laboratory, "which gives (as you expect) much milder results, which are nonetheless severe." Teller concluded by stating, "I believe you and I can agree that serious extensive efforts to clarify this question need to continue. There is a particularly important duty for technical people to make information available when it becomes sufficiently reliable."[41] The absence of hostility was not, however, the same as an endorsement. Sagan would later assert that Teller urged caution only when scientific evidence appeared to support an antinuclear view; when science supported nuclear weapons, Teller never hesitated to trumpet his views. But Sagan would soon learn that scientific critics besides Teller counseled patience on nuclear winter as well.

While TTAPS continued to refine the science behind nuclear winter, Sagan began to hash out his views on the political aspects of the theory. Just as he had with the TTAPS paper, Sagan sought out the views of various experts, but this time those of the foreign policy and national security establishments. George Kennan, the iconoclastic creator of containment and longtime skeptic of nuclear weapons, was one who fit the bill. In November 1983 Sagan sent Kennan a draft of his upcoming article in *Foreign Affairs* on the policy implications of nuclear winter.

"I think the article very powerful," Kennan wrote to Sagan. "It will be the crowning phase of an effort on your part, and on the part of your scientific associates, which I see as being of major significance—probably one of the great landmarks of the anti-nuclear

[41] ET to CS, Aug. 16, 1983, Folder: Sagan, Carl Dr., Box 283: Correspondence, Personal, ET Papers.

movement. I cannot tell you what a great thing I feel you have done." Kennan especially liked how Sagan challenged the fundamental beliefs of deterrence and the arms race. "You have provided what we all lacked and which no scientific layman could have provided: the clear and irrefutable demonstration of the enormity of the danger presented by these vast nuclear arsenals. I hope it will receive the widest possible distribution." Kennan congratulated Sagan "from the bottom of my heart" and urged him "not to rest until every intelligent person in the NATO and Warsaw Pact countries has understood what you have had to say."[42]

In fact, scientists and nuclear policy experts around the world apparently were drenched in a paper deluge of Sagan's drafts and ideas on nuclear winter. By the time Sagan and Turco gave the subject a book length treatment in 1991, their acknowledgements had become massive in scale. The authors thanked 28 readers who "read and commented on earlier drafts," including McGeorge Bundy, Robert McNamara, Paul Crutzen, Freeman Dyson, Richard Garwin, Jeremy Stone, Frank Press, and Frank von Hippel, in addition to an unspecified number of reviewers who preferred anonymity. Following that list was a roll call of 114 scientists, politicians, and others who "helped" by aiding "our understanding of nuclear war, nuclear winter, and their implications, or by stimulating our thinking on these issues." This list included Bethe, Luis Alvarez, Helen Caldicott, Daniel Ellsberg, Newt Gingrich, Barry Goldwater, Al Gore, Kennan, George Kistiakowsky, Richard Perle, William Proxmire, Joseph Rotblat, Brent Scowcroft, Edward Teller, and Tim Wirth. Such lengthy lists at the very least give evidence of extensive review, of having been carefully thought, rethought, written, rewritten, edited, and edited again. When Sagan received criticism of the

[42] George Kennan to CS, Nov. 1, 1983, Folder 22.27: Nuclear Winter continued, Box 22, HB Papers.

nuclear winter theory, he often told his critics that the TTAPS paper had been reviewed by more than 100 scientists.[43]

As spring of 1983 turned to summer, the TTAPS team continued to incorporate the many suggestions and criticisms of their peers. After attending the Cambridge peer review meeting, twenty scientists, led by Stanford biologist Paul Ehrlich, had been inspired to draft a companion paper to TTAPS elaborating on the biological consequences of nuclear war, including the effects on plants, animals, and human existence.[44] Upon completion, both groups of scholars submitted their papers to the peer-reviewed journal *Science*, and prepared to announce the results at a nuclear winter conference on Halloween 1983. In order to maintain an objective outlook, the organizing committee "decided that political discussion," including the implications for disarmament and arms control, "should not be a part of the proposed conference."[45] But just before the conference, Sagan first addressed the mainstream public; this jumping the gun would dog the nuclear winter theory in the eyes of many scientists who believed that science and public advocacy should not mix.

Going Public

As one who saw an urgent need for the public to be informed of and familiar with science in general (often dismissed as a popularizer), Sagan endeavored to reach those who would not attend or even notice a scientific conference. Accordingly, Sagan wrote an article on nuclear

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[43] Sagan and Turco, *Path*, xx–xxii. Also see acknowledgements in Ehrlich et al, *Cold and the Dark,* 29; "The Chilling Aftermath of a Nuclear War," reprinted in *Congressional Record,* Senate, 98th Cong., 2nd sess., Feb. 23, 1984, 3083.
[44] Ehrlich et al, *Cold and the Dark*, xviii.
[45] Ehrlich, "About the Conference," in "Nuclear Winter: A Forecast of the Climatic and Biological Effects of Nuclear War," *Bulletin of the Atomic Scientists*, April 1984, Vol. 40, No. 4, 12S–13S; Ehrlich et al, *Cold and the Dark,* xv.

winter for *Parade* magazine that reached millions of ordinary Americans on the Sunday before the conference.

Sagan began the article by explaining the effects of a two-megaton nuclear weapon on "a fairly large city." He described how "buildings would be vaporized," their occupants "reduced to atoms and shadows." A weapon detonated at ground level would leave a crater large enough that the landscape would resemble the moon. Such was the power of one weapon; meanwhile, the United States and Soviet Union possessed some 50,000 nuclear weapons with a total yield of 13,000 megatons—"enough to obliterate a million Hiroshimas." Sagan then explained the fallacy of nuclear weapons strategy, including limited nuclear wars. Military experts agreed, he wrote, that any nuclear war would quickly escalate. The world's nuclear weapons "now sit quietly and inconspicuously, in missile silos, submarines and long-range bombers, faithful servants awaiting orders." Once unleashed, carnage would cover the Earth: Sagan cited a recent World Health Organization study estimating that over 2 billion people—"almost half the humans on earth"—were imperiled by the direct effects of nuclear war.

But nuclear weapons had destructive effects beyond their immediate explosive and radioactive impact, and these long-term effects would threaten the roughly 2 billion people who would survive a nuclear war. Sagan then introduced his readers to nuclear winter. Before continuing, he paused to warn his audience. "Some of what I am about to describe is horrifying. I know, because it horrifies me," he wrote. "We must steel ourselves to contemplate the horrors of nuclear war." Here Sagan projected an image of a vastly more serious Herman Kahn, thinking about the unthinkable but in an urgent and earnest, almost sanctimonious, way. He estimated that 10,000 tons of dust entered the atmosphere for every

megaton burst, and with a baseline model of 5,000 megatons, tremendous amounts of dust, smoke, and soot would blot out the sun. In the Northern Hemisphere, "an unbroken and deadly gloom" would persist for weeks. The temperature would suffer a massive drop to well below 0° F, and crops, farm animals, and other food supplies would disappear. "Most of the human survivors would starve," he asserted.

The apocalyptic troika of coldness, darkness, and fallout would unleash "a severe assault on our civilization and our species," with disease rampant, hospitals destroyed, and health care all but impossible. One of the theory's more controversial aspects was the claim that nuclear winter had a threshold—that a certain number of megatons were necessary to trigger nuclear winter. TTAPS considered this a reasonable assumption based on the fact that no nuclear winter occurred after the first two atomic bombs fell on Japan in 1945. But Sagan believed this threshold to be very low, around a modest 100 megatons.

The authority of science supported nuclear winter, Sagan wrote. The results had been "carefully scrutinized" by over one hundred U.S., European, and Soviet scientists, who had agreed on the "global consequences." It was science, and not just Sagan, that predicted "subfreezing temperatures in a twilit radioactive gloom" for months after a nuclear war. Sagan also summarized the claims of the Ehrlich group: that many plants and animals would go extinct, and humans might follow. The article ended by briefly assessing recent arms control proposals and suggesting that concerned readers write to Reagan and Andropov.[46]

Sagan also jumped the gun in another way: on the morning of the conference, he first met with select members of Congress to brief them in private on nuclear winter. With the public, the government, and the scientific community alerted, the World after Nuclear

[46] Carl Sagan, "The Nuclear Winter," *Parade,* October 30, 1983.

War: The Conference on the Long-Term Worldwide Biological Consequences of Nuclear War began at the Washington, D.C., Sheraton Hotel on October 31, 1983, one day after the *Parade* article appeared. According to the proceedings, 500 participants and 100 media representatives from 20 nations attended the conference. To emphasize the global implications of nuclear winter, the conference featured an innovative "Moscow Link," a live discussion of nuclear winter with scientists in the Soviet Union. Sponsors of the conference included the Sierra Club, the Wilderness Society, the Federation of American Scientists, and the American Institute of Biological Sciences.[47]

The conference's purpose was to showcase the scientific credibility of nuclear winter as much as it was to express the dire threat it posed. Stanford biologist Donald Kennedy opened the conference by declaring himself "neither a likely technical resource for an arms control conference not a promising candidate for cheerleader at a peace rally." In fact, he said, the TTAPS report was "serious scientific analysis." When Sagan spoke, he described to the audience the "witch's brew of radioactive products" that a nuclear war would release into the atmosphere, including fallout, smoke, soot, and dust. He mentioned the "general agreement" that "a period, lasting at least for months, of extreme cold in a radioactive gloom" would follow a nuclear war, and that the destruction would spread to the Southern Hemisphere. He closed by pointing out the appropriateness of the Halloween setting for the conference, as nuclear winter would make real the fanciful themes of fire, winter, and death reflected in the holiday's pagan origins.[48]

In the questions following Sagan's talk, a British physicist asked Sagan about peer review. "Clearly, anything that is new and startling needs review by many peers," Arnold

[47] Ehrlich et al, *Cold and the Dark*, xviii, 35.
[48] Ehrlich et al, *Cold and the Dark,* xxi, xxiv, xxv, xxxiii, 9, 24, 26.

Wolfendale stated. Was the TTAPS team planning on consulting scientists or, for example, the National Academy of Sciences for their views? In his answer, Sagan defended the TTAPS work as having endured criticism from "individuals of many different political persuasions, including representatives of the government weapons laboratories." Sagan added that the TTAPS and Ehrlich papers had both passed peer review by *Science*.[49]

Paul Ehrlich followed Sagan and presented the possible biological consequences of long-term effects, emphasizing that the results reflected "the consensus of a large and distinguished group of biologists." He added that the findings were "robust"—that is, it was difficult to conceive of a nuclear war that would *not* lead to a biological catastrophe. The effects "could result in the end of civilization in the Northern Hemisphere." While the blast alone could kill some 750 million—"vaporized, disintegrated, mashed, pulped, and smeared over the landscape"—further effects would menace the unhappy survivors. The climate would kill "virtually all land plants" in the Northern Hemisphere, and most animals would die, leaving "rats, roaches, and flies the most prominent animals shortly after World War III." The extinction of *Homo sapiens* was a distinct possibility. A later panel on biological consequences reaffirmed an acceptance of the results. On Tuesday, astrogeophysicist Walter O. Roberts brought the conference to a close, reemphasizing the strong scientific integrity of the nuclear winter theory. "We are together, basically, regarding the physical and biological matters discussed at the Conference," he stated, though he did add that this consensus did not extend to the political implications of the theory.[50]

In describing the conference to readers of the *Bulletin of the Atomic Scientists*, Anne Ehrlich also stressed the scientific credibility on display. "Unlike most scientific

[49] Ehrlich et al, *Cold and the Dark,* 31.
[50] Ehrlich et al, *Cold and the Dark*, 43–59, 128, 155.

conferences," she wrote, "this one offered no controversies or disagreements." To affirm the apolitical credentials of the conference organizers, Ehrlich mentioned the ban on policy pronouncements in order to "focus on the stark facts." Far from premature, she asserted, "the findings were held in confidence until they had been carefully reviewed by dozens of competent specialists and even confirmed in other studies." Ehrlich nevertheless concluded that nuclear winter challenged current U.S. policies, asking, "Can the world risk *everything* on the shaky hopes based on deterrence?"[51]

After the conference, Sagan next appeared in the December 1983 issue of the journal *Science* as co-author of the two peer-reviewed papers on nuclear winter. The TTAPS paper introduced the idea of nuclear winter and set the terms of the ensuing scientific and political debate. After presenting possible "nuclear exchange scenarios" based on world nuclear arsenals, the TTAPS paper stated that dust, fires, soot, and smoke caused by thermonuclear conflagrations would significantly alter the global climate. In the ensuing weeks after a war, temperatures would drop, various ecosystems could shut down, agriculture would decay, and people far removed from the conflict would suffer or even starve. The second *Science* article emphasized the wide array of support for the article in its list of authors, led by Paul Ehrlich but including Sagan and eighteen other co-authors. "Long-Term Biological Consequences of Nuclear War" elaborated on the terrible plight of the 2.3 billion humans who would survive a nuclear war. The paper ominously concluded that "the possibility of the extinction of *Homo Sapiens* cannot be excluded."[52]

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[51] Anne Ehrlich, "Nuclear Winter: A Forecast of the Climatic and Biological Effects of Nuclear War," *Bulletin of the Atomic Scientists*, April 1984, Vol. 40, No. 4, 3S, 14S. See also Ehrlich et al, *Cold and the Dark,* 155.

[52] Turco, Toon, Ackerman, Pollack, and Sagan, "Nuclear Winter: Global Consequences of Multiple Nuclear Explosions," and Ehrlich et al, "Long-Term Biological Consequences of Nuclear War," *Science,* New Series, Vol. 222, No. 4630 (Dec. 23, 1983), 1299.

With the scientific work established, Sagan's movement turned political in the new year. On New Year's Eve, 1983, Sagan gave a talk at the Cathedral of St. John the Divine in New York City that mapped out his next phase. Sagan began by describing the evolution of human society from hunter-gatherer times, "when all life on the planet was in a nearly perfect ecological harmony, and when the uranium was still in the rocks." But as 1984 began, "The uranium is no longer entirely in the rocks." The 18,000 nuclear warheads in the United States and Soviet Union threatened human existence with a cataclysm of war and nuclear winter. "So this is my New Year's Resolution," he vowed,

> I invite you to share it with me. We will devote a significant part of 1984 to educating ourselves and our public officials on the apocalyptic implications of nuclear war. We will make certain that every candidate for the House of Representatives, for the Senate and for the Presidency has made a clear public statement, a clear position on the nuclear arms race. And then we will work to elect those who are committed—not to some vague and fence-straddling generalities, but to specific and consistent proposals for major and verifiable reductions mutual in the world inventories of strategic weapons.

Referencing George Orwell and Stanley Kubrick, he concluded: "Let us convert 1984 from a codeword for a government-managed nightmare, to a year in which we start freeing ourselves from the trap that we have carelessly and foolishly set for ourselves. I would like 2001 to dawn on a world that will truly initiate a new millennium, liberated from the danger of nuclear annihilation of the human species." As he hoped, public officials heard his message, including Rep. George Miller (D-CA), who placed Sagan's speech in the *Congressional Record.* Miller added that nuclear weapons have made the world "substantially less secure than we were at the end of the Second World War."[53]

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[53] *Congressional Record,* Extensions of Remarks, George Miller, "Carl Sagan's New Year's Resolution," 98th Cong., 2nd sess., Jan. 24, 1984, 326–27.

Freed from the moratorium on political statements at the Halloween conference, Sagan directly addressed the policy implications of nuclear winter in *Foreign Affairs*. He began the article with an immediate statement of nuclear winter's scientific credibility: "Apocalyptic predictions require, to be taken seriously, higher standards of evidence than do assertions on other matters where the stakes are not as great." Sagan then framed his argument as having been thoroughly vetted by readers from all across the scientific and strategic communities. A footnote thanked Kennan as well as many others, including Bethe, McGeorge Bundy, Freeman Dyson, Richard Garwin, Averell Harriman, Robert McNamara, Joseph Rotblat, Herbert Scoville, Edward Teller, and Albert Wohlstetter. The article briefly summarized the findings of the TTAPS and Ehrlich papers, though it extended the argument at some points. For example, to offer a possible death toll, Sagan estimated not only the world population of 4 billion, but also potential as-yet-unborn generations, arriving at an unfathomable prediction of 500 trillion fatalities.[54]

The policy implications of nuclear winter lay at the heart of the *Foreign Affairs* article, as the theory made traditional nuclear strategy all but obsolete in Sagan's view. Four specific implications stood out in future nuclear winter debates. First, a nuclear first-strike would be "tantamount to national suicide for the aggressor—*even if the attacked nation does not lift a finger to retaliate*." Second, the nations of the third world were at risk, even if they avoided any involvement in a war whatsoever. Third, nuclear winter made civil defense pointless, as any survivors would be unable to live in the destroyed biological and agricultural environment. Finally, nuclear winter rendered useless technological defenses such as ABMs and SDI. Sagan suggested ways out of the trap of "apocalyptic threats and doctrines" that had created

[54] Carl Sagan, "Nuclear War and Climatic Catastrophe: Some Policy Implications," *Foreign Affairs,* Winter 1983/ 1984, 257–59, 275.

a working version of Herman Kahn's "Doomsday Machine," including confidence building measures, de-MIRVing of missiles, and reductions of as much as 90 percent of world nuclear arsenals. "It is nowhere ordained," Sagan optimistically concluded, "that we must remain in bondage to nuclear weapons."[55] Soon, many U.S. citizens and politicians would rely on nuclear winter to challenge the concept of nuclear deterrence.

Elsewhere Sagan calculated that the number of nuclear weapons around the world was nearly 20 times larger than the "small" nuclear war scenario of 1000 weapons of 100 kilotons each used in TTAPS estimates. He stated that "The danger is so grave and so open-ended as to suggest an urgent need for a massive, bilateral, verifiable decrease in the global inventory of nuclear weapons to below the threshold at which Nuclear Winter could be triggered, while still preserving, if we so wish, the doctrine of strategic deterrence."[56] Although many politicians would soon use the threat of nuclear winter to argue against nuclear deterrence, Sagan clearly offered ways for deterrence to remain U.S. policy while still making a nuclear winter impossible. Such a combination of scientific authority, environmental alarm, and quasi-millennial prophecy created a powerful antinuclear argument, one that appeared in scientific publications, newspapers, magazines, foreign policy journals, and the *Congressional Record.*

Initial Reactions: November 1983 to August 1984

The Public

[55] Sagan, "Policy Implications," 276, 292, emphasis in original.
[56] "The Chilling Aftermath of a Nuclear War," reprinted in *Congressional Record,* Senate, 98th Congress, 2nd sess., Feb. 23, 1984, 3083.

Sagan aimed his campaign at three different constituencies: politicians, scientists, and the general public, though his actions and rhetoric addressed all three at once. This approach differed from that of other scientists—Hans Bethe, for example, supported a Freeze as a political statement but not an actual proposal.[57] Among the public, nuclear winter added a scientific edge to the widespread nuclear scares of the early 1980s, including the Three Mile Island meltdown, Schell's *Fate of the Earth*, and ABC's television movie *The Day After*. The phrase "nuclear winter" seamlessly worked its way into mainstream discussions of nuclear weapons already influenced by the theory's wintry cousin, the Nuclear Freeze.

The *New York Times* greeted nuclear winter with harsh skepticism in an editorial following the Halloween conference. Noting the nearly simultaneous appearance of the concept of nuclear winter and *The Day After*, the *Times* asked, "Why this deluge of restating the obvious?" Surely everyone already knew "that nuclear disaster is hazardous to human health." Deterrence remained the best way to prevent nuclear war: "Deterrence works because it is based on horror. . . . There's no visible alternative to deterrence, no matter how ghastly the ways nuclear war would kill." Nuclear winter and other "profiles in apocalypse" had some value if they convinced the Reagan administration that nuclear war is neither winnable nor survivable. But nuclear winter "should not be confused with science; it has not yet been published or properly checked." Roughly ten months later, another *Times* editorial repeated this skepticism, especially of claims that nuclear winter required rethinking of nuclear strategy. "Not even a new dimension of terror can undercut policies based on deterrence," the editorial argued. But the *Times* accepted that if the theory proved that soot would devastate the climate, "nuclear winter would . . . mean the end of civilization. That

[57] "The Arms Race: A Sandia Colloquium by Hans A. Bethe, July 28, 1982," 15–16, Folder 17.12: Arms Control Sandia Colloq. '82, Box 17, HB Papers.

prospect has to make a difference eventually, however successful the policy of nuclear deterrence has been so far, and however invisible the alternatives."[58]

Not everyone agreed with the editorial board. An opinion piece in the November 20 *Times* argued that nuclear winter "could profoundly alter our strategic outlook." Echoing Sagan's claim that use of nuclear weapons would be suicidal, the authors endorsed the idea of "minimum deterrence." Elsewhere, in a review of the published proceedings of the Halloween conference, *Times* science reporter William J. Broad asked, "So what?" about nuclear winter. "So everything," he answered. Nuclear winter "undermines military strategy and doctrines of targeting and communications—and thus the concept of deterrence that has dominated superpower relations for a third of a century." He hailed the theory as "a remarkable testament to the power of free scientific inquiry."[59] Demands for Sagan's time came even from some unexpected sources. In 1984 Pope John Paul II invited Sagan to brief him on the concept of nuclear winter. Despite being an avowed and intense atheist whose popular novel *Contact* pitted science against religion, Sagan accepted the invitation. (Sagan had refused an invitation from the Reagans to dine at the White House.) After the meeting, the Pope released a statement warning the world of the danger of nuclear winter.[60]

Nuclear winter also appears to have made its way into more mainstream areas of U.S. culture. In a survey of reactions to *The Day After*, broadcast on November 21, a reporter quoted a high school teacher whose class had told him that "the program was too much like a soap opera. . . . Many felt the show was overdramatized and romanticized. They've been

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[58] "The Winter After the Bomb," *New York Times*, 6 November 1983, E20; "Nuclear Winter and Its Smoke," *New York Times*, 19 August 1984, E18.
[59] Robert J. Lieber and Dan Horowitz, "Live, Die: Moot Point," *New York Times*, 20 November 1983, E21; William J. Broad, Review 7, *New York Times*, 12 August 1984, BR21.
[60] Davidson, *Sagan*, 57, 80, 349, 377–78.

reading about the phenomenon of 'nuclear winter' and they felt the film was inaccurate in showing that the bombs would be so survivable." Sagan would likely have appreciated that his theory had inspired such skepticism about survival. Nuclear winter also infiltrated art and music. The cover art for the U.S. 12-inch single of Frankie Goes to Hollywood's hit "Two Tribes" featured a "news item" on nuclear winter "and the possible extinction of the human race."[61] A 1985 art show by Roger Brown featured a painting titled "Nuclear Winter" in which "a red, white and black spiral in the sky identifies nuclear destruction with a tornado," which the *Times* critic deemed "striking," though lacking "the ability to move us."[62]

In an open letter to Reagan and Andropov, the International Physicians for the Prevention of Nuclear War included nuclear winter as one of the reasons that nuclear weapons have put "all human life . . . in critical condition." But the reaction of other antinuclear groups to nuclear winter is unclear. Badash argues that the Freeze movement used the idea of nuclear winter only "opportunistically," Davidson doubts the claim that the concept of nuclear winter gave the movement hard scientific credibility, and Poundstone sees no connections between the two. For Sagan's part, he occasionally voiced support for a Freeze, but hardly mentioned the movement in his Congressional appearances or in his 1991 book on nuclear winter.[63] Across the Atlantic, the British Christian Campaign for Nuclear Disarmament relied on nuclear winter as evidence that the threat of nuclear weapons "has grown worse," referring to the nuclear winter hypothesis as laying bare "the probably

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[61] Glenn Collins, "Students Voice Fear and Hopelessness In Talks the Day After 'The Day After,'" *New York Times*, 22 November 1983, A26; Robert Palmer, "The Pop Life," *New York Times*, 5 September 1984, C21. Palmer, in one of his less accurate predictions, declared that "Frankie Goes to Hollywood's sweeping, almost cinematic, musical vistas, driving rhythms and pop-song craftsmanship add up to more than another short-lived fad."
[62] Michael Brenson, "Art: 'New Horizons' At the Guggenheim," *New York Times*, 20 September 1985, C21.
[63] Peter Kerr, "Physicians Urge End to Arms Race," *New York Times*, 10 January 1984, 17. On nuclear winter and the Freeze, see Badash, "Nuclear Winter," 91–92, and Davidson, *Sagan*, 360. Although conservative opponents often tried to link Sagan to the Freeze movement, Sagan rarely mentioned the movement in public, and in *Path* mentions the Freeze only once, 278–79.

catastrophic consequences of a nuclear war for our environment and all living things, and therefore the risks which the nuclear powers are taking with God's creation."[64] To coincide with the third European Nuclear Disarmament (END) Convention in Perugia, Italy, the editors of *ENDpapers* reprinted Sagan's "Nuclear War and Climatic Catastrophe" in summer 1984 "for the attention of the Perugia delegates."[65]

Science: An Uncertainty Principle

The scientific reaction to the nuclear winter hypothesis was undeniably mixed, but contained more positive assessments than scholars have recognized. Bethe had been so impressed with nuclear winter that he initiated a Los Alamos study on the subject in the summer of 1983, and in his personal papers at Cornell University left a handwritten note for future researchers to find on the folder titled "Nuclear Winter" that boasts, "Note: This work was initiated at Los Alamos by H.A. Bethe in summer 1983."[66] Other scientists also took care to credit Bethe with initiating nuclear winter research at the lab, such as Los Alamos scientist Robert Malone, who wrote to Bethe in the fall of 1983 thanking him for his support of their nuclear winter research and sharing some of their initial data. The Los Alamos study would follow Bethe's vision by using a three-dimensional model that exceeded the capabilities of the

[64] "Open Letter to the General Synod of the Church of England," Nov. 15, 1984, doc. 73, 6/1 Christian CND (33/127), CND Additions (I), Campaign for Nuclear Disarmament Papers, London School of Economics and Political Science. Sagan himself would have pointed out that the possibility of nuclear winter did not mean that the threat of nuclear weapons had "grown worse," but rather that the threat of nuclear winter had only been recognized in the 1980s; the superpowers had possessed enough nuclear weapons to initiate a nuclear winter since at least the 1960s.
[65] "Introduction," *ENDpapers Eight*, Spokesman 46, Summer 1984, 1.
[66] Folder 22.26: Nuclear Winter, Box 22, HB Papers.

TTAPS one-dimensional model, and would yield results supporting the possibility of nuclear winter in the coming years.[67]

In the meantime, however, other scientists pounced aggressively at nuclear winter's uncertainties. S. Fred Singer, a professor of environmental sciences and a consultant to the White House Science Advisor, challenged the theory on scientific and political grounds. In the *Wall Street Journal* Singer linked nuclear winter to a Freeze and facetiously wondered if the reason Soviet scientists believed in nuclear winter was because a Freeze would be good for Soviet objectives. Like most opponents of nuclear winter, Singer hammered the theory's uncertainties: "the range of uncertainty is so great . . . that the prediction isn't particularly useful." Singer suggested that the greenhouse effect disrupted by nuclear debris—which Sagan argued would keep warmth out—might actually help keep the remaining warmth from escaping. He criticized TTAPS for using Mars to guide their calculations, since "Martian dust has different physical and optical properties than smoke and soot," and even claimed that the immense burning after a nuclear war would help keep the Earth warm. In addition, the immense particulate matter in the atmosphere would force itself to be rained out. Finally, Singer argued that nuclear winter failed as a deterrent because the threshold might make a "small nuclear power" feel free to launch a single tactical nuclear bomb.[68]

Sagan responded to Singer's challenge by immediately defending the scientific integrity of nuclear winter and the lack of comparable integrity in Singer's claims. The TTAPS study, he wrote, "has been carefully reviewed by over 100 scientists, and roughly

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[67] Robert C. Malone to Bethe, Sept. 13, 1983, Folder 22.26: Nuclear Winter, Box 22, HB Papers; See Memo, R.C. Malone to C.P. Robinson, Nov. 30, 1984: Annual Progress Report, Nuclear War Climate Studies, Folder 22.26: Nuclear Winter, Box 22, HB Papers.

[68] S. Fred Singer, "Nuclear Winter and Nuclear Freeze," *Disarmament: A Periodic Review by the United Nations*, Vol. VII, No. 3, Autumn 1984, 63–71; and S. Fred Singer, "The Big Chill? Challenging a Nuclear Scenario," *Wall Street Journal*, 3 February 1984.

similar conclusions have since been drawn in separate calculations by scientists of many nations." Singer, by contrast, "offers no calculations. Our extensive computer modeling takes account of a variety of plausible values of parameters associated with coagulation of particles, rainout and other scavenging mechanisms." Elsewhere, he argued that his opponents who complained of the theory's uncertainties missed the point. Humans frequently took precautions in the face of uncertainty, Sagan often argued, pointing to the popularity of flood insurance as evidence of rational approaches to improbable—but still dangerous—events.[69]

The *Bulletin of the Atomic Scientists* printed a special 15-page supplement on nuclear winter in April 1984. The editors endorsed Sagan's view on urgency, stating that "on an issue so vital to the planet, a worst-case analysis is the only prudent approach."[70] Except for this supplement, however, the *Bulletin* hardly mentioned nuclear winter at all, and printed no letters to the editor on the subject. A great deal of ink, by contrast, was spent on SDI.

One of the rare exceptions to this absence of discussion in the *Bulletin* was Joseph V. Smith's review of the published proceedings of the Halloween conference. Smith instantly cast doubt on the theory, asking, "If we cannot accurately forecast the weather a week in advance, what is the meaning of 'nuclear winter'?" This attempt to shrink the nuclear winter debate down to a guess about the local weather was grossly misleading. While estimates of exact rainfall and temperatures are often off-base, scientific estimates about seasonal temperature ranges are quite reliable. Smith also questioned the scientists' objectivity: "the

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[69] "The Chilling Aftermath of a Nuclear War," reprinted in *Congressional Record,* Senate, 98th Cong., 2nd sess., Feb. 23, 1984, 3083. For one instance of Sagan's flood insurance example, see U.S. House of Representatives, Natural Resources, Agriculture Research and Environment Subcommittee of the Committee on Science and Technology. *The Climatic, Biological, and Strategic Effects of Nuclear War*. 98th Cong., 2nd sess. Hearing, September 12, 1984 (Washington, D.C.: U.S. Government Printing Office, 1985), 7.

[70] Editors' introduction, "Nuclear Winter: A Forecast of the Climatic and Biological Effects of Nuclear War," *Bulletin of the Atomic Scientists*, April 1984, Vol. 40, No. 4, 2S.

more uncertain a prediction," he wrote, "the more likely an emotional discussion." On a technical level, the TTAPS one-dimensional model was particularly inadequate. "Let me emphasize," he explained, "that criticism of nuclear winter does not make me, or any other critic, a supporter of nuclear war. Indeed, I believe that nuclear war is humanity's most horrible invention . . . . Finally, let me emphasize my admiration for the pioneers of the concept of nuclear winter. Their moral indignation and plain guts are needed to shake us up before we slip down the path to disaster." He deemed Sagan's article "convincing when the assumptions of the models are accepted," but rejected a specific threshold as unlikely, based on his inability to find a single meteorologist who supported the idea. "This valuable book," he summarized, "should further develop an emotional climate about the problems of nuclear war. Readers, however, might wish to reserve judgment about whether the present concept of nuclear winter might turn out to be a metaphor rather than a reliable basis for planning military and civil defense."[71]

Scientific reaction to nuclear winter made for heated debate; the pages of *Nature* show how far the theory spread in scientific discourse. The journal's editor, John Maddox, heaped heavy skepticism on the theory in his initial editorial on nuclear winter, asking that Sagan's conclusions "be plainly stamped with the label QUALITATIVE for fear that their apparent precision may prove spurious." In ensuing commentary, he declared that it was "too soon" to draw conclusions about the climatic consequences of nuclear war. The TTAPS paper, he wrote, "is less than convincing," as was demonstrated by "the pardonable simplicity" of their calculations. Maddox feared for the good name of science when he counseled that "there is the strongest case for asking that the prospect of a nuclear winter

[71] Review of *The Cold and the Dark: The World after Nuclear War,* by Joseph V. Smith, *Bulletin of the Atomic Scientists,* January 1985, Vol. 41, No. 1, 49–51.

should not be made into a more substantial bogeyman than it is by those who earnestly wish to avert the prospect of nuclear war as such. By clouding the case with disputable predictions, they are in danger of weakening it."[72]

As Maddox had predicted, his editorial was but the first salvo in a "prolonged and contentious argument" over nuclear winter that played out across the pages of his journal. In the ensuing months, scientists from Finland to Australia argued over a disparate array of topics related to nuclear winter. Two scientists claimed that their "study of frost rings as indicators of climatically effective volcanic eruptions" suggested that the climatic effects of nuclear war could be "grave indeed." Others who offered their two cents discussed the importance of meteors, volcanoes, World War II fire bombings, sunspots, forest fires, and atmospheric humidity as sources of data. Alan Robock analyzed "long-term seasonal cryospheric interactions with . . . sea ice/ thermal inertia feedback," and found reason to believe that nuclear winter "might persist longer than previously calculated." When S. Fred Singer contended that a nuclear war would cause mere "patch clouds which thin out rapidly—hardly a cataclysmic nuclear winter," other scientists argued back that "the cooling need not be down to -40º C nor last for several weeks or more to produce devastating agricultural damage." They also criticized Singer for having "not offered quantitative grounds" for his doubts about nuclear winter.[73] With so much at stake, scientists eagerly

[72] John Maddox, "From Santorini to Armageddon," *Nature*, Vol. 307, 12 January 1984, 107; John Maddox, "Nuclear winter not yet established," *Nature*, Vol. 308, 1 March 1984, 11.
[73] Tom Reuter, "Telling all," *Nature*, Vol. 311, 25 October 1984, 700; A.D. Brown, "Teller's cold comfort," *Nature*, Vol. 312, 13 December 1984, 587; Valmore C. LaMarche Jr. and Katherine K. Hirschboeck, "Nuclear war models," *Nature*, Vol. 309, 17 May 1984, 203; William H. Bown, Jan Peczkis, "Nuclear war—counting the cost," *Nature*, Vol. 310, 9 August 1984, 455; Alan Robock, "Snow and ice feedbacks prolong effects of nuclear winter," *Nature*, Vol. 310, 23 August 1984, 667, 670; Jonathan Katz, "Atmospheric humidity in the nuclear winter," *Nature*, Vol. 311, 4 October 1984, 417; S. Fred Singer; and Starley L. Thompson, Stephen H. Schneider, and Curt Covey, "Is the 'nuclear winter' real?" *Nature*, Vol. 310, 23 August 1984, 625.

awaited the results from nuclear winter studies underway by the National Academy of Sciences (NAS) and the International Council of Scientific Unions (ICSU).

Adversary: Edward Teller

As he had with Pauling's antinuclear campaign, Edward Teller, the old nemesis of disarmament, rushed to confront Sagan head-on. Although Teller had in the past spoke of his belief that scientists should make information known to the public and let the government decide on a course of action, he now counseled Sagan against publicizing his information. Teller wrote to Sagan and revealed how he would combat the nuclear winter campaign: "My concern is that many uncertainties remain and that these uncertainties are sufficiently large as to cast doubt on whether the nuclear winter will actually occur," he wrote. Teller then turned to his familiar Red Scare tropes of appeasement, telling Sagan that "you are more concerned with frightening the American public than with giving them an accurate assessment of the nuclear war situation. . . . If the American public is sufficiently frightened, it will call for surrender to Soviet demands in a time of crisis. This is, of course, the path followed by Britain prior to World War II with dire consequences."[74] Teller had used similar rhetoric to good effect against Pauling, but without the Red Scare such epithets failed to stick to Sagan.

Sagan responded to Teller's challenge. In one instance, he took particular offense that Teller had dubbed him a "propagandizer." Sagan retorted,

> My own impression is that the broad outlines of the Nuclear Winter work are very well-based and repeatedly confirmed, including by workers at Lawrence Livermore Laboratory, and that any political implications have flowed directly and naturally out

[74] ET to CS, Art Broyles Version, undated, Folder: Sagan, Carl Dr., Box 283: Correspondence, Personal, ET Papers.

> of this research. You yourself told me that Nuclear Winter was the only serious unanticipated consequence of nuclear war you were aware of.

Sagan made a conscious point of appealing to Teller's respect for the scientific discipline, stating his refusal to stoop to "ad hominem arguments," as "they seem inconsistent with the method of science . . . . These are values you share. You have repeatedly stated that we must follow the truth wherever it leads." Sagan reminded Teller of the lengthy TTAPS draft he had sent in 1983: "You have yet to reply. Do you not think it better to state your scientific objections explicitly before making vague public pronouncements?"[75] Teller fired back that "a propagandist is one who uses incomplete information to produce maximum persuasion. I can compliment you on being, indeed, an excellent propagandist—remembering that a propagandist is the better the less he appears to be one."[76]

And thus a scientific duel began, waged across the pages of scientific journals and eventually in the halls of the U.S. Capitol. In a *Nature* article, Teller deemed it his "important responsibility" to inform the public about the uncertainties surrounding the possibility of nuclear winter. The article's blurb promised readers "reason to doubt" nuclear winter—not even Teller could say it was certain that a nuclear winter lay outside the realm of possibility. In the article itself, Teller argued that fallout in uninvolved nations would not be severe, and that fallout in belligerent nations would be ameliorated with civil defense. Teller criticized Sagan's claim that the TTAPS smoke data were, in Teller's paraphrase, "scientifically robust" (TTAPS had not used the term), labeling them inconclusive.[77] Teller also asserted that smoke particles would rain out of the atmosphere more quickly than TTAPS suggested. "A severe

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[75] CS to ET, Feb. 23, 1984, Folder: Sagan, Carl Dr., Box 283: Correspondence, Personal, ET Papers, emphasis in original. [Also in Folder 22.27: Nuclear Winter, continued, Box 22, HB Papers.]
[76] ET to CS, Teller Version, undated, Folder: Sagan, Carl Dr., Box 283: Correspondence, Personal, ET Papers.
[77] Teller, "Widespread after-effects of nuclear war," *Nature,* Vol. 310, 23 August 1984, 621–22, 624.

climatic change must be considered dubious rather than robust," he argued, though he admitted that a nuclear winter of some severity could not be entirely ruled out. In conclusion, Teller scolded Sagan: "Highly speculative theories of worldwide destruction—even the end of life on Earth—used as a call for a particular kind of political action serve neither the good reputation of science nor dispassionate political thought."[78]

Sagan counterpunched with a mostly scientific rebuttal in *Nature* over a year later, though a draft from late 1984 contained a great deal of anti-Teller vitriol. The original draft began with an epigraph from Robert Browning clearly intended as a pointed barb at Sagan's rival: "So absolutely good is truth, truth never hurts/ The teller." Another jab that did not survive the final edit was criticism of Teller's article as an "unrefereed" opinion, as Sagan's paper appeared in the same "Commentary" section of *Nature* as Teller's had. Sagan first stressed the solid scientific work behind the theory of nuclear winter, confessing that "we do not know of a single major flaw in the work that has so far been discovered." Sagan expected and accepted "competent criticism" as "natural and healthy," but he bemoaned "a tendency in many expert pronouncements to downplay these consequences [of nuclear war]." He accused Teller of "selective inattention to the data, misquotation of sources, occasional distortion of the facts, and simple misconceptions" in his attempt to characterize predictions about nuclear winter as "exaggerated."[79]

Sagan disputed Teller's assertions on fallout and the ozone layer as having "no quantitative foundation." His claim that the climatic effects of dust would be "by no means severe" was "a conclusion with insufficient technical base and in the face of contrary

[78] Teller, "Widespread," 623–24.
[79] Sagan, "Confidential Draft: On Minimizing the Consequences of Nuclear War," 1–2, late 1984, Folder 22.25: Sagan, Box 22, HB Papers. See also Sagan, "On minimizing the consequences of nuclear war," *Nature*, Vol. 317, 10 October 1985, 485–88.

evidence." Teller could only explain away nuclear winter by "invoking a 'meteorological miracle' . . . that is, speculative 'abnormal mechanisms,' as opposed to careful quantitative calculations."[80]

The men on both sides of this argument leveled similar accusations. Sagan pointed out explicit technical flaws, but it was Teller's "marked propensity for mixing science and politics" that truly riled him. For 45 years, Sagan wrote, Teller had accepted that there were no long-term effects of nuclear weapons. But now that scientists had found long-term effects, Teller "is vigorous in minimizing these consequences, and in reminding us that not all the evidence is in. There is a clear double standard of scientific evidence at work." Teller never counseled caution or calm, never wanted to wait until the data was in during hysterias such as the missile gap or the bomber gap. Only for antinuclear science did he counsel patience.[81]

Hans Bethe, who knew Teller well, gave Sagan some advice on parrying Teller's scientific attacks. Though most of them were quite minor, next to a passage describing the TTAPS numbers as a cautious underestimate, Bethe urged Sagan to make the passage "Stronger!" and Sagan did remove some tentative language for the final *Nature* draft. Where Sagan wrote "Blandly minimizing public perception of the dangers of nuclear war, while stridently maximizing fears of the potential nuclear adversary is a tactic becoming increasingly unworkable," Bethe scrawled: "Unfortunately it still works." Elsewhere Bethe counseled, "I would emphasize: Teller says things are uncertain. So do you. Worst-case assumption."[82] Sagan frequently did admit that he was looking at the most severe possibility

[80] "Confidential Draft," 6–9.
[81] "Confidential Draft," 19–22.
[82] "Confidential Draft," 16; handwritten notes by Bethe, undated, Folder 22.25: Sagan, Box 22, HB Papers.

of an uncertain prediction. But in public he often downplayed the uncertainties since, he argued, his political opponents, and a few of his scientific rivals, exaggerated the uncertainties in order to delay research or reconsider U.S. nuclear policy.

At this time, Teller was aligned with the civil defense industry that Sagan declared obsolete, and maintained communication with Cresson Kearny, a civil engineer, geologist, and civil defense consultant out of Colorado. In 1979 Kearny had published a book called "Nuclear War Survival Skills" through Oak Ridge National Laboratory. The "first-of-its-kind" book promised Americans "information and instructions that will significantly increase their chances of surviving a possible nuclear attack." Kearny offered advice on fallout shelter etiquette such as "homemade life-support equipment," "the processing and cooking of whole grains and legumes," and "improvised footwear and clothing." Kearny described nuclear war between the superpowers as "a tragedy," but "far from the end of human life on earth." The threat of nuclear weapons has "been distorted and exaggerated" into "demoralizing myths, believed by millions of Americans." Fear that future generations would be mutated or exterminated was based on "misleading calculations."[83]

Kearny shared Teller's complaints about nuclear winter. He wrote the physicist that the "U.S. mass media . . . proclaimed [nuclear winter] as established fact," even though Kearny knew scientists who saw only "unrealistic assumptions and erroneous calculations." The result, according to Kearny, was that "public support for needed new weapons and for civil defense continues to be undermined." Kearny closed by stating that "Nuclear Winter

[83] Cresson H. Kearny, "Nuclear War Survival Skills," Oak Ridge National Laboratory, 1979, reprinted in U.S. House of Representatives, Committee on Science and Technology, *The Consequences of Nuclear War on the Global Environment*, 98th Cong., 1st sess. Report 98–344, August 3, 1983 (Washington, D.C., U.S. Government Printing Office, 1983), 74, 80–81.

already has become part of the mythology of the Nuclear Age," and affirmed his appreciation of Teller's "efforts to develop truly defensive weapons."[84]

U.S. Government

While scientists attempted to interpret the possibility of nuclear winter, the U.S. government faced a similar challenge. In contrast to Badash's claim that the "policy debate over nuclear winter was largely limited to voices from the scientific community,"[85] many government officials embraced nuclear winter as a scientific basis for the criticism of deterrence in general, and the Reagan administration in particular, especially its forecast of "winnable" nuclear wars and promotion of SDI. The executive branch, on the other hand, steeled itself against changing course, but the debate over nuclear winter and deterrence mobilized a vast array of political and scientific figures.

Documents declassified during the 1980s indicate that the Navy instantly accepted the nuclear winter theory as plausible and kept tabs on the research even in its earliest stages. A memo written in summer 1983 discussed the research by a "Cornell group"—evidently TTAPS—and stated that their work "indicates important implications for military planning." The memo's author described the theory as a "scientifically sound first cut," while a second Navy memo suggested that the DoD study nuclear winter, as it "might be a source of embarrassment" if the Pentagon appeared oblivious to it.[86]

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[84] Cresson H. Kearny to ET, April 22, 1984, Box 3, Folder 3.1: Kearny, Cresson H. (1984–86), ET Papers.
[85] Badash, "Nuclear Winter," 79.
[86] R.F. Bacon, "Memorandum for the Chairman, Military Liaison Committee, July 26, 1983, reprinted in U.S. House of Representatives, Natural Resources, Agriculture Research and Environment Subcommittee of the Committee on Science and Technology, *The Climatic, Biological, and Strategic Effects of Nuclear War*, 98th Cong., 2nd sess. Hearing, September 12, 1984 (Washington, D.C.: U.S. Government Printing Office, 1985), 230; R.F. Bacon, "Memorandum for the Chief of Naval Operations," Oct. 7, 1983, reprinted in *The Climatic, Biological, and Strategic Effects of Nuclear War*, 229.

A memo to the chief of Naval Operations written just days after the Halloween conference stated that nuclear winter theories "are probably generally valid and will be widely accepted in the scientific community." It described the science behind nuclear winter as "extensively reviewed" by scientific experts, while the claims were "generally similar" to a study conducted by Under Secretary of Defense Richard DeLauer. These sources indicate that the Navy was not opposed to the theory but expressed great concern that the Soviet Union would exploit nuclear winter for propaganda purposes. In fact, the memo stated that the results of nuclear winter "deserve serious study." The memo's author was especially impressed with the quantitative expertise Sagan demonstrated at the conference, while he dismissed Ehrlich's presentation on biological effects as "qualitative rehash." Overall, the Navy appreciated the scientists' presentation as "a serious, credible, result of a scientific enquiry."[87] A subsequent memo agreed with the scientists' claim that "a SIOP scale exchange could result in the extinction of man" and ordered policy studies in light of this finding. Another memo to the Chief of Naval Operations offered suggestions about how to counter Soviet nuclear winter propaganda.[88]

As the Navy observed the Halloween conference, Congress reacted as well. On the morning after Sagan's Sunday *Parade* article appeared, Congressmen rushed to endorse Sagan's theory. Democratic Senator William Proxmire of Wisconsin was one of several to have the article inserted into the *Congressional Record.* In his remarks, Proxmire explained that "few persons and no Members of Congress can speak with more authority on the

[87] J.A. Lyons, "Memorandum for the Chief of Naval Operations," incl. "Technical Summary" and "Conference Synopsis," Nov. 7, 1983, reprinted in *The Climatic, Biological, and Strategic Effects of Nuclear War*, 214–21. Sagan and Under Secretary of Defense DeLauer did exchange views in correspondence in February 1984. Richard DeLauer to CS, Feb. 13, 1984; CS to DeLauer, Feb. 23, 1984, Folder 22.27: Nuclear Winter, continued, Box 22, HB Papers.
[88] L.F. Brooks, Memo, "New In-house Study," Nov. 7, 1983, reprinted in *The Climatic, Biological, and Strategic Effects of Nuclear War*, 222–23; J.A. Lyons, "Memorandum for the Chief of Naval Operations," Nov. 19, 1983, reprinted in *The Climatic, Biological, and Strategic Effects of Nuclear War*, 224–25.

consequences of nuclear war than Carl Sagan." For Proxmire the implications of nuclear winter were obvious: enact arms control measures before the arms race takes humans "down the road to suicide." He specifically proposed a "mutual and verifiable" Freeze and "an antiproliferation agreement" with "teeth." In the lower house that same day, Mel Levine (D-CA) also put Sagan's article into the *Record*. Levine stressed that nuclear winter showed how the Reagan administration's pet projects of the MX, Pershing II, and cruise missiles were "destabilizing and threaten to weaken deterrence." Levine called Sagan's article "sober reading" that disputed the Pentagon's belief "that nuclear wars are somehow winnable."[89]

Just over two weeks later, Rep. Ted Weiss (D-NY) also placed the *Parade* article into the *Record*. Weiss lamented "the suicidal policies of the two superpowers, which have brought us closer to the brink of annihilation than at any other time in the nuclear age." He mentioned that *The Day After* would force viewers to confront "the possibility of a nuclear holocaust," but even this bleak movie might "delude" Americans into thinking they could survive a nuclear war. To negate this wishful thinking, Weiss suggested the *Parade* article as a sobering antidote.[90]

On December 8, 1983, Senators Edward Kennedy (D-MA) and Mark Hatfield (R-OR), co-sponsors of the 1982 House Freeze resolution, brought Sagan to their forum on the world-wide consequences of nuclear war. Sagan mentioned the broad array of scientific institutions in the United States and overseas studying nuclear winter, and claimed that three-dimensional models appeared to confirm the TTAPS study. He adamantly implored

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[89] *Congressional Record*—Senate, "Carl Sagan on the Consequences of Nuclear War," Oct. 31, 1983, 98th Cong., 1st sess., 30031–32; *Congressional Record*, Extensions of Remarks, Mel Levine, 98th Cong., 1st sess., "Dangers of Nuclear War," Oct. 31, 1983, 30175.

[90] *Congressional Record,* Extensions of Remarks, Ted Weiss, 98th Cong., 1st sess., "The Chilling Effects of Nuclear War," Nov. 18, 1983, 34872.

his Congressional hosts to rethink nuclear policies. If U.S. nuclear weapons can accidentally destroy the United States, he asked, is a huge arsenal even a credible deterrent? Sagan made it clear that a minimum deterrent—with a maximum explosive power below the nuclear winter threshold—was feasible and patriotic for both superpowers. He described a nuclear first strike as "An elaborate way of committing national suicide," adding that "The ashes of communism and capitalism will be indistinguishable."[91]

The forum included Soviet scientists who had been studying nuclear winter. Vladimir V. Alexandrov discussed how his three-dimensional model showed "significant cooling" and other evidence that the post-nuclear war environment would be "hostile to human beings." Alexander S. Pavlov stated that a nuclear war would destroy humans, while Sergei Kapitza dubbed deterrence "dubious" and mentioned a Freeze. Yevgeny P. Velikhov spoke last and described nuclear weapons as weapons of suicide. A commentary in the *Times* saw the forum as having "made short work" of the Pentagon's arguments for SDI and claims that a nuclear war could remain limited and winnable.[92]

Later in the Senate, Proxmire again brought up nuclear winter as a strong argument for arms control. Proxmire highlighted Sagan's scientific credibility, praising the theory as "based on elaborate and careful calculations by a number of scientists from a number of countries." Recognizing the critics of nuclear winter, Proxmire declared, "I shall take Sagan and the scientific evidence. It is like putting the deans of our best medical schools up against a primitive, chain-smoking witch doctor on whether smoking contributes importantly to

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[91] "A partial transcript of the DC forum on the world-wide consequences of nuclear war, sponsored by Senators Kennedy and Hatfield, Dec. 8, 1983," in *Disarmament: A Periodic Review by the United Nations,* Vol. VII, No. 3, Autumn 1984, 34–35, 37–38; Philip Shabecoff, "U.S.-Soviet Panel Sees No Hope in an Atomic War," *New York Times*, 9 December 1983, A13.
[92] "A partial transcript," 40–42, 47, 54, 56–57, 62; Tom Wicker, "A Grim Agreement," *New York Times*, 12 December 1983, A27.

cancer and heart disease. In that case, I would take the medical school deans." When Proxmire placed a summary of Sagan's *Foreign Affairs* article into the *Record,* he confessed that it "leaves this Senator with the conclusion that a comprehensive nuclear arms freeze, followed by a massive reduction of all nuclear armaments, should remain our prime objective."[93] On May 4 Senator William Cohen (R-ME) wrote to Reagan alerting the president to nuclear winter and the TTAPS study. He urged the president to consider the "very serious policy implications" of the possibility that "even a limited nuclear exchange" could produce "devastating results, affecting . . . perhaps all of the globe." Cohen then asked Reagan to put the power of the Executive behind a serious study of nuclear winter's policy ramifications.[94]

Sagan was eager to return to Congress and influence debates over nuclear weapons. As justification, he pointed to a February 1984 letter he had received from a resident of Wisconsin. Upon meeting his congressman Steve Gunderson, Thomas Peterson wrote Sagan, "I found out that Congressman Gunderson had never heard of Nuclear Winter or any such concept."[95] In May 1984 members of Congress, and Rep. Tim Wirth (D-CO) especially, were equally eager to bring Sagan back to the Capitol. Thus Sagan arrived for a whirlwind visit at National Airport at 11.02 a.m. on the morning of Tuesday, May 15, where a member of Wirth's staff escorted him to a lunch meeting with the Democratic Caucus Committee on Party Effectiveness. A briefing of legislative assistants followed at 3.30 p.m., where he spoke on the "long-term and environmental dimensions" of nuclear weapons. The

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[93] *Congressional Record,* Senate, 98th Cong., 2nd sess., "The Real Argument for Comprehensive Arms Control," Feb. 23, 1984, 3082; *Congressional Record,* Senate, 98th Cong., 2nd sess., "Why Survival Depends on Reducing Strategic Nuclear Weapons by 97 Percent," April 25, 1984, 9805–06.
[94] Cohen to Ronald Reagan, May 4, 1984, Folder 3: Nuclear Winter, Box 196, Timothy E. Wirth Papers, University of Colorado (hereafter TEW Papers).
[95] Thomas C. Peterson to CS, Feb. 16, 1984, Folder 3: Nuclear Winter, Box 196, TEW Papers.

first day culminated in a Congressional Clearinghouse on the Future event with cocktails at 6 p.m. and "a delicious dinner" at 7 p.m.; the 41 attendees quickly set the room to "overflowing." Wednesday the 16th started early with a meeting with the Democratic Steering and Policy Committee and Democratic Whips at 8.30 a.m. After discussions with Senators Cohen and Proxmire, the editorial board of the *Washington Post* hosted Sagan at a 12.30 p.m. luncheon. The highlight of the day was a "members only" event featuring an "off-the-record dialogue" between Sagan and Teller from 3 p.m. to 5 p.m. on the first floor of the Capitol, with 82 members of Congress in attendance. Sagan at one point repeated to Wirth's staff a rumor he had heard that in preparation for the dialogue, Teller had been briefed by Secretary of Defense Caspar Weinberger and presidential science advisor George Keyworth. Wirth's assistant wryly responded that "the debate had been worthwhile if only to keep or tie up Weinburger and Keeworth for that period of time" [sic].[96]

Wirth had envisioned bringing the two scientists together after he took an interest in the heated discussions surrounding nuclear winter. He knew that Sagan and Teller "didn't like each other," but hoped they would spark political thinking. When the May 16 discussion started, Teller went first, and Sagan followed with his own statement. In his recollection of the event, Wirth recalled that while Sagan was speaking, Teller's assistant Lowell Wood moved to the back of the room, where caterers had set up a table with glasses of water. The glasses sat on big, flat, metal trays stacked on a cart, next to large containers of ice. Wood proceeded to take glasses out of the cart and put ice into them, "making a ton of noise in the process." Wirth asked Wood to stop, but a minute later he began stirring the ice cubes in the

[96] "United States Congress Congressional Clearinghouse on the Future invitation"; Wirth and Gillis Long, invitation, May 4, 1984; Wirth and Gingrich invitation; invitation to Democratic Steering and Policy Committee May 9, 1983; "Congressional Staff Briefing"; "Master Schedule for Dr. Sagan"; "Sagan—Teller Meeting Attendees," May 16, 1984; Memo to TEW from CC, undated; "Carl Sagan Dinner," May 15, 1984, Folder 3: Nuclear Winter, Box 196, TEW Papers.

metal containers. This created a "racket" in the long, narrow room. After four or five disruptions, Wood finally stopped. "The point is," Wirth reflected, "that this protégé of Teller's didn't want Sagan to be heard."[97] At 8.22 p.m. Sagan finally left D.C.

After Sagan's whirlwind tour of the Capitol, Wirth immediately took action. Along with Reps. Leach, Gingrich, Gore, and Roemer, Wirth drafted an amendment to the pending DoD budget reauthorization. The amendment mandated "comprehensive study of the atmospheric, climatic, environmental, and biological consequences of nuclear war and the implications that such consequences have for the nuclear weapons strategy and policy, the arms control policy, and the civil defense policy of the United States." A letter to their peers in the House described Sagan's theory as "a sobering premonition of a dying world," while in an interview about the amendment, Wirth stated that nuclear winter "requires a complete rethinking" of U.S. national security and arms control.[98]

On May 31, 1984, Wirth and his allies introduced the amendment on the House floor, where it sparked only brief debate. Wirth justified the amendment as necessary for full understanding of the strategic and tactical nuclear weapons programs that Congress was responsible for funding. In particular, he mentioned the "sobering" TTAPS study and described the amendment as "a moral imperative," as nuclear winter could cause "national suicide." In support of the amendment, Al Gore praised the credibility of nuclear winter studies, and added that "We need to know what these experts make of the threat of nuclear winter." Mel Levine echoed Gore in stating the need for scientific input and guidance. But

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[97] Telephone interview with Tim Wirth, Jan. 24, 2008.

[98] "Amendment to H.R. 5167," Leach, Gingrich, Wirth, Gore, and Roemer letter, May 21, 1984, Folder 3: Nuclear Winter, Box 196, TEW Papers; Caspar Weinberger, "The Potential Effects of Nuclear War on the Climate," March 1985, reprinted in U.S. Senate, Committee on Armed Services. *Nuclear Winter and Its Implications*. 99th Cong., 1st sess. Hearings, October 2 and 3, 1985 (Washington, D.C.: U.S. Government Printing Office, 1986), 73; "Nuclear Winter Interview Question Responses," 3, Dec. 5, 1984, Folder 7: Nuclear Winter—Correspondence, Box 195, TEW Papers.

since Congress could easily get scientific input from hearings and other studies, such claims suggest that the amendment was an attempt by Congress to assert control over the DoD and force it to adopt their concerns about nuclear weapons. Representative Samuel Stratton (D-NY) had earlier introduced an amendment essentially barring the DoD from conducting nuclear winter studies; after that amendment failed to pass, Wirth's passed easily, 298–98. In between the two votes on nuclear winter, the House passed funding for the MX missile by the exceedingly narrow margin of 199–196.[99]

Deterrence on Trial: First Congressional Hearings, July and September 1984

Whereas Linus Pauling appeared in front of Congress only to refute charges of communism, Sagan was often feted at Congressional hearings as a scientific hero. No one who appeared in front of Congress denied the possibility of a nuclear winter, while many witnesses and members of Congress alike used the theory to challenge the Reagan administration and the DoD. Certain themes permeated the hearings: believers in nuclear winter emphasized its scientific credibility, its profound implications for nuclear policy, and its challenge to nuclear deterrence. Opponents of nuclear winter incessantly attacked the theory's uncertainties, yet paradoxically felt enough certainty existed to state that the nuclear winter prediction only reinforced the nuclear policies of the Reagan administration.

William Proxmire, as vice-chairman of the Subcommittee on International Trade, Finance, and Security Economics, brought the first hearings on nuclear winter to order on July 11, 1984. The government's lack of reaction to the theory of nuclear winter he declared "a sad commentary," and asked the witnesses to instruct the committee on what questions

[99] *Congressional Record*—House, May 31, 1984, 5089–100.

Congress and the Reagan administration should be asking. Sagan spoke first, showing an assortment of slides including charts, graphs, and artists' renderings of nuclear winter. He hoped that nuclear winter would convince "those who have not yet seen the light" to admit that a nuclear first-strike was madness, an "elaborate and very expensive form of national suicide." In addition he wanted to alert those nations that had assumed they could safely sit out a nuclear war. Finally, he dismissed civil defense as a political prop. The witnesses that followed hailed Sagan and his research on nuclear winter: Admiral Noel Gayler deemed nuclear winter scenarios "quite reasonable," while former arms control official Paul Warnke stated that Sagan had "convincingly demonstrated" that more nuclear weapons had not made the United States more secure. The members of the subcommittee were impressed with what they saw. Senator James Sasser (D-TN) referred to Sagan as a "very distinguished" expert doing "a great service to all mankind," and used nuclear winter to criticize mutually assured destruction as "a cocked hat." Sasser particularly liked Sagan's metaphor of the arms race as "two implacable enemies standing in a room which is ankle deep in gasoline. One of them has 9,000 matches and the other has 7,000 matches. They are debating whether there is a match gap." Proxmire brought the first session to a close by describing nuclear winter as "enormous" but also "plausible."[100]

On the following morning, representatives of the DoD, the Arms Control and Disarmament Agency (ACDA), and the Federal Emergency Management Agency (FEMA) appeared before the subcommittee. Relatively hostile to these witnesses, the subcommittee essentially put deterrence on trial. Proxmire began by announcing that using nuclear

[100] U.S. Congress, International Trade, Finance, and Security Economics Subcommittee of the Joint Economic Committee, *The Consequences of Nuclear War*, 98th Cong., 2nd sess. Hearings, July 11 and 12, 1984 (Washington, D.C.: U.S. Government Printing Office, 1986), 1–2, 56, 65, 75–76, 80, 86.

weapons "may be shooting ourselves in the head," and that "the strategic options we have adopted to assure deterrence could also assure our self-destruction." Richard Wagner, Assistant Secretary of Defense for Atomic Energy, surprisingly admitted that despite some uncertainties, "there could indeed be a nuclear winter." But, he added, nuclear winter "does not change the most fundamental aspects of our policy." David Emery, Deputy Director of ACDA, attacked belief in nuclear winter on the grounds that its many uncertainties about duration, magnitude, and threshold preclude any "final conclusions." In questioning, Wagner was asked about the deterrence strategy by Rep. Parren Mitchell (D-MD), who argued that since use of U.S. nuclear weapons would also destroy the United States, deterrence lacked credibility. Wagner responded that deterrence aimed to show the Soviets that it was not in their best interests to attack. When Mitchell offered that "the deterring factor becomes a destruction factor," Wagner replied "that is the essence of deterrence."[101]

After Proxmire noted the importance of a DoD representative recognizing the possibility of nuclear winter, he brought Sagan back in to critique the two administration witnesses. Sagan proclaimed himself "delighted" that the DoD and ACDA were taking nuclear winter seriously. But when Proxmire attempted to link nuclear winter to a Freeze, Emery objected. In fact, he stated, the threats to the global climate "reinforce" the administration's arms control policies, stressing, "I can't envision any study that would invalidate deterrence." When Proxmire suggested that the prospect of nuclear winter made nuclear weapons obsolete, Emery responded that "It would depend on whether or not the Soviets reacted accordingly." Proxmire insisted, stating that "the most eminent scientists in the world who are agreeing and we have the Defense Department representative saying this

[101] *Consequences of Nuclear War*, 99, 101–02, 112–13, 116, 118–119.

morning that in his judgment this is probably going to turn out to be verified and correct. It's a fact now." Emery, however, remained adamant that the threat of nuclear winter could be ameliorated by current Reagan administration policy, including the SDI.[102]

In the final session, Sidney G. Winter of the Yale economics department exclaimed that "The nuclear winter study is quite unprecedented in the credibility and explicitness of its apocalyptic speculations." The TTAPS study "must be considered to inaugurate a new era in the discussion of nuclear armaments." Furthermore, nuclear winter gave moral critiques of nuclear weapons new validity, though such views were once "dismissed as naïve and uninformed." A UPI wire story from that same day announced "Pentagon Aides Agree On a 'Nuclear Winter'" and quoted Wagner's admittance that "there could indeed be a 'nuclear winter' or there could be little effect." The story also emphasized that the Pentagon aides "held fast to the policy of deterrence."[103]

A second hearing on nuclear winter amplified the increased opposition to deterrence brewing in Congress. James Scheuer (D-NY), as chairman of the House Natural Resources subcommittee, opened the hearings on September 12, 1984, by stating that a nuclear winter might threaten the Earth's ability to sustain life. If the prediction was accurate, he continued, "the implications of this hypothesis impose new realities on our thinking about nuclear deterrence." Introducing Sagan, the well-known evolutionary biologist Stephen Jay Gould, and Edward Teller, Scheuer described the panel as "some of this country's most eminent scientists." Sagan spoke first, explaining the development of the nuclear winter theory as it had progressed from Mars and back to Earth. Sagan again made a point of emphasizing the theory's scientific credentials. Despite "differences of opinion," he said, most of the

[102] *Consequences of Nuclear War*, 130–31, 135–39.
[103] *Consequences of Nuclear War*, 147; "Pentagon Aides Agree On a 'Nuclear Winter,'" *New York Times*, 13 July 1984, A13.

"published work" shows TTAPS "somewhere in the neighborhood of what might happen." Referring to the July hearing, Sagan noted Wagner's admission that a nuclear winter could follow a nuclear war as an indication of how "the convergence of opinion" reflected a growing consensus. He then listed the policy implications, including that a first strike would be suicidal, uninvolved nations would be destroyed, and civil defense would not work. Gould, one of the 20 co-authors of Ehrlich's *Science* article, followed Sagan. He emphasized as well the "substantial agreement" with the TTAPS paper by "distinguished scientists." Gould himself was "impressed" by the "convergence of basic results." Nuclear winter, he declared, is not "fanciful conjecture" but a "remarkable convergence" that reflected the "widespread scrutiny" to which the theory had been subjected.[104]

Teller spoke last, and went on the offensive against nuclear winter, telling the committee that he had worried about far graver threats to national security. He deflected attention from his inability to deny the possibility of a nuclear winter, and instead said the TTAPS assertions were as bland a statement as 2+2=4; that no one would disagree with their result given their assumptions, but "If you make the wrong assumptions, you get the wrong results." For example, he believed that smoke would rain out of the atmosphere, but "If you assume there will be no rainout, there will be no rainout and no calculation will change it." He disputed the idea of scientific agreement on nuclear winter, claiming that repeated verification of the TTAPS model "shows nothing except that our computing machines are working."[105]

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[104] U.S. House of Representatives, Natural Resources, Agriculture Research and Environment Subcommittee of the Committee on Science and Technology, *The Climatic, Biological, and Strategic Effects of Nuclear War*, 98th Cong., 2nd sess. Hearing, September 12, 1984 (Washington, D.C.: U.S. Government Printing Office, 1985), 1–2, 4–7, 8–9.
[105] *Climatic, Biological, and Strategic Effects of Nuclear War*, 18–22.

During questioning, Teller and Sagan began to bicker. When Teller accused Sagan of misquoting Wagner, Sagan accused him of understating the danger of a temperature drop of even a few degrees. Teller responded that he was concerned about such a drop, but that extra food storage would provide adequate protection. Sagan retorted that such a plan would entail storing food for everyone on the planet. After further questions, Teller upbraided Sagan for his claim to have "robust" conclusions, and stated that such exaggeration was not "in the spirit of science." Exasperated, he told the committee, "If somebody is dedicated to exaggeration, we can't stop him."[106]

The hearings had supplied critics of the Reagan administration with ample ammunition. The executive branch appeared to feel the heat and took action to address—and attempt to co-opt—nuclear winter. An August 1984 State Department memo to Secretary of State George Schultz stated that "the implications for US policy of the nuclear winter theory as it is being argued by Turco, Toon, Ackerman, Pollack, and Sagan . . . could be profound if the administration-sponsored studies agree with Turco *et al.*'s conclusions and/ or if, by default, congressional and public attitudes are moulded by those results."[107] In September 1984 the White House Office of Science and Technology Policy announced a multimillion dollar study of nuclear winter involving the National Oceanic and Atmospheric Administration, the Department of Energy, and the Defense Nuclear Agency. One unnamed scientist, however, told *Nature* that "he fears that the administration may seek to use the scientific uncertainties as an excuse to respond only by commissioning research."[108]

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[106] *Climatic, Biological, and Strategic Effects of Nuclear War*, 32–40.
[107] Quoted in Tim Beardsley, "Mechanics of SCOPE report," *Nature*, Vol. 317, 19 September 1984, 192.
[108] Tim Beardsley, "US plans for studies proliferate," *Nature*, Vol. 311, 27 September 1984, 287.

In late September 1984 Sagan appeared on CNN, where he argued—in contrast to some of his defenders in Congress—that nuclear winter need not invalidate deterrence. Though Sagan personally opposed the concept, he publicly stated that deterrence could remain with just a "tiny" number of nuclear weapons. Since a single U.S. nuclear submarine could destroy 160 Soviet cities, Sagan explained, "how many nuclear weapons . . . do you need in order to have a reliable retaliatory capability and invulnerable deterrent? And the answer is, you do not need ten thousand strategic warheads." The U.S. and Soviet arsenals were "absurdly more than is needed for strategic deterrence," he said, recommending arsenal reductions of 90 percent or more. After the interview, he immediately joined an all-night vigil at Lafayette Park across from the White House to demonstrate for arms control and disarmament.[109]

New Study, Renewed Controversy: September 1984–April 1985

Sagan's antinuclear activism took him from Congress to Lafayette Park to sites overseas and back again, even to the nuclear testing site in Nevada, where he was twice arrested during protests.[110] He also defended the nuclear winter theory in print. Responding to *Nature*'s dismissive editorials, TTAPS wrote to the journal's editor in September 1984. The scientists argued that "we took extraordinary measures to have our calculations reviewed by a large number of experts," and questioned "whether Maddox has carefully read the work he is criticizing." Sensing that Maddox ultimately disapproved of the public campaign based on nuclear winter, they countered that "open and informed debate on this issue is the only

[109] "Carl Sagan on Nuclear Winter," transcript of interview with CNN, 1, 7, 22, Sept. 25, 1984, Folder 3: Nuclear Winter, Box 196, TEW Papers.
[110] On Sagan's direct action protests at the Nevada testing site, see Davidson, *Sagan*, 376, and Poundstone, *Sagan*, 339.

responsible approach, given the gravity of the potential climatic catastrophe we believe we have uncovered." Maddox confirmed their suspicions in his response, when he wrote, "it seems to me improper that the results of calculations should be published even in sober language without a warning to all potential readers of the pitfalls there must be. This is doubly unfortunate when, as on this occasion, a purportedly scientific publication is so fully amplified by popular articles . . . in *Parade*."[111] It was the popularization that burned more than the science. Another scientist questioned the evidence TTAPS had relied on, asking, "what good is a technique that is only *partly* calibrated? . . . [And] of what real comparative value is the planet Mars? . . . And as for citing lots of background material and getting the opinions of a large number of experts, what does that *prove*? Absolutely nothing, which is precisely the point made by John Maddox, and one which I heartily endorse."[112] After accusations of faulty evidence, political bias, and headline-seeking, the fate of the nuclear winter hypothesis appeared to hinge on the opinion of the National Academy of Sciences.

The NAS Report

The National Academy of Sciences (NAS) released their highly anticipated report on nuclear winter in late 1984, but rather than settle the debate, controversy ensued over what the report actually said. The NAS scientists did appear to endorse Sagan's urgency, stating that because so much was at stake, they found it prudent to make at least general predictions. Despite "enormous uncertainties," the NAS recognized the possibility of "severe" effects. They offered "qualified, preliminary" estimates, "clearly and emphatically of an interim character," that the planet "could be severely affected." Fires, smoke, soot, and dust, would

[111] TTAPS and John Maddox, "Nuclear winter to be taken seriously," *Nature*, Vol. 311, 27 September 1984, 307.
[112] Sherwood B. Idso, "Calibrations for nuclear winter," *Nature*, Vol. 312, 29 November 1984, 407.

block sunlight, but the amount of any temperature drops remained uncertain. Ozone could be substantially reduced, while damage to the Southern Hemisphere "cannot be ruled out." Because specific quantitative conclusions were impossible, the academy recommended "a major effort" to remove the uncertainties.[113]

With such a vague statement, both sides claimed vindication. A William Broad *New York Times* article on the NAS report received the headline "'Nuclear Winter' Is Seen As Possible." The article stated the "clear possibility," according to the NAS, that a nuclear war could cause "severe drops in temperature." Broad quoted George Carrier, chair of the NAS study group, as describing the findings as "quite consistent" with the original nuclear winter studies. Broad explained that "Reaction to the report was that it enhanced the topic's overall credibility," though to support this he quoted Turco, one of the original TTAPS authors.[114] This interpretation contrasted greatly with a *Times* editorial published two days later that argued that the NAS report had proven nuclear winter "increasingly uncertain" and its effects "impossible to define." Tom Wicker presented a dissenting view in a column that appeared opposite the editorial, writing that nuclear winter "has now been given legitimacy" by the NAS, adding that "nuclear winter raises at least the possibility of human extinction following a Soviet-U.S. war. It certainly means that after such a war anything remotely like civilization would cease to exist in the Northern Hemisphere." Wicker did add that nuclear winter challenged the arms race more than it challenged deterrence.[115]

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[113] "Report of the United States National Academy of Sciences: Summary and Conclusions," in *Disarmament: A Periodic Review by the United Nations,* Vol. VIII, No. 1, Spring 1985, 110–20.
[114] William J. Broad, "'Nuclear Winter' Is Seen As Possible," *New York Times*, 12 December, 1984, A15.
[115] "Nuclear Winter, Star Wars," *New York Times*, 14 December 1984, A34; Tom Wicker, "Tambora's Lesson," 14 December 1984, *New York Times*, A35.

Nature, however, contended that claims that the NAS endorsed the nuclear winter hypothesis were gross misinterpretations. In fact, Carrier told *Nature* that Sagan had been taking "recent calculations . . . too literally." Maddox himself argued that the significance of the NAS report was "to emphasize more clearly than has been customary the uncertainties in detailed calculations of what a nuclear winter would be like or how long it would last." Although Maddox admitted that the NAS report "qualitatively confirmed" the TTAPS prediction, he also stated that the NAS assessment was "hedged around with so many qualifications that a null outcome could well be compatible with the academy committee's analysis." Maddox worried that scientists who believed in the possibility of nuclear winter "give the impression that the issue has been decided before the study is complete," and Maddox suspected that they were doing so "not for reasons that are political in the partisan sense but apparently in the belief that the threat of nuclear winter will persuade governments towards effective arms control."[116]

Sagan confronted the uncertainties directly in a letter to the *Times* denouncing obsession with uncertainties as an excuse for inaction. "Nuclear winter is not amenable to experimental verification—at least not more than once," he wrote, "and few of us wish to perform the experiment. It is possible to diminish the range of uncertainty, but not to be absolutely sure, short of nuclear war itself. This is not an unfamiliar circumstance for policy makers who must make decisions in the face of uncertainty." He also claimed that the NAS report supported the possibility of nuclear winter, a claim that a member of the NAS study

[116] Stephen Budiansky, "US National Academy urges greater caution," *Nature*, Vol. 312, 20/ 27 December 1984, 683; John Maddox, "Nuclear winter and carbon dioxide," *Nature*, Vol. 312, 13 December 1984, 593; John Maddox, "Where now with nuclear winter?" *Nature*, Vol. 312, 20/ 27 December 1984, 696.

group later disputed, interpreting the report as only identifying "the very large uncertainties plaguing all calculations of this phenomenon."[117]

The NAS report failed to solve the impasse over the theory, but science still suffered from nuclear winter fever, as the phenomenon infected the realms of international science, geopolitics, and even criminal intrigue. In *Nature*, scientists from Czechoslovakia defended the TTAPS model, while two Norwegian scientists criticized "the overly detailed examination of minutiae" of nuclear winter as "deflect[ing] attention and concern from . . . the death and suffering of hundreds of millions of human beings, starting milliseconds after the first flash and continuing for years and generations." Teller counter-rebutted Sagan's rebuttal of his article as "ambiguous and inaccurate," while a British scientist argued that "There are many areas of uncertainty . . . but the *risk* of a nuclear winter now seems undeniable." Meanwhile, Canadian scientists set controlled forest fires to create data for nuclear winter studies.[118] *Nature*'s editor noted how attempts to understand nuclear winter had inspired numerous environmental studies and spurred new developments in computer modeling and simulations, while a researcher at Sandia National Laboratory's Future Options Group admitted that he had "thought about" the implications of nuclear winter for deterrence, but was not prepared for "earnest" study of the topic.[119]

On January 28, 1985, the leaders of Argentina, Greece, India, Mexico, Sweden, and Tanzania gathered in New Delhi to issue a "Declaration on the Arms Race." Their statement

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[117] Carl Sagan, "A Nuclear Theory That Can't Be Tested," *New York Times*, 29 December 1984, 20; Johnathan Katz, "Nuclear Winter Effects Not Settled," *New York Times*, 5 January 1985, 20.
[118] Per Oftedal and John Ormerod; Frantisek Hauser and Milan Kotva, "Aftermath of nuclear war," *Nature*, Vol. 313, 28 February 1985, 732; Teller, "Climatic change with nuclear war," *Nature*, Vol. 318, 14 November 1985, 99; P.M. Kelly, "In the aftermath," *Nature*, Vol. 315, 9 May 1985, 161, emphasis in original; Tim Beardsley, "Canadian forest burn as model," *Nature*, Vol. 316, 8 August 1985, 479.
[119] John Maddox, "New ways with aggregation," *Nature*, Vol. 314, 25 April 1985, 667; Tim Beardsley, "Looking to the future," *Nature*, Vol. 315, 20 June 1985, 620.

showed that the nuclear winter campaign had indeed alerted non-nuclear states to their own peril at the hands of the superpowers. "Almost imperceptibly, over the last four decades," they stated,

> every nation and every human being has lost ultimate control over their own life and death. For all of us, it is a small group of men and machines in cities far away who can decide our fate. . . . As a result of recent atmospheric and biological studies, there have been new findings which indicate that in addition to blast, heat and radiation, nuclear war, even on a limited scale, would trigger an arctic nuclear winter which may transform the earth into a darkened, frozen planet, posing unprecedented peril to all nations, even those far removed from the nuclear explosions. We are convinced that this makes it still more pressing to take preventive action to exclude forever the use of nuclear weapons and the occurrence of a nuclear war.[120]

Most bizarrely, V.V. Alexandrov, the Soviet scientist who had been involved with nuclear winter modeling since the 1983 peer review meeting, "disappeared" while in Spain, never seen again after apparently being abducted by unidentified thugs. His associates in the United States feared a KGB plot; their Soviet counterparts saw evidence of an identical plot by the CIA. Apparently some Soviet scientists took to calling Alexandrov "the first casualty of nuclear winter."[121]

Reagan Reacts

Back in the United States, many in Congress were convinced that nuclear winter had substantial implications for U.S. Cold War policy, especially SDI. At a Committee on Foreign Relations hearing, Senator John Kerry (D-MA) suggested to Pentagon aide Richard

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[120] "New Delhi Declaration on the Nuclear Arms Race, 1985," in Edmund Jan Ozmanczyk, *Encyclopedia of the United Nations and International Agreements,* Anthony Mango, ed. (New York: Routledge, 2003, third edition), 1548–1550. See also Sagan and Turco, *Path*, 179.

[121] Vera Rich, "Nuclear winter expert vanishes without trace," *Nature*, Vol. 316, 4 July 1985, 3; Vera Rich, "Aleksandrov still not found," *Nature*, Vol. 316, 8 August 1985, 479; Tim Beardsley, "Soviet missing person," *Nature*, Vol. 317, 19 September 1985, 191; Sagan and Turco, *Path*, 135–42.

Perle that researching nuclear winter would be a better use of the $3.7 billion being squandered on the possibly "irrelevant" SDI.[122] The Reagan administration continued its awkward balancing act of questioning the validity of nuclear winter while half-heartedly trying to co-opt it as justification for existing policy. A *New York Times* article on the many government nuclear winter studies included an interview with Reagan's science advisor, George Keyworth, who stated, "Clearly this [nuclear winter] is an area of public concern. . . . It deserves far better scientific assessment than it's had to date." Keyworth also stated "If the gravest impacts turn out to be true, . . . it would limit serious consideration by the Soviets of a first strike." Elsewhere President Reagan plainly accepted nuclear winter. He mentioned that "as a great many reputable scientists are telling us, . . . such a war could just end up in no victory for anyone because we would wipe out the earth as we know it." After discussing the Tambora eruption that erased summer in 1816, Reagan rhetorically asked his interviewer, "Now if one volcano can do that, what are we talking about with the whole nuclear exchange, the nuclear winter that the scientists have been talking about?" Reagan conceded: "It's possible," though he instantly linked nuclear winter to SDI, stating "I think if you have a defensive weapon . . . Let's put it in such a way that those missiles aren't going to get to their target."[123]

The DoD delivered its report on nuclear winter as ordered to Congress in March 1985. The report contained a scientific explanation of the temperature drops possible after a nuclear war, though it avoided use of the phrase "nuclear winter." The report discussed the possibility of effects spreading to the Southern Hemisphere, but overwhelmingly emphasized

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[122] Notes, February 25, 1985, Folder 7: Nuclear Winter—Correspondence, Box 175, TEW Papers; see also *Commitments, Consensus and U.S. Foreign Policy*, Committee on Foreign Relations Hearings.
[123] William J. Broad, "U.S. Weighs Risk That Atom War Could Bring Fatal Nuclear Winter," *New York Times*, 5 August 1984, A1; "Middle East Review," *New York Times*, 12 February 1985, A10.

the uncertainties of TTAPS, detailed over several pages. The document insisted there was no way to predict biological consequences.[124]

In its discussion of policy implications, the DoD wrote that nuclear winter helps "strengthen" existing U.S. policy that a nuclear war can and must be prevented in the present through deterrence and arms control, and in the future with SDI—elements of U.S. policy that "remain fundamentally sound." To those who argued that nuclear winter made deterrence obsolete, the DoD responded that "We strongly disagree, and believe we cannot lower our standards for deterrence because of any such hope"; the many uncertainties with nuclear winter demanded no less. *Nature* summarized the DoD report tersely: "Pentagon says yes, it may happen, but 'so what?'"[125] Ensuing hearings proved that Congress did not appreciate the Pentagon's flippancy.

Deterrence on Trial, part two: Joint Hearing, March 14, 1985

The Joint Hearing on nuclear winter of March 1985 most explicitly challenges Badash's notion that the Pentagon "co-opted" nuclear winter, as the hearing's purpose was ostensibly to evaluate—but actually to harangue—the DoD's report. Democratic Representative Morris Udall of Arizona began the hearing by innocently claiming that the goal was to answer questions about nuclear winter, such as "What is it?" But since the basic questions of nuclear winter had been addressed in hearings before, the political goals quickly became clear. Udall doubted that salvation would come from technical advances, specifically SDI. "I

[124] Caspar Weinberger, "The Potential Effects of Nuclear War on the Climate," March 1985, reprinted in *Nuclear Winter and Its Implications*, 75–77, 83.
[125] Weinberger, "The Potential Effects of Nuclear War on the Climate," reprinted in *Nuclear War and Its Implications*, 83–85. See also Wayne Biddle, "Pentagon Agrees Nuclear Warfare Could Block Sun, Freezing Earth," *New York Times*, 2 March 1985, 1; Stephen Budiansky, "Pentagon says yes, it may happen, but 'so what?'" *Nature*, Vol. 314, 14 March 1985, 121.

hope we will not be distracted by strategic snake oil," he said. "The belated discovery of nuclear winter would help us to realize at long last that the path to peace may be through arms control rather than through a never-ending succession of weapons systems disguised as bargaining chips." James Scheuer of New York followed with a description of nuclear winter as the "ghoulish season," and challenged the DoD's analysis of nuclear winter, declaring, "The implications of nuclear winter impose new realities on nuclear arms control strategy and nuclear deterrence." Claudine Schneider (R-RI) piled on, calling the DoD report "delusional thinking" and unscientific. She hoped for experts "courageous enough to follow the data wherever that may lead," while Wirth similarly chimed in that the DoD ignored the many policy implications of nuclear winter.[126]

Somewhat predictably, Sagan appeared as the first witness and instantly castigated the DoD report. He joked that if it had been a seminar paper at Cornell he would have given it a D, "maybe a C minus if I was in a friendly mood." While TTAPS had recognized the "serious policy implications" of nuclear winter early on, gleaned from discussions with "senior" nuclear experts "and other masters of dark arts," the DoD had shown no evidence of recognizing those implications. He criticized the DoD for clinging to current nuclear policy, and was mystified as to why the report showed "no agonizing reappraisals" of policy in the face of "the destruction of our global civilization." Asked about disagreement among scientists, Sagan responded that "increasing numbers of competent scientists" agreed that nuclear winter "looks serious." Though he admitted there would never be 100 percent

[126] U.S. House of Representatives, Natural Resources, Agriculture Research and Environment Subcommittee of the Committee on Science and Technology, and the Subcommittee on Energy and the Environment of the Committee on Interior and Insular Affairs, *Nuclear Winter*, 99th Cong., 1st sess. Hearing, March 14, 1985 (Washington, D.C.: U.S. Government Printing Office, 1985), 1–2, 6, 10–12.

consensus, he defended some uncertainty as well within normal bounds. "Scientists tend to cloak their predictions in a shroud of ifs and buts," he said.[127]

Assistant Secretary of Defense Richard Perle followed Sagan's appearance with an aggressive condemnation of the exobiologist. "I didn't hear a word of science this morning," he began. "I heard a shallow, demagogic, rambling policy pronouncement," he continued, and gave Sagan an F. Perle fumed "there hasn't been a shred of evidence" to support any reconsideration of current policy. He admitted that nuclear winter might deserve consideration, but not until the many uncertainties had been resolved. In response to a question from Schneider about the biological and social implications of nuclear winter, Perle scoffed: "it's a bottomless pit for research." When Schneider pointed out that Perle had just advocated more research but also disdained such research as useless, Perle responded, "I realize there is disappointment that we did not at the conclusion of this report say we are going to abandon our historic attitude toward the construction of our strategic forces and the nature of our deterrent." Later he explained that current policy was undoubtedly reinforced by nuclear winter, not changed by it, despite the many doubts that surround the theory.[128]

Next, Jeremy Stone of the Federation of American Scientists argued that, in spite of Perle's claims, nuclear winter compelled "a new approach to deterrence." In sharp contrast to Stone, George Rathjens of MIT stated that despite being a "dove" and a "disarmer," he believed the "public has been misled . . . aided and abetted by the scientific community . . . where there have been careless and hyperbolic statements, even irresponsible ones." He argued that the oceans would help retain warmth and smoke would rain out, preventing a

[127] *Nuclear Winter*, 19–36
[128] *Nuclear Winter*, 37–67; John McCain of Arizona also gave Sagan an F.

disastrous nuclear winter. Stephen Schneider of the National Center for Atmospheric Research (NCAR) addressed such uncertainties, describing them as "not the same thing as no effect." In fact there was a high probability of bad consequences, and a lower probability of the worst case scenario, the extinction of humans. Congresswoman Schneider agreed, stating that though nuclear winter might have some "fuzzy parameters" and lack "scientific precision," it was "accepted" by science and even the DoD, who insisted on overstating the uncertainties.[129]

In a letter distributed to their congressional colleagues, Wirth and Rep. Jim Leach (R-IA) had deemed the DoD report unsatisfactory for its review of a paltry three nuclear winter studies, its narrow focus on SDI, and its scornful attitude toward the idea of nuclear winter in general. In February 1986, Wirth and eleven other members of Congress informed the DoD that the report was unacceptable, and mandated a second analysis. This directive ordered the DoD to look at the NCAR and Los Alamos studies, as well as more rigorous analysis of policy implications including first-strike suicide, targeting of cities, civil defense, and SDI. The second report, however, also disappointed Wirth, who described it as a "shallow" five pages.[130] With nuclear winter still contested, expectations were high for the International Council of Scientific Unions (ICSU) report due in late 1985.

Confirmation: Summer 1985–Late 1986

[129] *Nuclear Winter*, 69–79, 81–82, 125, 153–54.
[130] Letter from Jim Leach and TEW, March 6, 1985, Folder 7: Nuclear Winter—Correspondence, Box 195, TEW Papers; Wirth et al to Caspar Weinberger, Feb. 3, 1986, Folder 7: Nuclear Winter—Correspondence, Box 195, TEW Papers. The other signers were Proxmire, Gary Hart, Newt Gingrich, Schneider, Udall, Gore, Leach, Roemer, Scheuer, Levine, and McKinney. "Statement—SCOPE Press Conference," May 28, 1987, Folder 7: Nuclear Winter—Correspondence, Box 195, TEW Papers.

The SCOPE study

The ICSU's Scientific Committee on Problems of the Environment (SCOPE) began to study the environmental effects of nuclear war in October 1983 with a distinct effort "to arrive at something approaching consensus." In a letter to Wirth, a member of the committee emphasized the unique aspects of SCOPE: the study would emanate from an international group of hundreds of scientists from over 20 nations, and include a full study of climatic and biological effects.[131] When SCOPE released its study on the environmental consequences of nuclear war (ENUWAR) in late 1985, nuclear winter received a serious boost in credibility, but also a significant shift in emphasis. The SCOPE/ ENUWAR study predicted "near- or sub-freezing summer temperatures over much of North America, Europe, and north and central Asia during the first few weeks following a nuclear war." Even temperature drops of just 10° C "would probably have very significant environmental effects." Climatic disturbances could continue for months or years, rainfall would lessen, and sunlight reaching Earth's surface could decrease by 10–20 percent.[132]

In the *Bulletin of the Atomic Scientists*, Thomas F. Malone, general secretary of SCOPE in the 1970s, presented the group's findings as a response to the ICSU's call upon scientists to alert the world to the threat of nuclear war. SCOPE had originally strictly focused on "scientific knowledge of physical effects and biological responses," though at a SCOPE-ENUWAR meeting in November 1984, some participants departed from the pursuit of pure science in order to consider "ethics and morality . . . and called for further collaboration

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[131] Gilbert White to TEW, March 6, 1985, and "Report on the progress of the SCOPE ENUWAR project since October 1983, Folder 7: Nuclear Winter—Correspondence, Box 195, TEW Papers; *SCOPE 28: Environmental Consequences of Nuclear War*, 2 volumes, (Chichester: John Wiley & Sons, 1989, second edition). "ICSU project hunts for data," *Nature*, Vol. 309, 14 June 1984, 577.

[132] Mark A. Harwell and Christine C. Harwell, "Updating the 'Nuclear Winter' Debate," *Bulletin of the Atomic Scientists*, October 1987, Vol. 43, No. 8, 42–44

between science and religion in the 'quest for a just and peaceful world.'" Malone's summary of the two volumes of the SCOPE report hewed closely to TTAPS, and even emphasized that "the majority of the world's population is at risk of starvation in the aftermath of a nuclear war." As for the extinction of humanity that Ehrlich et al. had insisted "cannot be excluded" as a possibility, the SCOPE report stated, "The indirect effects [of nuclear war] could result in the loss of one to several *billions* of humans. How close [this] projection would come to loss of *all* humans is problematical, but the current best estimation is that this result would not follow from the physical and societal perturbations currently projected." Addressing the uncertainties that had dogged nuclear winter research from the start, Malone explained: "Whatever uncertainties remain . . . would have a relatively minor impact on the crucial issue of agricultural production and therefore do not call into question the credibility of the report's findings." SCOPE had avoided use of the term "nuclear winter" not because it was misleading, Malone explained, but because "even modest temperature changes would devastate global agriculture. It is not necessary to envision a world dripping with ice before such disasters would occur."

The study offered "substantial reason to believe" that temperature and precipitation changes and a decrease in sunlight would follow a nuclear war. Such disruptions "would cause large numbers of deaths associated with insufficient food." In conclusion, "the indirect effects on populations of a large-scale nuclear war—particularly the climatic effects caused by smoke—could be more consequential globally than the direct effects, and *the risks of unprecedented consequences are great for noncombatant and combatant countries alike*." "The scientists have spoken," Malone averred, "and policy-makers now have much to do by way of framing an appropriate response." Malone praised the SCOPE-ENUWAR report in distinctly moral

terms, hoping it would provide "scientific foundation" to the "intuitive ethical concern" expressed by scientists at a September 1982 meeting in Rome, where they declared: "It is the duty of scientists to help prevent the perversion of their achievements and to stress that the future of humankind depends upon the acceptance by all nations of moral principles transcending all other considerations."[133]

A memo from Wirth's office praised the "comprehensive" nature of the SCOPE study, and noted that the "survival of our species may depend on re-tooling our nuclear defense policy."[134] But after the study's broad release, even many of the nuclear winter theory's old antagonists began to change their minds. The *Times* asserted that the new study "sharply questioned" previous judgments that a nuclear war could be survived. The new findings on smoke in particular suggested that while "hundreds of millions" would die instantly, a nuclear conflict "could be followed by a nuclear winter that would kill hundreds of millions more." The editorial went on to state that "These judgments of nuclear effects carry many possible implications for nuclear strategy." Overall, "deterrence must not be allowed to fail," and nuclear arsenals must be reduced. An article in *Nature* stated: "The likelihood of a nuclear winter . . . is thus given further support," while Schneider of the NCAR said that SCOPE's findings "should enhance the credibility of suggestions of a nuclear winter."[135]

The SCOPE study even forced John Maddox to begrudgingly admit that TTAPS had not been too far off the mark. After trudging through SCOPE's "monumental report,"

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[133] Thomas F. Malone, "International Scientists on Nuclear Winter," *Bulletin of the Atomic Scientists*, December 1985, Vol. 41, No. 11, 52–55, emphasis in original; see also "Declaration on Prevention of Nuclear War," *Bulletin of the Atomic Scientists*, December 1982, Vol. 38, No. 10, 4–5. *SCOPE 28*, Vol. II, 490.
[134] Memorandum/ Talking Points, Sept. 11, 1985, Folder 7: Nuclear Winter—Correspondence, Box 175, TEW Papers.
[135] "Rethinking Nuclear War," *New York Times*, 29 September 1985, E20; Peter Gambles, "Cautious support from SCOPE," *Nature*, Vol. 315, 13 June 1985, 534. See also Tim Beardsley, "International committee echoes gloomy forecasts," *Nature*, Vol. 317, 19 September 1985, 191.

Maddox relented that "For the time being, the nuclear winter must firmly be listed among the consequences of substantial nuclear wars, remembering that time (but, it is hoped, not experience) will show that even present fears are immoderate." He added that "from now on it will probably be wise to follow SCOPE's conclusion that nuclear winter is a probable consequence of nuclear war." But Maddox, determined to find something to criticize, chastised SCOPE for having "steered clear of taking up the implications of its conclusions." He worried about the "obvious danger" that "technical arguments and conclusions will be misinterpreted."[136] Yet just two weeks earlier, Maddox had attacked those who believed the nuclear winter theory for trumpeting the policy implications of the theory. Nuclear winter, he had argued, was not a "unique" argument for arms reductions—"people's fond wish to sleep at night is a sufficient case for that." He had also complained that "the assumption that all nuclear wars must be all-out wars is surely mistaken." To the contrary, Maddox wrote, "there is no obvious way in which the concept of nuclear winter affects strategy and international relations in ways that are unique."[137]

Further discussions in *Nature* further revealed that for many critics of the nuclear winter theory, Sagan's primary sin had not been bad science, but the poor taste of using science to support political activism. K.A. Emanuel, of MIT's Center for Meteorology and Physical Oceanography, wrote that the fear of nuclear winter was an outgrowth of many scientists' beliefs that "responsibility for the influence of their achievements rests partially with them and should not reside solely with politicians." He continued, "this attitude has at times tainted the objectivity that is crucial to the scientific endeavor." In particular, he

[136] John Maddox, "What to make of nuclear winter," *Nature*, Vol. 317, 19 September 1985, 189–90.
[137] John Maddox, "Nuclear winter can cross Equator," *Nature*, Vol. 317, 5 September 1985, 11. It is unclear why Maddox felt that the nuclear winter theory should be criticized for its non-unique implications for nuclear strategy, though perhaps he simply wanted to downplay the theory's importance while still supporting nuclear arms control.

asserted that nuclear winter research "has become notorious for its lack of scientific integrity," evidently dismissing the SCOPE report. Emanuel attacked not only the uncertainties, but also "the appearance of the results in popular literature before being exposed to the rigours of peer review," even though the TTAPS article had in fact been accepted by *Science* by the time the *Parade* article appeared. Emanuel could not deny that "serious research is beginning to qualify the earlier bold assertions," though he hoped such results would remove nuclear winter "from a means of political advocacy to a scientific exercise."[138] Schneider and Starley Thompson defended their own work on nuclear winter as exemplary precisely because they had refrained from political statements and instead followed "a logical progression of scientific research." George Rathjens of MIT continued to deplore the "irresponsible" statements to the press and the public about nuclear policy, while yet another attack on Sagan fretted that because of inaccurate statements, "credibility is lost and messages from the scientific community will eventually be ignored." Still another critic scored TTAPS for "grasping at some very strange straws" to prove their hypothesis, and accused them of ignoring evidence against nuclear winter.[139]

The Final Hearing, October 2 and 3, 1985

As scientists started to come to terms with nuclear winter, members of Congress continued to struggle with the Reagan administration and the defense establishment. The final nuclear winter hearing took place on October 2 and 3, 1985, under the auspices of the Committee

[138] K.A. Emanuel, "Towards a scientific exercise," *Nature*, Vol. 319, 23 January 1986, 259.
[139] Schneider and Thompson, "The mesoscale effects of nuclear winter," *Nature*, Vol. 320, 10 April 1986, 491–92; Tim Beardsley, "Has winter become fall?" *Nature*, Vol. 320, 13 March 1986, 103; Joseph B. Knox and Charles S. Shapiro, "The real hazards of nuclear fallout," *Nature*, Vol. 321, 1 May 1986, 21–22; S.B. Idso, "Nuclear winter and the greenhouse effect," *Nature*, Vol. 321, 8 May 1986, 122.

on Armed Services, chaired by Barry Goldwater. The elder Republican Senator from Arizona admitted at the start that he believed in the "scientific possibility" of nuclear winter, though he emphasized that preventing war "has been the centerpiece of our deterrence philosophy" since the birth of the arms race. C.M. Gillespie of the Defense Nuclear Agency spoke first and repeated concern over nuclear winter's uncertainties. He said that the oceans would help retain warmth, that TTAPS was a worst-case scenario, and that humans would not go extinct—though theorists of nuclear winter had stopped voicing such fears. The NAS and SCOPE studies he described as "upper limits for any realistic assessment of what the exchange scenarios would be," though he did agree that the Southern Hemisphere would receive smoke, "which ultimately leads to famine and starvation." Mark Harwell, a participant in the SCOPE study, argued that biological systems were so fragile "that many of the uncertainties that Dr. Gillespie was referring to are unimportant." In a nuclear war, he estimated that more people would die in India than in the United States and Soviet Union combined; more would die in Africa than in Europe.[140]

During questioning, Harwell argued that even a moderate temperature drop would be calamitous. Gillespie repeated that claims of the extinction of humans were "not based on any very careful or thoughtful assessment." But Gillespie agreed that nuclear winter had policy implications "having to do with the credibility of deterrence," while Michigan Senator Carl Levin (D) wondered if nuclear winter would be a better, cheaper deterrent than SDI.[141]

On the hearing's second day, Goldwater declared that his committee "will continue to support deterrence of war though the modernization of our deterrent forces." William Cohen, on the other hand, questioned current nuclear policy. "What if deterrence fails?" he

[140] *Nuclear Winter and Its Implications*, 2, 5–9, 15–18
[141] *Nuclear Winter and Its Implications*, 93–108.

wondered, and denounced the DoD's report as a "last-minute effort." Sagan then appeared in front of the committee, mentioning a Los Alamos study that showed how "heating of the sun will loft the smoke to still higher altitudes." This process would make nuclear winter worse than originally predicted because air—and thus the particulate matter in the air—travels faster at higher levels. He explained what he saw as the implications for military and strategic policy, including how smoke could cause satellite and communications problems, as well as his oft-repeated theories of the suicide of a first strike, the folly of civil defense, and the danger for uninvolved nations. Finally, he criticized SDI as too risky and untenable to serve as a real defense.[142]

Perle again followed Sagan as a witness. He told the committee that the contribution of nuclear winter to "our thinking about war and peace is far from clear," and bemoaned the "mindless" support of a Freeze in light of nuclear winter; he also labeled Sagan's critique of SDI as "looney tunes." During questioning, Sam Nunn of Georgia (D) told Perle he was "confusing" the public, while Sagan argued that TTAPS was no exaggeration. He repeated that the superpowers could maintain arsenals that were too small to cause nuclear winter but large enough to deter the other side. Gary Hart of Colorado (D) told Sagan, "Doctor, those of us who are not scientists practically worship the precision of science, the certitude of science." Yet with SDI, "the scientific community" is "at wide variance." Sagan forthrightly explained the differences as simply and entirely political. Perle again blasted the uncertainties of nuclear winter, while Cohen argued back that Perle was demanding more precision from nuclear winter while doubting the need for more research.[143]

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[142] *Nuclear Winter and Its Implications*, 110, 124–32.
[143] *Nuclear Winter and Its Implications*, 133–35, 137, 140, 141, 143, 147, 150, 151. See also Maxine Clarke, "US arms control policy doubts," *Nature*, Vol. 317, 10 October 1985, 466.

As Congress and defense officials remained locked in struggle over nuclear winter, the theory continued to move people worldwide. In April 1986, Bishops of the United Methodist Church released a statement on Cold War nuclear policy that drew moral outrage from nuclear winter. Their pastoral letter stated: "We have said a clear and unconditioned 'no' to nuclear war and to any use of nuclear weapons. We have concluded that nuclear deterrence is a position which cannot receive the church's blessing." A "foundation document" that accompanied the letter directly linked their challenge to nuclear winter: "We write in defense of Creation. We do so because the Creation itself is under attack. Air and water, trees and fruits and flowers, birds and fish and cattle, all children and youth, women and men live under the darkening shadows of a threatening nuclear winter." The declaration continued: "*Nuclear deterrence* has too long been reverenced as the idol of national security. . . . The moral case for deterrence, even as an interim ethic, has been undermined by unrelenting arms escalation. . . . Nuclear deterrence has become a dogmatic license for perpetual hostility between the superpowers and for their rigid resistance to significant measures of disarmament." The Bishops concluded, "The ideology of deterrence must not receive the churches' blessing."[144]

Soviet officials and other leaders apparently also endorsed the theory. In a speech at the United Nations, Soviet Foreign Minister Eduard Shevardnadze mentioned nuclear winter, and expressed his desire that, in the future, "the word 'winter' . . . [would] retain in all languages of the world the one and only meaning, its original one, and be identified solely with the season of the year which is beautiful and joyful in its own way." Gorbachev himself said in August 1986: "The explosion of even a small part of the existing nuclear arsenal

[144] "Excerpts From Pastoral Plan," *New York Times*, 27 April 1986, 34, emphasis in original.

would be a catastrophe, an irreversible catastrophe, and if someone still dares to make a first nuclear strike, he will doom himself to agonizing death, not even from a retaliatory strike, but from the consequences of the explosion of his own warheads." After France paid New Zealand reparations for sinking Greenpeace's *Rainbow Warrior* vessel, the New Zealand government used the money to fund a study of nuclear winter, while the British Labour party relied specifically on nuclear winter for their opposition to the nuclear defense of Europe in 1986.[145] At a European Nuclear Disarmament (END) conference in June 1985, a workshop on British-French cooperation recommended the dubbing into French of a nuclear winter film.[146]

"Tell the truth about the Nuclear Winter phenomenon to our people": Science and Human Rights

Though potent, scientific antinuclear activism never involved more than a minority of scientists. Hugh Gusterson, in his anthropological study of the Livermore weapons lab at the end of the Cold War, found that plenty of Livermore scientists were still inclined to endorse nuclear deterrence. The tremendous peace movement of the 1980s rooted in antinuclearism eventually came to focus less on nuclear weapons and more on human rights. Nuclear weapons, though still a menace, were but a symbolic display of the superpowers' lack of concern for fundamental human rights and dignity. When historian E.P. Thompson launched END in 1980, he declared its goal as "tearing down the ideological East/ West frontiers." At the July 1984 END conference in Perugia, Italy, participants struggled over the

[145] "Excerpts From U.N. Speeches by Shultz and Shevardnadze," *New York Times*, 25 September 1985, A8; quoted in Sagan and Turco, *Path*, 183; Sagan and Turco, *Path*, 181.
[146] END French Group Newsletter, 3, No. 2, Sept. 1985, European Nuclear Disarmament Papers, London School of Economics and Political Science (hereafter END Papers).

direction of the movement. Czechoslovak delegates criticized the "myopic pacifism" that failed to recognize that "peace and human rights were indivisible." Separation of the two, they declared, would be comparable to "building peace on the ruins of civil liberties." One Italian declared, "I request freedom, respect, and the right to knowledge, intervention and speech for those in the Eastern countries who are discussing, criticising and asking questions in the name of peace and human rights. . . . I cannot conceive of the defeat of nuclear power unless people know, think, count and intervene, especially where the masters of the missiles are in power." The British Foreign and Commonwealth Office concluded in a briefing paper that "It was clear that many at the Convention wanted to develop closer links with 'the grass-roots activists in Eastern Europe.'"[147]

But scientific protest against the arms race and the Cold War was more resonant than has been recognized, as science and scientists played a substantial role in Soviet human rights activism, especially the physicist Andrei Sakharov. The 1980s saw the issue of human rights grow to tremendous importance, becoming one of the major factors that led to the end of the Cold War, according to historian Sarah B. Snyder. Sagan's nuclear winter campaign had challenged both U.S. and Soviet nuclear policy, alleging that both superpowers failed to adequately address the danger of, and even at times the possibility of, nuclear winter. In 1985, Sagan asked of the U.S. government, "Is there a serious study ongoing in the Government? Is there money for research outside the Government on policy and doctrinal implications? Why is it that there is such a sparse amount of money available for studying nuclear winter and its implications?"[148] Sagan had hoped his nuclear winter

[147] "Third European Nuclear Disarmament (END) Convention, Perugia, July 1984," September 1984, FO 973/ 386, UK National Archives, 3.
[148] *Nuclear Winter and its Implications*, 146.

campaign would influence Soviet scientists and government officials, and accordingly he brought V.V. Alexandrov of the Soviet Academy of Sciences into nuclear winter studies during early stages of research.[149] In congressional hearings, Sagan expressed pleasure that "from publications in Pravda and programs on Soviet television, that there has been widespread coverage in the Soviet Union of the nuclear winter issue . . . so there is continuing public discussion in the Soviet Union about nuclear winter."[150] Alexandrov himself addressed Congress, stating that "So as for newspapers and magazines, I think we have pretty good coverage of this topic in the Soviet Union."[151] Elsewhere, Sagan explained that "there has been at least some permeation of the understanding of nuclear winter to the Soviet public."[152]

But the nuclear winter theory unexpectedly played a small part in the dissent brewing in the mid-1980s Soviet Union. Sagan probably did not know of the role his nuclear winter campaign had on Soviet dissident demonstrations, including the Moscow Trust Group, a human rights and peace organization that served "as a forum for independent discussion about major world problems, and for the exchange of knowledge and suggestions with visiting peace activists from the west." The group hoped to "facilitate an increase in the amount and extent of direct contacts and free exchange of information between the peoples of east and west." While the Trust Group did not publicly criticize the Soviet government for fear that "it would be much more brutally repressed if it publicly adopted a clearly critical and oppositional stance," members of the group were frequently arrested and imprisoned. END contacts reported that Soviet officials arrested physician Vladimir Brodsky in July 1985

[149] Paul Ehrlich et al, *The Cold and the Dark: The World after Nuclear War*, New York: Norton, 1984, xviii.
[150] *The Climatic, Biological, and Strategic Effects of Nuclear War*, 45.
[151] *The Climatic, Biological, and Strategic Effects of Nuclear War*, 52.
[152] *Nuclear Winter*, 34.

on charges of "hooliganism," which included "transmitting a letter to the Soviet Academy of Sciences requesting greater publicity about the nuclear winter." In late November Brodsky was transferred to a Siberian labor camp "so that he will experience the coldest possible conditions during his 3-year term of enslavement."[153] Brodsky went on hunger strike for two months at prison, and subsequently endured force-feedings.[154] Actions such as Brodsky's nuclear winter demands were part of a broad array of nonviolent demonstrations.[155] During a rally at Gagarin Square on April 12, 1985, protesters shouted "tell the truth about the Nuclear Winter phenomenon to our people." This demonstration came on the heels of one on March 9, where protesters chanted, "no more prisoners of peace." On May 16, the Trust Group demonstrated for "Solidarity of grass-roots peace activists East and West." Mass arrests followed each demonstration.[156] Soviet activists thus demonstrated their willingness to go to jail for nuclear winter, or rather that information on nuclear winter was on a par with other freedom demands. In dissidents' minds, Soviet authorities' refusal to inform the public about the possibility of nuclear winter reinforced their belief that the Soviet Union cared nothing for its people. In the United States, the Reagan administration took an opposite approach, quickly denouncing the theory and keeping it from drastically reshaping Cold War policy.

In another human rights case, END members believed that Soviet authorities had singled out linguist Oleg Radzinsky as a leader of the "academic/ scientific wing" of the movement, one of the "social groups inclined to support the Trust Group."[157] Radzinsky

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[153] Roderic Pitty, "A brief report on the situation of the MOSCOW TRUST GROUP," November 23, 1985, S1-85-11-23-1, END Papers.
[154] Letter from Peter Murphy, Sept. 12, 1985, S1B-85-9-12-1, END Papers.
[155] "Visit to Yuri and Olga Medvedkov 7.11.85," November 7, 1985, S1-85-11-7-1, END Papers.
[156] "Yori to Oleg Popov," August 4, 1985, S1A-85-8-4-1, END Papers.
[157] "News from USSR," undated, S1-84-2-0-1, END Papers.

was soon arrested and later placed in a psychiatric hospital. After a trial, Radzinsky was sentenced to a year's imprisonment and five years' internal exile. During Radzinsky's trial, documents used against him included one that claimed that "Uninformed people cannot be involved in discussion of disarmament problems because of their complexity. The appeal for freedom of information on armament and disarmament issues is tantamount to intending to reveal Soviet defence capabilities to a potential aggressor."[158] Elsewhere, an END paper discussing "relations between the British Nuclear Disarmament Movement and Official Soviet Bodies" suggested "links between anti-nuclear scientists and medical people" as one of the "valuable areas of contact with the USSR," though it did not elaborate on this statement.[159] While Soviet scientists wrestled for their basic rights, scientists in the West approached nuclear winter as a scientific question, one that was quickly becoming answered.

A Growing Consensus: late 1986 to 1989

The Los Alamos study initiated by Bethe resulted in four articles showing the results of a three-dimensional simulation of nuclear winter that gradually appeared between 1983 and 1986 in *Science*, the *Journal of Atmospheric Sciences*, and the *Journal of Geophysical Research.*[160] In recognition of the work, Los Alamos officials nominated Robert Malone, Lawrence Auer, and Gary Glatzmaier for a Distinguished Performance Award in 1986. The nomination letter

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158 Gerard Holden and Edward Thompson, "Protest for gaoled Soviet teacher," *END Journal*, No. 3, 7; "Documents condemn Trust Group," *END Journal*, No. 7, 7.

159 Stephen Shenfield, "Discussion Paper for END and CND," undated, S1-83-0-0-1, END Papers.

160 Robert C. Malone, Lawrence H. Auer, Gary A. Glatzmaier, Michael C. Wood, and Owen B. Toon, "Influence of Solar Heating and Precipitation Scavenging on the Simulated Lifetime of Post-Nuclear War Smoke," *Science,* Oct. 18, 1985, Vol. 230, 317–19; Eric J. Pitcher, Robert C. Malone, V. Ramanathan and Maurice L. Blackmon, and Kamal Puri and William Bourke, "January and July Simulations with a Spectral General Circulation Model," *Journal of the Atmospheric Sciences,* Vol. 40, No. 3, March 1983, 580–604; V. Ramanathan, Eric J. Pitcher, Robert C. Malone, and Maurice L. Blackmon, "The Response of a Spectral General Circulation Model to Refinements in Radiative Processes," *Journal of the Atmospheric Sciences*, Vol. 40, No. 3, March 1983, 605–30; and Robert C. Malone, Lawrence H. Auer, Gary A. Glatzmaier, and Michael C. Wood, and Owen B. Toon, "Nuclear Winter: Three-Dimensional Simulations Including Interactive Transport, Scavenging, and Solar Heating of Smoke," *Journal of Geophysical Research,* Jan. 20, 1986, vol. 91, No. D1, 1039–53.

downplayed the other studies of nuclear winter, stating that "The groups at Livermore, NCAR, and in Moscow began work on this problem but seemingly in a politically-charged, media-oriented atmosphere in which a premium was placed on quick results, too often at a cost of superficiality." The letter then explained the significance of Auer, Malone, and Glatzmaier's work: "During 1984 and 1985, the Nuclear Winter team successfully modified the computer program and have demonstrated that a number of processes (such as injection profiles and late-time rainout) have less effect on the results than had been previously estimated. And most importantly, they demonstrated the importance of self-lofting of the heated, smoke-ladened air." Eric Jones, the memo's author, expressed gratitude for Bethe's "large role in getting this work started."[161] Bethe supported the nomination of Malone, Auer, and Glatzmaier for the award, writing, "Their work on Nuclear Winter is outstanding," he wrote. "In my opinion it is by far the best that has been done in this active field." Bethe elaborated on Jones's praise:

> Their result answers essentially all the questions I have had on the subject of Nuclear Winter since I first became interested in this phenomenon about 1982. The interaction of the various physical processes has a very major effect on the prediction of the effects of Nuclear Winter. Essentially it makes Nuclear Winter worse, especially if the nuclear weapons are exploded in summer. It is very important that this result was derived from purely scientific calculations, without any political bias. . . . I believe that this work is one of the best scientific efforts that has come out of the Los Alamos laboratory ever.[162]

Bethe, of course, had been at Los Alamos during the Manhattan Project, and while an award letter would obviously lean toward hyperbole, Bethe was not in general prone to exaggeration.

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[161] Eric M. Jones, memorandum to C.F. Keller, March 13, 1986, Folder 22.26: Nuclear Winter, Box 22, HB Papers; Eric M. Jones, Los Alamos, to HB, March 13, 1986. Folder 22.26: Nuclear Winter, Box 22, HB Papers.
[162] HB to Charles Keller, March 18, 1986, Folder 22.26: Nuclear Winter, Box 22, HB Papers.

Research conducted by scientists from SCOPE to Los Alamos had begun to confirm the nuclear winter theory, and even sometimes suggested that TTAPS was a conservative estimate. In contrast, a 1986 GAO report to Congress argued that uncertainties continued to plague the nuclear winter theory; the report's title, "Nuclear Winter: Uncertainties Surround the Long-Term Effects of Nuclear War," indicated as much. The GAO report stated that "controversy" over nuclear winter "has polarized views about its scientific basis and potential policy implications." The only "consensus" that the GAO observed was that "further research" was required. The report made no definitive statement on the implications of nuclear winter, but did hypothesize that nuclear winter could challenge deterrence—and might even *be* deterrence.[163]

Other studies also took shots at the credibility of the original TTAPS paper while basically confirming its findings aside from human extinction. Thompson and Schneider of the NCAR published "Nuclear Winter Reappraised" in *Foreign Affairs* in the summer of 1986. At the time, this article sounded the death knell for the nuclear winter theory. Thompson and Schneider began, as had Sagan, with a scientific discussion of doomsday scenarios. As far as such scenarios were concerned, nuclear winter "has been much more compelling scientifically," they wrote. The NAS and SCOPE studies had "helped to legitimize" nuclear winter as a topic of serious scientific research. But the main importance of nuclear winter, they argued, was its three serious challenges to the credibility of U.S. nuclear policy: the threat of human extinction, the suicidal risk of a first strike, and the existence of a threshold. Their survey of recent studies had convinced Thompson and Schneider that nuclear winter had been reduced "to a vanishingly low level of probability." The original "global freeze

[163] GAO Report to Congress, "Nuclear Winter: Uncertainties Surround the Long-Term Effects of Nuclear War," March 1986, 2–3, 16, 30.

scenarios" and "apocalyptic conclusions" predicted by TTAPS were not likely "on scientific grounds," nor was there evidence of a threshold. Thus the two aspects of nuclear winter "with the most important implications for policy have been removed." The three dimensional models showed, according to Thompson and Schneider, that average temperature changes "are considerably smaller" than the TTAPS one-dimensional model, changes that "more closely describe a nuclear 'fall' than a nuclear winter." Specifically, ocean warmth, smoke rainout, and a smoke-enhanced greenhouse effect would prevent severe cooling. They somewhat incongruously added that the remaining problems of nuclear "fall" could "produce unprecedented worldwide human misery," such as "mass starvation" in India. In all, Thompson and Schneider concluded, the global effects could still outweigh the direct effects of nuclear war.[164] Although heralding the defeat of the nuclear winter theory, Thompson and Schneider's article merely gave the effects of nuclear winter a different and less-catchy name.

As nuclear winter became established science, its political credibility sank. But it seems unlikely that this was mere coincidence—only when nuclear winter was stripped of its political content could it become real science in the eyes of many. In addition, Cold War tensions were dwindling. In a *Times* story about "nuclear autumn," Thompson regretted how scientists had gone public with nuclear winter: "People really have in their minds the image of frozen lakes and frozen cornfields and having to dig through frozen ground to bury the dead, and those images are too extreme. It was an excellent attention grabber, but those deep-freeze images are an exaggeration." Turco disputed the notion that a milder scenario overturned the original concern, asking "Does the world have to freeze to an ice cube before

[164] Starley L. Thompson and Stephen H. Schneider, "Nuclear Winter Reappraised," *Foreign Affairs,* Summer 1986, Vol. 64, No. 5, 981–83, 989, 993–94, 998, 1005.

people become concerned about what's going to happen?" Schneider admitted receiving "a lot of unhappy reaction from our former friends," but consoled himself by pledging his allegiance to scientific objectivity: "One has to clear the air sooner or later. . . . We're trying to substitute credibility for drama."[165]

In a similar vein, at the February 1987 AAAS meeting in Chicago, scientists "clashed sharply" over nuclear winter. According to the *Times*, "some speakers contended that the public image of science had been seriously damaged" by the debate, and "few recent issues had so highlighted the perils of injecting uncertain scientific conclusions into public policy debates." The article heralded "nuclear fall" as the new standard, and quoted a distraught George Rathjens on nuclear winter: "I think in the long run it will discredit this community of which we are all a part." *Nature* mentioned S. Fred Singer's contention that the infrared radiation of smoke and ice-clouds would mitigate nuclear winter, arguments that Schneider dismissed as an "infrared herring," while elsewhere Edward Teller held fast to deterrence. Sagan biographer Poundstone also accepted nuclear fall as the final word, praising the NCAR as having created the "freest research climate" possible.[166]

But a continuation of the SCOPE/ ENUWAR studies showed that nuclear fall was not the final word. Members of SCOPE decided that the concept of nuclear fall, derived from Thompson and Schneider's analyses suggesting that temperature drop would be one of 15° C (27° F) in summer, failed as a descriptor. The predictions of milder effects

> have drawn attention away from and indirectly cast doubt upon the important findings of the ENUWAR study. . . . Most of the public debate has focused only on the worst-case results from this 'TTAPS' article . . . and thus there is a public

[165] James Gleick, "Less Drastic Theory Emerges on Freezing After a Nuclear War," *New York Times*, June 22, 1986, p. 1.
[166] James Gleick, "Science and Politics: 'Nuclear Winter' Clash," *New York Times*, 17 February 1987, C6; John Maddox and Joseph Palca, "Chiefs dominate indians at an annual science smorgasbord," *Nature*, Vol. 324, 26 February 1987, 750; Edward Teller, "In the worst event . . ." *Nature*, Vol. 328, 2 July 1987, 23; Poundstone, *Sagan*, 332–37.

> perception that Antarctic-like conditions in Kansas in July have been replaced by typical mid-October scenes of leaf color changes in New England. This perception is refuted by numerous scientific studies, undertaken both in the United States and abroad, which document a much broader, more compelling basis for the conclusion that a global nuclear war would create devastating environmental consequences.[167]

A February 1987 ENUWAR workshop in Bangkok again studied nuclear winter, finding no reason to revise the 1985 conclusions. In fact, the Bangkok conference reaffirmed that agricultural systems "are the most vulnerable to the physical and societal disruptions that could follow a large-scale nuclear war." Agriculture in the Northern Hemisphere would be limited or even shut down for the first (and maybe other) growing seasons after nuclear war. "For most countries, and thus for most of the people on earth, the food would run out in a matter of a few months if there were no agricultural production for just one season. . . . *Consequently, the majority of the earth's human populations is vulnerable to starvation following a large-scale nuclear war.*" As had Sagan, SCOPE pointed out that "people living far from the scenes of direct destruction and playing no central role in a nuclear war would be at a risk of losing their lives through the *indirect* effects of nuclear war. The number of people who might starve or die from diseases associated with insufficient food could vastly exceed the number of people who would die from direct effects of such a war." Though most government figures had lost interest in nuclear winter, Wirth, by this time a Senator, trumpeted the results of the 1987 SCOPE study, stating that: "So long as responsible scientists . . . say that nuclear winter cannot be ruled out, the proper course to follow is one of caution, not business as usual." He added, "Nuclear winter is simply too important to our national security to be ignored."[168]

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[167] Mark A. Harwell and Christine C. Harwell, "Updating the 'Nuclear Winter' Debate," *Bulletin of the Atomic Scientists*, October 1987, Vol. 43, No. 8, 42–44

[168] Harwell and Harwell, "Updating the 'Nuclear Winter' Debate," 42–44, emphasis in original; "Statement—SCOPE Press Conference," May 28, 1987, Folder 7: Nuclear Winter—Correspondence, Box 195, TEW Papers.

As nuclear winter increasingly fell on deaf political ears, science continued to accept its predictions. The June 1988 issue of *Environment* hailed the new SCOPE results and a recent UN study as "A new scientific consensus."[169] An overview by Sir Frederick Warner of SCOPE praised the five years of "unprecedented scientific cooperation" of the ENUWAR study as an endeavor on par with the International Geophysical Year in 1957. A Moscow conference assessing the five years of research since the initial SCOPE study "recorded wide consensus" on the climatic and environmental effects on people and ecosystems, and declared that ultimately, "The major conclusions have stood the test of time." Three-dimensional models "have confirmed the predictions that the smoke produced by the burning of the majority of cities in the combatant nations could lead temperatures to drop by as much as 15° to 30° C [27° to 54° F] for days to weeks in some regions during the Northern Hemisphere summer." Smoke particles, they discovered, had actually five times *more* absorption capacity than assumed in early nuclear winter studies; in essence, "smoke has become darker." The indirect effects of nuclear war could be greater than the direct effects, with "unprecedented consequences" for combatants and noncombatants alike, leaving "A majority of the world's population at risk of starvation." Uncertainties remained, but "confidence in the forecast of nuclear winter is now much broader and stronger than that just following its discovery in 1983."[170]

In December 1986, the UN General Assembly had directed the Secretary-General to produce a study "on the climatic and potential physical effects of nuclear war, including nuclear winter, which would also examine its socio-economic consequences." The final

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[169] *Environment*, June 1988, Vol. 30, No. 5, 1.
[170] Sir Frederick Warner, "Consensus and Uncertainties: The Environmental Effects of Nuclear War," *Environment*, 2–7; R.P. Turco and G.S. Golitsyn, "A Status Report: Global Effects of Nuclear War," *Environment*, 14.

report, completed in April 1988, showed that "a major nuclear war would entail the high risk of a global environment disruption." The greatest risk came from a nuclear war targeting cities in the summer months. A "widespread starvation" was indeed possible, and though some uncertainties remained, "residual scientific uncertainties are unlikely to invalidate this conclusion." The UN report also reflected the growing environmental consciousness of the late 1980s, linking nuclear winter with acid rain, deforestation, global warming, and ozone depletion.[171]

The findings noted "a clear convergence towards consensus." A nuclear war could create "widespread starvation," even in non-targeted nations, while "The direct effects of a major nuclear exchange could kill hundreds of millions; the indirect effects could kill billions." Furthermore, "long-term recovery would be uncertain," because socio-economic systems would break down, making recovery nearly impossible. "The scientific evidence is now conclusive that a major war would entail the high risk of a global environmental disruption," the report concluded.[172]

Near the Cold War's end, scientists continued to confirm the nuclear winter thesis. Maddox had finally made peace with nuclear winter, announcing in 1988 that nuclear winter had "come of age" as "an accepted ingredient of academic study." He admitted that a nuclear war would start fires and smoke would block the sun "for weeks or even months." Although Maddox scorned the nuclear winter "bandwagon" for its distasteful "press conferences," he confessed that *Nature* "was probably over-sour in its comments on the affair." Maddox connected nuclear winter to new concerns about global warming and the

[171] Department for Disarmament Affairs, Report of the Secretary-General, "Study on the Climatic and Other Global Effects of Nuclear War," Disarmament Study Series 18 (New York: United Nations, 1989), vi.
[172] "Study on the Climatic and Other Global Effects of Nuclear War," 6–7.

greenhouse effect, but relished a final sting at nuclear winter, hoping that scientists concerned about climate change "will respond more confidently than to the issue of nuclear winter with opinions properly hedged with specific qualifications about the ways in which the models are incorrect."[173]

Meteorologist Alan Robock summarized new experiments in 1989, stating that "The basic theory of nuclear winter has remained unchanged since it was first described." The cold and the dark would ruin at least one growing season, "resulting in a global famine similar to that seen in Sudan and Ethiopia. In a nuclear war between the United States and the Soviet Union, more people would die in India or China than in the target countries combined." Continued research since 1983 had only "strengthened the scientific basis of the theory." The concept of nuclear fall, he wrote, allowed people to believe that the effects would be mild, while they would actually be quite awful. "The consensus on nuclear winter is broad," he asserted, citing studies by the NAS, the Soviet Academy of Sciences, Los Alamos, Livermore, and many others. Noting with regret the decreased interest in Congress, Robock maintained that "The implications of nuclear winter are clear: the use of nuclear weapons would be suicide for all the peoples of the planet." Still other experiments and analyses added to the consensus.[174]

Acceptance of nuclear winter has only increased since the 1980s. Indeed, the concept has shown staying power in science. As war erupted in the Middle East in 1991, scientists—including but by no means limited to Sagan—worried that Iraq might set Kuwaiti oil wells

[173] John Maddox, "What happened to nuclear winter?" *Nature*, Vol. 333, 19 May 1988, 203.
[174] Alan Robock, "New Models Confirm Nuclear Winter," *Bulletin of the Atomic Scientists*, September 1989, Vol. 45, No. 7, 32–35; Jenny Nelson, "Fractality of sooty smoke: implications for the severity of nuclear winter," *Nature*, Vol. 339, 22 June 1989, 611.

on fire, causing thick, black clouds on a par with those predicted by nuclear winter.[175] Nuclear winter earned the ultimate sign of maturation in the early 1990s when one science textbook devoted an entire chapter to the concept. After the Cold War, Sagan transformed nuclear winter into an environmental argument incorporating the greenhouse effect and global warming. In 2006 and 2007 further analyses by Robock using the most recent computer models indicated that just "100 Hiroshima-sized" nuclear weapons could trigger a nuclear winter, a number lower than the original TTAPS estimate.[176]

In 1984, Wirth had been asked how historians would judge Sagan and the idea of nuclear winter. "With admiration and gratitude for pursuing the study of Nuclear Winter and forcing our leaders and the Department of Defense to consider the implications of this phenomenon," he had optimistically predicted. But history has not been nearly so kind. Despite the eventual validation of the theory, vindication never arrived for Sagan. One assessment accused Sagan of "willful misrepresentation" and a "relaxation of professional standards" regarding nuclear winter.[177] In a premature "postmortem" on nuclear winter, Poundstone suggests that out of kindness to Sagan, obituaries of the man "downplayed or omitted nuclear winter." Poundstone argues that the nuclear winter debate, like all scientific debates, went back and forth until scientists arrived at an agreeable medium, and that what confounded the matter was that this process took place in public. "The scaling back of

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175 "The technology of Wednesday's war," *Nature*, Vol. 349, 10 January 1991, 91; Peter Aldhous, "Oil-well climate catastrophe," *Nature*, Vol. 349, 10 January 1991, 96; K.A. Browning et al, "Environmental effects from burning oil wells in Kuwait," *Nature*, Vol. 351, 30 May 1991, 363.
176 John W. Birks, "The end of innocence," *Nature*, Vol. 349, 7 February 1991, 472; Sagan, *Billions*, 99–107; Rex Dalton, "What happens when two nations battle with nukes?" *Nature* online, 12 December 2006; Harvey Leifert, "Extreme events: Climate catastrophe," *Nature Reports: Climate Change*, Vol. 4, September 2007, 50.
177 "Nuclear Winter Interview Question Responses," Dec. 5,1984, Folder 7: Nuclear Winter—Correspondence, Box 195, TEW Papers; J.L. Heilbron, "Honesty's the best policy," *Nature*, Vol. 340, 24 August 1989, 608, review of Tony Rothman, *Science a la Mode: Physical Fashions and Fictions* (Princeton: Princeton University Press, 1989).

nuclear winter had a political dimension quite unlike most scientific work," Poundstone writes, and this political aspect led many in the public to view the revisions "as a betrayal."

But some scientists were equally agitated over Sagan's efforts to popularize the nuclear winter theory. In fact, Sagan's biographers as well as science journalist Chris Mooney have made the claim that the NAS punished Sagan for his nuclear winter campaign by rejecting his nomination for membership in 1992. Mooney writes, "there has long been a culture in the world of science that disdains mere 'popularizers' and those who shirk research for less 'pure' activities: Everyone in science remembers what happened to the great public communicator Carl Sagan, who was denied membership in the prestigious National Academy of Sciences."[178] Notably, the NAS awarded Sagan not with membership but its Public Welfare Medal in 1994, which specifically praised his popularization achievements.[179] Such a theory strains credibility, since a more plausible explanation is that Sagan's purely scientific work, though prolific, did not measure up to the high standards of the Academy.

Sagan and Turco argued in their book on nuclear winter that the controversy over nuclear winter was not about the science behind it but the theory's political implications. They protested that the nuclear winter theory had "evolved in an orderly manner."[180] Nuclear winter did not end the Cold War, it is true. But the failure of the nuclear winter campaign to drastically reshape Cold War policy should not require historians to ignore or disparage it. Before its scientific plausibility could be fully established, historic events ended the Cold War, robbing the study of nuclear winter of its urgency. Furthermore, the theory's scientific validity indicates how Reagan administration policy was based less and less on

[178] Chris Mooney, "Hard Science," *The New Republic*, April 23, 2008, Vol. 238, No. 4,834, 11–12.
[179] Poundstone, *Sagan*, 346–47, 356–58; Davidson, *Sagan*, 389–92, 397.
[180] Sagan and Turco, *Path*, 33–34.

rational, scientific thought, a trend that has continued into more recent presidential administrations. But at a time when one scientist wanted science to speak to society, other scientists tried to silence him. It was not the theory's specific political arguments, but rather that the theory was political at all that harmed it. It was the strict and inviolable apolitical nature of Sagan's beloved science itself that caused the zero sum result of nuclear winter.

Chapter Seven: "An Emotional Grassroots Offensive": Scientists, SDI, and the Moral Challenge to Nuclear Weapons in the 1980s

Nuclear winter was just one current of antinuclear sentiment that arose during the 1980s. The broader antinuclear movement drew activists who criticized nuclear weapons from a broad array of perspectives, including feminism, environmentalism, and religion. These activists consequently had little patience for the highly technical arguments for arms control offered by many scientists. The technical approach to arms control and disarmament that these scientists had adhered to since the 1950s had finally divided them from grassroots antinuclear activists. Meanwhile, government proponents of nuclear weapons painted their scientific opponents as advocates of mutually assured destruction, in another attempt to co-opt the antinuclear claims of scientists. Scientists thus had little role in the antinuclear movement; where scientists had once provided leadership in the opposition to nuclear weapons, they now could only offer their support as citizens, rather than experts.

Resistance: The Antinuclear Movement of the 1980s

At the dawn of the 1980s, stalwart arms control scientists could draw new encouragement from the legions of people around the world who mobilized for disarmament. As Paul Boyer, Lawrence Wittner, and other scholars have recognized, grassroots antinuclear activism erupted on many fronts in the 1980s.[1] For some activists, the Ronald Reagan administration's caustic remarks about the "evil" Soviet "empire" and flippant quips about

[1] Paul Boyer, *Fallout: A Historian Reflects on America's Half-Century Encounter with Nuclear Weapons* (Columbus: Ohio State University Press, 1998), 167–93, and "From Activism to Apathy: the American People and Nuclear Weapons, 1963–1980," *Journal of American History* 70 (Mar. 1984): 821–44. Lawrence Wittner, *The Struggle Against the Bomb, Vol. 3: Toward Nuclear Abolition* (Stanford: Stanford University Press, 2004).

"winnable" nuclear wars aroused ire. In other cases, antinuclear efforts evolved out of a tradition of activism forged in the civil rights, counterculture, and environmental protest movements of the 1960s and 1970s. Across the Atlantic, European protesters rushed to confront the attempts to station nuclear cruise and Pershing missiles in NATO nations as well as the deployment of Soviet SS-20s. Reflecting its many origins, the antinuclear movement of the 1980s encompassed a wide array of activists and causes. In the United States, many movement participants came to focus on Randall Forsberg's "nuclear freeze" proposal that endeavored "to stop the nuclear arms race quite literally, by stopping the development and production of all nuclear-weapon systems" in the United States and Soviet Union.[2] A political activist group of the same name led a nationwide campaign for local and federal Freeze legislation. The organization Ground Zero engaged in public demonstrations, while many smaller movements like Connecticut's Coalition to Stop Trident embarked on protests against nuclear weapons in distinctly local fashion.

The flood of policy proposals unleashed in the 1980s could have quickly become more ineffective "first steps" in ending the nuclear arms race. One engineer endorsing the Freeze wrote that although "the nuclear freeze is crucial, it is only a first step."[3] The One Less Bomb Committee in Concord, Massachusetts, distributed a petition asking the U.S. president "to take the *first step* toward disarmament by ordering the disassembling of 1 nuclear bomb."[4] But for a great many activists, policy demands were not at the heart of the 1980s movement. One of the leaders of the European Nuclear Disarmament campaign asserted that "END is not an arms control proposal but a political objective," one sloganized

[2] Randall Forsberg, "A Bilateral Nuclear-Weapons Freeze," *Scientific American*, November 1982, Vol. 247, No. 5, 52.
[3] Douglas Mattern, "Requiem for a Not So Special Session," *Bulletin of the Atomic Scientists,* November 1982, Vol. 38, No. 9, 59.
[4] Advertisement, "One Less Bomb," *Bulletin of the Atomic Scientists*, April 1981, Vol. 37, No. 4, 7, emphasis in original.

as "a Europe free of nuclear weapons from Poland to Portugal."[5] The movement embraced a wide array of activists, a great many of them bound by solidarity in the belief that nuclear weapons were immoral. The editorial of the first issue of *END: Journal of European Nuclear Disarmament* declared, "The peace movement is here to stay." It continued: "The strength of the movement grows through the spread of ideas . . . . The very diversity of the movement is the source of inspiration and creativity."[6]

In *END Journal*, one female West German activist remarked on this new sensibility. Eva Quistrop wrote that she had participated for years in the women's movement until the mid 1970s, when she began to see connections between the nuclear complex and the patriarchal society around her. "No," she wrote, "we are not aggressive in the male sense but we declare war, not on another nation, or race or sex, but against this destructive technology. This is the form patriarchy takes today, and it is a destructive form of power that affects everything." Environmental protest against pollution caused Quistrop to oppose nuclear power. But at some point, she realized "that what we as women had to do was figure out how to combine these issues—feminist issues, ecological issues, Third World issues and the issue of freedom. In particular we had to focus on nuclear technology because it simply is not compatible with a female future." Eventually Quistrop incorporated nuclear weapons into the targets of her protest and helped start a movement of German Women for Peace. "Together with the women of the world we will turn our powerlessness into power," her movement declared. "We will no longer continue to accept the struggle for power waged by the superpowers."[7] Feminism had quickly combined with antinuclear sentiment to unleash

[5] Mary Kaldor, "END Can be a Beginning," *Bulletin of the Atomic Scientists*, December 1981, Vol. 37, No. 10, 45.
[6] "Editorial," *END: Journal of European Nuclear Disarmament,* December 1982/ January 1983, No. 1, 2.
[7] Eva Quistrop, "Women Reclaim the Future," *END,* December 1982/ January 1983, No. 1, 28–29.

the famous civil disobedience campaign at Greenham Common in 1981, and an article in *END Journal* reported that women in West Germany, Italy, Australia, Canada, and Seneca Falls, New York, adapted the Greenham Common model to local situations. In the fall of 1983, according to one estimate, roughly 5 million West Europeans participated in antinuclear demonstrations.[8]

The Nobel Peace Prize winning Swedish activist Alva Myrdal rejected the term "peace movement" as inadequate to describe "the newly rekindled fire of resistance" to nuclear weapons. She preferred the term "resistance movement," which, she wrote, better described "the acute situation of today . . . . The mightily growing movement against nuclear weapons is supported by hundreds of thousands of people who now personally realise that their survival is threatened." She saw this resistance movement as offering a way of life that refused to be "snared by the allurements of either the one or the other of the superpowers, not by the one in the east nor by the one in the west." Myrdal demanded that both the United States and Soviet Union remove their intermediate range nuclear missiles destined for deployment in the European theater.[9]

Back in the United States, nuclear protest had a distinctly different tone from scientists' efforts in earlier decades. Scholars have noted how in the late 1960s and 1970s, many Americans (and some scientists themselves) had begun to fear a trend toward technocracy, while left-leaning scientists decried the militarism and class, race, and gender inequalities rampant in U.S. science; consequently, many Americans began to return to

[8] Myrna Greenfield, "Peace camp inspirations," *END*, October/ November 1983, No. 6, 6–7. Wittner, *Toward Nuclear Abolition*, 133–39, 168.
[9] Alva Myrdal, "The New Resistance Movement," *ENDpapers Six*, Winter 1983–84, Spokesman 44, 3–4.

religion as a moral arbiter in society.[10] As fears of nuclear war revived in the 1980s, moral messages became crucial to the peace movement, as they resonated with the public in a way that strictly technical endorsements of arms control and disarmament could not. Looking at just a single issue of the U.S. antinuclear magazine *Nuclear Times*, one finds techno-religious claims—"The solution to avoiding nuclear annihilation is to convince everyone on Spaceship Earth that we are all God's children and that it is His desire that we live peacefully together"—mixed among appeals for the movement to reach out to labor unions, endorsements of a Freeze from financial experts concerned about fiscal responsibility, and calls to "unite the peace-and-freeze movement with the black [civil rights] movement."[11] The antinuclear movement brought together a great variety of activism of which religion was just one among many. But the religious aspect of activism bears further analysis here because religion's moral critique of nuclear weapons in general contrasts greatly with many scientists' technical opposition to only some nuclear weapons systems.

Religious Tradition in the Antinuclear Movement

Religion had long been a factor in Britain's largest and most mainstream antinuclear organization, the Campaign for Nuclear Disarmament (CND). Since the late 1950s, CND had offered Britons a moral appeal against nuclear weapons that urged a unilateral renunciation of nuclear weapons. Much of the organization's moral urgency came from the Christian subgroup of CND.

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[10] On the turn toward religion in the 1960s and 1970s United States, see Isserman and Kazin, *America Divided: The Civil War of the 1960s* (New York: Oxford University Press, 2000), and Bruce J. Schulman, *The Seventies: the Great Shift in American Culture, Society, and Politics* (New York: Free Press, 2001). On the counterculture revolt against science in the 1960s and 1970s, see Kelly Moore, *Disrupting Science: Social Movements, American Scientists, and the Politics of the Military, 1945–75* (Princeton: Princeton University Press, 2008), 130–33.

[11] Richard Grossman, "Unions Concerned"; Anthony R. Benedetto, "Genie No Dream"; "Dollars & Sense"; "Growing Bold in Washington"; *Nuclear Times,* January 1983, Vol. 1, No. 3, 3, 5, 8.

Christian CND tactics during a September 1961 "Peace Pilgrimage to Chichester" consisted of silent prayer, while 350 and 400 demonstrators held banners that asked "Is your faith in the Cross or the Bomb?" A statement released by Christian CND before the Chichester march read: "We appeal to all Christians [of] all denominations to join in this great expression of our desire for an end to nuclear weapons and the Cold War, and the beginning of real peace. We hope that people will come from every church in the district, besides 'unattached' Christians and non-Christian supporters."[12] Christian CND hoped that the march would "bring home the serious moral responsibility of the possession of nuclear weapons to ordinary people in a country town and district, including masses of week-end motorists," and that it would "encourage similar pilgrimages to Cathedrals all over the country."[13] The London branch of Christian CND, meanwhile, embarked on a similar pilgrimage to Canterbury. A press release explained: "We go to Canterbury as the centre of the established Church believing that as we are members of one body, we have a responsibility towards each members, no matter how exalted or humble, to help them create the new political morality which is implicit in unilateral nuclear disarmament, and we go humbly believing that those who seek, find."[14]

A 1963 Christian CND resolution passed at the annual CND convention called upon "people of all religions and of none in East and West who believe in the absolute evil of

[12] "Peace Pilgrimage to Chichester," undated, 6 [1962], 1/26: Christian CND, 1963–1965, Section 1: CND Central Constitution, Minutes of Committees, Subject Files, 1958–1971, Campaign for Nuclear Disarmament Papers, London School of Economics and Political Science (hereafter CND Papers).
[13] "Christian CND Pilgrimage to Chichester, September 1st, 1962," 7, by Sybil Cookson, 1/26: Christian CND, 1963–1965, Section 1: CND Central Constitution, Minutes of Committees, Subject Files, 1958–1971, CND Papers.
[14] "London Region Christian CND—Pilgrimage to Canterbury," 13, 1/26: Christian CND, 1963–1965, Section 1: CND Central Constitution, Minutes of Committees, Subject Files, 1958–1971, CND Papers.

nuclear weapons."[15] This moral abhorrence of nuclear weapons influenced CND itself, as a CND policy statement declared "that nuclear weapons are strategically suicidal and totally immoral" and "wholly evil." By unilaterally renouncing nuclear weapons, CND hoped "that Britain, by its example, can give moral and political leadership to the world."[16]

Religious activism had a recognizable, if inconsistent, tradition in U.S. social movements from abolition to the civil rights movement. In similar spirit, many religious officials and ordinary adherents joined the antinuclear movement. An auxiliary bishop of Detroit, Michigan, praised *Nuclear Times*, telling the magazine, "I hope you will continue the fine work you are doing to encourage understanding and involvement in this important issue [of peace]."[17] Not everyone saw the church as a force for peace; a letter from the president of American Atheists asked "where, indeed, have the churches been?" The writer castigated churches for having supported—"in return for innumerable favors"—the U.S. government "in the spiraling cold war and armaments race." Instead of supporting peace, churches stood aside to protect their "positions of prestige, tax exemptions, government largess, and leading places in implementing social policies."[18] But in *Nuclear Times,* at least, coverage emphasized the antinuclear side of many churches and congregations, with articles featuring titles such as "Churches Attack Weapon Business," and "Churches to Kick Off Peace with Justice Week."[19] An article in the June 1983 issue hailed Catholic bishops' endorsement of a Nuclear

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[15] "Minutes of Christian CND National Committee," Nov. 11, 1963, 11, 1/26: Christian CND, 1963–1965, Section 1: CND Central Constitution, Minutes of Committees, Subject Files, 1958–1971, CND Papers, LSE. See also "The Christian Contribution to World Peace," p. 14, 1/26: Christian CND, 1963–1965, Section 1: CND Central Constitution, Minutes of Committees, Subject Files, 1958–1971, CND Papers.
[16] "Draft for Policy Statement Leaflet," p. 84, 5/ 10: Policy (1); Major Campaign Policy: London Region correspondence, especially Michael Craft and George Clark, on CND policy and statements by national and London region CND, 1960–64, Section 5: London Region Minutes, Correspondence, CND Papers.
[17] Bishop Thomas J. Gumbleton, "Peace and War," *Nuclear Times*, April 1983, Vol. 1, No. 6, 2.
[18] Madalyn Murray O'Hair, "Church Key," *Nuclear Times*, April 1983, Vol. 1, No. 6, 2.
[19] Corinna Gardner, "Churches Attack Weapon Business," *Nuclear Times*, April 1983, Vol. 1, No. 6, 9; Walter Lew, "Churches to Kick Off Peace with Justice Week," *Nuclear Times*, May 1983, Vol. 1, No. 7, 12.

Freeze, stating, "They would break with nearly four decades of their nation's nuclear arms policies and declare that the use of nuclear weapons 'could not be morally justified.'"[20] The Catholic bishop of Amarillo, Texas, home of the Pantex nuclear weapons assembly plant, wrote to *Nuclear Times* himself, concerned that activists only accepted the church's antinuclear stand while rejecting the other aspects of its belief system. Nevertheless he assuaged his doubts with the thought that "the urgency of the nuclear threat is such that we must all work together on it with courage and commitment."[21]

Pope John Paul II directly called on scientists to embrace morality in an address titled "Science, Technology, and Peace," delivered at Hiroshima in late February 1981. The Pope described "scholars and research workers" who felt "the anxiety of the scientific world in the face of an irresponsible use of science, which too often does grievous damage to the balance of nature, or brings with it the ruin and oppression of man by man." Far from opposing science, John Paul II believed that "science and technology are a wonderful product of a God-given human creativity," albeit one that "can be used either for man's progress or for his degradation." The fact that nuclear war threatened the world "should finally compel everyone to face a basic moral consideration: from now on, it is only through a conscious choice and through a deliberate policy that humanity can survive. The moral and political choice that faces us is that of putting all the resources of mind, science and culture at the service of peace." Later in the address, he declared that "humanity must make a moral about-face" to confront this generation's "great moral challenge." The Pope urged "all scientists, centers of research and universities to study more deeply the ethical problems of the technological society," and resist "the temptation to subject technological development

[20] Patty Edmonds, "The Greater Work Still Ahead," *Nuclear Times*, June 1983, Vol. I, No. 8, 16.
[21] L.T. Matthiesen, "Meddling Prophets?" *Nuclear Times*, May 1983, Vol. 1, No. 7, 2.

to the pursuit or maintenance of power, as happens when it is used for military purposes."[22] By embracing an antinuclear ethos, the Catholic church—though not known for an especially progressive view of women—found itself allied with feminists as well as many other types of activists against the common nuclear menace.

Clergy of all denominations spoke out, unburdened by scientists' restraints of objectivity. One letter to the *Bulletin of the Atomic Scientists* in 1982 scored world leaders for absolving themselves of responsibility for the nuclear danger. "The most grotesque aspect of the nuclear arms race," Rev. William W. Rankin of All Saints Church in Pasadena, California, declared, "is the sense we have that its momentum is invincible and that no one is in charge." The "rising military expenditures, boisterous foreign policy rhetoric, and the piling up of nuclear weapons" amounts to "provocation," which "in the nuclear age is tantamount to criminal insanity." Rankin closed his letter with a call to action: "Never before has the urgency of personal morality been so starkly apparent. Never before have we felt so desperately the need for courage, imagination and vision. Never before have the stakes been higher."[23]

Across the Atlantic, the Church of England endorsed disarmament in 1983, advocating cancellation of the Trident program, refusal of the Polaris missile, and a halt to the production of cruise missiles. *END* also reported on churches in East Germany that actively participated in the peace movement.[24] One petition, distributed by Pastor Reiner Eppelmann, declared that nuclear weapons "won't save us, but destroy us. We will all be

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[22] Pope John Paul II, "Science and Conscience," *Bulletin of the Atomic Scientists*, April 1981, Vol. 37, No. 4, 7–8.
[23] Rev. William W. Rankin, "Where are our leaders?" *Bulletin of the Atomic Scientists*, June 1982, Vol. 38, No. 6, 70.
[24] Barbara Eggleston, "Controversy over Church Report," *END*, December 1982/ January 1983, No. 1, 2; Wolfgang Muller, "Delicate Compromise between State and Grassroots: the Difficult Task of the East German Churches," *END,* December 1982/ January 1983, No. 1, 12–13. See also *The Church and the Bomb: Nuclear Weapons and Christian Conscience* (London: Hodder and Stoughton, CIO Publishing, 1982).

long dead when the soldiers in their tanks and at the missile bases as well as the generals and politicians in their bunkers, on whose protection we have relied, are still living and continuing to destroy whatever remains." Signers of his petition included craftsmen, white and blue collar workers, deacons, a writer, a scientist, a nurse, and a mechanic.[25] Meanwhile, Christian CND estimated in 1983 that 23 percent of all CND members considered themselves practicing Christians, a percentage that exceeded that of practicing Christians in the United Kingdom as a whole.[26] Christian CND denounced "the failure of H.M. Government to take any practical steps towards nuclear disarmament," and added that "we believe that the possession of nuclear weapons is totally incompatible with being a follower of Jesus Christ."[27] Christian CND capitalized on what they saw as the establishment of "nuclear disarmament at the forefront of the Church's agenda" by launching a "Nuclear-Free Parishes Campaign" that promoted grassroots efforts, such as "urging Christian congregations to pledge support for nuclear disarmament in their churches" and "actively seeking ways of becoming peacemakers and helping to create a nuclear-free future."[28] On October 18, 1983, Christian CND joined physicians for a demonstration in Whitehall. The two groups had chosen the day—St. Luke's Day—for its special significance. "St Luke was both an evangelist and a doctor," Christian CND announced, "and we intend that the government will realise the extent of opposition to its nuclear policies both among the religious and the medical community."[29] In 1984 Christian CND also challenged the Church

[25] "What Will Bring About Peace?" CC Docs 1982, April '82 Meeting, European Nuclear Disarmament Papers, London School of Economics and Political Science (hereafter END Papers).
[26] "Press statement," April 27, 1983, doc. 10, Folder 6/1 Christian CND (33/127), CND Additions (I), CND Papers.
[27] "Christian CND School for Action," doc. 2, Folder 6/1 Christian CND (33/127), CND Additions (I), CND Papers.
[28] Press release: 'Christian CND on "Church and the Bomb" "Nuclear-Free Parishes Campaign" Launched,' doc. 2a, Folder 6/1 Christian CND (33/127), CND Additions (I), CND Papers.
[29] "Christian CND in Pre-Demo Action," Oct. 11, 1983, doc. 38, Folder 6/1 Christian CND (33/127), CND Additions (I), CND Papers.

of England General Synod to follow up on its antinuclear statement from a year earlier. The Church of England, Christian CND alleged, had failed in its self-appointed task "to give a moral lead to the nation." The group wondered:

> how can the Church of England give a moral lead if its central body remains silent while the nuclear governments persist with so-called defence policies which create an illusion of security at home by generating real insecurity abroad, which involve the conditional intention to inflict genocide on millions of innocent people, and consume resources which should be devoted to the needs of the world's poor? . . . We believe that God is now calling the Christian community in this country to respond to a crucial need and a unique opportunity to influence public policy by proclaiming the judgement [sic] and peace of the Gospel to a government and people overshadowed by the darkness of death. We must not evade the challenge of the Holy Spirit."[30]

A New Voice of Scientific Expertise

Antinuclear activists still envisioned a role for scientists, but one based on a fairly generic image of scientists rather than close connections to them. In her 1983 manifesto, Myrdal described the antinuclear resistance movement as entering a second phase. The first phase had been "the massive emotional protest against nuclear weapons." The second phase, now begun, opposed the state's attempts to stifle the movement by "meeting with realistic counter-arguments all subterfuges, all false arguments and alibis." She declared that "Scientists stand in the first row" of this second phase, calling on them to counter the "superpower and allied propaganda" that opposed the antinuclear movement. "Scientists do not lie," she wrote; "Their evidence stands invincible." Myrdal was evidently unaware of the vast and rancorous disputes between scientists over nuclear weapons, including Teller's rivalries with Pauling and Sagan. Myrdal had faith that scientists and other technical experts

[30] "Open Letter to the General Synod of the Church of England," Nov. 15, 1984, doc. 73, Folder 6/1 Christian CND (33/127), CND Additions (I), CND Papers.

could influence public opinion since "they form the bastion of knowledge and willpower which will endure long past 1983." But scientists were just one influential group among many. Myrdal emphasized the essential roles of other professionals, including "engineers, architects, authors, psychologists, nurses, clergymen," and physicians. "Most important," she wrote, "must be the workers in their great numbers."[31]

Some scientists, physicians, and engineers joined this vibrant mixture of religious, feminist, internationalist, and moral sentiment, creating a new voice of nuclear protest. In particular, the Australian female pediatrician and antinuclear activist Helen Caldicott of the Physicians for Social Responsibility (PSR) stands out. As just one example of her rhetoric, the following section briefly examines a debate over the Nuclear Freeze featuring Caldicott, actor and conservative activist Charlton Heston, retired Navy admiral Gene La Rocque, and pronuclear physicist Edward Teller that took place on Valentine's Day 1983 at Clemson University in South Carolina. Caldicott's opening statement discussed the effects of a nuclear war in numerical terms, befitting her identity as a doctor: "750 million of the 1.3 billion people who live in urban areas in this hemisphere would be killed, and 350 million would be seriously injured, to die later. That is a very conservative estimate." She detailed the effects and casualties at ground zero of a 20-megaton H-bomb explosion: "It would dig a crater three-quarters of a mile wide and 800 feet deep—converting the people, the buildings, and the earth below to radioactive fallout, which would be injected up into the mushroom cloud in the stratosphere to circulate from west to east around the earth, coming down as fallout with low pressure systems." Everyone within a radius of six miles would be killed—or "vaporized," she said, to be more precise. She extended her analysis out to 20 miles,

[31] Alva Myrdal, "The New Resistance Movement," *ENDpapers Six*, Winter 1983–84, Spokesman 44, 4–5.

describing how "every person would be killed or lethally injured." At 26 miles, "the heat is still so intense that your clothes could instantaneously ignite, and you would become a walking, flaming torch." At 35–40 miles, the cataclysm could start "a firestorm . . . of 1,500 to 3,000 square miles." True to the scientific nature of her field, Caldicott explained that her predictions were only an educated hypothesis. But she then mixed scientific and religious language: "The only way to find out scientifically is to do the experiment, and I would submit, as a physician, that that is medically contraindicated, that this would create the final epidemic that the human race would ever know. In fact, we are talking about destroying God's creation."[32]

In the question and answer session, Caldicott further revealed the moral, religious, and feminist underpinnings of the 1980s antinuclear movement. Though she was trained in the scientific and medical disciplines, she emphasized that she was not just a doctor, but a doctor who cared for children. "I hear what Mr. Heston and Dr. Teller are saying, but I would like to submit, as a woman, a pediatrician, and a mother, that this is prenuclear thinking," she stated. "The thing is," she continued, "I am a woman. You can tell I am a woman. And I do not think it is the right way to settle a conflict—conflict resolution by being strong on both sides. . . . That does not produce conflict resolution in a marriage. If you come on with strengths from both sides, the marriage ends in divorce. Russia and America are married on this planet. If we do not live together, we die together."[33] Here

[32] "The Nuclear Arms Freeze," Proceedings of a Roundtable Discussion, 5–7, Feb. 14, 1983, Folder 29.6: Nuclear Arms Freeze, Box 29, Edward Teller Papers, Hoover Institution (hereafter ET Papers). Caldicott recognized that contemporary nuclear strategy recommended using "many small bombs on one city [rather] than one big bomb," but because she believed that "the Russians have big crude weapons," she focused on the effects of one 20 megaton H-bomb. She also said that the effects of such a bomb were "simpler and easier to describe." She might also have added more startling. Much of Caldicott's data came from Jonathan Schell's *Fate of the Earth*.
[33] "The Nuclear Arms Freeze," 13.

Caldicott also incorporated the concept of a shared coexistence from the environmental ethos.

A question from the audience directly addressed religion and biblical prophecy: "The destruction of the world is foretold in the Book of Revelation. As a Christian, I already know about that destruction. It appears to be inevitable. Do you agree?" After Heston disputed the idea that God wanted to destroy the world and affirmed an American's duty to protect the country, Caldicott responded in language that embodied the internationalist and moral nature of the antinuclear movement. She stated, "about 92 percent of the world's people do not live in Russia or America. I come from Australia, so I am one of them. And these people want to live. . . . As a pediatrician and a mother, I am talking about preserving God's creation for the rest of time. So to talk about patriotism now is anachronistic."[34]

Caldicott's closing statement again combined the wide array of arguments against nuclear weapons. "If you look at it clearly," she said, "the world is run by men. It is a fact. And many of them are old. This is not a sexist statement. It is a reality statement." Most of these men had no conception of what a nuclear explosion would do. "Understand emotionally the magnitude of the forces we are talking about," she said. "How many leaders of the world have witnessed the miracle of the birth of a baby? How many leaders of the world have helped a child to die and supported the parents in their grief?" She closed by outlining a plan for the world's "acute clinical emergency where we are about to die," emphasizing a bilateral nuclear weapons Freeze.[35]

Caldicott was able to speak in a way the typical arms control scientist could not. Many of the earlier antinuclear scientists had come from physics, a field dominated by men

[34] "The Nuclear Arms Freeze," 15–16.
[35] "The Nuclear Arms Freeze," 18.

and, because of its need for massive equipment, frequently dependent on defense establishment funding. Like Caldicott, Carl Sagan embraced the moral ethos of the 1980s and crafted political arguments based on his scientific authority. Such a chorus of scientific authority, environmental alarm, and quasi-millennial prophecy helped challenge the arms race and the strategy of deterrence on a level beyond arms control.

Meanwhile, advocates of nuclear weapons in the 1980s scorned the peace movement. Aghast over the widespread concern about nuclear winter and support for the peace movement in general, a civil defense consultant and geologist wrote to the staunch nuclear advocate Edward Teller: "Perhaps you have not seen an announcement of Carl Sagan's planned 'Response to the Defense Department's Findings' regarding 'nuclear winter.' I will miss my guess if you are not one of the men whom Carl Sagan will attack as enemies of peace and the hope of mankind's survival. . . . Will an emotional grassroots offensive undermine the hope of the Free World's developing and deploying effective, truly defensive weapons?"[36]

"Mutually Assured Survival": The Moral Message of Nuclear Advocates

Nuclear advocates of the 1980s, many of them influential hawks in the Reagan administration like Teller, respected the salience of moral and religious arguments, most clearly because they used such rhetoric themselves. Historian Paul Boyer has surveyed the apocalyptic talk within the Reagan cabinet. In 1983, according to Boyer, Reagan himself told an Israeli lobbyist "You know, I turn back to your ancient prophets in the Old Testament and the signs foretelling Armageddon, and I find myself wondering if we're the generation

[36] Cresson H. Kearny to ET, Feb. 11, 1985, Folder 1: Cresson H. Kearny, Box 3, ET Papers.

that's going to see that come about. I don't know if you've noted any of those prophecies lately, but believe me, they certainly describe the times we're going through." One year earlier, Secretary of Defense Caspar Weinberger had said, "I have read the Book of Revelation and yes, I believe the world is going to end—by an act of God, I hope—but every day I think that time is running out." James Watt, Reagan's Secretary of the Interior, had chimed in, "I do not know how many future generations we can count on before the Lord returns." Such apocalyptic talk often indicated hostility to disarmament, as when evangelist Jerry Falwell in 1983 called the Freeze campaign a "suicidal effort." According to Boyer, many Americans began to think that belief in millennial prophecy indicated that these leaders would not attempt to prevent World War III, seeing it as the Almighty's plan.[37]

But when Reagan introduced the Strategic Defense Initiative (SDI), the tone changed from impending apocalypse to technological salvation. Accordingly, Reagan administration officials often tried to smear their antinuclear opponents and opponents of SDI as immoral advocates of mutually assured destruction. During a 1985 Committee on Armed Services hearing on nuclear winter, Pentagon aide Richard Perle argued that Carl Sagan's proposal to reduce nuclear arsenals to a number low enough to avoid nuclear winter but still capable of deterring the Soviets was simply an endorsement of mutually assured destruction.[38] The Pentagon official in charge of the Strategic Defense Initiative Organization (SDIO), Lt. Gen. James Abrahamson, repeated these accusations in a 1986 debate with Sagan. When Sagan questioned the reliability of SDI, Abrahamson changed the subject, countering that the only alternative to SDI was a policy of mutually assured destruction. "What guarantee," he asked

[37] Boyer, *Fallout*, 150–55.
[38] U.S. Senate, Committee on Armed Services, *Nuclear Winter and Its Implications*, 99th Cong., 1st sess. Hearings, October 2 and 3, 1985 (Washington, D.C.: U.S. Government Printing Office, 1986), 147.

Sagan, "do you offer that the population and the establishment of our society really can be protected by the strategy or the condition of mutual assurance, mutually assured destruction? After all, we are dealing with deterrence." Later in the debate, Abrahamson asked Sagan, "Why do you think some critics of SDI find deterrence based on the threat of offensive retaliation a superior form of deterrence over types of deterrence which rely primarily on strategic defense?"[39] In his memoirs, Teller framed opponents of SDI, including scientists such as Hans Bethe and Victor Weisskopf, as believing "the policy of mutually assured destruction . . . a better, more stable policy."[40]

Edward Teller had spent the 1970s out of the limelight to an extent. He popped up periodically, such as during debates over the energy crisis, when he urged that for the sake of future U.S. national security, "we should immediately cut gasoline consumption massively."[41] During the Three Mile Island nuclear power plant meltdown, Teller frantically defended his beloved nuclear power to various news media as well as in front of Congress. After Congressional testimony about the meltdown, Teller suffered a minor heart attack, leading him to declare in a pro-nuclear power newspaper advertisement, "I was the only victim of Three Mile Island."[42] As opinion turned against nuclear power, U.S. government support for Teller's nuclear excavation initiative also faded. But Project Plowshare, defunct by 1974 and officially cancelled in 1977, lived on in Teller's enthusiasm.[43] The same year as Plowshare's U.S. cancellation, Teller took his marketing efforts overseas, and toward Australia in

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[39] "Transcript of Debate between Dr. Carl Sagan and Lt. Gen. James Abrahamson, July 31, 1986," 3, 19, Folder 20.46: Sagan–Abrahamson Debate, Box 20, Hans Bethe Papers, Cornell University (hereafter HB Papers).
[40] Edward Teller, *Memoirs: A Twentieth-Century Journey in Science and Politics* (Cambridge: Perseus Publishing, 2001), 532.
[41] Teller, "Conflict in the Middle East: Time for an American Energy Contingency Plan," reprinted in *Congressional Record—House*, Feb. 18, 1981, 97th Cong., 1st sess., 2365–66.
[42] Peter Goodchild, *Edward Teller: The Real Dr. Strangelove* (Cambridge: Harvard University Press, 2004), 326–27.
[43] Scott Kirsch, *Proving Grounds: Project Plowshare and the Unrealized Dream of Nuclear Earthmoving* (New Brunswick: Rutgers University Press, 2005), 4, 203.

particular. He saw uranium deposits east of Darwin as a "treasure trove," and told 62 State Parliamentarians that "Every day [you] delay the export of yellow cake may mean the death or starvation of great multitudes. It may mean danger for world stability."[44] In 1981 Teller promoted Australia as the ideal site for Plowshare, where "the use of nuclear explosives to loosen up material deep underground . . . could make available minerals at much greater depth."[45]

But as a conservative revival swept U.S. politics in the early 1980s, Teller was once again poised to influence U.S. nuclear policy. Although in 1978 Teller had declared that his preferred candidate for the 1980 presidency was Dixy Lee Ray, the conservative former chair of the AEC and Democratic governor of Washington, he quickly became a fan of the new President after Ronald Reagan's first hundred days. He wrote a friend that "I am on the whole very much satisfied with what is going on at present."[46] While Teller was asked by the Reagan Presidential Transition Office to give his opinion on cabinet appointments, none of his recommendations were selected.[47]

Teller remained active in electoral politics; according to one biographer, he even considered a run for a California Senate seat until his heart attack put a stop to such thoughts.[48] He lent his name to a campaign fundraising appeal for Jack Hickey, who was running against incumbent congressman Tom Lantos, like Teller a naturalized Jewish-Hungarian immigrant. The letter framed Teller as a concerned scientist and immigrant from

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[44] "Presenting the Real Life Dr. Strangelove," *Gamut*, May 24, 1977, Folder 4, Box 1, ET Papers.
[45] ET to Gina Hayward, April 17, 1981, Folder 4, Box 1, ET Papers.
[46] ET to H. Peter Metzger, December 29, 1978, Box 3, Folder 7: H. Peter Metzger, ET Papers; ET to Gina Hayward, April 17, 1981, Box 1, Folder 4, ET Papers.
[47] Teller had unsuccessfully offered John Foster for Sec. of Defense, Dixy Lee Ray for Sec. of Energy, Hans Mark for NASA Administrator, Leo Cherne for CIA Director, and Frederick Seitz for Science Advisor. Teller did suggest Alexander Haig for Secretary of State, who received the position, but probably not because of Teller's recommendation. ET to Richard Allen, undated, Folder: Reagan, Ronald, Box 282: Correspondence: Personal, ET Papers.
[48] Goodchild, *Edward Teller*, 326.

Hungary, and began by describing Lantos as a "truly appalling" congressman. Though Lantos claimed to see the dangers of Soviet "domination" and "dishonesty," Teller charged, his advocacy of a Freeze revealed his duplicitous nature. The "main obligation" of immigrants was "to use the knowledge inherent in our origins for the benefit of those who have welcomed us into their land of freedom." Thus Teller endorsed Hickey.[49] In 1984 Teller served as the National Chairman of Hungarian American Voters for Reagan-Bush; his increased role in political activism reflected a shift in voting patterns after 1976, when, during an unsuccessful but influential primary challenge to Gerald Ford, Reagan rallied voters of Eastern European ethnicity into a voting bloc opposed to Ford's pursuit of détente. Reagan had charged that the Helsinki Final Act, signed under Ford's tenure as president, condoned Soviet violations of human rights in Warsaw Pact nations.[50]

Teller explained his resilient Cold War fervor in a 1980 interview that dripped with sarcasm and rancor toward. "I am not an advocate of the arms race," he said,

> I am an advocate of a technological race which is something very different because you produce new, different, surprising things which is the real source of power. The technological race is the real possibility to deter the noble aims of the Soviet Union which consists in their sharing their noble way of life with the rest of the world by extending their domination throughout the world. I am considering the present period as one in which the Industrial Revolution will of necessity spread throughout the world. For instance, the exploitation of the seas for food, introduction of mariculture, weather and climate control, the prevention of the oncoming Ice Age which is about due to happen, all this has to involve the world as a whole. Therefore, some world order is unavoidable. The Soviets are willing to offer some good old and well-tried lines namely; imperialism and domination. We have only one alternative: and it is a noble experiment, agreement, spontaneous cooperation, democracy with all its difficulties and contradictions. This is not an easy job under any conditions, but where you have a determined competitor like the noble Soviets who are nobly

[49] Hickey letter, by ET, Sept. 17, 1984, Folder 2.18: Jack Hickey Correspondence, Box 2, ET Papers. Lantos trounced Hickey in the election, 147,607–59,625, *Statistics of the Presidential and Congressional Elections of November 6, 1984* (Washington, D.C.: U.S. Government Printing Office, 1985), 5.

[50] George R. Salem to ET, June 18, 1984, Folder 2: Politics and Government, Presidential Elections, 1984, Box 34, ET Papers. See Sarah B. Snyder, "The Helsinki Process, American Foreign Policy, and the End of the Cold War," Ph.D. diss., Georgetown University, 2006, especially Chapter Five, 125–52.

> prepared to turn the human race into an ant heap, in that case some preparation is necessary in order to prevent war. There must be some power in the hands of those who do not want war.[51]

By 1981 Teller had largely set aside his other interests in order to focus on nuclear weapons defense systems—specifically ballistic missile defense. Well before Reagan announced SDI, Teller declared defensive weapons as the most important breakthrough physicists could achieve in the 1980s. He characterized missile defense as comparable to the urgency and nobility of scientists' efforts during World War II, writing, "Those of us who remember the mid-1930s know that Britain was saved by the development of radar and even more by . . . code breaking." He continued, "Today, the problem is similar yet different. In both cases, the danger came from an ambitious tyranny. But Hitler was an adventurer while the men of the Kremlin are cool, practical and conservative. This time, success in developing defense could prevent war." He reminded his readers that during World War II, "If scientists had not played their remarkable role in the past, National Socialists could well be ruling the world today."[52] In testimony before the House Committee on Armed Services, Teller continued to hammer home his warnings of appeasement when he deemed Nuclear Freeze proposals "as effective as Chamberlain's umbrella was in producing the Second World War."[53]

Along with advocating missile defense, Teller attacked those who threatened the weapons he advocated. In a 1982 article in the *Journal of Civil Defense* he focused his ire on the

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[51] World Research, Inc., "The Energy Crisis: No Contingency Plan. An Exclusive Interview with Dr. Edward Teller," 1980, Folder 1: "The Energy Crisis," Box 7, ET Papers.
[52] Teller, "Role of Physicists in the 1980's," reprinted in *Congressional Record*, Extensions of Remarks, 97th Cong., 1st sess., March 2, 1981, 3341–42.
[53] U.S. House of Representatives, Committee on Armed Services, *Defense Department Authorization and Oversight, Hearings on HR 2287, Department of Defense Authorization of Appropriations for Fiscal Year 1984 and Oversight of Previously Authorized Programs*, 98th Cong., 1st sess., Part 5 (Washington, D.C: U.S. Government Printing Office, 1984), 1367.

"new tidal wave of disarmament propaganda." In particular, Teller was alarmed by opposition to nuclear weapons among religious organizations. "When church people support peace, they do not differ from any other Americans," he wrote. "When church people raise rigid opposition to the preparation of weaponry that decreases the danger of war itself, their action appears more dubious." Worried by the moral and religious challenge to nuclear weapons, he continued: "Independent of what beliefs we hold, we should pray that revolutionary students, politicians and archbishops do not make common cause in supporting an oversimplified solution which will lead to disaster."[54]

Teller reserved some vitriol for activists of all sorts, including Caldicott's PSR. "Who are these physicians who call themselves 'Physicians for Social Responsibility'?" he rhetorically asked in an interview. "Does that mean all other physicians not belonging to this small group are not responsible?" He then clarified: "'The so-called 'Physicians for Social Responsibility' . . . are, in fact, highly irresponsible. . . . If the doves prevail, if an arms freeze is forced upon America, then a nuclear war is a 'practical certainty.'" Shifting to a defense of his allies, Teller stated that "President Reagan is the first U.S. leader who has had the guts to tell his people this sad and terrible fact. That we are second best. We may be 10 years behind, we may be as much as 15 years behind, or we may be only six years behind. But we are decisively behind." Teller also continued to try and smear his opponents with the same Red Scare tropes he had used since the late 1940s:

> Physicians who try to scare people about nuclear war are right in trying to prevent war, but they are wrong in trying to disarm the U.S. as the means of preventing war. In the 1930s, do not forget, it was the Depression that brought Hitler to power in Germany. The anti-war groups in Britain, France, and the U.S. thought that they would prevent a repeat of World War I, but instead they helped bring Hitler to

[54] Teller, "Deep Freeze for Nuclear Arms," reprinted in *Congressional Record,* Extensions of Remarks, 97th Cong., 2nd sess., June 7, 1982, 12859.

> power and helped encourage the Germans to launch an even greater slaughter in World War II . . . . I do not want history to repeat itself.[55]

When not on the attack, Teller's moral language helped put a positive spin on nuclear weapons, especially SDI, the plan to arm satellites with nuclear-powered lasers that would blast enemy missiles. In the summer of 1982, Teller wrote to inform Reagan about this "important new class of defensive nuclear weapons systems." The weapons would be "spectacularly destructive," Teller wrote; "however, it is enemy warheads, rockets and satellites, not lives, which will be destroyed." Calling it "the most important [matter] in strategic military affairs since the advent of the hydrogen bomb," Teller requested an additional $55 million in research funds for Livermore Nuclear Laboratory. Emphasizing their morality, Teller wrote that with these weapons, "we may end the Mutual Assured Destruction era and commence a period of assured survival on terms favorable to the Western Alliance." As an added bonus, satellite weapons would eliminate a pesky nuisance: "this effort may also constitute a uniquely effective reply to those advocating the dangerous inferiority implied by a 'nuclear freeze.'" On September 14, Teller visited Reagan to tell him about how nuclear-powered lasers could defend the United States from Soviet missiles, an idea the President described as "exciting" in his diary.[56]

While not religious himself, Teller saw the value of supporting his position on nuclear weapons in moral terms. In a letter to the conservative icon William F. Buckley, Teller again used the language of morality to justify nuclear weapons, writing, "what is really immoral is not nuclear weapons, but rather aggression. Defense is justified." The "proper

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[55] "Father of H-bomb Warns of U.S. Weakness," reprinted in *Congressional Record—House*, 97th Cong., 2nd sess., Aug. 5, 1982, 19663–64.
[56] ET to Ronald Reagan, July 23, 1982, Folder: Reagan, Ronald, Box 282 Correspondence—Personal, ET Papers; Ronald Reagan, *The Reagan Diaries*, Douglas Brinkley, ed. (New York: HarperCollins, 2007), 100.

and morally correct purpose" of nuclear weapons was as "defensive instruments," he explained. "Our difficulty is that this idea is contrary to general assumptions, and it is extremely hard to make people believe it. A nuclear freeze would preclude ongoing development of purely defensive nuclear devices."[57]

On March 23, 1983, with Teller, Bethe, and other influential scientists watching, Reagan announced the Strategic Defense Initiative to the public. "What if free people could live secure in the knowledge that their security did not rest upon the threat of instant U.S. retaliation to deter a Soviet attack, that we could intercept and destroy ballistic missiles before they reached our own soil or that of our allies?" the President asked. "I call upon the scientific community of our country, those who gave us nuclear weapons, to turn their great talents now to the cause of mankind and world peace, to give us the means of rendering these nuclear weapons impotent and obsolete." Teller later wrote, "Those wonderful words moved me deeply." But how could Reagan's statement not have pleased Teller? After all, he had practically provided the President with the speech. In July, Teller and other SDI advocates again visited Reagan, urging that SDI should receive the same priority that the Manhattan Project had during World War II. After the meeting, Reagan wrote in his diary, "I have to agree with them it's the way to go."[58]

In contrast, other elite U.S. scientists advocated a ban on space weapons. Even before Reagan's SDI announcement, a February 1983 scientists' petition, drafted by Sagan and Richard Garwin, had declared: "We believe that the testing or deployment of any weapons in space—in part by threatening vital satellite assets—significantly increases the likelihood of warfare on Earth. . . . Once such weapons systems are established in national

[57] ET to William F. Buckley, May 19, 1982, Folder: Buckley, William F., Box 274: Correspondence: Personal, ET Papers.
[58] Teller, *Memoirs*, 531–32; Reagan, *Diaries*, 165.

arsenals they become very difficult to displace. . . . If space weapons are ever to be banned, this may be close to the last moment in which it can be done." Signatories included such U.S. scientific luminaries as Bethe, Sidney Drell, Lee DuBridge, Frank Long, Carson Mark, Philip Morrison, Wolfgang Panofsky, I.I. Rabi, Glenn Seaborg, Victor Weisskopf, Jerome Wiesner, and Herbert York.[59]

To Congress Teller described defensive nuclear weapons, including SDI and the x-ray laser, as "a third generation of nuclear weapons," adding, "What counts is no longer the big size of nuclear explosions. What counts is no longer their enormous destructive power." Because the x-ray laser would use a nuclear explosion to power a laser, the explosives no longer needed to be devastatingly large, he explained: "These explosions can be relatively small. Thus the third generation weapons can be used, not for mass destruction, but to destroy very specific targets such as offensive weapons in action." Turning then to his opponents, Teller stated, "Some people, I am sorry to say many scientists, whose minds are stuck in the old concept of no defense, have contradicted him [Reagan]. They have been aided and abetted by our bureaucratic rules of secrecy which allow only imperfect proposals to be put forward, which then can be shot full of holes."[60] During House debate over SDI, a representative from Arizona advocated for the program based on Teller's judgment, inaccurately claiming that Teller had also been the sole advocate of the Hydrogen bomb in

[59] AAAS, *Science 83*, June, Vol. 4, No. 5, 17; also in "Ban Space Weapons," *Bulletin of the Atomic Scientists*, November 1983, Vol. 39, No. 9, 2–3.
[60] U.S. House of Representatives, Committee on Armed Services, *Defense Department Authorization and Oversight*, 98th Cong., 1st sess., Part 5: Research, Development, Test, and Evaluation, Hearings, March 1, 2, 9, 15, 17, 23, 24; April 12, 18, 19, 20, 22, 25, 26, 27, 28, 1983 (Washington, D.C.: U.S. Government Printing Office, 1984), 1355–56.

the late 1940s and early 1950s. "Again in the scientific world he becomes a lone voice," Republican Eldon Rudd declared. "Thank God for Dr. Teller."[61]

Ensconced at the Hoover Institution on the campus of Stanford University, Teller defended SDI and a new conception of deterrence. Particularly galling was the growing popularity of the derisive nickname "Star Wars." "This is supposed to sound, and in actual fact is, impractical," he wrote to a government defense researcher. "To put up battlestations of any form is expensive. To shoot them down is cheap. Furthermore, the press has prepared the public to oppose these ideas and, therefore, any resemblance between the actual plan and battlestations in space will not only be technically impractical but also politically impossible to execute."[62] He bemoaned the "wide-spread anti-Reagan attitude of the press" for confusing the public over defensive weapons (though he also faulted excessive U.S. secrecy restrictions). "Star Wars" was not what Reagan had proposed, Teller argued—instead, he had proposed a system "to replace the truly horrible idea of mutual assured destruction." Answering claims that the system was not 100 percent effective, Teller urged the public not to discount an 80 percent effective defense system and claimed that "Two hostile camps primarily armed with shields is less dangerous than if armed with swords." Teller preferred deterrence based on defense over deterrence based on "devastating retaliation," and he argued that defensive deterrence was actually more effective since it relied on uncertainty by creating doubt in the minds of the Soviets.[63] "Our purpose is to deter war by making the success of aggression less likely, thereby we can contribute to

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[61] "Strategic Defense Initiative is Truly a Reagan Initiative," *Congressional Record—House*, October 23, 1985, 99th Cong., 1st sess., 28681.
[62] ET to James C. Fletcher, July 28, 1983, Folder 5, July–Sept. 1983, Box 433: Chronological Correspondence, 1981–1991, ET Papers.
[63] ET to Peter Renz, July 30, 1984, Folder 3.11: Peter Renz, correspondence, Box 3, ET Papers. Even a mere 20% of Soviet missiles would have been more than enough to annihilate the United States.

deterring aggression," Teller told Congress. "One should not expect that aggression will become impotent and obsolete overnight, but as we demonstrate step-by-step that defense can be made less expensive and more effective than aggression . . . the threat of aggression, the most dreadful concomitant of technological development, can indeed be lifted from mankind."[64]

Far from being a lone voice in the scientific wilderness, Teller had the backing of many other scientists on SDI. The Science and Engineering Committee for a Secure World formed in May 1986 in order to "strongly endorse the Strategic Defense Initiative." "We are confident," they declared, "that there are thousands of scientists and engineers across America and elsewhere who agree with us that it is unscientific and unwise to hastily oppose the promising Strategic Defense Initiative proposal at this early stage of its research and development, and who believe that the concept of developing a defensive system to protect our people from a nuclear attack makes good common and good moral sense." They claimed that scientists and engineers "have the opportunity to play a positive and critically important part in reducing and perhaps eventually eliminating the threat of nuclear war" by working on SDI. "Indeed, we ask our fellow scientists and engineers, is it not our responsibility as professionals and as concerned human beings to utilize our talents and energies now to see if we can render the nuclear threat militarily ineffective and therefore obsolete?" The group argued that SDI would turn U.S. policy "away from the unreliable, outdated MAD doctrine and its death dealing nuclear missiles, to a Mutually Assured Survival policy based on new, life protecting defensive systems," a policy they described as "ethically superior." The group announced its intention to "Demonstrate to the public that

[64] "Testimony of Dr. Teller on SDI," reprinted in *Congressional Record—Senate*, 99th Cong., 2nd sess., May 15, 1986, 10766–68.

there is significant support for SDI from informed, respected and articulate scientists and engineers." The group was chaired by former NAS president Frederick Seitz; other members included Teller, Eugene Wigner, Fred Singer (who had frequently challenged Sagan on nuclear winter), Hans Mark, Alvin Weinberg, John Wheeler, Harold Agnew, and Dixy Lee Ray.[65]

Teller made no secret of his political allegiance. After Reagan survived an assassination attempt in 1985, Teller showered him with flattery and sycophancy worthy of Lyndon B. Johnson: "There are few occasions when I dare to use the name of God," he wrote. "Now I thank him for your recovery from your operation." He continued, "I have expressed my gratitude to you for your proposal and continuing support of the Strategic Defense Initiative." He advised Reagan not to trade away SDI for some mere short-term arms control agreements. Rather, "To establish SDI as the protector of the free world, with the help of our allies, would make your role more remarkable than that of Lincoln. He saved the Union. You have set out to save freedom." He concluded, "My hope for your future acts is even greater than my admiration of what you have accomplished. Many of us are doing our best to turn your dream into reality."[66]

Technically Supporting the Freeze: Antinuclear Scientists in the 1980s

While Teller could get away with making moral arguments because he was pronuclear and anticommunist, many scientists continued to oppose nuclear weapons by favoring technical arguments and remaining ambivalent to grassroots efforts. Because moral arguments against

[65] "Statement on SDI by the Science and Engineering Committee for a Secure World," reprinted in "Testimony of Dr. Martin J. Hoffert on SDI," *Congressional Record—Senate*, 99th Cong., 2nd sess., May 15, 1986, 10768–70.
[66] ET to Reagan, Aug. 6, 1985, Folder: Reagan, President Ronald, Box 282: Correspondence: Personal, ET Papers.

nuclear weapons most effectively mobilized the grassroots, the old guard of antinuclear scientists found themselves followers rather than leaders of the movement, since—unlike scientific and technical issues—they had no unique authority over moral concerns. In contrast to earlier antinuclear activists, the movement in the 1980s valued moral authority over technical expertise, a shift due in part to the state's ability to define the Cold War in the 1980s as a moral—rather than technological—struggle with communism. This definition put Cold War policy beyond the purview of scientists and served to make them less influential as a social force. Some of these scientists felt a sense of paternalism toward the antinuclear movement, since they had given birth to nuclear dissent decades earlier. "The community of physical scientists has been struggling with the problem of avoiding nuclear war since the end of World War II," wrote Bernard Feld, the new editor of the *Bulletin of the Atomic Scientists.*[67] This was of course true, but scientists were being overtaken as many other activists representing a wide array of causes catalyzed the antinuclear movement.

Scientists shared no single reaction to the grassroots movement; a survey of the *Bulletin* reveals a variety of positions. One writer dismissed grassroots efforts such as the enormous antinuclear rally in Central Park on June 12, 1982, where roughly 750,000 people demonstrated for disarmament. John Isaacs, a member of the Council for a Livable World, discussed the political prospects for Freeze legislation in the 1982 November elections. In one of the *Bulletin's* rare mentions of the Central Park rally, Isaacs gave the rally a nice pat on the head. "Many backers of the nuclear freeze felt that . . . demonstrations such as the huge June 12 New York rally were important in the campaign to stop the nuclear arms race, but," he added, "a definite political focus is also necessary to have an impact on decision-makers

[67] Bernard T. Feld, "Campaign for a Livable World," *Bulletin of the Atomic Scientists,* June/ July, 1981, Vol. 37, No. 6, 1.

in Congress."[68] Scientists had been mentioned only as an afterthought in a *New York Times* description of the rally's participants, near the bottom of a list including pacifists, anarchists, Buddhists monks, and jugglers.[69]

In contrast, some scientists could be supportive of grassroots activism. The *Bulletin of the Atomic Scientists* began a "Bulletins" section devoted to "public interest organizations and activities," such as the first Ground Zero week of protest in 1982.[70] Douglas Mattern, an electronics engineer and disarmament activist, hailed the antinuclear movement in the *Bulletin* and castigated the Reagan administration for running the arms race at full speed. "The failure [of disarmament measures] cannot be blamed on the lack of public support," Mattern wrote, praising the June 12 demonstrations. Instead, "the U.S. government poses the main problem in the disarmament dilemma." Mattern called on "the American people" to "build enough pressure to force the Administration to abandon its escalation program and accept the bilateral nuclear freeze."[71]

When it came to Freeze legislation, the U.S. Congress did not turn to scientists for guidance. Instead, government officials, foreign relations specialists, and arms control experts were more likely to be consulted. A 1983 hearing by the House Foreign Affairs Committee, for example, brought Perle, Sovietologist Raymond Garthoff, and Randall Forsberg to the hill for their views on Freeze proposals.[72] On the other hand, scientists were

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[68] John Isaacs, "The Freeze," *Bulletin of the Atomic Scientists*, October 1982, Vol. 38, No. 8, 9–10.
[69] Paul L. Montgomery, "Throngs Fill Manhattan to Protest Nuclear Weapons," *New York Times,* 13 June 1982, 1.
[70] "Ground Zero Week 1982," *Bulletin of the Atomic Scientists*, April 1982, Vol. 38, No. 4, 57.
[71] Douglas Mattern, "Requiem for a Not So Special Session," *Bulletin of the Atomic Scientists,* November 1982, Vol. 38, No. 9, 58–59.
[72] U.S. House of Representatives, Committee on Foreign Affairs, *Calling for a Mutual and Verifiable Freeze on and Reductions in Nuclear Weapons.* 98th Cong., 1st sess., Hearings, February 17, March 2 and 8, 1983 (Washington, D.C.: U.S. Government Printing Office, 1983).

heavily consulted during hearings on nuclear winter, and less frequently for their views on SDI.[73]

As for scientists'—specifically physicists'—personal attitudes toward the arms race in the 1980s, at least one survey was conducted. In 1986 Peter D. Hart Research Associates surveyed 549 physicists selected randomly from the American Physical Society's directory. "By a margin of 54% to 29%," the confidential report stated, "physicists view the Strategic Defense Initiative as a step in the wrong direction for America's national security policy. By way of contrast," the report continued, "majorities or pluralities voice support for a variety of other elements of a defense system." These elements included Trident submarines (63 percent right direction, 11 percent wrong direction), cruise missiles (45 percent right direction, 26 percent wrong direction), and the Stealth bomber (39 percent right direction, 24 percent wrong direction).[74] Many scientists clearly opposed SDI, but unlike grassroots activists in the United States and Europe, they accepted some nuclear weapons as acceptable. Just as Oppenheimer had argued in 1954 that he had supported the H-bomb once a "technically sweet" design had been conceived, many scientists in the 1980s felt comfortable opposing only weapons that they could contest on technical grounds.

Scholar Rebecca Slayton has catalogued the activism of computer professionals against SDI, finding that experts such as the Computer Professionals for Social Responsibility challenged SDI in terms of the inherent complexity and difficulty of the

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[73] See for example, U.S. Congress, International Trade, Finance, and Security Economics Subcommittee of the Joint Economic Committee, *The Consequences of Nuclear War*, 98th Cong., 2nd sess. Hearings, July 11 and 12, 1984 (Washington, D.C.: U.S. Government Printing Office, 1986); U.S. House of Representatives, Natural Resources, Agriculture Research and Environment Subcommittee of the Committee on Science and Technology, *The Climatic, Biological, and Strategic Effects of Nuclear War*, 98th Cong., 2nd sess. Hearing, September 12, 1984 (Washington, D.C.: U.S. Government Printing Office, 1985); U.S. Senate, Committee on Armed Services, *Nuclear Winter and Its Implications*, 99th Cong., 1st sess. Hearings, October 2 and 3, 1985 (Washington, D.C.: U.S. Government Printing Office, 1986).

[74] Peter D. Hart Research Associates, "A Survey of Physicists' Attitudes Toward the Strategic Defense Initiative," March 1986, Folder 20.51: Reaction to Star Wars Announcement, Box 20, HB Papers.

computing requirements such a weapons system would require. One critic wrote, "Even if we could somehow magically build computers which were fast enough and reliable enough, even if we could devise systems which [were] completely reliable, even if we could write software which would be completely error free—the bottom line is that no one knows what to tell the computers to do."[75] At least one British computer scientist demonstrated that such skepticism was not limited to U.S. scientists. Richard Ennals resigned his government research position "as a protest against the British government's decision to participate in Star Wars research." Ennals, along with several of his fellow Imperial College computer scientists, explained their resignation in a letter to Margaret Thatcher that detailed their objections to SDI. They deemed it "not possible" to build a reliable SDI, and argued that the attempt to do so would "divert scarce human and technological resources away from civil applications." Furthermore, they feared that SDI research would spark a nihilistic "Star Wars Race" between East and West.[76]

While not prone to such public demonstrations, physicist Hans Bethe had continued to do what he could for arms control and disarmament since the early 1960s. When the Kennedy administration signed a test ban agreement with the Soviet Union in 1963, Bethe reconsidered his earlier doubts and voiced his belief that the Senate should ratify the treaty. During the ensuing decades, Bethe had continued to pursue arms control measures, always in the technical language of the scientist. The 1980s found Bethe still active in arms control efforts and still viewing disarmament through the prism of U.S. national security.

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[75] Quoted in Rebecca Slayton, "Speaking as Scientists: Computer Professionals in the Star Wars Debate," *History and Technology*, 2003, Vol. 19 (4), 357.
[76] Richard Ennals, "Why I Quit Over SDI," *END*, February/ March 1986, No. 20, 10–11.

On September 22, 1982, the *New York Times* published an op-ed piece co-authored by Bethe that refuted Reagan administration reasons for rejecting a Freeze. An early draft of the editorial in Bethe's papers argued that "the U.S. should want a freeze," and predicted that such a measure would "reduce the risks of war and the costs of military programs."[77] Bethe also spoke at the October 13, 1981, meeting of the Tompkins County Nuclear Weapons Freeze Campaign, where he advocated a Freeze.[78] At an arms race colloquium at Sandia Laboratories in July 1982, Bethe was asked to comment on the Freeze proposal. "As you have seen from my talk, that is not the position I advocate," he began.

> I consider the freeze movement a very important political movement. It has to be simple to be understood by the public. The public understands the freeze; the public would have difficulty understanding the slides I showed you. The freeze is important, therefore, as a means of alerting the public and, thereby, giving a signal to our government that we do not want to continue the arms race. I would be very much against stopping the production of warheads for the cruise missile, because I consider it the second-most important part of our arsenal. Therefore, I don't endorse the freeze as an actual measure, but I do endorse it as an easily understood public movement. I think it has done its job in this respect.[79]

Bethe approved of new nuclear weapons as long as they replaced larger and more destabilizing weapons of previous generations, but such nuance did not easily jibe with the goals of grassroots social movements.

Bethe had an ambivalent view of social movements. In 1949 he had mistakenly dismissed the NAACP as "a Communist front organization," and in 1963 he remained skeptical of antinuclear activism. Referring to disarmament and public opinion, Bethe had

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[77] Bethe and Frank A. Long, "The Value of a Freeze," *New York Times*, 22 September 1982, A27; Hans Bethe and F.A. Long, "The Case for a Nuclear Freeze between the U.S. and the U.S.S.R.: A Response to Richard Perle," undated, Folder 17.16: Arms Control, Box 17, HB Papers.

[78] Ruth Yarrow to HB, Oct. 18, 1981; Bethe to Henry Kendall, Oct. 12, 1981, Folder 33.26: Nov. 11, 1981, National Teach-in on Nuclear War, Box 33, HB Papers. Tompkins was the county of Ithaca, New York, home to Bethe's Cornell University.

[79] "The Arms Race: A Sandia Colloquium by Hans A. Bethe, July 28, 1982," 15–16, Folder 17.12: Arms Control Sandia Colloq. '82, Box 17, HB Papers.

written to one correspondent that "going directly to the people" was unwise. "The people," he explained, "are unfortunately swayed by the political climate of the moment. For instance, the German hating campaign of the first World War, the McCarthy Era and much worse, operations in dictatorship countries like Nazi Germany and Russia. In the heat of international conflict I would not trust the people to remain loyal to the disarmament agency in preference to their own country."[80]

Bethe attempted to walk a fine line between supporting the Freeze as a "public movement" on the one hand, and opposing the Freeze "as an actual measure" on the other. When a November 11, 1981, national antinuclear teach-in organized by the Union of Concerned Scientists brought together scientists, academics, and students in grassroots actions modeled after teach-ins during the Vietnam War, Bethe took part. About a month before the teach-in, Bethe informed a colleague that he would participate in the event and included a list of talking points for himself and other scientists who might take part. The list clearly demonstrated the technical nature of Bethe's arms control advocacy, as he defended arms control and disarmament as not harming the military security of the United States. Bethe began his list with the statement that "The premise of the Reagan government that we are inferior to the Soviet Union in nuclear weapons is false. What matters for our security from surprise attacks are submarines and bombers. The penetration of the latter will be assured by cruise missiles, which will become operational in a few months. In these two categories we are superior to the USSR." The next item asserted that "Since cruise missiles assure the ability to penetrate into the Soviet Union the B1 is unnecessary." The accuracy of

[80] HB to E.T. Dunham, June 26, 1962, Folder 5.41: Correspondence related to disarmament, Box 5, HB Papers. Bethe's skepticism was hardly unique. In 1968 Eugene Rabinowitch summed up his thoughts on the global student upheaval in his article "Student Rebellion: The Aimless Revolution?" A quick perusal of the article reveals the title's question mark to be superfluous. *Bulletin of the Atomic Scientists,* September 1968, Vol. XXIV, No. 7, 7–10.

Trident missiles, he added, ensured that "MX is not needed from this point of view."[81] Just as he had in the late 1950s, Bethe chose to argue that arms control would not harm U.S. nuclear superiority, rather than vocally embrace the moral appeal of disarmament.

Bethe also endorsed the Freeze in a public appeal signed by just under 100 scientists from four continents on both sides of the Iron Curtain in 1982. The petition aimed at "our colleagues of the world's scientific community," asking them to "accept responsibility and become directly involved in actions to avert nuclear war." The accompanying text endorsed a Freeze: "To reverse the present arms race we must first stop racing. This calls for a 'standstill freeze' on current nuclear arsenals as an effective way of initiating the essential process of nuclear disarmament."[82]

Eventually, Bethe's private and public thoughts on a Freeze caused confusion. He was accordingly forced to clarify his ambivalent stand. In 1983, he wrote to the news editor of the *Ithaca Journal*:

> In the Journal of March 28, your staff member, John Huenneke, published an interview with me which in general was very good. However, one sentence, only slightly related to the main topic, has been misinterpreted by several of my friends: "I don't think the freeze, taken literally, is the right thing to do." The emphasis is on "taken literally". I am very much in favor of the freeze as a popular movement, because it is a signal to our government that the people of the U.S. want to stop the arms race. But I don't believe it would be helpful just to stop exactly where we are.
>
> In particular, I believe we should deploy the cruise missile on our bombers. This is essential to keep the "second leg" of our strategic nuclear force, namely, the bombers, effective. Once we have the cruise missile, we do not need the B-1 with its fancy electronics designed to penetrate into the Soviet Union; the bomber need only

[81] HB to Henry Kendall, Oct. 12, 1981, Folder 33.26: Nov. 11, 1981, National Teach-in on Nuclear War, Box 33, HB Papers. It is unclear where Bethe spoke. In this letter he indicated he might speak at the University of New Mexico, the Los Alamos lab, or "somewhere else."

[82] "We Now Appeal," *Bulletin of the Atomic Scientists*, November 1982, Vol. 38, No. 9, 3–4. Another endorsement of the appeal came from Linus Pauling, who may have been more than a little peeved to see the name of George Wald below his on the petition's list of signatories. In 1957, Wald had refused to sign Pauling's test ban petition, declaring that "I decided once and for all to sign no more petitions." Wald to LP, December 6, 1957 [1.74], LP Safe Contents, Drawer 2, Folder 2.002, Ava Helen and Linus Pauling Papers, Oregon State University.

> approach the Soviet Union, and the B-52 is sufficient for this purpose. I am however against deploying cruise missiles either on ships or on the ground. . . .
>
> Our real security lies in the submarine launched ballistic missiles which would survive any possible enemy attack."[83]

Elsewhere, Bethe considered a pet project of Teller's, the x-ray laser. "Insofar as the x-ray laser is concerned, it is too early to tell whether this device can ever be developed into an effective defensive weapon," he wrote to a Pentagon official. "However, until the U.S. actively supports a comprehensive test ban, the x-ray laser should not be disqualified simply because it exploits a nuclear explosion as a power source. In the absence of a CTB," he stated, "I therefore support a research program that would explore the x-ray laser."[84] Though an avid arms controller, Bethe did not express his personal opposition to nuclear weapons in moral terms. Instead, he coolly calculated each new weapon and analyzed it in regard to other weapons and the political context, accepting or rejecting it after logical, unemotional thought. This is in contrast to scientists like Pauling or Sagan, who mixed moral and scientific language. While this approach suited Bethe's personal temperament, it ensured that he and other scientists would be all but irrelevant to the antinuclear movement of the 1980s.

Bethe expressed his own ideology in a January 20, 1983, talk at the Center for the Study of Democratic Institutions on the subject of "Scientists' Responsibility." He spoke specifically on military research, asking "Should you participate?" Bethe mentioned radar as a clear cut case where participation had no down side, while participation in the A-bomb and H-bomb projects had more nuance. Nevertheless, Bethe stated, "Non-participation is not an effective way to stop a military development." Instead, "decisions about these programs

[83] HB to the Editor, *Ithaca Journal*, April 4, 1983, Folder: Bulletin, Long, Box 1 (unprocessed collection), HB Papers.
[84] HB to Gerold Yonas, April 12, 1985, Folder: Government Officials, Box 276: Correspondence: Personal, ET Papers.

must be made by the elected representatives of the people." In a rationalization similar to Teller's, Bethe believed "It is the scientist's duty to inform public and government of the implications of his developments. In the course of time others . . . will and should take over." Yet scientific advice should continue to inform government policy: "The existence . . . of a weapon should be discussed openly, w/o classification. Then also scientists not in military development should get informed & participate." A note to himself tellingly added, "Important that they [scientists] get informed & not argue on emotional grounds." The section concluded, "It is important that scientists dedicated to restraining armaments be in weapons development, just as dovish senators should be on [the] Armed Services Comm[ittee]."[85]

Like Teller, Bethe attended the White House briefing just prior to Reagan's announcement of SDI in March 1983. Less than a week later, Bethe wrote to the President expressing his concerns. Bethe believed "the chances of success of an effective defense to be incredibly small." Even more troubling to Bethe was that such a system would create instability "far worse than anything we have seen." He was certain that the Soviets would see SDI as aggressive and would subsequently launch attacks on U.S. satellites. He concluded that "without far-reaching conciliatory diplomatic moves toward the Soviet Union, the program of developing these defensive weapons would make this country far less secure than we are today."[86]

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[85] Handwritten notes for talk at Center for Study of Democratic Institutions, Jan. 20, 1983, Folder: Responsibility of Scientists/ Hutchins Center, Box 11 (SL), HB Papers, emphasis in original.
[86] HB to Reagan, March 29, 1983, Folder 20.32: SDI: Keyworth, Maj. Worden, Box 20, HB Papers. Slayton has argued that physicists' opposition to SDI, framed as rooted in natural law, was more effective than other technical experts' dissent, such as computer professionals, whose objections to SDI were based more on anecdote and uncertainty. See "Speaking as Scientists," 338, 347, 353.

In November 1983, Bethe appeared at Harvard's JFK School of Government to debate Teller on SDI. Teller began by framing SDI as a humane alternative to the "repulsive" policy of mutually assured destruction. Because the officials in the Kremlin were cautious, Teller explained, they would overestimate the efficacy of U.S. defenses. Thus a system that was only 50 percent or 80 percent effective in actuality would, because of the Kremlin's mindset, be essentially 95 percent effective. At the very least, "we have raised the level of uncertainty and gained time for cooperation." Later he added, "I believe it is a moral and practical necessity to develop defense."[87]

When it was his turn, Bethe stated, "I do not believe that President Reagan's 'Star Wars' proposal, as it is generally known, can work." The Soviets would consider SDI an aggressive move and would build more weapons to compensate, Bethe argued, while the United States would respond in kind, further escalating the arms race. Furthermore, it would be nearly impossible to attack missiles effectively because although missiles are vulnerable in their launch phase, the time window of that phase lasts only briefly. Once the missiles made it to the atmosphere, there would be, because of MIRVs, roughly 100,000 objects to target, far too many for the satellites to destroy. It would also be prohibitively expensive to launch 300 satellites weighing 100 tons each, and finally, Bethe concluded, if the superpowers pursued SDI, no one would care about arms control or disarmament.[88]

Teller responded by saying that countering Bethe's arguments would require him to reveal classified information. Instead, he simply argued that arms control agreements would not work. Bethe nevertheless expressed confidence that nuclear weapons treaties could be

[87] "Transcript of Teller-Bethe Debate," November 1983, Folder 20.45: Teller-Bethe Debate, Box 20, HB Papers; published in *Technology Review* 87, April 1984, 38(3).
[88] "Transcript of Teller-Bethe Debate," HB Papers.

policed, and that SDI would be prohibitively expensive, costing "hundreds of billions of dollars." Teller grumbled that "I would be very happy to be exceedingly quiet if only Hans Bethe would come and help, and thereby, even beyond contributing his own talents, provide an example to young people that we must search for a ballistic-missile defense."[89]

Bethe continued to do his best to prevent the futile and expensive SDI research efforts. He wrote to a scientist with the Pentagon's Strategic Defense Initiative Organization (SDIO) in 1985: "As you know, I am skeptical about SDI. In my opinion, it will be extremely difficult to find and develop defensive weapons which combine the two criteria stated . . . (a) to be cost-effective at the margin and (b) to be invulnerable to attack on themselves."[90] In a speech later that summer, he touched on the arms race that began after Hiroshima:

> U.S. policy has tried to rely on superior technology. Whenever there was a chance to make nuclear weapons more devastating, we took it. We introduced the H-bomb and the transcontinental bomber, we escalated the number of nuclear weapons and later that of nuclear missiles, and (worst of all) we introduced MIRV. In every case, the Soviets followed suit, 3 to 5 years later, and we were less secure than before. Our newest technological escalation is SDI, Star Wars. I am firmly convinced that it will end up the same way, by making us still less secure. I have looked at the proposed technologies: Some, I think, will never work, others may work after decades of effort if the Soviets offer no opposition. But the individual technologies are not the real problem; it will be far more difficult to join them together in a system that would have to work in the unimaginable confusion of nuclear war, without ever having been fully tested.[91]

One month later, Teller complained to Bethe of his anti-SDI activities, telling him "your published stance has made our national effort in a vital field much more difficult. This single fact has a greater influence on me than everything else put together." Here Teller

[89] "Transcript of Teller-Bethe Debate," HB Papers.
[90] HB to Gerold Yonas, April 12, 1985, Folder: Government Officials, Box 276: Correspondence: Personal, ET Papers.
[91] Bethe, "16 July 1985," Folder 17.57: Commemoration of Trinity, 7/16/85, Box 17, HB Papers.

echoed his belief that in the 1950s J. Robert Oppenheimer had hampered H-bomb research. Such disagreements wounded Teller in a personal way (or so he claimed): "all this is very sad and I suspect that your feelings may be similar. At the same time, I must pay more attention to my responsibilities as I see them rather than to my feelings. Indeed, the hope and effort for a useful defense in the strict and narrow sense of the word is the one remaining motivation for which I continue to work."[92]

In his notes for a response to Teller, Bethe wrote that they each differed fundamentally over how to achieve U.S. security. Teller wanted "a technology race," while Bethe believed "we must talk, negotiate." He wrote that Teller "spends full time on persuading others, is presently in agree[ment] with Nat[ional] gov[ernmen]t. I spend maybe 1/3 of my time on it, still try to do physics." He defended his anti-SDI beliefs, and also spoke about their personal relationship: "I remember fondly the years of our friendship, back in 30's & early 40's. I am sorry that politics has separated us so far. But can't we be personally friendly?"[93]

Frustrated by their private correspondence, Teller took his complaints public. In an "Open Letter to Hans Bethe," Teller repeated his claim that Bethe was bringing down the entire SDI effort practically single-handedly. "The prominent vocal opponents of SDI in the scientific community include a dozen names or less," Teller began. "Among these, you are the most important due to your solid, well-deserved reputation and to your long-standing and continuing knowledge of and interest in the related technical issues." Since Bethe was "at the core of an increasingly vocal movement of scientists against SDI," Teller had written this open letter "in the hope that you and others may find some way to move from polemic

[92] ET to HB, May 23, 1985, Folder 33.54: 1985–86, Livermore—Teller, Box 33, HB Papers.
[93] Bethe, "Letter to Edward," undated draft, Folder 33.54: 1985–86, Livermore—Teller, Box 33, HB Papers.

debate and confrontation toward technical criticism, understanding, and cooperation."[94] In his notes about Teller's open letter, Bethe wrote simply that "SDI is not science," and that "Our 'polemic debate' counters false claims of SDIO."[95] He also told Teller, "Some of this history in the Open Letter I agree with, but on much of it I disagree. And of course I don't agree at all on your assessment of SDI, technically as well as politically."[96]

Teller even mentioned Bethe in testimony before Congress. He explained that many scientists' opposition to SDI "arises from their being uninformed." Teller recounted Bethe's attendance at a Livermore briefing of SDI work where he told Teller, "You have a splendid idea." "But did he change his public position?" Teller asked. "No. Instead of objecting on scientific and technical grounds, which he thoroughly understands, he now objects on the grounds of politics, on grounds of feasibility of military deployment, on other grounds of difficult issues which are quite outside the range of his professional cognizance or mine." Teller complained that "For every Bethe, there are a hundred others who speak up and who don't even know the basics of what they are talking about."[97]

For many U.S. scientists who objected to SDI, the idea of nuclear-powered satellite lasers bristled not just as a weapon but as a tool of the Pentagon used to dominate U.S. science and control its funding. In 1986, a Yale mathematician wrote to Bethe: "The whole SDI enterprise is serving the unavowed purpose (conscious in some, unconscious in others) of funding science, technology, and engineering via the Defense Department, and starving the NSF, among other things. My clear opinion is that I object to this sleight of hand and

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[94] "National Defense and the Scientists: An Open Letter to Hans Bethe from Edward Teller," undated, 1, 17, Folder 33.54: 1985–86, Livermore—Teller, Box 33, HB Papers.
[95] "Teller Open Letter," handwritten notes, undated, Folder 33.54: 1985–86, Livermore—Teller, Box 33, HB Papers.
[96] HB to ET, Jan. 24, 1987, Folder 33.54: 1985–86 Livermore—Teller, Box 33, HB Papers.
[97] *Defense Department Authorization and Oversight*, 1360.

deviousness."[98] To the antinuclear segment of the world public, however, SDI transcended science. The program was just one more nuclear weapon for an already bloated arsenal that consisted of MX, cruise, and Trident missiles. The resistance movement against nuclear weapons had moved beyond the debate over different weapons to call for a halt to nuclear weapons production, and removal of missiles from Europe. In a sense, the antinuclear movement thus challenged scientists to transcend science as well.

"The powers of ethics, morality and conviction": Scientists Adapt to Moral Arguments

No longer the leaders of antinuclear protest, and growing more distant from the public, some of the scientists who had been active in arms control and disarmament debates throughout the Cold War felt compelled to shift to moral and religious critiques of nuclear weapons during the 1980s. While doing so, they tried to balance this moral shift with continued reliance on their credibility as objective, scientific experts. Nevertheless, scientists found themselves in new territory as they allied with religious leaders and grassroots movements.

One such alliance occurred in 1980, when Pope John Paul II convened the Pontifical Academy, an honorary group of papal science advisors, most of whom were not Catholic, in order "to discuss the consequences of a nuclear war and to offer suggestions concerning possible statements and actions by the Pope." These scientists took quickly to their charge. At the final meeting, in October, 1981, their time "was devoted specifically to the health consequences of the use of nuclear weapons and to the absence of any meaningful medical

[98] Serge Lang to HB, Oct. 15, 1986, Folder 20.18: Letters and Articles Against SDI, Box 20, HB Papers.

response." The final statement of the Academy deplored nuclear weapons as immoral, and made a special appeal to scientists:

> Science can offer the world no real defense against the consequences of nuclear war. There is no prospect of making defenses sufficiently effective to protect cities since even a single penetrating nuclear weapon can cause massive destruction. There is no prospect that the mass of the population could be protected against a major nuclear attack or that devastation of the cultural, economic and industrial base of society could be prevented. The breakdown of social organization, and the magnitude of casualties, will be so large that no medical system can be expected to cope with more than a minute fraction of the victims. . . .
>
> It is the duty of scientists to help prevent the perversion of their achievements and to stress that the future of humankind depends upon the acceptance by all nations of moral principles transcending all other considerations. Recognizing the natural rights of the human race to survive and to live in dignity, science must be used to assist humankind toward a life of fulfillment and peace. . . .
>
> To avoid wars and achieve a meaningful peace, not only the powers of intelligence are needed, but also the powers of ethics, morality and conviction.
>
> The catastrophe of nuclear war can and must be prevented. Leaders and governments have a grave responsibility to fulfill in this regard. But it is mankind as a whole which must act for its survival. This is the greatest moral issue that humanity has ever faced, and there is no time to be lost.

In its closing words, the document implored scientists "to use their creativity for the betterment of human life and to apply their ingenuity in exploring means of avoiding nuclear war and developing practical methods of arms control." In all, 58 scientists from five continents participated and signed the declaration.[99]

John Paul II then asked the leaders of the nuclear powers as well as the United Nations to receive his scientific delegates. On December 14, 1981, Reagan met with the U.S. delegation, consisting of Manhattan Project veteran Victor Weisskopf; David Baltimore of

[99] "Declaration on Prevention of Nuclear War," *Bulletin of the Atomic Scientists,* December 1982, Vol. 38, No. 10, 4–5. See also "Declaration on Prevention of Nuclear War," Sept. 24, 1982, Folder: Weisskopf, Vatican Declaration, Box 1 (unprocessed collection), HB Papers.

MIT; Marshal Nirenberg of the National Institutes of Health; and Howard Hiatt, dean of Harvard's School of Public Health. According to Weisskopf's report, Hiatt began the presentation in terms designed to startle the president. He described

> a one-megaton bomb detonated over the White House: total destruction within a circle three miles in diameter, 600,000 people killed at once, and 800,000 severely injured. Of 6,000 physicians in the Washington area, 1,500 would be killed and 2,000 seriously hurt, leaving 2,500 surviving doctors to treat 800,000 severely wounded people. . . . Apologizing for personalizing his remarks, Dr. Hiatt noted that following the assassination attempt last March, the President's life was saved by the modern medical technology and personnel of the George Washington University Hospital. But all physicians would agree that had a dozen patients in the President's condition been simultaneously brought to that emergency room, the capacity of the institution would have been overwhelmed. He noted further that in the event of a nuclear attack, George Washington University Hospital itself would have been destroyed. Where would victims be taken? Georgetown? D.C. General? Howard University Hospital? All are within the ring of greatest destruction.[100]

In an attached postscript to his report, Weisskopf recounted how he had received a mild but distinct impression of what it felt like to be a political activist when the Reagan administration had subtly resisted the scientists' visit. "We were received by the President standing up, and he never asked us to sit down," he noted. For fifteen minutes the delegation stood as the presentation proceeded. At one moment in the meeting, Weisskopf made reference to the Pope's letter of introduction, in Weisskopf's words "a moving document about nuclear war." Reagan, however, "said he did not remember ever having received such a letter. I told him that a copy was enclosed in the copy of the statement that I handed over to him. This showed how little attention and how little briefing the President

[100] "Report on the Mission of the Pontifical Academy to President Reagan and the UN," Victor Weisskopf, undated, 1–5, Folder: Weisskopf, Vatican Declaration, Box 1 (unprocessed collection), HB Papers.

devoted to this reception." At the end of the meeting, Reagan made sure to reference the Biblical passage from Revelations on Armageddon.[101]

Despite Weisskopf's frustrations, other scientists also saw religion as a good way to spread their message. In 1984 Carl Sagan put aside his atheism to join with the Pope and other religious leaders to bring attention to the threat of nuclear winter. In the years after the Cold War, Sagan encouraged an alliance with religious leaders to mobilize the public against global warming.[102]

While not adopting a religious focus, one of the more active antinuclear organizations to feature a moral argument was Physicians for Social Responsibility, described by *Bulletin of the Atomic Scientists* editor Bernard Feld as "the most hopeful sign that has appeared on the international horizon in more than a decade."[103] An explanation of the organization's fundamental values appeared in the March 1981 *Bulletin*, written by the head of the San Francisco Bay Area PSR, Peter Joseph. He explained the vision of the group as an optimistic and life-affirming quality rooted in science, but more human than technical. "Physicians are constantly confronted with bad news," he wrote,

> with difficult matters of individual life and death. The subject of nuclear war confronts each of us with intense feelings about the meaning of our lives, and of our deaths. We know the present situation is grave, the prognosis guarded; but we also know that in medicine there is always hope, for the annals of medicine are full of tales of critically ill patients who have outlived their pessimistic physicians. Hope is a prerequisite for medical practice. It is an absolute necessity for functioning in the nuclear age.

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[101] "Addendum to the Report on the Visit to President Reagan," Victor Weisskopf, undated, Folder: Weisskopf, Vatican Declaration, Box 1 (unprocessed collection), HB Papers.
[102] Keay Davidson, *Carl Sagan: A Life* (New York: John Wiley and Sons, 1999), 57, 80, 349, 377–78. See also "Religion and Science: An Alliance," in *Billions and Billions: Thoughts on Life and Death at the Brink of the Millennium* (NYC: Random House, 1997), 137–46.
[103] Feld, "Campaign for a Livable World," *Bulletin of the Atomic Scientists*, June/ July 1981, Vol. 37, No. 6, 1.

Joseph also touched on environmentalist themes when he wrote: "as far as we know, we are all alone in this corner of the universe, hurtling through space on a small, fragile, isolated gem of a planet. As physicians and scientists, we feel a deep responsibility to protect from harm the biosphere which nourishes all life. The health of *all* species, including ours, depends on its integrity."[104]

Finally, some veteran scientist-activists endorsed grassroots actions. Former Manhattan Project chemist and presidential science advisor George Kistiakowsky had once questioned the value of scientists' disarmament efforts. But over time he was converted to the cause, eventually cutting ties with the Pentagon in 1968 over the Vietnam War. In 1981 he hailed PSR and the Council for a Livable World because "The message of the effects of nuclear war must be carried throughout the world." In slight exaggeration (more appropriate to the activist than the scientist), he wrote that "public protest" had resulted in the 1963 test ban treaty, declared that "the youth movement forced the termination of the Vietnam War," and added that grassroots opposition had halted U.S. antiballistic missile deployment. "When a large number of people understand the monstrous consequences of nuclear wars," he concluded, "they will force governments to move toward arms control and disarmament negotiations and the peaceful settlement of disputes."[105] Once the science advisor to President Eisenhower, Kistiakowsky had become a cheerleader for the grassroots antinuclear movement.

This new role for scientists was exquisitely captured by physicist Barry M. Casper in an endorsement of grassroots activism in the *Bulletin of the Atomic Scientists*. Casper wrote that his journey from student to activist had begun during his graduate school years at Cornell

[104] Peter G. Joseph, "Doctors Speak Up," *Bulletin of the Atomic Scientists*, March 1981, Vol. 37, No. 3, 17.
[105] Kistiakowsky, "Carrying the Message," *Bulletin of the Atomic Scientists*, March 1981, Vol. 37, No. 3, 10.

University. His mentors Bethe, Philip Morrison, and Jay Orear—the "vanguard" of antinuclear physicists—were pivotal. "Their commitment [to arms control] was infectious. Concerned from the beginning about the threat of nuclear war, in a matter of months, I came to share their commitment," he wrote. A summer arms control study group "was as essential a part of my initiation into the physics fraternity as was learning quantum field theory. Ever since then nuclear war has been my issue too," he wrote. But by the 1970s, Casper was "saddened to see how many of my friends in the academic arms control community had adapted themselves to the Washington version of reality. Much of what had begun as a countervailing force in the 1960s had become 'professionalized' in the 1970s." Casper yearned for "a social movement committed to stopping the arms race," a "redefinition of political reality," in the form of a second "vanguard of that movement for a new nuclear arms reality." Based on his experiences in the civil rights movement and the anti-Vietnam War movement, Casper made "a plea to physicists. . . . Given the historic role of the physics fraternity in creating nuclear weapons, we physicists have both a responsibility and an opportunity to strengthen this movement which is seeking a way out of the predicament those weapons have brought about." He challenged physicists:

> Do whatever you can: research, education, talking to groups in your community, creating materials for public information. . . . We need experts to lend authoritative support to the notion that the arms race has gone too far and must be stopped. And we need experts to carry the message to ordinary citizens that you don't have to be an expert to know that this is true. Don't let artificial barriers of professional "respectability" stand between you and meaningful action. . . . We can choose instead a role of moral leadership, joining with the physicians and church leaders and other courageous figures in sounding the alarm and helping to stop the world's mad rush toward nuclear suicide.[106]

[106] Barry M. Casper, "An Appeal to Physicists," *Bulletin of the Atomic Scientists*, Oct. 1984, Vol. 40, No. 8, 9–13.

Scientists' role, then, was to spread the message that one need not be a scientist to engage in antinuclear protest, essentially to proclaim their own irrelevance.

During the 1980s, the majority of elite, antinuclear U.S. scientists remained wedded to the official avenues of disarmament—diplomacy, expertise, and advising. After almost a decade, antinuclear activists witnessed the ratification of the Intermediate Nuclear Force treaty, an arms control measure that—at last—actually reduced weapons.[107] In the *Bulletin's* first issue of 1988, the editors hailed the INF treaty, setting the clock back three minutes to six minutes to midnight. And with the requisite, unyielding optimism shared only by arms controllers and fans of hapless baseball teams, the editors followed this announcement with the obligatory praise: "The INF Treaty, combined with improvements in U.S.-Soviet relations and greater international concern about common security matters, are significant first steps in a new direction."[108]

Eventually the Cold War ended—not with a bang, but an uprising of protest and reform in the economically stagnating Soviet Union. The U.S. national security state, dedicated to both piling up enough nuclear weapons to destroy the Soviet Union several times over and beating it into economic submission with a costly arms race, had contained scientists' challenge for long enough.

[107] Ronald Powaski refers to the INF as "historic" in that is was the first reduction in nuclear arsenals and elimination of a specific class of nuclear weapons. *Return to Armageddon: The United States and the Nuclear Arms Race, 1981–1999* (New York: Oxford University Press, 2000), 69.
[108] "Six Minutes to Midnight," *Bulletin of the Atomic Scientists*, January/ February 1988, Vol. 44, No. 1, 3.

Conclusion

The Future of Nuclear Weapons

In 1989, Edward Teller turned 81. At a birthday celebration for the elder physicist, his friends commemorated the occasion, encouraging him to "Go for a hundred!" and "Don't Stop Now!" Others exclaimed in the guestbook, "Looking forward to magic Nos. 82—126," and "You have always been an inspiration and an example. 1000 years."[1]

Entering his ninth decade had not dimmed Teller's enthusiasm for nuclear weapons. During the late 1980s, his pet project had been Brilliant Pebbles, a more nimble version of the Strategic Defense Initiative consisting of small satellites weighing just over 100 pounds. According to Teller's plans, thousands of the devices would patrol the atmosphere until they sensed an incoming missile. At that point, those satellites within range of the missile would, according to Teller, "scatter destructive pebbles in the path of the attack missile." Brilliant Pebbles served "to keep the strategic defense program alive over the long haul," in the words of Teller's ally Hans Mark, former NASA missile scientist and chancellor of the University of Texas system in the late 1980s. Mark had written to Teller to express his concern that strategic defense programs like Brilliant Pebbles could be neither tested nor deployed "without violating the ABM Treaty." Not that Mark was bothered by the prospect of violating the treaty; rather, he was upset that the George H.W. Bush administration lacked the will to revise or violate the treaty, "or even seem to violate it." He told Teller: "They will

[1] Guestbook, Folder 2, Box 1, Edward Teller Papers, Hoover Institution (hereafter ET Papers).

support 'Brilliant Pebbles' with lots of money to keep us quiet but they will not deploy a system based on the concept."[2]

Along with strategic defense, Mark also reassured Teller that the alignment between the national security state and academic institutions remained tight. "We have done well here in Texas in bringing defense related research work to the University," Mark boasted. He listed $20 million a year for "electro-magnetic guns for the Army," $30 million a year for Navy "anti-submarine devices," and $800 million a year to research "Over the Horizon radars" for the Air Force.[3]

After the end of the Cold War, Teller pondered the future of his own Lawrence Livermore Laboratory, which he had served in many capacities since its founding in the 1950s. Determined to keep the lab thriving, he continued to emphasize defensive weapons systems. He declared it "essential" that Livermore "maintain its work on radically innovative developments in defense" in a 1994 letter to his fellow former Livermore director John Foster. "It is a mistake to assume that elimination of weapons and elimination of new ideas equals stability," he asserted. In Teller's mind, this unstable, post-Cold War world required "new types of weapons and ideas. In this general area, Los Alamos, Livermore and Sandia [labs] have played an important role."[4]

As Teller looked to the future, his old rival Hans Bethe reflected on his own career in arms control and science. In a speech accepting the Einstein Peace Prize in 1992, Bethe asked rhetorically how the Cold War ended. "Did we bring this about by Arms Control?" he

[2] Hans Mark to ET, June 5, 1989, Folder: Mark, Hans, Correspondence, 1980s, Box 279: Correspondence, Personal, ET Papers.
[3] Teller, *Memoirs: A Twentieth-Century Journey in Science and Politics* (Cambridge: Perseus Publishing, 2001), 535–36; Hans Mark to ET, June 5, 1989, Folder: Mark, Hans, Correspondence, 1980s, Box 279: Correspondence, Personal, ET Papers.
[4] ET to John Foster, July 1, 1994, Folder: Foster, John S. Jr., Box 275: Correspondence: Personal, ET Papers.

asked. "No!" he answered. The main factors had been the "peaceful revolution" of the Soviet people and the reforms of Mikhail Gorbachev. But, he added, "We, the Arms Control community, also contributed." Bethe specifically mentioned "Planting the idea [that] there was an alternative to Arms Race" and "Getting to know each other" at Pugwash conferences and "the actual negotiations of treaties." Turning to the future, Bethe set an agenda for arms control. "[T]here is no longer any good reason to design still more sophisticated nuclear weapons," he stated, as though responding directly to Teller's vision of the post Cold War world. But Bethe did not endorse getting rid of the nuclear deterrent, explaining, "of course, we shall still need to keep some nuclear weapons, and a competent group to maintain and supervise them."[5]

Bethe's desire to retain some nuclear weapons was based on the possible proliferation of nuclear weapons to places like Iraq and the former states of the Soviet Union. Bethe in fact could not envision a future without nuclear weapons. "For the more distant future," he said, "we cannot tell how low the ultimate arsenal of nuclear weapons should be. Perhaps it only needs to be large enough to deter any new country from developing or buying nuclear weapons, and then hide them. This may mean an arsenal of 100 weapons or even less." Bethe concluded that "The nuclear world looks much better today . . . but a lot remains for us to do."[6]

Science Contained

[5] Bethe, "Chicago 3–12–92" [notes for talk], Folder 35.23: 1992 Einstein Peace Prize—Chicago Talk, Box 35, Hans Bethe Papers, Cornell University.
[6] Bethe, "Chicago 3–12–92" HB Papers.

The tasks that remained for arms controller scientists were in large part a direct result of the challenges they had used against nuclear weapons since 1954 as well as the government's response to these challenges. In these formative years, scientists' professional identities, social commitments, and ideologies collided with U.S. national security interests. During World War II, the race to develop nuclear weapons, as well as the success of new technologies such as radar, had forged a bond between scientists and government. Immediately after the war, scientists predicted that the newfound power of the atom would revolutionize many areas of life, including energy and the economy. But the importance of the atom also created a dilemma: the technology that promised an "atomic shield" and limitless nuclear power could also be used against the United States, a possibility foreshadowed most clearly when the Soviet Union developed atomic weapons in 1949.

As the atomic age began, scientists gained influence over government policy and attained a measure of public authority. Politicians, dazzled by the success of the Manhattan Project and eager for the prosperity promised by rapidly growing new technologies, opened their doors to scientists. The public, relatively uninformed about the atomic bomb, counted on scientists for information. Even those who came to oppose nuclear weapons relied upon scientists to lend credibility to their arguments against what was known simply as "the bomb." Nearly everyone expected that science would provide clear answers to the atomic dilemma. By emphasizing the objective nature of science, elite scientists found that they could influence U.S. nuclear policy to an extent.

As the Cold War continued, a significant number of scientists engaged in political activism to appose the arms race in a variety of ways. But these substantial challenges from elite scientists, this dissertation has argued, were neutralized by the state. Just as national

security policy against communism came to rely on the flexible doctrine of containment, so the state displayed an equally vast repertoire of tactics to contain scientific dissent. Technical arguments for disarmament were countered by opponents with equally scientific credentials, while moral concerns about nuclear weapons were rebutted with accusations of disloyalty. Still other scientists were dismissed as naïve or uninformed. And at still other times, the state attempted to co-opt scientific claims about the arms race.

The "scientific community" was never as cohesive as it seemed. Even scientists unaligned with the national security state often muted scientific disarmament efforts by questioning the professionalism of those who dared to link science to social commitment. Disagreement over nuclear issues was great enough that scientists' interpretations came to be seen as subjective, tarnishing scientists' prized reputation for objectivity. Nongovernment challengers like Pauling found their loyalty questioned by the government and their objectivity questioned by their peers. By the time grassroots antinuclear activism returned on a large scale in the 1980s, scientists found themselves largely divorced from this movement as activists embraced moral arguments against nuclear weapons. When Carl Sagan tried to link science to the antinuclear movement, many of his peers rewarded him skepticism and even scorn. Other scientists advocated arms control in the 1980s by using traditional technical arguments against nuclear weapons systems. Government advisors, Pugwash scientists, and outsiders alike were ultimately unable to convince enough policymakers that eliminating nuclear weapons was good for U.S. national security—not because their arguments were wrong, but because the state neutralized them with an onslaught of insinuations, weapons innovations, and pronuclear scientists.

Science, the State, and Society in the Twenty-First Century

In modern times, scientists have faced a steady demand from the public and the government for guidance about the forces of technology quickly demolishing old ways of life. In his landmark *Age of Extremes*, historian Eric Hobsbawm notes that "No period in history has been more penetrated by and more dependent on the natural sciences than the twentieth century. Yet no period, since Galileo's recantation, has been less at ease with it."[7] There is no reason to think that clamor from the public and the government for science will cease in the near future, as the planet and nation state system face such challenges as biological weapons, nuclear proliferation, toxic wastes from modern technologies, and global warming. In addition, nation-states around the globe—from North Korea to Iran to Russia to the United States—see nuclear weapons as the ultimate guarantor of their national security. Consequently, states will continue to rely on scientists to develop both life-affirming and destructive technologies.

When nuclear weapons initiated a potentially more deadly era of international relations at the beginning of the Cold War, U.S. scientists, as the guardians of nuclear knowledge, played a substantial role in shaping their nation's nuclear policy. But though the end of the Cold War has not diminished the danger of nuclear weapons, scientists have largely disappeared from policy-making circles, even as they are as present as ever in the laboratories that create military technologies. Already in the twenty-first century, sociologist Jeanne Guillemin writes, "in the name of national security, a new generation of scientists and technicians is being recruited to study defenses against a broad range of disease agents and toxins that might be candidates for biological weapons. As in the past, the participation of

[7] Hobsbawm, *The Age of Extremes: A History of the World, 1914–1991* (New York: Vintage, 1994), 522.

capable experts is as essential to any programmatic degradation of the life sciences as it is to their protection."[8] Another scholar has written that "scientific disciplines reshaped by Cold War politics will continue to align American science along a military axis long after the end of the Cold War."[9]

But in the public sphere, scientists are seldom seen. Since the dismantling of the Office of Technology Assessment in 1995, the U.S. Congress has confronted scientific dilemmas and threats, such as global warming, stem cell research, genetically modified organisms, diseases from AIDS to bird flu, and nuclear terrorism, without a ready supply of independent scientific advice. Meanwhile, the executive branch has been heavily criticized for selecting scientists to fill its agencies based on their adherence to partisan standards rather than their scientific merit.[10] Journalists and other observers have decried that as a consequence, policies are shaped by politicians with little or no understanding of the science and technology behind them. To these critics, scientists' lack of active involvement in policymaking can only have a detrimental effect on the life of the nation.[11] Yet such claims only reach partial truths since they have been based on a limited understanding of the development of this disconnect. As this dissertation has shown, many scientists have attempted to shape policy in many ways, from quasi-grassroots campaigns to scientific internationalism to cabinet-level access. But just as many obstacles blocked these efforts.

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[8] Jeanne Guillemin, "Seduced by the State," *Bulletin of the Atomic Scientists*, September/ October 2007, 14–16.
[9] Stuart W. Leslie, *The Cold War and American Science: The Military-Industrial-Academic Complex at MIT and Stanford* (New York: Columbia University Press, 1993), 11.
[10] Chris Mooney, *The Republican War on Science*, New York: Basic Books, 2005.
[11] Petroski, Henry. *Beyond Engineering.* New York, St. Martin's Press: 1986; Chris Mooney, "Requiem for an Office," *Bulletin of the Atomic Scientists,* September/ October 2005, Vol. 61, No. 5, 40–49. A recent *New York Times* article indicated that the number of scientists serving as members of Congress has increased slightly in recent years. Cornelia Dean, "Physicists in Congress Calculate their Influence," 10 June 2008, F2.

More often than not, presidential administrations throughout the Cold War actively challenged scientists who sought to push U.S. Cold War policy toward nuclear disarmament.

This dissertation has also asserted that scientists themselves bear some responsibility for this outcome. If this skewed dynamic is to be resolved, scientists will have to assert themselves in the realm of politics and society. In a recent column, science journalist Chris Mooney saw little reason to be optimistic. "Scientists recoil from the messy political process," he wrote, and "fear attacks on their carefully guarded objectivity."[12] By displaying such a near-disdain for the mingling of science, politics, and social commitment, it is no wonder science has lost public authority in recent years to simplistic evangelical religions. If scientists refuse to be more than mouthpieces enlisted to support or oppose various policy positions in the culture wars such as abortion, global warming, or stem cell research, they could find themselves with their objective reputations fully intact but completely divorced from the greater society.

Outsiders, Activists, and Foreign Policy

This dissertation has also spoken to the academic debate over the influence of activists and other government outsiders on foreign policy. At one time, foreign policy was seen purely as diplomatic history, where the thoughts and actions of statesmen were the most important subject matter. Thankfully, many scholars have recently addressed the notion that outsiders might have some influence foreign policy, often in a progressive manner.[13] But this

[12] Chris Mooney, "Hard Science," *The New Republic*, April 23, 2008, Vol. 238, No. 4,834, 11–12.
[13] See Lawrence Wittner, *The Struggle Against the Bomb: A History of the World Nuclear Disarmament Movement* (Stanford: Stanford University Press, 1993, 1997, 2003); Akira Iriye, *Global Community: the Role of International Organizations in the Making of the Contemporary World* (Berkeley: University of California Press, 2002); Thomas Borstelmann, *The Cold War and the Color Line: American Race Relations in the Global Arena* (Cambridge: Harvard University Press, 2001); Mary L. Dudziak, *Cold War Civil*

dissertation has offered a different view. No matter what these elite antinuclear scientists did, the state muted or at least contested their influence. Insiders, outsiders, rule breakers, and rule followers were all thwarted as long as they urged restraint in the arms race. Edward Teller, meanwhile, who knew no restraint when it came to nuclear weapons, was rewarded with influence. Despite being significant figures in society, these antinuclear scientists could not set U.S. Cold War policy on a different course.

Lawrence Wittner has argued the opposite in his work about the antinuclear movement. "[T]he reduction of the nuclear danger," he writes, "may have resulted, at least in part, from the struggle against the Bomb." These "opponents of the Bomb, by subjecting it to an onslaught of criticism, helped turn public sentiment against the weapon and thereby made it politically less acceptable as an instrument of war and diplomacy."[14] But this argument has a hint of desperation about it. First, hardly any government figures advocated the use of nuclear weapons after World War II. Second, is there much difference between atomic diplomacy and arms control diplomacy? All of the arms control treaties Wittner praises gave up some fanciful or irrelevant nuclear weapons, and none of them halted the arms race; after every treaty came a new innovation in weaponry that escalated the arms race.

Eric Hobsbawm offers a different assessment of the antinuclear movement. He writes,

> In short, the expensive high-technology hardware of superpower competition proved indecisive. The constant threat of war produced international peace movements, essentially directed against nuclear arms, which from time to time became mass movements in parts of Europe and were regarded by the Cold War crusaders as secret weapons of the communists. The movements for nuclear

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Rights: Race and the Image of American Democracy (Princeton: Princeton University Press, 2000); Penny von Eschen, *Race against Empire: Black Americans and Anticolonialism, 1937–1957* (Ithaca: Cornell University Press, 1997); and Sarah Snyder, "The Helsinki Process, American Foreign Policy, and the End of the Cold War," Ph.D. diss., Georgetown University, 2006.

[14] Wittner, *One World or None*, p. ix–x.

> disarmament were not decisive either . . . . At the end of the Cold War these movements left behind a memory of good causes and some curious peripheral relics, such as the adoption of the anti-nuclear logo by the post-1968 counter-cultures and an ingrained prejudice among environmentalists against any kind of nuclear energy.[15]

Yet Hobsbawm's assessment seems too bleak—could millions of activists have protested completely in vain? A similar question has surrounded Jeremi Suri's recent analysis of the 1968 uprisings. None less than Immanuel Wallerstein described the "world revolution" of 1968 as having "undermined the centrist liberal geoculture that was holding the world-system together." But Suri's book shows that the true legacy of these great uprisings was the counterrevolution of détente that they inspired.[16]

Suri reminds us of a painful truth: protest is met most often not with success, but with counterprotest. Failure is the norm among social movements. Instead of searching for minor ways protesters achieved a small, token aspect of their goals, scholars would do well to remember that the failure of protest can be seen as an indictment of the states against which protest is waged. If scientists were largely ignored during the Cold War, it shows that U.S. policy was based less and less on rational thought. In an ostensible democracy, scientists' social commitment was scorned by the government and other scientists. Indeed, it is no surprise—and no shame—that protest failed.

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[15] Hobsbawm, *Age of Extremes*, 237–38.
[16] Immanuel Wallerstein, *World-Systems Analysis: An Introduction* (Durham: Duke University Press, 2004), x; Jeremi Suri, *Power and Protest: Global Revolution and the Rise of Détente* (Cambridge: Harvard University Press, 2003).

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