"THE MIGHTY MICROBE CAN GO TO WAR:" SCIENTISTS, SECRECY, AND AMERICAN BIOLOGICAL WEAPONS RESEARCH, 1941-1969

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Much of the credit for this dissertation goes to all of these people. All errors in it are, of course, my own.

Abstract

During WWII and the early Cold War, an American research program centered on Fort Detrick, Maryland sought to transform germs into weapons. Though sponsored by the military officials of the US Army Chemical Corps, this program was heavily intertwined with the civilian microbiologist community. Some of the most prominent members of this community had organized biological weapons research and staffed Detrick during WWII, and microbiologists' influential relationship with the military continued into the Cold War. In this dissertation, I examine this relationship, focusing in particular on the roles that military secrecy played in these scientists' lives. I examine the informal agency of scientists who served as military 'advisors' within the classified world, as well as the role of these same scientists in disciplining dissent within their own scientific community. I examine the evolution of laboratory safety technologies at Detrick from a means to contain secrets to one of the major legacies of biological weapons research in the 'open' world. I examine the contingent nature of the secrecy system itself, and how scientists variously subverted and supported it. Finally, I examine how would-be scientific critics of biological warfare negotiated their position as 'outsiders' of the secrecy system to contribute to the eventual end of this research at Detrick in 1969. With this work, I contribute to the underdeveloped historiography of the biological sciences in the Cold War, as well as to that of secrecy and science.

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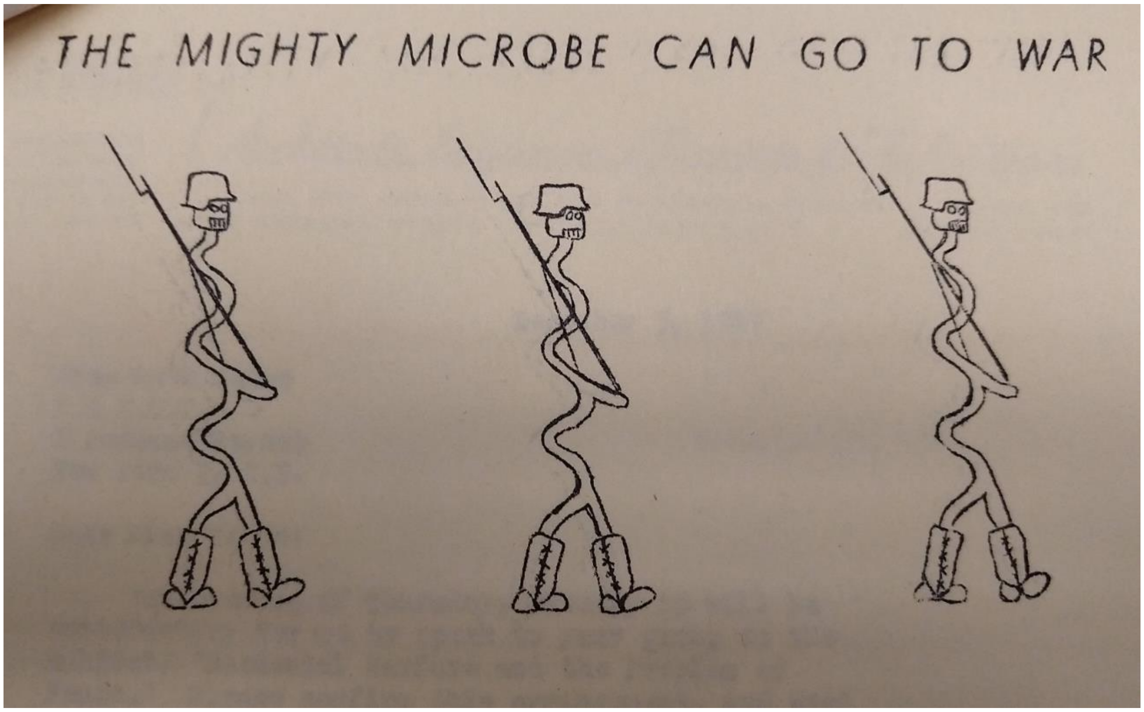

Figure 1: Flyer Advertising Lecture by Theodor Rosebury, entitled "Bacterial Warfare and the Problem of Peace," New York, NY: January 15, 1948, sponsored by the Science Forum of the Federation of Architects, Engineers, Chemists, and Technicians Local 231[1]

[1] Held in National Library of Medicine (NLM) Theodor Rosebury Papers (MS C 634), Box 2 Folder 16 (Correspondence 'F').

Introduction: Secret Science at Camp Detrick

In early December 1942, Professor Ira L. Baldwin of the University of Wisconsin joined a meeting at the National Academy of Sciences (NAS).[2] An expert on soil microbiology, he had been summoned to Washington by an Army colonel working with his colleague and mentor, soil bacteriologist and dean of Wisconsin's graduate school, E. B. Fred. Baldwin knew that Fred, who was one of the few bacteriologist members of the Academy, had been traveling regularly to Washington for almost a year and a half, but Fred's activities were a mystery. A little over a year after Pearl Harbor, Baldwin could probably guess that Fred and the NAS were involved with some sort of research to support the American war effort, just as Baldwin himself had been working on projects to produce penicillin and artificial rubber. But what he heard at that meeting was a shocking surprise that, as Baldwin later remembered, redefined his professional life. Since the fall of 1941, Fred revealed, Academy scientists had been secretly working with the US military to determine whether germs could be used as practical weapons of war. Fred and the other NAS scientists did not know it, but the research program that they were helping to establish would endure until 1969.

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[2] This meeting is described in Ira L. Baldwin, *My Half-Century at the University of Wisconsin: Adapted from an Oral History Interview by Donna Taylor Hartshorne,* Madison, WI: Privately Printed by Ira L. Baldwin, 1995, pp 121-122. The National Academy of Sciences was founded by an act of Congress in 1863, but was a civilian organization not formally connected with the US government. Nonetheless, by WWII it had established a tradition of lending scientific support to the government and military in times of war (having established the National Research Council to coordinate research during WWI). Fred's "WBC committee" was representative of this tradition. See the (now-dated) official history, Rexmond C. Cochrane, *The National Academy of Sciences: The First Hundred Years, 1863-1963,* Washington, DC: National Academy of Sciences, 1978.

The general outline of the U.S. biological weapons program, to which I return below, is a familiar one to historians of Second World War and Cold War science.[3] Scientists who never would have dreamt that their work would have military significance when they began their careers developed *ad hoc* relationships with the state during the emergency of the Second World War. As the Cold War began, these *ad hoc* relationships became more permanent. Many scientists, with motivations ranging from a sense of patriotism to the cornucopia of financial support the military could offer, chose to accept the compromises of continuing to participate in military research. Some of these compromises were ethical, as scientists confronted the role their work played in developing weapons for an increasingly dangerous arms race. Many others were practical, with the military sponsors who held the purse strings being able to direct the focus of research, and military secrecy systems isolating scientists from the professional rewards of publishing their work.

This dissertation analyzes the relationship between biological scientists and this military-sponsored research program between the 1940s and the 1960s. It argues that both sides were deeply affected by their interaction with each other, and focuses in particular on the role that military secrecy played in all aspects of this exchange.

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[3] Major histories of the American biological weapons program which Fred and his compatriots were helping to organize include Barton J. Bernstein, "Origins of the U.S. Biological Warfare Program," in Susan Wright (ed), *Preventing a Biological Arms Race*, Cambridge, MA: MIT Press, 1990, pp 9-25; Ed Regis, *The Biology of Doom: The History of America's Secret Germ Warfare Project*, New York: Henry Holt Co., 1999; John Ellis van Courtland Moon, "US Biological Warfare Planning and Preparedness: The Dilemmas of Policy," in Erhard Geissler and John Ellis van Courtland Moon, eds., *Biological and Toxin Weapons: Research, Development and Use from the Middle Ages to 1945 (SIPRI Chemical & Biological Warfare Studies 18)*, New York: Oxford University Press, 1999, pp 215-254; John Ellis van Courtland Moon, "The US Biological Weapons Program," in Mark Wheelis, Lajos Rózsa, and Malcolm Dando (eds), *Deadly Cultures: Biological Weapons since 1945*, Cambridge, MA: Harvard University Press, 2006, pp 9-46. For a general introduction to the history of biological warfare, see Jeanne Guillemin, *Biological Weapons: From the Invention of State-Sponsored Programs to Contemporary Bioterrorism*, New York: Columbia University Press, 2005.

Microbiologists like Fred and Baldwin were integral to organizing and supporting military research, reflecting the important new-found role of scientists in the nascent American national security state. This relationship with the state likewise affected the scientists, with their understanding of germs as a public health concern transformed by military priorities and ways of thinking, and their community guided by many of its leaders into still closer collaboration with the military. Not all microbiologists agreed with these changes, and just as some of their colleagues had played a central role in organizing American biological weapons research, many who wished to resist this military co-option of their science were integral in bringing this research to an end. As subsequent chapters show, all of these scientists developed their own relationships with military secrecy, from lobbying to soften its impact on their community to supporting it in the public sphere to (in the case of would-be protesters) resolving the dilemma of opining about knowledge to which they were not always privy.

Background: "Biological Warfare" from Science Fiction to Practical Reality

This transformation of American microbiology into an ally of the military was not predictable, and certainly not inevitable. To most American scientists in the 1930s, bacteriological warfare was a feature of science fiction, mentioned by authors like Aldous Huxley among the possible horrors of a future war. Biological weapons had been banned by the 1925 Geneva Protocol, but this was essentially an afterthought for the Protocol's drafters, whose major concern had been to keep chemical weapons off the battlefield in the wake of the First World War.[4] The mainstream American military consensus was that

[4] On the 1925 Geneva Convention, see Jerzy Witt Mierzejewski and John Ellis van Courtland Moon, "Poland and Biological Weapons," in Geissler and van Courtland Moon, eds., *Biological and Toxin Weapons*, pp 63-69. In Aldous Huxley's 1931 *Brave New World*, "anthrax bombs" as well as chemical and

germs would be impractical weapons, easily combated by modern medicine. US Army Medical Corps officer Leon Fox summed up this viewpoint in a 1933 article that critiqued the "biological warfare" ideal by valorizing his own field of study. Military medicine, he opined, had perfected its ability to keep armies in the field from being devastated by diseases like cholera and dysentery. Likewise, introducing plague-carrying rats would simply lead to both armies being infected, and no-one knew how to induce epidemics of respiratory diseases like influenza. Trying to deliberately deploy germs as weapons was a fool's errand, Fox concluded.[5]

In the years between Fox's paper and Baldwin's 1942 NAS meeting, however, some scientists had begun seriously thinking about how to use their knowledge of microorganisms to create biological weapons. Unknown to Fox, French researchers under Auguste Trillat had already begun investigating whether microbes could survive being spread by the explosion of a bomb or shell, research which would continue up to the fall of France in 1940.[6] In Japanese-occupied Manchuria, military physician Shirō Ishii organized a secret full-scale biological warfare research program called Unit 731 in 1937, which later became infamous for its brutal human experimentation and attempts to introduce diseases like plague to the Chinese population.[7] In the United States, meanwhile, ideas about how respiratory diseases can be spread were being pursued by

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high-explosive bombs are used in the apocalyptic "Nine Years' War" that ushers in the Fordist World State. Aldous Huxley, *Brave New World*, London: Chatto & Windus, 1932.

[5] Leon A. Fox, "Bacterial Warfare: The Use of Biologic Agents in Warfare," *Military Surgeon* 72 no 3 (1933), pp 189-207. Fox's paper can be taken at face value; he was not attempting concealment of a germ weapon program. The timing is interesting, since it was during the Truth and Reconciliation Commission (World War I) that German sabotage of the neutral powers, using crude biological agents, came to light.

[6] Olivier Lepick "French Activities Related to Biological Warfare, 1919-45," in Geissler and van Courtland Moon, eds., *Biological and Toxin Weapons*, pp 70-90.

[7] Sheldon H. Harris, *Factories of Death: Japanese Biological Warfare, 1932-1945, and the American Cover-Up, rev. ed.*, New York: Routledge, 2002.

scientists like William Firth Wells, whose studies of the airborne transmission of such diseases over relatively long distances challenged earlier droplet transmission ideas that respiratory disease only spread between people a few feet from one another.[8] The science of "aerobiology" inspired by Wells' work was well-known among biologists in the early 1940s. Some, including Karl F. Meyer of the G. W. Hooper Foundation's Institute for Medical Research speculated that even microbes like the causative species of brucellosis, whose lifecycles did not normally rely on respiratory transmission, could nonetheless waft through the air of the laboratory and find their way into the bodies of researchers. This would explain why such microbes were so notorious for causing unexplained laboratory infections.[9] Microbiologist Theodor Rosebury and immunologist Elvin Kabat of Columbia University expanded on Meyer's observation. Rosebury and Kabat penned a 1942 report, read by the NAS group, suggesting that if one wanted to use germs as weapons, such laboratory infection-causing diseases were a promising avenue to investigate.[10] The British and Canadians, concerned that the German enemy had a working biological weapon, had been pursuing their own biological warfare programs since the late 1930s. They began sharing information about anthrax as a weapon with their new American allies under a tripartite agreement.[11]

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[8] See Gerard James Fitzgerald, "From Prevention to Infection: Intramural Aerobiology, Biomedical Technology, and the Origins of Biological Warfare Research in the United States, 1910-1955," PhD diss, Carnegie Mellon University, 2003.

[9] K. F. Meyer and B. Eddie, "Laboratory Infections Due to *Brucella*," *The Journal of Infectious Diseases* 68 no 1 (1941), pp 24-32.

[10] E. B. Fred, "Memorandum: Subject: Conference with Prof. A. R. Dochez and discussed the paper on Bacterial Warfare by Drs. Theodor Rosebury, Elvin A. Kabat and Martin H. Boldt," September 11, 1942 in National Academy of Sciences Archives collection "Committees on Biological Warfare, 1941-1948" (NAS BW) Box 7 Folder 19 ("Fred, E.B.: Memoranda (Black Book): 1942-1943")

[11] Brian Balmer, *Britain and Biological Warfare: Expert Advice and Science Policy, 1930-65*, New York: Palgrave Macmillan, 2001; Donald Avery, *Pathogens for War: Biological Weapons, Canadian Life Scientists, and North American Biodefense*, Toronto: University of Toronto Press, 2013.

Spreading germs was not the only problem confronting Fred's NAS committee as they began setting up an American counterpart to the Anglo-Canadian research programs in December 1942. There was also the question of growing them. To be sure, any of the microbiologists in the room could cultivate a microbe such as *Bacillus anthracis* (which had been established as the cause of the disease anthrax in 1876) on a petri dish.[12] The skill of cultivating *B. anthracis* and organisms like it was integral to the training of a microbiologist, whether they studied pathogens or whether, like Fred and Baldwin, their research focused on what historian Eric Kupferberg has called the 'productive microbes' of soil, dairy, and industrial microbiology.[13] A microbial culture from a petri dish, with a mass measured in grams, however, would be entirely insufficient for whatever bombs, spray tanks, or other weapons system a biological weapons program might develop. The meeting that Baldwin joined was devoted to the question of whether it was possible to grow pathogenic microbes by the pound or ton. While many of his colleagues around the table were skeptical, Baldwin himself answered in the affirmative. He and his Wisconsin colleagues were specialists in doing exactly that with nonpathogenic microbes, and as Baldwin recalled confidently asserting, "if you can [grow pathogens] in a test tube, you can do it with a ten-thousand-gallon tank… all you have to do is make the same conditions in a ten-thousand-gallon tank that you make in a test tube."[14]

Baldwin's brash can-do attitude impressed the military officials at the meeting, and a few weeks later he was placed in charge of organizing a research team to make his

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[12] See Chapter 2 of Susan D. Jones, *Death in a Small Package: A Short History of Anthrax*, Baltimore: Johns Hopkins University Press, 2010.
[13] Eric D. Kupferberg, "The Expertise of Germs: Practice, Language, and Authority in American Bacteriology, 1899–1924," PhD diss, Massachusetts Institute of Technology, 2001.
[14] Baldwin, *My Half-Century*, p 122.

assertions about large-scale production into a reality. This site would be one part of an organization co-sponsored by the military and the NAS group called the War Research Service, which had recruited pharmaceutical executive George Merck to lead it earlier in the year. Like the larger Office of Scientific Research and Development (OSRD), the WRS sought to organize civilian scientific expertise to support the American war effort through a system of research contracts granted to university-based scientists. Baldwin's task, in contrast, was to establish a centralized research site for large-scale production research, work which was seen by WRS leaders and the NAS' new "ABC" committee as unsuitable for a university contract. Instructed to keep such research far enough from Washington to preserve secrecy and safety, but close enough to be easily accessible by WRS officials and advisors, Baldwin selected a sleepy National Guard airbase called Detrick Field, outside the town of Fredrick, Maryland, as the site to begin his team's research. Throughout 1943 Camp Detrick (as the site came to be known) witnessed an explosive growth, first as temporary buildings were erected to study how to produce botulism toxin and anthrax in bulk, and later as more-permanent laboratory buildings began to spring up. These represented an increasing centralization of WRS research, as university-based projects were annexed to the central site of Detrick, and a staff which Baldwin and the WRS leaders had originally envisioned as small and focused on bulk production ballooned into the hundreds. Biological weapons, it seemed to WRS and military leaders, were a promising way of making war after all, and the Americans, not content to just play wholesale supplier to the British bioweapons program, duplicated the British research program on anthrax and expanded on it by investigating other notorious laboratory infections like brucellosis, tularemia, and psittacosis.[15] Teams working on plant diseases, and another working on chemical herbicides were also brought to Detrick, to serve a loosely-defined vision of 'biological warfare' which could include attacking enemy food crops as well as enemy bodies. Following a similar logic, another American team collaborated with the Canadians to research the cattle disease rinderpest on a remote island in Quebec.[16] The Army's Chemical Warfare Service, which had been the major military partner in these endeavors, took an ever-greater interest as Detrick grew, and by 1944 had fully annexed the WRS' program to its administrative control.

By the end of the Second World War in September 1945, Camp Detrick was a bustling facility with personnel measured in the thousands, a subordinate archipelago of facilities including the Canadian rinderpest laboratory and a planned production plant and field-testing site, and research projects on a variety of different pathogens. Many of these projects were incomplete, however. The closest the Allies had come to using 'biological warfare' (broadly construed) were plans to attack the Japanese rice crop with herbicides. This was forestalled by the end of the war, and even though the full-scale production plant was ready to produce anthrax by the ton as Baldwin had promised, it had not begun to do so when Japan surrendered.[17] Scientists like Baldwin (who had himself returned to Wisconsin earlier in the year) generally expected to return to their universities and resume a normal scientific life, far from the military secrecy and work to weaponize their

[15] See the post-war official history, Rexmond C. Cochrane, *History of the Chemical Warfare Service in World War II, Volume 2: Biological Warfare Research in the United States*, Edgewood Arsenal: Historical Section, Office of the Chief, Chemical Corps, 1947.
[16] Amanda Kay McVety, *The Rinderpest Campaigns: A Virus, Its Vaccines, and Global Development in the Twentieth Century*, New York: Cambridge University Press, 2018.
[17] See Barton J. Bernstein, "America's Biological Warfare Program in the Second World War," *Journal of Strategic Studies* 11 (1988), pp 292-317.

science which had colored their experience of the wartime emergency. There seemed to be a decent chance that the American dalliance with biological warfare would end, relegated to the best-forgotten footnotes of the Second World War.

Instead, Detrick and its research program were kept active, and biological warfare research quietly became yet another feature of the nascent American Cold War state.[18] While most wartime Detrick researchers returned to civilian life, a minority remained as permanent employees, supplemented by new hires. For these scientists, the disruptions of wartime life were made permanent. They lived under a military secrecy system which prevented them from publishing their work without permission, and worked in a research center staffed by hundreds of scientists, far larger than the norm in the university world. Perhaps more importantly, biological warfare research, summarized by observers as "public health in reverse" challenged the very core of microbiological research.[19] Microbiologists were presented with the ethical question of whether their science of studying pathogens, commonly valorized as a war on disease, should be turned toward *spreading* disease for war. This question had also confronted the scientists who had organized and participated in the program during the Second World War. Some, like Baldwin, justified the use of their science for war with a self-conscious refusal to make a moral distinction between killing with germs and killing with bombs or bullets.[20] This reflected the rhetoric that supporters of chemical warfare (including Detrick's military

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[18] Major discussions of the melding of biological knowledge and military planning in this period include Edmund Russell, *War and Nature: Fighting Humans and Insects with Chemicals from World War I to* Silent Spring, New York: Cambridge University Press, 2001 and Jacob Darwin Hamblin, *Arming Mother Nature: The Birth of Catastrophic Environmentalism*, New York: Oxford University Press, 2013.
[19] The term "public health in reverse" was somewhat of a cliché among commenters on biological warfare. See e.g. its use in a popular book on the subject, Theodor Rosebury, *Peace or Pestilence: Biological Warfare and How to Avoid It*, New York: Whittlesey House, 1949.
[20] Baldwin, *My-Half Century*, pp 124-125.

sponsors in the CWS) had been using for decades, complete with the assertion that if anything, attacking the enemy with germs (or gas) was *more* humane than using guns, high explosives, or flamethrowers.[21]

Others, like Theodor Rosebury, were conscious of the emergency of a globe-spanning total war, fearing that the Axis powers were engaged in biological warfare preparations of their own. Post-war scientists justified their continued participation in biological warfare research in similar ways. Some saw continuing to work at Detrick, or (like Baldwin) to support it as a military advisor as the best defense against presumed Soviet bioweapons research. Others continued to rely on a rhetoric of studied amorality, in which ethical objections to using germs as weapons were dismissed as 'emotional' or not 'objective,' and certainly unbefitting a scientist.[22] Whatever their motivations, large numbers of civilian microbiologists had worked at Detrick, circulated through it to learn new techniques, or supported its continuing existence as a Cold War necessity. The biological weapons research at Detrick therefore kept developing as the uncertainty of the initial postwar transitioned to the chill of the Cold War.

[21] An example of this sort of rhetoric (from a prominent British supporter of gas warfare) can be found in J.B.S. Haldane, *Callinicus: A Defence of Chemical Warfare*, New York: E. P. Dutton & Company, 1925.

[22] "Objectivity" (which as a matter of course entailed personal political orthodoxy) was an important cultural currency for scientists involved in policy during the early decades of the Cold War, reflecting the cultural authority of science writ large in mid-20th century America. For discussions of this rhetoric of "objectivity" (and particularly the challenges it presented to scientists who wished to challenge the so-called Cold War consensus prevailing in mainstream politics in the 1950s), see e.g. Kelly Moore, *Disrupting Science: Social Movements, American Scientists, and the Politics of the Military, 1945-1975*, Princeton: Princeton University Press, 2008; Jessica Wang, "Physics, Emotion, and the Scientific Self: Merle Tuve's Cold War," *Historical Studies in the Natural Sciences* 42 no 5 (2012), pp 341-388; Paul Rubinson, *Redefining Science: Scientists, the National Security State, and Nuclear Weapons in Cold War America*, Boston: University of Massachusetts Press, 2016; and Audra J. Wolfe, *Freedom's Laboratory: The Cold War Struggle for the Soul of Science*, Baltimore: Johns Hopkins University Press, 2018.

Historiography: Comparing the Physical and Biological Sciences

Historians of Cold War science have explored similar themes in varying degrees, particularly focusing on the impact of military priorities on the development of the physical sciences.[23] However, historians have paid far less attention to the biological sciences' relationship to the national security state in this period. The military secrecy system, in turn, has been studied in far less detail by even historians of the physical sciences than the 'militarization' of research priorities or even the ethical issues of weapons research. The biological weapons program, with its military sponsorship of biological research steeped in secrecy, is an ideal case with which to analyze ethics and military secrecy.

Two major works which touch on secrecy in the Second World War and Cold War physical sciences are particularly helpful in contextualizing secrecy in the biological weapons program. Peter Westwick's *The National Labs* is an examination of the Atomic Energy Commission's National Laboratories in the 1940s through the 1960s- the same period when offensive research was taking place at Detrick.[24] Westwick's history primarily focuses on the administrative history of how the various National Laboratories were founded and developed, arguing that despite the *ad hoc* nature of this history, the laboratories can best be understood as a cohesive system rather than a set of disparate institutions. An important feature of this system was its secrecy, stemming from the

[23] See Paul Forman, "Behind Quantum Electronics: National Security as Basis for Physical Research in the United States, 1940-1960," *Historical Studies in the Physical and Biological Sciences* 18 no 1 (1987), pp 149-229 and Daniel Kevles, "Cold War and Hot Physics: Science, Security, and the American State, 1945-56," *Historical Studies in the Physical and Biological Sciences* 20 no 2 (1990), pp 239-264, for a foundational debate on the subject.
[24] Peter J. Westwick, *The National Labs: Science in an American System, 1947-1974*, Cambridge, MA: Harvard University Press, 2003.

Atomic Energy Act under which the AEC had been founded. Individual scientists working in the Laboratories faced major professional costs from this secrecy, such as a reduced ability to publish their work and burnish their reputation among their 'open' peers. The scientists working in the Laboratory system, Westwick argues, reacted to these restraints by essentially re-creating their scientific community within the classified world. They set up and published papers in journals, organized conferences, and established reputations among their peers, all classified and requiring a security clearance. Fitting with Westwick's framework of viewing the Laboratories as an interlocking system, he portrays AEC secrecy as a closed door, behind which scientists replicated their 'open' world.

This physicists'-eye view of nuclear secrecy contrasts with historian Alex Wellerstein's examination of the nuclear secrecy system itself in various works culminating in his recent *Restricted Data: The History of Nuclear Secrecy in the United States*.[25] Nuclear secrecy, according to Wellerstein, has been fraught and contested since nuclear research began in the United States. A voluntarist system spearheaded by Leo Szilard (in which nuclear physicists willingly refrained from publishing their work as war clouds loomed) was superseded by the formal military secrecy to which those working on the Manhattan Project were subjected during the war, but neither was envisioned as a long-term solution. Schemes to control nuclear knowledge under pre-existing legal structures like the patent system risked publicizing that knowledge, which undergirded the unprecedented and constitutionally dubious prior restraint on speech in the Atomic

[25] Alex Wellerstein, "Patenting the Bomb: Nuclear Weapons, Intellectual Property, and Technological Control," *Isis* 99 no 1 (2008), pp 57-87; Alex Wellerstein, "Knowledge and the Bomb: Nuclear Secrecy in the United States, 1939-2008," PhD diss, Harvard University, 2012; Alex Wellerstein, *Restricted Data: The History of Nuclear Secrecy in the United States*, Chicago: University of Chicago Press, 2021.

Energy Act. Even after the secrecy system of "Restricted Data" was created by the Act, Wellerstein argues, the proper purview and boundaries of atomic secrecy remained contested and maintaining the day-to-day operations of the secrecy system took significant effort by the AEC and later the Department of Energy. A key point in Wellerstein's argument is that 'open' knowledge was as important an element in the secrecy system as that which was hidden. The public act of revelation in events like the 1945 publication of the book-length 'Smyth Report' on the physics of the atomic bomb implicitly reinforced the status of other knowledge as 'the' atomic secret, justifying the most extreme of measures to protect it. Classified information, meanwhile, was not quite the same thing as unknown or secret knowledge, as incidents in which college students and journalists 'designed' bombs of their own attested. The line between 'secret' and 'open' knowledge was neither clear nor uncontested, and moreover, elements of both worlds were in practice integral to upholding the nuclear secrecy system as a whole.

Despite being most heavily studied by scholars like these, the AEC's nuclear secrecy system was far from the only one in the mid-20th century US' national security state, and the system surrounding the biological weapons program reveals important differences. Formally speaking, nuclear 'Restricted Data' and the system to control them was different from the military secrecy system under which Detrick operated (for one thing, there was no statute like the Atomic Energy Act under which microbiological knowledge could be 'born secret'). The less-formal social world of scientists working under the Detrick system also differed from that of the AEC's National Laboratories. Detrick was, like the Laboratories, a large centralized center working on heavily classified research, but nonetheless Detrick researchers retained far closer ties to 'open' instruments of credit like journals and conferences than Westwick argues the physicists enjoyed. Conversely, however, there was a far smaller canon of 'open' knowledge to complement the secret world of biological warfare than that about nuclear weapons. In contrast to the book-length Smyth Report, the postwar 'Merck Report' that revealed the existence of biological warfare research to the public was only a few pages long.[26] As Wellerstein shows, subsequent technical developments in nuclear weapons were discussed and sometimes hotly debated in the public sphere (as in the Truman-era controversy over whether to pursue research on the thermonuclear 'Super'); in contrast, a culture of official silence on even the most general elements of biological warfare following the Merck report made the very existence of the American biological weapons program a kind of open secret, theoretically known about but not able to be discussed with any specificity. Examining the nature of biological warfare secrecy can complement the relatively well-studied case of nuclear secrecy and enrich our understanding of secrecy's role in the American security state. Of broader interest to historians of science, however, is what understanding biological warfare secrecy can tell us about the relationship between secrecy and science in general.

The study of this relationship has been a fruitful and growing area of scholarship in recent years. Most of these recent studies of secrecy and science take as their tacit jumping-off point a rejection of sociologist Robert Merton's assertion that the two words are diametrically opposed. Writing in 1942, the same year that the NAS was debating biological warfare and other scientists around the world were pursuing any number of

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[26] George W. Merck, "Official Report on Biological Warfare," *Bulletin of the Atomic Scientists* 2 no 7-8 (1946), pp 16-18.

military-related projects for the global war that was raging, Merton consciously sought to identify normality in an extraordinary time.[27] Among norms of science that he identified was 'communism,' or the free and open sharing of knowledge, the polar opposite of which was the secrecy of research in 'totalitarian' regimes or even (and by necessity) in the wartime United States. From a Mertonian perspective, the structures of scientific secrecy that were made permanent as the national security state solidified in the Cold War were unfortunate aberrations from good science at best and unethical corruptions of it at worst. By implication, the pre-WWII scientific past was Edenic, with scientists freely sharing and publishing knowledge before biting the apple of state sponsorship.

Historiography: Secrecy and Science

More recent scholarship, in contrast, paints a more nuanced and less dichotomous view of the relationship between secrecy and science both in the 20th century and in other historical periods. Historical sociologist Brian Balmer, for instance, has examined the secrecy system surrounding the British biological weapons program centered on the military research facility of Porton Down, a close contemporary of the American program centered at Detrick.[28] He focuses principally on the administrative aspects of the British secrecy system using sources like meeting minutes and policy memoranda. Like Wellerstein, Balmer depicts the secrecy system as contingent, in continuous need of maintenance, and in general as far less of a stable monolith than it might appear to an outside observer. Besides problematizing the model of secrecy as a clear demarcating line

[27] Robert Merton, "Science and Technology in a Democratic Order," *Journal of Legal and Political Sociology* 1 (1942), pp 115-126.
[28] Brian Balmer, *Secrecy and Science: A Historical Sociology of Biological and Chemical Warfare*, London: Ashgate Publishing, 2012.

between two domains of knowledge, Balmer examines how science conducted under such a secrecy system was affected by this instability. Rather than simply looking like ‘open’ science conducted behind closed doors, he argues, the social values of secret microbiology in Britain were fundamentally affected by the secrecy system. Accidental events which could not be repeated for both ethical and secret-preserving reasons functioned with greater weight as sources of ‘experimental’ knowledge, for instance. Likewise, the rhetorical power of scientific certainty was turned on its head: while ‘open’ scientists advocating particular policies would typically use certainty to buttress their authority, scientists from the secret world of Porton Down used their *uncertainty* about what was possible to justify their work by painting worst-case pictures of what their presumed secret Soviet counterparts might do. Far from being an inferior or perverted form of an idealized model of science, however, Balmer argues that the microbiology operating in the social milieu of secrecy was simply different than sciences operating outside of it.

This point is particularly worth making when discussing science in historical context, particularly outside of the context of the mid-20th century. Scholars of early modern science like Mario Biagioli and Koen Vermeir, for instance, have pointed out how secrecy- both the concealment of knowledge and the public performance of concealment- was an integral part of how natural philosophers presented themselves and their work to the world.[29] A similar theme in the 20th century world of professionalized science can be seen in the work of Stephen Hilgartner. In a paper on biotechnology

[29] See Mario Biagioli, *Galileo, Courtier: The Practice of Science in the Culture of Absolutism*, Chicago: University of Chicago Press, 1993; Koen Vermeir, “Openness Versus Secrecy? Historical and Historiographical Remarks,” *British Journal for the History of Science* 45 no 2 (2012), pp 165-188.

researchers in the 1990s, Hilgartner examines the commercial secrecy of their work as one facet of their communication strategies.[30] Selective revelation of both the existence and content of hidden knowledge was a major tactic for the scientists he studies, used to warn off potential rivals and in truncated exchanges of information. Far from being dichotomous with open publication, Hilgartner argues that secrecy is part "of the same overarching category, namely practices that aim to effect control over which knowledge becomes available to whom, when, under what terms and conditions, and with what residual encumbrances."[31] This view of secrecy as one of many communications strategies is very similar to Wellerstein's discussion of the role of revelation in the nuclear secrecy system, but Hilgartner's specific focus on scientists cuts particularly at the heart of Mertonian presumptions.

Hilgartner and Balmer have backgrounds in the sociology of science, and their interest in secrecy in part reflects how sociologists, anthropologists, and philosophers have paid more attention to the subject than historians. One outgrowth of this interest which has incorporated historians, however, is the burgeoning field of agnotology. Agnotology, literally the study of ignorance, turns both the study of secrecy and the general epistemological interests of scholars of science on their heads by focusing on social processes by which ignorance is actively constructed and maintained. There are several strains to this area of study, which can be seen in the programmatic 2008 edited volume *Agnotology: The Making & Unmaking of Ignorance*.[32] Scholars like Robert

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[30] Stephen Hilgartner, "Selective Flows of Knowledge in Technoscientific Interaction: Information Control in Genome Research," *The British Journal for the History of Science* 45 no 2 (2012), pp 267-280.
[31] Ibid, p 268.
[32] Robert N. Proctor and Londa Schiebinger (eds), *Agnotology: The Making and Unmaking of Ignorance*, Stanford: Sanford University Press, 2008.

Proctor, Naomi Oreskes and Eric Conway focus on how "merchants of doubt" working for the tobacco and fossil fuel industries of the mid-20th century produced public ignorance through uncertainty, while Londa Schiebinger discusses how the origins of indigenous and other subaltern knowledge became obscured in colonial knowledge-making. What unites these disparate approaches, however, is a focus on ignorance itself as an entity requiring work to produce and maintain, akin to how knowledge is examined in the more-venerable field of epistemology. Viewed in this way, secrecy is at least an exercise in "anti-epistemology" (as Peter Galison puts it in an essay on the enormity of the domain of classified knowledge that appears in modified form in this volume), requiring substantial social and physical resources to maintain.[33] As in the case of 20th century corporate obfuscation, ignorance of a fact need not be absolute, and even ambiguous lack of knowledge can have significant social and political impacts.

This dissertation draws on all this scholarship to examine the functions that secrecy played in mediating scientists' relationships with the US military in the biological weapons program. The biological weapons secrecy system shared a number of similarities with its atomic counterpart in mediating these relationships. Most obviously, secrecy functioned as a reasonably effective means of controlling information about what was happening at Detrick, and during WWII, that such research was taking place at all. As with the Manhattan Project, the absolute secrecy surrounding Detrick during the war kept knowledge of American capabilities and intentions out of the hands of Axis leaders. During the subsequent Cold War, the less-absolute but still tight secrecy that surrounded

[33] For the original essay, see Peter Galison, "Removing Knowledge," *Critical Inquiry* 31 no 1 (2004), pp 229-243.

information directly pertaining to weapons served a similar role of denying the Soviet Union militarily useful intelligence. Secrecy also had implications in domestic as well as international politics. The absolute wartime secrecy of both the atomic and biological weapons programs, for instance, precluded public scrutiny of or ability to approve or disapprove of such research, with even most elected representatives knowing little to nothing about what the Army and its scientific collaborators were doing.

After the war, however, atomic and biological secrecy diverged. While a grand theatre of selective public revelation added mystique to 'the' atomic secret, 'biological warfare' remained a virtual black box in the public eye, about which few details other than its existence were authoritatively known. While public debates about nuclear weapons could turn on relatively nuanced details like the potential power of thermonuclear reactions, biological weapons' status as an 'open secret' stymied concerted public opposition to or support for such research alike. Biologists who wished to campaign against such research, unlike their physicist counterparts, faced a correspondingly more difficult task of nailing down the thing about which they were making their claims.

Scientists and military officials who supported further bioweapons research faced a corresponding conundrum. While the secrecy surrounding biological warfare allowed them to dismiss would-be critics as simply ignorant of important facts and thus unscientifically 'sensationalistic' in their claims, it also made it more difficult to make converts of the civilian scientific community more broadly. Claims undergirding the entire 'bioweapons' project, like the ability of artificially generated microbial aerosols to survive and infect human hosts reliably enough to serve as a weapon were far harder to defend with the most relevant research being classified. Scientists and military officials 'in the know' worried that skepticism and indifference about these claims among their peers would deny them an important constituency to draw upon in higher-level budgetary battles. One result of this was a strategy of selective revelation, such as a series of open conferences in the 1960s expressly organized to advertise previously classified information on airborne microbes with a wider scientific audience. Similarly, a strategy of maintaining a relatively liberal publication policy for Detrick researchers in the 1950s and 1960s was largely intended to encourage recruitment of scientists worried about careers outside of the classified world. As with the AEC's National Laboratory system, secrecy threatened to cut off Detrick researchers from the social reward structures of their 'open' scientific community, but this liberal publication policy sought to bridge this division where the formation of a parallel community described by Westwick accepted and strengthened it. Selective openness was certainly an important part of both the atomic and biological secrecy systems, but in different ways and often serving different functions.

Scientists played an important role in setting these strategies of selective openness while serving in various roles as military 'advisors.' For these scientist-advisors, holding a security clearance and being privy to Detrick's secrets granted them substantial agency to influence military policies, and to support a research program they earnestly believed was important for American national security. In so doing, they also supported their colleagues at Detrick, both by securing them continued funding and by facilitating their connections with the wider scientific community through open publication. This use of scientists' agency to open holes in the veil of secrecy makes intuitive sense to our inner

Merton: even if they were willing to participate in the secrecy system, it seems logical for these scientists to challenge it whenever they thought it warranted. Far less intuitive, however, were the positions these scientists took in *support* of more secrecy and increased public ignorance. Just as scientist-advisors like Ira Baldwin were pushing for a liberalized publications policy in the late 1940s, for instance, they were also seeking to shut down public discussion of biological warfare for fear of a blanket statute being imposed on microbiology like the Atomic Energy Act had on nuclear physics. To uphold the professional interests of their scientific community, they used secrecy as much as openness.

Chapter Structure

The body of this dissertation uses a series of case studies to illuminate these themes further. Chapter 1 begins at the end of the war, examining the role of scientists in advocating for and maintaining military-sponsored biological warfare research for the next quarter-century. It focuses in particular on Ira Baldwin's post-war career as a scientific 'advisor' for various government and military decision-makers, showing the heterogeneous roles he played as a mediator between the classified worlds of Detrick and the military, and the broader scientific community. Formally speaking, Baldwin's role changed radically in this time, from being an important member of the military decision-making process as a committee chairman of the Pentagon's Research and Development Board in the late 1940s to serving as an advisor of the US Army Chemical Corps serving at the pleasure of the Corps' commander after 1953. In practice, however, Baldwin's importance to the biological weapons program and his erstwhile military sponsors transcended this formal 'advisory' role, as he was an influential figure within bureaucratic struggles, and close confidant and informal representative of disaffected Detrick scientists. Even more importantly to the generals he worked with, he was a central figure of a network of civilian scientists who dubbed themselves 'friends of Detrick,' representing an important 'open' world constituency for leaders of secret military research. As a liminal figure, with far more agency and influence than his formal titles would suggest, Baldwin's career highlights the often-porous divide between the secret and 'open' worlds, across which people, knowledge, and political influence flowed.

Chapter 2 shifts from focusing on Baldwin and his network's relationship with military officials to examing their relationship with their fellow scientists in the Society of American Bacteriologists (SAB). The SAB was a professional home for microbiologists in the US, including those who had worked at Detrick during WWII and those who continued to research biological warfare during the Cold War, and the network of Detrick 'friends,' who often held important leadership roles in the SAB, were eager to maintain professional ties with their Detrick constituents. The SAB was also a potentially powerful political tool, lending these scientists authority in their relationship with the military when they could claim to represent it, but also threatening to give members who were critical of using their science for warfare a platform to challenge the bioweapons program. This chapter examines how Detrick 'friends' within the SAB used military secrecy as a rhetorical tool to clamp down on such dissent within their community, while still maintaining professional ties with their Detrick colleagues between the mid-1940s and the mid-1960s. It then concludes by examining how critics of biological weapons research successfully challenged this system in the late 1960s, using the SAB (by then renamed the American Society for Microbiology) as a platform for criticism that would contribute to the Nixon administration's renunciation of biological warfare research in 1969.

Chapter 3 turns from these communities surrounding the bioweapons program to Detrick itself, by examining how the practice of laboratory safety was transformed there. With one major exception, early safety practices at Detrick initially reflected those of microbiology more broadly, with an emphasis on individual researchers' skill and laboratory director's paternalistic good judgment. The exception stemmed from the imperative to preserve the secrecy of what was happening at Detrick by preventing germs from escaping the Camp. In Detrick's first months, a formal Safety Division was founded with the responsibility of containing germs in the name of containing secrets. Already representing a deviation from normal laboratory practice more akin to the professionalized safety organizations prevalent in industry, the postwar Safety Division began to further challenge the primacy of skill in safety with research on microbial aerosols in the laboratory. Under its postwar director Arnold Wedum, the Safety Division's initial ideal of containing germs within Detrick tightened to containing germs within circumscribed laboratory spaces using specialized safety equipment and transformed laboratory practice. By the 1960s, this containment ideal had transformed laboratory practice in the classified world of Detrick, assisted by the authority of a growing expert community that Wedum and his followers had built in the wider civilian world. 'Containment' of germs, originally part of the military secrecy system, had become one of the major ideas to escape that system.

Chapter 4 turns to examine the nature of that secrecy system itself. Drawing on the work of scholars like Wellerstein and Hilgartner, and their focus on secrecy as a communications-management system, this chapter examines three major facets of the secrecy system used by the bioweapons community. First, it examines how secrecy impacted Detrick scientists' links to the civilian scientific community, particularly their ability to publish their work. It then turns to examining the material realities of working within the secrecy system, focusing on the specific practical and material realities which a scientific advisor like Ira Baldwin lived with and how these changed over time. Finally, it examines the political uses of secrecy, from shielding bioweapons research from public scrutiny to serving as a rhetorical bludgeon against opponents of the Detrick program. In all three lines of inquiry, we can see the agency of the scientists who interacted with this secrecy system. They sometimes sought to limit the power of secrecy, as in the case of scientific publications, sometimes to avoid practices of secrecy that they found inconvenient or absurd, and sometimes even diverged from their military sponsors in supporting secrecy when doing so served the professional interests of their science. In short, the secrecy system itself, but also scientists' relationship with it, was contingent and complicated, challenging our preconceptions about neatly divided 'secret' and 'open' worlds, and scientists universally chafing under the former in their longing for the latter.

Secrecy allowed 'friends' of Detrick to dismiss would-be critics of biological weapons research as ill-informed. Chapter 5 follows some of these critics and examines the varying tactics they used to overcome this dilemma. For Theodor Rosebury (author of the 1942 report), secrecy needed to be carefully navigated. Emerging as a post-war critic of continued biological weapons research after working at Detrick during WWII, Rosebury pursued a strategy of public education with articles, talks, and a book on the topic. In all of these, he not only worked with openly available information, but had to affirmatively prove that such information, rather than his wartime experience, was his source. After being driven from this activism during the McCarthy era, he re-emerged as a critic in the 1960s, flouting Cold War norms of scientific 'objectivity' by instead making ethics-based arguments to lay audiences. At the same time, scientists of the transnational Pugwash movement focused on policy elites on either side of the Iron Curtain, using compiled open-source information and a rhetoric of what possible research might exist in the classified world to argue that biological weapons were militarily ineffective and destabilizing to the Cold War balance of terror. By the late 1960s, this focus on influencing elites dovetailed with rising public discontent that Rosebury's activism served, and both were ultimately instrumental in the US' renunciation of its biological weapons program.

The story of secrecy and biological weapons research in the mid-20th century is not only of historical interest. While offensive weapons research has not taken place at Detrick for over fifty years, terrorists, rogue states, and even Detrick's own employees have continued to confront the world with the threat of germs being used as weapons.[34] Advancing biological knowledge and decades-old weapons research alike continue to pose questions of how and when such knowledge should be controlled. The basic structure of the secrecy systems developed at the birth of the American national security

[34] Detrick microbiologist Bruce E. Ivins was the prime suspect in the anthrax letters attack of September-October 2001, which killed five people and sickened several more. Ivins committed suicide before his guilt or innocence could be established. See the Epilogue.

state in the 1940s, of which the one surrounding Detrick was an example, continues today in a form that first coalesced during the early 1950s.

Chapter 1: Ira Baldwin and the Social World of a Scientific Advisor

It was the summer of 1952, and Ira Baldwin was the bearer of bad news. Camp Detrick, the US Army Chemical Corps' decade-old research center for biological warfare, was in a morale crisis, and most of its senior scientific staff were actively considering resigning. "The situation at Detrick seems to be deteriorating very rapidly… I am afraid there will be resignations of several of the… key personnel very shortly since I know they are actively considering other positions," Baldwin wrote to Harold V. Gaskill, the Army's Chief Scientist, urging that the Detrick scientists be placated by giving bioweapons research greater autonomy within the Corps in accord with the recommendations of the previous year's Killian Committee.[35] Baldwin was well acquainted with the Chemical Corps and the management of Detrick, having served as its first scientific director during the Second World War, chaired a committee on biological warfare for the Pentagon's Research and Development Board since 1946, and having been a member of the Killian Committee itself the previous year. The Army would do well to listen to Baldwin's advice, as indeed they ultimately (if begrudgingly) did. Though Baldwin was only a consultant for the military, serving as an administrator at the University of Wisconsin for his main career, his long, deep, and varied experience with bioweapons research made him one of the most important figures, civilian or military, in the management of the Detrick program. Theodor Rosebury, a scientist critical of bioweapons research, recalled being known as "Mr. B.W." among the physicist-

[35] Ira L. Baldwin to Harold V. Gaskill, August 8, 1952 in University of Wisconsin Archives (UWA) Ira L. Baldwin Papers (Series 9/10/11), Box 14 Folder 8.

dominated Atomic Scientists' movement of the late 1940s.[36] Baldwin was as close as anyone came to being "Mr. B.W." within Defense Department circles.

Baldwin held another role through the 1952 crisis at Detrick however: that of scientific colleague and personal confidant of many of the scientists revolting at Detrick. He listened sympathetically to their frustrations with what they saw as heavy-handed military management of their research, counselled them to pursue their own personal and scientific well-being even if it clashed with the interests of the Detrick program, and helped to find them new academic jobs when those who were too fed up with Detrick quit. When Baldwin advised the Army that accepting the Killian report was necessary to triage the crisis at Detrick, he was giving his sincere opinion, but he was also in many ways acting as a kind of labor representative for Detrick's microbiologists. This is not to say that Baldwin disingenuously sought only to advance his colleagues' interests: he clashed with Detrick scientists in other instances where they tried to stray from what he considered proper avenues for research, and he tried to smooth over disputes between the military and other, more radical, scientist-advisors. In almost a quarter-century of government consulting, Baldwin was always cordial with disparate groups with disparate interests, from Department of Defense officials to Chemical Corps officers to Detrick researchers, fellow scientist-advisors, and other microbiologists unconnected to government work, but almost never showed full allegiance to any of them. He most closely adhered to a group of fellow microbiologists, some of whom served as government advisors, who were united by a vision of "BW" as a destructive and

[36] Theodor Rosebury to Edgar Z. Friedenberg, December 23, 1971 in National Library of Medicine (NLM) Theodor Rosebury Papers (MS C 634), Box 2 Folder 16 (Correspondence 'F').

dangerous form of warfare for which the United States had to prepare. He and his fellow-advisors enjoyed the access to advocate this position and the information to buttress it because of their willingness to participate in the secrecy system surrounding Detrick, but within this system they exercised more agency than their reliance on formal security clearances suggested.

The social world of Baldwin and this BW community was multifarious, challenging the neat dichotomous categories of “expert” versus “decision-maker” and the “objective” scientific voice versus “emotional” moral and political rhetoric which suffused the mentality of early Cold War America. They were advisors who consciously tried to direct policy, scientists who argued about the political implications of unproven ideas, and, indeed, academic civilians who found influence in military and government circles. So too does Baldwin’s experience challenge our own historiographic dichotomies, between “science” and “the military,” between “open” and “secret” scientific communities, and between morality tales of scientists duped (or perhaps corrupted) by the lavish support of the Cold War national security state and of scientists boldly resisting such forces. Baldwin’s community were ‘open’ scientists with one foot in a secret military world, but they were neither pawns or dupes. They attempted to negotiate and mediate the relationship between these worlds, seeking to draw their field closer to military concerns in some ways, while resisting what they saw as the dangers of too much entanglement with military secrecy in others. Their ultimate failure to square this circle was part of the undoing of the research program they supported, but it took over two decades and titanic shifts in the politics of American science for these contradictions to prove fatal. The story of Baldwin’s community during those two decades, from the mid-1940s to the late 1960s, is a crucial part of understanding the course of the biological weapons program they supported, but it also offers us a new window into the relationship between science and the American state in those crucial early Cold War years.

Science and the Military in the Early Cold War

Baldwin was far from the only American scientist who came out of the Second World War with an unexpected relationship with the military. Mobilizing scientists to develop military technologies had been a major element of the American war effort, and as the postwar period became the Cold War, continuing this wartime system of military-sponsored technological development seemed to many leaders to be a crucial element of the new conflict.[37] American science was to be one of the major foundations of state power, both through "applied" research intended to develop specific technologies, and through government-sponsored "basic" research to underlie future technological developments.[38] By the 1950s and 1960s, direct sponsorship by the military services and by civilian bodies equally integral to the incipient national security state (like the Atomic Energy Commission and CIA) joined funding from bodies like the National Science Foundation and National Institutes of Health to interject the Federal government into American science to an unprecedented degree.

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[37] See e.g. Michael J. Hogan, *A Cross of Iron: Harry S. Truman and the Origins of the National Security State, 1945-1954*, New York: Cambridge University Press, 1998; Aaron L. Friedberg, *In the Shadow of the Garrison State: America's Anti-Statism and Its Cold War Grand Strategy*, Princeton: Princeton University Press, 2000 (especially Chapter 8); Mary Ann Heiss and Michael J. Hogan (eds), *Origins of the National Security State and the Legacy of Harry S. Truman*, Kirksville, MO: Truman State University Press, 2015.
[38] See Mario Daniels and John Krige, "Beyond the Reach of Regulation?: "Basic" and "Applied" Research in the Early Cold War United States," *Technology and Culture* 59 no 2 (2018), pp 226-250 for a discussion of the political uses of this distinction.

Tracing and assessing the nature of these links between scientists and the national security state is a central theme of the historiography of Cold War science. In the 1980s, the impact of state sponsorship on the course of physics was of particular interest, reflecting the ongoing debate about the influence of social forces on the development of scientific knowledge.[39] The debate between historians of physics Paul Forman and Daniel Kevles is the most notable example of this question. Forman, in a 1987 article, traced the weight of military financial influence on American solid-state physics and argued that this financial patronage necessarily "militarized" the field, directing its research priorities and consequent development.[40] Opposing this viewpoint, Kevles responded by acknowledging the importance of government funding for physicists, while asserting that the development of their science was not meaningfully altered by these additional resources.[41] At the root of their disagreement was a quasi-moral question of perverted agency, which in retrospect reflected the anxiety of an apparent Reagan-era "Cold War II." Were physicists of the 1950s marionettes entangled in the strings of military patrons' priorities, or willing partners of the state and its resources in an untainted quest to do their physics? With the collapse of the Soviet Union this debate ended, not with a bang, but a historiographical whimper. The Cold War historiography of the 1990s took a revisionist and generally optimistic turn, with "now we know" reexaminations of taken-for-granted presumptions about Soviet intentions and discussion of how seeming subalterns in the Cold War world- peace organizations, nonaligned countries, dovish politicians on both

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[39] See e.g. Ian Hacking, "Weapons Research and the Form of Scientific Knowledge," *Canadian Journal of Philosophy* Supplementary Vol 12 (1986), pp 237-260

[40] Paul Forman, "Behind Quantum Electronics: National Security as Basis for Physical Research in the United States, 1940-1960," *Historical Studies in the Physical and Biological Sciences* 18 no 1 (1987), pp 149-229.

[41] Daniel Kevles, "Cold War and Hot Physics: Science, Security, and the American State, 1945-56," *Historical Studies in the Physical and Biological Sciences* 20 no 2 (1990), pp 239-264.

sides- had in fact been historical actors of great significance in ending the conflict.[42] Questions of military perversion of science seemed less urgent than in the Reagan years, and regardless of the particular truth of Forman's thesis, the idea that a social factor like military funding could affect the character of a science was less historiographical contentious besides.[43]

The subsequent 'second generation' historiography of Cold War American science has expanded the focus of these early debates in several major ways.[44] Reflecting the "Global Cold War" approach of Odd Arne Westad, some scholars have decentered (but not removed) the Cold War state by focusing on transnational flows of scientific knowledge both within alliances like NATO and between ideological rivals.[45] Other

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[42] See John Lewis Gaddis, *Now We Know: Rethinking Cold War History*, New York: Oxford University Press, 1997; Matthew Evangelista, *Unarmed Forces: The Transnational Movement to End the Cold War*, Ithaca: Cornell University Press, 1999.

[43] Notable exceptions to this trend had strong roots in the scholarly environment of the 1980s. See e.g. Stuart W. Leslie, *The Cold War and American Science: The Military-Industrial-Academic Complex at MIT and Stanford*, New York: Columbia University Press, 1993 (published shortly after the collapse of the Soviet Union) and Paul N. Edwards, *The Closed World: Computers and the Politics of Discourse in Cold War America*, Cambridge, MA: MIT Press, 1996 (based on Edwards' 1988 dissertation).

[44] See review discussion of "second generation" Cold War science historiography in Hunter Heyck and David Kaiser, "Focus: New Perspectives on Science and the Cold War, Introduction," *Isis* 101 no 2 (2010), pp 362-366 and Elena Aronova, "Review: Recent Trends in the Historiography of Science in the Cold War," *Historical Studies in the Natural Sciences* 47 no 4 (2017), pp 568-577.

[45] Odd Arne Westad, *The Global Cold War: Third World Interventions and the Making of Our Times*, Cambridge: Cambridge University Press, 2005; Gabrielle Hecht (ed), *Entangled Geographies: Empire and Technopolitics in the Global Cold War*, Cambridge, MA: MIT Press, 2011; Naomi Oreskes and John Krige (eds), *Science and Technology in the Global Cold War*, Cambridge, MA: MIT Press, 2014. See also John Krige, *American Hegemony and the Postwar Reconstruction of Science in Europe*, Cambridge, MA: MIT Press, 2006; John Krige, *Sharing Knowledge, Shaping Europe: US Technological Collaboration and Nonproliferation*, Cambridge, MA: MIT Press, 2016; Simone Turchetti, *Greening the Alliance: The Diplomacy of NATO's Science and Environmental Initiatives*, Chicago: University of Chicago Press, 2018. Notable examinations of cross-Iron Curtain biomedical exchange (in particular) include Nikolai Krementsov, "In the Shadow of the Bomb: U.S.-Soviet Biomedical Relations in the Early Cold War, 1944-1948," *Journal of Cold War Studies* 9 no 4 (2007), pp 41-67; Anna Geltzer, "In a Distorted Mirror: The Cold War and U.S.-Soviet Biomedical Cooperation and (Mis)understanding, 1956–1977," *Journal of Cold War Studies* 14 no 3 (2012), pp 39-63; Dóra Vargha, *Polio Across the Iron Curtain: Hungary's Cold War with an Epidemic*, New York: Cambridge University Press, 2018. Though not strictly focused on the Cold War *per se*, recent studies of global/transnational 20th century science perhaps inevitably have strong Cold War themes. See e.g. a special issue of *Osiris* entitled "Global Power Knowledge: Science and Technology in International Affairs," led by John Krige and Kai-Henrik Barth, "Introduction: Science, Technology, and International Affairs," *Osiris* 21 (2006), pp 1-21; Paul N. Edwards, *A Vast Machine: Computer Models,*

scholars have expanded on the physics-oriented work of the 1980s, examining military links with other physical sciences (most notably the earth sciences) and especially with American social sciences in the pursuit of a psychological "total Cold War" in the 1950s and '60s.[46] More generally, historians have tended to shy away from both the stark

Climate Data, and the Politics of Global Warming, Cambridge, MA: MIT Press, 2010; Simone Turchetti, Nestor Herran, and Soraya Boudia, "Introduction: Have We Ever Been 'Transnational'? Towards a History of Science Across and Beyond Borders," *British Journal for the History of Science* 45 no 3 (2012), pp 319-336; Patrick Manning and Mat Savelli (eds), *Global Transformations in the Life Sciences, 1945–1980*, Pittsburgh: University of Pittsburgh Press, 2018; John Krige (ed), *How Knowledge Moves: Writing the Transnational History of Science and Technology*, Chicago: University of Chicago Press, 2019.

46 The apt term "total Cold War" is Kenneth Osgood's, used to describe the Eisenhower administration's turn to "psychological warfare" as a non-overtly-military way of prosecuting the conflict with the Soviets. See Kenneth Osgood, *Total Cold War: Eisenhower's Secret Propaganda Battle at Home and Abroad*, Lawrence, KS: University of Kansas Press, 2006. Exemplars of the social sciences turn in the recent historiography of Cold War science include Mark Solovey, "Project Camelot and the 1960s Epistemological Revolution: Rethinking the Politics-Patronage-Social Science Nexus," *Social Studies of Science* 31 no 2 (2001), pp 171-206; Joy Rohde, "Gray Matters: Social Scientists, Military Patronage, and Democracy in the Cold War," *Journal of American History* 96 no 1 (2009), pp 99-122; David C. Engerman, "Social Science in the Cold War," *Isis* 101 no 2 (2010), pp 393-400; Janet Martin-Nielsen, "'This War for Men's Minds': The Birth of a Human Science in Cold War America," *History of the Human Sciences* 23 no 5 (2010), pp 131-155; Hamilton Cravens and Mark Solovey (eds), *Cold War Social Science: Knowledge Production, Liberal Democracy, and Human Nature* (London: Palgrave Macmillan, 2012); Joy Rohde, *Armed with Expertise: The Militarization of American Social Research During the Cold War*, Ithaca: Cornell University Press, 2013; Mark Solovey, *Shaky Foundations: The Politics-Patronage-Social Science Nexus in Cold War America* (New Brunswick: Rutgers University Press, 2013); Jamie Cohen-Cole, *The Open Mind: Cold War Politics and the Sciences of Human Nature*, Chicago: University of Chicago Press, 2014; David H. Price, *Cold War Anthropology: The CIA, the Pentagon, and the Growth of Dual Use Anthropology*, Durham, NC: Duke University Press, 2016. Ties between the military and earth scientists are explored by John Cloud, "Crossing the Olentangy River: The Figure of the Earth and the Military-Industrial-Academic Complex, 1947-1972," *Studies in the History and Philosophy of Modern Physics* 31 no 3 (2000), pp 371-404; John Cloud, "Imaging the World in a Barrel: CORONA and the Clandestine Convergence of the Earth Sciences," *Social Studies of Science* 31 no 2 (2001), pp 231-251; a 2003 special issue of *Social Studies of Science*, most notably Ronald E. Doel, "Constituting the Postwar Earth Sciences: The Military's Influence on the Environmental Sciences in the USA after 1945," *Social Studies of Science* 33 no 5 (2003), pp 635-666; Naomi Oreskes, "A Context of Motivation: US Navy Oceanographic Research and the Discovery of Sea-Floor Hydrothermal Vents," *Social Studies of Science* 33 no 5 (2003), pp 697-742; and Kai-Henrik Barth, "The Politics of Seismology: Nuclear Testing, Arms Control, and the Transformation of a Discipline," *Social Studies of Science* 33 no 5 (2003), pp 743-781; Jacob Darwin Hamblin, *Oceanographers and the Cold War: Disciples of Marine Science*, Seattle: University of Washington Press, 2005; Kristine C. Harper, *Weather by the Numbers: The Genesis of Modern Meteorology*, Cambridge, MA: MIT Press, 2008; James Fleming, *Fixing the Sky: The Checkered History of Weather and Climate Control*, New York: Columbia University Press, 2010; Simone Turchetti and Peder Roberts (eds), *The Surveillance Imperative: Geosciences During the Cold War and Beyond*, New York: Palgrave Macmillan, 2014; Ronald E. Doel, Kristine C. Harper, and Matthias Heymann (eds), *Exploring Greenland: Cold War Science and Technology on Ice*, New York: Palgrave Macmillan, 2016; Kristine C. Harper, *Make It Rain: State Control of the Atmosphere in Twentieth-Century America*, Chicago: University of Chicago Press, 2017; Naomi Oreskes, *Science on a Mission: How Military Funding Shaped What We Do and Don't Know about the Ocean*, Chicago: University of Chicago Press, 2021.

dichotomies and moral valence of the Forman-Kevles debate, emphasizing the agency of scientists and the contingency of the relationships they developed with the state during the early Cold War.[47]

Relatively little of this recent scholarship has focused on the relationship between biological scientists and the Cold War state; still less about direct linkages between biologists and the military like that surrounding the biological weapons program.[48]

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[47] Major examples include Rebecca S. Lowen, *Creating the Cold War University: The Transformation of Stanford*, Berkeley: University of California Press, 1997; Jessica Wang, *American Science in an Age of Anxiety: Scientists, Anticommunism, and the Cold War*, Chapel Hill: University of North Carolina Press, 1999; Allan A. Needell, *Science, Cold War, and the American State: Lloyd V. Berkner and the Balance of Professional Ideals*, London: Harwood Academic Press, 2000; Jennifer S. Light, *From Warfare to Welfare: Defense Intellectuals and Urban Problems in Cold War America*, Baltimore: Johns Hopkins University Press, 2003; Atsushi Akera, *Calculating a Natural World: Scientists, Engineers, and Computers During the Rise of U.S. Cold War Research*, Cambridge, MA: MIT Press, 2006; Thomas C. Lassman, *Edward Condon's Cooperative Vision: Science, Industry, and Innovation in Modern America*, Pittsburgh: University of Pittsburgh Press, 2018. The historiography of Cold War science has increasingly focused on the 1970s in recent years, with David Kaiser and W. Patrick McCray, editors of an influential edited collection entitled *Groovy Science*, advancing the term to describe American science in the 1970s as a corrective to simplistic narratives of the 1960s counterculture having done irreparable damage to the cultural cachet of science. Science-state relations, and the influence of the ongoing Cold War on American science, they argue, were certainly altered by the tumult of the 1960s, but substantial continuities remained. See David Kaiser and W. Patrick McCray (eds), *Groovy Science: Knowledge, Innovation, and American Counterculture*, Chicago: University of Chicago Press, 2016. Also see David Kaiser, *How the Hippies Saved Physics: Science, Counterculture and the Quantum Revival*, New York: W. W. Norton & Co, 2011; W. Patrick McCray, *The Visioneers: How a Group of Elite Scientists Pursued Space Colonies, Nanotechnologies, and a Limitless Future*, Princeton: Princeton University Press, 2013; and a special issue of *Centaurus* entitled "Spotlight on: 1970s: Turn of an Era in the History of Science?," led by Matthias Heymann, "Introduction to Spotlight on 1970s: Turn of an Era in the History of Science?," *Centaurus* 59 no 1-2 (2017), pp 1-9. A notable counterexample of a 'second generation' work focused on military control of science is Peter Westwick, *The National Labs: Science in an American System, 1947-1974*, Cambridge, MA: Harvard University Press, 2003, which studies the Atomic Energy Commission's National Laboratory system (the holy of holies of early Cold War military-sponsored science), and engages heavily with the impact of military secrecy on scientists in the system. Indeed, Westwick argues that military sponsorship effectively split the Laboratories' scientists into a scientific community operating in parallel to the 'open' one, complete with a system of classified journals and conferences. Nonetheless, Westwick also emphasizes the contingent and contested formation and development of the Laboratory system, and the often troublesome agency of scientists working within the system.

[48] Examples in the historiography of biology with Cold War themes include Toby A. Appel, *Shaping Biology: The National Science Foundation and American Biological Research, 1945-1975*, Baltimore: Johns Hopkins University Press, 2000; Karen A. Rader, "Alexander Hollaender's Postwar Vision for Biology: Oak Ridge and Beyond," *Journal of the History of Biology* 39 no 4 (2006), pp 685-706; a 2012 special issue of the *Journal of the History of Biology* entitled "The Lysenko Controversy and the Cold War," led by William deJong-Lambert and Nikolai Krementsov, "On Labels and Issues: The Lysenko Controversy and the Cold War," *Journal of the History of Biology* 45 no 3 (2012), pp 373-388 and Audra J. Wolfe, "The Cold War Context of the Golden Jubilee, or, Why We Think of Mendel as the Father of Genetics," *Journal of the History of Biology* 45 no 3 (2012), pp 389-414; Angela N. H. Creager, *Life*

Nonetheless, three areas of recent scholarship are useful to understand Baldwin's role as a military advisor. Sheila Jasanoff and Sang-Hyun Kim's concept of "sociotechnical imaginaries" is a fruitful way to think about the idea of "biological warfare" that motivated the alliance between Baldwin and the Chemical Corps in the first place.[49] Like the visions of the future discussed by Jasanoff and Kim, the idea of using microbes as weapons was a programmatic vision of technological development and a social order that would accompany it around which Baldwin's community of scientist-experts and military experts coalesced. The knowledge-claims of such experts, in turn, needed to defend this vision within higher-level military bureaucracies. Work led by Lynn Eden on "organizational frames" within American nuclear weapons targeting highlights how bureaucratic organizations like the Cold War military could be predisposed against heterodox knowledge claims like those of fire experts, computer scientists (who Rebecca Slayton argues did not make "arguments that count[ed]" as much as physicists' in such an environment), or indeed biologists.[50] In a bureaucratic environment oriented toward using

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Atomic: A History of Radioisotopes in Science and Medicine, Chicago: University of Chicago Press, 2013; Warwick Anderson, "Nowhere to Run, Rabbit: The Cold-War Calculus of Disease Ecology," *History and Philosophy of the Life Sciences* 39 no 2 (2017), pp 1-18; David P. D. Munns, *Engineering the Environment: Phytotrons and the Quest for Climate Control in the Cold War*. Pittsburgh: University of Pittsburgh Press, 2017; Joanna Radin, *Life on Ice: A History of New Uses for Cold Blood*, Chicago: University of Chicago Press, 2017; Manning and Savelli (eds), *Global Transformations in the Life Sciences*. See also histories of Second World War and Cold War biological and environmental sciences that intersect with chemical and biological warfare, e.g. Roy MacLeod, "'Strictly for the Birds': Science, the Military, and the Smithsonian's Pacific Ocean Biological Survey Program, 1963-1970" *Journal of the History of Biology* 34 no 2 (2001), pp 315-352; Nicolas Rasmussen, "Plant Hormones in War and Peace: Science, Industry, and Government in the Development of Herbicides in 1940s America," *Isis* 92 no 2 (2001), pp 291-316; Audra J. Wolfe, "Germs in Space: Joshua Lederberg, Exobiology, and the Public Imagination," *Isis* 93 no 2 (2002), pp 183-205; David Zierler, *The Invention of Ecocide: Agent Orange, Vietnam, and the Scientists who Changed the Way We Think about the Environment*, Athens, GA: University of Georgia Press, 2011; Jacob Darwin Hamblin, *Arming Mother Nature: The Birth of Catastrophic Environmentalism*, New York: Oxford University Press, 2013.

[49] Sheila Jasanoff and Sang-Hyun Kim (eds), *Dreamscapes of Modernity: Sociotechnical Imaginaries and the Fabrication of Power*, Chicago: University of Chicago Press, 2015.

[50] Lynn Eden, *Whole World on Fire: Organizations, Knowledge, and Nuclear Weapons Damage*, Ithaca: Cornell University Press, 2004; Rebecca Slayton, *Arguments that Count: Physics, Computing, and Missile Defense, 1949-2012*, Cambridge, MA: MIT Press, 2013. Frank L. Smith III, *American Biodefense: How*

high explosives as weapons, the sociotechnical imaginary of germs as weapons faced an uphill battle to enlist supporters. Ostensibly, this was not Baldwin's job as an advisor. It was a mantra in early Cold War science advising that scientists should "be on tap, not on top," available to lend their expertise for political and military decision-making but, as 'objective' advisors, never exercising decision-making power of their own.[51] The ideal, put in other words, was a simplified model of 'patronage,' with scientists simply offering advice to their political masters, a viewpoint tacitly adopted by historiography focused on scientists' subjection to the state. The reality that an 'advisor' like Baldwin experienced was more complex and contingent, however, as he acted as a political force in his own right among both military officials and microbiologists. Historian of physics Benjamin Wilson's work on the "social world" of contemporaneous scientist-consultants is extremely helpful for understanding Baldwin's role.[52] Wilson traces interpersonal, institutional, and intellectual links between the classified world of high-energy laser research and the burgeoning field of nonlinear optics in the early 1960s, showing how the lived realities of physicists' social world made these ties far more porous than formal divisions between 'secret' and 'open,' or 'advisor' and 'decision-maker' might suggest. We can see a similar porousness in Baldwin's social world.

Dangerous Ideas about Biological Weapons Shape National Security, Ithaca: Cornell University Press, 2014 relies upon similar ideas to explain the failure of military officials to respond to biological warfare threats, particularly in the first Gulf War, but does not provide much discussion of early Cold War scientists' role.

[51] See e.g. Zuoyue Wang, *In Sputnik's Shadow: The President's Science Advisory Committee and Cold War America*, New Brunswick: Rutgers University Press, 2008 for a discussion of the 'on tap, not on top' ideology of scientists in policy-making and its implications.

[52] Benjamin Wilson, "The Consultants: Nonlinear Optics and the Social World of Cold War Science," *Historical Studies in the Natural Sciences* 45 no 5 (2015), pp 758-804.

The Polymorphous Career of a Scientific Advisor

At the beginning of 1945 Ira Baldwin was the Scientific Director of Camp Detrick, having spent the past year and a half organizing the camp and the research being conducted there. What had been a sleepy National Guard airstrip two years before had been transformed into one of the largest microbiology research centers in the United States, employing and housing hundreds of researchers pursuing dozens of projects all devoted to developing germs into "biological weapons." A protégé of the University of Wisconsin's E. B. Fred and exemplar of Wisconsin's strength in the study and manipulation of the "productive microbes" of soil and industrial bacteriology, Baldwin was preparing to leave Detrick, which was still bustling with new construction and active projects, to return home to Wisconsin.[53] Detrick seemed to him to be firmly established, and University of Wisconsin administrators were growing restless with the duration for which their professors were being 'loaned' to the government. Heeding this pressure, Baldwin began to arrange to turn the administration of research at Detrick (which itself had recently shifted from the authority of the quasi-civilian War Research Service to the Army's Chemical Warfare Service (CWS)) over to his deputy, Ohio State University microbiologist Oram Woolpert.[54] Baldwin had been more administrator than research scientist for the past decade, having served as Associate Dean of Wisconsin's College of Agriculture from 1932, perennial secretary-treasurer and one-time president of the Society of American Bacteriologists (SAB) from the late 1930s, and of course

[53] "Productive microbes" as an object of study (as opposed to the pathogens of medical microbiology) are a major topic of Eric D. Kupferberg, "The Expertise of Germs: Practice, Language and Authority in American Bacteriology, 1899-1924," PhD diss., MIT, (2001).
[54] See e.g. H. T. Herrick to I. L. Baldwin, November 9, 1944, in UWA Baldwin Papers, Box 11 Folder 2.

administrator of Detrick.[55] In returning to Wisconsin, he cemented this career shift into administration by accepting the Deanship of the Graduate School. No sooner had he begun to settle into his Deanship when Fred, newly appointed President of the university, created a new position of Vice President of Academic Affairs in anticipation of a post-war student influx, and recruited Baldwin to fill it.[56] From organizing the BW program to running the University of Wisconsin, then, Baldwin had by 1946 followed Fred firmly out of research science and into the burgeoning field of academic administration. He would remain in various high-level administrative roles at Wisconsin until his retirement in the late 1960s.

Though Baldwin was back at Wisconsin, however, his ties to weapons research had only just begun. He remained an active participant in the next year of wartime research at Detrick, frequently visiting as a civilian 'consultant' throughout 1945 while settling into his new role at Wisconsin. As a 'consultant' he was also able to shape the management of new projects growing out of the Detrick program, especially the construction of a large-scale anthrax production plant outside of Terre Haute, Indiana, which was based on Detrick pilot plant designs and parallel work on penicillin production.[57] With the end of the war, these consulting trips slowed then halted as the feverish pace of work at Detrick itself slowed to a halt, facilities like the never-used Terre Haute "Vigo" plant were prepared for sale, and most of the hundreds of scientists who had come to Detrick for emergency wartime work returned to their home institutions and regular lines of research. In the postwar world, Baldwin soon acquired a new role:

[55] Ira L. Baldwin, *My Half-Century at the University of Wisconsin: Adapted from an Oral History Interview by Donna Taylor Hartshorne* (Madison, WI: Privately Printed by Ira L. Baldwin, 1995), pp 545-548.
[56] Ibid, p 181.
[57] See e.g. Baldwin to Porter June 7, 1945 in UWA Baldwin Papers, Box 11 Folder 2.

scientific advisor to military and civilian policy makers. He served in a variety of roles over the two decades remaining in his career, reflecting in microcosm the newfound importance of scientific advice in military policy.

After a brief flirtation with advising the CWS, Baldwin's first major role was as the chairman of a committee on biological warfare, known as "Committee X" to preserve secrecy, of the newly-formed Joint Research and Development Board (JRDB).[58] Staffed primarily by part-time civilian scientific advisors like Baldwin, JRDB was founded to evaluate the feasibility and budget priorities of new technological projects like guided rockets, electronics, and biological warfare for the military, in principle answering through its chairman to the Joint Chiefs of Staff.[59] As this arrangement proved unworkable, the JRDB was reorganized by the 1947 National Security Act as the Research and Development Board (RDB), with much the same organization of subject-specific committees, but now operating on a statutory basis and answering to the new Secretary of Defense. Baldwin directed Committee X through all of these changes, from its organization until the RDB's demise in 1953. Under his guidance, his committee sought to establish biological warfare as feasible and dangerous, meriting increased research priority, producing reports led by the 1948 "Baldwin Report" which sought to be canonical within decision-making circles. Though by no means always effective, the lobbying of Baldwin and his committee contributed to the continuation of Detrick and its

[58] Baldwin served on the CWS Advisory Board for about a year. See Porter to Baldwin, March 10, 1945 in UWA Baldwin Papers, Box 11 Folder 2.
[59] See Joint Research and Development Board, Office Order #13 (Revision 1), May 2, 1947 in UWA Baldwin Papers, Box 15 Folder 1.

research program on a permanent basis in the late 1940s, and to a large influx of funding for bioweapons research with the outbreak of the Korean War.

Baldwin was recruited to chair Committee X by the JRDB's founder and first chairman Vannevar Bush, former head of the OSRD and effective face of government-sponsored science. With the end of WWII, the Office of Scientific Research and Development (OSRD) wound down, and fully disbanded in 1947, but Bush's vision of a centrally directed Federal role in scientific and technological research that it entailed remained.[60] His 1945 report *Science: The Endless Frontier* influentially argued for a continued civilian science funding organization, and was followed by years of Congressional debate which would eventually result in the 1950 establishment of the National Science Foundation.[61] Many of the arguments used for such an organization were based on national security concerns, with Bush, drawing on a distinction between "pure" (or "basic") and "applied" scientific research, arguing that support for basic

[60] See the postwar quasi-official history, Irvin Stewart, *Organizing Scientific Research for War: The Administrative History of the Office of Scientific Research and Development*, Boston: Little, Brown, 1948.
[61] Bush's position, embodied in Congress by the Magnuson Bill and focusing on control of scientific funding by scientific experts, was a conservative response to the Kilgore Bill, which embodied more fully the New Deal liberal tradition by providing for more politically-accountable and geographically distributed funding decisions. Both bills served to block one another, and by the time five years of Congressional maneuvering had finally let a NSF bill through (which more closely corresponded to Bush's vision than Kilgore's), the original vision of a centralized science funding agency was a dead letter in the face of already-large research budgets from institutions like the military services, the Atomic Energy Commission, and the National Institute of Health. Some major works studying this episode include Daniel J. Kevles, "The National Science Foundation and the Debate over Postwar Research Policy, 1942-1945," *Isis* 68 no 1 (1977) pp 5-26; Nathan Reingold, "Vannevar Bush's New Deal for Research: Or, The Triumph of the Old Order," *Historical Studies in the Physical and Biological Sciences* 17 no 2 (1987), pp 299–344; Daniel Lee Kleinman, "Layers of Interests, Layers of Influence: Business and the Genesis of the National Science Foundation," *Science, Technology, & Human Values* 19 no 3 (1994), pp 259-282; Jessica Wang, "Liberals, the Progressive Left, and the Political Economy of Postwar American Science: The National Science Foundation Debate Revisited," *Historical Studies in the Physical and Biological Sciences* 26 no 1 (1995), pp 139-166; Toby A. Appel, *Shaping Biology: The National Science Foundation and American Biological Research, 1945-1975*, Baltimore: Johns Hopkins University Press, 2000; Mark Solovey, "Riding Natural Scientists' Coattails onto the Endless Frontier: The SSRC and the Quest for Scientific Legitimacy," *History of the Behavioral Sciences* 40 no 4 (2004), pp 393-422; Michael Aaron Dennis, "Reconstructing Sociotechnical Order: Vannevar Bush and US Science Policy," in Sheila Jasanoff (ed), *States of Knowledge: The Co-Production of Science and the Social Order*, London: Routledge (2004), pp 225-253.

research was a necessary predicate for applied results, and with it national power, in the future. Nonetheless, this vision of long-term Federal support for (largely university-based) science was necessarily a more expansive one than the short term military-oriented work of the OSRD. In a post-Hiroshima age in which a rhetoric of the technological basis of military power held strong currency in Washington, the military therefore continued to directly support research programs of its own, including explicitly "basic" research sponsored by the Office of Naval Research as rival NSF bills foundered in Congress.[62]

In an immediate postwar period in which declining budgets (though still high by pre-war standards) and calls for increased integration of the often-rival military services were the order of the day in Washington, Bush sought to centralize and rationalize this military research as well. Through political maneuvering in the spring of 1946, Bush defeated military plans for a research management organization run by the services themselves, and instead helped found a Joint Research and Development Board (JRDB) based on the OSRD model of placing civilian scientists in positions of authority. While the Army and Navy (soon to be joined by an independent Air Force) would retain their own research budgets, the JRDB, in Bush's vision, would serve as a coordinating guide for this research, selecting projects for prioritization and others for elimination as wasted or duplicative effort.[63] Michael Aaron Dennis has argued that Bush's ultimate goal

[62] Silvan S. Schweber, "The Mutual Embrace of Science and the Military: ONR and the Growth of Physics in the United States after World War II," in Everett Mendelsohn, Merritt Roe Smith, and Peter Weingart (eds), *Science, Technology and the Military*, Dordrecht, Netherlands: Kluwer Academic Press, 1988, pp 3-46; Harvey M. Sapolsky, *Science and the Navy: The History of the Office of Naval Research*, Princeton: Princeton University Press, 1990.

[63] Elliott V. Converse III, *Rearming for the Cold War, 1945-1960 (History of Acquisition in the Department of Defense Volume 1)*, Washington, DC: Office of the Secretary of Defense Historical Office, 2012, pp 22-25. For more discussion of debates about the institutional future of American science contemporaneous

(ironically, in light of how anticommunism increasingly colored all of these military considerations) was a body which could articulate a 'five year plan' for military research and development, prioritizing the supposedly objective judgment of scientists over the parochial interests of generals, admirals, and politicians.[64]

The JRDB was accordingly organized like the OSRD as a system of quasi-independent committees organized around a particular subject (like guided missiles and electronics), staffed by scientists with relevant expertise.[65] Though officers from the military services also served on these committees, their influential agenda-setting chairmen were also civilian scientists, who in turn answered to Bush and a small central staff. This allowed the newly enlarged post-1945 state to access expertise from scientists who, as Baldwin put it in 1950, "would be impossible to secure… as full-time employees."[66] In principle, the committees would assess possibilities and priorities within their particular field of research, and conferences of the committee chairmen as a whole could then recommend priorities for military research for Bush to deliver to the Joint Chiefs of Staff. In practice, the Board was faced from the earliest days with a tension between the ancillary advisory and centralized planning roles ascribed to it which would dog it throughout its existence. Was the JRDB's advice just that (thus freely ignorable),

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with the NSF debate and formation of the JRDB, see Daniel J. Kevles, "Scientists, the Military, and the Control of Postwar Defense Research: The Case of the Research Board for National Security, 1944-46," *Technology and Culture* 16 no 1 (1975), pp 20-47; Nathan Reingold, "Choosing the Future: The U.S. Research Community, 1944-1946," *Historical Studies in the Physical and Biological Sciences* 25 no 2 (1995), pp 301-328; Daniel Lee Kleinman, *Politics on the Endless Frontier: Postwar Research Policy in the United States*, Durham, NC: Duke University Press, 1995.

[64] Michael Aaron Dennis, "Our Monsters, Ourselves: Reimagining the Problem of Knowledge in Cold War America," in Sheila Jasanoff and Sang-Hyun Kim (eds), *Dreamscapes of Modernity: Sociotechnical Imaginaries and the Fabrication of Power*, Chicago: University of Chicago Press, 2015, pp 56-78, 71.

[65] A JRDB organization chart from October 4, 1946 can be found in UWA Baldwin Papers, Box 15 Folder 3.

[66] Baldwin to Robert E. Wilson, January 11, 1950, in UWA Baldwin Papers, Box 15 Folder 10.

or did it carry more serious formal or informal authority? Its position was unclear within a postwar military structure in which joint collaboration in the name of fiscal and military efficiency was the watchword of the day but in which, in practice, jealous services were eager to return to their prewar rivalries. Moreover, the position and the authority of the JRDB was unclear relative to traditional research and development institutions within the services themselves, like the Army's Ordinance Board. These institutions, staffed by uniformed military officers, quietly resisted any exertion of the JRDB's nominal authority over their research budgets.[67] The connection between the committees' findings and actual research was thus sometimes inconsistent, sometimes contested, and always ambiguous. However, as Dennis notes, the epistemic authority of scientists gathered in particular committees could influence new fields beyond their formal bureaucratic power, by establishing standard lexicons and understandings of what was scientifically and technologically possible (for example, examining the so-called 90-minute pendulum problem upon which the feasibility of inertial guidance for rockets rested).[68] JRDB committees thus held a great deal of potential power to shape the sociotechnological imaginaries surrounding novel technologies both within and outside of the military-technologies like guided rockets, nuclear-powered submarines, and biological weapons.

[67] See Thomas C. Lassman, "Putting the Military Back into the History of the Military-Industrial Complex: The Management of Technological Innovation in the U.S. Army, 1945-1960," *Isis* 106 no 1 (2015), pp 94-120, for an examination of the Army's traditional arsenal system, including the Ordinance Board, during this period. See especially Ibid, pp 103-106 for an examination of the antagonistic relationship between the Ordinance Board and the JRDB.

[68] Dennis, "Our Monsters, Ourselves," in Jasanoff and Kim (eds), *Dreamscapes of Modernity*, pp 71-72. On the development of inertial guidance systems, see Donald MacKenzie, *Inventing Accuracy: A Historical Sociology of Nuclear Missile Guidance*, Cambridge, MA: MIT Press, 1990. John Cloud briefly argues that RDB deliberations may be read as a "Rosetta stone" mapping out the course of subsequent military-funded transformations in the geophysical sciences. See John Cloud, "Introduction: Special Guest-Edited Issue on the Earth Sciences in the Cold War," *Social Studies of Science* 33 no 5 (2003), pp 629-633.

“Committee X” was one of the founding committees of the old JRDB, and it existed in virtually the same form until the dissolution of the RDB. Baldwin chaired it through all this period (with the exception of a few months at the end), making its informal sobriquet as the “Baldwin committee” particularly apt. Having been recruited to chair the committee by Bush in fall 1946 as the JRDB was being organized, Baldwin drew upon his personal networks from wartime administration and from the SAB to recruit other members of the committee. Alongside military representatives sent to him by the Army and Navy, he recruited two colleagues with backgrounds in human and veterinary medicine: the University of Pennsylvania’s Raymond Kelser, retired wartime chief of the Army Veterinary Corps and Kenneth Maxcy, head of Johns Hopkins’ Epidemiology Department, who had served in the Army Epidemiological Board during the war.[69] While neither had been directly involved with biological weapons research, Baldwin had dealt extensively with them and their organizations to plan defenses against biological attack while serving as Detrick’s Scientific Director. With this central committee established by early 1947, Baldwin then set about recruiting staff for panels on specific aspects of the sprawling topic of “biological warfare” (human diseases, animal diseases, and the like), which would do much of the actual work of writing assessments of possibility.[70] Five such panels had been established by May 1947, with several more

[69] Vannevar Bush to Baldwin, October 8, 1946, in UWA Baldwin Papers, Box 15 Folder 3. See Kelser and Maxcy’s NAS biographical memoirs: Richard E. Shope, “Raymond Alexander Kelser, 1892-1952,” *Biographical Memoirs of the National Academy of Sciences* (BMNAS) 28 (1954), pp 199-221; W. Barry Wood, Jr. and Mary Lee Wood, “Kenneth Fuller Maxcy, 1889-1966,” *BMNAS* 42 (1971), pp 161-173.
[70] In 1947, the “Panel on Man,” which focused on human pathogens, was dominated by physicians and epidemiologists, consisting of Maxcy, former Army Medical Corps General George Callender, Case Western Reserve Professor John Dingle (who had chaired the Army Epidemiological Board’s Commission on Acute Respiratory Diseases during the war), virologist John Enders (a future Nobel Prize winner), the University of Michigan’s Walter Nungester, and new Johns Hopkins epidemiologist Alexander Langmuir, who had worked with Dingle during the war. See Langmuir to Cole, June 27, 1947, p 3 in UWA Baldwin Papers Box 15 Folder 1.

being considered.[71] Over the next few months their part-time members began to meet, sometimes consulting directly with the remaining staff at Detrick, producing reports for Baldwin's central committee to be used in turn to write reports to Bush's central JRDB staff. (Panel reports were explicitly treated as internal-to-the-committee documents, not to be shared outside of it unless the JCS explicitly requested them, which served to further strengthen the hegemonic power of committee chairs like Baldwin by making their reports the sole documents to regularly come out of their committees). Close consultations with Detrick, like several multi-day tours of Detrick by Maxcy's "Panel on Man," highlighted what an administrative fiction the "objective" assessment of Committee X could be: not only were members of a close-knit field "assessing" the potential of their colleagues' work, they sometimes explicitly drew on their colleagues' expertise to do so.[72]

By late 1947, Baldwin's central committee set about compiling reports on the military potential of biological warfare from these panel assessments. The most noteworthy of these, a late 1947 "Appraisal of the Technical Aspects of Biological Warfare" and a 1948 "Report on Special BW Operations" (known informally as the "Baldwin Report"), were the first major documents purporting to establish the identity of "biological weapons" within the postwar government. The 1947 report did so only in the

[71] A May 2, 1947 directory of JRDB committees and panels lists 5 panels within Committee X, unhelpfully supplying code names (XTA, XOM, XOA, XOP, and XOI) without titles in keeping with the secrecy of Committee X's very name. See Joint Research and Development Board, Office Order #13, Revision 1, May 2, 1947, p 4, in UWA Baldwin Papers Box 15 Folder 1. Later in 1947, Baldwin's Wisconsin colleague William Sarles penned a handwritten list of recommended names for 7 different topic-specific panels of Committee X. This list was clearly speculative (it lists Columbia's Theodor Rosebury, for instance, who after May 1947 would be *personna non grata* in Baldwin's circle and who certainly never served on the JRDB), so it is unclear if all 7 of these panels were ever officially established. See Sarles, "Suggestions for Panels" (ms), n.d. in UWA Baldwin Papers, Box 15 Folder 1.
[72] The Panel on Man met at Detrick June 16-21, 1947, and planned to do so again July 16-17. See Alexander Langmuir to Cole, June 27, 1947 in UWA Baldwin Papers, Box 15 Folder 1.

most general terms, using a promissory rhetoric of potential and possibility. "Theoretically," it asserted, "by the choice of the proper disease agents and conditions for their application, it should be possible to destroy an enemy population by incapacitating a significant number of its members or by destroying its food supply," and while "no biological agent has been developed to a point where it has all [of a list of desirable] characteristics… a number of agents appear to be suitable for specific purposes."[73] Putting this theory into practice was easier said than done, and would involve substantial time, money, and most importantly, data that military planners could rely upon. "Such agents as anthrax spores, botulinum toxin, and *Brucella*," (which the wartime program had focused on) could "probably could be ready for production and large-scale trials within two years," if the Committee's budgetary wish list was fulfilled.[74]

With the incorporation of the still-uncooperative military services into a unified National Military Establishment by the National Security Act of 1947 (renamed the Department of Defense when the Act was amended in 1949), the JRDB was reorganized as the Research and Development Board (RDB).[75] This new RDB was now part of the civilian staff of the Department of Defense, answering to the Secretary of Defense rather than the Joint Chiefs of Staff, a strengthened position which was in turn further buttressed by having a statutory basis which the JRDB had lacked. Despite this change in

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[73] JRDB Committee on Biological Warfare, "Report on the Appraisal of the Technical Aspects of Biological Warfare," Aug 26, 1947, p 5, retrieved from Brill Online, "Primary Source Collection: Weapons of Mass Destruction," https://primarysources.brillonline.com/browse/weapons-of-mass-destruction.
[74] Ibid, p 10.
[75] The National Military Establishment, Research and Development Board, "Research and Development Board: History and Functions," Washington, DC: Government Printing Office, 1948. The 1949 amendment ostensibly revamped the authority of the RDB, but this seems to have had no real impact. See [Secretary of Defense] Louis Johnson, Memorandum for Doctor Karl T. Compton, Chairman, Research and Development Board, September 9, 1949, in UWA Baldwin Papers, Box 15 Folder 4, which discusses the ostensible implications of the 1949 act.

bureaucratic position, the internal organization of the RDB remained essentially the same as the JRDB, still staffed by part-time civilian advisors and based on federated subject-based committees, many of which (like Committee X) remained virtually unchanged through the transition. Ultimately, so too did the contradictions and ambiguities faced by the Board. The essential tension remained: was it an advisory body, or one with authority over research budgets? Committee chairs like Baldwin certainly tried to fill both roles, producing reports about what lines of research were possible and desirable, while also negotiating with other committee chairs about the priority ranking of projects in their subject area for a document which would be delivered to the Secretary of Defense. In practice, however, the military services remained jealous and often-successful guardians of their interests, better-staffed than the Office of the Secretary of Defense.[76]

In an attempt to make the new RDB more than just a forum for inter-service compromise, Bush began to argue for a new body to provide scientific advice to the military. In theory, the delineation of responsibility between the RDB and the military services, represented by the Joint Chiefs of Staff (JCS) under the 1947 National Security Act was clear: the RDB was to provide policies for military R&D, in consultation with the JCS and the priorities they set, leaving the direct management of research programs to the responsible services. In practice, however, the delineation of authority between the RDB and JCS was confused, and the two often worked at cross-purposes. Bush's proposal, for a body that would become known as the Weapon Systems Evaluation Group (WSEG) amounted to a power grab, as it would undertake the JCS' task of setting strategic priorities for R&D to translate into policy, answering to the chairman of the

[76] Hogan, *A Cross of Iron*, pp 229-234; Friedberg, *In the Shadow of the Garrison State*, pp 316-317.

RDB itself.[77] Inevitably, the JCS resisted this move, and this time, Bush was out-maneuvered. The WSEG that was eventually founded in late 1948 was to answer principally to the JCS, thus actually creating a powerful bureaucratic rival to the RDB. The WSEG was modeled on the Air Force's new RAND Corporation, designed to serve as an in-house 'think tank' staffed by full-time employees.[78] Unlike RAND, the WSEG was to be an explicit part of the Pentagon structure rather than an ostensibly independent contractor, partially staffed by uniformed military officers and directly answering to both the JCS (the highest-ranking uniformed military leadership) and to the Office of the Secretary of Defense (the personal staff of the Secretary of Defense, thus bypassing other civilian offices within the Pentagon like the Department of the Army). While in theory these two masters, military and civilian, presaged potential contradiction, in practice the JCS tended to be the most active managers of the WSEG, and the OSD tended to defer to them. The WSEG, effectively directly controlled by the military services, was thus

[77] The WSEG has been largely neglected by historians, probably reflecting the high degree of classification surrounding its activities and reports. The principal source available on the group is an Institute of Defense Analyses (IDA) report touting the efficacy of the group authored in the late 1970s: John Ponturo, "Analytical Support for the Joint Chiefs of Staff: The WSEG Experience, 1948-1976 (IDA Study S-507)," Arlington, VA: Institute for Defense Analyses, 1979. This document, produced by the WSEG's former sponsor, is quite partisan in its favorable interpretation of the WSEG's usefulness and objectivity, but its detailed narrative, drawing on otherwise classified WSEG documents, remains one of the most useful unclassified sources for information on the body available, including summaries of the conclusions of most of its reports. One of the WSEG's best-known employees was Hugh Everett III, developer of the Everett or 'many-worlds' interpretation of quantum physics. See Peter Byrne, *The Many Worlds of Hugh Everett III: Multiple Universes, Mutual Assured Destruction, and the Meltdown of a Nuclear Family*, New York: Oxford University Press, 2010. IDA is better known to historians of Cold War science as the sponsor of the JASON group of part-time scientific advisors. See Ann Finkbeiner, *The Jasons: The Secret History of Science's Postwar Elite*, New York: Penguin, 2006.

[78] The RAND Corporation looms large in Cold War historiography, particularly for its nuclear war planning and social science research. Some major scholarship on the organization itself includes Fred Kaplan, *The Wizards of Armageddon*, Stanford: Stanford University Press, 1983; David Hounshell, "The Cold War, RAND, and the Generation of Knowledge, 1946-1962," *Historical Studies of the Physical and Biological Sciences* 27 no 2 (1997), pp 237-267; Martin J. Collins, *Cold War Laboratory: RAND, the Air Force, and the American State, 1945-1950*, Washington, D.C.: Smithsonian Institution Press, 2002; Janet Farrell Brodie, "Learning Secrecy in the Cold War: The RAND Corporation," *Diplomatic History* 35 no 4 (2011), pp 643-670.

exactly the advisory group that Bush had sought to avoid when maneuvering for the creation of the JRDB in the first place.[79]

By 1948 Bush had resigned from the leadership of the RDB, further weakening its political position, as the military services proposed their own research and development budgets for fiscal year 1950 without consulting the RDB.[80] The Board remained active through the remainder of the Truman administration, diligently producing reports and recommendations, but in the absence of binding administrative authority, only the former had any real influence. With the advent of the Korean War, expanding defense budgets somewhat lowered the stakes of the vicious intra-Pentagon politics of research and development of the late 1940s, but while a group within the RDB like Committee X might celebrate the newfound budgetary bounty enjoyed by their pet programs, they do not seem to have had much of a direct role in securing this bounty. In fairness, the harder-to-trace "indirect" role of establishing a hegemonic identity for a particular sociotechnical imaginary (like inertial guidance or biological weapons) within the Pentagon should not be ignored. Nonetheless, whatever "soft power" (to borrow Joseph Nye's term) RDB committees enjoyed over the Pentagon's collective technological *Weltanschauung*, the Board had by the early 1950s proven to be a dismal failure in the "hard power" bureaucratic politics of dictating what money went where.[81] In the wake of the 1953 ascension of Dwight Eisenhower, the RDB was one of the first institutions to be discarded in a sweeping reorganization of the Department of Defense. Eisenhower was determined to bring the unruly military services into check, and thus sought to strengthen

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[79] Converse, *Rearming for the Cold War*, p 30; Ponturo, "Analytical Support for the Joint Chiefs of Staff," pp 5-40.
[80] Converse, *Rearming for the Cold War*, p 38.
[81] Joseph Nye, *Soft Power: The Means to Success in World Politics*, New York: PublicAffairs, 2004.

the central civilian staff of the Pentagon. The RDB was thus replaced by Assistant Secretaries of Defense for Research and Development and Applications Engineering, answering to the Secretary of Defense and supported by full-time civilian staffs.[82] For a part-time advisor like Baldwin (who had resigned from his chairmanship a few months before this reorganization was finalized), this professionalization of his former role spelled the end of his formal access to that level of Pentagon decision-making.[83]

In 1948, as the reorganization of the JRDB into the RDB was being completed, the idea of combining Committee X and the JRDB's committee on chemical warfare was officially broached by RDB leadership.[84] Baldwin voraciously resisted such a move, arguing that this increased purview would unacceptably increase his already-high workload and that of his peers, and offering his resignation should any such amalgamation take place.[85] Baldwin did have a penchant for taking on more administrative work than there were hours in a 7-day work week (which had even led to a month-long nervous breakdown in 1941), and probably did legitimately feel overworked.[86] Nonetheless, he adapted quite readily to the increased responsibilities of dealing with chemical as well as biological warfare topics when serving as a member of the Chemical Corps Advisory Board a decade later, in his early sixties. Committee X's executive secretary, H. I. Cole, believed that this workload would be manageable if a few

[82] These two offices were in turn combined in yet another reorganization in 1957 as the Office of the Assistant Secretary of Defense for Research and Engineering. Converse, *Rearming for the Cold War*, pp 416-419.
[83] In practice, Baldwin retained cordial and occasionally influential interpersonal relations with key full-time scientist-advisors within this new staff, like Army Chief Scientist Harold Gaskill.
[84] L. R. Hafstadt to Baldwin, May 3, 1948, in UWA Baldwin Papers, Box 15 Folder 2. Hafstadt was Bush's executive secretary at the time.
[85] Baldwin to L R. Hafstadt, May 8, 1948, in UWA Baldwin Papers, Box 15 Folder 2.
[86] Baldwin discusses this 1941 episode (which took place just before the death of one of his daughters) in Baldwin, *My Half-Century*, p 115.

more members were added to a combined committee, but Baldwin explicitly rejected this solution.[87] Combining chemical and biological warfare on the RDB would also, even he admitted, "be a very logical arrangement," given their shared institutional home within the Chemical Corps (as the CWS was known after 1947) and shared identity in the imagination of politicians and Pentagon officials.[88] This was probably exactly why he resisted this proposal, however. Chemists, as far as Baldwin was concerned, had too much influence over microbiological matters they knew little about already, between Chemical Corps leadership and bodies like the Chemical Corps Advisory Council. Like Committee X, the Council was an advisory group of part-time academic scientists, but which advised the Chemical Corps directly rather than the RDB. During the 1940s Baldwin had little patience for the Council (on which, ironically, he would later serve in the 1950s and '60s), whose members were almost all chemists or chemical engineers in background, and who had the potential to exercise more control over the scientists of Detrick than he did.[89] This vexed him so much that he occasionally advocated for the Advisory Council to be disbanded outright.[90]

[87] Cole to Baldwin, May 4, 1948, in UWA Baldwin Papers, Box 15 Folder 2. Baldwin explicitly denied that adding more members would help in his May 8 letter to Hafstadt. See Baldwin to Hafstadt, May 8, 1948, in UWA Baldwin Papers, Box 15 Folder 2.
[88] Baldwin to Hafstadt, May 8, 1948 in UWA Baldwin Papers, Box 15 Folder 2.
[89] In 1948, for instance, the 11-member the Research Council of the Advisory Board (which had the most direct say on R&D programs like those at Detrick), 7 were chemists or chemical engineers, 2 were physicians with extensive background in the treatment of gas casualties (Chicago's F. C. McLean and Yale's M. C. Winternitz), one was an expert on plant breeding (which had bearing on anti-crop biological warfare) and one (epidemiologist Alexander Langmuir) had expertise directly bearing on biological warfare against humans (at the time he was primarily known for his work on the epidemiology of airborne infection). None were microbiologists. See the list entitled "Research Council of the Chemical Corps Advisory Board," n.d. (attached to letters dated August 1948), in UWA Baldwin Papers, Box 15 Folder 2. "I do not believe," Baldwin dryly put it at the time, "that [this list] represents a fair balance so far as the various lines of interest are concerned." See I. L. Baldwin to H. I. Cole, August 19, 1948 in Ibid.
[90] Baldwin to Major General A. C. McAuliffe, January 19, 1950, in UWA Baldwin Papers, Box 15 Folder 10.

Besides fending off integration with the chemists, the transition from JRDB to RDB seems to have had little impact at the level of Committee X, which continued on with the same members, same organization, and same activities through the ostensible change in management. Following the delivery of the 1947-1948 reports, the Committee focused particularly on its other purview: establishing the budgetary priority of various biological warfare projects *vis a vis* all other military research and development projects, to contribute to an annual RDB recommendation to the Secretary of Defense and JCS. In practice, this RDB recommendation, lacking any affirmative authority, was effectively ignored in subsequent Pentagon budget negotiations. While this reflected the structural weakness and contradictory role of the RDB as a whole, the experience of Committee X highlights how little the RDB's advice represented anything more than the political capital of RDB committee chairmen. Baldwin's committee and select members of its subordinate panels, met as an "ad hoc panel" several times a year to review the progress and prospects of Detrick programs, which would serve to assign priorities to these programs. These meetings, however, were often as not held at Detrick itself, in explicit consultation with Detrick's leadership.[91] A skeptic might rightly ask how independent the RDB group were of the Detrick leaders they were ostensibly supposed to assess. Such skepticism would be particularly warranted because informally, Baldwin himself consulted closely with his former Detrick colleagues, swapping advice, opinion, and relevant news in a collegial social world reminiscent of their shared academic backgrounds (see below.) The external social world of the uniformed military members of the committee was a similarly powerful influence: Baldwin later noted that these

[91] See e.g. Edward Wetter to Ira L. Baldwin, December 7, 1948 in UWA Baldwin Papers Box 15 Folder 2.

members, though otherwise engaged and open to discussion, tended to be unwavering advocates of their home service's budgetary interests when it came time to vote or write reports.[92] This was presumably certainly true of two Army representatives on the Ad Hoc Panel: Colonel C. E. Loucks, the head of the Chemical Corps Research and Engineering Division, and Major General Alden Waitt, commander of the Chemical Corps, a substantial portion of whose budget was ultimately the thing under discussion![93] All of this is to say that as a practical matter, the job of Baldwin's committee was not to fill its ostensible role as a skeptical judge of Detrick's programs, but to act as a lobby for Detrick within the RDB. This, in turn, entailed an elaborate process of negotiation and second-guessing, in which Baldwin's group calibrated their recommended priority of various Detrick projects based on how they expected their pet projects to fare in the larger RDB scrum.[94] If Committee X's experience is anything like a representative case, it is easy to imagine that the RDB's lack of formal bureaucratic authority to enforce its recommendations was enhanced by a lack of credibility regarding the ostensible "objectivity" of its scientists.[95]

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[92] See Baldwin, *My Half-Century*, p 344 for a discussion of the military members' voting habits.

[93] Loucks and Waitt (with full title given) are among the members of the ad hoc panel who received an official commendation letter from Baldwin 1948. See Baldwin to Col. C. E. Loucks, June 9, 1948, in UWA Baldwin Papers, Box 15 Folder 2. Baldwin and Maxcy discussed the inclusion of Waitt earlier that year: see Maxcy to Baldwin, December 30, 1947 and Baldwin to Maxcy, January 12, 1948, in Ibid.

[94] This process of budgetary negotiation took place about a year and a half or so before the relevant budget was implemented. Thus, priorities for the RDB's FY1951 budget recommendations (for a budget which would begin October 1, 1950) were negotiated in the spring of 1949, with the RDB delivering its official recommendations to the Secretary of Defense in late August. See Karl T. Compton to Baldwin, September 1, 1949, in UWA Baldwin Papers, Box 15 Folder 4, for a sanguine official reflection on the FY1951 recommendations. (This 1949 work was in fact rendered moot by the outbreak of the Korean War less than a year later, which inspired a flood of emergency defense appropriations in the fall of 1950).

[95] "Objectivity" (which as a matter of course entailed personal political orthodoxy) was an important cultural currency for scientists involved in policy during the early decades of the Cold War, reflecting the cultural authority of science writ large in mid-20th century America. For discussions of this rhetoric of "objectivity" (and particularly the challenges it presented to scientists who wished to challenge the so-called Cold War consensus prevailing in mainstream politics in the 1950s), see e.g. Kelly Moore, *Disrupting Science: Social Movements, American Scientists, and the Politics of the Military, 1945-1975,*

This routine was upended in mid-1950, as the outbreak of the Korean War and a subsequent sense of near-panic as Mao's China entered the war in the winter of 1950-1951 brought a sudden influx of emergency funding virtually everywhere in the military, including the Chemical Corps and Detrick. The sudden reversal of Truman administration budget cuts sent Baldwin's community scrambling as their fondest wishes seemed within their grasp, as the final years of Baldwin's Committee X correspondence focus more on administrative issues like setting up a long-awaited open-air testing cite at Dugway Proving Ground in Utah than on the game of budgetary musical chairs which dominated the late 1940s. The flood of money that Detrick had seen with the outbreak of the Korean War allowed the completion of long-awaited capital investments (like the million-gallon "8-ball" aerobiology chamber, planned since 1945), expanded research and testing schedules, and the first military adoption of a microbe (a strain of *Br. suis*) as a "standardized" piece of equipment.[96] "Things," Baldwin wrote in late July, a month after the beginning of the war, "are moving rapidly at the present time… [and] I think there will be adequate support now."[97] Baldwin also branched out to serve on other Pentagon committees, most notably the 1951-1952 "Killian Committee" on the organization of a

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Princeton: Princeton University Press, 2008; Jessica Wang, "Physics, Emotion, and the Scientific Self: Merle Tuve's Cold War," *Historical Studies in the Natural Sciences* 42 no 5 (2012), pp 341-388; Paul Rubinson, *Redefining Science: Scientists, the National Security State, and Nuclear Weapons in Cold War America*, Boston: University of Massachusetts Press, 2016; and Audra J. Wolfe, *Freedom's Laboratory: The Cold War Struggle for the Soul of Science*, Baltimore: Johns Hopkins University Press, 2018. The JRDB/RDB's leadership were quite explicit in appealing to the cultural authority of "objectivity," with RDB chairman Karl Compton asserting in 1949 that "the single factor that distinguishes these [RDB] Committees from other Departmental [of Defense] groups is the existence of the competent, disinterested, civilian components." See Karl T. Compton to N. Paul Hudson, July 20, 1949, in UWA Baldwin Papers, Box 15 Folder 4.

[96] US Department of the Army, *U.S. Army Activity in the US Biological Warfare Programs, Volume 1*, Washington, DC: US Department of the Army, 1977, pp 33, 36-37.

[97] Baldwin to Nungester, July 24, 1950, in UWA Baldwin Papers, Box 15 Folder 10. Bullene put it still more directly in a 1952 speech: "today, thanks to Joe Stalin, we are back in business." E. F. Bullene, "The Needs of the Army," *Armed Forces Chemical Journal* 6 no 1 (1952), p 8.

biological warfare program which, with this influx of wartime funding and capital investment, now seemed permanent. As was their habit, Baldwin and Detrick leaders swapped gossip about the Committee's deliberations, with Baldwin assuring them that the ruling would be favorable to them by "recommend[ing] that all of the activities in which you and I are interested should be separated from the other activities of the Corps."[98]

Meanwhile, events at Detrick thrust him into the role of informal mediator between Bullene and Detrick's leading scientists. Relations between Detrick's researchers and the military managers of the Chemical Corps soured considerably during the Korean War, as increased budgets had come with increased Chemical Corps interest in a research project about which its officers had promised much. With this interest came what Detrick researchers saw as administrative meddling with their work. This perceived meddling came from the new Chief Chemical Officer, General Egbert "Frank" Bullene, a career Chemical Corps officer who had commanded Edgewood and whose 1951 appointment, following Anthony McAuliffe of Bastogne "Nuts" fame, represented a return to promotion from within the Corps.[99] The production of pathogens like *Br. suis* and *B. anthracis* for field tests at Dugway was accelerated, with attendant safety concerns from Safety Division director Arnold Wedum, and Bullene sought to increase the authority of the military commander of Detrick over the (civilian) Scientific Director.[100]

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[98] Baldwin to Fothergill, April 5, 1952, in UWA Baldwin Papers, Box 11 Folder 10.
[99] "General Bullene, Led Chemical Corps," *New York Times*, Feb 23, 1958, p 92.
[100] Major General E. F. Bullene to Oram C. Woolpert, November 2, 1951 in UWA Baldwin Papers, Box 11 Folder 10. Wedum's safety concerns are expressed in a comprehensive 1953 report on the activities of Detrick's Safety Division since its 1943 inception. On Wedum and the Safety Division, see Chapter 3, below. Concerns about the production schedule for field tests can be found in Arnold G. Wedum, "Safety Program at Camp Detrick, 1944-1953 (Special Report No. 185)," Frederick, MD: Chemical Corps Biological Laboratories, 1953, p 65.

Oram Woolpert, who was serving in this post, protested that this scheme "will introduce the hazard that the Commanding Officer at times may be called on or tempted to exercise judgment in technical matters beyond the scope of his competency." Woolpert instead wanted to emulate the example of "our British and Canadian friends [who] commonly go to the opposite extreme and set up their senior civilian scientists in complete charge of the installations at which technical activities are carried on."[101] This left the Killian Committee's recommendations as Detrick's best recourse. The Committee, on which Baldwin (as head of Committee X) had served, had recommended that biological weapons research be centralized, but not directly under Chemical Corps leadership. As Baldwin explained it to J. B. "Joe" Wilson, a Wisconsin colleague temporarily serving as scientific director of the new Dugway proving ground, "all research and development would be under a single civilian director of research, regardless of where the work is conducted. This would include all the work at Detrick, the work at Dugway in our field, the work of Plum Island, all of Jeff Norman's activities wherever they may be located, and so on." (Plant physiologist Arthur Geoffrey Norman, who had served at Detrick since its inception, and who had been director of anti-crop research since 1946, was considering leaving Detrick over the management fiasco). "In essence, the report calls for a special assistant to the Chief who is to have full charge of Research and Development" preferably drawn from industry (in short, someone like Merck, filling his wartime role).[102] The report thus effectively called for something as close to the removal of bioweapons research from Chemical Corps control as possible without this move being formally made. Under its civilian R&D czar, the post-Killian BW program would be

[101] Woolpert to Bullene, November 10, 1951 in UWA Baldwin Papers, Box 11 Folder 10.
[102] Baldwin to Wilson, May 15, 1952 in UWA Baldwin Papers, Box 14 Folder 5

effectively autonomous, albeit formally operating under the top leadership of the Chemical Corps. “This individual would be located at Detrick and… would be in complete charge of Camp Detrick, with the C.O. reporting to him on everything except straight military matters.”[103] Keeping Detrick at arms’ length from the Corps, with a civilian intermediary in-between, would hopefully solve a fundamental problem with recruiting and retaining researchers that had dogged the program for years. As Baldwin had noted in 1947, “there was a very real reluctance on the part of many scientists to accept employment in research laboratories which were to be directed by Army personnel. Whether or not the reputation is justified, the Army does not enjoy a good reputation so far as its conduct of research work is concerned.”[104]

Unsurprisingly, therefore, the Killian Report was received favorably at Detrick, but this mood soured as it became clear that the Chemical Corps was quietly resisting it. Months after the report was issued, Baldwin confidant Arthur Geoffrey Norman reported that “no overt action whatsoever [had] been taken within the Chemical Corps to implement [its] proposals,” despite the act that “quite clear promises of immediate action [had been] made to the senior staff members” at Detrick. As a result, “the effect on morale [was] devastating.”[105] For a number of Detrick’s senior scientists, this runaround was the last straw. Several of Baldwin’s closest correspondents, including Scientific Director Oram Woolpert, confided their frustration in him and solicited his help in finding new jobs in academic research and administration. Baldwin, in turn, was sympathetic and cooperated with their job searches, reiterating in his correspondence that

[103] Ibid.
[104] I. L. Baldwin to Vannevar Bush, May 2, 1947 in UWA Baldwin Papers, Box 15 Folder 1.
[105] A. G. Norman to I. L. Baldwin, July 16, 1952 in UWA Baldwin Papers, Box 11 Folder 10.

while he was interested in maintaining the strength of the BW program, he was more interested in his colleagues' personal well-being.[106] Without naming names, he then reported this staff unhappiness to Army officials, using the imminent threat of mass resignations to argue for the implementation of the Report. Over the course of the year, he relayed Bullene's disingenuous assurances that the Report would be implemented soon back to colleagues at Detrick who were on the fence about leaving, while counseling patience and caution.[107] Through all of this, he quietly lobbied Pentagon officials like Chief Scientist Harold V. Gaskill and advisors like George Merck (who had recently returned to BW advising) to pressure Bullene to accept the Killian recommendations: "I understand that a number of… top men at Detrick are serious considering other offers. I am very much afraid that we will lose many of our most competent people if there is not some positive action to implement the Killian report very shortly."[108] In essence, he was acting as a kind of tacit labor representative to the management of the Chemical Corps, "pushing General Bullene to give more recognition to bacteriologists in connection with the BW program" (as he put it the next year).[109] Ultimately, Bullene needed more than threats before he would consider budging, and by August, Baldwin warned Gaskill that "the situation at Detrick seems to be deteriorating very rapidly… I am afraid there will be resignations of several of the… key personnel very shortly since I know they are actively considering other positions. These

[106] As he told Norman when he first heard of his resignation from Detrick, "from the standpoint of my interest in your program at Detrick I am very sorry to see you leave. On the other hand, from the standpoint of my interest in your welfare, I am very happy that you have found a position which seems to offer you more opportunities." See Baldwin to A. G. Norman, May 29, 1952 in UWA Baldwin Papers, Box 11 Folder 10.
[107] See e.g. I. L. Baldwin to A. G. Norman, March 20, 1952 in UWA Baldwin Papers, Box 11 Folder 10.
[108] Baldwin to Merck, August 11, 1952, in UWA Baldwin Papers, Box 16 Folder 2.
[109] Baldwin to Gail M. Dack, August 26, 1953, in UWA Baldwin Papers, Box 12 Folder 1.

resignations are occurring because of the feeling of futility which prevails up at Frederick. Unless some positive action to implement the Killian report is taken soon, I am afraid that we will lose a great majority of the best of the people. To replace them with others equally good will be almost impossible."[110] It was not until a number of the most disgruntled Detrick leaders including Woolpert, Norman, and assistant director Keith Lewis resigned that Bullene finally relented, and began paying more than lip service to the Killian Report.[111] This was in turn a drawn-out process taking months.[112] In those months of late 1952 and early 1953, however, an even larger issue confronted Baldwin, as a new presidential administration was elected, revamped the organization of the Pentagon staff, and in so doing abolished the RDB.[113] Baldwin, who had seen the writing on the wall a few months before this reorganization was finalized, had already resigned.

There was thus considerable irony in the fact that the Chemical Corps' longstanding invitation to its Advisory Council was the best avenue to continue to support bioweapons research available to Baldwin. This invitation had been proffered for the past 8 years by the various Chief Chemical Officers (the Advisory Council ostensibly worked directly for him at his pleasure) and when it was reiterated by Bullene in 1953, Baldwin quickly accepted. Baldwin personally disliked Bullene and (particularly in light of the 1952 crisis) did not think him competent to manage Detrick effectively, regarding him as a general more interested in moving troops than directing research, who "ha[d]

[110] Baldwin to Gaskill Aug 8 1952 in UWA Baldwin Papers, Box 14 Folder 8
[111] Bullene explicitly attributed this reversal to the fact that "we have already lost three very good men at Camp Detrick." Bullene to Baldwin, August 12, 1952 in UWA Baldwin Papers, Box 11 Folder 10.
[112] By the end of February 1953, the Corps had still not appointed the BW czar, though they were allegedly interviewing a civilian interested in the post. See Gaskill to Baldwin, February 20, 1953 and Baldwin to Gaskill, February 26, 1953 in UWA Baldwin Papers, Box 14 Folder 8.
[113] Baldwin's colleague on the RDB, Ohio University microbiologist N. Paul Hudson, joked that "FBI = Fired By Ike." See Hudson to Baldwin, June 9, 1953 in UWA Baldwin Papers, Box 14 Folder 17.

very little knowledge of or interest in the research phases of the BW program."[114] The feeling was mutual: Bullene distrusted scientists, who he saw as meddling without authority in military matters, going so far as to chide the Advisory Council "to devote itself to advising him as to the proper approach to the Corps' various technical problems rather than enlarging their scope to where it might be looked upon as a second Research and Development Board" shortly after Baldwin's ascension.[115] The fact that Bullene nonetheless appointed Baldwin to this role reflects the importance of Baldwin within both formal government circles and within the informal BW network: the expertise of the former director of Detrick and chairman of Committee X could not safely be ignored.

Forced to pay lip service to the Killian report by 1953, Bullene subsequently sought to dispense with Detrick entirely by turning it over to a contractor, the Mathieson Chemical Corporation. Researchers working for private contractors had been integrated into work at Detrick for the past few years, with the Ralph M. Parsons Company managing a significant amount of the aerobiological testing happening at the '8-Ball' for the past few years, and the Corps was ostensibly interested in expanding the use of research contracts as a cost-saving measure.[116] Turning Detrick over to an organization like Mathieson would also serve to remove the generals from the direct management of Detrick's scientists and render the Killian report's BW czar irrelevant, removing that irritant in their relationship. However, though apparently serious contract negotiations took place through late 1953 (even being reported in *Science*), they ultimately fell

[114] Baldwin to Gaskill, April 7, 1952 in UWA Baldwin Papers, Box 14 Folder 8.
[115] Bullene was quoted in C. B. Marquand, "Memorandum for Members of the Chemical Corps Advisory Council, Subject: Meeting 28-31 October 1953 (Suggested Change- Week of 9 November)," September 23, 1953, in UWA Baldwin Papers, Box 12 Folder 1.
[116] Wedum, "Safety Program at Camp Detrick," p 149.

through, leaving Detrick still in Chemical Corps hands.[117] Faced with these two embarrassing defeats, Bullene retired soon afterward, in early 1954.[118]

This retirement gave Baldwin's network another opportunity to unofficially influence policy. They now had a preferred candidate to serve as the new leader of the Corps: General William Creasy, commander of the Corps' Research and Development command. Creasy had worked with Baldwin for years on the RDB, held a master's degree in chemical engineering, and was much more interested in supporting research than Bullene or Waitt had been.[119] Baldwin did not fully trust even Creasy to manage microbiological research well, noting that "many of the administrative actions taken by General Creasy have served to destroy the confidence of the research workers in him," but nonetheless considered him to be much more open to scientific advice than his predecessors.[120] Both men were personally friendly, corresponding by first name and sending more than the typical perfunctory letters of congratulations upon major career milestones.[121] Thus, when rumors of Bullene's retirement began to circulate, Baldwin and a number of his Advisory Council compatriots began to quietly lobby for "the desirability of having a chief who is competent in the field of research as well as a competent administrator," and that "General Creasy met this combination better than anyone else in

[117] See "News and Notes," *Science* 118 no 3072 (Nov. 13, 1953), p. 584. A. P. Colburn to I. L. Baldwin, February 11, 1954 in UWA Baldwin Papers, Box 12 Folder 1.
[118] A. P. Colburn to Members of the Chemical Corps Advisory Council, January 29, 1954 in UWA Baldwin Papers, Box 12 Folder 1.
[119] See George W. Carnachan, "Biographical Sketch: Brigadier General William M. Creasy," n.d. (c. April 1954), in UWA Baldwin Papers, Box 12 Folder 1.
[120] Baldwin to Gaskill, April 7, 1952 in UWA Baldwin Papers, Box 14 Folder 8.
[121] See e.g. I. L. Baldwin to Brigadier General William M. Creasy, August 15, 1951, in UWA Baldwin Papers, Box 16 Folder 1. As Baldwin noted to Detrick's Leroy Fothergill upon Creasy's appointment, "I am well acquainted with Creasy and am perfectly willing to tell him what I think on any subject." Baldwin to Fothergill May 4, 1954 in UWA Baldwin Papers, Box 12 Folder 1.

the Corps."[122] Baldwin himself "discussed the situation with a number of [his] friends in the Department of Defense who [he] thought might be in a position to do something."[123] It is unclear how influential this lobbying was, but even though the Army was initially considering candidates from outside the Corps for the post, Creasy ended up getting the job.[124] Baldwin's network thus had cause for celebration as their preferred dark horse candidate came to power.[125]

With Creasy installed and Detrick's autonomy preserved, the mid-1950s were a time of quiet routine for Detrick and Baldwin. Post-Korean War budgets at Detrick had been slashed, and Detrick (now a Fort, a more-permanent installation than the Camp it has been) returned by and large to basic research. This was led by a human experimentation program, Project Whitecoat, born out of collaboration with the Army Medical Corps.[126] Baldwin, meanwhile, remained a diligent member of the Advisory Council, which, combined with his extensive administrative experience, political acumen, and friendly relationship with Creasy, made him a candidate to replace the chairman of

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[122] Baldwin to Alan P. Colburn, February 15, 1954, in UWA Baldwin Papers, Box 12 Folder 1. The most vocal Council members in favor of Creasy tended, like Baldwin, to come from the biological or medical sciences, including Longenecker, Langmuir, Cox, and Audrieth. See A. P. Colburn to Baldwin, February 11, 1954, in Ibid.

[123] Baldwin to Carnachan, May 4, 1954, in UWA Baldwin Papers, Box 12 Folder 1.

[124] Pentagon rumors, according to both Baldwin and one of his correspondents, George Carnachan, had it that two non-Corps generals were the Army's initial picks for the role. See Baldwin to Alan P. Colburn, February 15, 1954 and Carnachan to Baldwin, April 14, 1954, both in UWA Baldwin Papers, Box 12 Folder 1. There was ample precedent for the Army appointing a Corps outsider to the role in the wake of a fiasco like the Detrick crisis. Bullene's predecessor, General Anthony McAuliffe of Bastogne "Nuts" fame, had been such a commander, appointed after "the late and unlamented Alden Waite" (as Carnachan put it) had apparently sabotaged the political standing of his subordinates.

[125] A Milwaukee civil defense official and confidant of Baldwin, George Carnachan, had lobbied Wisconsin Senator Alexander Wiley and the Secretary of the Army on Creasy's behalf, receiving assurances from both. He, at least, believed that "our efforts on [Creasy's] behalf have not been in vain" upon hearing that Creasy had received the post. See Carnachan to Baldwin, April 27, 1954, in UWA Baldwin Papers, Box 12 Folder 1.

[126] See U.S. Army Medical Research Institute of Infectious Diseases, "Project Whitecoat: A History" (1974); Robert L. Mole and Dale M. Mole, *For God and Country: Operation Whitecoat: 1954-1973*, New York: TEACH Services, 1998.

the Council when he retired in 1957. The Council remained dominated by chemists, (though Baldwin had been joined by a few biologists, such as Harold Cox and Halvor O. Halvorson), but Creasy promoted him to chairman of the Council nonetheless. As with Committee X, this would prove to be a perennial role, with Baldwin remaining in it until the Council itself was dissolved in 1965. Ironically, the very regimented, hierarchical nature of the military structure within the Chemical Corps made influence and authority a highly contingent affair. Formally speaking, the Corps, like the military in general, was seemingly always reorganizing, receiving reports on reorganization, preparing to reorganize, or resisting reorganization, with the result that no-one's bureaucratic victory was ever more than temporary. For instance, just two years after the BW cadre strong-armed the Corps into giving BW research its Killian Report-recommended autonomy, this autonomy was threatened by another (Corps-instituted) committee with its own organizational recommendations.[127] Less formally (but more importantly for scientific advisors like Baldwin), networks of interpersonal relationships between military officers and civilians were in regular flux due to changing tours of duty. A fraught relationship between scientific advisors and the Chief Chemical Officer could be a relatively predictable reassignment or retirement away from a close alliance with a new occupant of that role, as in the case of the transition from Bullene to Creasy. As a quiet fixture of the Chemical Corps world, with greater longevity than any particular mode of organization or the tenure of any particular military officer in a position, someone like Baldwin was

[127] This was the 1955 "Miller Committee," led by Chevron executive Otto N. Miller. This time it was Baldwin's turn to be obstructionist: upon learning secondhand about the committee's recommendations for "the abolishment of the autonomy the BW group now has," he immediately began enlisting Detrick allies to blunt its impact. See Baldwin to John Schwab, September 23, 1955, in UWA Baldwin Papers, Box 11 Folder 3. A copy of the report can be found later in the same folder. Evidently, this obstruction campaign worked, as there is little mention of the Miller Committee in subsequent correspondence with colleagues at Detrick and certainly nothing like the air of outrage which had prevailed in 1952.

often well-served by simply out-waiting opponents like Bullene. When Creasy was succeeded in turn by General Marshall Stubbs, Baldwin continued on as chairman of the Advisory Council, closely consulting (albeit less cordially) with this new leader.

Between Baldwin's willingness to take on and skill in undertaking administrative roles, and his diplomatic personality, his position on the Advisory Council was never seriously threatened by forces within the Corps. Rather, it was external developments which put an end to it, as the Council itself was abolished in the early 1960s. Falling prey to a sweeping McNamara-era reorganization of the Army technical services, leadership of the Chemical Corps was folded into a new Army Material Command, which thus inherited Detrick and the other BW sites as tiny parts of a vast administrative archipelago in an Army slowly drifting into war in Vietnam.[128] A council advising the now-defunct position of Chief Chemical Officer was redundant. Baldwin's advisory career was not over, as the former Advisory Council chairman was invited to join the Army's Munitions Advisory Group, a similar organization of part-time civilians which now included Detrick among its responsibilities. He was joined by Walter Nungester, and both set about their usual routine of zealously advancing Detrick's interests. As aging small fish in a vast pond, however, the influence of both men was vastly diminished, and with University of Wisconsin administrative matters and his own retirement looming, Baldwin resigned from the Munitions Group in mid-1969.[129] Mere months later, Detrick as he knew it was abolished, too, with the Nixon administration renouncing offensive biological warfare research, and Detrick scientists and their 'friends' scrambling to find some other *raison*

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[128] See James E. Hewes, Jr., *From Root to McNamara: Army Organization and Administration, 1900-1963*, Washington, D.C.: US Army Center of Military History, 1975, pp 345, 363.
[129] I. L. Baldwin to Robert W. Loux, August 20, 1969 in UWA Baldwin Papers, Box 13 Folder 7.

d'être for the base. In many ways, however, the real end of Baldwin's post-war social world was a half-decade before, when the reorganization of the Corps changed his relationship with the Cold War state, for the first time in two decades, to one basically peripheral to any day-to-day decision-making. In this, he was not alone: for all that particular circumstances differed, many other civilian scientists found their relationship with the Cold War state of the 1940s and '50s unraveling in the 1960s, with the explosive growth of jobs and grants tapering off by the end of the decade, Vietnam-era protest souring their relationship with the state and with a younger generation steeped in the politics of the New Left vocally rejecting such a relationship outright.[130] For all that the 1960s transformed science-state relationships, and the social status of science itself (rather than destroying both outright), it is undeniable that the "Cold War science" of the 1940s and '50s was a different thing from that of the 1970s and '80s.[131] Baldwin's social world, of microbiologists' flirtation with the military and access to minor policy-influencing positions, was ultimately an ephemeral one, but that is true of science more generally as well.

[130] This *denouement* in the late 1960s is a common theme in histories of science in the first two decades of the Cold War. For three representative examples, see Appel, *Shaping Biology* for a discussion of plateauing federal funding in the period; Wang, *In Sputnik's Shadow* for a discussion of the fraying relationship between PSAC science advisors and the Johnson and Nixon administrations; and Moore, *Disrupting Science* for a discussion of the rise of New Left protest in Vietnam-era science.

[131] The historiography of Cold War science has increasingly focused on the 1970s in recent years, with David Kaiser and W. Patrick McCray, editors of an influential edited collection entitled *Groovy Science*, advancing the term to describe American science in the 1970s as a corrective to simplistic narratives of the 1960s counterculture having done irreparable damage to the cultural cachet of science. Science-state relations, and the influence of the ongoing Cold War on American science, they argue, were certainly altered by the tumult of the 1960s, but substantial continuities remained. See David Kaiser and W. Patrick McCray (eds), *Groovy Science: Knowledge, Innovation, and American Counterculture*, Chicago: University of Chicago Press, 2016. Also see Jon Agar, "What Happened in the Sixties?," *British Journal for the History of Science* 41 no 4 (2008), pp 567-600.

Not Just "On Tap:" Ira Baldwin's Social World

In principle, even this two-decade experience should have been sharply bifurcated by the events of 1953. Before that year, as chairman of Committee X, Baldwin was theoretically in a position of some (abet ambiguous) authority over the Corps. After that year, Baldwin served as an advisor to the Corps ostensibly at the pleasure of the Chief Chemical Officer, seeminglly having no power base of his own. In reality, however, not very much changed in Baldwin's advisory career after the disbanding of the RDB. Baldwin continued to manage a network of correspondents inside and outside of Detrick, serving as a multifarious resource for his Detrick colleagues and marshalling the support of pro-BW colleagues in the SAB. He continued to be a close ally of the Chemical Corps' interests when he saw them aligning with Detrick's, and a thorn in the Corps' side when they didn't. He continued to advance the same arguments about the potential of bioweapons research and the folly of abandoning it well into the 1960s. Fundamentally, for all that his bureaucratic title changed substantially after 1953, the nature of his social world did not. This highlights the essential fact about Baldwin's advisory career: he was not merely an "on-tap" advisor of idealized sort, meekly filling whatever role an organization chart assigned him to. (Nor, it should be noted, were he or any of his companions shadowy Dr. Strangeloves with hegemonic power over bioweapons research.) Rather, Baldwin filled an ambiguous but influential role by virtue of his position mediating between two communities: microbiologists and allied biomedical experts, a community which Detrick remained firmly a part of, and the Chemical Corps sponsors of Detrick's research (who in turn traditionally relied heavily on alliances with scientists in their own relationship with the wider military). As Benjamin Wilson argues in his examination of the origin of nonlinear optics, it can be counterproductive to focus too heavily on formal institutional boundaries, given the fluidity of the relationship between academic scientists and classified research in the early Cold War. Rather, Wilson argues, it is the "social world" of the community of scientists operating across these boundaries which is the most fruitful object to study to understand the nature of the Cold War science-government relationship.[132] Adopting this viewpoint, I now turn to an examination of the social world Baldwin operated in, an ambiguous, sometimes-contested, but ultimately fairly coherent community of scientists and soldiers built upon the sociotechnical imaginary of germs-as-weapons.

The Network of Detrick "Friends"

Microbiology was a small and tight-knit field in the early 20th century United States, with its major professional organization, the Society of American Bacteriologists, boasting fewer than a thousand members in the 1930s.[133] As a prominent faculty member of one of the country's major microbiology programs at Wisconsin, and more importantly as a gregarious and administration-minded scientist who served as the SAB's Secretary-Treasurer for over half a decade, Baldwin maintained a wide-ranging network within this already small community when he became involved with weapons research in the early 1940s.[134] A substantial fraction of this community then joined Baldwin at Detrick, where

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[132] Benjamin Wilson, "The Consultants: Nonlinear Optics and the Social World of Cold War Science," *Historical Studies in the Natural Sciences* 45 no 5 (2015), 758-804.

[133] Baldwin's 1944 SAB presidential address mentions 755 members in 1932, rising to 1,977 in 1943. See Baldwin, "Where Does the Trail Lead?," p 1, in American Society for Microbiology Archives (ASM) Series 13-IIAT (Ira L. Baldwin Presidential Papers), Folder 3. He later recalled that because of this small field, "I knew practically every bacteriologist in the United States" in the early 1940s. Baldwin, *My Half-Century*, p 122.

[134] One of three leadership positions in the SAB of the period, the Secretary-Treasurer was Lord High Everything Else to the largely ceremonial President and Vice Presidents of the organization. Effectively, Baldwin was the one-man staff of the SAB during his tenure in this role.

he served as their wartime Scientific Director, acting as a vocal advocate for the microbiologists and their interests with military authorities. Even after formally leaving this role in early 1945, Baldwin remained a common sight at Detrick for over a year as a "consultant" to his colleague and successor, Oram Woolpert.[135] With this background in mind, it should be no surprise that he remained a close and collegial correspondent of a large cadre of Detrick's scientific leadership throughout his postwar advisory career. Detrick researchers generally retained close ties to the larger microbiological community during this period, holding (as critic and former member Theodor Rosebury put it in the early 1960s) "a status… more or less equivalent to that of a great university," but for Baldwin and other microbiologist-advisors of his generation, such links were as much personal friendships and political alliances as they were scientific ties.[136]

Though ostensibly the major judge of the merit of Detrick research, Baldwin's Committee X was closely tied to Detrick in its official duties, with panels explicitly drawing upon Detrick expertise in multi-day visits to compose their assessments.[137] As the chairman of the Committee, Baldwin partook in this official contact, but also informally relied on Detrick correspondents in fields in which he wasn't an expert for

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[135] See e.g. the contents of UWA Baldwin Papers, Box 11 Folder 1 "BW- Baldwin, I. L.- Appointment as CWS Consultant (November 1944)."

[136] See Theodore Rosebury, "Medical Ethics and Biological Warfare," *Perspectives in Biology and Medicine* 6 no 4 (1963), pp 512-532. It is important to note that not all of Baldwin's Detrick correspondents were microbiologists: he was for example an especially warm correspondent of plant pathologist Arthur Geoffrey Norman. Norman had been a member of the wartime Detrick community and remained there as director of "anti-crop" research into the early 1950s before leaving for a research position at the University of Michigan. I use "microbiologists" as a shorthand to describe Baldwin's scientific community because they were the majority of his social world, and the Society of American Bacteriologists was the scientific society principally connected to Detrick. Microbiologists were likewise Baldwin's community before the war, while he first met the non-microbiologists in his network of Detrick 'friends' in direct connection with bioweapons research. Nonetheless, it should not be forgotten that given the wide-ranging subject matter of "biological warfare," some of the players in this social world would have primarily identified as another sort of biologist (as in the case of Norman), or with a medical field, as in the case of epidemiologist Alexander Langmuir.

[137] See e.g. H. I. Cole to I. L. Baldwin, August 26, 1948, in UWA Baldwin Papers, Box 15 Folder 2.

advice. For instance, he drew upon the advice of plant pathologist Arthur Geoffrey Norman when appointing experts in that subject to his RDB committee.[138] More broadly Baldwin maintained a reciprocal flow of information and opinion with a number of friendly correspondents at Detrick, generally but not exclusively high-ranking scientific administrators like Woolpert, his successor Leroy Fothergill, and Norman. During the 1952 crisis, for instance, his dire warnings about researchers' morale at Detrick drew heavily upon this correspondence. As discussed above, these warnings, though probably not disingenuous, were also politically charged, used by Baldwin and his allies to strong-arm General Bullene's Chemical Corps into granting Detrick greater autonomy. The ability to enlist non-governmental correspondents like Baldwin to act as a friendly lobbying force was most certainly not lost on Detrick's leadership, with Fothergill once responding to rumors of budget cuts by sending Baldwin "a couple of recent newspaper clippings which describe a situation of considerable concern to us," (the rumored cuts), thinking "that if you were informed by newspaper, then no one could be accused of going 'out of channels.'"[139] For both Detrick leaders like Fothergill and advisors like Baldwin, the ability to coordinate outside official 'channels' was an invaluable informational and political resource. For all their prevalence, such informal flows of information had to be handled discreetly, with Baldwin once going so far as to suggest that it would "be wise for you to destroy this letter and copy of the suggestions I sent [regarding an upcoming Advisory Council report] so that they do not appear in your files."[140] Even recruitment

[138] Baldwin suggested "asking Norman for another suggestion" to replace one of the RDB's plant pathologist members in 1951 for instance. See Baldwin to H. I. Cole, June 12, 1951, in UWA Baldwin Papers, Box 16 Folder 1. See also Cole to Baldwin June 7 1951 in Ibid.
[139] Fothergill to Baldwin, April 24, 1947, in UWA Baldwin Papers, Box 15 Folder 1.
[140] Baldwin to Fothergill, June 1, 1953, in UWA Baldwin Papers, Box 12 Folder 1. Later that same letter, Baldwin suggested "on second thought" that Fothergill take advantage of the secrecy system and simply return the offending (and secret) documents to him.

could draw upon this bypassing of ‘channels,’ as in 1949 when Baldwin helped Woolpert identify a candidate for a planned staff editor position despite an official hiring freeze precluding a formal job search.[141] Baldwin was a particularly good contact for channeling people to Detrick, as his identification with bioweapons research within his broader professional networks made him a kind of informal forwarding address. During the Korean War, he was contacted by a number of industrial scientists interested in collaborating with Detrick, particularly during the sense of national emergency (or, more cynically, of explosively growing defense budgets) pervading during the fall and winter of 1950-1951.[142] His reputation apparently extended outside of bacteriological circles: for instance, in 1949 Baldwin handled an inquiry from an Ethyl Corporation representative interested in Detrick’s herbicide research, putting him in contact with Detrick plant physiologist Arthur Geoffrey Norman.[143] Even virologist Richard Shope, who had worked on rinderpest as a biological weapon during the war and who knew Woolpert, was uncertain whether Baldwin or Woolpert was the appropriate person to contact about prospective recruits, and opted for the former.[144]

The network of correspondence between Detrick researchers and academic colleagues was more than a marriage of political convenience, however. Baldwin’s relationships with some of his Detrick correspondents were close-knit and antedated Detrick itself. Their discussions of the situation at Detrick and tacit appeals for political

[141] Woolpert to Baldwin, September 1, 1949, in UWA Baldwin Papers, Box 15 Folder 4.
[142] See e.g. probes from Lederle Laboratories and the Midwest Research Institute in T. H. Marshall to I. L. Baldwin, September 6, 1950 and M. H. Thornton to I. R. Baldwin [sic], October 17, 1950, both in UWA Baldwin Papers, Box 11 Folder 10.
[143] A. G. Norman to Lawrence A. Monroe, April 21, 1949, and Norman to Baldwin, April 21, 1949, both in UWA Baldwin Papers, Box 15 Folder 4.
[144] See Richard Shope to Baldwin, January 25, 1951, in UWA Baldwin Papers, Box 11 Folder 10.

aid were amidst chatty letters discussing gossip and their families. Indeed, when Baldwin's Detrick correspondences began to consider resignation during the 1952 crisis, he listened sympathetically to their frustrations, counseled them about finding new jobs (generally in academic administration), and recommended them to potential employers.[145] For Detrick scientists like Woolpert and Fothergill, corresponding with Baldwin in much the same way they had as university scientists was an embodiment of their ongoing ties with the academic community. This was further demonstrated by the relative ease with which they transitioned back into university jobs with his help, with Woolpert, for instance, taking a job managing the Ohio State University Research Foundation.[146] With a number of Baldwin's correspondents leaving Detrick that year, his interpersonal ties to the Camp (soon to be Fort) were weakened, but they were never completely abated. Baldwin particularly cultivated a friendly relationship with Riley Housewright, a medical microbiologist who had come to Detrick as a young researcher during the war and succeeded Fothergill as Scientific Director in 1956.[147]

Beyond maintaining community ties and exchanging information and favors with Detrick colleagues, Baldwin also sent them promising students. He was particularly active in lobbying his military contacts on behalf of Wisconsin graduate students inducted by the draft, seeking to divert them from general military duties into work at Detrick, which would allow them to use their military service to advance their scientific career, as well as avoiding the dangers of deployment abroad.[148] This lobbying

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[145] See e.g. I. L. Baldwin to Keith H. Lewis, January 7, 1952, and I. L. Baldwin to A. G. Norman, March 20, 1952, both in UWA Baldwin Papers, Box 11 Folder 10.
[146] Oram Woolpert to Ira L. Baldwin, January 30, 1952, in UWA Baldwin Papers, Box 11 Folder 10.
[147] I. L. Baldwin to Riley D. Housewright, December 20, 1956 in UWA Baldwin Papers, Box 12 Folder 4.
[148] See e.g. Baldwin to Edward Wetter, June 29, 1951, in Baldwin Papers Box 16 Folder 1. Baldwin interceded with Wetter, then-chairman of the RDB, on behalf of James Halpin, a University of Wisconsin

particularly accelerated with the outbreak of the Korean War, and he even began preemptively warning his students that "if you find that you are called up for active service, let me know and I will do my best to get you assigned to the program at Frederick."[149] Such influence-peddling on behalf of his students served a dual purpose. On the one hand, trying to direct drafted students to relatively safe and professionally rewarding positions at Detrick can be seen as a kind of professional service to the scientific community as a whole; a slightly extreme example of the reciprocal teacher-student exchanges upon which a healthy scholarly community is built. Directing talented students to Detrick, however, also served Detrick (which was always starved for competent personnel). Young draftee-researchers could be an immediately valuable resource, but perhaps more importantly could serve as a long-term pool of allies to supplement Baldwin's wartime generation. Woolpert was extremely enthusiastic about shoring up these *ad hoc* links with the universities, suggesting during the Korean War that Baldwin help direct more Wisconsin students to Detrick and that "we might use the same mechanism at several other institutions through the kindness of our other friends, such as Walt Nungester at Ann Arbor and N. Paul Hudson at Columbus." Drawing on these interpersonal networks of 'friends,' "it might be possible in time to acquire a very appreciable stockpile of valuable manpower. Most of these young men… will, of course, be faced with the problem of call to military duty in one way or another, so that they would probably welcome an opportunity for assignment to institutions where their

agronomy student assigned to general corpsman duties in the Army. Beyond being more professionally rewarding for Halpin, research at Detrick, particularly in plant pathogens, would doubtless have been safer than deployment to Korea.

[149] Baldwin to Koffler, February 6, 1951, in UWA Baldwin Papers, Box 16 Folder 1.

technical training could be put to good use."[150] The draft helped push young personnel into other government biomedical organizations of the period as well: Langmuir's Epidemic Intelligence Service, for instance, offered young doctors an alternative to military service.[151] In the case of Detrick, however, this push was accentuated by "friends" of the program like Baldwin who served as tacit recruiters.

While apparently successful in the short term at keeping Detrick staffed, this informal recruitment system seems to have failed to accomplish its arguably more important task of incorporating members of the younger generation of microbiologists into the academic community of Detrick's "friends." Even a few years from WWII, the central event in the identity of this community, the need to draw upon colleagues too young to have had this formative experience was evident to Baldwin's group. For any number of possible reasons, however, attempts of this nature were by and large a failure. A large number of Detrick researchers spent a few years there (see analysis in Chapter 4, below), before returning to the civilian world, but few seem to have joined Baldwin's group of advisor/'friends.' Baldwin's Detrick-related correspondence retains much the same cast of characters in the 1960s as in the late 1940s, albeit diminished by job changes, retirement, and death. By the time Baldwin retired from military advising in 1969, even younger members of his community (like Langmuir) were nearing their own retirements.[152] Bilateral links between Detrick researchers and younger scientists than

[150] Woolpert to Baldwin, January 17, 1951, in UWA Baldwin Papers, Box 14 Folder 1 "Placement-Students in CWS, 1946-1954."
[151] On the EIS, see Mark Pendergrast, *Inside the Outbreaks: The Elite Medical Detectives of the Epidemic Intelligence Service*, New York: Houghton Mifflin Harcourt, 2010. On the draft more generally, see George Q. Flynn, *The Draft, 1940–1973*, Lawrence, KS: Kansas University Press, 1993.
[152] Langmuir retired from his position as director of the EIS in 1970. See "Alexander D. Langmuir- A Brief Biographical Sketch: With Emphasis on His Professional Activities," *American Journal of Epidemiology* 144 no 8 (Issue Supplement), 1996, pp S1–S10.

Baldwin did, to be sure, thrive in this period (Joshua Lederberg, who had been a precocious 16-year-old freshman when Pearl Harbor was bombed, maintained a friendly collaboration with some Detrick bench scientists in the early 1960s, for instance).[153] Such continued links with the academic scientific community, however, did not bring in much new blood for the community of advisors who acted as an informal Detrick lobby with the military. This growing and essentially generational political isolation of Detrick was an obvious weakness by the late 1960s, and was accentuated by circumstances. When Vietnam-era protest against institutional ties with Detrick rose within the American Society for Microbiology (former SAB) in 1968, and when a 1969 Nixon administration review of bioweapons policy found a tepid defense from even the Army, the failure of Baldwin's group to interest younger members contributed to their political impotence (see Chapter 2).

The Chemical Corps

Another major element of Baldwin's social world was an alliance with the Army's Chemical Corps. The Corps had been the institutional home of Detrick since the then-Chemical Warfare Service had taken control of biological weapons research from the civilian War Research Service in 1944.[154] In an immediate (and particularly budgetary) sense, then, what was good or bad for the Corps was good or bad for Detrick, meaning that whenever Baldwin's group argued for higher budgets for Detrick, they were

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[153] Lederberg drew upon his preexisting contacts with Detrick researchers to enlist their expertise for space probe sterilization in the early 1960s, for instance. See e.g. Joshua Lederberg to Riley Housewright, July 23, 1959 in Joshua Lederberg Papers, National Library of Medicine (NLM) Profiles in Science (NLM ID: 101584906X6189). See also, Audra J. Wolfe, "Germs in Space: Joshua Lederberg, Exobiology, and the Public Imagination," *Isis* 93 no 2 (2002), pp 183-205.
[154] Rexmond C. Cochrane, *History of the Chemical Warfare Service in World War II, Volume 2: Biological Warfare Research in the United States*, Edgewood Arsenal: Historical Section, Office of the Chief, Chemical Corps, 1947, pp 27-29.

tacitly arguing for the Corps' bottom line as a whole. The subordination to biological weapons research to the Corps, however, was not a necessary or even obvious relationship, and if Detrick had changed hands before, it certainly could again. I say that Baldwin had an alliance with the Corps not merely for the obvious reason that in supporting Detrick, he was supporting a part of the organization, but because he sought to keep Detrick there. This is not to say that the alliance could not be fraught: as the 1952 crisis demonstrated Detrick's interests and those of the Corps could diverge for Baldwin, while members of his community of advisors who were less enamored with the Corps occasionally needed to be convinced to support the alliance. What is noteworthy about all of the features of the alliance, however, was how stable it was in the face of major changes in Baldwin's titular position. As chairman of Committee X, Baldwin in some sense sat in judgment of the Corps, while after 1953, his ostensible position was that of advisor to the Corps, serving at the pleasure of its commander. Practically speaking, however, his relationship with the Corps' leadership remained on a more equal footing than this formal position would suggest.

This relationship revolved around three major exchanges: Baldwin served as a conduit to the scientists at Detrick and the larger scientific community, he and his compatriots served as informal sources for information and communication outside "channels," and they occasionally acted as a political constituency supporting the Corps in its bureaucratic struggles for control and resources. In return for all of these services, Baldwin received access, serving for practical purposes as a permanent fixture of the Chemical Corps decision making process after 1953. Baldwin's role in mediating between the Detrick community and Corps leadership was evidenced by the 1952 crisis (see above), in which even Baldwin's sympathetic relationship with the Detrick community offered the Corps information about how far they could really be pushed. Baldwin would even occasionally take the side of the Corps against Detrick when he felt it would benefit the BW program as a whole, such as during a Corps-Detrick controversy over whether to pursue variola research in the early 1960s.[155] More broadly, Baldwin and other civilian advisors were a useful conduit to the broader scientific community. From its earliest days as the Chemical Warfare Service, the Corps had occupied a politically marginal place within the Army, and had relied on a close alliance with the chemistry community for political support. In the early 1920s, for instance, it had been the American Chemical Society, not a largely-hostile Army leadership, who had supported the CWS during Congressional deliberations over whether to abolish the organization.[156] As much as specific technical "advice," the function of institutions like the Advisory Council was to maintain this relationship with prominent chemists, and with the incorporation of pro-BW biologists like Baldwin, the Corps sought to build and maintain a similar relationship with this new constituency of microbiologists. Indeed, much like the ACS, the SAB willingly sponsored an "Advisory Committee" of members, directly tying the organization to research at Detrick (see Chapter 2, below). Chemists (and now microbiologists) were also more directly a constituency because researchers had to be recruited from within their ranks. The fact that someone like Baldwin served as a conduit between the military and microbiological social worlds also therefore gave the Corps the

[155] See C. B. Marquand to Baldwin, January 10, 1961 and Baldwin to Charles L. Wisseman, January 19, 1961, both in UWA Baldwin Papers, Box 13 Folder 3 "US Army Chemical Corps 1961." The variola virus is commonly known as smallpox, a highly contagious disease which was typically dismissed as a biological weapon in the early Cold War due to the prevalence of vaccination on both sides of the Iron Curtain. It is unclear for this reason why Detrick researchers were flirting with studying the virus in the early 1960s.
[156] See Thomas I. Faith, *Behind the Gas Mask: The U.S. Chemical Warfare Service in War and Peace*, Urbana: University of Illinois Press, 2014, pp 56-76.

opportunity to spread pro-Detrick messages among possible recruits. A young researcher contemplating a scientific career at Detrick might (rightly) be leery of the idea. Beyond the omnipresent factor that some of their work would probably be secret and thus unpublishable, Detrick's budgets, particularly in the late 1940s and late 1950s, were low and inconsistent, with research projects continually disrupted by what RDB chairman Karl Compton called budgetary "fire drills."[157] Corps leaders thus viewed communicating any successes the Corps had in increasing its budgets or supporting Detrick projects as an important part of recruiting new researchers. When Chief Chemical Officer Alden Waitt wrote to Baldwin about the successful resolution of a personnel-cut scare in 1947, for example, he made a point to emphasize that "it is quite important… that our scientists who would have been affected understand that the Corps has stood behind them strongly and has battled effectively for them" particularly because "many of them were worried about the security of their positions."[158] Waitt, later remembered in Baldwin's circle as a "political general" more concerned with politicking than managing the details of R&D programs, "fe[lt] it is desirable to have it known by our friends outside the Service that the War Department has kept faith with us."[159] The implication was clear that Baldwin should spread this message throughout his scientific networks, where it would hopefully assuage the fears of Detrick researchers and those "outside the Service" who might be contemplating joining them. Baldwin was happy to spread such positive messages (which sometimes represented his own political victories, as in a late 1950s controversy over the

[157] Karl T. Compton to Ira L. Baldwin, November 4, 1949 in UWA Baldwin Papers, Box 15 Folder 4.
[158] Waitt to Baldwin, May 19, 1947, in UWA Baldwin Papers, Box 15 Folder 1.
[159] Ibid. On "the late, unlamented Alden Waite" as a "political general," see Carnahan to Baldwin, April 14, 1954 in Baldwin Box 12 Folder 1.

federal pay grade of bacteriologists), as someone who perennially "would like to see us interest more bacteriologists in the work of the Chemical Corps."[160]

Much like the Baldwin-Detrick correspondence, his relationship with the Corps also allowed both parties access to information outside of formalized "channels." Baldwin maintained an active interest in the internal politics of the military, paying particular attention to policies that might affect Detrick. The benefits of this exchange worked both ways. The publicity-conscious Corps, for instance, was disallowed by Pentagon regulations from subscribing to a commercial newsclipping service, and relied instead on exhorting its network of scientist-advisors to send in any press mentions of the Corps or chemical and biological warfare that they saw.[161] When a Congressman from Baldwin's home state, Robert Kastenmeier, openly questioned the Corps' secrecy and research in 1959, this network went into crisis mode, with Baldwin providing detailed analysis and advice for how to manage Kastenmeier to Chief Chemical Officer Stubbs.[162] Baldwin's aid during "the Kastenmeier situation" (as Stubbs put it) exemplifies the political dimension of the alliance.[163] While outright Congressional attention was a rare enough thing, the Corps often found itself embroiled in the bureaucratic politics of securing responsibilities and funding, and the politics of expertise embodied in reports by bodies like the JRDB, WSEG, and PSAC on the utility of biological warfare. Baldwin

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[160] On SAB lobbying over federal pay, see e.g. ASM 13-IIAT (Baldwin Presidential Papers), Folder 42.2. Quote from Baldwin to Carl P. Marquand, February 19, 1959, in UWA Baldwin Papers, Box 12 Folder 5.

[161] See e.g. The Secretariat of the US Army Chemical Corps Advisory Council, *Committee (ACS and SAB) Advisory to the Chemical Corps Newsletter* 100, June 20, 1961, p 25. Copies of the 1958-1962 run of this newsletter are held at the National Library of Medicine.

[162] See e.g. Baldwin to Major General Marshall Stubbs, January 22, 1960, and Baldwin to Stubbs, January 28, 1960, in UWA, Baldwin Papers, Box 12 Folder 6.

[163] By February 1960, Stubbs reported to Baldwin that "the Kastenmeier situation has been brought under control." See Stubbs to Baldwin, February 5, 1960, in UWA Baldwin Papers, Box 12 Folder 6.

could be a useful ally in both brands of politics. As chairman of Committee X, he had been in an obvious official position to deliver reports favorable to the Corps, but even informally, he aided the Corps in its conflicts with bureaucratic rivals like the Army Medical Corps. This institution had been traditionally skeptical of the concept of biological warfare, and early in WWII supporters of the "BW" ideal within the Corps like Colonel James Simmons had worked outside of it to bring the issue to the attention of the NAS.[164] In a postwar world of permanent research at Detrick, however, the Medical Corps became interested in a bureaucratic land grab from the CWS, arguing that biological defense research like vaccine development fit better under its preview (all of Detrick's research was, after all, more closely aligned to medicine than to the chemical engineering that constituted most CWS research). Needless to say, the CWS (which after 1946 achieved its longstanding goal of being promoted to a full Army technical Corps like the Medical Corps), resisted such a chipping away at its responsibilities and budgets, prompting a tense series of meetings by the two groups in the fall of 1947.[165] Baldwin, a civilian outside of the official command structure, busied himself to act as an intermediary and peace-maker between the two groups, but betrayed his allegiance by consulting closely with General Alan Waitt, chief of the new Chemical Corps, throughout the process.[166] Both officially as Chairman of Committee X and unofficially as someone

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[164] See e.g. "Conference on Biological Warfare," August 20, 1941 in NAS BW Box 1 Folder 3 ("Beginning of Program: 1941").
[165] Baldwin outlined the history of this controversy (including "at least three meetings between Generals Bliss [of the Medical Corps] and Waitt" and "the desire of Dr. Maxcy and myself to see whether there was any possibility of aiding in the solution of the problem on an unofficial basis") in a letter to fellow Committee X member Raymond A. Kelser. See Baldwin to Kelser, October 28, 1948, in UWA Baldwin Papers, Box 15 Folder 2 "General Correspondence Research and Development Board 1948"
[166] See e.g. Baldwin to R. A. Kelser, November 6, 1948; Waitt to Baldwin and Maxcy, October 29, 1948; Baldwin to Waitt, November 6, 1948; Waitt to Baldwin, November 9, 1948, all in UWA Baldwin Papers, Box 15 Folder 2 "General Correspondence Research and Development Board 1948"

outside of "official channels," Baldwin tacitly supported the Chemical Corps' claim to unified control of Detrick and its research. Whatever influence he had contributed to the Chemical Corps' bureaucratic victory, which was so complete that the next time the Medical Corps involved itself with BW research in the early 1950s, it was as an ally of the Chemical Corps.[167] He continued this informal aid after joining the Advisory Council in 1953, as the "Kastenmeier situation" demonstrated. He also advanced a pro-BW position in the politics of expertise, which by supporting Detrick also served the Corps' budgetary interests. Most directly, Baldwin was an important "on-tap" expert for scientific advisory groups elsewhere in the government, serving, for instance, on a special committee on biological warfare of the new Presidential Science Advisory Committee (PSAC) in 1959, service which contributed to the committee's favorable report.[168] A few years before, when "a proposed contract for review of the Camp Detrick program" was proffered by the Chemical Corps in 1954, the Corps unofficially encouraged Baldwin to accept a consultant position with the contractor. A consultant like Baldwin of the "highest caliber" was "a 'sine qua non' if the contractor [was] to do a satisfactory job for the Corps," but the obvious fact that a long-established ally of Detrick like Baldwin would produce a friendly review cannot have been lost on anyone.[169] Baldwin occasionally questioned the appropriateness of such arrangements, but tended to defer to the opinion of his Chemical Corps sponsors, who in turn took a very lax view of potential conflicts of

[167] This was the agreement to collaborate on Project Whitecoat- the deliberate infecting of human volunteers. This program was run using Chemical Corps facilities at Detrick as a Medical Corps program under Medical Corps Colonel William Tigertt from the mid-1950s to the early 1970s.
[168] See correspondence in UWA Baldwin papers, Box 14 Folder 4 "BW Committee- President's Scientific Advisory Board 1959" [sic], especially Baldwin to Joseph P. Mares, February 25, 1959 (with attached "Memorandum- Report of Panel"), in Ibid. On the history of PSAC, see Wang, *In Sputnik's Shadow*.
[169] H. I. Stubblefield to Baldwin, June 22, 1954, in UWA Baldwin Papers, Box 12 Folder 1 "BW- Chemical Corps Advisory Council General Correspondence 1953-1954."

interest.[170] Influencing who was consulted even if it was not him was also an option. For instance, after Johns Hopkins University set up a semi-independent Operations Research Office under contract with the Army in 1948, intended to provide operations research-based advice on weapons akin to that offered by the Pentagon-wide Weapons Systems Evaluation Group (WSEG), Baldwin was approached a few years later by its new director for possible experts to evaluate biological warfare within the office.[171] Baldwin then drew upon his interpersonal networks to hand-pick typhus researcher Clara Nigg, a former student and close friend and confidant of the University of Kansas' Cora Downs, for the role.[172] (Downs had served at Detrick during WWII, did extensive tularemia research under Army contract during the war and into the 1960s, and was a generally sympathetic member of the network of Detrick's 'friends').[173] By thus installing an expert inclined to be friendly to the BW network's views, Baldwin was able to forestall a threat to the epistemic authority of his committee from the Operations Research Office, which does not seem to have produced any subsequent research on BW challenging RDB views.

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[170] See e.g. Baldwin to Creasy, June 11, 1954, in UWA Baldwin Papers, Box 12 Folder 1 "BW- Chemical Corps Advisory Council General Correspondence 1953-1954."

[171] See George Shortley to I. L. Baldwin, October 24, 1951 in UWA Baldwin Papers, Box 16 Folder 1. The founding of the ORO is discussed in Ron Theodore Robin, *The Making of the Cold War Enemy: Culture and Politics in the Military-Intellectual Complex*, Princeton: Princeton University Press, 2001, pp 51-52.

[172] Cora M. Downs to I. L. Baldwin, October 22, 1951; I. L. Baldwin to Clara Nigg, October 30, 1951; and Clara Nigg to I. L. Baldwin, January 17, 1952, all in UWA Baldwin Papers, Box 16 Folder 1.

[173] See "Interview with Cora Downs," transcript of an interview by Phyllis Lewin, *Oral History Project of the K.U. Retirees' Club*, 1984, available online at https://digital.lib.ku.edu/ku-endacott/135, pp 14-21. Downs was a noted tularemia researcher before the war; examples of her subsequent military-sponsored tularemia work include C. M. Downs, L. L. Coriell, et al. "Studies on Tularemia. I. The Comparative Susceptibility of various Laboratory Animals," *Journal of Immunology* 56 no 3 (1947), pp 217-228; Cora M. Downs, "Studies on the Pathogenesis and Immunity of Tularemia: Progress Report of Work Done under Navy Contract N6-ONR-26007 from January 1,1953 to June 30,1953," (DTIC #: AD0015528, Retrieved November 12, 2019); Henry T. Eigelsbach and Cora M. Downs, "Prophylactic Effectiveness of Live and Killed Tularemia Vaccines I. Production of Vaccine and Evaluation in the White Mouse and Guinea Pig," *Journal of Immunology* 87 no 4 (1961), pp 415-425.

This was an extremely valuable service for the Chemical Corps, as the hostility of the WSEG in the 1950s attested. Unlike with the ORO, Baldwin's network seems to have had no hand in recruiting experts for the WSEG, which produced a series of reports throughout the 1950s critical of specific Chemical Corps projects and the idea of biological warfare in general.[174] Corps leadership was thus forced to devote substantial political capital to blunting the impact of these reports, faced with the same dilemma of lacking data worthy of "an engineer's handbook" that Walter Nungester had bemoaned a decade earlier.[175] As Chemical Corps Colonel John Hayes put it in 1956, "an impasse has resulted," from WSEG-Chemical Corps negotiations, "which must be gotten around in some manner. It must be demonstrated to the satisfaction of biologists and non-biologists alike, that calculated human dosage values have validity and can be used. The results of a Hiroshima or Nagasaki have not been available to the BW program."[176] Corps leadership thus had great cause to appreciate that similar criticism, and a similar uphill battle in constructing their data as reliable, was not coming from bodies where Baldwin had more influence, like the ORO or PSAC.

Despite this zealous defense of a Chemical Corps program, we should not forget that Baldwin's true goals were the American pursuit of bioweapons research, preferably

[174] These included R-8 "Offensive Biological Warfare Weapons Systems Employing Manned Aircraft" (1952), R-14 "The Status of Biological Warfare Weapons Systems" (1955), and R-31 "Reappraisal of Biological Warfare (1958). See lists of report titles in Ponturo, "Analytical Support for the Joint Chiefs of Staff," pp 101, 170. According to former WSEG analyst Paul Johnstone, these reports (and several others on chemical warfare) were generally uncomplimentary of the Chemical Corps and its 'products,' with the 1958 report in particular focusing on the difficulties of controlling biological weapons and the dangers they posed to friendly troops. See Paul H. Johnstone, *From MAD to Madness: Inside Pentagon Nuclear War Planning*, Atlanta: Clarity Press, 2017, pp 131-133.

[175] Walter J. Nungester to Baldwin, November 9, 1949, UWA Baldwin Papers, Box 15 Folder 4.

[176] Col. John J Hayes, "Status of the BW Program," April 23, 1956, p 6, retrieved from Brill Online, "Primary Source Collection: Weapons of Mass Destruction," https://primarysources.brillonline.com/browse/weapons-of-mass-destruction.

located at Detrick, which only incidentally coincided with the interests of the Corps. Indeed, significant tensions existed between the two which could have driven the alliance apart. Most of these tensions ultimately revolved around the uneasy meeting of cultures between microbiologists and chemists, and between civilian academics and military hierarchies. An able politician, Baldwin did not directly challenge the value of gas to his erstwhile Chemical Corps allies who had invested their careers in it, but he seemed to regard chemical weapons as being of minor value compared with what was being done at Detrick. In this way, though he and the Chemical Corps were embattled allies in high-level budgetary fights, both facing the indifference or outright hostility of many military officials, their interests nonetheless fundamentally clashed within the socially constructed category of "CEBAR" weapons. Political scientist Frank Smith III has argued that the "chem-centric" Chemical Corps tended to treat Detrick as an afterthought and biological aerosols as yet another gas, using the same research strategies, production techniques, and visions of tactical employment, which certainly helps to explain why Detrick's biologists guarded their prerogatives so jealously.[177] More fundamentally, whatever the attitudes of Corps leadership, their professional backgrounds made them suspect managers of biology research for many of Baldwin's cohort. Baldwin put it bluntly in 1952, "relatively few of the officers in the Chemical Corps have had any adequate background in the biological phases of the subject."[178] Even Corps officers with civilian graduate degrees (relatively more common in the insular and technically-oriented Corps than the Army as a whole) tended to be trained as chemists and chemical engineers.

[177] Frank L. Smith III, *American Biodefense: How Dangerous Ideas about Biological Weapons Shape National Security*, Ithaca: Cornell University Press, 2014.
[178] Baldwin to Harold V. Gaskill, April 7, 1952, in UWA Baldwin Papers, Box 14 Folder 8.

While thus being able to effectively control what Corps leaders knew about biology gave Baldwin a certain kind of power (as one incoming Chief Chemical Officer acknowledged to Baldwin, "the sum of my present knowledge in the BW field… stems from conferences with you and from the splendid report which you and your committee submitted"), it is easy to see why Baldwin did not want Detrick's biologists being too closely managed by such people.[179] He was particularly candid about these concerns in a 1952 "personal and confidential" letter to Army Chief Scientist Harold Gaskill during the early stages of the crisis over management of Detrick. Even someone like General William Creasy (who had served as director of research and development at Edgewood Arsenal, the chemical counterpart to Detrick) and who "considering his lack of background, has arrived at a very fair understanding of many of the technical biological problems… has convinced himself that he has more technical knowledge of the biological field than he really has. As a result he many times acts on the basis of his own judgment of technical situations rather than seeking and accepting the advice of specialists in the biological field." Creasy, furthermore, at least had a research background (for all that "many of [his] administrative actions… have served to destroy the confidence of the research workers in him"). Worse still was then-Chief Chemical Officer Egbert Bullene, who "has very little knowledge of or interest in the research phases of the BW program," and who "stated frankly to me on one occasion that his interest was primarily with troop activities and that he felt all of the officers of the Corps should be oriented in the same direction." Needless to say, Baldwin concluded that "such

[179] Quote from Major General A. C. McAuliffe to Baldwin, October 25, 1949, in UWA Baldwin Papers, Box 15 Folder 4. McAuliffe (best known for his Battle of the Bulge defense of Bastogne) was an anomalous Chief Chemical Officer for coming from outside of the Corps, but none of the other Chief Chemical Officers in this period had any more background in biology than he did.

an attitude is not conducive to the development of a strong research and development program."[180] Baldwin's solution to these concerns was to keep the management of Detrick at arms' length from that of the Corps as a whole.

Not everyone with his community of scientific "friends" of Detrick agreed, however, with more-radical members arguing that they should seek to completely sever the relationship between Detrick and the Corps. Ever-opinionated Walter Nungester was an exemplar of this position, viewing the Army in general and the Chemical Corps in particular as hopelessly inept managers of a biological research program, and privately arguing in 1949 for Committee X to recommend that a civilian bioweapons research organization, akin to the recently-organized Atomic Energy Commission, take custody of the program at Detrick.[181] Though an otherwise impassioned Cold Warrior, Nungester's political conservatism also made him generally suspicious of the military, leading him to argue in early 1948 that "a council of 'elder statesmen' should be set up to serve as a check on all new methods of warfare by our nation, including atomic bombs, biological warfare, gas warfare, flame throwers, area bombing, etc."[182] Baldwin demurred from these radical positions, arguing that while "there would be some very real advantages in being set up as a quasi-independent agency, such as the Atomic Energy Commission… the disadvantages would outweigh" them.[183] More inclined to working within established

[180] Baldwin to Harold V. Gaskill ("Personal and Confidential"), April 7, 1952, in UWA Baldwin Papers, Box 14 Folder 8.
[181] W. J. Nungester to Ira Baldwin, November 9, 1949, in UWA Baldwin Papers, Box 15 Folder 4.
[182] Walter J. Nungester, "A Proposed Control on Use of New Weapons," p 2, n.d. (attached to Nungester to Baldwin, March 23, 1948, and written "several months ago") in UW Baldwin Papers Box 15 Folder 2. Nungester generally seemed attracted to the isolationist wing of the Republican Party in this period, for instance lauding a December 1950 Herbert Hoover speech which advocated American withdrawal from the Korean War as a lost cause. See W. J. Nungester to Senator Robert A. Taft, December 21, 1950 in UWA Baldwin Papers, Box 16 Folder 1.
[183] I. L. Baldwin to W. J. Nungester, November 21, 1949, in UWA Baldwin Papers, Box 15 Folder 4.

relationships and institutions than to constructing new ones, Baldwin pointed to the small victories the RDB committee had gained, including an increase in Detrick's budget greater in percentage than military R&D budgets in general had experienced, in arguing against taking Detrick into the bureaucratic wilderness.[184] Perhaps more important, if unstated, was the legally-mandated secrecy which the AEC evoked. Baldwin elsewhere deplored how atomic publicity "has brought on the Atomic Energy Commission and on physicists everywhere a very unfortunate situation with respect to security," using this example as a principal argument for avoiding public "sensationalism" about biological warfare.[185] In the political climate of the late 1940s, microbiology needed to avoid attracting the sort of attention that would be entailed by creating an AEC-like organization. Ultimately, Nungester, who professed a great "faith in [Baldwin's] judgment in matters both great or small," deferred to his viewpoint and confined himself to complaining privately for the next two decades, but this episode highlights that Baldwin's work in maintaining the Chemical Corps alliance entailed managing differences of opinion within his own community as much as their relations with the Corps.[186]

The antagonism felt by scientists like Nungester could flow both ways, with military officers like Chemical Corps Colonel Donald Hale decrying "the role assumed by some of the advisory committees…" which "with very little understanding of the entire problem, make far-reaching recommendations" in the early 1960s. "Oftentimes,"

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[184] Ibid. The increase Baldwin mentioned, from "around two million dollars a year" in late 1946, had indeed more than doubled Detrick's budget- but as Nungester noted, this several million dollars was part of a national military budget of $15 billion.
[185] Ibid.
[186] W. J. Nungester to Ira Baldwin, November 9, 1949, in UWA Baldwin Papers, Box 15 Folder 4.

Hale argued, "they allow themselves to be used as sounding boards by our people in rather minor positions who have axes to grind."[187] Civilian advisors could disrupt tidy relationships of military hierarchy, and lend authority to otherwise-dismissible subordinates' arguments, through the interpersonal flow of information and opinion along networks of 'advisors' rather than official "channels." More broadly, officers like Hale confronted in microcosm the ambiguity between "on-tap" scientific advisors and "on-top" decision-making power felt throughout the Cold War state. If experts, *qua* experts, were to have any influence in government and military decision-making structures, were they not themselves assuming powerful roles? This was more than a theoretical question related to the role of scientific expertise in democratic society, as the influence of the likes of Edward Teller over bureaucrats, generals, and politicians demonstrated. For the Chemical Corps especially, with its marginal political position, technically-oriented preview, and consequential close alliance with civilian scientists and engineers, the practical power of civilian advisors was a particularly fraught question.

Conclusion

If this alliance was filled with so many tensions and contradictions, why did it endure? Why did Baldwin defend the Chemical Corps so consistently, given how often he clashed with its administration, and how little interest the chemical officers obviously had in microbiological research? The simplest answer is that his alliance with the Corps was a known quantity, upon which much of his political capital in Washington was based. Even as the chairman of Committee X, his informal relationships with the Corps'

[187] Donald Hale to Per Frolich, September 23, 1955, in UWA Baldwin Papers, Box 12 Folder 4.

staff, dating back to WWII were a vital asset for him, and after 1953, his principal access to policymaking circles was through the Corps. He also probably supported the Corps in its power struggle with the Medical Corps in 1947 because victory for the latter would have divided management of the Detrick program rather than transferring it outright. Additionally, it seems that Baldwin and his cohort did not particularly trust the Medical Corps' commitment to either the offensive research that they felt was necessary, or to the validity of the "BW" idea generally.[188] Even more-radical members of the group like Nungester (himself a physician) who advocated removing Detrick from Chemical Corps management never suggested that it be given to the Medical Corps. Baldwin, a diplomatic pragmatist who had a long history of successfully working with his Chemical Corps allies and who quietly opposed such a radical suggestion, thus had little incentive to abandon the alliance. Finally, probably underlying all of Baldwin's conservatism, he had a deep-seated dread of public controversy and "sensationalism" regarding biological warfare, which any attempt to challenge Chemical Corps control over Detrick would risk engendering.[189] The Corps could be convinced (or sometimes, as in 1952, bullied) into giving Detrick a long leash. Why rock the boat?

It is important, however, to reiterate that the alliance was just that- not a patron-client relationship. In theory, Ira Baldwin, civilian university professor, held little

[188] The received military opinion when the National Academy of Sciences began to consider biological warfare research during WWII was that germs were useless as weapons of war, because of already-extant work to prevent natural infection by the Sanitary and Medical Corps of the Army. This was a Medical Corps view, with the major pre-war text dismissing the science-fictional idea of "bacteriological warfare" penned by a prominent Medical Corps officer. See Leon A. Fox, "Bacterial Warfare: The Use of Biologic Agents in Warfare," *Military Surgeon* 72 no 3 (1933), pp 189-207.
[189] See Chapter 4 for a more detailed discussion of this fear. "Sensational" was a pejorative applied to public discussion throughout the BW advisor community. See, for instance, a suggestion to military officials by Baldwin's Wisconsin colleague William Sarles that "someone should look into" a planned *Collier's* article on biological warfare because "it sounds sensational." William B. Sarles to Edward Wetter, December 12, 1952 in UWA Baldwin Papers, Box 16 Folder 2.

institutional power, and after 1953 most of the power he did hold was at the sufferance of the Chemical Corps. In practice, however, he was at the center of a wide-ranging network of Detrick researchers, Pentagon staff, and civilian microbiologists who were the principal constituency supporting the program at Detrick. He was an important figure for the Chemical Corps to enlist to maintain the support of this network. Furthermore, as the former scientific director of Detrick and chairman of Committee X, he was one of the most experienced scientific advisors on biological warfare available to the Advisory Council or to other organizations inside and outside the government (like PSAC and the NAS), thus meaning that he could not reliably be silenced simply by shutting him out of Chemical Corps deliberations, and that he could help influence these organizations to support the Chemical Corps' research if an alliance with him was maintained. The irrelevance of personal animosity in Baldwin's advisory career (such as his strained relationship with General Bullene, who nonetheless first appointed him to the Advisory Council) highlights the structural power he derived from his expertise and connections to the BW network. This network, and Baldwin himself, in turn pursued their own interests, not those of their erstwhile patrons. *They*, not the Chemical Corps officers who ostensibly led the biological warfare program, were that program's most zealous advocates, and they needed to be listened to by the Corps, as the crisis of 1952 demonstrated. The realities of the social world surrounding a well-connected and experienced scientific 'advisor' like Baldwin confounded neat ideals of his contemporaries, that expert advisors should be "on tap, not on top," but it also challenges our own historiographic tendency to see such military-science relationships in hierarchical patron-client terms.

The leading role of microbiologists in supporting biological weapons research meant that their community, as much as the conference rooms of the Pentagon, was a major site for leaders like Baldwin to enlist allies. Besides supporting Detrick researchers in Chemical Corps deliberations, Baldwin and other leaders in the Society of American Bacteriologists sought to keep them tied to the world of civilian microbiology. These leaders guided students to research at Detrick, and lent official SAB support for Detrick at moments like the 1952 crisis. Finally, and most importantly, they sought to discourage opposition to Detrick and its research within their own community. Microbiologists were not unanimous in their support for turning their science to war, and just as pro-bioweapons microbiologists could be among the loudest voices supporting Detrick, those who opposed such research were potentially its most committed opponents. Managing the organization and community of the SAB to discourage such dissent was as much an imperative for those who supported the "biological warfare" sociotechnical imaginary as advancing it within the government. In the next chapter, we will examine how SAB leaders managed their community's relationship with the biological weapons program, and how their attempts to suppress dissent broke down in the late 1960s.

Chapter 2: The Society of American Bacteriologists and the Politics of Military Research

On the evening of May 29, 1953, Cold Spring Harbor Laboratory was celebrating the completion of a long-awaited construction project with its staff, officials from the sponsoring Carnegie Institution, and a keynote speaker, the "grand old man of American science" Vannevar Bush. Bush, an engineer by training who had been a major figure in government science advising in the 1940s, had by the 1950s left government service behind, but retained a variety of administrative roles in business and foundations, including the presidency of the Carnegie Institution itself.[190] On that night in 1953, the engineer spoke admiringly of the biologists' work at Cold Spring Harbor, asserting that with the fantastic scientific vistas being explored there, "if I were a young man that would be the field I would plunge into." He particularly admired how the Cold Spring Harbor staff could conduct their research "with no thought whatever as to the utility of their results. They can do so with no interference whatever on the part of any government or any dictator… guided only by their own instincts as to what is important." "In these days," he continued, "when we fear that all of these efforts may be terminated in a struggle of desperate nations… it is very much worth while to have among us a company which is bearing forward the understanding of many, for no reason whatever except that it is the privilege of man to try to understand."[191]

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[190] G. Pascal Zachary, *Endless Frontier: Vannevar Bush, Engineer of the American Century*, Cambridge, MA: MIT Press, 1997.

[191] Bush is quoted in Cold Spring Harbor's 1953 annual report. See Milislav Demerec, *Annual Report of the Biological Laboratory*, Cold Spring Harbor, NY: Long Island Biological Association, 1953, pp 14-15, retrieved from: http://repository.cshl.edu/id/eprint/36696/

While the audience to whom Bush spoke were doubtless flattered by this paean to the purity of their science, they may not have agreed that their work was completely unaffected by considerations of utility, government priorities, or Cold War anxieties. For the past few years, after all, several of them had done their work under contracts with the Army Chemical Corps' biological warfare research laboratory of Camp Detrick. The most notable of these was Vernon Bryson, a bacterial geneticist whose work had been supported by the Chemical Corps since he had first joined Cold Spring Harbor in 1943, and who would continue to work on such contracts until he left for Rutgers University in 1956.[192] Bryson had begun by studying germicidal aerosols during WWII, a topic of immediate relevance for the biological warfare program, moved on to the mutation of bacteria exposed to nitrogen mustard (a close relative of mustard gas, which the Chemical Corps was considering pairing with biological attacks), and by the 1950s was immersed in the genetics and biochemistry of antibiotic resistance in bacteria, a topic squarely within the burgeoning field of molecular biology, but which was also of great interest to Detrick microbiologists endeavoring to grow deadlier germs.[193] His was not the only Detrick work: his project on antibiotic resistance continued under other researchers after

[192] Milislav Demerec, *Annual Report of the Biological Laboratory*, Cold Spring Harbor, NY: Long Island Biological Association, 1943, pp 18, retrieved from: http://repository.cshl.edu/id/eprint/36625/; Milislav Demerec, *Annual Report of the Biological Laboratory*, Cold Spring Harbor, NY: Long Island Biological Association, 1956, p 9, retrieved from http://repository.cshl.edu/id/eprint/36635/

[193] Milislav Demerec, *Annual Report of the Biological Laboratory*, Cold Spring Harbor, NY: Long Island Biological Association, 1945, p 19, retrieved from http://repository.cshl.edu/id/eprint/36627/; Milislav Demerec, *Annual Report of the Biological Laboratory*, Cold Spring Harbor, NY: Long Island Biological Association, 1947, p 12, retrieved from http://repository.cshl.edu/id/eprint/36628/; Milislav Demerec, *Annual Report of the Biological Laboratory*, Cold Spring Harbor, NY: Long Island Biological Association, 1949, pp 19-22, retrieved from http://repository.cshl.edu/id/eprint/36630/; Demerec, *Annual Report*, 1956, p 9. For a more general discussion of American nitrogen mustard research on human subjects (generally racial minorities) during WWII, see Susan L. Smith, *Toxic Exposures: Mustard Gas and the Health Consequences of World War II in the United States*, New Brunswick: Rutgers University Press, 2017.

he left in 1956, while his colleague E. Carlton MacDowell worked on a Detrick contract to study induced resistance to leukemia in mice.[194]

Read cynically, this intrusion of the military into the "pure" science of Cold Spring Harbor can be seen as a story of a serpent in Eden, "militarizing" the practice and conclusions of the scientific fields affected. Concerns of this nature, particularly evident in the historiography of science in work done on the physical sciences during the 1980s, should be taken seriously, given at the very least the simple magnitude of funding made available by the growth of the Cold War state.[195] Such reductionism, however, obscures other insights to be drawn from cases like Bryson's. His social and scientific world was a complex one. He worked at a quintessential institution of the 'open' biological community, which did not become a mere adjunct of military research even as he and other researchers maintained military contracts. Bryson's contract work itself was classified, and he was unable to publish most of it, but as his transition to non-military research at Rutgers attested, he nonetheless did not find his career confined to the classified world.[196] He swapped ideas with 'open' colleagues at Cold Springs Harbor, and the University of Wisconsin's Joshua Lederberg, but also hosted Detrick researchers in summer symposia on bacterial genetics.[197] Bryson's military contracts made him more of

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[194] Bryson's work (and Chemical Corps contract) were continued by P. D. Skaar and H. Davidson. See Demerec, *Annual Report*, 1956, pp 31-34; E. Carlton MacDowell, *First Quarterly Progress Report of Research Carried Out by Long Island Biological Association for the Biological Department, Chemical Corps, Camp Detrick*, Cold Spring Harbor, NY: Long Island Biological Association, 1952, retrieved from https://repository.cshl.edu/id/eprint/36799.

[195] For leading works advancing this thesis, see Ian Hacking, "Weapons Research and the Form of Scientific Knowledge," *Canadian Journal of Philosophy* Supplementary Vol 12 (1986), pp 237-260; Paul Foreman, "Behind Quantum Electronics: National Security as Basis for Physical Research in the United States, 1940-1960," *Historical Studies in the Physical and Biological Sciences* 18 no 1 (1987), pp 149-229.

[196] See e.g. Vernon Bryson to Joshua Lederberg, November 1, 1950, in Joshua Lederberg Papers, National Library of Medicine (NLM) Profiles in Science, (NLM ID: 101584906X1837).

[197] For an example of correspondence with Lederberg, see Ibid. On the presence of Detrick researchers at Cold Spring Harbor, see e.g. Milislav Demerec, *Annual Report of the Biological Laboratory*, Cold Spring

a curious professional amphibian, with one foot in both the 'open' and 'closed' worlds of biological research, than they made him a marionette of 'militarized' science.

Bryson's experience was not unique; nor was it accidental. Many of his microbiologist colleagues had similar boundary-transcending stories of career paths that took them from classified research to open academic jobs and opportunities to share ideas with colleagues from both sides of the military-civilian divide at the annual meeting of their field's major professional organization, the Society of American Bacteriologists (SAB).[198] This porousness of the barrier between civilian and military microbiology was in turn no accident. A whole generation of SAB leaders (many of whom, like Ira Baldwin, themselves transcended the barrier by serving parallel careers as academic scientists and Pentagon science advisors) actively sought to construct and maintain these links between the SAB members working at and for Detrick and those fully in the 'open' scientific community. Doing so entailed influence-peddling in their government positions, for instance by lobbying for loosened restrictions on publishing for Detrick researchers, but they also sought to shape the governance of the SAB to keep it welcoming to members at Detrick. They sought to suppress ethically-motivated protests against bioweapons research, to professionally reward Detrick scientists despite the secrecy of their work, and eventually, to directly advise this work. There are two major points that we can take from these actions. The first is that we should be wary of thinking

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Harbor, NY: Long Island Biological Association, 1951, p 17, retrieved from http://repository.cshl.edu/id/eprint/36632/. Detrick researchers also commonly joined an advanced summer seminar on bacterial genetics during the 1950s: see e.g. the list of attendees of the 1955 version of this seminar in Demerec, *Annual Report*, 1956, p 50.

[198] Bryson himself joined the SAB in the mid-1940s, first appearing in the organization's 1947 directory (the first published since 1941). See Society of American Bacteriologists, "Directory and Constitution, 1947," Ann Arbor: Society of American Bacteriologists, 1947, in American Society for Microbiology (ASM) Archives, Series 7-IIH ("Membership Directories").

monolithically of “Cold War science.” Bryson’s career at Cold Spring Harbor is as much an example of the meeting of the scientific and military worlds in the early Cold War as solid-state physicists overburdened with military funding. The second is that at least in the case of microbiology, we should not think of scientist-state interactions as those of atomistic individuals. The institution of the SAB, influenced by a particular cadre of leaders, was an important actor in constructing and maintaining those relationships, including supporting the Detrick secrecy system by silencing dissent within the microbiological community. For would-be dissidents, in turn, the SAB was just as much an institution that needed to be changed as a neutral site for protest. Far from being removed from the world of government secrecy that some its members lived under, this private association of ‘open’ scientists was a major actor in maintaining or challenging that world.

Scientific Societies: Neglected Actors

The SAB was founded in 1899 as a formalization of side-meetings by American practitioners of the new science of bacteriology at the annual meetings of the Society of American Naturalists. This founding at the dawn of the 20th century took place at both a historical and historiographic inflection point. As historian Toby Appel has argued, the SAB was one of a proliferation of field-specific professional societies being founded within the American life sciences in the Progressive era, a trend she argues contributed to perceptions of disciplinary disunity.[199] More broadly, the turn of the 20th century was a period of professionalization in American science, with universities cementing their

[199] Toby A. Appel, “Organizing Biology: The American Society of Naturalists and its ‘Affiliated Societies,’ 1883-1923,” in Ronald Rainger, Keith R. Benson, and Jane Maienschein (eds), *The American Development of Biology*, Philadelphia: University of Pennsylvania Press, 1988, pp 87-120.

status as a pedagogical and professional obligatory point of passage for would-be scientists. This was in turn reflected in the changing nature of scientific societies, which became increasingly the domain of professional scientists to the exclusion of non-professional 'amateurs.' This shift, in turn, marks a historiographic contrast, between a small but rich group of studies of American scientific societies in the 18th and 19th centuries, and a comparative dearth of such studies for the 20th century.

Scientific societies have existed in one form or another since the early modern era, and are featured more prominently in the historiography of that period than that of the last two centuries.[200] For scholars of the Scientific Revolution, the 17th century foundation of societies like the *Accademia dei Lincei*, *Académie des sciences* and especially the canonical Royal Society are important events, as these groups offered natural philosophers social institutions alternate to the less-welcoming universities.[201] By the 19th century, as the professional identity of 'scientist' began to cohere and universities grew in importance for scientific careers in Europe, the role of scientific societies began to morph into mediating between the new professional and 'amateur' communities, and between metropole and periphery.[202] A similar trend was evident in mid-19th century

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[200] For a synthetic overview of the topic focused on the 17th-19th centuries, see Denise Phillips, "Academies and Societies," in Bernard Lightman (ed), *The Wiley Blackwell Companion to the History of Science*, Oxford: Wiley Blackwell, 2016, pp 224-237.

[201] See e.g. Martha Ornstein, *The Role of Scientific Societies in the Seventeenth Century*, New York: Arno Press, 1975 (Reprint of 1913 Edition); Roger Hahn, *The Anatomy of a Scientific Institution: The Paris Academy of Sciences, 1666-1803*, Berkeley: University of California Press, 1971; Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life*, Princeton: Princeton University Press, 1985; John Gascoigne, "The Royal Society and the Emergence of Science as an Instrument of State Policy," *The British Journal for the History of Science* 32 no 2 (1999), pp 171-184; Michael Hunter, *The Royal Society and its Fellows 1660-1700: The Morphology of an Early Scientific Institution, 2nd ed.*, London: British Society for the History of Science, 1994. A potential antecedent to this period is discussed in Girolamo Ruscelli, William Eamon and Francoise Paheau, "The *Accademia Segreta* of Girolamo Ruscelli: A Sixteenth-Century Italian Scientific Society," *Isis* 75 no 2 (1984), pp 327-342.

[202] See Jack Morrell and Arnold Thackray, *Gentlemen of Science*: *Early Years of the British Association for the Advancement of Science*, Oxford: Oxford University Press, 1981; Diarmid A. Finnegan, *Natural History Societies and Civic Culture in Victorian Scotland*, Pittsburgh: University of Pittsburgh Press, 2009;

American science, with small regional scientific societies coexisting alongside self-consciously national institutions like the American Association for the Advancement of Science (AAAS), and heterogeneous communities existing within them.[203] As Gilded Age American society began to urbanize and professionalize, subject-specific societies began to arise, with the American Chemical Society being founded in 1874, the American Society of Naturalists in 1883, and the American Physical Society in 1899. By the 20th century, universities and eventually industrial and government-sponsored research laboratories came to serve as the preeminent institutional homes of American scientists. These scientific societies and narrower subject-specific societies like the SAB shifted to ancillary but important roles supporting a professional membership, from publishing journals and sponsoring conferences to serving as advocates for their members' professional interests. This was the principal role of the SAB in the early Cold War.

The 19th century is the principal focus of the historiography of American scientific societies.[204] In part, this reflects the age of much of this historiography, which saw its heyday in the mid-20th century with synthetic studies like Ralph S. Bates'

Jenny Beckman, "Editors, Librarians, and Publication Exchange: The Royal Swedish Academy of Sciences in the Long 19th century," *Centaurus* Special Issue Article (2020); See also studies of scientific societies in the 18th century, e.g. James E. McClellan, *Science Reorganized: Scientific Societies in the Eighteenth Century*, New York: Columbia University Press, 1985; Andrea Rusnock, "Correspondence Networks and the Royal Society, 1700–1750," *The British Journal for the History of Science* 32 no 2 (1999), pp 155-169; Michael Gordon, "The Importance of Being Earnest: The Early St. Petersburg Academy of Sciences," *Isis* 91 (2000), pp 1-31; Margaret C. Jacob and Dorothée Sturkenboom, "A Women's Scientific Society in the West: The Late Eighteenth-Century Assimilation of Science," *Isis* 94 no 2 (2003), pp 217-252.

[203] Sally Gregory Kohlstedt, Michael M. Sokal, and Bruce V. Lewenstein, *The Establishment of Science in America: 150 Years of the American Association for the Advancement of Science*, New Brunswick: Rutgers University Press, 1999; Daniel Goldstein, "Outposts of Science: The Knowledge Trade and the Expansion of Scientific Community in Post–Civil War America." *Isis* 99, no 3 (2008), pp 519-46.

[204] A classic conference proceeding focused on this topic is Alexandra Oleson and Sanborn C Brown (eds), *The Pursuit of Knowledge in the Early American Republic: American Scientific and Learned Societies from Colonial Times to the Civil War*, Baltimore: Johns Hopkins University Press, 1976.

eponymous *Scientific Societies in the United States*, a systematic if brief survey first published in 1945 and revised twice in the 1950s and 1960s.[205] The institutional focus of such studies made them a less comfortable fit with the historiographic trends of latter decades, as scientific historians' turns to material culture, microstudies of individual laboratories, and studies of the social resolution of controversies mirrored a shift from institutional approaches within general American historiography. Those studies of scientific societies which have been produced within the past few decades have tended to coincide with a centennial or sesquicentennial of a 19th-century society. Such studies have focused on questions of identity: that of community members (particularly the status of 'amateurs'), of disciplines, and of the growing profession of 'scientist.' These are particularly appropriate questions for the 19th century, a time of radical transitions in both American society and the nature of 'science' as a social activity. There are fewer studies of scientific societies in the 20th century, in part due to a dearth of similarly ponderous anniversaries, but more broadly to historiographic 'turns' away from institutional history in favor of society, material culture, and the like.

This trend generally holds true in the historiography of the American life sciences, but there are a few exceptions. A particularly notable example is a paper by Toby Appel, published in the late 1980s in *The American Development of Biology*, the first of two *festschrifts* for the centennial of the American Society of Zoologists.[206] Appel

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[205] Ralph S. Bates, *Scientific Societies in the United States, 3rd ed*., Cambridge, MA: MIT Press, 1965. For a contemporaneous review article on the general subject, see Robert E. Schofield, "Histories of Scientific Societies: Needs and Opportunities for Research," *History of Science* 2 no 1 (1963), pp 70-83.

[206] Appel, "Organizing Biology," in Rainger, Benson, and Maienschein (eds), *The American Development of Biology*, pp 87-120. See also Toby A. Appel, "Biological and Medical Societies and the Founding of the American Physiological Society," in Gerald L. Geison (ed), *Physiology in the American Context, 1850–1940*, New York: Springer, 1987, pp 155-176. While not a study of a scientific society *per se*, Appel's *Shaping Biology*, an institutional history of the first two decades of NSF funding for life science research,

covers many of the same themes as historians of other late 19th century scientific societies in her discussion of the American Society of Naturalists (as the ASZ was originally known), but argues that American life scientists in particular were conscious of the balkanization of their disciplinary societies in the early 20th century. For life scientists, Appel argues, the model their inchoate disciplines should live up to was that of the American Chemical Society, a large and politically active organization representing the interests of chemists (and eventually chemical engineers) from disparate fields and workplaces ranging from the lab to industry.[207] As Keith Benson noted in the companion volume, however, neither the re-christened ASZ nor the post-WWII American Institute for Biological Sciences (AIBS) proved capable of living up to the ACS model, leaving disciplinary-level societies as the organizational backbone of 20th century American life sciences.[208] These societies in turn consciously sought to enhance the perceived prestige

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also bears notice as a valuable historiographic anomaly. See Toby A. Appel, *Shaping Biology: The National Science Foundation and American Biological Research, 1945-1975*, Baltimore: Johns Hopkins University Press, 2000.

[207] Appel, "Organizing Biology," in Rainger, Benson, and Maienschein (eds), *The American Development of Biology*, pp 87-88. Reflecting the general neglect of scientific societies in the historiography of American science, even the story of an institution as noteworthy as the ACS is dominated by official and quasi-official histories. Major examples include Charles Albert Browne and Mary E. Weeks, *A History of the American Chemical Society- Seventy-Five Eventful Years*, Washington, DC: American Chemical Society, 1952; Herman Skolnik and Kenneth M. Reese, *A Century of Chemistry: The Role of Chemists and the American Chemical Society*, Washington, DC: American Chemical Society, 1976; Kenneth M. Reese, *The American Chemical Society at 125: A Recent History, 1976-2001*, Washington, DC: American Chemical Society, 2002. The ACS appears as an important actor in some histories of the early 20th century American chemical industry. See e.g. Terry S. Reynolds, "Defining Professional Boundaries: Chemical Engineering in the Early 20th Century," *Technology and Culture* 27 no 4 (1986), pp 694-716; Kathryn Steen, *The American Synthetic Organic Chemicals Industry: War and Politics, 1910-1930*, Chapel Hill: University of North Carolina Press, 2014. The ACS also looms large in the historiography of chemical warfare in the US, having served as an influential lobby in favor of the Chemical Warfare Service in the early 1920s. See e.g. Daniel P. Jones, "American Chemists and the Geneva Protocol," *Isis* 71 no 3 (1980), pp 426-440; Hugh R. Slotten, "Humane Chemistry or Scientific Barbarism? American Responses to World War I Poison Gas, 1915-1930," *The Journal of American History* 72 no 2 (1990), pp 476-498; Thomas I. Faith, *Behind the Gas Mask: The U.S. Chemical Warfare Service in War and Peace*, Urbana: University of Illinois Press, 2014.

[208] Keith R. Benson, "Epilogue: The Development and Expansion of the American Society of Zoologists," in Keith R. Benson, Jane Maienschein, and Ronald Rainger (eds), *The Expansion of American Biology*, New Brunswick: Rutgers University Press, 1991, pp 325-335. See also Keith R. Benson and C. Edward

and legitimacy of their disciplines. For the less-fashionable older disciplines like zoology and botany, society actions like encouraging disciplinary unity were self-conscious attempts to retain waning status.[209] (By the late 20th century, Betty Smocovitis argues, the Botanical Society of America went so far as to seek to incorporate the very amateurs they had been founded to exclude earlier in the century).[210] For younger disciplines like ecology, the trajectory was often the opposite, but the motivations were the same, with the Ecological Society of America seeking to advance the professional prestige and fortunes of both individual members and the field as a whole.[211]

These motivations were particularly pronounced for the young science of bacteriology. Historians of American microbiology have emphasized the importance of institutions like the SAB in stabilizing the professional identity and authority of the nascent science at the end of the 19th century.[212] This was an explicit goal for the SAB

Quinn, "The American Society of Zoologists, 1889–1989: A Century of Integrating the Biological Sciences," *American Zoologist* 30 no 2 (1990), pp 353–396.

[209] See e.g. Kristin Johnson, "The Return of the Phoenix: The 1963 International Congress of Zoology and American Zoologists in the Twentieth Century," *Journal of the History of Biology* 42 no 3 (2009), pp 417-456.

[210] Vassiliki Betty Smocovitis, "One Hundred Years of American Botany: A Short History of the Botanical Society of America," *American Journal of Botany* 93 no 7 (2006), pp 942-952. See also Vassiliki Betty Smocovitis, "The Voice of American Botanists: The Founding and Establishment of the American Journal of Botany, 'American Botany,' and the Great War (1906-1935)," *American Journal of Botany* 101 no 3 (2014), pp 389-397.

[211] See Sara Fairbank Tjossem, "Preservation of Nature and Academic Respectability: Tensions in the Ecological Society of America, 1915-1979," PhD diss, Cornell University, 1994; Gina Rumore, "Preservation for Science: The Ecological Society of America and the Campaign for Glacier Bay National Monument," *Journal of the History of Biology* 45 (2012), pp 613–650; S. Andrew Inkpen, "Demarcating Nature, Defining Ecology: Creating a Rationale for the Study of Nature's 'Primitive Conditions,'" *Perspectives on Science* 25 no 3 (2017), pp 355-392. For a discussion of the self-conscious role of another professional society in disciplinary formation, see Vassiliki Betty Smocovitis, "Organizing Evolution: Founding the Society for the Study of Evolution (1939-1950)," *Journal of the History of Biology* 27 no 2 (1994), pp 241-309. A British counterpart to this case can be found in Keith Vernon, "Desperately Seeking Status: Evolutionary Systematics and the Taxonomists' Search for Respectability 1940–60," *The British Journal for the History of Science* 26 no 2 (1993), pp 207–27.

[212] Major examples of this literature include Patricia P. Gossel, "The Emergence of American Bacteriology, 1875-1900," PhD diss, Johns Hopkins University, 1989; Susan Barbara Spath, "C.B. van Niel and the Culture of Microbiology, 1920-1965," PhD diss, University of California, Berkeley, 1999; Eric D. Kupferberg, "The Expertise of Germs: Practice, Language, and Authority in American Bacteriology, 1899–

itself, which was founded (in the retrospective words of one member) with the aim of "emphasiz[ing] the position of bacteriology as one of the biological sciences."[213] As Olga Amsterdamska argues, this concern with bacteriology's place within biology reflected an acute status anxiety of a field closely associated with medical service roles, an anxiety which would last into the mid-20th century.[214] Principally motivated by this anxiety, the SAB was an important actor in steering bacteriology in directions its leaders saw as more prestigious, such as sponsoring the canonical *Bergey's Manual of Determinative Bacteriology* to stabilize the contested field of bacterial taxonomy. While the historiography of bacteriology has been particularly responsive to the practice turn, the institution of the SAB is nonetheless featured even in much of this literature.[215] Nonetheless, there has been no comprehensive scholarly history of the SAB as an institution, leaving quasi-official histories and reminisces from SAB members (especially

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1924," PhD diss, Massachusetts Institute of Technology, 2001; Funke Iyabo Sangodeyi, "The Making of the Microbial Body, 1900s-2012," PhD diss, Harvard University, 2014; Powel H. Kazanjian, *Frederick Novy and the Development of Bacteriology in Medicine*, New Brunswick: Rutgers University Press, 2017.

[213] W. T. Sedgwick, quoted in Olga Amsterdamska, "Inventing Utility: Public and Professional Presentations of Bacteriology Before the Second World War," *Accountability in Research* 5 (1997), pp 175-195, 185.

[214] Ibid.

[215] Bacteriology, an inherently instrument-mediated science of manipulating the difficult-to-manipulate, has understandably been a fruitful case study for historiography focusing on scientific instrumentation and technique. Major works in this literature include Patricia Peck Gossel, "A Need for Standard Methods: The Case of American Bacteriology," in Adele E. Clarke and Joan H. Fujimura (eds), *The Right Tools for the Job: At Work in Twentieth-Century Life Sciences*, Princeton: Princeton University Press, 1992, pp 287-311; James Strick, "Swimming against the Tide: Adrianus Pijper and the Debate over Bacterial Flagella, 1946-1956," *Isis* 87 no 2 (1996), pp 274-305; Nicolas Rasmussen, *Picture Control: The Electron Microscope and the Transformation of Biology in America, 1940-1960*, Stanford: Stanford University Press, 1997. In contrast to the tightly focused case-study nature of much of this scholarship, see Mathias Grote, "Petri Dish versus Winogradsky Column: A *Longue Durée* Perspective on Purity and Diversity in Microbiology, 1880s-1980s," *History and Philosophy of the Life Sciences* 40 no 1 (2018). Despite this 'material turn,' the SAB appears as an important site for establishing a professional identity and disputing evidentiary claims in Gossel and Strick's work (respectively).

Johns Hopkins' Barnett Cohen, who served as a long-standing archivist for the organization) as a principal source for historians.[216]

Scientific societies also occupy a place in the historiography of Cold War science, but it is generally an implicit one. Inter- and transnational organizations are a notable part of this historiography, with themes like the influence of official American anticommunism on international scientific meetings.[217] The meetings of societies like the AAAS also feature in the historiography of Cold War scientific protest within the United States as venues for intentionally disruptive protests by radical scientists in the late 1960s and early 1970s.[218] This historiography is often focused on the protesters themselves, with the scientific societies serving as backdrops rather than actors in their own right. As the case of the SAB/ASM discussed below shows, however, it is worth paying attention to how societies' leaders created and maintained the implicit political stances that more-radical protesters eventually challenged. To do so is, in a sense, simply to take these contestants' belief in the significance of their professional societies seriously (as, indeed, a body of sociology of science scholarship on the social responsibility of professional societies written shortly afterward did).[219] There is a curious mixing of subject and object

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[216] C.-E. A. Winslow, "The First Forty Years of the Society of American Bacteriologists," *Science* 91 no 2354 (February 9, 1940), pp 125-129; Barnett Cohen, *Chronicles of the Society of American Bacteriologists, 1899-1950,* Baltimore: Williams and Wilkins, 1950.

[217] See e.g. Ronald E. Doel, Dieter Hoffmann and Nikolai Krementsov, "National States and International Science: A Comparative History of International Science Congresses in Hitler's Germany, Stalin's Russia, and Cold War United States," *Osiris* 20 (2005), pp 49-76; Rena Selya, "Defending Scientific Freedom and Democracy: The Genetics Society of America's Response to Lysenko," *Journal of the History of Biology* 45 no 3 (2012), pp 415-442.

[218] Kelly Moore, *Disrupting Science: Social Movements, American Scientists, and the Politics of the Military, 1945-1975,* Princeton: Princeton University Press, 2008.

[219] See e.g. Rosemary A. Chalk, "Scientific Society Involvement in Whistleblowing," *Newsletter on Science, Technology, & Human Values* 22 (1978), pp 47-51; Muzza Eaton, "Scientific Freedom and Responsibility Activities of Scientific Societies," *Science, Technology, & Human Values* 5 no 29 (1979), pp 24-33; Carol L. Rogers, "Science Information for the Public: The Role of Scientific Societies," *Science, Technology, & Human Values* 6 no 36 (1981), pp 36-40.

in studying events from the mid-20th century historically, as scholarship from that very period can be important for one's own. In recent years, some historians and philosophers of science like George Reisch and Elena Aronova have adopted a reflexive approach stemming from this reality, tying Second World War and Cold War concerns to foundational scholarship within their own field, from that of Robert Merton to Thomas Kuhn's to the canonical importance of Gregor Mendel in the historiography of genetics.[220] In a similar light, it is probably not a coincidence that classic historiography on scientific societies written at the time reflect the concerns of a period in which the role of institutions in American society and the attendant malaise of the "organization man" attracted the attention of scholars and lay social commentators alike, and science underwent a rapid transformation with an influx of government funding.[221] Insofar as historiography is as revealing about the times in which it was written as that about which it was written, both the scholarly conclusions of this mid-century literature and the concerns underlying its very existence can inform our understanding of how and why mid-century scientists sought to shape their own professional institutions. Though mid-century scholarship on scientific societies is in a sense young enough to appear old-

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[220] See e.g. Elena A. Aronova, "Studies of Science Before "Science Studies": Cold War and the Politics of Science in the U.S., U.K., and U.S.S.R., 1950s-1970s," PhD diss, UC San Diego, 2012; Audra J. Wolfe, "The Cold War Context of the Golden Jubilee, or, Why We Think of Mendel as the Father of Genetics," *Journal of the History of Biology* 45 no 3 (2012), pp 389-414; George Reisch, "When *Structure* Met Sputnik: The Cold War Origins of *The Structure of Scientific Revolutions*," in Naomi Oreskes and Elena Aronova (eds), *Science and Technology in the Global Cold War*, Cambridge, MA: MIT Press, 2014, pp 371-392; Elena Aronova and Simone Turchetti (eds), *Science Studies during the Cold War and Beyond: Paradigms Defected*, New York: Palgrave Macmillan, 2016; Wolfe, *Freedom's Laboratory*; George Reisch, *The Politics of Paradigms: Thomas S. Kuhn, James B. Conant, and the Cold War "Struggle for Men's Minds"*, Albany: State University of New York Press, 2019.

[221] The term (but not the anxiety) originated from William H. Whyte, *The Organization Man*, New York: Simon & Schuster, 1956.

fashioned, it is worth taking its focus seriously when trying to understand the period in which it was written.

The Society of American Bacteriologists/American Society of Microbiologists

The Society of American Bacteriologists was a community of contradictions in the first two decades of the Cold War. On the one hand, microbiology was enjoying enormous institutional success in the United States, with new fields of bacterial genetics and antibiotics research opening new intellectual vistas both 'pure' and 'applied,' a torrent of federal research funding offering the means to explore these vistas, and thousands of new researchers flooding into the field to take advantage of this. Bacteria themselves, previously conceptualized as forms of life more primitive than macroscopic organisms, were increasingly viewed as equivalently complex and worthy of biologists' attention, an attitude underlying biochemist Jacques Monod's celebrated dictum that "what is true for *E. coli* is true for an elephant." Programmatically embodying this ideal, the "General Microbiology" movement grew in prominence in the decades after the Second World War, with one of its major American adherents, C. B. van Niel, elected SAB president in 1954.[222]

On the other hand, the ideals of General Microbiology, that microbes were worthy of 'pure' biological study untainted by the medical or industrial service roles of older style 'microbe hunting' protested too much, reflecting the long-standing and deep status anxieties of a field which had largely been dominated by such roles. Nor were microbiologists steeped in traditions of infectious disease research or research on the

[222] Susan Barbara Spath, "C. B. van Niel and the Culture of Microbiology, 1920-1965," PhD diss, University of California, Berkeley, 1999.

'productive microbes' of industrial and soil microbiology pleased by declarations that their focus on these organisms was too narrow or insufficiently 'pure;' and they certainly did not disappear even as the star of General Microbiology ascended.[223] The meteoritic growth of the SAB, in turn, was disruptive to the social relationships of what had been a small and close-knit organization, and accentuated anxieties about just who was qualified to call themselves a "bacteriologist." At stake was the credibility of the field in general, but also in particular the salaries that bacteriologists who were part of the federal Civil Service could command depended on how high-status the term was. The SAB also faced questions felt more generally about what exactly the role of a scientific society was in early Cold War America. It organized the annual meetings and published the journal, but how far should it go as a lobbying organization to advance bacteriologists' professional interests? What official notice should the Society take of "social issues" like the ongoing segregationism of the American South?[224] Even the minor issue of terminology was an irritant: were the SAB's scientists "bacteriologists," even if they studied fungi, viruses, or other non-bacterial microbes, or "microbiologists," a term which some more traditional members found unnecessary or alienating? Thus, for all its success, the SAB of the mid-20th century was an anxious community, riven by conflict over professional certification, political commitments, and what it meant to be a bacteriologist.

[223] "Productive microbes" as an object of study (as opposed to the pathogens of medical microbiology) are a major topic of Kupferberg, "The Expertise of Germs."
[224] This question arose in planning for the 1956 annual meeting, which was to be held in Texas, with segregationist laws and a 'whites-only' hotel. Some liberal members of the SAB protested the impact this choice of venue would have on African-American members, and were opposed by a faction which viewed it as inappropriate for the SAB to take up a 'political' stance against the segregationist system. The choice of venue ultimately stood. This episode is discussed in Spath, "C. B. van Niel and the Culture of Microbiology," pp 258-259.

It was amidst these anxieties that the SAB negotiated its relationship with biological weapons research. In a time of chilling Cold War, what relationship the organization should have with the growing national security state complimented other questions about its *raison d'être*. This question was far from academic. A large number of SAB members had been personally connected to BW research, particularly during the Second World War, and befitting the Detrick community's ongoing and deep ties to civilian microbiology, Detrick researchers remained an active part of the SAB community. Indeed, many of the SAB's most influential leaders in the 1940s and '50s, including a full half of its presidents in the latter decade, were members of an informal community of 'friends' of Detrick, convinced of the practicability of biological warfare and intent on supporting American research. This was not a universal view within the SAB, however. Many members seem to have been quietly skeptical of such claims, which manifested as indifference to the histrionics of the would-be biological Cold Warriors, much to the annoyance of the latter. Other SAB members, meanwhile, held a deep ethical discomfort with the use of their science to develop weapons of war, particularly in the midst of a Cold War foreign policy with which many of these skeptics disagreed. A complex tangle of scientific, ethical, and political disagreements thus threatened to make biological warfare yet another flashpoint within the SAB community. Instead, the SAB community saw very little debate on the subject until the late 1960s, while the organization itself lent institutional support to Army bioweapons research. Both of these facts owed a great deal to the active efforts of the generally pro-BW leadership of the SAB. It was not until the Vietnam era when they lost control of a now-greatly expanded and younger community, and debate over the organization's relationship with the Army fully flowered. Just as the SAB had been a quiet buttress to the Detrick community in previous decades, this wave of public dissent within the Society contributed to the ultimately fatal political isolation of Detrick in 1969.

The SAB's 1945-1969 relationship with biological warfare can be divided into three periods. The first was an actively constructed period of community silence on the subject during the late 1940s and early 1950s. Even as popular discussion and other microbiological societies debated the efficacy and ethics of biological weapons research, SAB leaders actively and largely successfully sought to quiet such discussion within the organization. By the second period from the early 1950s to the mid-1960s, meanwhile, these leaders extended their organization's support to the Detrick community, establishing an increasingly institutionalized relationship with the Army Chemical Corps as they realized their broader goals of raising microbiology's professional standing. The final period was when this relationship came crashing down in the late 1960s, as the American Society for Microbiology (as the organization had been renamed in 1961) became a venue for the increasingly bitter debate within Vietnam-era American science about the ethics of supporting government weapons research.

Constructing Community Silence: 1945-1952

The strict secrecy system during WWII bifurcated the microbiological community between those who worked at Detrick or were otherwise tied to bioweapons research, and those who did not. Candidates for the former group were formally vetted for 'security risks' by the FBI, but were often selected in the first place because they were known members of the interpersonal networks of those managing Detrick.[225] In addition to scientific skills, their political orthodoxy and probable acceptance of such war work would have been a known factor before they were even considered for the program.[226] In addition, anyone whose colleagues and government misjudged them and who objected to weaponizing their science would nonetheless have been bound by wartime secrecy to keep their objections private. With the very existence of American biological weapons program a closely-guarded secret, those in the SAB community who were not privy to it knew that their colleagues were doing vital war work, that correspondence should be sent to the National Academy of Sciences to be forwarded to them, and (perhaps) that they were somewhere in Maryland, but little else. Even if the SAB itself had not been

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[225] See e.g. National Academy of Sciences Archives collection "Committees on Biological Warfare, 1941-1948" (NAS BW), Box 1 Folder 4 ("WBC Committee Biographical Data 1941") for a collection of vignette biographies of potential staff members considered in mid-1943 (as Detrick was being established). Many of these people were explicitly suggested because of their interpersonal connections with ABC Committee members, and some were rejected by other members based on personal acquaintances. An example of the latter was University of Wisconsin biochemist Karl Paul Link, whose name was raised but "held in abeyance" as the suggestion of "Drs McClean, Hudson, and Goodner." See "Biographical Data: Link, Professor Karl Paul," in NAS BW Collection Box 1 Folder 4. Link was remembered by biographer Robert Burris as personally flamboyant and outspokenly politically liberal, which may have contributed to this rejection. See Robert H. Burris, "Karl Paul Link," *Biographical Memoirs of the National Academy of Sciences* (BMNAS) 65 (1994), pp 175-195, 191.

[226] The FBI, whose director J. Edgar Hoover was suspicious of the perceived cosmopolitan tendencies of scientists, erred on the side of paranoia in such investigations. The story of University of Illinois chemist Roger Adams was a case in point. Adams was an extremely influential figure within American chemistry and personal friend of OSRD/NDRC leaders like James Conant, who recruited him to direct OSRD organic chemistry research in 1940 (he would also serve on the NAS' BW committees). His security clearance was delayed for several months by Hoover's FBI, however, who identified the conservative Adams with a "Professor Adams" with alleged communist sympathies. It was only under intense political pressure from OSRD director Vannevar Bush that Hoover relented. While the strictures of political orthodoxy seem to have loosened somewhat as the war progressed (for instance, the left-leaning and foreign-born Theodor Rosebury received a security clearance), it still seems probable that the clearance process was a substantial force in winnowing out anyone who would risk imprisonment by revealing the secrets of Detrick. See Ronald E. Doel, "Roger Adams: Linking University Science with Policy on the World Stage," in Lillian Hoddeson (ed), *No Boundaries: University of Illinois Vignettes*, Champaign, IL: University of Illinois Press, 2004, pp 124-144, 129 for a discussion of the almost farcical Adams episode.

disrupted by the war (the 1944 annual meeting was cancelled, for instance), it was simply not a realistic venue for anyone to debate Detrick and its research during the war.[227]

The end of the war and public revelation of the Detrick program in 1946 opened up the first realistic possibility of debate about biological warfare within the SAB. Suddenly, those members who had not been part of the wartime program knew at least in general terms what their colleagues had been doing, and as declassified papers based on wartime research began to appear in 1946 and 1947, those generalities became more specific. Another genre also began to appear: popular science writing on the formerly science-fictional but now apparently feasible topic of “bacteriological warfare.” Deplored by members of Ira Baldwin’s network of proponents of biological warfare research as irresponsible and unscientific “sensationalism,” such popular speculation was nonetheless effective at establishing an identity for “bacteriological warfare” as something akin to the new atomic bomb. This identity of BW as a “weapon of mass destruction” was further cemented by the subject’s appearance in negotiations over the international control of atomic energy, with Soviet delegates arguing that germ as well as atomic methods of warfare would need to be effectively controlled for such an agreement to be acceptable. With the appearance of a review article on the topic by bacteriologist Theodor Rosebury in the majority of the April 1947 issue of the *Journal of Immunology*, the question of whether the SAB should lend its scientific authority to refining the identity of “BW” seemed increasingly pressing to some of the organization’s more liberal members. Even before this Rosebury article appeared, a group of them, led by bacterial geneticist

[227] See ASM 13-IIAT (Baldwin Presidential Papers), Folder 13, for correspondence about the 1944 meeting and its cancelation.

Salvador Luria had begun "discussing the implications of the secret work on bacteriological warfare," (as Luria put it), and "have felt that it would be useful to make the profession and the public more aware of the dangers involved."[228] They thus began to plan to advance a resolution to be presented at the next SAB meeting to accomplish this, soliciting help from prominent colleagues like immunologist Michael Heidelberger, virologist Wendell Stanley, and bacteriologists C. P. van Niel and Barnett Cohen.[229] These invitations evidently misjudged either the personal views or political courage of these luminaries, and the most prominent member the group ended up attracting was Yale public health pioneer C. E. A. Winslow (though one of van Niel's protégées, Roger Stanier, also joined them).[230] Additionally, Luria consulted with Rosebury, who also avoided lending his name to Luria's scheme as "I cannot discuss this matter as freely as I might wish." Continuing to allude to the secrecy that bound him and other ex-Detrick researchers, Rosebury counselled that Luria recruit Stuart Mudd or Thomas Anderson of the University of Pennsylvania, because they "are among the few bacteriologists not associated with official BW activities who have been actively interested in this matter."[231] The secret knowledge of an ex-Detrick researcher was a serious constraint on any criticism of bioweapons research they wished to offer, forcing them to carefully

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[228] S. E. Luria to Michael Heidelberger, February 24, 1947, in NLM Profiles in Science Michael Heidelberger Papers (NLM ID 101584940X195).

[229] Ibid. Heidelberger quickly demurred, arguing that "once an effective set-up had been organized for atomic energy, control of V-2, bacteriological warfare, etc. will be that much easier in the form of whatever pattern is established." Michael Heidelberger to S. E. Luria, February 27, 1947, NLM Profiles in Science Michael Heidelberger Papers (NLM ID 101584940X197).

[230] Luria, Windlow, and Stanier were joined by Pennsylvania's Stuart Mudd, and Thomas Anderson, evidently the suggestion of Theodor Rosebury (see Footnote 234, below). See J. Howard Mueller to Walter J. Nungester, Stuart Mudd, Thomas Francis, Jr., and Leland W. Parr, June 2, 1947 in University of Wisconsin Archives (UWA) Ira L. Baldwin Papers (Series 9/10/11), Box 6 Folder 1, which cc'd Mudd's compatriots.

[231] Rosebury to Luria, March 4, 1947, in NLM Theodor Rosebury Papers (MS C 634), Box 3 Folder 11 (Correspondence 'L' 1 of 2).

demonstrate a non-classified basis for every factual assertion they made (as indeed Rosebury did in his subsequent 1949 book, *Peace or Pestilence*).[232] Mudd's rare combination of overt concern about biological warfare without this constraint, home turf advantage (the SAB meeting was to be held in Philadelphia), and financial independence (he funded most of his laboratory's research from his own personal fortune) made him a perfect standard-bearer for the group, and Luria took Rosebury's advice by recruiting Mudd to deliver the petition.[233] Luria also planned a lunchtime discussion of biological warfare to be held at the Philadelphia meeting, presumably to further build interest within the SAB community.[234]

What followed was an astute outmaneuvering of the Luria group by pro-Detrick SAB leaders. When the meeting began in May, a month after the Rosebury article appeared, Mudd surprised SAB president Thomas Francis, Jr. by proposing the Luria group's resolution.[235] This would declare the SAB's public apprehension about biological warfare and how it "does not lend itself even to the degree of control that atomic warfare does," and consequent support for general peace negotiations.[236] Francis parried, instead

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[232] See e.g. Theodor Rosebury, *Peace or Pestilence: Biological Warfare and How to Avoid It*, New York: Whittlesey House, 1949, p 194.
[233] Mudd is extensively discussed in Chapter 2 of Rasmussen, *Picture Control*.
[234] Luria invited Rosebury to this meeting, but he demurred at the last minute. See Luria to Rosebury, May 5, 1947 and Rosebury to Luria, May 10, 1947, both in NLM Rosebury Papers, Box 3 Folder 11 (Correspondence 'L' 1 of 2). It is unclear if this luncheon was even held, but if it was, it seems to have had little impact.
[235] Thomas Francis, Jr. to J. Howard Mueller, June 3, 1947, in UWA Baldwin Papers, Box 6 Folder 1. Baldwin, who was unable to attend the Philadelphia meeting, received a report of the events from his Wisconsin colleague (and fellow Detrick alum) William Sarles "that the resolution was not adopted, but was referred to a special com.- composition unknown- for report." See Baldwin ms on Draft Press Release dated May 25, May 17, 1947, in UWA Baldwin Papers, Box 6 Folder 1.
[236] Draft Press Release (dated May 25, but with Baldwin ms dated May 17, 1947), in UWA Baldwin Papers, Box 6 Folder 1. The full text of the resolution was: "Whereas the advances of science and technology have produced weapons of destruction, of which biological warfare is one, which in our professional judgment endangers the survival of modern civilization, and whereas we believe that biological warfare does not lend itself even to the degree of control that atomic warfare does; out conclusion is that every possible effort should be directed toward building a system of world-wide

referring the resolution to an ad hoc SAB committee, consisting of Francis himself, SAB secretary-treasurer Leland Parr, and Mudd, along with Harvard's J. Howard Mueller, and the University of Michigan's Walter Nungester, both of whom had done research for Detrick during the war.[237] Mudd, who had ended up being "more responsible than anyone else for the actual wording of the proposed resolution," acquiesced readily as his text and particularly his comparison to atomic warfare had touched a sore spot with other SAB members.[238] "I feel that our Society can be of greatest service to the cause of peace, as well as the security of our own nation, by refraining from adding to the useless and unscientific publicity concerning the 'horrors of biological warfare,'" Nungester grumbled.[239] Mueller was still more direct. "In my professional judgment there is very little basis in fact to link the possibilities of bacterial warfare with the atomic bomb," he wrote. "The matter seems to me quite ridiculous."[240] Mudd retreated, writing apologetically to Mueller, "I feel that I must take the blame for [the resolution's] lending itself to misunderstanding," explaining that while "I do not believe that it would be possible at this time to obtain agreement among microbiologists as to relative importance of biological warfare, other than that it is certainly a hazard of less magnitude than atomic warfare… the significant point… is that it does not lend itself to international control."[241] For all that Mudd temporalized, however, the resolution's implicit comparison to the

cooperation through the United Nations to ensure the world against war." A copy of this resolution can be found in in ASM Series 8-IE "Ad Hoc BW Committees," Folder 1.

[237] J. Howard Mueller to Walter J. Nungester, Stuart Mudd, Thomas Francis, Jr., and Leland W. Parr, June 2, 1947 in UWA Baldwin Papers, Box 6 Folder 1 address the membership of the committee.

[238] Stuart Mudd to J. Howard Mueller, June 6, 1947, in UWA Baldwin Papers, Box 6 Folder 1.

[239] W. J. Nungester to J. Howard Mueller, June 3, 1947 in UWA Baldwin Papers, Box 6 Folder 1.

[240] J. Howard Mueller to Walter J. Nungester, Stuart Mudd, Thomas Francis, Jr., and Leland W. Parr, June 2, 1947 in UWA Baldwin Papers, Box 6 Folder 1. This letter, addressed to the members of the committee, also cc'd the other members of Mudd's group.

[241] Stuart Mudd to J. Howard Mueller, June 6, 1947, in UWA Baldwin Papers, Box 6 Folder 1.

atomic bomb (which was not in an earlier draft suggested by Luria) was a fatal error, invoking the specter (as Baldwin put it elsewhere) of an "over-emphasis [on the power of atomic bombs which] has brought on the Atomic Energy Commission and on physicists everywhere a very unfortunate situation with respect to security."[242] Francis' decision to shunt the resolution to a committee and staff it with (in Ira Baldwin's words) "about as widely divergent viewpoints as it would be possible to secure" (particularly with the outspokenly pro-BW Nungester) was an astutely executed move to kill the resolution, which never again saw the light of day.[243] In marked contrast, for all that the SAB leadership was invested in avoiding a public position on biological warfare, the Americans could not similarly control the transnational community of microbiologists when the 4th International Congress for Microbiology met in Copenhagen a few months later.[244] There, with Americans like Mudd and Luria attending, the Congress adopted a resolution of its own by acclamation, outright condemning biological warfare research.[245] Back in the SAB, however, silence prevailed on the subject of biological warfare for the next several years.

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[242] Baldwin to W. J. Nungester, November 21, 1949 p 2, in UWA Baldwin Papers Box 15 Folder 4. One of Luria's early drafts of the petition is attached to Luria to Rosebury, February 24, 1947, in NLM Rosebury Papers, Box 3 Folder 11 (Correspondence 'L' 1 of 2).

[243] Ira Baldwin to Walter Nungester, June 12, 1947 in UWA Baldwin Papers, Box 6 Folder 1. Mueller, too, would have been a highly unsympathetic judge: this was about the same time that he was campaigning for Columbia to fire Rosebury and his co-author Elvin Kabat for publishing their 1947 article. See Elvin Kabat, "Getting Started 50 Years Ago: Experiences, Perspectives, and Problems of the First 21 Years," *Annual Review of Immunology* 1 (1983), pp 1-32, 19-23.

[244] This was the first meeting of the Congress since the outbreak of the Second World War, and for many American attendees represented the first chance to reconnect with their European colleagues since 1939.

[245] Ralph St. John-Brooks, "Fourth International Congress for Microbiology," *Nature* 160 (November 1, 1947), pp 596-597. Luria discussed his plans to attend the Congress in S. E. Luria to Michael Heidelberger, February 24, 1947, in NLM Profiles in Science Michael Heidelberger Papers (NLM ID 101584940X195). Mudd's presence is mentioned in "International Congress for Microbiology: Copenhagen, July 20-26," *The Lancet* 250 (August 2, 1947), pp 183-184.

When this silence was broken, it was in the midst of the Korean War and the presidency of Walter Nungester. A medical microbiologist who since 1947 had been chairman of the University of Michigan's Department of Microbiology, Nungester had been a researcher at Detrick during WWII and had since been an outspoken advocate of bioweapons research. Politically conservative and pessimistic about the competence of American military and political leadership to prosecute the Cold War, Nungester was in the best of times prone to bombarding confidants like Ira Baldwin with opinionated and generally despairing letters decrying what he saw as insufficient military investment in a major new threat.[246] With the outbreak of the Korean War and particularly the Truman administration's declaration of a national emergency following Chinese intervention in December 1950, Nungester and a group of like-minded Midwestern colleagues in the SAB's Michigan Branch grew still more vocally concerned about biological warfare, particularly when used for covert attack, and in 1951 set about composing an official inquiry to Michigan civil defense officials about what their group could do to guard against biological sabotage.[247] It is no surprise, then, that Nungester used his 1951 election to the presidency of the SAB as a soapbox to further these views. Soon after his ascension in the spring of 1951, he organized an "Ad Hoc Committee on Biological

[246] See, e.g. W. J. Nungester to Ira Baldwin, November 9, 1949 in UWA Baldwin Papers, Box 15 Folder 4.
[247] Walter Nungester to Ray Sarber, April 24, 1951 in UWA Baldwin Papers, Box 16 Folder 1. The theme of guarding against covert biological attack was a popular one in the biomedical community in 1950-1951. Most notably, this was one of the principal justifications used by Alexander Langmuir, former member of Baldwin's Committee X and new chief epidemiologist for the CDC, to organize his field epidemiology oriented "Epidemic Intelligence Service." James Colgrove, Amy L. Fairchild, and Ronald Bayer, *Searching Eyes: Privacy, the State, and Disease Surveillance in America*, Berkeley: University of California Press, 2007, p 17. Langmuir, like Nungester, tried to draw the interest of the medical and microbiological communities to biological warfare in this period, publishing several articles in medical journals arguing about the dangers of biological attack. See Alexander D. Langmuir, "The Potentialities of Biological Warfare against Man: An Epidemiological Appraisal," *Public Health Reports* 66 no 13 (1951), pp 387-399; Alexander D. Langmuir and Justin M. Andrews, "Biological Warfare Defense: The Epidemic Intelligence Service of the Communicable Disease Center," *American Journal of Public Health* 42 no 3 (1952), pp 235-238. In his later assessment, this campaign fell on deaf ears.

Warfare," consisting of the University of Washington's Charles Evans and Alfred Lazarus (formerly of Karl Meyer's George Hooper Foundation), the University of Illinois' H. Orin Halvorson, and Yale's Henry P. Treffers. Unlike the 1947 proposal, the intent of this committee was openly hawkish, intended to pressure Federal civil defense planners to pay more attention to biological warfare by offering SAB 'help' with their public relations efforts. Nungester, in 1951, was fixated on using publicity about biological warfare (at least among biomedical communities) to encourage more military investment in Detrick, arguing for instance to Detrick's Oram Woolpert that "we should tell many more people about the field trials either by declassifying the information or clearing about 4,000 or 5,000 medics, and bacteriologists."[248] The SAB committee was to be one more front in this campaign.

Like the 1947 committee on which Nungester himself had served, the 1950 group were astutely selected to produce the results the SAB president wanted, and their report, issued within a month of the committee's appointment, was indeed in accord with Nungester's desire for liberalized technical knowledge. The committee divided the public into four groups: medical personnel, public health administrators, laboratory technicians,

[248] Walter Nungester to O. C. Woolpert, April 24, 1951, in UWA Baldwin Papers, Box 16, Folder 1. Joint Anglo-American field testing conducted in the late 1940s was a major piece of evidence for the efficacy of germs as weapons, but their data were heavily guarded secrets. See Peter M. Hammond and Gradon B. Carter, *From Biological Warfare to Healthcare: Porton Down, 1940-2000*, London: Palgrave Macmillan, 2001. Ira Baldwin, always more cautious and at the time the chairman of the RDB's Committee X, was unimpressed by Nungester's idea. "I grant that the problem of preparing adequate civil defense is difficult without information of the type suggested," but "on the other hand, I see no reason to hand potential enemies information which would be of value to them at the present time." Nungester continued to push, however, arguing that "statements have been published including: 1) the Merck Report, 2) Rosebury book 'Peace or Pestilence,'" and various popular articles from which "the Russians certainly have been able to put the story together in a general way." It was "our civilian folks not having the military background of BW development either at home or abroad [who] are the only ones in the dark! It's the old problem of secrecy or security. They are not always synonymous." See Baldwin to Nungester, May 8, 1951 and Nungester to Baldwin, May 11, 1951, both in UWA Baldwin Papers, Box 16 Folder 1.

and “civilian groups with an intelligent interest but without special responsibility with respect to civilian defense.” The first three groups, the committee argued, had a wide range of legitimate questions with “answers [that] do not involve secret information and should be given publicity among health personnel.” For civil defense officials to increase such publicity, they argued, would improve responses to a biological attack (and, they left unstated, enlarge the constituency of those with biomedical training concerned about biological warfare). Any secret techniques which the government might have developed to speed up the detection of pathogenic microbes was of particular interest to the group, which urged that they be advertised to public health laboratories. “An hour’s delay in detection may render some potentially protective measures useless or of greatly reduced value,” they explained, and while “this degree of urgency is not present in civilian bacteriologic work… a major aspect of civilian defense is jeopardized by lack of rapid detection methods.” In contrast to this liberal attitude toward releasing information to medical communities, the committee was far less interested in informing the general public. “The psychological impact of a B.W. may be very severe because many people with no technical knowledge of microorganisms lose their perspective entirely at the thought of ‘germs’ or ‘poisons’ in their environment,” they explained. Instead of further stoking this popular fear by releasing further information, “having… technical personnel sufficiently well informed to react in a level-headed way” would have to suffice “to break the chain of panic in a community.” This paternalistic attitude accorded well with the politics of secrecy within the community of pro-bioweapons microbiologists, with their abhorrence of public “sensationalism” but simultaneous desire to enlist more scientific colleagues to their cause, and certainly fit Nungester’s desire to do so. Pleased with the committee's report, Nungester railroaded a resolution of his own through the SAB's general meeting that May, calling for more information from federal civil defense officials.[249] He then used this resolution, with the endorsement of the SAB behind it, as a bludgeon to argue with Pentagon officials that "our country is taking entirely inadequate steps in developing this field not only from the point of view of civilian and military defense, but also in preparing by education, research and development, to use these weapons offensively."[250]

Soon, however, events on the other side of the world completely changed the tenor of such discussions. In February 1952, the North Korean and Chinese governments claimed that the United States had attacked their troops in Korea with a veritable smorgasbord of biological weapons, from plague-infected fleas to feathers coated in anthrax spores. The Americans, in turn, denied these charges, which became another front in the propaganda battles of the Cold War throughout 1952. This sudden notoriety quickly shifted the connotation of public discussions of biological warfare among American cold warriors from boosterism to defensive silence. Government policy quietly returned to the culture of silence of 1946-1949, sharply curtailing Chemical Corps efforts to promote the military value of their wares. Small details about the American research program (which was almost certainly incapable of enabling the attacks the Americans were accused of) became far more jealously guarded secrets than before if they could at all corroborate the North Korean-Chinese account.[251] Within the SAB, too, the culture of

[249] Walter J. Nungester to Millard Kaldwell, July 24, 1951, in UWA Baldwin Papers, Box 16 Folder 1. This letter contains a copy of the SAB resolution and questions.
[250] Walter Nungester to General George Marshall, July 30, 1951, in UWA Baldwin Papers, Box 16 Folder 1.
[251] The most noteworthy example of this is American research on disease-carrying insects, which officials falsely denied the very existence of in the wake of the accusations. This denial made what had been a very

silence which had been so carefully maintained before the Korean War replaced Nungester's brief flirtation with open (and pro-BW) discussion. (Nungester himself seems to have dropped his briefly held schemes for declassifying field tests to enlist more biomedical support around this time as well.) Some of the SAB's leaders initially planned to gather information from military contacts to publicly refute an accusatory document by World Council of Peace president Frederick Joliet-Curie, but they then seem to have thought better of this plan, instead defaulting to a studied silence on the matter. As in 1947, this silence was in part actively constructed. When Squibb Institute researcher Richard Donovick privately suggested to SAB leaders that the Society officially consider an anti-BW resolution modeled on one recently adopted by the British Society for General Microbiology, these leaders were quick to argue against this idea.[252] Secretary-Treasurer Henry W. Scherp responded by repeating pieties about the moral equivalence of whatever weapons are used in war, which were common in the pro-BW camp, before arguing frankly that "in addition… the S.A.B. would be guilty of naïveté and hypocrisy to pass [the] resolution… Many of our most esteemed members and an even greater number of those below the salt worked not only willingly, but in some cases eagerly, on B.W. during the War and for all I know may still be abetting the project… I notice many entries from Camp Detrick in each list of new members of the S.A.B. and we have

minor area of research one of the most closely-guarded secrets of the bioweapons program in the 1950s. Stephen Endicott and Edward Hagerman, *The United States and Biological Warfare: Secrets from the Early Cold War and Korea*, Bloomington: Indiana University Press, 1998, pp 74-77. Endicott and Hagerman's book is intended to support the North Korean and Chinese claims, and presumes American malice wherever possible with a prosecutorial fervor, but their archival work on the insect issue appears to be sound. The interpretation that American work on insects was more-deeply classified in the 1950s to avoid political embarrassment, rather than to conceal a sinister conspiracy in Korea, is, needless to say, my own.

[252] The Society for General Microbiology's resolution was in turn modeled on the 1947 4th Congress condemnation. See Richard Donovick to Henry W. Scherp, April 21, 1952 in ASM 8-IE (Ad Hoc BW Committees) Folder 2.

granted them space for scientific exhibits and places on the program of our Annual Meetings… The point… is that unless the Society is prepared to anathematize all of these people and their successors, it is in a pretty poor position to start riding a moral high horse."[253] Upcoming president Gail Dack concurred with this "excellent" reply, and echoed Scherp in arguing to incumbent president René Dubos that "an appreciable number of our members either are or have been connected with BW activities… much of this work is secret" and "the recent charges by the Russians… have served only to heap coals of fire on the whole issue of BW," and that therefore "since facts could not be presented and emotional tensions are high, I can see no good or objective accomplishment to be gained in a symposium on biological warfare."[254] Donovick retreated, asserting defensively that "the purpose of this is really only to attempt to get a sounding out of opinions on this matter," and that "I am not anxious to become a martyr."[255] Martyrdom was certainly a distinct prospect in the chilled McCarthyist academic climate of loyalty oaths and cancelled grants, and Donovick didn't press the issue any further.

The experience of the Berkeley bacteriological faculty at this time showcase in microcosm the pressures felt in the macrocosm of the SAB. Berkeley's Bacteriology

[253] Henry W. Scherp to Richard Donovick, June 10, 1952, in ASM 8-IE (Ad Hoc BW Committees) Folder 2. In the common parlance of early Cold War American science, this abhorrence of "emotional" or "non-objective" discussions had a heavy connotation of such discussions being politically heterodox. See Moore, *Disrupting Science*; Jessica Wang, "Physics, Emotion, and the Scientific Self: Merle Tuve's Cold War," *Historical Studies in the Natural Sciences* 42 no 5 (2012), pp 341-388; Paul Rubinson, *Redefining Science: Scientists, the National Security State, and Nuclear Weapons in Cold War America*, Boston: University of Massachusetts Press, 2016; and Audra J. Wolfe, *Freedom's Laboratory: The Cold War Struggle for the Soul of Science*, Baltimore: Johns Hopkins University Press, 2018 for discussions of the equation of scientific objectivity with Cold War political orthodoxy in the 1950s.
[254] G. M. Dack to Henry W. Scherp, June 13, 1952 and G. M. Dack to Rene J. Dubos, June 23, 1952, both in ASM 8-IE (Ad Hoc BW Committees) Folder 2.
[255] Richard Donovick to Henry W. Scherp, June 13, 1952, ASM 8-IE (Ad Hoc BW Committees) Folder 2.

Department was scientifically and politically split between competing visions of what microbiology was and what relationship it should have with the military. On one side were a group exemplified by Roger Stainer, best known for his foundational work on microbial taxonomy in the 1950s and ‘60s. Stainer and his colleague Michael Doudoroff were protégés of Stanford’s C. B. van Niel, a prominent evangelist of “General Microbiology,” which conceived of microbiology as a basic biological science in conscious rejection of the perceived parochialism of medical and industrial microbiology’s practically-oriented focus on relatively narrow classes of microbes. The star of General Microbiology was rising in the 1950s US, with van Niel (who was elected SAB president in 1954) constructing a historical identification of it with a longer-duration “Delft school” of microbiology in which he himself was trained. The rising star was not hegemonic, however, and at Berkeley, an older tradition of infectious disease research was also well-represented by faculty members like Alfred P. Kreuger and brucellosis expert Sanford Elberg. Kreuger and Elberg were themselves respectively an associate and a protégé of another California giant, the George Hooper Foundation’s Karl F. Meyer. An infectious disease expert *par excellence*, Meyer had long been a symbol for van Niel of all that was parochial and wrong with microbiology, and historian Susan Spath has argued that the tumultuous discord of Berkeley’s Bacteriology Department in this period represented a clash of the competing visions for microbiology represented by the students of van Niel and those of Meyer (who had led the department into the mid-1940s).[256]

Biological warfare also contributed to this discord, however. Stainer, who was politically left-leaning, had had no connection to military research during WWII and had

[256] Spath, “C. B. van Niel and the Culture of Microbiology,” pp 218-220.

joined Mudd and Luria's group of SAB petitioners in 1947. A few years after this, the specter of military research confronted him at home. The Naval Laboratory Research Unit was the brainchild of Kreuger, a naval reserve officer, in the mid-1930s. It was to be a paper institution co-sponsored by Berkeley and the Navy, ready to be activated to do influenza research in the event of war. When WWII did break out, however, the institution, known after 1943 as Naval Medical Research Unit 1 (NAMRU-1) was repurposed for biological weapons research.[257] This research, ostensibly focused on defense against biological attack, continued after the war, and by the early 1950s, it was clear that the Naval Biological Laboratory (NBL) created in the wake of the wartime NAMRU-1 had become a permanent institutionalized link between Berkeley and the military. While the NBL ostensibly contrasted with Detrick in only pursuing 'defensive' research (probably reflecting how it was sponsored by the Navy's Bureau of Medicine), this was an extraordinarily fine distinction to make about a secretive, military-funded laboratory in the tense days of the Korean War, and it was certainly one that Stainer and his still-more left-leaning colleague Doudoroff were disinclined to make. They were outraged that Berkeley was playing host to an institution like the NBL, and in the early 1950s protested vigorously to their colleagues. Among these colleagues, however, were the likes of Kreuger, who had served as the NBL's director since it was activated, and Sanford Elberg. Elberg's scientific biography was a sharp contrast with Stainer's. A student of Meyer's, like his mentor he had worked on biological weapons during WWII, serving at Camp Detrick and then briefly at NAMRU-1. Though he spent almost all of his

[257] Sanford S. Elberg, "Sanford S. Elberg: Graduate Education and Microbiology at the University of California, Berkeley, 1930-1989," Transcript of an interview conducted 1989 by Ann Lage. Oral History Center, The Bancroft Library, University of California, Berkeley, 1990, p 84. Several other NAMRUs were sponsored by the Navy for more traditional medical research during the war.

subsequent career at Berkeley, he essentially never left his Detrick work behind. Elberg had been principally focused on brucellosis (a disease of cattle which is highly infectious but usually non-fatal among humans) during and after the war, seeking both to assess the group of causative organisms' potential as offensive weapons and to develop a vaccine to protect against such a use. "My last two years of service, which was carried out at Camp Detrick, on brucellosis," he later assessed "was a key factor in professional growth."[258] His subsequent career was effectively dominated by brucellosis research ("consumed with it," as he put it), leading most notably to his development of the live-culture Rev 1 vaccine in the 1950s and extensive service as a World Health Organization consultant using this vaccine for anti-brucellosis campaigns in livestock around the world.[259] Nor did Elberg lose touch with the weapons roots of his work: in the subsequent decades he retained deep links with the Detrick community, spent research sabbaticals at Detrick, and served on a SAB committee of advisors to Detrick (see below). It should be no surprise, then, that a Detrick 'friend' like Elberg clashed with Berkeley critics like Stainer and Doudoroff in much the same way that other 'friends' did with other critics in the nation-wide SAB, and as in the national SAB, fears of Red Scare political 'martyrdom' eventually silenced Stainer and Doudoroff's criticism. After months of conflict within the department, an anonymous colleague loosed an FBI investigation for Communist ties on Doudoroff after he openly criticized Berkeley's links to the NBL at an officer's club dinner hosted by Kreuger. "Someone at another table," as Elberg later recalled dryly, "must have brought an accusation." Doudoroff, already in a precarious political position

[258] Elberg, "Graduate Education and Microbiology at the University of California, Berkeley," p 28.
[259] Quote from Elberg, "Graduate Education and Microbiology at the University of California, Berkeley," p 41. Decades after Elberg developed it, the Rev 1 vaccine continued to be one of the best available.

for his opposition to a controversial loyalty oath instituted within the University of California system, lost his NIH funding, and both he and Stainer (who was also a known opponent of the oath and NBL) subsequently toned down their rhetoric. Elberg believed that their quiet hostility contributed to Kreuger leaving Berkeley a few years later, and they continued to obstruct the link to the NBL by resisting seating the NBL's new director in the Berkeley department a few years later.[260] These were private actions, however: publicly, neither Stainer or Doudoroff criticized the NBL too loudly again, and the NBL itself remained tied to Berkeley until it was disbanded in the late 1980s.[261]

The divided Berkeley community is illustrative in another way, however, because it highlights the contradictions and ambiguities that existed within microbiology alongside the clear-cut forces of Red Scare repression. Interpersonal networks and collegial friendships could transcend political and ethical disagreements in a way that a larger scientific field probably would not have experienced. Elberg, for instance, had befriended Columbia immunologist Elvin Kabat, regarded Kabat and his mentor Michael Heidelberger's "Immunologic Revolution" to be one of the most important scientific events of his career, and made a point of hosting Kabat at Berkeley just as the NBL controversy was growing in 1950.[262] Kabat, who like Doudoroff would lose NIH funding for his left-leaning political views, co-authored a controversial 1947 review article on biological warfare with Theodor Rosebury which prompted accusations that both men

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[260] Elberg, "Graduate Education and Microbiology at the University of California, Berkeley," p 89. The illness and subsequent death of Kreuger's wife probably also contributed to his decision to retire. See A. P. Kreuger to I. L. Baldwin, October 7, 1952 in UWA Baldwin Papers, Box 16 Folder 2.
[261] Elberg did move the NBL to Berkeley's School of Public Health in 1957, presaging a general divorce between the medical and general microbiologists that took place in the early 1960s.
[262] Elberg, "Graduate Education and Microbiology at the University of California, Berkeley," p 73. See Andor Szentivanyi and Herman Friedman (eds), *The Immunologic Revolution: Facts and Witnesses*, Boca Raton: CRC Press, 1994.

were Communists, and was introduced for an NBL lecture as “a member of a number of a large number of organizations which I had better not mention,” but he recalled receiving a warm welcome at Berkeley nonetheless.[263] Roger Stainer, meanwhile, played host in 1964 to British microbiologist Harry Smith, who had spent his entire scientific career up to that point at the British biological weapons research facility at Porton Down, researching the pathogenesis of bacterial diseases like anthrax. This visit was admittedly near the end of Smith’s Porton career, as he would soon go on to join the faculty at Birmingham University with Stainer’s encouragement, but the fact remains that Stainer had chosen to be an enthusiastic and collegial host to a researcher whose scientific contributions were unabashedly steeped in weapons work.[264] This, in microcosm, was the state the SAB tacitly remained in for a decade and a half after 1952. In private, the community was divided on the ethics and wisdom of biological warfare research, with both opponents and equally passionate proponents of such research coexisting. A culture of public silence on such matters prevailed to the benefit of the latter group, who contributed to actively constructing it because a quiescent SAB supported the status quo of continued Detrick research. But for all that the two groups disagreed, the issue of biological warfare does not seem to have particularly split the networks of collaboration and friendship that constituted the SAB community until the late 1960s. It was ultimately

[263] Kabat discusses this sabbatical, including the anecdote about his organizational affiliations (which included the left-leaning American Association of Scientific Workers) being openly discussed at the NBL, in Elvin Kabat, “Getting Started 50 Years Ago: Experiences, Perspectives, and Problems of the First 21 Years,” *Annual Review of Immunology* 1 (1983), pp 1-32, 28-29. Kabat’s politically-motivated loss of NIH funding in 1953 was ironically enough softened by an Office of Naval Research grant.
[264] Alan Rickinson, “Harry Smith CBE,” *Biographical Memoirs of the Fellows of the Royal Society* 60 (2014), pp 397-411, 404.

an ancillary issue to questions of SAB-led professionalization that attracted the real emotional divisions during this period.

Strengthening Military Ties: 1955-1966

While the SAB community publicly remained silent on the issue of biological warfare research, SAB leaders quietly made an effort to maintain ties with the "appreciable number of our members either are or have been connected with BW activities" (as Dack put it in 1952).[265] At one level, this entailed behind-the-scenes efforts to keep Detrick researchers integrated in the SAB community despite the veil of secrecy behind which they did much of their work. At another, the SAB acted for these members like professional societies do, seeking to support their interests in dealing with their employer, the Army Chemical Corps. The SAB became more and more closely enmeshed with the Corps through the 1950s, culminating in the creation of formalized ties between the two organizations in 1955. For all that such ties served the desires of pro-biological weapons members of the SAB community, they also ultimately reflected the motivation of increasing the professional standing of microbiologists generally held far more widely by SAB members.

As the professional society of an anxious, self-consciously striving science, SAB leaders found value in supporting the Detrick community regardless of what they thought about their research. Some SAB leaders were true believers in biological warfare, perhaps most notably 1951 president Walter Nungester, and the use of his SAB position as a soapbox to advance this view is unsurprising. Those leaders with less interest in the topic,

[265] G. M. Dack to Rene J. Dubos, June 23, 1952, in ASM 8-IE "Ad Hoc BW Committees," Folder 2.

however, still had a strong reason to maintain an interest in such military research as part of a broader drive for professionalization within the society. Anxieties about the identity and status of a "bacteriologist" were deeply engrained in the history of the SAB, and in the 1950s and '60s a rapidly growing Society and increased government funding and employment inspired a campaign by many of the SAB's most prominent leaders to solidify both. This campaign would be the primary tension within the politics of the group in those decades. There was, for instance, an almost two-decade long controversy over certification standards for microbiologists which, in the later words of Ira Baldwin, "threatened to tear the Society apart."[266] By the 1940s, American microbiologists often earned degrees in bacteriology rather than medical degrees, even if they worked in medically-oriented subfields.[267] Just what coursework an undergraduate major or even graduate degree in bacteriology entailed, however, was an idiosyncratic matter of individual universities' requirements and the research interests of its faculty, and the rigor and subject matter of bacteriology degrees was accordingly disparate. This inconsistency was increasingly intolerable to many SAB leaders, as the field grew, and particularly as they tried to raise the status of microbiology from a service role to that of a 'basic' science. Based on the explicit example of American Chemical Society standards which "rais[ed] the professional and economic status of their group," these leaders believed that establishing minimum standards with a certification system would be a good solution to

[266] Ira Baldwin to Werner Braun, December 6, 1968, in ASM 13-IIAT (Baldwin Presidential Papers), Folder 71.

[267] Only 10% of SAB members in 1951 held an MD. 25% held a PhD, 20% held an MS, and 45% held a BS. See Walter J. Nungester to Council of the SAB, October 2, 1951, p 3, in ASM Series 13-IIBA (Nungester Presidential Papers), Folder 5. Nungester himself was an exception to this trend, probably reflecting the example of his mentor Frederick Novy. See Powel H. Kazanjian, *Frederick Novy and the Development of Bacteriology in Medicine*, Rutgers: Rutgers University Press, 2017.

this problem.[268] First advanced in the mid-1940s, this idea received vehement resistance from a minority of the SAB's membership, who feared that certification would be used to stigmatize members without graduate degrees. The need to negotiate with related professional societies with their own accreditation goals, particularly the AMA and American Board of Pathology in the case of medical microbiologists was another impediment to the accreditation scheme, but it was the resistance of reticent SAB members that most delayed it. It took presidency after presidency, committee after committee, for almost a decade, for leaders like Baldwin, Nungester, and H. Orin Halvorson to finally force this scheme through in the mid-1950s, and it then took almost another decade to implement a voluntary exam system to replace an ad hoc system of judgements of "experience" as a criterion for certification.[269] The body which did this was not the SAB itself, but an ancillary body, the American Academy of Microbiology (AAM), founded in 1956 after 5 years of work.[270] The use of "Microbiology" in this name reflected another hotly debated reformist idea from the 1940s, of changing the name of the Society of American Bacteriologists to use the word "Microbiologist" instead, which was seen by proponents as more inclusive of protozoologists and especially the burgeoning field of virology.[271] This idea too saw substantial resistance,

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[268] Walter J. Nungester to Council of the SAB, October 2, 1951, in ASM 13-IIBA (Nungester Presidential Papers), Folder 5.

[269] While certification began in 1956, the exam system was not established until 1964. See Ira Baldwin to Werner Braun, December 6, 1968, in ASM 13-IIAT (Baldwin Presidential Papers), Folder 71.

[270] Ira Baldwin to Werner Braun, December 6, 1968, in ASM 13-IIAT (Baldwin Presidential Papers), Folder 71. During his 1951 presidency, Walter Nungester began the laborious process of "establish[ing] a broad accrediting organization 'American Institute for Microbiologists.'" See Walter J. Nungester, "Memorandum: Proposed SAB Program for 1951," January 6, 1951 in ASM 13-IIBA (Presidential Papers Walter Nungester).

[271] See e.g. Ira Baldwin to Walter Nungester, July 13, 1947, in ASM 13-IIAT (Baldwin Presidential Papers), Folder 23. For a brief overview of the early use of the word "bacteriology" and the campaign to change it, see J. W. Bennett and J. Karr, "'The New Branches into Which Bacteriology is Now Ramifying' Revisited," *Yale Journal of Biology and Medicine* 72 (1999), pp 303-311.

and it was not until 1961 that the SAB renamed itself the American Society for Microbiology (ASM).

The creation of the AAM solved another controversy, as well, allowing the microbiologists to explicitly lobby the government without (as some members feared) risking the tax-exempt status of the SAB itself in doing so. Almost immediately, the AAM (under much the same cadre of leaders as the SAB) launched into a controversy over the formal status of bacteriologists within the federal Civil Service.[272] Resentment over bacteriologists receiving a lower pay scale for a given GS grade than related classifications like biochemists had festered among these leaders for over a decade, and with the AAM available to serve as an explicit lobbying organization, they set about campaigning for bacteriologists' pay to be raised. This campaign (which was moderately successful) was of obvious interest to the Detrick community, and Ira Baldwin (who was heavily involved in the AAM lobbying push) drew upon Detrick contacts for information and gossip in making his case. Baldwin was particularly interested in supporting the argument that Detrick would retain researchers longer if Detrick salaries were more competitive compared to those offered by universities and industry. In one sense, this lobbying by a 'friend' of the Detrick community, using information from Detrick administrators, to financially benefit members of that community appears similar to moves to support Detrick like Nungester's 1951 committee. More realistically, however, even the most committed supporters of bioweapons development with the SAB leadership were equally or more interested in the improved professional standing

[272] "Bacteriologists," as a category, generally also included virologists in this period, though the temptation to try to be classified as a biochemist for the greater salary this could command was omnipresent.

increased Federal salaries would supply for microbiology as a whole. The Detrick community was just one SAB constituency aided by this move, albeit a large one. Even if this AAM campaign was not directly on behalf of Detrick, however, it did serve the Detrick community's interests. The size of the microbiological community at Detrick, which had (in Rosebury's words) "a status… more or less equivalent to that of a great university" meant that "unless the Society is prepared to anathematize all of these people" (as Henry Scherp said in 1952), even SAB leaders who cared nothing about biological warfare often found themselves on Detrick's side.[273]

While professionalization often entailed supporting such elements of the Cold War state, lending this support was sometimes an explicit goal. During Walter Nungester's presidency in 1951 (a year in which "we are faced with both Society and National Defense problems"), he justified many of his reformist measures as a way for the SAB to deal with both problems at once.[274] Nungester set out to "establish a broad accrediting organization 'American Institute for Microbiologists,'" to "appoint a special committee on Public Relations… to make a preliminary report… on the ways and means of improving our professional standing in the eyes of the public as the Chemists and Physicists have done," and to "prepare our own register [of microbiologists] and make it available to governmental agencies."[275] All of these ideas would serve to raise the standing of the science of microbiology. The role of a public relations committee in doing so is obvious, while certification and Nungester's scheme of compiling a register of

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[273] Rosebury quote from Theodore Rosebury, "Medical Ethics and Biological Warfare," *Perspectives in Biology and Medicine* 6 no 4 (1963), pp 512-532. Scherp quote from Henry W. Scherp to Richard Donovick, June 10, 1952, in ASM 8-IE "Ad Hoc BW Committees," Folder 2.
[274] Walter J. Nungester, "Memorandum: Proposed SAB Program for 1951," January 6, 1951 in ASM 13-IIBA (Presidential Papers Walter Nungester).
[275] Ibid.

microbiologists served to enhance microbiologists' job prospects with a large prospective employer.[276] As Nungester noted, a registry "could also be useful to our own Placement Bureau."[277] Nungester was also clear in his defense justification, however, arguing later that year that projects like certification were necessary because "it is hard to define a 'bacteriologist' at the present time despite the urgency to do so in terms of the needs of Civil Service and the Defense Department."[278] Offering the Cold War state a ready pool of certified researchers to draw upon whenever it needed them served patriotic and professionalizing impulses all at once.

For all that the SAB's leaders effectively prevented their society from becoming a venue for open discussion about biological warfare through the rest of the 1950s, behind the scenes they simultaneously enmeshed it more and more deeply in supporting the program at Detrick. Initially, SAB leaders like Nungester who were part of the informal network of 'friends' of the Detrick community sought to use the SAB to lend legitimacy to that community. For instance, in 1953 when the Detrick community (and attendant 'friends' like Ira Baldwin) found itself enmeshed in a power struggle with Chemical Corps leader General Egbert Bullene, SAB president and former Detrick researcher Gail Dack was happy to lend his support to Baldwin's campaign "to continue to continue pushing General Bullene to give more recognition to bacteriologists."[279] Coordinating with Baldwin, Dack planned a letter to Bullene ostensibly inquiring "whether there is anything the [SAB] might do that would be of help to the Chemical Corps in its program

[276] Ibid.
[277] Walter J. Nungester, "Memorandum: Proposed SAB Program for 1951," January 6, 1951 in ASM 13-IIBA (Presidential Papers Walter Nungester).
[278] Walter J. Nungester to Council of the SAB, October 2, 1951, both in ASM 13-IIBA (Nungester Presidential Papers), Folder 5.
[279] Baldwin to Dack, August 26, 1953, UWA Baldwin Papers, Box 12 Folder 1.

on biological warfare." Given the importance for the Corps of maintaining good relations with related scientific communities (especially, as Dack's planned letter noted, the American Chemical Society), the unstated emphasis of this letter was that the SAB was watching when the Corps came in conflict with its members at Detrick. Still more directly, Dack suggested that Baldwin mention unnamed SAB officers in his discussions with Bullene to resolve the Detrick crisis. As Baldwin frankly noted, "it would be desirable to have additional pressure from other sources," in his dealings with Bullene, "and it seems to me that you are in an admirable position to give a little pressure."[280] Notably, in all these schemes, Dack sought to speak for the whole SAB without any indication that he consulted with other members. The one-year SAB presidency was viewed as a generally honorific position for senior members of the microbiological community in this period, with the multi-year Secretary-Treasurer acting as the major manager of day-to-day SAB business. As the actions of presidents like Nungester and Dack demonstrated, however, it was nonetheless a position which gave its occupant access to the weight of the Society when they wanted to invoke it. A corollary of this, of course, was that who the president was any given year had a great deal to do with how much the Society (through them) sought to influence bioweapons research. For all of the enthusiasm of Detrick alums like Nungester and Dack (presidents in 1951 and 1953), for instance, the SAB seems to have ignored the issue of BW under the 1954 presidency of C. B. van Niel, who had had nothing to do with bioweapons research. Nonetheless, the

[280] Ibid.

fact that a full half of SAB presidents in the 1950s did have such connections meant that as often as not, a Detrick 'friend' was in control of the Society.[281]

Thus when in 1955 the SAB came under the presidency of H. Orin Halvorson, a member of the Chemical Corps Advisory Council, another Detrick 'friend' was in a position to further solidify the SAB-Detrick relationship. One of Baldwin and Dack's ideas in 1953 had been to suggest that the SAB sponsor a group to advise the scientists at Detrick, modeled on a similar committee sponsored by the American Chemical Society to support Chemical Corps chemists at Edgewood Arsenal.[282] General William Creasy, who replaced Bullene as head of the Corps shortly afterward, was generally interested in shoring up the Corps' relationship with the new scientific constituency of microbiologists and expressed his support for the idea to Baldwin in 1954.[283] As this was when the SAB was under the presidency of van Niel, Baldwin demurred, but the next year, when Halvorson ascended to the presidency, forming such a group was high on his docket.[284] Unlike earlier presidential pet projects, this "Committee Advisory to Fort Detrick" was to be a permanent fixture of the SAB, with volunteer members appointed for multi-year terms and new members recruited regularly. Reflecting the hybrid nature of a privately sponsored committee providing advice to government scientists, committee members would be appointed only with the understanding that they could hold security clearances

[281] See Table 1, below.
[282] "Suggested Letter from Gail M. Dack," attached to I. L. Baldwin to G. M. Dack, August 21, 1953, in UWA Baldwin Papers, Box 12 Folder 1.
[283] A. P. Colburn to Major General William M. Creasy, July 12, 1954 and I. L. Baldwin to A. P. Colburn, July 16, 1954, Major General William M. Creasy to A. P. Colburn, July 28, 1954, all in UWA Baldwin Papers, Box 12 Folder 1.
[284] The SAB typically elected future presidents to the vice presidency the year before their ascension. Baldwin thus would have known in 1954 that friendlier leadership in the SAB than van Niel's was forthcoming.

from the military, thus placing future SAB presidents in the potential position of appointing members to an SAB committee to which they themselves were not privy. In practice, this system relied heavily upon pre-existing networks of ex-Detrick researchers and close allies of Detrick, essentially institutionalizing the informal network of such Detrick 'friends' that already existed within the SAB. For instance, the initial appointees to the committee included Gail Dack himself and Arthur Geoffrey Norman (a University of Michigan plant pathologist who had left Detrick in 1952).[285] Future members would include Sanford Elberg, and Werner Braun, a bacterial geneticist who worked at Detrick for almost ten years before leaving for Rutgers University.[286] The fact that the SAB sponsored this committee was ostensibly no secret, with its membership being openly listed alongside those of other Society committees on SAB documents. Nonetheless, the culture of silence attached to biological warfare within the SAB seems to have extended to this committee as well: when the Northern California Branch of the then-American Society for Microbiology began debating this committee in 1967 (see below), many members were apparently surprised to learn that the committee existed at all.[287]

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285 See "S.A.B. Committees, 1957" in ASM Series 1-IIB "Governance" Folder 1 for a list of the original committee members.

286 Elberg joined the committee in 1959, Braun in 1963. See e.g. "Committees and Representatives, 1963-1964" in ASM 1-IIB "Governance" Folder 1. Elberg had done contract research for the BW program during WWII, and was briefly employed by Detrick after the war. See Elberg, "Graduate Education and Microbiology at the University of California, Berkeley," pp 28-38, 40. Braun, meanwhile, remained a vocal supporter of Detrick even before joining the Advisory Committee. For instance, he joined with Ira Baldwin in writing a 1961 memorandum for ASM president Colin McLeod advocating greater funding for Detrick. See "Confidential memorandum for Dr. C. McLeod to be used at his discretion," November 29, 1961 in ASM 13-IIAT (Baldwin Presidential Papers), Folder 54.

287 One participant in this Northern California Branch discussion noted that "many A.S.M. members confessed that they did not know of the existence of this Committee until the recent debate." See "Proposal Resolution from the American Society for Microbiology's Northern California Branch on its involvement in biological warfare preparation and defense," January, 1968, p 8 in NLM Profiles in Science Lederberg Papers (NLM ID: 101584906X18794). Berkeley's George Hegeman, for example, noted that "I know that at the time I joined [the ASM] I didn't know [the committee] existed and only learned of it fairly recently." See "Typescript of the proceedings of the American Society for Microbiology's Northern California Branch

In explicitly modeling their committee on the pre-existing ACS body, SAB leaders were once again turning to the chemists in their quest for professional status. Indeed, once the SAB committee was formed, the two groups collaborated closely, with the same Chemical Corps staff supporting both and with a joint newsletter being published by the late 1950s. Like the ACS group, the SAB group met a few times annually with scientists at Detrick, with individual members consulting closely with Detrick laboratories whose research matched their interests. Elberg, who chaired the Committee in the mid-1960s, explained in a 1967 report to the new ASM president, William Sarles (another Wisconsin Detrick alum) that "the usual plan of the meetings… is, essentially, to hear reports of research progress, actions taken on earlier recommendations, and visits with the research scientists in their laboratories. This… was accomplished by assigning to each committee member a group of laboratories reflecting his own area of research competence. These visits entailed 2 to 3 hour discussions of the current studies in whatever detail we desired," followed by "the committee reconvene[ing] to hear each member discuss candidly and critically his impressions of the directions and quality of the scientific investigations with the Scientific Director, Dr. Housewright."[288] This system provided direct scientific support to wide array of Detrick research programs ("cover[ing] the area of viruses, genetics, microbial physiology, tissue culture, histopathology, applied and basic immunology, pathogenesis of microbial infections, identification methods, and aerobiology" in 1967), though the practical impact

debating biological warfare on November 11, 1967," p 78 in NLM Profiles in Science Lederberg Papers (NLM ID: 101584906X18589)

[288] Sanford Elberg to William Sarles, March 30, 1967, in ASM Archives, Series 8-IA "Miscellaneous Committees Advisory to Fort Detrick," Folder 1.

of this support was limited by the fact that the Committee only met annually.[289] What was more important than direct aid is what the Committee represented. For the Detrick community, this formalized tie to the SAB/ASM represented a valuable endorsement in the politics of military research, particularly given the glowing reviews the Committee was prone to offering. Indeed, Elberg argued that the ASM should do more to rectify "adverse management control policies… fostered by the apparent failure of the Defense Department to coopt the highest quality of advisory personnel available in microbiology and immunology into the Defense Department's <u>highest levels</u> of committee structure or Civil Service" by "recommend[ing] that representations be made to the Secretary of Defense to invite the highest quality of the Society's membership to serve at the key levels of scientific administration of the Defense Department."[290] Conversely, ties between Detrick and the wider microbiological community were formalized and shored up by the existence of the Committee, particularly given the informal peer review that Committee members provided to Detrick scientists. It seems that this, like the political support, was a conscious goal of academic 'friends' of Detrick. As the Committee Advisory to Detrick (now under the chairmanship of Chicago's James W. Moulder) collectively noted when its future was in question in 1968, "the U.S. Army Biological Laboratories tend to become isolated from the rest of the scientific community because of unavoidable security restrictions, and it is, therefore, most important that they maintain effective relationships with leaders in appropriate fields of microbiology and medicine." "If the ASM Advisory Committee is discontinued," alternatives like "even more intensive use of non-governmental scientific consultants" or "the formation of a panel of technical

[289] Ibid.
[290] Ibid. Emphasis in original.

advisors…" with "no connection with the ASM other than the fact that many of its members would also be members of the Society" would be necessary to replace it.[291]

Interpersonal relationships like those fostered by the committee loom large in the stories of researchers who spent part of their careers at Detrick. 1961-1965 committee member Orville Wyss of the University of Texas, for instance, was at the center of a network of former and prospective students which cycled between Austin and Detrick in the 1950s and '60s. The story of bacterial geneticist and eventual University of Texas professor Thomas S. Matney is illustrative of the role such networks could play in a Detrick researcher's career. Upon earning his Master's at Trinity University in 1951, Matney volunteered for the Chemical Corps' officer's program to avoid the Korean War draft at the advice of one of his professors, Roy Mefford, Jr, who held a Corps commission himself. Mefford's advice steered Matney to Detrick and into Wyss' network, including putting him in contact with his eventual wife Glenda Oglesby, a former Wyss student then working at Detrick. After working at Detrick for several years in Werner Braun's bacterial genetics laboratory, Matney considered two universities for PhD work: Rutgers, where Braun had gone in 1955, and Wyss' UT Austin. Preferring the Texas location, Matney applied with an informal phone call to Wyss, earning his PhD by 1958 with the help of data from his time at Detrick. He then returned to Detrick for 4 years as a civilian researcher, before securing a job elsewhere in the UT system with another phone call to another member of Wyss' network, Felix Haas. Matney then spent

[291] "Report of the Committee Advisory to the U.S. Army Biological Laboratories to the American Society for Microbiology- March 15, 1968" in ASM Archives, Series 8-IA "Miscellaneous Committees Advisory to Fort Detrick," Folder 1.

the remainder of his career at Texas.[292] As Matney's case highlights, interpersonal relationships between Detrick-linked academic scientists and their students could be an effective conduit to channel those students to Detrick, but they were also an equally effective means for those students to leave Detrick- either for further graduate work, or to transition into an academic career.[293] Wyss' Detrick ties antedated his Advisory Committee membership, which was equally true of some other members like Braun, Elberg, or Wisconsin's J. B. Wilson.[294] Other members, like the NIH's Karl Habel, however, do not seem to have had such formalized ties before they joined the Committee. Recruiting such members thus gave Detrick scientists the potential to forge more interpersonal connections than they would otherwise have had access to. These connections enabled Detrick researchers to be relatively mobile: while some made their careers there, other post-WWII researchers spent just a few years there (including postdoctoral researchers like Dean O. Cliver), and others (like Werner Braun or A. G. Norman) were able to find academic jobs even after spending over a decade working at Detrick.[295] A 1965 survey of Detrick scientists provides a statistical window into the

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[292] Thomas S. Matney, "Oral History Interview with Thomas S. Matney, September 27, 2007," Recording of an interview conducted 2007 by Lesley Williams Brunet, University of Texas MD Anderson Center, 2007, retrieved from https://texashistory.unt.edu/ark:/67531/metapth163881/

[293] James Duff had a similar story, having worked at Detrick from 1949-1956 before moving to UT Austin to do graduate work under Wyss before returning to Detrick between 1959-1960. See James Duff, "Interview with Dr. James Duff, March 23, 1995," Transcript of an interview conducted 1995 by Carl G. Baker, Office of NIH History and Stetten Museum, 1995, p 1, retrieved from https://history.nih.gov/display/history/Duff%2C+James+1995. Yet another of Wyss' students, I. Cecil Felkner, credited Wyss for influencing his decision to serve as a Detrick researcher upon being drafted in the early 1960s, and subsequently returned to Texas to complete his PhD under Wyss. See "Army's Soldier-Scientist Program Gets Boost from Felkner," *Army Research and Development Newsmagazine* 3 no 7 (July 1962), pp 33-34.

[294] Elberg also drew upon Detrick contacts to place students, such as brucellosis specialist Sidney Silverman. See Elberg, "Graduate Education and Microbiology at the University of California, Berkeley," p 69.

[295] "Memorial Resolution of the Faculty of the University of Wisconsin-Madison On the Death of Professor Emeritus Dean O. Cliver," University of Wisconsin, Madison Faculty Document 2284, October 3, 2011. Joshua Lederberg to Werner Braun, September 25, 1954 in NLM Profiles in Science Lederberg Papers

workforce there (see Figure 2, below). A substantial fraction of the employees surveyed in 1965 had been at Detrick for a substantial portion of their career, with a few holdovers from WWII having been buttressed by a large number of Korean War-era hires. Another substantial portion, however, had only worked at Detrick for a few years, probably reflecting a substantial rate of staff turnover.[296]

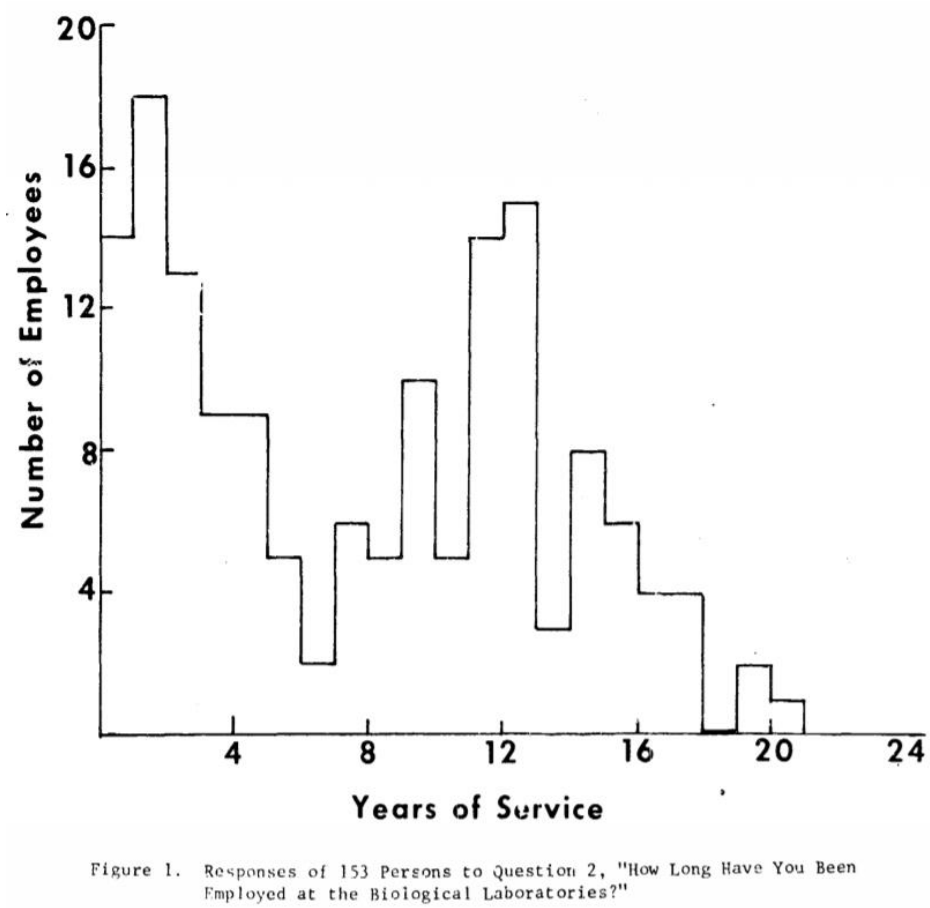

Figure 2: Results of a 1965 Survey on Scientific Communications at Detrick. NB spike at 12 years of service, a hiring date of 1952-1953[297]

(NLM ID: 101584906X5077); Ira L. Baldwin to A. G. Norman, May 29, 1952 in UWA Baldwin Papers, Box 11 Folder 10.

[296] The other possible explanation is that there was a hiring spike in the early 1960s. I have found no evidence of this, however, suggesting that these employees hired in the early 1960s were simply replacing other short-haul employees who had been hired in the late 1950s.

[297] George H. Nelson and Donald M. Hodge, “Biological Laboratories Communication (Fort Detrick Miscellaneous Publication 13),” Fort Detrick: United States Army Biological Laboratories Technical Information Division, 1965, p 16.

Protest and Dissolution: 1967-1971

By the mid-1960s, links between the Detrick community and the wider community of the newly renamed American Society for Microbiology seemed stronger than ever, with Detrick's Scientific Director, Riley Housewright, even serving as ASM president in 1966. The Advisory Community and less-formal interpersonal networks remained strong, and more than half of ASM presidents during the 1960s (including Housewright) were or had been connected to Detrick at some point in their careers. Developments both outside and inside the ASM community, however, would throw this seemingly strong relationship into turmoil by the end of the decade. The Vietnam War loomed over a broad array of American scientific communities in the late 1960s, and the ASM was no exception. For many scientists, particularly younger scientists influenced by the politics of the New Left, the war cast in stark relief the moral challenge of the collaboration with the Cold War state which their profession had undertaken over the past quarter-century. This anxiety was particularly acute where the subject of "chemical and biological warfare" was concerned, with American tear gas and herbicide use in Vietnam mobilizing criticism of germ and gas warfare not seen since the 1920s. It was perhaps inevitable, then, that the ASM would be a front line in this particular battle for the soul of science. Changes within the ASM community, too, contributed to challenge the relationship with Detrick. Simply put, while the cadre of increasingly senior scientists who had led the ASM community since the 1940s remained committed to the Detrick relationship, the community they led had grown too large and too young for them to really represent their constituents' opinions. Some young ASM members, like other young scientists of the period, held sharply different political and ethical views than their elders. More broadly, the culture of silence surrounding Detrick had over the years weakened its ties to younger members of the community. What had once been a shield from criticism was now a recipe for isolation. By the end of the 1960s, Detrick and the ASM leaders who supported it found themselves confronted by an increasingly vocal minority of opponents, and larger portion of the community who were simply indifferent to the survival of the carefully maintained relationship.

The presidents of the SAB/ASM could be significant actors in negotiating the relationship between their Society and the military (as in the case of Walter Nungester, Gail Dack, or H. Orin Halvorson), but they were also, in aggregate, a clear symbol of the intimacy of this relationship. Elected for one-year terms, the presidents of the group had accomplished careers behind them, and they were being honored with their position by their colleagues for their scientific work and service to the community. They represented the ideals of the community of American microbiologists. It is noteworthy, then, that of the presidents who served in the 40 years between 1930 and 1970, a full half had connections to bioweapons research at some point in their careers. These scientists can be divided into three groups: presidents from the 1930s who as prominent leaders in their field served on the NAS committees directing BW research during WWII (for example, Wisconsin's E. B. Fred), scientists who had served at Detrick or done contract research at their home institutions during the war (like Ira Baldwin), and scientists who worked at Detrick after the war, accepted research contracts from Detrick or exchanged sample cultures with Detrick scientists, or (most prominently) who served on postwar military advisory committees. This last group often wore multiple hats, with Halvorson and Herald Cox serving as Chemical Corps advisors during their presidency, and 1966 president Riley Housewright serving as Detrick's Scientific Director. Notably (and probably reflecting the sharp change in political winds after 1969), very few ASM presidents since 1970 have had the connections to biological warfare that their forbearers did. Indeed, virologist Alice S. Huang, who in 1968 was a protester against biological warfare at the 1968 ASM meeting (see below), became the group's president in 1988.[298]

[298] See David Baltimore, "David Baltimore (Oral History Transcript 0198)," Transcript of interviews conducted in 1994 and 1995 by Sondra Schlesinger, Science History Institute, 1995, p 56, which mentions her participation in this group. Baltimore is Huang's husband.

Table 1: SAB/ASM Presidents Connected to Biological Weapons Research

Presidency	Name	Connection
1930	Stanhope Bayne-Jones	NAS Committees; Advisory Committees[299]
1932	Edwin B. Fred	NAS Committees[300]
1933	William Mansfield Clark	NAS Committees[301]
1935	Karl F. Meyer	WWII Contract Research[302]
1936	Thomas M. Rivers	NAS Committees[303]
1937	James Morgan Sherman	WWII Contract Research[304]
1944	Ira L. Baldwin	Detrick (WWII); Advisory Committees[305]
1946	James Craigie	Canadian Collaboration with Detrick[306]
1951	Walter J. Nungester	Detrick (WWII); Advisory Committees[307]
1952	René Dubos	WWII Contract Research[308]
1953	Gail M. Dack	Detrick (WWII), Advisory Committees[309]
1955	Halvor O. Halvorson	Advisory Committees, Culture Sharing[310]

[299] See correspondence in NAS BW, Box 7, Folder 5 (Bayne-Jones, Dr. Stanhope: 1941-1942). Bayne-Jones later joined the Armed Forces Epidemiology Board in the late 1950s, a time when it was closely collaborating with Detrick's Project Whitecoat and making a power grab for control of all medical bioweapons research. See Albert E. Cowdrey, *War and Healing: Stanhope Bayne-Jones and the Maturing of American Medicine*, Baton Rouge: Louisiana University Press, 1992, pp 185-186.

[300] NAS BW Box 7, Folders 17-19, (Fred, E.B.: 1942-1943; Fred, E.B.: Memoranda (Black Book): 1943-1944; Fred, E.B.: Memoranda (Black Book): 1942-1943). Fred, one of the few bacteriologist members of the Academy before the war, had been a major force behind the NAS' initial interest in biological warfare, and personally enlisted his Wisconsin protégé Ira Baldwin to serve on the subsequent WBC Committee. See Ira L. Baldwin, *My Half-Century at the University of Wisconsin: Adapted from an Oral History Interview by Donna Taylor Hartshorne* (Madison, WI: Privately Printed by Ira L. Baldwin, 1995), p 121.

[301] NAS BW Box 7 Folder 9 (Clark, W. Mansfield: 1941-1942).

[302] BAS BW Box 8 Folder 17 (Meyer, Dr. Karl F.: 1942-1945); Karl F. Meyer, "Medical Research and Public Health," Transcript of interviews conducted 1961-1962 by Edna Tartaul Daniel. Oral History Center, The Bancroft Library, University of California, Berkeley, 1976, pp 200-201.

[303] NAS BW Box 8, Folder 22, (Rivers, Dr. Thomas M.: 1942-1944).

[304] NAS BW Box 9 Folder 1 (Sherman, James M.: 1941-1942).

[305] Baldwin, *My Half-Century*.

[306] Craigie sat on the Canadian advisory committee for the joint US-Canadian rinderpest research project at Grosse Île, Quebec. See Donald H. Avery, *The Science of War: Canadian Scientists and Allied Military Technology During the Second World War*, Toronto: University of Toronto Press, 1998, Chapter 6 note 63, p 329. He also did contract research on psittacosis and dysentery at the University of Toronto. See Martin L. Friedland, *The University of Toronto: A History, 2nd ed*, Toronto: Toronto University Press, 2013, pp 358-359. For more general background on the Grosse Île project, see Chapter 2 of Amanda Kay McVety, *The Rinderpest Campaigns: A Virus, Its Vaccines, and Global Development in the Twentieth Century*, New York: Cambridge University Press, 2018.

[307] Former Detrick scientific director Riley Housewright later remembered Nungester as "one of the senior civilians who was responsible for bringing a large number of his own students and other microbiologists to Fort Detrick during WWII." Riley D. Housewright to J. M. Joseph, January 6, 1982, in ASM 13-IIBA (Nungester Presidential Papers), Folder 2 ("Honorary Membership Material").

[308] NAS BW Box 7 Folder 11 (Dubos, Rene J.: 1942-1944).

[309] Dack was the first director of Detrick's Safety Division, and later served on the SAB/ASM Committee Advisory to Detrick.

[310] Not to be confused with his son Harlyn O. Halvorson, also a microbiologist and ASM president. The elder Halvorson also drew upon Detrick contacts for bacterial cultures, using a Detrick-grown spores for research in the mid-1950s. See G. G. Krishna Murty and H. Orin Halvorson, "Effect of Duration of

1957	Perry W. Wilson	WWII Contract Research[311]
1961	Herald R. Cox	Advisory Committees[312]
1964	J. Roger Porter	Detrick (WWII), Advisory Committees[313]
1965	Orville Wyss	Advisory Committees, Culture Sharing[314]
1966	Riley Housewright	Detrick (WWII and after)[315]
1967	William Sarles	Detrick and Porton Down (WWII)[316]
1969	Dennis W. Watson	Detrick (WWII)[317]
1970	E. M. Foster	Detrick (WWII)[318]
1975	Philipp Gerhardt	Detrick (WWII)[319]
1979	Edwin Lennette	Detrick (WWII and after)[320]
1990	Walter R. Dowdle	Post-war Contract Research[321]

Heating, L-Alanine and Spore Concentration on the Oxidation of Glucose by Spores of *Bacillus cereus* var. *Terminalis*," *Journal of Bacteriology* 73 no 2 (1957), pp 235-240.

[311] A University of Wisconsin colleague of Fred, Baldwin, and Sarles, Wilson stayed at Wisconsin during the war and (reflecting Wisconsin's industrial microbiology focus), did contract research on project "AU" (on the mass culture of spores). See NAS BW Box 9 Folder 15 (Wilson, Dr. P.W.: 1943-1944).

[312] Cox served on the Chemical Corps Advisory Council. See e.g. The Secretariat of the US Army Chemical Corps Advisory Council, *Committee (ACS and SAB) Advisory to the Chemical Corps Newsletter* 63, May 20, 1958, p 5. Copies of the 1958-1962 run of this newsletter are held at the National Library of Medicine.

[313] Porter was "one of the first half-dozen who started the operation [at Detrick] back in 1943." See J. Roger Porter, "Meeting with Dr. Vincent McRae, OST, October 2, 1970," in ASM 8-IA Folder 1 ("Miscellaneous Committees- Advisory to Ft. Detrick- Correspondence of D. E. Shay.") He later served on unspecified Defense Department advisory committees, apparently due to Walter Nungester's influence. See Walter Nungester, "Professional Activities of W.J. Nungester- Compiled for Personal Use December 1977," p 17, in ASM 13-IIBA Folder 2 ("Presidential Papers- Walter Nungester.")

[314] Wyss served on the ASM Committee Advisory to Fort Detrick between 1961 and 1965. Wyss also drew upon Detrick contacts for bacterial cultures, using Detrick-grown spores for research in the 1960s. See Ira Cecil Felkner and Orville Wyss, "Transformation in *Bacillus cereus* 569: A Correction of Strain Designation," *Biochemical and Biophysical Research Communications* 32 no 1 (1968), pp 44-47. Wyss' co-author on this paper, PhD student I. Cecil Felkner, had served as a Detrick researcher as a draftee earlier in the 1960s. See "Army's Soldier-Scientist Program Gets Boost from Felkner," *Army Research and Development Newsmagazine* 3 no 7 (July 1962), pp 33-34.

[315] Housewright joined Detrick in 1943 and spent the vast majority of his scientific career there, serving as its Scientific Director from 1956-1970 (including during his 1966 service as ASM president).

[316] Sarles held a commission as a Commander in the US Navy during the war, and served the bulk of this time acting as a liaison between Detrick and the British biological weapons program at Porton Down.

[317] University of Minnesota Archives (UMN) UA-01167 (Dennis Watson Papers) Box 3, Unnumbered Folder Entitled "Anthrax Biological Warfare Camp Detrick, 1944-1969."

[318] E. M. Foster, Recording of an interview by Barry Teicher, January 13, 2000, University of Wisconsin, Part 1, available at https://minds.wisconsin.edu/handle/1793/70327. Foster was also considered briefly to serve as Committee X's executive secretary. See Ira Baldwin to Colonel A. T. Thompson, April 24, 1947, in UWA Baldwin Papers, Box 15 Folder 1.

[319] See e.g. Lynn L. Gee and Philipp Gerhardt, "*Brucella suis* in Aerated Broth Culture: II. Aeration Studies," *Journal of Bacteriology* 52 no 3 (1946), pp 271-281, which lists the authors' affiliation with Camp Detrick.

[320] Edwin H. Lennette, "Edwin H. Lennette: Pioneer of Diagnostic Virology with the California Department of Public Health," transcript of an interview conducted in 1982, 1983, and 1986 by Sally Smith Hughes, Regional Oral History Office, The Bancroft Library, University of California, Berkeley, 1988.

[321] See Walter R. Dowdle and P. Arne Hansen, "Labeling of Antibodies with Florescent Azo Dyes," *Journal of Bacteriology* 77 no 5 (1959), pp 669-670; Walter R. Dowdle and P. Arne Hansen, "A Phage-Fluorescent Antiphage Staining System for *Bacillus anthracis*," *Journal of Infectious Diseases* 108 no 2 (1961), pp 125-135. Both papers acknowledge that they were "performed under contract with the United

While many of these leaders were part of networks of friendship and correspondence which had been established in the 1930s and '40s, the constituency they represented was changing rapidly. The SAB, which had been a small and close-knit organization of a few hundred members for its first few decades, began to grow explosively in the 1940s. From a Depression-era nadir in 1932, the SAB grew to 1,977 members in 1943, a number which had soared to 4000 members in 1951, and a little above 5000 members in 1956.[322] This number doubled within a decade, with the newly christened ASM boasting about 10,000 members by 1966 and 12,000 by 1968. Newly elected ASM president E. M. "Mike" Foster (a Wisconsin professor and Detrick alum) commented on this growth in 1970 in the presidential address traditionally held at the annual meeting, helpfully supplying charts like Figure 3 (below).[323] Logarithmic growth curves of the sort pictured there would have been immediately familiar to his listeners: they were those of a bacterial population, enjoying (through the largesse of a researcher) unbounded resources upon which to grow. A similar story lay behind this curve of professional affiliation, which like many American sciences was particularly fueled by the post-war influx of research funding from institutions like the National Institutes of Health and National Academy of Sciences. Microbiology in particular had been shaped by the rise of the field of bacterial genetics, which made microbes like *E. coli*

States Army Chemical Corps, Fort Detrick." Fluorescent staining techniques were of particular interest to Detrick researchers in the late 1950s because they seemed like a good candidate for an automatic rapid detector of airborne pathogens.

322 Baldwin's 1944 SAB presidential address mentions 755 members in 1932, rising to 1,977 in 1943. See Baldwin, "Where Does the Trail Lead?," p 1, in ASM Series 13-IIAT (Baldwin Presidential Papers), Folder 3. Walter Nungester mentioned 4000 members in 1951. See Walter Nungester, "Presidential Message: Our 1951 Program" in ASM Archives Series 13 II-BA (Nungester Presidential Papers) Folder 5. Baldwin later recalled that because of this small field, "I knew practically every bacteriologist in the United States" in the early 1940s. Baldwin, *My Half-Century*, p 122.

323 E. M. Foster, "Unde Et Quo" (ASM Presidential Address, April 28, 1970), in ASM Archives Series 13 II-AT (Baldwin Presidential Papers), Folder 68. Foster served at Detrick during WWII.

increasingly attractive model organisms for the burgeoning field of molecular biology. The population of "microbiologists" was no longer confined to a 'service' field of "microbe-hunters," fulfilling early SAB founders' dreams of raising their field's status within the life sciences. For the older generation of SAB members, however, the shifting qualitative and quantitative nature of the community that had served as an intellectual home was a bittersweet thing, with even Foster "shar[ing] the nostalgia of those among you who miss the good old days when the Annual Meeting was small and the beer at the Saturday Night Mixer was free."[324]

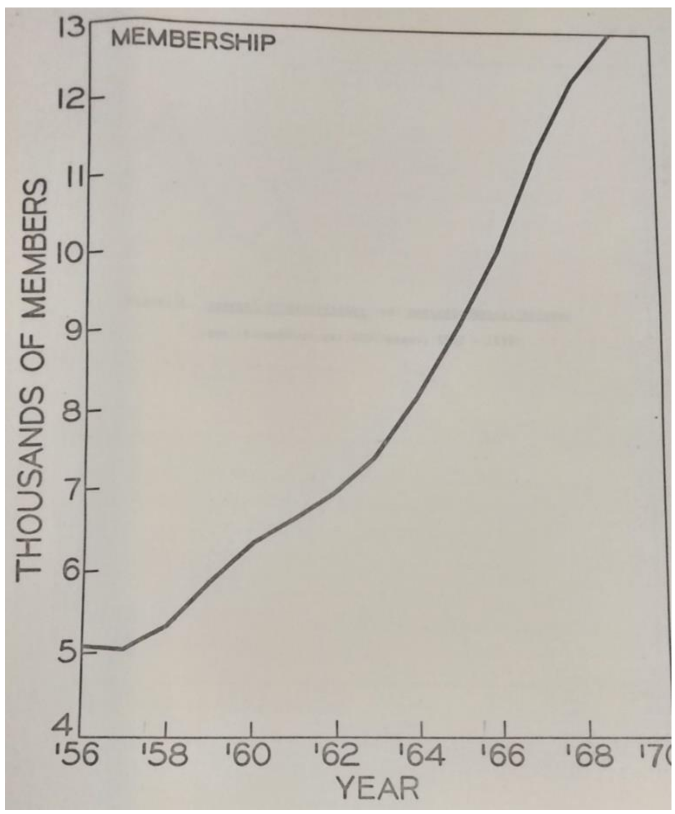

Figure 3: Logarithmic Growth of the ASM[325]

[324] Ibid, p 9.
[325] Foster, "Unde Et Quo," Figure 5.

The culture of silence about biological warfare prevailed within the ASM through this expansion into the late 1960s, but by the middle of the decade, the escalating Vietnam War increasingly strained it. The American use of herbicides and tear gas in the conflict combined with a growing anti-war sentiment within the general scientific community to produce a wave of critical attention to "chemical and biological warfare" outside the ASM, which soon impacted discussions inside it as well. A 1966 petition that the Johnson administration discontinue the use of CBW weapons in Vietnam began to circulate and eventually attracting thousands of signatures.[326] Privately, this petition concerned Detrick leadership and their confidants, though in public they kept quiet.[327] This increased notoriety also inspired a wave of science reporting on the chemical and biological weapons programs, which attracted more attention to those programs in turn.[328] Most notably within the ASM, a widely-read series of 1967 articles in *Science* by journalist Elinor Langer contained a discussion of the ASM's links to Detrick, including the fact that 1966 president Riley Housewright was Detrick's scientific director and that the ASM Committee Advisory to Detrick existed. Though the membership and general activities of the Advisory Committee were ostensibly public knowledge, being published in official ASM documents, Langer's reporting about its existence was evidently news

[326] See Moore, *Disrupting Science*, p 134.
[327] See e.g. Ira Baldwin, "Memo of Phone Call with Riley Housewright," September 20, 1966, UWA Baldwin Papers, Box 13 Folder 5.
[328] This is reminiscent of a shift in news reporting about the Hanford nuclear facility described by Daniele Macuglia. Before the 1980s, he argues, the matter-of-fact tone of post-1945 news reporting about accidents with radioactive material at Hanford contributed to a public culture of ambiguous silence about the dangers represented by the site, replaced in that decade by a genre of critical revelatory reporting that attracted increasing public attention. As with Detrick, this reporting did not reveal some grand secret about the existence of Hanford and basic sense of what went on there, but its public disruption of a previous culture of silence was significant nonetheless. See Daniele Macuglia, "Talking About Secrets: The Hanford Nuclear Facility and News Reporting of Silence, 1945-1989," in Felicity Mellor and Stephen Webster (eds), *The Silences of Science: Gaps and Pauses in the Communication of Science*, New York: Routledge, 2017, pp 115-134.

for many members of the organization's Northern California branch, including its president, Berkeley's Alvin J. Clark.[329] Writing a front-page editorial in the branch's March 1967 newsletter, Clark confirmed that "the committee reported by Ellinor Langer [sic] exists," and opined that "there are many questions which require clarification," most notably whether "the existence of an ASM advisory committee to the Army Biological Laboratories impl[ies] a moral commitment of the ASM to the precepts of biological warfare."[330] Seeking to answer these questions, Clark attempted to arrange for a lecture on biological warfare as part of an annual series funded by a bequest from Selman Waksman. Various names were floated and rejected: Rutgers' Werner Braun would be too expensive to fly out, Sanford Elberg (just finishing his tenure as chairman of the Advisory Committee) would embarrass Berkeley too much by publicly speaking about biological warfare, and "others have refused on various grounds including fear of breaking security restrictions."[331] To thwart this cloak of secrecy, Clark reached out to invite Housewright himself, diplomatically suggesting that "to avoid distracting publicity, we plan to hold the meeting outside of Berkeley." Knowingly or not, however, Clark undermined his own case in the same letter by suggesting that "one possible format for discussion would be a panel of three talking on professional, political, and moral aspects of biological warfare," including "notions like a kind of non-proliferation treaty for

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[329] Elinor Langer, "Chemical and Biological Warfare (I): The Research Program," *Science* 155 no 3759 (January 13, 1967), pp 174-179; Elinor Langer, "Chemical and Biological Warfare (II): The Weapons and the Policies," *Science* 155 no 3760 (January 20, 1967), pp 299-303.
[330] Alvin J. Clark, "President's Message," *American Society for Microbiology Newsletter Northern California Branch* 2 no 1 (March 1967), pp 1-2. Copies of this newsletter can be found in ASM Archives Series 2-IIY (Branches: Northern California) Folder 7. There is a certain irony in the fact that this newsletter was printed by the Naval Biological Laboratory in Oakland: evidently, unlike Berkeley's Stainer and Doudoroff, most Northern California Branch members did not regard the NBL as a biological warfare program in their midst.
[331] Alvin J. Clark to Riley D. Housewright, March 21, 1967, in ASM 8-1A Folder 2.

biological weapons…" and "moral aspects [which] may involve the Nuremberg trials."[332] This was precisely the kind of public discussion that ASM 'friends' of Detrick had been trying to avoid for the past two decades, and Housewright quickly fell back on the old tactic of stonewalling with secrecy. As he confided to William Sarles when he first caught wind of Clark's plans, "I can see no useful purpose in sending a 'sacrificial goat' to stand in front of a group of people to answer questions when the nature of the questions demonstrates a fixed and biased position."[333] Clark's open suggestion that morality had something to do with a discussion about biological warfare irredeemably tainted such a discussion for Housewright's cadre.

Housewright was not quite this explicit in his reply to Clark, instead subtly suggesting that a discussion of morality was unscientific by asserting that Waksman's bequest was for "a lecture or discussion on purely scientific subjects," and that consequently "a lecture or forum sponsored by the Foundation for Microbiology is inappropriate."[334] (This was a highly questionable assertion. As Clark dryly noted in his reply, "last year the Northern California Branch enjoyed a Foundation lecture from H. Orin Halvorson on the future of microbiology and the history of the ASM. At that time there was no objection to our desire to use the Foundation lectures to gain a closer and more personal view of our Society and the scientific forces shaping it than can be gained

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[332] Ibid.

[333] Riley D. Housewright to William B. Sarles, 16 March 1967, in ASM 8-1A Folder 3. While in the official ASM archives, this folder contains material which were originally in Housewright's personal files at Detrick, and thus includes more candid correspondence among Detrick 'friends' than can be found in the rest of this collection. These papers did not find their way into the ASM's hands until after Housewright retired from Detrick in 1970. On the provenance of these papers, see Dorothy S. Winpigler to L. S. McClung, September 3, 1970, in ASM 8-1A Folder 3.

[334] Riley D. Housewright to Alvin J. Clark, March 29, 1967, in ASM 8-1A Folder 2.

from the Newsletter.")[335] Instead, Housewright countered by offering to have a private conversation at the upcoming ASM meeting in New York. He also suggested J. Rothschild, the Chemical Corps' former Director of Research and Development, as an expert who "might offer a point of view that differs from one segment of your membership."[336] Rothschild had chaffed at official secrecy while serving in the Corps, and in retirement had become a vocal advocate of chemical and biological warfare, publishing a laudatory book on the subject in 1965. As Clark noted, however, Rothschild (a chemical engineer by training) was not an ASM member and thus could not authoritatively address questions about the Advisory Committee in particular.[337]

Faced with Housewright's stonewalling, Clark quietly let the idea of a June discussion drop, but began to arrange to host a meeting later in the year (see below). Worse still, from the perspective of Housewright and other Detrick 'friends,' Clark's Northern California branch instead turned to the national ASM for answers, submitting an official resolution asking information about the purpose and function of the Advisory Committee.[338] The national ASM meeting in New York now had to take official notice of these questions, precisely the kind of notoriety the pro-Detrick leadership had tried to avoid. This notoriety was further increased at the meeting as a group of members led by Issar Smith of the Albert Einstein College of Medicine circulated a petition decrying biological weapons research, identifying members of the Advisory Committee by name,

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[335] Alvin J. Clark to Riley D. Housewright, April 6, 1967, in ASM 8-1A Folder 2.
[336] Riley D. Housewright to Alvin J. Clark, March 29, 1967, in ASM 8-1A Folder 2.
[337] Alvin J. Clark to Riley D. Housewright, April 6, 1967, in ASM 8-1A Folder 2. A brief biographical sketch of Rothschild can be found in *Nuclear Test Ban Treaty: Hearing Before the Committee on Foreign Relations, United States Senate* 88th Cong. 1 (1963), p 743.
[338] H. J. Phaff, North California Branch, ASM, "Resolution to Council, American Society for Microbiology," April 7, 1967 in ASM 8-1A Folder 2.

and calling for the Committee to be dissolved.[339] They forced a formal floor vote on this last proposal, which was overwhelmingly defeated, but while most ASM members were apparently unwilling to support a proposal couched in the unabashedly moralistic language of the anti-war movement, the fact that the culture of public silence had been breached portended a bitter controversy over the Advisory Committee over the next few years.[340]

Pressure from the Northern California Branch and Smith's New York group merely compounded the unease of the Committee's allies in the spring of 1967. Their main concern was about changes in leadership: Sanford Elberg, the firmly pro-Detrick brucellosis expert who had served on the committee for the last 8 years and chaired it for the past two was scheduled to step down that summer, in favor of the University of Chicago's James W. Moulder, who had also had Detrick connections but who was less vocal in his support for the Fort. Worse still, Salvador Luria was slated to serve as the ASM president in 1968. Luria (who would win the Nobel Prize in 1969) was an eminent molecular biologist, precisely the kind of high-stature scientist the ASM tended to honor with the presidency, but he was also an outspoken opponent of the Vietnam War and biological weapons.[341] He had in fact been a ringleader of the group that had commissioned Stuart Mudd's 1947 petition calling for the then-SAB to issue a public statement on biological warfare, and had recently been a prominent signatory of the 1966

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[339] I. Smith, et al, "An Open Letter to the Members of the A.S.M.," May 1967, in ASM 8-1A Folder 1.
[340] The next year, Detrick defender Merrill J. Snyder recalled that this vote had been "defeated by some 300 plus to some 30 votes (at least a 10 to 1 ratio)." See Merrill J. Snyder, Statement at Annual Business Meeting of the ASM, May 7, 1968, in ASM 8-1A Folder 1.
[341] See Chapter 6 of Rena Elisheva Selya, "Salvador Luria's Unfinished Experiment: The Public Life of a Biologist in a Cold War Democracy," PhD diss, Harvard University, 2002 for a discussion of Luria's political activism in the 1960s, including his ASM presidency.

petition as well. 'Friends' of Detrick like 1967 president William Sarles were thus worried that "we are in for some rather emotional speeches and proposals," and when Luria expressed concern about the Advisory Committee in an April 1967 conversation with Sarles, the latter was quick to warn Elberg that "it is best to be prepared to supply the facts" to stymie Luria.[342] What Sarles suggested 'Sandy' do, in practical terms, "is to prepare a rather detailed statement that will clarify the reasons for and functions of the Advisory Committee," to forestall "attempts to get the Society to make a statement supporting or opposing" the government's bioweapons policy.[343] Elberg followed Sarles' advice, and prepared an unprecedentedly detailed official report to him about the Advisory Committee's Spring 1967 meeting at Detrick.

Meanwhile, the Northern California Branch's Alvin Clark continued his interest in holding a meeting on the Advisory Committee. The Branch eventually scheduled such a meeting for November 11, 1967, inviting as experts Rothschild, and Joshua Lederberg, a Nobel-winning bacterial geneticist who had maintained an ambiguous and circumspect relationship with Detrick and the subject of biological warfare throughout his career, but who had recently begun using a syndicated popular science column he wrote for the *Washington Post* to openly criticize the wisdom of BW research.[344] They were also joined by James Moulder, the new chairman of the Advisory Committee, sent as an

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[342] Both quotes from William B. Sarles to Sanford Elberg, April 3, 1967, in ASM 8-1A Folder 3. NB that this letter is in Folder 3 of the ASM collection, originating in Riley Housewright's personal files. It was cc'd to Housewright and ASM secretary (and Detrick 'friend' Philipp Gerhardt). See Footnote 333, above.
[343] Ibid.
[344] See e.g. Joshua Lederberg, "A Treaty Proposal on Germ Warfare," *Washington Post*, September 24, 1966; Joshua Lederberg, "Congress Should Examine Biological Warfare Tests," *Washington Post*, March 30, 1968; Joshua Lederberg, "Swift Biological Advance Can Be Bent to Genocide," *Washington Post*, August 17, 1968. Rothschild had served in various roles in the Corps, most notably as Director of Research and Development in the late 1950s, where he had so chaffed at the strictures of secrecy that after his retirement, he penned a popular jeremiad arguing for expanded CBW research. See J. H. Rothschild, *Tomorrow's Weapons: Chemical and Biological*, New York: McGraw-Hill, 1964.

official representative of the national ASM, and UCLA's William R. Romig, another Committee member. Hearing the standard Chemical Corps argument (that biological warfare was highly effective and ultimately more humane than conventional bullets and bombs) from Rothschild, and an idiosyncratic argument from Lederberg that "biological warfare is inherently a suicidal activity on the part of human beings," the California members began their debate.[345] The consensus of the room was quick to declaim agnosticism about the morality of BW research, reflecting a continued suspicion of discussing such 'irrational' considerations. Instead, the conversation quickly focused on how maintaining a professional relationship with such research reflected on the ASM. Was a committee that required a security clearance of its members consistent with an otherwise open scientific community? Was it consistent with an organization that, despite its name, included a small but noteworthy minority of non-American members? Most importantly did maintaining an official advisory committee serve as an official ASM endorsement of BW research? This last question turned the 1950s abhorrence of 'political' or 'social' questions (like the ethics of BW research or of meeting in a segregationist city) on its head, identifying the Advisory Committee as having its own political valence. The group's best argument in favor of the committee tacitly conceded this point, arguing that the Advisory Committee's existence was the ASM's best opportunity to influence bioweapons policy. The group wanted no part of policy, however, and voted to petition the national ASM to disband the Advisory Committee while diplomatically affirming the possible utility of bioweapons research for public health. The group thus did not explicitly challenge bioweapons research *per se,* but did

[345] Lederberg quote in "Typescript of the proceedings of the American Society for Microbiology's Northern California Branch debating biological warfare on November 11, 1967,", p 11.

strike against the ASM's tacit culture of support for such work by advocating that institutional ties be abandoned. Moulder and Romig, meanwhile, had been tepid defenders of the Committee, and Moulder in particular seems to have been convinced by arguments that the Committee should be replaced by a "committee on public policy" explicitly geared toward influencing government decision making and not requiring a security clearance.

Fresh from this meeting, Moulder and Romig joined the rest of the Advisory Committee for their biannual meeting at Detrick on November 27-28. Unlike the apparently-routine meeting they had held that spring, this meeting was devoted to the rising challenges to the Committee's very existence. Pessimistic about the value of retaining the Committee and conscious of growing opposition to it from his California trip, Moulder pushed for the solution he had accepted at the November 11 meeting: that they dissolve, and recommend that the ASM replace them with a 'public affairs' committee without formal government ties. As Moulder summarized this position a few months later, "I do not think it good for the Society to have a standing committee for any government agency."[346] Securing a unanimous vote to this effect, Moulder wrote to Luria, who would become president in a few months, alerting him that the committee would deliver a formal report with these recommendations in a few months. Luria, doubtless pleased that the Committee he was so suspicious of was proposing its own dissolution, naturally responded favorably. Other committee members, meanwhile, reiterated their support for this move over the next few weeks, but several also

[346] "Minutes of ASM Business Meeting, New Business: Advisory Committee to Fort Detrick," May 8, 1968, p 1 in ASM 8-1A Folder 1.

emphasized that their support for dissolving the committee stemmed from its perceived toothlessness, not an opposition to involving the ASM with Detrick. "[The Committee] really cannot serve in a real advisory capacity by meeting only two days a year," wrote Wisconsin's Joe Wilson. "If Detrick wants the ASM Committee as a truly advisory committee then… it could be enlarged and operated like the old Chemical Corps Advisory Council with a number of subject matter subcommittees that would meet several times a year."[347] "I gather that out the outset, the Committee was advisory to the Chief Chemical Officer and served a function as such," continued Wilson, who had only recently joined the Committee. Washington's Erling Ordal, who had chaired the Committee in the early 1960s before the 1965 Pentagon reorganization dissolved the old Chemical Corps, was more explicit, arguing that the "Committee performed a very useful function in the past when it was directory advisory to the General in charge of the Chemical Corps…" but "under the present circumstances after several reorganizations of the military structure, the ASM Committee no longer is advisory at several levels above the Technical Director and prior justification for official ASM status no longer exists."[348] Thus, though 1968 began with seeming consensus that the Committee should be discontinued, there were in fact three quite contradictory strains of opinion supporting this move within the ASM: those like Ordal and Wilson, who thought that the Committee was no longer effective at supporting Detrick, those like Moulder, who felt that the Committee's official ties to Detrick were an embarrassment to the ASM in such

[347] J. B. Wilson to James W. Moulder, January 3, 1968, in ASM 8-1A Folder 3.
[348] Erling J. Ordal to James W. Moulder, January 4, 1968 in ASM 8-1A Folder 3.

politically charged times, and those like the New York protesters and the new president Salvador Luria, who thought Detrick should not be supported in the first place.

These fault lines would be exposed in the subsequent spring months of 1968. Defenders of Detrick like Riley Housewright began waging a quiet influence campaign to save the Committee even before its official report was delivered, with Housewright counter-offering to place the Committee in touch with higher-ranking Defense Department officials to address the complaint that the Committee lacked influence. Assuming that anyone believed that Housewright had the political influence to deliver on this promise, it had the potential to attract the support of the Ordal-Wilson faction, offering Committee members like Donald Merchant (who had warned darkly about "evidence of poor organization… at Fort Detrick" in the wake of the Committee's waning influence) a new opportunity to "[establish]… a strong board of consultants… to give coordination in the direction of the supposed mission of Fort Detrick."[349] Erling Ordal, at least, was tempted by this prospect.[350] A parallel influence campaign from November 1968 called on Pentagon scientific advisors to evangelize about Detrick's "unclassified contributions… not only to the Defense Department but to the Scientific Community in general."[351] Statistics counting the number of open publications stemming from Detrick research (now numbering over a thousand) were a particularly valuable

[349] Donald J. Merchant to James W. Moulder, December 14, 1967, in ASM 8-1A Folder 3.
[350] Erling J. Ordal to James W. Moulder, July 9, 1968 in ASM 8-1A Folder 3. This letter, composed several months after Housewright's campaign, expressed regret that the ASM had not taken him up on his offer.
[351] This seems to have been spearheaded by Walter Nungester, who was now working for the Army Munitions Advisory Board. See W. J. Nungester, "Memorandum to Members of the B.W. Subpanel, Subject: Tentative Report of Meeting at Fort Detrick on 11/22/68," November 26, 1968, in UWA Baldwin Papers, Box 13 Folder 7 ("Munitions Advisory Group 1968-9").

talking point for this campaign to paint Detrick as a good scientific citizen.[352] At the same time, scientists opposed to associating with Detrick entirely were gaining increased notoriety, attacking these very links between the Detrick and open communities. 1968 was the 25th anniversary of Fort Detrick's founding, and among a series of celebratory dinners and speeches, Housewright planned to host a symposium, co-sponsored by the American Institute of Biological Sciences, on the hot-button topic of recombinant DNA. Though this symposium commemorated a special occasion, there was nothing particularly abnormal about Detrick hosting such events, with a "Symposium on Immunology" held in 1951, and a "Symposium on Nonspecific Resistance to Infection" (also sponsored by the AIBS) in 1959, for example.[353] 1968 was a very different year than 1959, however, and as word of the upcoming symposium circulated increasingly organized protest campaign based at the Rockefeller University and New York's Public Health Research Institute arose. Facing a crescendo of criticism from their colleagues, 16 of 20 invited speakers (including future co-founder of Genentech Herbert Boyer) withdrew from the symposium. The AIBS itself was divided over whether to continue its sponsorship of the symposium, but ultimately opted to do so. The symposium went ahead, with some substitute speakers drawn from the pool of 'friends' of Detrick

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[352] See e.g. "Meeting of B.W. Subpanel of M.U.C.O.M. Advisory Committee At Fort Detrick, November 22, 1968," in UWA Baldwin Papers, Box 13 Folder 7 ("Munitions Advisory Group 1968-9"), where Detrick's Harold Glassman proudly discussed 7 symposia having been held since 1960 and 1375 scientific papers having been published throughout Detrick's existence.
[353] See Program of MV Division, Camp Detrick, "Symposium on Immunology," January 10-Janurary 12, 1951, in UWA Baldwin Papers, Box 11 Folder 10; "Meetings: Nonspecific Resistance," *Science 130* no 3373 (August 21, 1959), pp 460-461.

(including the Advisory Committee's William Romig), but this incident highlighted the increasingly controversial status of Detrick within the microbiological community.[354]

With these events on the mind, the ASM Executive Council met to formally consider the Advisory Committee's report at the annual meeting in May 1968. They voted to accept the Committee's recommendation that it be dissolved, seemingly putting a quiet end to the ASM's formalized ties to Detrick. What followed was anything but quiet, however. At his traditional presidential address at the opening banquet of the full ASM meeting that followed, Luria announced and lauded this decision, presenting it as an ethical stand against secret military research by the ASM, and approvingly invoking the 1947 4th International Congress of Microbiology condemnation of biological warfare. This speech, accompanied by a shocked lack of applause, was followed by an official ASM press release, approved by Luria the next day, which quoted him as saying that "the ethical problems implicit in the association of a professional society with the defense establishment have always been present in the minds of the officers of the Society and have often been debated in its councils."[355] By invoking the question of ethics, Luria had stepped on a landmine. On the whole the ASM's leadership belonged to the Moulder faction, viewing the Committee as a professional embarrassment, or the Ordal-Wilson faction, viewing it as toothless. Neither faction rejected Detrick, however, and the accord between them and those members who did rested on maintaining the pretense of political and ethical agnosticism. Unintentionally or not, by invoking the question of ethics Luria

[354] Philip M. Boffey, "Detrick Birthday: Dispute Flares Over Biological Warfare Center," *Science* 160 no 3825 (April 19, 1968), pp 285-288.
[355] See ASM Press Release, May 8, 1968, in ASM 8-1A Folder 1. On the noteworthy lack of applause for Luria's Presidential Address, see remarks made by M. Chase, Minutes of ASM Business Meeting, May 8, 1968, p 2 in ASM 8-1A Folder 1.

had stripped away that pretense, tacitly acknowledging the fact that discontinuing the Committee did in fact have political connotations. Discontinuing the Committee to take an official stance against biological warfare had already been unpopular the year before, and among the leaders of the ASM Executive Council, remembered by 1969 anti-BW protester David Baltimore as being "close to or even [coming] from Fort Detrick" taking any sort of stand against BW research was seen "as a direct attack on members of the ASM."[356]

Luria's statements provoked an immediate backlash from these pro-Detrick leaders as the ASM Governing Council met a day after his offending speech. "I… was assured that pains would be taken to make clear that the reasons for the decision to discontinue the Advisory Committee had nothing to do with moral issues" thundered Johns Hopkins' Merrill J. Snyder, representative of the Maryland branch and vocal Detrick defender. "Those of you who were at last evening's banquet can understand how shocked I was at the content of our distinguished President's address." Luria's ethical stand "I maintain does <u>not</u> represent the intent of the majority… the virus of minority dissent and action has been sown and the epidemic effect of its virulence is evident in our communities, our publications, and in our centers of learning. I am saddened that the scientific community has become infected and that our society has succumbed to the fever of the times. The harm has been done but I see one solution to treatment-prophylaxis is too late. THEREFORE, I MOVE THAT A NEW COMMITTEE ADVISORY TO THE U.S. ARMY BIOLOGICAL LABORATORIES BE APPOINTED

[356] Baltimore, "David Baltimore (Oral History Transcript 0198)," p 56.

BY THE AMERICAN SOCIETY FOR MICROBIOLOGY."[357] For all of Luria's apologetic backpedaling, most members of the Governing Council were sympathetic to Snyder's Nixonian rhetoric.[358] A Dr. Greenberg summed up the mood of the room. "I am personally in favor of the suggestion made by Dr. Snyder and was very disturbed by the Presidential release," he opined. "Why has all this noise about biological warfare suddenly been pushed up to the forefront of this Society?... Many here had had their careers furthered by Fort Detrick, if not directly, then indirectly by a grant or some such thing."[359] The meeting recommended that the Committee be reinstated by a vote of 172-58. As *Science* magazine noted, the incident "is a significant reminder that many scientists have not changed their minds about military-oriented research."[360]

Now it was the fractured coalition of opponents to the Committee waging a behind-the-scenes influence campaign. Luria promised to poll general ASM membership by mail because (as an ally put it) the Governing Council "might not be a representative group."[361] With some trepidation from other ASM leaders, he eventually pushed through a suggestion by Sarles that the broader ASM Council membership be polled instead. This time, Luria and his allies avoided talk of ethics, instead focusing on what would professionally benefit the ASM. Luria apologetically explained to the Council that "my remarks represented my own personal point of view…" and that "I have carefully

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[357] Merrill J. Snyder, Statement at Annual Business Meeting of the ASM, May 7, 1968, in ASM 8-1A Folder 1. Emphasis in original.

[358] Richard Nixon would similarly reject protest by invoking a "silent majority" of pro-Vietnam middle Americans the next year, in a November 3, 1969 televised address.

[359] Remarks made by Greenberg, Minutes of ASM Business Meeting, May 8, 1968, p 2 in ASM 8-1A Folder 1. Greenberg openly acknowledged that he had previously worked at Detrick in the same remarks.

[360] Bryce Nelson, "Micro-Revolt of the Microbiologists Over Detrick Tie," *Science* 160 no 3830 (May 24, 1968), p 862.

[361] Remarks made by Young, Minutes of ASM Business Meeting, May 8, 1968, p 5 in ASM 8-1A Folder 1.

refrained from expressing my opinion on the matter to any members of the Advisory Committee."[362] Given what had happened in May, this backpedaling was an astute move: as UC Davis microbial ecologist Robert Hungate summarized it, "much of the opposition to the discontinuation of the Committee Advisory to Fort Detrick has arisen out of the conviction that it represented disapproval by the ASM of the activities of Fort Detrick. This is not the business of the society."[363] Housewright, meanwhile, resumed his own quiet influence campaign, passive-aggressively forwarding a statement by British researchers in favor of their biological weapons research at Porton Down to ASM officials.[364] Both sides, essentially, were attempting to seize control over the sacred community value of professional advancement: the supporters of the Committee by appealing to the long-standing justification of supporting the large Detrick constituency, their opponents by identifying the Committee as a political embarrassment for the ASM. However, despite this convergence of tactics, proposed outcomes remained divergent. While Hungate recommended a compromise resolution, re-affirming the ASM's commitment to government advising, but replacing the committee by providing a less-formal list of consultants to Detrick to avoid the issues attendant with maintaining a classified committee, Luria forestalled this by issuing a ballot offering two choices: to reaffirm the initial disbanding of the Committee, or to accept the recommendation of the Executive Council that it be reinstated.[365] This, and rhetoric that steered clear of ethical questions, allowed a return to the quiet abandonment of the Committee, and the votes

[362] "Explanatory Comments by President Luria," attached to Salvador Luria, Memo to Members of the ASM Council, June 21, 1968 in ASM 8-1A Folder 1.
[363] R. E. Hungate, Memo to Members of the CPC, Subject: Committee Advisory to Fort Detrick, May 24, 1968, in ASM 8-1A Folder 2.
[364] See Riley D. Housewright to Donald E. Shay, July 19, 1968, in ASM 8-1A Folder 1.
[365] Copies of this ballot can be found in ASM 8-1A Folder 1.

tallied that summer did so. By October, Minnesota's Dennis W. Watson, the upcoming president for 1969, officially delivered the news to Housewright.[366]

Even if many ASM members had convinced themselves that this was an apolitical move, it served to contribute to Detrick's deepening isolation as 1969 began. Outside of the ASM, Detrick was becoming the object of increasing popular scrutiny from journalists like Seymour Hersh and politicians like New York Representative Richard McCarthy. New revelations of secret military-science links, most notably the Army's secret sponsorship of a Smithsonian ornithology survey in the Pacific Ocean in preparation for open-air bioweapons testing there, further contributed to this scrutiny.[367] "CBW" was also increasingly decried in the student protests against the Vietnam War in particular and the military-industrial complex in general which grew that spring, culminating in protests on campuses ranging from MIT to Stanford on March 4. Defenders of such research remained adamant, with Merrill Snyder delivering an impassioned talk in favor of Detrick at Stanford on March 4, but they were thrown increasingly on the defensive by the simple fact that they were facing a public limelight they had sought for avoid for over twenty years.[368] ("The audience, if unfailingly polite, was plainly restive" for Snyder's talk, observed *Science*'s Elinor Langer.)[369] The ASM's

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[366] Dennis W. Watson to Riley Housewright, October 1, 1968, in ASM 8-1A Folder 1. It is not clear why several months ensued between the vote disbanding the Committee and the official notification of Detrick. It is certainly noteworthy that Watson, who had worked at Detrick on anthrax research during WWII, was a more palatable messenger than Luria.

[367] Roy MacLeod, "'Strictly for the Birds': Science, the Military and the Smithsonian's Pacific Ocean Biological Survey Program, 1963–1970," *Journal of the History of Biology* 34 no 2 (2001), pp 315-352.

[368] A copy of Snyder's March 4 speech, "The University and BW Defense Research" can be found in ASM 8-1A Folder 1. The fact that this speech found its way into the official ASM archives, maintained at the time by Snyder's nearby colleague Donald E. Shay, probably speaks to the clannishness of much of the ASM's leadership in this period.

[369] Elinor Langer, "A West Coast Version of the March 4 Protest… At Stanford- Convocation, Not Confrontation." *Science* 163 no 3872 (March 14, 1969), pp 1176-1177. Speakers at the more acrimonious MIT protests that day were far more critical of such 'establishment' views of Detrick, with Nobel Prize-

1969 annual meeting also reflected this growing controversy, as a group of protesters organized by MIT's David Baltimore and Alice Huang (along with Richard Novak of the Public Health Research Institute, who had likely been involved in the 1967 and 1968 protests) came prepared to protest not just the existence of a committee, but biological warfare research in general.[370] "We set up tables and had sessions during the annual meeting," Baltimore later recalled. "We felt that it was inappropriate for the ASM to be a respectable outlet for these people who were doing secret work anyway. You never knew what they were doing and they were, in a sense, perverting science." The ASM's leaders "didn't like it at all, because many of them were close to or even came from Fort Detrick… and saw this as a direct attack on members of the ASM, which it was."[371] Compounding their unhappiness was the protesters' last-minute insistence of including a talk by Harvard's Matthew Meselson, a prominent molecular biologist (and non-ASM member) who had fashioned himself into a leading critic of biological warfare over the past few years. Unhappy though the ASM leadership may have been, attention toward biological warfare was growing, with the protesters estimating that over 400 people

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winning Harvard biologist George Wald "lambast[ing]" the AIBS' sponsorship of the previous years' Detrick symposium as an example of scientists being corrupted by defense ties. See Bryce Nelson, "M.I.T.'s March 4: Scientists Discuss Renouncing Military Research," *Science* 163 no 3872 (March 14, 1969), pp 1175-1178.

[370] Novak had been the co-signatory of an anti-CBW letter in *Nature* with 1967 protester Issar Smith and several other Public Health Research Institute scientists, which lauded Luria's speech and condemned the AIBS Detrick symposium. These were almost certainly the group which had lead the anti-symposium campaign in 1968 and the anti-Advisory Committee campaign (when their home city was hosting the ASM) in 1967. See David Dubnau, Eunice Kahan, Leonard Mindich, Richard Novick, and Issar Smith, "Correspondence: Chemical and Biological Warfare," *Nature* 218 (June 22, 1968), p 1188. A clipping of this letter can be found in ASM 8-1A Folder 1, accompanied by a letter from Iowa's J. R. Porter (former Detrick employee and ASM president) declaring that it "is the best reason I know why the Council Policy Committee should avoid letting the Society get too deeply involved in moral and ethical problems." See J. R. Porter to Dennis W. Watson, July 1, 1968, in ASM 8-1A Folder 1.

[371] Baltimore, "David Baltimore (Oral History Transcript 0198)," p 56.

attended the Meselson talk, and that 60 had joined their movement.[372] Apparently sensing which way the cultural winds were blowing, the leadership, represented by the University of Texas' S. Edward Sulkin (who himself had deep ties with Detrick's Safety Division) were conciliatory.[373] This was a savvy move to avoid deeper divisions: as Huang wrote to Sulkin later that month, "it was evident that a radical caucus would be unnecessary" coming out of the meeting, which was apparently not assured until "aspiring young microbiologists as well as many older ones… gained new confidence in the Society" due to Sulkin's decisions.[374] Even Detrick had felt the pressure of changing times, seeking to keep its head down by withdrawing a planned exhibit at the ASM meeting (a marked contrast to its determined continuation of the AIBS symposium the year before).[375]

ASM and Detrick leaders were now responding to events beyond their control. Rumors had already circulated that Meselson was assembling a group of experts to write a UN report on biological warfare (with attendant loss of professional prestige as the prospect arose "that a microbiologist may end up representing say Ethiopia, but our country could be represented by a biochemist, cell biologist, or geneticist"), a fear which was borne out when the list was announced in April.[376] Meselson was then booked for an AAAS panel on CBW scheduled for December, where again "there [were] not any names

[372] See "Microbiologists' Committee on Chemical & Biological Warfare Newsletter #1," July 1969, in UMN Watson Papers, Box 3, Unnumbered Folder Entitled "Anthrax Biological Warfare Camp Detrick, 1944-1969."
[373] Sulkin's ties to the Safety Division are discussed in Chapter 3, below.
[374] Alice S. Huang to S. Edward Sulkin, May 27, 1969 in ASM 8-1A Folder 1. Then-president Dennis W. Watson of Minnesota (a Detrick alumnus), also praised Sulkin's tactics. See Dennis W. Watson to S. Edward Sulkin, May 31, 1969, in Ibid.
[375] Joseph V. Jemski to R. W. Sarber, April 11, 1968, in ASM 8-1A Folder 1.
[376] Quote from J. R. Porter to Dennis Watson, January 10, 1969, in ASM 8-1A Folder 1.

[recognizable] from the field of microbiology."[377] The ASM's traditional culture of silence about biological warfare was now damaging the very thing it had been intended to protect- the professional status of microbiologists- by ceding leadership on the issue to biologists they did not regard as legitimate members of their field. As this became increasingly clear during the last months of 1969, ASM leaders began discussing whether they should have their own symposium at the annual meeting in May 1970, or rely on a meeting planned at the upcoming 10th International Microbiology Congress by Swedish bioweapons opponent Carl-Göran Hedén.[378] Within a few years, they had gone from their culture of silence to playing catch-up.

Richard Nixon rendered this discussion moot. Following an internal administration review which concluded that protests like those breaking out at the ASM meetings made biological weapons research a political embarrassment for little military gain, on November 21, 1969 Nixon publicly announced that the US would abandon offensive bioweapons research and support a draft British proposal for a treaty banning such weapons completely.[379] This announcement abruptly shifted the worries of the ASM leadership. What was to happen to Detrick and its nearly 1500 employees, many of whom were ASM members? While early rumors had it that Detrick was to be transferred to the

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[377] William T. Kabisch to J. R. Porter, November 21, 1969 in ASM 8-1A Folder 1. A copy of the speaker list for the symposium is attached to this letter. Along with Meselson, this symposium included PSAC's Ivan Bennett, and Victor W. Sidel of Physicians for Social Responsibility.

[378] C.-G. Hedén, I. Málek, et al, to Pugwash Continuing Committee, August 25, 1966, p 3, in Cambridge University Churchill Archives Center GBR/0014/RTBT (Joseph Rotblat Papers) (RTBT), Series 5/4/12/14 Folder 3. See Chapter 5, below. This planned meeting at the Congress is also mentioned in "Microbiologists' Committee on Chemical & Biological Warfare Newsletter #1," July 1969, p 1 in UMN Watson Papers, Box 3, Unnumbered Folder Entitled "Anthrax Biological Warfare Camp Detrick, 1944-1969."

[379] Jonathan B. Tucker, "A Farewell to Germs: The U.S. Renunciation of Biological and Toxin Warfare, 1969-1970," *International Security* 27 no 1 (2002), pp 107-148; David I. Goldman, "The Generals and the Germs: The Army Leadership's Response to Nixon's Review of Chemical and Biological Warfare Policies in 1969," *Journal of Military History* 73 no 2 (2009), pp 531-569. Also see Chapter 5, below.

National Institute of Health, by the middle of 1970, it became increasingly clear that Detrick was a political and budgetary hot potato which NIH administrators (and their Department of Health, Education, and Welfare superiors) were not prepared to accept. Throughout this year of indecision, Detrick shed hundreds of employees, and Riley Housewright himself resigned from Detrick's scientific directorship to take a position in private industry that July.[380]

With this move, Housewright joined other ASM leaders outside of the government seeking to lobby for Detrick's survival. In the months following Nixon's announcement, the network of Detrick 'friends' shifted abruptly from influence-peddling in favor of biological warfare research to influence-peddling for Detrick to be repurposed. In February, the 1970 ASM Council accepted and passed one members' resolution lauding Nixon's decision with little fuss, and by March, the organization began officially lobbying defense officials for a "careful review [to] be made of the separation procedure applied at Fort Detrick" for extraneous staff, "a large number of whom are members of the American Society for Microbiology."[381] By the fall, as it became increasingly apparent that Detrick would continue to languish in administrative limbo, this lobbying increased. The members of the informal pro-Detrick network were joined by full-time employees at the ASM's recently opened Washington headquarters, an

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[380] Dorothy S. Winpigler to L. S. McClung, September 3, 1970, in ASM 8-1A Folder 3. Housewright's new position was as an executive at Microbiological Associates, Inc. of Bethesda, Maryland, a major federal contractor for biologicals like sera, tissue cultures, and diagnostic tests. Microbial Associates had been a major contractor for NASA's testing of lunar samples for pathogens (a project which had also enlisted Detrick expertise), and in the 1970s would secure major contracts with the National Cancer Institute, the institution which eventually took charge of Detrick. Hiring Housewright, it seems, was a wise move within the world of federal contracting. Staff numbers from J. R. Porter to Joshua Lederberg, October 7, 1970 in ASM 8-IA Folder 1.
[381] American Society for Microbiology to Defense Secretary Melvin R. Laird, March 11, 1970, in ASM 8-1A Folder 6.

example of a trend in scientific societies decried by members of the March 4th movement.[382] Officials like the White House's Vincent McRae thus found themselves in a series of meetings that fall with official ASM representatives like Executive Director Asger F. Langlykke as well as unofficial heralds like J. Roger Porter and Robert Hungate, who meanwhile bombarded congressional offices with letters.[383] A number of the Detrick 'friends' seem to have seized a role as politically relevant stakeholders in the process, as Ivan Bennett of the Presidential Scientific Advisory Committee (PSAC) dutifully enlisted their views in a mass mailing, and some of them, like Iowa's J. Roger Porter, found their way onto a panel on Detrick's fate jointly sponsored by PSAC and the National Academy of Sciences.[384] To his consternation, Housewright was notably excluded from Bennett's mailings, but swiftly caught wind of them nonetheless along the 'friends' grapevine that had informally operated for over a quarter-century.[385] Ultimately, their lobbying was successful, but pyrrhic: Detrick was largely repurposed for the National Cancer Institute's search for a cancer-causing virus, but only in 1972, after a large number of its staff had been slashed.[386]

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[382] Harvard biologist George Wald was particular critical of organizations like AIBS setting up Washington offices in his March 4, 1969 public remarks. See Nelson, "M.I.T.'s March 4," p 1177.

[383] J. Roger Porter, "Meeting with Dr. Vincent McRae, OST, October 2, 1970," in ASM 8-IA Folder 1 ("Miscellaneous Committees- Advisory to Ft. Detrick- Correspondence of D. E. Shay."); J. R. Porter to Senator Jack R. Miller, October 14, 1970, in Ibid; "Conversations Concerning Fort Detrick," December 14, 1970 in ASM 8-IA Folder 5.

[384] J. R. Porter to Ivan L. Bennett, August 28, 1970; J. R. Porter to Senator Jack R. Miller, October 14, 1970, both in ASM 8-IA Folder 1.

[385] As Housewright commented upon seeing a copy of Bennett's correspondence with the network of 'friends,' "they have not taken the trouble to ask the former Scientific Director these same questions." Riley D. Housewright to Donald E. Shay, September 4, 1970; Donald E. Shay to Riley D. Housewright, September 2, 1970 both in ASM 8-IA Folder 1.

[386] See Robin Wolfe Scheffler, *A Contagious Cause: The American Hunt for Cancer Viruses and the Rise of Molecular Medicine*, Chicago: University of Chicago Press, 2019.

Conclusion

This lobbying effort in favor of Detrick overtly reflected the most basic of ASM motivations: the professional interests of its members. Earlier defenses of biological warfare were not reflected in this rhetoric, which often started with a perfunctory statement of support for Nixon's decision. Otherwise, the ASM culture of silence about biological warfare returned and the quarter-century episode of ASM-Detrick links seems to have quickly become something that the community's historical memory would rather not acknowledge. This certainly does not mean that the Detrick 'friends' were willing converts to the brave new world in which even government officials openly constructed biological warfare as 'dirty,' instead recalling their defeat with bitterness. Elberg later unfavorably compared the ASM's reaction to protest in 1968-1969 to that of "the chemists, who… simply rode it through, didn't see anything particularly harmful to their chemical profession to have such a committee of them advising the chemical [warfare] crowd." "An interesting contrast in professional and ethical maturity," he noted sarcastically.[387] Riley Housewright was similarly bitter about what he saw as the violated boundaries of professional expertise. Writing about Meselson in the early 1980s, he opined that "Matt Meselson has made a career posing as an expert on BW. He is a certified expert in ½ truths & outright lies re: BW… He is not a microbiologist."[388] The

[387] Elberg, "Graduate Education and Microbiology at the University of California, Berkeley," p 97.
[388] Riley D. Housewright, Untitled ms (Memo on 1950 San Francisco *Serratia marcescens* tests), n.d. (ca 1981, attached to a note dated March 16, 1981), pp 1-2, in ASM Series 13-IIBP ("Presidential Papers: Riley D. Housewright"), Folder 26 "BW Materials- *Serratia marcescens*." Housewright was referring to a recent appearance by Meselson on the television program "60 Minutes" regarding a 1981 lawsuit against the US Army by San Francisco lawyer Ed Nevin III. Nevin's grandfather had died of a bladder infection by *Serratia marcescens* (a normally non-pathogenic organism that is nonetheless now known to opportunistically infect immunocompromised patients) in San Francisco in 1950. This death took place shortly after a secret military test of bioweapons dispersion patterns, in which a cloud of *Serratia marcescens* was sprayed from a Navy ship in San Francisco Bay. Following the public revelation of this test in 1977, Nevin sued the Army, alleging that his grandfather's infection stemmed from the airborne

simple fact of the matter, however, was that their bitterness simmered in a community that would rather forget that there had ever been a controversy, much less close ties to bioweapons research. In a notable contrast to the 1960s, only three ASM presidents after 1970 had ties to Detrick at some point during their career, one of whom (Walter R. Dowdle, president in 1990) only had the tenuous link of a research fellowship in graduate school. There was a three-decade hiatus before the ASM again officially engaged with the question of biological warfare, and when it did so in the 1990s, it was with as a security threat to be guarded against, consistent with the growing concern with 'biosecurity' rising in US government circles.[389]

These decades of communal forgetting obscured the legacy of the decades that had preceded them. The boundary between the open world of microbiology and military-sponsored biological weapons research was a permeable one during the early Cold War, with knowledge, people, and political support passing between the two worlds. This permeability was actively constructed and maintained by SAB/ASM leaders, making the blow of rising ASM opposition to biowarfare research all the more severe for their allies

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Serratia marcescens cloud. For more on this episode, see Leonard A. Cole, *Clouds of Secrecy: The Army's Germ Warfare Tests Over Populated Areas*, Lanham, MD: Rowman & Littlefield, 1988. The point that Housewright did not consider Meselson, who had made his scientific reputation for work done with *E. coli*, an authoritative microbiologist is noteworthy. This attitude highlights the professional tensions between scientists like Housewright, with well-developed expertise in manipulating often intransigent microorganisms, and molecular biologists like Meselson, who used well-understood model microbes like *E. coli* as a means to a biochemical end. Dennis Watson expressed this tension somewhat more gently in comments on Joshua Lederberg's fears about biological warfare. "Lederberg is a great geneticist," he wrote privately, "but he has no concept of pathogenesis." Dennis Watson, ms note on copy of Joshua Lederberg, "Biological Warfare- A Global Threat," ca. 1971, in UMN Watson Papers, Box 3, Unnumbered Folder Entitled "Anthrax Biological Warfare Camp Detrick, 1944-1969."

[389] See Kenneth I. Berns, Ronald M. Atlas, Gail Cassell & Janet Shoemaker, "Preventing The Misuse of Microorganisms: The Role of The American Society For Microbiology in Protecting Against Biological Weapons," *Critical Reviews in Microbiology* 24 no 3 (1998), pp 273-280. On the rise of "bioterrorism' as an object of state concern in the 1990s, see Susan Wright, "Terrorists and Biological Weapons: Forging the Linkage in the Clinton Administration," *Politics and the Life Sciences* 25 no 1/2 (2006), pp 57-115.

at Detrick. Nonetheless, even as American microbiologists of the 1970s and 1980s lived a life far less connected to the military than those of the 1950s and 1960s had, they continued to live with the legacies of the SAB/Detrick entanglement, from early career experiences to Detrick-derived ways of doing their science. The most notable instance of this latter legacy was an interlocking system of safety expertise, practice, and technologies which by the 1970s was finding its way into civilian microbiology laboratories. Stemming from a system developed by Detrick's Safety Division intended to 'contain' microbes and avoid laboratory infections, this system of containment-based 'biosafety' spread into 'open' microbiology through the links maintained in the 1960s. The next chapter will trace the development of this system, from a wartime attempt to contain the secret of Detrick's very existence to this spread into civilian microbiological practice.

Chapter 3: The Detrick Safety Division and the Containment of Microbes

Growing anthrax in bulk is a tricky business. Far hardier in its essential inert spore form than in its vegetative growth phase, *B. anthracis* must be treated with care to be grown above the petri dish scale most microbiologists were used to in the 1940s. Anthrax grown at these scales, ideally by the ton, had emerged by 1944 as an important production goal for the American biological weapons program, which sought to fill a British order for tens of thousands of anthrax-filled cluster bombs to give the Allies any means, however crude, to retaliate in kind should Germany attack with their own biological weapons. Despite growing American military doubts about the utility of *B. anthracis* as a weapon of war, alliance diplomacy and the plausibility of German microbiologists being ahead of their Anglophone counterparts were compelling forces behind a crash program to develop an anthrax-growing pilot plant at Camp Detrick, and a full production plant at Terre Haute, Indiana.[390] In principle, as Detrick's scientific director, Ira Baldwin believed, growing microbes by the ton in commercially available fermentation tanks should be as simple as the skill of growing these microbes on a petri dish, a skill all microbiologists developed during their training.[391] In practice, however, this increase in scale required that researchers learn how to modify their procedures and equipment to ensure that their anthrax grew without problems. The pesky bacteria needed to be kept at the right temperature, be fed a growth medium with the proper chemistry

[390] Ed Regis, *The Biology of Doom: The History of America's Secret Germ Warfare Project*, New York: Henry Holt Co., 1999, pp 70-74.
[391] Ira L. Baldwin, *My Half-Century at the University of Wisconsin: Adapted from an Oral History Interview by Donna Taylor Hartshorne* (Madison, WI: Privately Printed by Ira L. Baldwin, 1995), p 122.

and composition, and be prevented from mutating away from desired virulence or being supplanted by other bacteria equally eager to feed and multiply upon the nutritional bounty offered to them by the humans.

One of the simplest challenges introduced by this increase in scale was also one of the trickiest. An obligate pathogen with a complex life cycle, *B. anthracis* needs oxygen to enter its sporulation phase.[392] On a petri dish this is not a concern, but a *B. anthracis* culture in a multi-ton fermentation tank must be aerated, with a flow of air mixed into the slurry and vented from the tank. While this technique was used by industry to grow yeasts, blowing air through a tank of a deadly pathogen was another matter, and Detrick researchers built their equipment to channel this exhaust air into an incinerator, where it would hopefully be sterilized of any pathogens. Nonetheless, on September 22, 1944, three researchers made a horrifying discovery at one of the pilot plants: the plant's slurry of growth medium and anthrax had overflowed the tank, welled up through the incinerator's exhaust air vent, and was pouring on the ground. Nearby was an open storm drain, which flowed into a creek and the homes and dairy farms of Frederick, Maryland beyond. Acting quickly, the three researchers built a dirt dam around the pooling slurry, keeping it out of the storm drain. A potential catastrophe had been averted, and the three researchers received official (but secret) commendations a month later.[393] They had, however, also been exposed to countless numbers of anthrax spores, and doubtless spend the next weeks of prophylactic antibiotic treatment more worried about whether they

[392] See Susan D. Jones, *Death in a Small Package: A Short History of Anthrax* (Baltimore: Johns Hopkins University Press, 2013).
[393] This incident is discussed in Rexmond C. Cochrane, *History of the Chemical Warfare Service in World War II, Volume 2: Biological Warfare Research in the United States*, Edgewood Arsenal: Historical Section, Office of the Chief, Chemical Corps, 1947, p 126.

would contract anthrax than whether they would receive recognition.[394] This worry would not have been unfounded, as this fate befell 25 of their colleagues at Camp Detrick during the Second World War.[395]

This incident highlights an essential fact about biological weapons agents that one can forget when examining rationalized military plans for their production and use: that they were dangerous and hard to control. As tiny entities, microorganisms could exploit the smallest holes in equipment to get into spaces where they were not wanted. As living organisms, they could multiply and grow unpredictably in whatever space they occupied. As pathogens, they could colonize the bodies of humans and other animals, posing a danger to the humans who tried to tame them as weapons that only increased the more useful in this role they were. In the same vein, as Detrick researchers came to discover, they could encounter human bodies not only though touch, injection, or being ingested, but also through the air. This property of aerosols of pathogens was a fundamental basis for selecting these pathogens to become “biological weapons” in the first place, but aerosols did not need to be deliberately generated by humans to permeate laboratory spaces. Working in close proximity to these pathogens was dangerous, as the 25 victims of anthrax infection and another 35 people infected by other pathogens during the Second World War could attest.[396] During the quarter century of offensive biological weapons

[394] Prophylactic antibiotic treatment for anthrax exposure was introduced at Camp Detrick shortly before this incident, and would almost certainly have been used in this case. See Cochrane, *Biological Warfare Research*, p 252.
[395] All 25 of these cases were cutaneous anthrax, and none were fatal. The deadlier pulmonary form of the disease did kill 2 Detrick researchers in 1951 and 1958, however. See Directorate of Industrial Health and Safety, *Occupational Laboratory Infections at Fort Detrick, 1943-1970: Statistical Summaries and Analyses, Published Case Reports*. Fort Detrick, MD, n.d., held in American Society for Microbiology Archives (ASM).
[396] See Cochrane, *Biological Warfare Research*, p 172.

research at Detrick, 456 laboratory infections were identified among those working there, 3 of which were fatal.[397]

Mitigating (and ideally, controlling) this danger was the responsibility of Detrick's Safety Division, an original part of the 1943 organization that continued throughout the offensive program into the 1960s.[398] The Division began as an organization largely devoted to monitoring for infection, using a few safety precautions drawn from the civilian world of microbiology laboratory practice, and principally concerned with containing microbes within the grounds of Camp Detrick to preserve the secrecy of what was going on there. Over the next three decades, however, the Safety Division's view of their mission and the tools to accomplish it developed radically. The realization that the laboratory air that researchers breathed was a potential vector for infection was a Safety Division development, as were a gamut of containment technologies intended to mitigate this danger. An increasingly elaborate system of forms, accident investigation, and regulation of laboratory practice insinuated the Division into the management of research at Detrick. All of these developments produced results, as laboratory infection rates at Detrick dropped, particularly in the 1960s. However, this is only a story of triumph if one chooses to interpret it as such (as the Safety Division' leaders by and large did). Laboratory infections dropped as technologies and procedures

[397] US Department of the Army, *U.S. Army Activity in the US Biological Warfare Programs, Volume 2*, Washington, DC: US Department of the Army, 1977, p G-3.
[398] Safety at Fort Detrick has been discussed by historians Gerard Fitzgerald and Melanie Armstrong. See Chapter 4 of Gerard James Fitzgerald, "From Prevention to Infection: Intramural Aerobiology, Biomedical Technology, and the Origins of Biological Warfare Research in the United States, 1910-1955," PhD diss, Carnegie Mellon University, 2003, pp 155-206; Chapter 2 of Melanie Armstrong, *Germ Wars: The Politics of Microbes and America's Landscape of Fear*, Oakland: University of California Press, 2017, pp 68-96. Armstrong's study focuses on the cultural valiance of germ warfare "containment," while Fitzgerald focuses on the Second World War and the development of one particular apparatus (the so-called Reyniers chamber; see below).

for containing microbes in circumscribed spaces proliferated, but these infections were never eliminated, as microbes proved recalcitrant to such control. Humans, too, were recalcitrant in the face of professional safety officers, removed from the ordinary course of research, inserting themselves into the most minute of details regarding researchers' work practices and their very bodies. 'Safety' was a contingent and constructed product of the Division's activities.

Nonetheless, the managerial and technological system constructed at Detrick held significance for more than the American military's flirtation with biological warfare. Pathogenic microbes do not care whether the humans bringing them into their spaces intend to use them as weapons against their enemies, subjects for their research, or the basis for a medical diagnosis. In any of these cases, such close proximity with potential hosts is an opportunity, one which pathogens have been exploiting since the earliest days of such human spaces existing. Laboratory infection, as a noticed and talked-about phenomenon, dates back to the earliest days of modern microbiology. By the time Detrick was founded in the 1940s, it had become an increasing subject of concern among American microbiologists, as researchers infected with exotic new diseases like Q fever and virulent old diseases like brucellosis came under increasing scrutiny.[399] Nothing the Detrick Safety Division did was entirely unprecedented: organized safety practices and regulations existed, technologies to contain microbes were developed and deployed, and laboratory infection statistics were gathered and published in contemporary civilian

[399] See e.g. F. M. Burnet and M. Freeman, "Note on a Series of Laboratory Infections with the Rickettsia of 'Q' Fever," *Medical Journal of Australia* 1 (1939), p 11; I. Forest Huddleson and Myrtle Munger, "A Study of an Epidemic of Brucellosis Due to *Brucella melitensis*," *American Journal of Public Health* 30 no 8 (1940), pp 944–954; K. F. Meyer and B. Eddie, "Laboratory Infections Due to *Brucella*," *The Journal of Infectious Diseases* 68 no 1 (1941), pp 24-32.

institutions. Nonetheless, what was happening at Detrick was different, with a professionalized safety organization, far more exhaustively developed and maintained containment technologies, and a nearly panopticonic view of the laboratory about which other laboratory infection researchers could only dream. As concern about laboratory infection in general and new research entities like cancer viruses and genetically modified bacteria grew between the 1950s and '70s, the Safety Division, and its leader, Arnold Wedum, emerged as the center of a network of safety experts and technologies proliferating throughout American microbiology. "Biosafety," as a professional field, emerged from Safety Division expertise and conferences, and is one of the most enduring legacies of the offensive biological weapons program in microbiology today. The story of how the Safety Division, originally founded to contain secrets, developed a network of technologies and knowledge that ultimately spread from the classified world of Detrick into the open one of civilian microbiology highlights the contradictions and paradoxes of the secrecy regime under which Detrick operated, and the deeply entangled relationship between civilian microbiologists and the 'closed world' of Detrick.

Safety, Inside and Outside the Laboratory

"There is surely no more useful skill in the practice of scientific research," sociologist of science Benjamin Sims has noted, "than the knack for not accidentally killing oneself with the laboratory equipment."[400] Safety concerns cut to the heart of scientific research as embodied, material labor, rather than as a disembodied intellectual exercise, a theme which broadly has been explored by literature emerging from the

[400] Benjamin Sims, "Safe Science: Material and Social Order in Laboratory Work," *Social Studies of Science* 35 no 3 (2005), pp 333-366.

'practice turn' in the historiography of science.[401] Little of this literature is focused on laboratory safety, however, as classics in this literature tend to focus instead on the material roots of the historiography of science's most classic concern, knowledge-making.[402] Somewhat more attention is paid to safety by work in the STS tradition of laboratory ethnography, of which Sims' work is a part, reflecting this tradition's focus on small details of laboratory life less likely to be explicitly noticed by actors and written documents.[403] Insofar as a history of safety is a history of absence, of "not killing

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[401] Some recent historical and ethnographic discussions of scientific embodiment include Cyrus C. M. Mody, "The Sounds of Science: Listening to Laboratory Practice," *Science, Technology, & Human Values* 30 no 2 (2005), pp 175-198; Steven Shapin, *Never Pure: Historical Studies of Science as If It Was Produced by People with Bodies, Situated in Time, Space, Culture, and Society, and Struggling for Credibility and Authority*, Baltimore: Johns Hopkins University Press, 2010; and Chapter 6 of Janet Vertesi, *Seeing Like a Rover: How Robots, Teams, and Images Craft Knowledge of Mars*, Chicago: University of Chicago Press, 2015.

[402] For a review discussion of this shift in the historiography of the life sciences, see Hannah Landecker, "The Matter of Practice in the Historiography of the Experimental Life Sciences," in Michael R. Dietrich, Mark E. Borrello, and Oren Harman (eds), *Handbook of the Historiography of Biology*, Dordrecht, Netherlands: Springer, 2021, pp 243-264. Major works closely interrogating the relationship between laboratory practice and knowledge-making include Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life*, Princeton: Princeton University Press, 1985; Robert Kohler, *Lords of the Fly:* Drosophila *Genetics and the Experimental Life*, Chicago: University of Chicago Press, 1994; Angela N. H. Creager, *The Life of a Virus: Tobacco Mosaic Virus as an Experimental Model, 1930-1965*, Chicago: University of Chicago Press, 2002. Also see Robert Kohler, "Lab History: Reflections," *Isis* 99 no 4 (2008), pp 761-768 for a pessimistic discussion of the state of the laboratory as a subject of inquiry. For a response to this Kohler paper which devotes some attention to the role of safety concerns in laboratory design, see Peter J. T. Morris, *The Matter Factory: A History of the Chemistry Laboratory*, London: Reaktion Books, 2015. Peter Galison, *Image and Logic: A Material Culture of Microphysics*, Chicago: University of Chicago Press, 1997 is a major study of laboratory practice that includes brief discussion of formal laboratory safety regulations. Also see Amy E. Slayton, "Safety Equipment," in Joseph D. Martin and Cyrus C. M. Mody (eds), *Between Making and Knowing: Tools in the History of Materials Research*, Singapore: World Scientific Publishing, 2020, pp 129-140.

[403] Besides Sims, "Safe Science," see Hille C. Bruns, "Leveraging Functionality in Safety Routines: Examining the Divergence of Rules and Performance," *Human Relations* 62 no 9 (2009), pp 1399-1426; Nicolas Rossignol and Michiel van Oudheusden, "Learning from Incidents and Incident Reporting: Safety Governance at a Belgian Nuclear Research Center," *Science, Technology, & Human Values* 42 no 4 (2017), pp 679-702. See also literature on the closely allied concept of "contamination" in the laboratory in the ethnographic work of Cyrus C. M. Mody, "A Little Dirt Never Hurt Anyone: Knowledge-Making and Contamination in Materials Science," *Social Studies of Science* 31 no 1 (2001), pp 7-36 and the historical work of Christophe Lécuyer, "From Clean Rooms to Dirty Water: Labor, Semiconductor Firms, and the Struggle over Pollution and Workplace Hazards in Silicon Valley," *Information & Culture* 52 no 3 (2017), pp 304-333. A foundational study on the concept of 'contamination' and its role in constituting social order is Mary Douglas, *Purity and Danger: An Analysis of Concepts of Pollution and Taboo*, London: Routledge, 2002 (1966).

oneself" by avoiding accidents or other discrete events of 'un-safety,' it is perhaps unsurprising that ethnographers, with their non-documentary sources, have paid the most attention to laboratory safety.

The broader historiography of safety is more than a story of absence, however. "Safety" is a concept closely bound up with that of "risk:" an environment perceived to be *unsafe* is one in which *risk* of calamity is perceived to be present.[404] There is a rich literature on risk, much of which emphasizes the socially constructed nature of the concept: certain hazards are identified as unacceptable breaches of the normal order, while others are tacitly ignored.[405] Whose risk perceptions matter is also a fraught

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[404] Disaster is the third concept in this interrelationship, examined by a heterogeneous scholarly tradition of disaster studies. Studies of disaster derived from humanistic disciplines, in particular, tend to identify the social in events conventionally identified as "natural," tracing social allocations of risk underlying who comes to experience disaster, and the socially-mediated aftermath of disaster. See e.g. Roberto E. Barrios, "What Does Catastrophe Reveal for Whom? The Anthropology of Crises and Disasters at the Onset of the Anthropocene," *Annual Review of Anthropology* 46 (2017), pp 151-166. Historians have generally adopted a similar orientation, albeit with a diachronic perspective, in both the study of particular disasters and of "disaster" and "accident" as general categories. See Roger Cooter and Bill Luckin (eds), *Accidents in History: Injuries, Fatalities, and Social Relations*, Athens, GA: Rodopi, 1997; Peter Galison, "An Accident of History," in Peter Galison and Alex Roland (eds), *Atmospheric Flights in the Twentieth Century*, Dordrecht, Netherlands: Kluwer Academic Publishers, 2000, pp 3-44; Kevin Rozario, *The Culture of Calamity: Disaster and the Making of Modern America*, Chicago: University of Chicago Press, 2007; Scott Gabriel Knowles, *The Disaster Experts: Mastering Risk in Modern America*, Philadelphia: University of Pennsylvania Press, 2011. See also Sara E. Wermiel, *The Fireproof Building: Technology and Public Safety in the Nineteenth-Century American City*, Baltimore: Johns Hopkins University Press, 2000; James B. McSwain, *Petroleum and Public Safety: Risk Management in the Gulf South, 1901-2015*, Baton Rouge: Louisiana University Press, 2018; Jacob A.C. Remes and Andy Horowitz (eds), *Critical Disaster Studies*, Philadelphia: University of Pennsylvania Press, 2021. The study of accidents is also influenced by sociologist Charles Perrow's concept of "normal accidents" in large sociotechnological systems like nuclear power plants. Perrow argues that in such "tightly coupled" systems, inevitable human, technological, or hybrid errors will in turn be necessarily magnified, causing more errors in turn in a chain reaction of calamity. Accident, Perrow argues, is an inevitable epiphenomenal feature of such complex systems. Charles Perrow, *Normal Accidents: Living with High-Risk Technologies, Updated Edition*, Princeton: Princeton University Press, 1999 (1984).

[405] This is the central thesis of Mary Douglas and Aaron Wildavsky, *Risk and Culture. An Essay on the Selection of Technological and Environmental Dangers*, Berkeley: University of California Press, 1982. Ulrich Beck, *Risk Society: Towards a New Modernity*, trans. Mark Ritter, London: Sage Publications, 1992 (1986) is a leading work on the theory of risk, arguing that contemporary society is inextricably committed to the management of the risks inherent in modernity, in contrast to the danger of external calamities experienced by pre-industrial society. Historians have responded to and problematized this implicitly historical claim. See a special issue of *History and Technology* entitled "Risk and 'Risk Society' in Historical Perspective," led by Soraya Boudia and Nathalie Jas, "Introduction: Risk and 'Risk Society' in

question, as the views of someone directly exposed to a hazard can differ from those of a manager, an expert, or a lawyer. "Safety," then, is as much a socially constructed view of how the world should be as it is a view of actual physical conditions. Changing views of which conditions are unacceptable breaches of this order reflect the co-production of knowledge about these conditions and views of them.

In the specific context of the mid-20th century United States in which Detrick was founded, the discipline of industrial safety research was a major force in constructing "safe" workplaces. Industrialization in the United States brought with it novel workplace conditions and consumer products, which introduced new dangers into the lives of many 19th and 20th century Americans.[406] By the early 20th century a growing profession of

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Historical Perspective," *History and Technology* 23 no 4 (2007), pp 317-331; Arwen P. Mohun, *Risk: Negotiating Safety in American Society*, Baltimore: Johns Hopkins University Press, 2013; and a special issue of *Historical Social Research* entitled "Risk as an Analytical Category: Selected Studies in the Social History of the Twentieth Century," led by Peter Itzen and Simone M. Müller, "Risk as a Category of Analysis for a Social History of the Twentieth Century: An Introduction," *Historical Social Research* 41 no 1 (2016), pp 7-29. The historiographies of finance and capitalism offer a parallel, genealogy-tracing approach to the concept of 'risk,' which originated as a term for an instrument of maritime insurance in the 17th century. See e.g. Jonathan Levy, *Freaks of Fortune: The Emerging World of Capitalism and Risk in America*, Cambridge, MA: Harvard University Press, 2012.

[406] See e.g. David Rosner and Gerald Markowitz (eds), *Dying for Work: Workers' Safety and Health in Twentieth-Century America*, Bloomington: Indiana University Press, 1989; David Rosner and Gerald Markowitz, *Deadly Dust: Silicosis and the Politics of Occupational Disease in Twentieth-Century America*, Princeton: Princeton University Press, 1991; Christopher Sellers, "Factory as Environment: Industrial Hygiene, Professional Collaboration and the Modern Sciences of Pollution," *Environmental History Review* 18 no 1 (1994), pp 55-83; Claudia Clark, *Radium Girls, Women and Industrial Health Reform*, Chapel Hill: University of North Carolina Press, 1997; John Fabian Witt, *The Accidental Republic: Crippled Workmen, Destitute Widows, and the Remaking of American Law*, Cambridge, MA: Harvard University Press, 2004; Richard Greenwald, *The Triangle Fire, Protocols of Peace and Industrial Democracy in Progressive Era New York*, Philadelphia: Temple University Press, 2005; Rachel Maines, *Asbestos and Fire: Technological Trade-Offs and the Body at Risk*, New Brunswick: Rutgers University Press, 2005; Christopher Sellers and Joseph Melling, "Towards a Transnational Industrial-Hazard History: Charting the Circulation of Workplace Dangers, Debates and Expertise," *British Journal for the History of Science* 45 no 3 (2012), pp 401-424; Christopher Sellers and Joseph Melling (eds), *Dangerous Trade: Histories of Industrial Hazard across a Globalizing World*, Philadelphia: Temple University Press, 2012. I use the term "industrialization" broadly here, encompassing the growing extraction of raw materials through activities like agriculture, logging, and mining to feed factories as much as the factories themselves. In this, I follow global historians like Sven Beckert in their focus on the unity of global networks of production from raw materials to commodities to finished goods. See e.g. Sven Beckert, *Empire of Cotton: A Global History*, New York: Knopf, 2014. Industrialized landscapes like fields and mines were sites of new dangers as much as factories, and safety in those places of work is a notable focus of part of the historiography of workplace

safety experts housed in their own departments in large firms sought to alleviate these dangers, through a combination of regulating the physical environment of the workplace and the practices of workers.[407] Though the interest of many of these experts in reducing the rate of workplace accidents was genuine, economic historian Mark Aldrich argues their core *raison d'être* for large firms was to forestall the omnipresent threat of legal regulation and sanction in a Progressive Era and later New Deal political climate.[408] These efforts were, for the most part, successful, as the workplace safety policy of states and an increasingly active federal government remained largely voluntarist, with state and federal departments of labor sponsoring studies as their primary weapon of inducement.[409] This voluntarist regime, tempered in the mid-century by the increased

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safety. See e.g. William Graebner, *Coal Mining Safety during the Progressive Era: The Political Economy of Reform*, Lexington: University of Kentucky Press, 1976; Mark Aldrich, "Preventing the Needless Peril of the Coal Mine: The Bureau of Mines and the Campaign against Coal Dust Explosions, 1910-1940," *Technology and Culture* 36 no 3 (1995), pp 483-518; Derek Oden, "Selling Safety: The Farm Safety Movement's Emergence and Evolution from 1940-1975," *Agricultural History* 79 no 4 (2005), pp 412-438; Mark Aldrich, "Engineers Attack the 'No. One Killer' in Coal Mining: The Bureau of Mines and the Promotion of Roof Bolting, 1947-1969," *Technology and Culture* 57 no 1 (2016), pp 80-118.

[407] Dianne Bennett and William Graebner, "Safety First: Slogan and Symbol of the Industrial Safety Movement," *Journal of the Illinois State Historical Society* 68 no 3 (1975), pp 243-256; Christopher C. Sellers, *Hazards of the Job: From Industrial Disease to Environmental Health Science*, Durham, NC: University of North Carolina Press, 1999; Mark Aldrich, *Death Rode the Rails: American Railroad Accidents and Safety 1828-1965*, Baltimore: Johns Hopkins University Press, 2006; Christopher Sellers, "A Prejudice Which May Cloud the Mentality: The Making of Objectivity in Early Twentieth-Century Occupational Health," in John W. Ward and Christian Warren (eds), *Silent Victories: The History and Practice of Public Heath in Twentieth-Century America*, New York: Oxford University Press, 2007, pp 230-252; John Burnham, *Accident Prone: A History of Technology, Psychology, and Misfits of the Machine Age*, Chicago: University of Chicago Press, 2010; Ross Wilson, "The Museum of Safety: Responsibility, Awareness and Modernity in New York, 1908–1923," *Journal of American Studies* 51 no 3 (2016), pp 915-938. A major institution of this movement was the National Safety Council. On the origin and early years of the Council, see Thomas Kartchner Hafen, "Safe Workers: The National Safety Council and the American Safety Movement, 1900 1930," PhD diss., University of Chicago, 2005. The Progressive-era rise of industrial safety experts can be seen as a reflection of the contemporaneous ideology of "scientific management" in American industry. See e.g. Donald R. Stabile, "The Du Pont Experiments in Scientific Management: Efficiency and Safety, 1911-1919," *The Business History Review*, 61 no 3 (1987), pp 365-386.

[408] Mark Aldrich, *Safety First: Technology, Labor, and Business in the Building of American Work Safety, 1870-1939*, Baltimore: Johns Hopkins University Press, 1997. For a less business-focused perspective, see Donald W. Rogers, *Making Capitalism Safe: Workplace Safety and Health Regulation in America, 1880-1940*, Champaign: University of Illinois Press, 2009.

[409] David Rosner and Gerald Markowitz, "Research or Advocacy: Federal Occupational Safety and Health Policies during the New Deal," *Journal of Social History* 18 no 3 (1985), pp 365-381; Christopher Sellers,

power of organized labor, was replaced whole-cloth at the beginning of the 1970s with a federal regulatory framework through the Occupational Health and Safety Act.[410] This replacement of voluntarism with legally binding regulation in the 1960s and ‘70s also intersected with preexisting parallel research traditions on the safety of consumer goods and the potentially toxic waste products of industry.[411] Detrick research, taking place under military auspices, was in no danger of having safety legislation imposed on it, but existence of professionalized safety expertise, with managerial techniques like tracking accidents per worker-hour provided a pre-made model for Detrick safety professionals to draw upon.[412]

"The Public Health Service's Office of Industrial Hygiene and the Transformation of Industrial Medicine," *Bulletin of the History of Medicine* 65 no 1 (1991), pp 42-73.

410 Josiah Rector, "Environmental Justice at Work: The UAW, the War on Cancer, and the Right to Equal Protection from Toxic Hazards in Postwar America," *The Journal of American History* 101 no 2 (2014), pp 480-502; David Rosner and Gerald Markowitz, "A Short History of Occupational Safety and Health in the United States," *American Journal of Public Health* 110 no 5 (2020), pp 622-628.

411 Joel A. Tarr and Mark Tebeau, "Managing Danger in the Home Environment, 1900-1940," *Journal of Social History* 29 no 4 (1996), pp 797-816; John C. Burnham, "Why Did the Infants and Toddlers Die? Shifts in Americans' Ideas of Responsibility for Accidents: From Blaming Mom to Engineering," *Journal of Social History* 29 no 4 (1996), pp 817-837; Gerald Markowitz, and David Rosner, *Deceit and Denial: The Deadly Politics of Industrial Pollution*, Berkeley: University of California Press, 2003; Linda Nash, "The Fruits of Ill-Health: Pesticides and Workers' Bodies in Post-World War II California," *Osiris* 19 (2004), pp 203-219; Johnathan Rees, ""I Did Not Know… Any Danger Was Attached": Safety Consciousness in the Early American Ice and Refrigeration Industries," *Technology and Culture* 46 no 3 (2005) pp 541-560; C. Sellers, "Cross-Nationalizing the History of Industrial Hazard," in Virginia Berridge and Martin Gorsky (eds), *Environment, Health and History*, New York: Palgrave Macmillan, 2012, pp 178-205; Barbara Young Welke, "The Cowboy Suit Tragedy: Spreading Risk, Owning Hazard in the Modern American Consumer Economy," *The Journal of American History* 101 no 1 (2014), pp 97-121; Frederick Rowe Davis, *Banned: A History of Pesticides and the Science of Toxicology*, New Haven: Yale University Press, 2014. On automobile safety (a particular prominent issue in the OSHA era in the wake of Ralph Nader's 1965 *Unsafe at Any Speed*), see Joel W. Eastman, *Styling vs. Safety: The American Automobile Industry and the Development of Automotive Safety, 1900-1966*, Lanham, MD: University Press of America, 1984; David Blanke, *Hell on Wheels: The Promise and Peril of America's Car Culture, 1900-1940*, Lawrence: University Press of Kansas, 2007; Amy Gangloff, "Safety in Accidents: Hugh DeHaven and the Development of Crash Injury Studies," *Technology and Culture* 54 no 1 (2013), pp 40-61; a 2015 special issue of *Technology and Culture* entitled "(Auto)Mobility, Accidents, and Danger," especially Peter Norton, "Four Paradigms: Traffic Safety in the Twentieth-Century United States," *Technology and Culture* 56 no 2 (2015), pp 319-334; Lee Vinsel, *Moving Violations: Automobiles, Experts, and Regulations in the United States*, Baltimore: Johns Hopkins University Press, 2019.

412 Radiation safety (associated with the Manhattan Project and subsequent American nuclear weapons complex) and chemical weapons safety were two parallel concerns of military-sponsored research during the offensive program at Detrick. See Barton C. Hacker, *The Dragon's Tail: Radiation Safety in the*

One major difference between industrial safety expertise and the challenges faced at Detrick lay in the hazards that Detrick workers faced. Detrick researchers, like their civilian microbiologist peers, worked with and on non-human organisms ranging from experimental animals to pathogenic microbes. On paper, one could describe these organisms as passive subjects of research, manipulated by human scientists to produce knowledge. A practicing microbiologist would be well aware of how active these organisms could be: animals could scratch, bite, and otherwise be recalcitrant research subjects, and microbes would readily infect researchers' bodies, whether through those human-animal interactions, accidents in using technologies like syringes and pipettes, or through some other unknown mechanism. Nonhuman organisms, to adopt the theoretical language of some scholars of science, had an agency of their own when humans interacted with them in the laboratory.[413] The evolution of laboratory safety practices at Detrick, which is to say the art of avoiding infection by those microbes, is in part a story of humans developing a professionalized appreciation for and knowledge of that agency.

Manhattan Project, 1942-1946, Berkeley: University of California Press, 1987; J. Samuel Walker, *Permissible Dose: A History of Radiation Protection in the Twentieth Century*, Berkeley: University of California Press, 2000; Russell B. Olwell, *At Work in the Atomic City: A Labor and Social History of Oak Ridge, Tennessee*, Knoxville, TN: University of Tennessee Press, 2004; and Part 1 of Brinda Sarathy, Vivien Hamilton, and Janet Farrell Brodie (eds), *Inevitably Toxic: Historical Perspectives on Contamination, Exposure, and Expertise*, Pittsburgh: University of Pittsburgh Press, 2018. See also Simone M. Müller, "'Cut Holes and Sink 'em:' Chemical Weapons Disposal and Cold War History as a History of Risk," *Historical Social Research* 41 no 1 (2016), pp 263-286.

[413] Major works exploring the agency of non-human organisms (and especially microbes) include Bruno Latour, *The Pasteurization of France*, Cambridge, MA: Harvard University Press, 1988; Chapter 1 of Timothy Mitchell, *Rule of Experts: Egypt, Techno-Politics, and Modernity*, Berkeley: University of California Press, 2002; Donna J. Haraway, *When Species Meet*, Minneapolis: University of Minnesota Press, 2007. Also see Amanda Rees, "Animal Agents?: Historiography, Theory and the History of Science in the Anthropocene," *British Journal for the History of Science Themes* 2 (2017), pp 1-10.

The Safety Division

The dream (or nightmare) of using the germ theory and its accompanying science of microbiology to spread disease as a weapon of war was decades old when construction began at the small American airbase of Detrick Field, Maryland in the spring of 1943.[414] For American military officials and microbiologists, however, the opening of Camp Detrick, as it was renamed, was the concrete actualization of what had been a vague, almost science-fictional idea. While some of the major research programs launched at Detrick were novel, like attempts to assess minimum concentrations of airborne germs needed to infect experimental animals or to grow pathogenic microbes by the ton, the pathogens used in this research and the risks entailed working with them were not. The risk of laboratory researchers being infected by the subjects of their research was as old as microbiology and had been a noted phenomenon among microbiologists for almost as long. These risks were generally a tacitly accepted part of a microbiologist's life, accepted by individual microbiologists and mitigated with individual skill. By the early 1940s, the phenomenon of laboratory infection and means to mitigate it were a minor, if

[414] The use of germs used for warfare and terrorism had been a theme in science fiction for decades, appearing in stories like H. G. Wells' 1894 "The Stolen Bacillus," Jack London's 1910 "The Unparalleled Invasion," and Aldous Huxley's 1932 *Brave New World*. See H. G. Wells, "The Stolen Bacillus," in *The Stolen Bacillus and Other Incidents*, London: Methuen & Co., 1895; Jack London, "The Unparalleled Invasion: Excerpt from Walt Nervin's *Certain Essays in History*," in I. F. Clarke (ed), *The Tale of the Next Great War, 1871-1914: Fictions of Future Warfare and of Battles Still-to-Come*, Syracuse, NY: Syracuse University Press, 1995, pp 257-270; Aldous Huxley, *Brave New World*, London: Chatto & Windus, 1932. The use of modern germ theory in warfare had also entered into the realm of military officers and diplomats. Imperial Germany had secretly used germs for biological sabotage of neutral countries (including the US) during the First World War, and the 1925 Geneva Protocol, negotiated to ban the use of chemical weapons in war, had a prohibition against the use of bacteriological weapons appended to it as well. See Mark Wheelis, "Biological Sabotage in World War I," and Jerzy Witt Mierzejewski and John Ellis van Courtland Moon, "Poland and Biological Weapons," both in Erhard Geissler and John Ellis van Courtland Moon, eds., *Biological and Toxin Weapons: Research, Development and Use from the Middle Ages to 1945 (SIPRI Chemical & Biological Warfare Studies 18)*, New York: Oxford University Press, 1999, pp 35-62, 63-69.

growing, part of the scientific literature.[415] Likewise, the danger of exotic pathogens escaping from human laboratories to invade spaces where they had not been before was an active concern of public health researchers, mitigated by policies about the geographic locations where research on these pathogens could take place.[416] With this expedient unavailable for the growing research center on the outskirts of the town of Frederick, Maryland, and the danger of researchers infecting themselves in their deliberate attempts to make already infectious microbes as dangerous as possible, concerns about the safety of Detrick research quickly became pronounced. As anthropologist Mary Douglas has argued, "risk" is culturally mediated, as much a social construction of a danger as unacceptable as a disinterested calculation.[417] It is perhaps unsurprising, then, that in a social world built on conceptualizing pathogens as dangerous weapons rather than passive objects of research, these risks seemed so pressing.

Established in August 1943 as research at Detrick was beginning, the Safety Division (as it would be known from 1944 on) was responsible for keeping the microbes at Detrick under control. This was seen as an important mission by Detrick leadership for both ethical and pragmatic reasons, and reflecting this priority, the Division grew quickly, with 86 people, or 4% of the entire population of Detrick in by the end of its first

[415] See e.g. R. R. Parker and R. R. Spencer, "Six Additional Cases of Laboratory Infection of Tularæmia in Man," *Public Health Reports* 41 no 27 (1926), pp 1341-1355; G. W. McCoy, "Accidental Psittacosis Infection Among Personnel of the Hygienic Laboratory," *Public Health Reports* 45 no 16 (1930), pp 843-845; Meyer and Eddie, "Laboratory Infections Due to *Brucella*."

[416] American and Canadian veterinary officials, for instance, banned research on the cattle disease rinderpest from the North American mainland. Such research (including for the WWII-era biological warfare program) was relegated to isolated islands. See Amanda Kay McVety, *The Rinderpest Campaigns: A Virus, Its Vaccines, and Global Development in the Twentieth Century* (New York: Cambridge University Press, 2018).

[417] See e.g. Mary Douglas and Aaron Wildavsky, *Risk and Culture: An Essay on the Selection of Technical and Environmental Dangers*, Berkeley: University of California Press, 1982.

year in August 1944.[418] Leadership of the division fell to University of Chicago bacteriologist Gail Dack. Dack was an expert on botulinum toxin who would go on to establish the Food Research Institute, devoted to the study of foodborne diseases like botulism, and was thus also skilled at researching this highly toxic substance and its parent bacteria safely. He was thus a logical choice to oversee safety when early Detrick research was focusing so intently on the construction of a botulinum-producing pilot plant (informally called "Black Maria" by Detrick researchers).[419] Dack and his Safety Division worked closely with the team designing and operating this plant, and as the plant's director, E. M. Foster, subsequently put it, the two research programs of "safety" and "unsafety" closely complemented one another.[420] This collaboration was evidently successful, as unlike most other agents researched at Detrick during the war, no-one in the camp suffered a case of botulinum poisoning. This collaboration continued as the Pilot Plant team shifted to developing anthrax production methods in mid-1944, though evidently with less-than-perfect results, as the accident of September 22, 1944 demonstrates. Though the Safety Division was responsible for the safety of laboratory research as well as the pilot plants, their record there was likewise mixed, though the Division did dissuade the Army from enforcing military routines of "white glove inspections" on these hazardous spaces.[421]

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[418] U.S. Army Corps of Engineers St. Louis District, "Archives Search Report Operational History for Potential Environmental Releases Fort Detrick," June 16, 2014, p 31.
[419] Regis, *The Biology of Doom*, pp 43-46.
[420] See 1:24:20 in recording of E. M. Foster, Recording of an interview by Barry Teicher, January 13, 2000, University of Wisconsin, Part 1, available at https://minds.wisconsin.edu/handle/1793/70327
[421] See Theodor Rosebury, "Five Morbid Pieces," (unpublished manuscript, n.d.) p 56 in National Library of Medicine (NLM) Theodor Rosebury Papers (MS C 634), Box 10 Folder 27 ("Five Morbid Pieces"). This unpublished manuscript is a book-length memoir of five of Rosebury's near-death experiences (including a 1945 psittacosis infection at Detrick), apparently written in the early 1970s. Rosebury was making a living as an author at the time and evidently wrote the manuscript for publication (albeit unsuccessfully).

The creation of the Safety Division reflected Detrick leadership's humanitarian and practical concern with avoiding illness among valuable trained personnel and the community in which they lived, but it also reflected an obsessive concern with maintaining the absolute secrecy of every aspect of the BW program, including its very existence. The practice of military secrecy, typically devoted to keeping control over documents and spaces deemed secret, faced the additional threat at Detrick of human bodies, particularly those of laboratory workers. Infected or atypically immunized bodies, if they fell in the hands of the enemy, could serve as a telltale sign of American work on biological warfare, reflecting the logic Allied intelligence analysts used when they interpreted botulinum immunization in captive Germans as a sign of a German intent to attack the D-Day landings with that toxin.[422] To prevent Axis intelligence from reaching a similar conclusion using American bodies, Detrick researchers were prohibited from donating blood, required to clear any treatment for illness with Detrick medical authorities, and were required to sign a document giving military authorities control over their body for a secret burial should they be killed by a BW agent.[423] This last requirement, like many things at Detrick, served a second, epistemic purpose, with the bodies of BW victims serving as a potential treasure trove of data (indeed, one of Detrick's most virulent anthrax strains was isolated from the body of William Boyles, a 1951 pulmonary anthrax victim who was the first person to be killed at Detrick).[424] Civilian bodies, should the pathogens at Detrick escape from the facility, posed another

[422] Cochrane, *Biological Warfare Research*, p 137

[423] Ibid, p 125. It is unclear how consistently signing this document was required of incoming researchers. For instance, Rosebury recounts being prompted to sign this document when hospitalized with a psittacosis infection, implying that he had not done so before. Nonetheless, at least in a potentially fatal case like his, this legal control over secret-bearing bodies was sought. See Rosebury, "Five Morbid Pieces," p 75 in NLM Rosebury Papers.

[424] Jones, *Death in a Small Package*, p 161.

threat, as an outbreak of disease in the civilian population of Frederick "would be disastrous because of the resultant publicity."[425] The elaborate safety measures undertaken at Detrick, from laboratory designs intended to contain microbes, to a regimen of showers and disposable clothing for researchers leaving these laboratories, to special facilities to incinerate waste air and sewage, all served to protect secrecy as much as the citizens of Frederick.

The Safety Division was officially bifurcated into a "biological protection" branch, responsible for the immunization and surveillance of workers' bodies, and a "operational control" branch, responsible for ensuring the safety of laboratory and pilot plant procedures and monitoring for the escape of microbes from these spaces.[426] Less officially, the Division's mission was bifurcated along somewhat different lines. As Rosebury later put it, their "job… was to prevent accidents, laboratory infections, and such things, as well as to make sure nothing dangerous got out to the surrounding community. In the second area," he noted dryly, "they did a good job."[427] The Safety Division did indeed deploy an array of measures to achieve the first goal (the individual protection of Detrick researchers) but its major emphasis was on this second, community protection mission. Virtually all safety measures instituted at Detrick during the war served this purpose directly or indirectly, helping to maintain a regime containing Detrick microbes within the grounds of the camp.

There were around 70 confirmed laboratory infections at Detrick during the Second World War and its immediate aftermath, and none outside of Detrick itself (see

[425] Cochrane, *Biological Warfare Research*, p 126.
[426] Ibid, p 151.
[427] Rosebury, "Five Morbid Pieces," p 73 in NLM Rosebury Papers.

Figure 4, below). This record is open to interpretation. On the one hand, considering the number of researchers, the pace of work, and the virulence of the pathogens studied at Detrick, this laboratory infection record was normal or even remarkably low (probably a testament to the skill of the researchers recruited for Detrick). Pathogens like *Coxiella burnetii* or the various species of *Brucella* that caused brucellosis were expected to infect most or all of those researching them in contemporary civilian microbiology laboratories, and indeed, laboratories like those of the George Hooper Foundation or the National Institutes of Health experienced outbreaks of these diseases at least as bad as those at Detrick.[428] The wartime laboratory infection rate of *B. anthracis* (all of which was recorded as cutaneous rather than the more serious pulmonary anthrax) was in fact so 'disappointingly' low that that many in the American program (including Dack) discounted that organism's utility as a weapon by the end of the war.[429] No one died at Detrick during the war, despite elaborate preparations for secret autopsies and sealed caskets. The advent of penicillin, which was used extensively for both experimental treatments and for post-accident prophylaxis, may have contributed to this achievement, but it was an achievement nonetheless. Finally, and most importantly, the Safety Division seemingly achieved the basics of its community protection goal, with no release of

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[428] Meyer and Eddie, "Laboratory Infections Due to *Brucella*"; Robert J. Huebner, "Report of an Outbreak of Q Fever at the National Institute of Health II. Epidemiological Features," *American Journal of Public Health* 37 no 4 (1947), pp 431-440.

[429] See e.g. "DEF Committee Panel, Report of Meeting, January 11, 1945," in National Academy of Sciences Archives collection "Committees on Biological Warfare, 1941-1948" (NAS BW), Box 6 Folder 11 ("Projects: 'N:' Meetings, 1944-1945"); Cochrane, *Biological Warfare Research*, p 253. This skepticism of anthrax as a weapon was so pronounced by the summer of 1944 that a group of researchers led by Dennis Watson convened an impassioned meeting to argue that rickettsial diseases like Q fever should be prioritized instead. See Dennis W. Watson, "The Rickettsial Diseases and their Application to B.W.," Camp Detrick, July 7, 1944 in University of Minnesota Archives (UMN) UA-01167 (Dennis Watson Papers) Box 3, Unnumbered Folder Entitled "Anthrax Biological Warfare Camp Detrick, 1944-1969." A follow-up meeting apparently became so heated that Watson and one of his allies (both of whom held officer's commissions) received formal military reprimands for their conduct. See Cochrane, *Biological Warfare Research*, p 480.

microbes from Detrick or outbreak among the wider community of Frederick being recorded. On the other hand, while the community protection mission did not catastrophically and obviously fail, it is far from clear that smaller failures (especially in a sewage treatment system with serious defects that would become clear later in the decade) would have been detected and recorded in the first place.[430] It had in large part been the Safety Division's good fortune, not its actions, that had kept anyone from dying at Detrick; fortune not only for humanitarian reasons but also because it kept political scrutiny away from Detrick and its leadership.[431] Finally, and most fundamentally, dozens of people were still infected by dangerous and virulent pathogens, and given the lackluster performance of the base hospital's diagnosis (see below), it is unclear how many other laboratory infections went unrecorded. The tacit expectation that such infections would occur as a normal part of pathogen research was common in microbiology at the time, but for the bureaucratized safety organization at Detrick, this tacit acceptance of infection became an increasingly unacceptable standard after WWII. By the Safety Division's own standards of a decade later, the organization had in large part failed during the wartime years.

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[430] The Safety Division conducted extensive testing of the sewage treatment system in the late 1940s, finding that it was insufficient to sterilize *Bacillus globigii* spores (and thus, presumably, *B. anthracis* spores). The system was extensively refit into the 1950s, reflecting a common phenomenon of Detrick safety where standards of acceptable safety constantly changed in the face of more study. See Arnold G. Wedum, "Safety Program at Camp Detrick, 1944-1953 (Special Report No. 185)," Frederick, MD: Chemical Corps Biological Laboratories, 1953, pp 111-129.

[431] Rosebury recalled that he and his fellow researchers "tended to picture [the Safety Division] as spending most of their time sitting at desks and worrying about possible future Congressional investigations." Rosebury, "Five Morbid Pieces," p 74 in NLM Rosebury Papers.

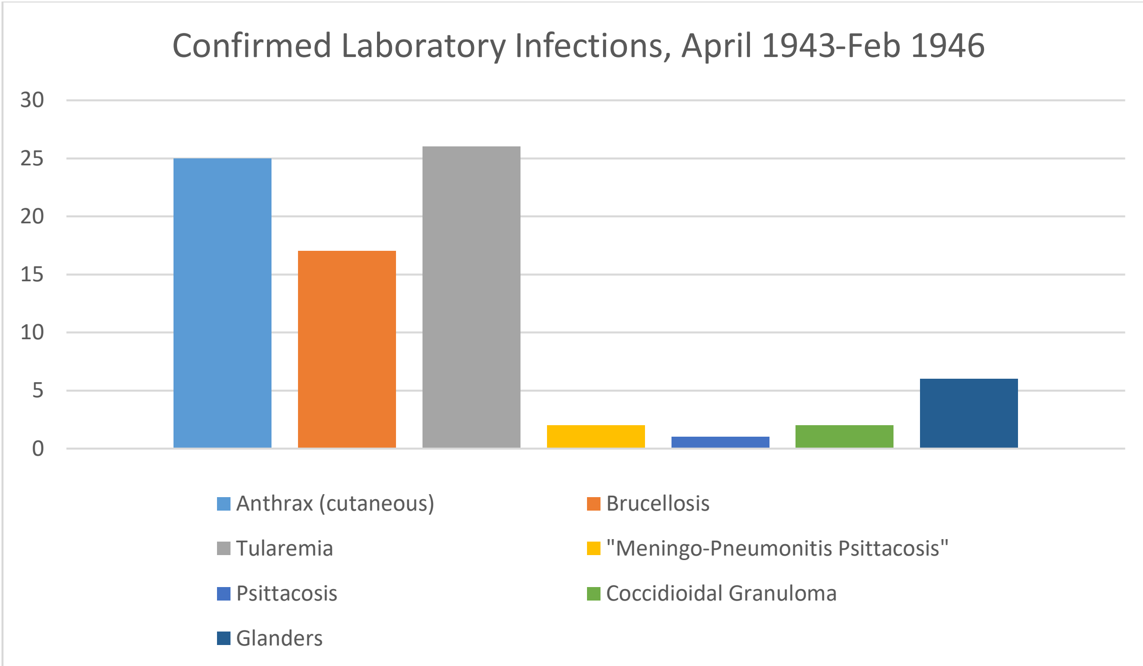

Figure 4: Confirmed Accidental Laboratory Infections, WWII and Immediate Aftermath[432]

In the months following the end of WWII, Detrick's researchers demobilized, with a large number returning to civilian research, leaving only a small core behind. For several months, the fate of Detrick and the entire biological weapons program was uncertain, with some former members of the Detrick community going so far as to prepare arguments for the facilities to be turned over for civilian research when the military abandoned them.[433] As it stood, the military ultimately decided to keep the program active (albeit at sharply reduced funding levels) amidst the early stirrings of Cold War rivalry in 1946, and over the next few years, the leadership which would staff Detrick over the next two decades, largely drawn from this core, took shape. Among these was Arnold G. Wedum, who would take over the directorship of Detrick's Safety Division from Gail Dack when Dack returned to research. Wedum would serve in this

[432] Data from Cochrane, *Biological Warfare Research*, p 172.
[433] Ira Baldwin, "Some Suggestions Concerning Plans for the Future of Military Research," n.d. (ca. 1945) in University of Wisconsin Archives (UWA) Ira L. Baldwin Papers (Series 9/10/11), Box 11 Folder 2.

post for the next 23 years of the offensive biological weapons program and subsequently in Detrick's cancer research until his death in 1976. In this position, he would lead the Safety Division to develop a program of individual protection, and a community of expertise to underlie it which would be influential in the larger world of microbiology. Wedum was born in 1903, and unusually for an American microbiologist, held an MD as well as a PhD in bacteriology. He was a professor of bacteriology at the University of Cincinnati before coming to Detrick at the recommendation of Walter Nungester, with research focusing on rheumatic fever in collaboration with his wife Bernice, who was also a MD.[434] Wedum's subordinates recalled him as an active leader and personal mentor, collaborating closely with them and with other groups at Detrick on the smallest details of research and safety technologies.[435] Much of the staff of the Safety Division he joined would also remain there for the next few decades, with scientists like Morton Reitman, Gardner G. Gremillion, and G. Briggs Phillips making careers of safety at Detrick (unprecedented in the larger world of microbiology).

[434] Biographical detail about Wedum is difficult to reconstruct, but Wedum mentions serving as a professor of bacteriology at the University of Cincinnati from 1937 to 1943 in a 1974 letter to Ludwik Gross, who was at Cincinnati's Christ Hospital at the time. See Wedum to Gross, March 4, 1974 in NLM Ludwik Gross Papers (MS C 504), Box 3 Folder 55. Nungester, like Wedum, was a MD as well as a PhD. On his recommendation of Wedum for the Detrick job, see Walter Nungester, "Professional Activities of W.J. Nungester- Compiled for Personal Use December 1977," p 17, in ASM 13-IIBA Folder 2 ("Presidential Papers- Walter Nungester.")
[435] W. Emmett Barkley, "In Celebration of Dr. Arnold Wedum's Legacy," *Journal of the American Biological Safety Association* 1 no 1 (1996), p 6.

Figure 5: Arnold Wedum in September 1947[436]

Freed from the frenetic pace of wartime research, Wedum's Safety Division of the late 1940s shifted much of its focus to expanding a wartime research program on the accidental generation of aerosols in the laboratory. This research reflected the continuing importance of the aerobiological vision at Detrick. An airborne mechanism for unexplained laboratory infections had been suggested by researchers, including Karl F. Meyer, since the 1930s. The Rosebury-Kabat Report, an influential document in the formation of the bioweapons research program, had focused specifically on pathogens notorious for such unexplained infections, reasoning that if they were prone to airborne transmission in the laboratory, they could do so on the battlefield as well.[437] Wartime

[436] Photo from Richard M. Clendenin, *Science and Technology at Fort Detrick: 1943-1968*, Frederick, MD: Fort Detrick Technical Information Division, 1968, p 17. For an updated compliment to this official history, see Norman M. Covert, *Cutting Edge: A History of Fort Detrick, Maryland, 1943-1993*, Fort Detrick, MD: US Army Garrison, 1993.

[437] Meyer and Eddie, "Laboratory Infections Due to *Brucella*"; Rosebury and Kabat published their paper in 1947 (see Chapters 2 and 5). Theodor Rosebury and Elvin A. Kabat, "Bacterial Warfare: A Critical Analysis of the Available Agents, Their Possible Military Applications, and the Means for Protection Against Them," *Journal of Immunology* 56 no 1 (1947), pp 7–96. Their paper impressed leaders of the National Academy of Sciences group directing the nascent bioweapons program. See E. B. Fred, "Memorandum: Subject: Conference with Prof. A. R. Dochez and discussed the paper on Bacterial Warfare

work at Detrick lent credence to this hypothesis. Most notably, Dack's Safety Division used high-speed photography to identify that common laboratory operations like introducing microbes to an agar plate could produce small droplets, which could potentially desiccate and aerosolize whatever microbes were in them. The so-called Cloud Chamber Project under Rosebury, which was in charge of establishing data on airborne "doses" for weapons development, had similarly used such photography to recreate an accident leading to a psittacosis infection.[438] Under Wedum, this research continued, using simulant organisms like *Bacillus globigii* and *Serratia marcescens* and air samplers to investigate common laboratory operations from running high-speed blenders to pipetting to operating the decade-old ultracentrifuge, and finding that most generated considerable aerosols.[439] As these experiments were occurring, epidemiological evidence from a major outbreak of Q fever among National Institutes of Health researchers in 1946-1947 also suggested that many of these researchers had been infected by airborne transmission of the pathogen.[440] Inspired by these developments, Wedum's Safety Division began to tighten the ideal of "containment" which had prevailed during the war, seeking to keep individual researchers and these laboratory aerosols apart.

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by Drs. Theodor Rosebury, Elvin A. Kabat and Martin H. Boldt," September 11, 1942 in NAS BW Box 7 Folder 19 ("Fred, E.B.: Memoranda (Black Book): 1942-1943")

[438] K. R. Johansson and D. H. Ferris, "Photography of Airborne Particles during Bacteriological Plating Operations," *The Journal of Infectious Diseases* 78 no 3 (1946), pp 238-252; Theodor Rosebury, Harold V. Ellingson, Gordon Meiklejohn and Frank Schabel, "A Laboratory Infection with Psittacosis Virus Treated with Penicillin and Sulfadiazine, and Experimental Data Bearing on the Mode of Infection," *The Journal of Infectious Diseases* 80 no 1 (1947), pp 64-77. Rosebury himself was the victim of this accident.

[439] A. G. Wedum, "Nonautomatic Pipetting Devices for the Microbiologic Laboratory," *Journal of Laboratory and Clinical Medicine* 35, no 4 (1950), pp 648-651; Raymond E. Anderson, Leon Stein, Marcus L. Moss, and Noel H. Gross, "Potential Infectious Hazards of Common Bacteriological Techniques," *Journal of Bacteriology* 64 no 4 (1952), pp 473–481; Morton Reitman, Milton A. Frank, Sr., Robert Alg, and Arnold G. Wedum, "Infectious Hazards of the High Speed Blendor and Their Elimination by a New Design," *Applied Microbiology* 1 no 1 (1953) pp 14–17. The Safety Division's adoption of specialized aerobiological instruments like air samplers can be seen as an example of Detrick serving as a "trading zone" of the sort described by Peter Galison. See Galison, *Image and Logic*.

[440] Huebner, "Report of an Outbreak of Q Fever at the National Institute of Health."

This tightening containment ideal was ultimately underpinned by a transformed vision of the laboratory's ecological relationships. From the perspective of most humans, the microbiological laboratory was a site of knowledge production, a human-directed space in which non-human animals were used as experimental subjects, and microbes were grown and manipulated (especially, in the case of pathogen research, by being injected into the bodies of experimental animals), all to produce an output of scientific knowledge for the humans. However, the meaning of this space and these relationships was quite different from the perspective of the other organisms, which was tacitly the perspective that the Wedum group increasingly adopted. Experimental animals, living their lives and dying their deaths in laboratories and attached menageries were not just passive laboratory instruments, and could bite their handlers, interact with other animals, and carry microbe loads or even diseases of their own. In turn, for pathogenic microbes, the laboratory was awash with opportunities to flourish. Humans endeavored to provide them with bountiful growth mediums, free from competitor species, and would punctuate this bounty by regularly introducing them into the bodies of animal hosts. Furthermore, in the case of human disease agents, the bodies of the humans themselves offered valuable potential resources for growth. A microbe could be accidentally swallowed by a human overzealous with a pipette, could enter a human body in alliance with a scratching experimental animal, or could find new animal hosts among densely packed cages. Most importantly, however, the Wedum group's aerobiology research showed that these microbes could waft through the air in an aerosol generated by any number of common human activities, to be breathed into human or animal lungs. The containment ideal was meant to flatten these complex ecological relationships, breaking these links that made the laboratory as bountiful a space for microbial growth as it was a space for human knowledge production.

The Tightening Containment Ideal

The initial mission of the Safety Division in the Second World War was to detect and prevent any escape of microbes from Detrick into the wider world, thus preventing what could be a medical and intelligence disaster, and to minimize laboratory infection among Detrick's researchers. Of these two, the community protection mission took priority, with far more Safety Division effort and spending upholding this mission of containing germs within the base than the individual protection mission of keeping them from infecting researchers' bodies. Under the postwar Wedum regime, this community protection mission remained an important part of the Safety Division's activities, but one that was increasingly in the background as Wedum's group focused on tightening the containment regime from the base as a whole to specific research spaces within the laboratory. By the early 1950s, a technocratic 'containment ideal,' based on containment technologies like safety cabinets and on rationalized management of laboratory activities and hazards had become the basis for the Wedum group's campaign to eradicate laboratory infection entirely.

"Containment" of microbes most fundamentally entailed establishing and maintaining a control of space. Accordingly, a major component of the WWII base-wide containment regime lay in the architecture of the new buildings proliferating across the grounds of what had been a sleepy airfield. While the new buildings of Detrick were built quickly, their designs incorporated advice from the research teams who would be using them. This allowed these researchers much greater influence over the space they were to work in than they generally enjoyed in the civilian world, allowing them to incorporate recent aerobiologically-influenced ideas about microbiological safety. The Aerobiology building, for instance, was built in 1944 and completed in January 1945 to Cloud Chamber Project specifications, which segregated the building between 'hot' areas where pathogen research was taking place (including the further contained space of the cloud chambers themselves) and areas like lunchrooms, offices, and the like which supposedly would remain uncontaminated. An elaborate system of airlocks, changing rooms, and showers mediated the flow of researchers between these two spaces, which along with a liberal use of negative pressure airflow and germicidal ultraviolet lamps were intended to keep pathogens confined within the research spaces.[441] These research buildings, in turn, were separated from the rest of the facility in a fenced "Restricted Area."[442] Directional airflow was a particular feature of this architectural system. This was a relatively uncommon feature in normal microbiological research, in which "it [was] customary to perform many routine bacteriological operations in still air, often in a small enclosed cubicle, in order to minimize air-borne contamination" of experimental cultures.[443] In contrast, Detrick buildings commonly used negative pressure ventilation systems, which ensured that air (and airborne microbes) would flow out of rooms into the ventilation system itself (rather than between rooms), and this air was in turn channeled into gas-fired air incinerators, intended to destroy any errant microbes from these buildings before the air was vented to the outside world through large chimneys. Building and constantly

[441] Theodor Rosebury, *Experimental Air-Borne Infection*, (Baltimore: Williams and Wilkins Co, 1947), pp 6-10.
[442] U.S. Army Corps of Engineers St. Louis District, "Archives Search Report Operational History for Potential Environmental Releases Fort Detrick," June 16, 2014, pp 20-21.
[443] Rosebury et al, "Laboratory Infection with Psittacosis," p 77.

operating this system was a complex and financially costly proposition, reflecting the importance placed on containing microbes with the grounds of Detrick. The Safety Division likewise set up a similarly elaborate gas-fired sewage sterilization system.[444] The high priority accorded these systems and base-wide containment in general shows the importance of the community protection mission to the Safety Division, at the back of which lurked the obsession with secrecy pervading practically everything in Detrick's management. This secrecy, in turn, precluded Frederick's citizens from questioning the efficacy of this containment regime that was standing between dangerous pathogens and their bodies.

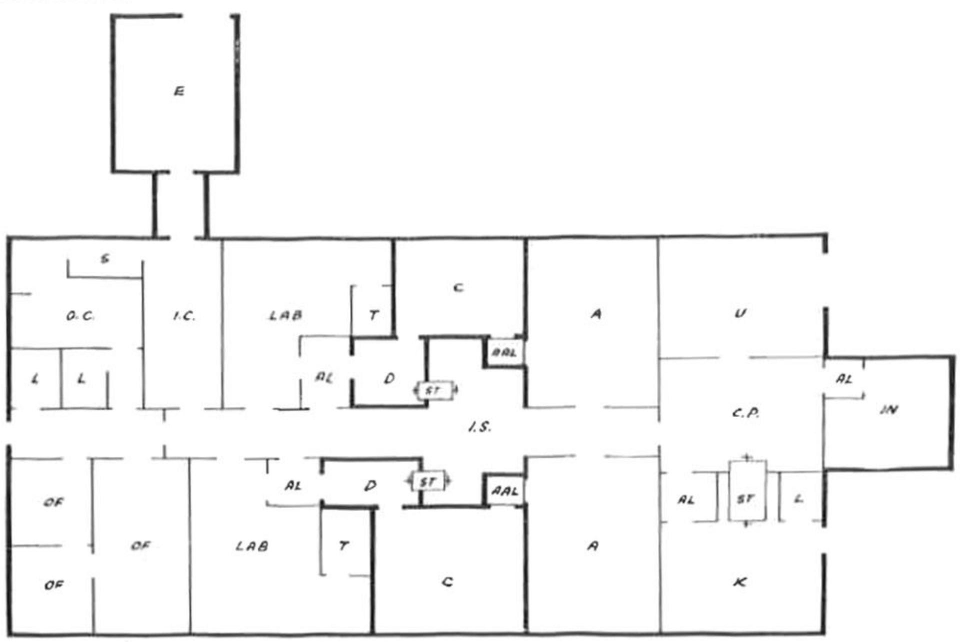

FIGURE 1. Floor plan of the cloud chamber building. *A*, animal room; *AAL*, animal airlock; *AL*, airlock; *C*, chamber room; *D*, decontamination room; *E*, shop; *I.C.*, inner change room; *IN*, incinerator room; *I.S.*, inner service area; *K*, kitchen; *L*, latrine; *LAB*, laboratory; *O.C.*, outer change room; *OF*, office; *ST*, sterilizer; *T*, transfer room; *U*, utility room.

Figure 6: Aerobiology Building Floor Plan, WWII[445]

[444] Wedum, "Safety Program at Camp Detrick, 1944-1953," p 111.
[445] Image from Rosebury, *Experimental Air-Borne Infection*, p 7.

Ensuring that these measures remained effective was a major part of the Safety Division's time and effort. Samples of sewage were regularly cultured, air was tested using the same aerobiological sampling devices used for research, and spaces like laboratories and pilot plants were swabbed and cultured, all to detect any errant microbes before they could breach base-wide containment. "S Division personnel were in constant circulation with swab and test tube, checking for leaks and alert for gross flaws."[446] Speed was of the essence in detecting the presence of pathogens where they shouldn't be, prompting the Safety Division to research culture techniques and animal assays to rapidly detect particular species. Should any escaping microbes be detected, a decontamination effort using germicidal substances would be put into effect, and even within spaces in which microbes were permitted a steady cycle of decontamination between experiments was a normal feature of research.[447]

The counterpoint to this community protection mission was the protection of individual Detrick researchers. Laboratory workers' bodies were a focal point for many of these safety measures, which in turn also indirectly served the community protection mission. A rigorous system of showers before entering and leaving the laboratory, changes of clothes, and regular medical examinations for cuts (a potential source of infection) loomed large in wartime Safety Division regulations.[448] So too did a mandatory system of vaccination, drawing upon both standardized and experimental vaccines (including vaccines developed as part of the BW research program itself). This

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[446] Cochrane, *Biological Warfare Research*, p 159.

[447] Cochrane, *Biological Warfare Research*, pp 162-163.

[448] Ibid, p 152. The system of showers was enforced even on high-ranking visitors. E. M. Foster recalled showering with University of Wisconsin President E. B. Fred when Fred visited him at Detrick in early 1945. See 1:18:18 in recording of E. M. Foster, Recording of an interview by Barry Teicher, January 13, 2000, University of Wisconsin, Part 1, available at https://minds.wisconsin.edu/handle/1793/70327

mandatory use of experimental vaccines was legally permissible with the permission of the military Surgeons General and the rather thin justification that the intent of this usage was therapeutic, not experimental.[449] Nonetheless, this inspired resistance among some Detrick workers, who were subsequently reassigned out of the laboratories and pilot plant. Only individuals conforming to these requirements would be allowed into the fenced "Restricted Area" in which 'hot' research took place.[450] Beyond hopefully protecting individuals, regulating the cleanliness and immunological profile of Detrick researchers allowed the Safety Division to impose control over a potential vector by which pathogens might breach base-wide containment. A similar logic of maintaining containment by regulating the bodies of macroscopic organisms was deployed in a campaign to kill insects and rodents both on the base and (in collaboration with the Public Health Service) in surrounding areas.[451]

While these individual protection measures were elaborate, they were also limited, generally focused on regulating researchers' bodies but not their actions within the laboratory. "The safety people," Rosebury recalled, "were not so good at tasks within the labs themselves; and a building like mine [the Aerobiology building] doubtless presented very special problems which they did not even attempt to deal with. All the

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[449] Cochrane, *Biological Warfare Research*, pp 152-158. Whatever the legal justification, Detrick personnel did tacitly serve as test subjects for the safety and (through their infection rate) efficacy of vaccines ultimately intended for wider use if Allied troops faced biological attack. As Susan Lederer has noted, the bodies of people serving in and connected to the military have been particularly available for such experimentation in American history. See Susan Lederer, *Subjected to Science: Human Experimentation in America before the Second World War*, Baltimore: Johns Hopkins University Press, 1995, pp 113-114. See also Jonathan D. Moreno, *Undue Risk: Secret State Experiments on Humans*, New York: W. H. Freeman, 2000; Susan L. Smith, *Toxic Exposures: Mustard Gas and the Health Consequences of World War II in the United States*, New Brunswick: Rutgers University Press, 2017.
[450] U.S. Army Corps of Engineers St. Louis District, "Archives Search Report Operational History for Potential Environmental Releases Fort Detrick," June 16, 2014, p 51.
[451] Cochrane, *Biological Warfare Research*, pp 168-169.

details were left to the workers themselves. We worked out our own safety problems, and the record testifies that we did it well."[452] Rosebury recalled testing out new safety procedures and devices himself, echoing the 'heroic ethos' of civilian laboratory practice.[453] Rosebury was often acerbic and dismissive when discussing authority figures, but his recollection here is largely borne out by the postwar official history of Detrick, which makes no mention of Safety Division regulation of laboratory practice in its exhaustive group of subjects, and by Safety Division documents from the 1950s which do discuss regulations of laboratory practice, dating them to after 1945. It is noteworthy that even the practice of oral pipetting, which was to blame for a number of early documented cases of laboratory infection and which was "outlawed" at Detrick's defensive naval counterpart, the Naval Medical Research Unit 1 in 1943, was not banned by Detrick's Safety Division until 1950.[454] What little Safety Division involvement there was with

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[452] Rosebury, "Five Morbid Pieces," p 73, in NLM Rosebury Papers. The Cloud Chamber project indeed suffered only one major infection, Rosebury himself.
[453] Ibid, p 58. On 'heroic' research risk-acceptance, see Lederer, *Subjected to Science*, pp 126-138. See also Rebecca Herzig, *Suffering for Science: Reason and Sacrifice in Modern America*, New Brunswick: Rutgers University Press, 2005. This work on scientific self-experimentation exists within a larger literature on human experimentation in general (notably including, in the American case, the infamous Tuskegee syphilis study). See e.g. Susan M. Reverby (ed), *Tuskegee's Truths: Rethinking the Tuskegee Syphilis Study*, Chapel Hill: University of North Carolina Press, 2000; Jordan Goodman, Anthony McElligott, and Lara Marks (eds), *Useful Bodies: Humans in the Service of Medical Science in the Twentieth Century*, Baltimore: Johns Hopkins University Press, 2003; Susan M. Reverby, *Examining Tuskegee: The Infamous Syphilis Study and Its Legacy*, Chapel Hill: University of North Carolina Press, 2009; Ulf Schmidt, *Secret Science: A Century of Poison Warfare and Human Experiments*, New York: Oxford University Press, 2015; Lisa Martino-Taylor, *Behind the Fog: How the U.S. Cold War Radiological Weapons Program Exposed Innocent Americans*, New York: Routledge, 2017; Ulf Schmidt, Andreas Frewer, and Dominique Sprumont (eds), *Ethical Research: The Declaration of Helsinki, and the Past, Present, and Future of Human Experimentation*, New York: Oxford University Press, 2020.
[454] On oral pipetting being "outlawed" at Detrick, see the Fort Detrick *Safety Bulletin* 5 no 2 (1958), p 1. Electronic copies of this internal Detrick publication from 1952-1963 are held in the National Agricultural Library Collection 359 (American Biological Safety Association) (NAL ABSA). Arnold Wedum, "Pipetting Hazards in the Special Virus Cancer Program" *Journal of the American Biological Safety Association* 2 no 2 (1997), pp 11-21, 16 discusses the NAMRU-1 ban on oral pipetting during the Second World War. This and several other of Wedum's papers penned in the 1970s were published in the inaugural issues of the ABSA journal, along with several papers reminiscing about Wedum and the origins of the ABSA community.

laboratory practice tended to be experimental rather than regulatory: for instance, the study of aerosol generation with high-speed photography. Outside of these studies, however, safety within individual laboratories was largely left to the leadership of the laboratories themselves, as was the practice in civilian microbiology.

Likewise, while the individual protection that existed focused on protecting and examining workers' bodies, this emphasis did not initially extend to the base's hospital. There, the medical staff were drawn from the regular Army Medical Corps pool and were not necessarily well-versed in the exotic diseases researched at Detrick, or even well-informed about activities on the post. Despite the importance of identifying laboratory infections as quickly as possible both to effectively treat them and to gather data, the early medical contingent treated illness in the camp as though it was any other Army facility. In October 1944, for instance, 3 patients who in retrospect probably had pulmonary anthrax were not diagnosed and "no effort was made to isolate the organism," despite the fact that they had previously been accidentally exposed to anthrax aerosol.[455] This blasé attitude imperiled these patients' lives, but perhaps more importantly from the Janus-faced standpoint of biological weapons research, it neglected crucial human data which could have been used by the weapons program.[456] Similarly, when Rosebury came down with psittacosis after an accident in which a defective vial sprayed culture on his

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[455] Cochrane, *Biological Warfare Research,* p 171.

[456] Laura Stark and Nancy Campbell's concept of "stowaways" in biology is a useful way to think about the status of 'accident' as a retrospective human 'experiment' in biological weapons research. See Laura Stark and Nancy D. Campbell, "Stowaways in the History of Science: The Case of Simian Virus 40 and Clinical Research on Federal Prisoners at the US National Institutes of Health, 1960," *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences* 48 part B (2014), pp 218-230. See also Brian Balmer, "How Does an Accident Become an Experiment? Secret Science and the Exposure of the Public to Biological Warfare Agents," *Science as Culture* 13 no 2 (2004), pp 197-228 for a discussion of the implications of secrecy on such 'accidental experiments.'

hands, it took some time for him to convince medical staff that he was not simply malingering.[457] Due mainly to Rosebury's personal initiative and authority, data gathered and reconstructed from this incident were used to establish a human dose for psittacosis aerosol, but this was despite the medical staff's handling of the infection. The staff did ultimately save Rosebury's life with experimental antibiotic treatment, but given the prevalence of laboratory infections and their importance for research, both safety and research concerns prompted reforms in the Medical Division beginning in mid-1945. New staff were brought in, and liaison relationships with the Safety and research divisions (and especially their diagnostic laboratories) were established.[458] By the post-war period, medical diagnosis and treatment had become an integral part of the Detrick safety system, with Wedum later identifying the postwar medical division's policy of initially regarding all illnesses as occupational as an integral element of his system.[459]

The showers, examinations, and vaccines used for individual protection during WWII can be generally characterized as medical and hygienic measures, centered on the bodies of workers themselves, while community protection, in turn, was principally served by the technological measures of building design and effluent sterilization. A major exception to this dichotomy, however, lay in Rosebury's Cloud Chamber project and its eponymous experimental spaces. The project was central to the Detrick research program, intended to investigate whether and under what concentrations and conditions aerosols of various pathogens could infect experimental animals. Research of this nature was key to constructing microbes as predictable 'biological weapons,' but it posed an

[457] Rosebury, "Five Morbid Pieces," pp 69-70 in NLM Rosebury Papers.
[458] Cochrane, *Biological Warfare Research*, pp 170-172.
[459] Arnold G. Wedum, "Laboratory Safety in Research with Infectious Aerosols," *Public Health Reports* 79 no 7 (1964), p 630.

obvious problem: if an experiment was at all successful in subjecting research animals to an infectious airborne dose, that same dose would pose a serious threat to the researchers as well. Following Thomas Hughes, historian Gerard Fitzgerald describes this safety problem as a 'reverse salient' in need of solution for the sociotechnical system of biological weapons research to be developed.[460] The solution, Fitzgerald argues, lay in the physical isolation of experimental spaces from those inhabited by researchers. To achieve this, the Cloud Chamber enlisted the expertise of Notre Dame University's LOBUND laboratory and that of its founder and director, James A. Reyniers. Though a microbiologist by training, Reyniers was mechanically skilled, reflecting what Fitzgerald describes as a background akin to the 'shop culture' of 19th century engineering. Using these skills, Reyniers developed an active research program beginning in the 1930s of building sterilized "germ-free" spaces, and of breeding populations of "germ free" research animals inside them with an elaborate system of caesarian sections, germicides, and air filtration. Though Reyniers' program did not directly address the problems in bacterial physiology capturing the attention of mainstream microbiology at the time, it did reflect a particular theoretical vision of microbiology as requiring simplified and standardized experimental animals (who Reyniers compared to standardized reagents in chemistry). Half engineering and half microbiology, this program was continued throughout the rest of the century by Reyniers and others, producing "germ-free,"

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[460] Gerard James Fitzgerald, "From Prevention to Infection: Intramural Aerobiology, Biomedical Technology, and the Origins of Biological Warfare Research in the United States, 1910-1955," PhD diss, Carnegie Mellon University, 2003, p 190. On "reverse salients" in the development of large sociotechnical systems, see Thomas Hughes, *Networks of Power: Electrification in Western Society, 1880-1930,* Baltimore: Johns Hopkins University Press, 1983.

"gnotobiotic" (carrying specific 'known' bacterial populations), and "specific pathogen free" research animals.[461]

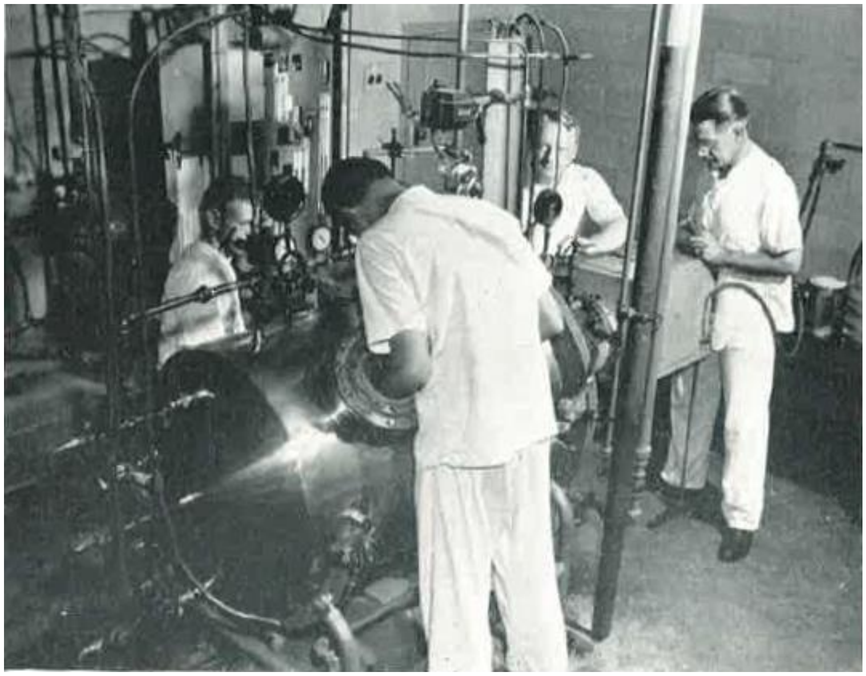

Figure 7: The Cloud Chamber in Operation[462]

Perhaps unsurprisingly, the art of keeping microbes out of a space was highly applicable to that of keeping them confined within a space. With the expertise of Reyniers, and the equipment manufacturing firm he ran to supply his laboratory, the Cloud Chamber was developed. This was a stainless-steel cylinder, a few cubic feet in volume, with removable gastight windows and glove ports, inputs and filtered outputs for air, water, and gas, and a door which could be hermetically sealed. The chamber was structurally quite strong, able to be pressurized overnight to detect any leaks (which the

[461] See Robert G. Kirk, "'Standardization through Mechanization:' Germ-Free Life and the Engineering of the Ideal Laboratory Animal," *Technology and Culture* 53 no 1 (2012), pp 61-93; Robert G. Kirk, "'Life in a Germ-Free World:' Isolating Life from the Laboratory Animal to the Bubble Boy," *Bulletin of the History of Medicine* 86 no 2 (2012), pp 237-75. Gnotobiology is also discussed in the larger context of developing ideas about human-microbe relations in Funke Iyabo Sangodeyi, "The Making of the Microbial Body, 1900s-2012," PhD diss, Harvard University, 2014.

[462] Rosebury, *Experimental Air-Borne Infection*, p 34

use of formaldehyde-laced steam to sterilize the interior was prone to creating).[463] First developed in 1944, the Chamber was used as an experimental space for researching the properties and infectivity of aerosols, and the efficacy of various devices intended to generate them, for the rest of the war. Fitzgerald argues that this technological solution closed the Hughesian salient of safety in Detrick laboratory research. This is an attractive argument, and there is some reason to believe that safety was a motivating factor in developing the Chamber. However, Fitzgerald overreaches by identifying the chamber as the technological solution to laboratory safety across all of Detrick. It was a single device used in a single (albeit obviously high-risk) project, and Reyniers developed the Chamber in conjunction with Theodor Rosebury's research team, just one of many at Detrick, and not for the Safety Division. It was not used for individual protection in other research at Detrick, which seems to have relied instead on the classic microbiological practice of skilled laboratory procedure, nor was it adopted by the wartime Safety Division. It is important to remember that "safety" is a constructed concept, requiring that a risk be recognized and regarded as unacceptable within an organizational moral economy.[464] It was not until after the war, when the Wedum group began to articulate its aerobiological model of laboratory infection and ideal of containing microbes to the laboratory bench, that operating without containment technologies came to be identified as *un*safe outside of specialized aerobiological research. To adopt Fitzgerald's Hughesian language, the

[463] Ibid, pp 19-20.

[464] Sims, "Safe Science," adopts the analytical term "moral economy" to describe the social norms and expectations implicated in laboratory safety practices. This is an adaptation of Robert Kohler's use of the term to describe the role of such norms and expectations in laboratory knowledge-making. See Robert Kohler, *Lords of the Fly: Drosophila Genetics and the Experimental Life*, Chicago: University of Chicago Press, 1994. Kohler, in turn, introduced the idea to the history of science from the work of social historian E. P. Thompson on the norms and expectations attached to literal economic transactions. E. P. Thompson, "The Moral Economy of the English Crowd in the Eighteenth Century," *Past & Present* 50 no 1 (1971), pp 76-136.

postwar Wedum group saw the sociotechnological salient of safety at Detrick very differently than the wartime Safety Division (or indeed, most Detrick research groups) had. For these wartime groups, the salient of safety was neither particularly novel compared to that experienced in civilian laboratories, nor in particular need of a technological solution.

Even the Rosebury team had strong motivations for developing the Cloud Chamber besides safety. By working with Reyniers, they deliberately eschewed using an already extant British instrument, the Henderson apparatus. This device, like the Cloud Chamber, could deliver controlled aerosol 'doses' to experimental animals, but did so through a mask fitted on a restrained animal rather than placing the animal in the sealed space of the Chamber. The British housed the Henderson apparatus on an open laboratory bench, with laboratory workers wearing protective masks.[465] In contrast, the Chamber provided a sealed space in which to investigate aerosols, protecting those working with it without the need to wear protective equipment, but there were also scientific distinctions between the two devices. While the Henderson apparatus could give fairly precise data about the 'dose' delivered to experimental animals, the Reyniers chamber was more versatile, functioning more like a miniature field test by allowing researchers to investigate aerosols in varying atmospheric characteristics like humidity and over different times, at the cost of reduced certitude about inhaled 'doses.' The Rosebury team suspected that even this precision of the British device was a questionable benefit, as the

[465] The device was described after the war in David W. Henderson, "An Apparatus for the Study of Airborne Infection," *Journal of Hygiene* 50 no 1 (1952), pp 53-68. The use of protective masks to operate the original tabletop version of the apparatus is described by a joint Detrick-Public Health Service training film on a late 1950s version that removed this flaw. See U.S. Department of the Army Chemical Corps, "Laboratory Methods for Airborne Infection Part 2: The Henderson Apparatus," U.S. Public Health Service, 1959. A copy of this film is held at the National Library of Medicine.

Henderson apparatus' close confinement of experimental animals may well have exaggerated the infectivity of aerosols by prompting the stressed animals to breathe deeply and rapidly.[466] By not confining animals as closely, the Reyniers chamber reduced their stress, potentially producing more useful infectivity data. The broad point, in any case, is that there were good scientific reasons to develop the Chamber. Safety was just one of many considerations behind the American team's decision to spend extra time and money doing so rather than using the already extant British technology.

One of the problems exacerbated by using Reyniers chamber instead of the Henderson apparatus was cross-infection among experimental animals and between them and humans. Because the whole bodies of animals like mice and guinea pigs were exposed to infectious aerosols in the chambers (in contrast to the Henderson apparatus, which largely exposed just the animals' heads to these aerosols), their fur could harbor large numbers of pathogens, carrying them and their threat of infection outside of the chamber into laboratories and animal care rooms. This was both a safety problem, compounding the dangers already inherent in handling infected animals with their teeth and potentially poor dispositions, and an epistemic one, threatening the integrity of data from different experiments when animals housed in close proximity infected one another. To solve both problems, Rosebury's Cloud Chamber group developed a system of ventilated animal cages, with features like removable bottoms to facilitate sterilizing the cages between uses.[467] They also used sealed bags to transport animals from experimental spaces to this housing, in principle ensuring that the spaces around experimental animals

[466] Rosebury, *Experimental Air-Borne Infection*, pp 208-209.
[467] Ibid, p 20.

would be contained throughout their post-experimental lives. This system of animal containment, which was further elaborated with airtight cages and transport cages which could be attached to modular safety cabinet systems in the 1950s, represented an attempt to simplify and control the ecological relationships of the laboratory which would be further articulated after the Second World War in the individualized containment ideal.[468]

Figure 8: Transporting Experimental Animals with a Disinfectant-Soaked Bag, WWII[469]

Experimental animals were a ubiquitous but often-invisible part of these ecological relationships, with their behavior and their bodies presenting microbes with a potential vector into the bodies of animals and other humans. Following the war, the

[468] G. B. Phillips and J. V. Jemski, "Biological Safety in the Animal Laboratory," *Laboratory Animal Care* 13 no 1 (1963), pp 13-20. The epistemic problem of cross-infection would continue to bedevil Detrick researchers after the war, increasing the Safety Division's imperative to flatten the laboratory ecology. See e.g. G. Briggs Phillips, Grover C. Broadwater, Morton Reitman and Robert L. Alg, "Cross Infections Among *Brucella* Infected Guinea Pigs," *Journal of Infectious Diseases* 99 no 1 (1956), pp 56-59.
[469] Photo from Rosebury, *Experimental Airborne Infection*, p 50.

Safety Division, under its new director Arnold Wedum, also identified the air itself as another vector for microbes with their experiments on aerosol generation by common laboratory operations. This aerobiological model of laboratory was a fundamental reconceptualization of the ecological relationships in a bacteriology laboratory. Microbes could enter the bodies of researchers not only through accidentally aspirated cultures, animal bites, or poorly washed hands, but also through the medium of small airborne droplets, lofted by any number of seemingly mundane laboratory operations. Researcher skill alone, though sometimes capable of reducing aerosols, could not be depended on to avoid laboratory infection in this new vision of laboratory ecology.[470] As Wedum and Morton Reitman put it in 1956, "practically every manipulation in the microbiological laboratory creates aerosols, and these aerosols are probably the source for many laboratory infections."[471] While seemingly all-pervasive, this danger of infection was nonetheless also a source of optimism for the Wedum group. If the laboratory bench, and everything on it, was a kind of inadvertent aerobiological testing site, then so too could this site be controlled by confinement technology like that of the cloud chamber. Much like contemporaneous visions of the eradication of infectious diseases, the Wedum group asserted that with complete technological control over the flow of air, *all* laboratory infection could in principle be averted.[472] The problem was to achieve this control.

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[470] There is an analogy here between the threat of disease accompanying the built space of the laboratory, and the "inescapable ecologies" of natural spaces entangling colonial settlers. See Linda Nash, *Inescapable Ecologies: A History of Environment, Disease, and Knowledge*, Berkeley: University of California Press, 2006.

[471] Morton Reitman and A. G. Wedum, "Microbiological Safety," *Public Health Reports* 71 no 7 (1956), p 661.

[472] See Nancy Leys Stepan, *Eradication: Ridding the World of Diseases Forever?*, Ithaca: Cornell University Press, 2011. The historiography of mid-century disease eradication is dominated by the World Health Organization's successful smallpox eradication campaign (and the Cold War politics of selecting smallpox over the Americans' favored eradication target of malaria), with official histories and insiders' accounts superseded in recent years by more critical scholarship. See e.g. Randall M. Packard, *The Making*

The simplest measures revolved around modifying laboratory procedures and technologies that were most responsible for producing aerosols. Syringes, for instance, were identified as dangerous for several reasons, from the aerosol produced when withdrawing fluid from a sealed bottle to the contamination of the plunger from within the syringe. In response, the safety program investigated and promulgated practices like surrounding the syringe needle with antiseptic-soaked cotton when withdrawing fluid, and "grinding down on the distal two-thirds of the plunger" when using a syringe.[473] They also developed a syringe with a rubber hood over its needle, which would presumably catch any aerosols generated.[474] The fundamental laboratory practice of pipetting was another source of danger, and developing and incorporating various mechanical pipetting devices was a particular focus of Safety Division. In most microbiological research, pipetting was often done with a simple oral pipette, a glass tube upon which a skilled operator could suck to manipulate precise quantities of liquid. This practice of oral pipetting, however, posed an obvious hazard of accidentally sucking too hard and pulling the experimental liquid (often containing infectious organisms) into the operator's mouth. Such accidents were obvious and catastrophic enough to be recorded in published literature which the Safety Division often drew upon to bolster their claims that this practice was unacceptably dangerous, and indeed, this record was a steady one of

of a Tropical Disease: A Short History of Malaria, Baltimore: Johns Hopkins University Press, 2005; Marcos Cueto, *Cold War, Deadly Fevers: Malaria Eradication in Mexico, 1955-1975,* Baltimore: Johns Hopkins University Press, 2007; Erez Manela, "A Pox on Your Narrative: Writing Disease Control into Cold War History," *Diplomatic History* 34 no 2 (2010), pp 299-323; Bob H. Reinhardt, *The End of a Global Pox: America and the Eradication of Smallpox in the Cold War Era*, Chapel Hill: University of North Carolina Press, 2015; Sanjoy Bhattacharya and Carlos Eduardo D'Avila Pereira Campani, "Re-Assessing the Foundations: Worldwide Smallpox Eradication, 1957–67," *Medical History* 64 no 1 (2020), pp 71-93.

[473] Wedum, "Safety Program at Camp Detrick, 1944-1953," p 47.

[474] Ibid, p 93.

infection due to this accident in microbiology laboratories throughout the early 20th century.[475] Additionally, the Safety Division's aerobiological experiments found that drawing liquid into pipettes in this manner could generate aerosols, as could practices like blowing on the pipette to remove the last drop of liquid (necessary, in a pipette calibrated for having this drop removed, to deliver an accurate volume of liquid). Seeking to remedy these concerns, the Safety Division promulgated several designs of mechanical pipettors in the 1940s and 1950s, and instituted safety regulations banning oral pipetting outright in 1950, though they continued to have difficulties in fully stopping this practice into the 1960s.[476] In addition to these technological modifications, they also sought to regulate the use of pipettors, exhorting researchers to refrain from practices like blowing out the last drop of liquid. Modifications of sanctioned laboratory practice like this were enshrined in revised safety regulations, and by required safety training for Detrick personnel. The attention to individual as well as community protection represented by these technological modifications and especially this regulation of laboratory practice represented an unprecedented extension of the Safety Division's control over individual Detrick researchers.

This extension of control over humans was ultimately intended to achieve more perfect control over microbes, by denying them the air as a route to infect human bodies.

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[475] See e.g. D. Riesman, "Two Cases of Diphtheria, One from Laboratory Infection, and One in an Infant Eleven Days Old," *The Philadelphia Medical Journal* 1 no 10 (1898), pp 422-424; Robb Spalding Spray, "Diphtheria: A Case of Laboratory Infection," *Journal of the American Medical Association* 89 no 2 (1927), p 112. For a retrospective discussion of this early literature, see also Arnold G. Wedum, "History & Epidemiology of Laboratory-Acquired Infections (In Relation to the Cancer Research Program)," *Journal of the American Biological Safety Association* 2 no 1 (1997), pp 12-29.

[476] G. B. Phillips, "Hazards of Mouth Pipetting," *American Journal of Medical Technology* 32 no 2 (1966), pp 127-129; Arnold G. Wedum, "Pipetting Hazards in the Special Virus Cancer Program." *Safety Bulletin* 5 no 2 (1958), p 1 discusses recent accidents caused by researchers violating this policy.

Under the aerobiologically influenced vision of laboratory ecology, however, modifying aerosol-generating technologies and techniques could only minimize the availability of this route, not eliminate it. To achieve such complete control over microbes, similarly complete control over the space around the laboratory bench to that provided by the Reyniers chamber was necessary. This was this logic that underlay the first Detrick safety cabinets. Designed by the Wedum group in 1948 in collaboration with hospital equipment manufacturer S. Blickman, Inc, this first "Blickman" cabinet was stainless steel, with windows and removable glove ports, and a ventilation system which ensured that laminar airflow- a steady flow of air in one direction without the turbulence introduced by cross-breezes- constantly sucked the contents of the cabinet's air away from its user and up into an air incinerator or later, filter. Lighter in construction than the cloud chamber, this cabinet was larger, intended to encompass an entire laboratory bench and the operations therein rather than serving a space for a single type of experiment like the chambers. It could be operated with attached gloves to provide a space that was in principle hermetically sealed (though without the structural strength of the cloud chamber the method of testing for leaks by pressurizing it could not be used), but these 'Blickmans' were originally more commonly used with the gloveports removed, relying on the directional flow of air to protect the user much like a chemist's fume hood.[477]

Prompted by this aerobiological vision of laboratory hazards and supplied with expanding budgets in the wake of the 1947 Baldwin Report and the Korean War, the Safety Division developed a heterogeneous array of cabinets of various sizes, designs, and functionality over the next five years. By 1953, a total of 62 safety cabinets were in

[477] Wedum, "Safety Program at Camp Detrick, 1944-1953," pp 39, 46.

operation at Detrick, and the Safety Division had developed a 3-group classification system for the array of designs.[478] This system was based on two elements: the testing to which these cabinets were subjected, and the use to which they were put. Five distinct types were grouped together as Class I cabinets, which relied on inward airflow to hopefully render any leaks irrelevant. Class II cabinets, in contrast, were pressure-tested like the old cloud chambers, and like them were constructed from commensurately stronger materials. Finally, Class III cabinets were Freon-tight, incorporating the recently developed gas Freon and halogen detectors as a simplified and highly accurate device to detect leaks, allowing these cabinets to be built more lightly than older Class II designs with equivalent reliability. Developed as a refrigerant in the 1930s by a collaboration of General Motors and DuPont Chemical, Freon was a cheap and apparently nontoxic gas (though it would subsequently be implicated in the erosion of the Earth's ozone layer in subsequent decades). Freon's principal value for Detrick lay in the extremely low concentrations of it that could be picked up by commercially available tools. GE's halogen detector, intended to detect small leaks in refrigeration systems, could equally well detect pinpoint flaws in an airtight cabinet filled with the gas. Freon testing swiftly became the center of Detrick maintenance routines, and would remain a gold standard for claiming the reliability of a germ-tight system in the network of biosafety expertise centered on Detrick into the 1960s, with the question 'is an object or procedure safe' coming to be tacitly identified with the question 'is a sealed space Freon-tight'?[479] The division between Class I and Class III cabinets initially rested on this test, with the same

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[478] Ibid, p 99.
[479] G. G. Gremillion, "The Use of Bacteria-Tight Cabinets in the Infectious Disease Laboratory," in *Proceedings of the Second Symposium on Gnotobiotic Technology*, Notre Dame, IN: Notre Dame University Press, 1959, pp 171-182, 173-174.

"Blickmans" potentially serving as laminar airflow Class I cabinets with their gloveports removed and Class III cabinets with gloves attached and airtightness verified with Freon testing.[480] Cabinets were later purpose-built to fill one "class" or another, reflecting the difficulty of achieving such a tight seal with removable parts.

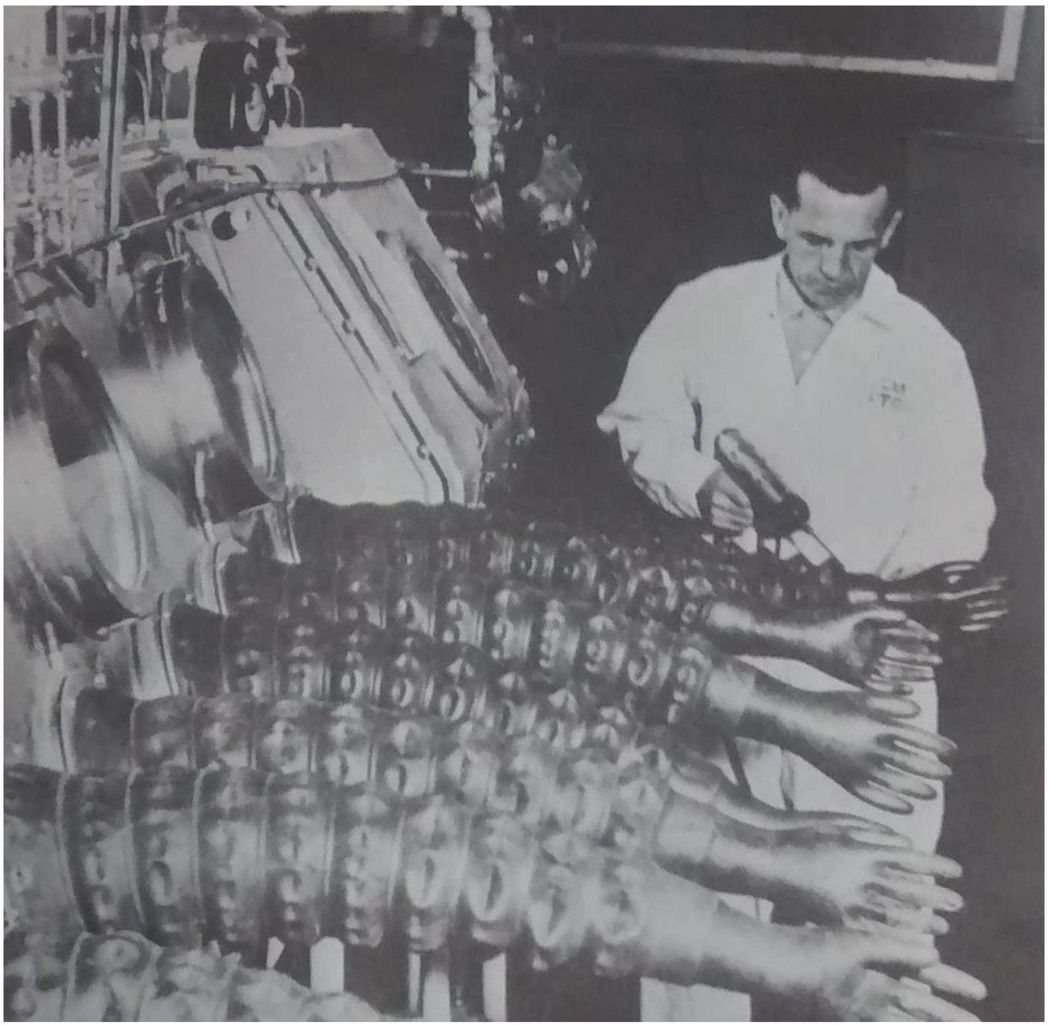

Figure 9: Using a Halogen Detector to Check a Class III Cabinet for Leaks[481]

[480] Wedum, "Safety Program at Camp Detrick, 1944-1953," p 4, mentions the same cabinets "used as" Class I or III depending on circumstances

[481] Photo from Richard M. Clendenin, *Science and Technology at Fort Detrick: 1943-1968*, Frederick, MD: Fort Detrick Technical Information Division, 1968, p 41.

Laboratory activities, in turn, were regimented by the class of cabinet in which they should take place. Class I cabinets were to be used for normal laboratory activities like pipetting and autopsying infected animals, while experiments with aerosols were confined to Class II and III cabinets, as in their Second World War cloud chamber progenitors.[482] Class III cabinets in 1953 tended to be newer, representing the design ideals that the Safety Division had developed over the past decade, and relying on Freon testing for maintenance, but given the up-front expense of any cabinet, this type only slowly superseded the patchwork of older designs. In 1953, Class III cabinets had only been installed in specific buildings conducting the most dangerous of aerosol work: Aerobiology, the Pilot Plant, and Special Operations. As the 1953 Safety Division judged that "this is the only type which will contain a highly persistent agent," it is probable that Detrick's work with *B. anthracis*, revived in priority during the Korean War, was reserved for these cabinets.[483] Indeed, following this increase in laboratory and pilot plant work with anthrax, the Safety Division concluded that "the implication of this situation is that there must be an increase in the amount of Class III safety equipment" used for such research,[484] This judgment highlights the continuously tightening construction of "safe" cabinets at Detrick, as Class II cabinets, most closely resembling the WWII cloud chambers in which extensive anthrax aerosol work had been done, were now not acceptable sites for this work. It is also unclear whether *all* "persistent agent" work was confined to Class III cabinets. If it was, this would represent an early break from the system of classification by task to classification by organism.

[482] Wedum, "Safety Program at Camp Detrick, 1944-1953," p 97.
[483] Ibid, p 98.
[484] Wedum, "Safety Program at Camp Detrick, 1944-1953," p xiii.

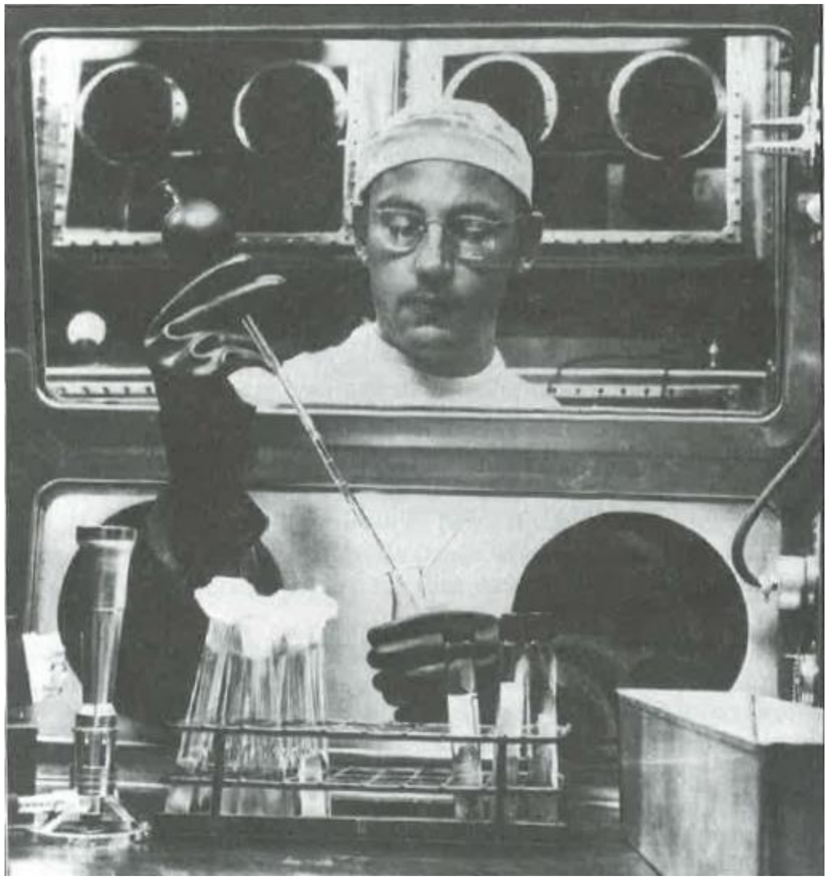

Figure 10: A "Blickman" cabinet in operation, n.d.[485]

Whether this was the case in 1953 or not, it had become so a decade later. By the early 1960s, the cabinet classification system had shifted, with particular species of microbes being reserved for particular cabinets, regardless of the work done with them. All research, aerobiological and otherwise, with particularly infectious pathogens like brucellosis and glanders was by regulation confined to hermetically sealed Class III cabinets, as was aerobiological research with most other organisms.[486] As budgets for safety equipment had increased in the early 1950s, and again in the 1960s, the Wedum group continued to collaborate with Blickman to produce a plethora of cabinet designs

[485] Photo from Robert L. Mole and Dale M. Mole, *For God and Country: Operation Whitecoat: 1954-1973*, New York: TEACH Services, 1998, p 82.
[486] See chart in G. Briggs Phillips, "Control of Microbiological Hazards in the Laboratory (Technical Manuscript 148)," Ft. Detrick, Frederick, MD: US Army Biological Laboratories, 1964, p 23.

for various tasks over the next two decades. The highlight of this collaboration, dating back to the early 1950s, was a modular system for these sealed Class III cabinets, ideally intended to serve as a completely contained laboratory bench. Groups of cabinets could be reconfigured for different laboratory operations with this system, allowing experimental animals, cultures, and other materials to pass from cabinet to cabinet without ever being exposed to the rest of the laboratory (see Figure 11, below). All other non-aerobiological research with less-infectious organisms was ideally confined to a laminar flow Class I cabinet. The "Class II" designation fell out of use as WWII-era Reyniers chambers were replaced by the newer Class III Blickman cabinets, though this expensive process proceeded more slowly than the Safety Division's regulations would imply (for instance, the Reyniers chambers installed in the newly constructed Aerobiology building, Building 376, in the early 1950s were not replaced with Blickman cabinets until 1963).[487] Nonetheless, at least on paper, the containment ideal was bifurcated by organism by the beginning of the 1960s, which in turn was built into the technology of the increasingly-ubiquitous "Blickmans."[488] "The impact of tuberculosis or chronic brucellosis upon the life of a young man or woman," Wedum noted in 1964, "has made me an uncompromising advocate for installation of a protective ventilated cabinet

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[487] Richard H. Kruse and Manuel S. Barbeito, "A History of the American Biological Safety Association Part II: Safety Conferences 1966–1977," *Journal of the American Biological Safety Association* 2 no 4 (1997), pp 10-25, 11. Building 376 was constructed in 1953 to replace the WWII-era Aerobiology Building seen in Figure 6. It is unclear whether the Reyniers chambers installed at that time were new or taken from the old building, but in either case, the fact that they, rather than new Blickman Class III cabinets, were installed in a new building highlights how the material culture of Detrick could lag behind the ideals promulgated by the Wedum group. For a brief history of the building, see U.S. Army Corps of Engineers St. Louis District, "Archives Search Report Operational History for Potential Environmental Releases Fort Detrick," June 16, 2014, pp 57-58.

[488] By the 1960s, the "Class II" designation was increasingly applied to commercial cabinets similar to Class I cabinets which filtered intake air as well as outgoing air. This was intended to achieve 'product protection' for growing areas of pharmaceutical and tissue culture research requiring sterile conditions. See Richard H. Kruse, William H. Puckett, and John H. Richardson, "Biological Safety Cabinetry," *Clinical Microbiology Reviews* 4 no 2 (1991), pp 207-241.

for routine work with their agents."[489] Complete segregation between species at all points of their laboratory life was increasingly the prescription to achieve full control over the ecological relationships of the laboratory.

Figure 11: A Blickman Modular System in Operation, late 1950s[490]

As continuing laboratory infections attested, however, such full control always eluded the Safety Division. Laboratory infections grew to their highest levels after WWII during the Korean War, despite the proliferation of safety cabinets in the early 1950s (there were a total of 62 installed by 1953).[491] Detrick suffered its first death (from

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[489] Arnold Wedum, "Airborne Infection in the Laboratory," *American Journal of Public Health* 54 no 10 (1964), p 1669.
[490] Photo from Gremillion, "The Use of Bacteria-Tight Cabinets in the Infectious Disease Laboratory," p 177.
[491] The figure of 62 cabinets is given in Wedum, "Safety Program at Camp Detrick, 1944-1953," p 99.

pulmonary anthrax) in 1951.[492] The infection rate remained steady throughout the 1950s, despite the Wedum group's efforts (see Figure 12, below). Yet another fatality from pulmonary anthrax in 1958 highlighted the continuing gulf between the ideal of eliminating laboratory infection completely and the reality at Detrick, and another (non-fatal) 1959 case of plague attracted the political embarrassment that Safety Division leaders had feared since the Second World War.[493] As Wedum lamented early in 1960, "the number of occupational infections at Fort Detrick has not shown a decrease during the last 6 years," and while "the number of illnesses in themselves are not a cause for alarm… these infections do show that we are not reaching our safety objectives."[494] This was particularly concerning to Wedum because work at Detrick, by definition, sought to make the agent they worked with still more dangerous. "An increase in infectivity, stability and virulence in certain agents would," he cautioned, "under present rates of infection, cause a curtailment or stoppage of certain projects."[495] It was not until the 1960s, when enough Class III cabinets had been procured from limited research budgets to enact full segregation between microbes and microbiologists that infection rates began to drop. Even then, reduced infection rates still did not meet the ideal of total eradicating such infections, and the continuous dangers of work at Detrick were highlighted in 1964 by yet another death, from viral encephalitis.[496]

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[492] US Department of the Army, *U.S. Army Activity in the US Biological Warfare Programs, Volume 2*, Washington, DC: US Department of the Army, 1977, p G-3.

[493] Detrick complied with legal requirements to report cases of plague to the WHO, but delayed doing so in this case to avoid attracting attention while Soviet Premier Nikita Khrushchev was visiting the United States. This case subsequently attracted criticism from Wisconsin congressman Robert Kastenmeier, who was emerging as a consistent critic of the Chemical Corps at the time. See Seymour Hersh, *Chemical and Biological Warfare: America's Hidden Arsenal*, Indianapolis: Bobbs-Merrill, 1968, p 130.

[494] *Safety Bulletin* 7 no 1 (1960), p 23.

[495] Ibid.

[496] US Department of the Army, *U.S. Army Activity in the US Biological Warfare Programs, Volume 2*, Washington, DC: US Department of the Army, 1977, p G-3.

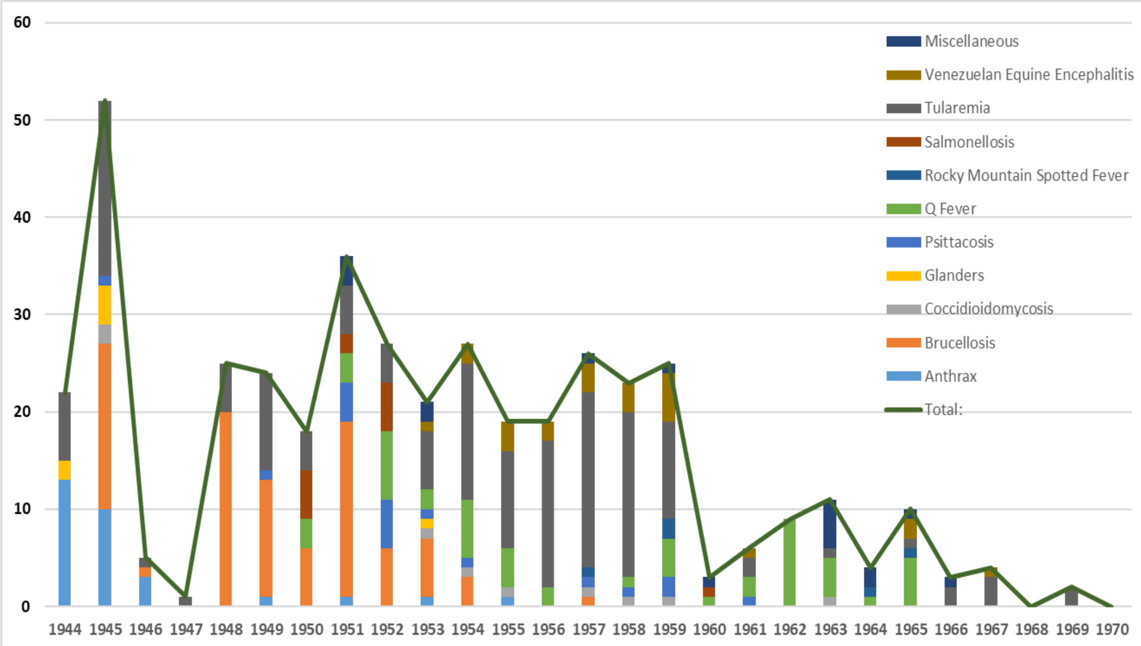

Figure 12: Detrick Laboratory Infections, 1944-1970[497]

Given the complexity of the laboratory, with microbes, experimental animals, humans, heterogeneous laboratory procedures and equipment, and the invisible flow of air all coexisting in the same space, it is perhaps unsurprising that 'normal accidents,' to borrow Charles Perrow's phrase, prevailed in this space.[498] Like the nuclear power plants studied by Perrow, the laboratory was a complex sociotechnological system where any single point of failure could produce an accident, a fact accentuated by the fact that microbes and experimental animals are living entities, able to act, change, and grow on their own. Furthermore, microbes are even harder for humans to routinely detect than ionizing radiation, requiring careful sampling, culturing, and time for humans to make their presence legible. It is no wonder, then, that the flattened laboratory ecology the Wedum group sought to construct could never be reliably achieved, particularly in light

[497] Data from *Occupational Laboratory Infections at Fort Detrick, 1943-1970,* held in ASM Archives.
[498] Perrow, *Normal Accidents*.

of the fact that they sought to *regulate*, rather than eliminate, pathogenic microbes in close proximity to their potential hosts.

Organization

These problems with establishing and maintaining this flattened laboratory ecology entailed more than just adding containment technologies to the laboratory. These technologies themselves had to be used and maintained properly to be efficacious in controlling what spaces microbes occupied, and had to be developed and deployed in unexpected places where the containment ideal was failing. To achieve this end, the Safety Division sought to establish an accident panopticon, to identify even mild laboratory infections and their source within workers' actions and the technologies they used. The Safety Division in turn sought to discipline workers' actions (and even their bodies) in the name of maintaining the containment ideal's strict separation between microbes and those bodies. Despite the technological emphasis of the containment ideal, disciplining humans through bureaucratic and administrative measures was often accorded more rhetorical importance than technologically disciplining microbes. As Arnold Wedum noted in 1961, "the control of laboratory airborne infection depends more on administrative and human factors than on the development of new procedures and equipment."[499] The Detrick safety *organization* was as essential a feature to the safety system as containment technologies, and like those technologies, was a novel development within microbiology laboratory practice of the time.

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[499] Arnold Wedum, "Control of Laboratory Airborne Infection," *Bacteriological Reviews* 25 no 3 (1961), pp 210-216.

Laboratory safety in earlier microbiological laboratories had been a paternalistic responsibility of laboratory directors, who exercised broad discretion in determining what people, practices, and research subjects were 'safe' or 'unsafe,' and of the skill of individual researchers. Individual microbiologists' skill in preventing contamination of their samples, an integral part of the craft of laboratory research, doubled as skill in avoiding infection. As veteran arbovirus researcher Edwin Lennette put it, if "a chemist… pour[s] things from [a] flask into [a] tube, and if they spill a little, they sort of stamp it out with their foot. You can't do that with bacteria and viruses… you have to handle them circumspectly… a microbiologist is trained to handle everything aseptically, handle everything without contamination from the environment or pollution of it. His working procedure must be second nature… this you develop only over time, with continual practice."[500] Laboratory directors relied heavily upon their workers' individual skill when assessing the safety of a line of research. Thus, for instance, Karl Meyer of the George Hooper Foundation banned junior researchers from research on the exceptionally infectious disease psittacosis in the 1930s, reserving such research for those who had already been infected and skilled senior researchers like himself, while attributing brucellosis infections among junior researchers to "carelessness or poor technic."[501] As Detrick safety researcher G. Briggs Phillips noted in the early 1960s, this system of individualized laboratory fiefdoms was especially strong outside the United States. According to Phillips, European laboratory directors, who unlike their American

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[500] Edwin H. Lennette, "Edwin H. Lennette: Pioneer of Diagnostic Virology with the California Department of Public Health," transcript of an interview conducted in 1982, 1983, and 1986 by Sally Smith Hughes, Regional Oral History Office, The Bancroft Library, University of California, Berkeley, 1988, pp 85-86. Speaking in the 1980s, Lennette was lamenting the lack of classical bacteriological skill in a younger, biochemically-trained generation of molecular biology researchers.
[501] Lederer, *Subjected to Science*, p 130; Meyer and Eddie, "Laboratory Infections Due to *Brucella*," p 29.

colleagues typically held a medical degree, exercised wide discretion over safety and often even served as their employees' physician.[502] Even within the US, however, this style of individualized safety management continued into the 1960s, with laboratory directors typically acting as the major source for whatever infection statistics and safety regulations existed for a particular laboratory. Biologist Bernard Davis, who worked on tuberculosis in René Dubos' laboratory in the 1950s, recalled its rudimentary safety procedures unfavorably in comparison to the system being developed at Detrick, attributing this to a tradition of necessary-risk-taking and "a rather 'macho' attitude on the subject [of laboratory infection]" besides. Davis contracted tuberculosis in the course of this research.[503] Even within large institutions like the National Institute of Health in the late 1940s, directors of individual disease units (the basic constituents of NIH research) held individual and final authority over what procedures, personnel, and equipment their research would use (for instance, banning people who had not previously had Q fever from research in that unit).[504] Despite the availability of "many ingenious 'gadgets,'" the safety philosophy at the NIH also reflected the ethos of individualized researcher skill and responsibility, as "sooner or later these devices are abandoned in favor of *skilled hands*, *alert minds*, and *workers disciplined* to be ever on the lookout for infection being spread about the laboratory."[505] A similar system had prevailed at Detrick

[502] G. Briggs Phillips, "Microbiological Safety in U.S. and Foreign Laboratories (Technical Study 35)," Fort Detrick, Maryland: U.S. Army Biological Laboratories (1961), p 48.
[503] Bernard D. Davis, "Two Perspectives: On René Dubos, and On Antibiotic Actions," in Carol L. Moberg, Zanvil A. Cohn (eds), *Launching the Antibiotic Era: Personal Accounts of the Discovery and Use of the First Antibiotics*, New York: Rockefeller University Press, 1990, pp 69-84, 73-74.
[504] See "Measures to Minimize Laboratory Infections in the Laboratories of the Microbiological Institutes," in Public Health Service National Advisory Health Council meeting minutes, June 10, 1950, pp 8-15. The Q Fever ban is discussed on p 14. These minutes are held in Johns Hopkins University (JHU) Chesney Archives Collection LanA (Alexander Langmuir Papers), Box 1, Folder "CDC- Arrangements"
[505] Ibid p 11. Emphasis in original.

during WWII, where Dack's Safety Division focused principally on the community protection mission, leaving laboratory safety procedures largely up to individual laboratories.

The Detrick system of the 1950s, in contrast, was both literally and organizationally technocratic. Literally, of course, the Detrick containment ideal was predicated on technologies to confine microbes, the lynchpin of which was the safety cabinet. While individual worker skill was necessary to use these technologies effectively, it was not regarded as the *basis* of safety (rather, workers' deviations from sanctioned procedures were regarded as a threat, to be removed from the smooth operation of a safety system through monitoring and training). More broadly, however, the Detrick system vested both the authority to determine what technologies and procedures were "safe" or "unsafe," and the promulgation and enforcement of safety regulations in a professionalized group of experts rather than individual laboratory administrators.[506] These centralized regulations in turn were constantly evolving as Safety Division research and accident investigation identified laboratory activities as 'unsafe.' The Detrick Safety Division had much more in common with professionalized safety organizations within American industry than it did with other microbiology laboratories. Detrick safety officers consciously cultivated this similarity, adopting statistical techniques of professionalized industrial safety and using industry as peer to which they compared accident rates.[507] They also built explicit relationships with

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[506] Wedum, "Safety Program at Camp Detrick, 1944-1953," p 44.
[507] See e.g. Wedum's use of accident rates per million-man hours worked, an industrial safety standard, to favorably compare Detrick both to other microbiology laboratories and to other American industries in A. G. Wedum, "Policy, Responsibility, and Practice in Laboratory Safety," in *Proceedings of the Second Symposium on Gnotobiotic Technology*, Notre Dame, IN: Notre Dame University Press, 1959, pp 105-119;

academic safety expertise in universities like New York University, where the Safety Division's G. Briggs Phillips completed a dissertation focused on biosafety at Detrick in the mid-1960s.[508]

Despite this centralization of safety authority and proliferation of containment technologies, the Wedum group's stated hope that laboratory infections could be completely eliminated remained unreached throughout the 1950s. Two major problems vexed the Safety Division in this period: economic constraints on the use of cabinet technology, and resistance from the laboratory workers whose actions and bodies they sought to monitor and control. Using safety cabinets slowed research by constraining the actions of researchers, but beyond this using safety cabinets at all entailed substantial costs for fabrication, installation, and maintenance. Even the simplest commercially available ventilated cabinets were expensive (Phillips estimated that a typical lab would incur a cost of $1000-$3000, including installation, in 1961), and more elaborate cabinets like airtight Class III designs were more expensive still.[509] Detrick's cabinets, in turn, were often customized designs built in collaboration with contractors like Blickman, and thus likely cost more than commercially available (and thus standardized) airtight designs. Any cabinet would cost a laboratory a substantial portion of a scientist's annual salary.[510] With Detrick as a whole operating under a limited budget and with long-term budget prospects uncertain in the face of the mercurial fortunes of the "biological

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Arnold G. Wedum, "Disease Hazards in the Medical Research Laboratory," *American Association of Industrial Nurses Journal* 12 no 10 (1964), pp 21-23.

[508] G. Briggs Phillips, "Causal Factors in Microbiological Laboratory Accidents and Infections," PhD diss, New York University, 1965.

[509] Phillips, "Microbiological Safety in U.S. and Foreign Laboratories," p 227.

[510] The mean salary of an American microbiologist in 1958 was $6,936. See National Science Foundation, *American Scientific Manpower, 1956-1958: A Report of the National Register of Scientific and Technical Personnel (NSF 61-45)*, Washington, DC: US Government Printing Office, 1961, p 71.

weapons" idea at the Pentagon, even equipment that the Safety Division had come to view as essential could only be budgeted for in limited quantities. As Wedum put it in 1960, "one way of further reducing the number of infections is the universal use of Class III cabinets. However, this complex type of equipment is very expensive. It must be understood that its general use on this installation would have the undesirable effect of decreasing the funds available for research. In view of this, it is desirable to restrict use of Class III cabinets to selected situations."[511]

With the prophylactic measure of deploying Class III cabinets *en masse* unavailable to them, the Safety Division instead focused on identifying and remedying the specific causes of infections, which could sometimes be done more cheaply than confining operations in an entire cabinet system. This, like maintaining the cabinets, was easier said than done, given the invisibility of the microbes involved and the spaces they could fit through. As G. B. Phillips put it in 1965, "it should be emphasized that the evidence [of accidents] is often presumptive because of the technical difficulties in continuously monitoring the laboratory environment. To illustrate, there is no biological equivalent of the geiger [sic] counter or radiation film badge that can be used in routine surveillance to detect exposures to infectious agents."[512] In the absence of such direct tell-tale signs of flaws in the containment system (beyond laboratory infections themselves), the Wedum group had to deploy an array of experimental and bureaucratic proxies to gain as panopticonic a view as possible of the laboratory. This often entailed accident reconstructions using simulant organisms, but more basically, identifying "accidents"

[511] *Safety Bulletin*, 7 no 1 (1960), p 9.
[512] Phillips 1965 "Causal Factors in Microbiological Laboratory Accidents and Infections," p 79.

required an elaborate bureaucratic technology of accident reporting forms and formal investigative procedures.[513] As in the case of the state writ large, one detected breaches to the order of "safety" by 'seeing like a biosafety officer' through written reports of minor laboratory incidents and infections (including subclinical cases).[514] Also like the state, this surveillance of subjects was vulnerable to their recalcitrance. Accident reporting particularly vexed the Safety Division, which changed its procedures twice in the late 1950s before settling on an accident investigation regime which emphasized a norm that no retaliation would come to those involved. Reading between the lines, it seems that this was a reaction to an earlier "exhaustive investigation" regime, whose intrusive interrogation of laboratory spaces and personnel likely produced resistance from laboratory workers in the form of underreporting, unwillingness to volunteer information, or outright concealing mistakes.[515] Supervisors, who were responsible for submitting the majority of a plethora of forms reporting even the most minor of accidents, were a particular focus of Safety Division ire, with publications like the *Safety Bulletin* continually emphasizing the message that "the burden of responsibility for the safety of working personnel must rest on the shoulders of their immediate superiors."[516] When dealing with supervisors who wanted to accomplish research without wasting time, effort, and limited research budgets, it seems that Safety Division claims that following their dictates "will safeguard the health of… personnel and… put more money into the Research and Development effort" did not fall on particularly enthusiastic ears.[517] Indeed,

[513] Wedum, "Safety Program at Camp Detrick, 1944-1953," pp 44, 106.
[514] James D. Scott, *Seeing Like a State: How Certain Schemes to Improve the Human Condition Have Failed*, New Haven: Yale University Press, 1998.
[515] *Safety Bulletin* 7 no 2 (1960), p 2.
[516] *Detrick Safety Bulletin* 7 no 1 (1960), p 2. For a list of forms required by safety regulations in the late 1960s, see Ibid, p 8.
[517] Ibid, p 9.

only 49% of Detrick laboratory workers surveyed in 1959 reported that their "supervisor always encourages reporting minor accidents," and Wedum identified resistance by supervisors as a major challenge faced at Detrick in the same year.[518]

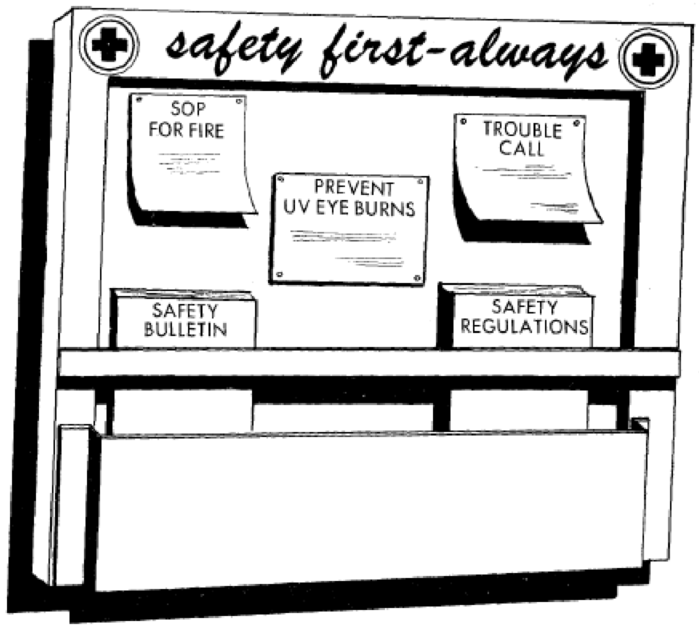

Figure 13: Safety Bulletin *Illustration of a Safety Bulletin Board*[519]

Also like the state, the Safety Division buttressed its actions with a universe of documents. The *Safety Bulletin*, principally targeted at an audience of laboratory supervisors, was distributed to each laboratory building at Detrick.[520] The *Bulletin* doubled as a kind of catalogue for new safety devices and procedures (often valorizing individuals for suggestions leading to these developments), and an exhortation for responsible managers to look to the Division's rationalized safety measures. The *Bulletin*

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[518] Phillips, "Causal Factors in Microbiological Laboratory Accidents and Infections," p 147; Wedum, "Policy, Responsibility, and Practice in Laboratory Safety," in *Proceedings of the Second Symposium on Gnotobiotic Technology*, pp 105-106.
[519] *Safety Bulletin* 6 no 1 (1959), p 8
[520] Wedum, "Safety Program at Camp Detrick, 1944-1953," p 43. Electronic copies of the *Safety Bulletin* from 1952-1963 are held in the National Agricultural Library Collection 359 (American Biological Safety Association) (NAL ABSA).

reiterated information that would have been available from more “official” sources like safety regulations (for instance, detailing who was responsible for what forms), suggesting that its Safety Division publishers thought that managers were not taking this information sufficiently to heart.[521] The safety regulations themselves were constantly being revised in the 1950s, and were part of a small library of safety documents that each laboratory was required to keep in a “Safety Bulletin Board”.[522] In turn, the Safety Division imitated the managerial model of industrial safety with a bevy of quantitative analyses and reports for Detrick’s leadership, emphasizing the rational basis for the Division’s constant claims for higher safety budgets and more stringent procedures.[523]

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[521] See Figure 14, below. This reiterated information is an invaluable source, as copies of the Detrick safety regulations of the 1950s are unavailable.

[522] Wedum, “Safety Program at Camp Detrick, 1944-1953,” p 43; *Safety Bulletin* 6 no 1 (1959), p 7. These documents included copies of base-wide and building-specific safety regulations, minutes from the latest Safety Council meeting, and yearly accident statistics (presumably *pour encourager les autres*).

[523] See e.g. Wedum, “Safety Program at Camp Detrick, 1944-1953.”

Forms to be Submitted in Event of an Accident, Injury, Illness or Accidental Exposure to Infectious or Toxic Agents

FORM	PURPOSE	BY	WHEN
FD 6-60	Minor Biological, Chemical & Mechanical Accident Report. (Or Agent Exposure) Not Lost Time	Supervisor, with help of employee	Within six (6) days, to Office of Safety Director.
DA 285 4 cys	Accident, Lost Time	Supervisor, with help of employee	Within 6 days, for lost-time injury, occupational illness, fire or property damage over $50. and for Army motor vehicle accidents. To Office of Safety Director.
CA 1* 2 cys	Employee's Notice of Injury of Occupational illness	Employee	Within 48 hours, for injury, illness, or significant exposure to agent. To supervisor, regardless of whether or not there is medical treatment or lost-time.
CA 2* 2 cys*	Official Superior's Report of Injury	Supervisor	Same as Form CA 1, if lost-time, or possible medical charge against the compensation fund. Also in case of recurrence of injury or illness.
CA 16 2 cys*	Request for Treatment of Injury Under the United States Employees' Compensation Act (When cause is known)	Supervisor	Within 48 hours after the accident in which medical services are required.
CA 17	Request for Treatment of Injury under the United States Employees' Compensation Act When Cause of Injury is in Doubt.	Supervisor	When cause of injury or illness is in doubt or in case of hernia.
CA 3 2 cys*	Report of Termination of Total or Partial Disability	Supervisor	Upon return to work after disability with sick leave, unless reported by Form CA 2. For clarification call Richard Graham, Civ. Personnel Ext. 3103.

* ALL CA FORMS WILL BE SENT TO REGISTRAR, U.S. ARMY HOSPITAL, FORT DETRICK, MD.

References - consult if slightest doubt arises, and for more detailed instructions:

1. Civilian Personnel Bulletin No. CI.4 — 27 August 1956
2. Post Memorandum No. 385-6 — 16 January 1959

Definition: "Lost-time" means inability to work on any subsequent duty day.

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Figure 14: The Safety Bulletin *reiterates who must fill out what forms, 1960*[524]

[524] *Safety Bulletin* 8 no 1 (1960), p 8

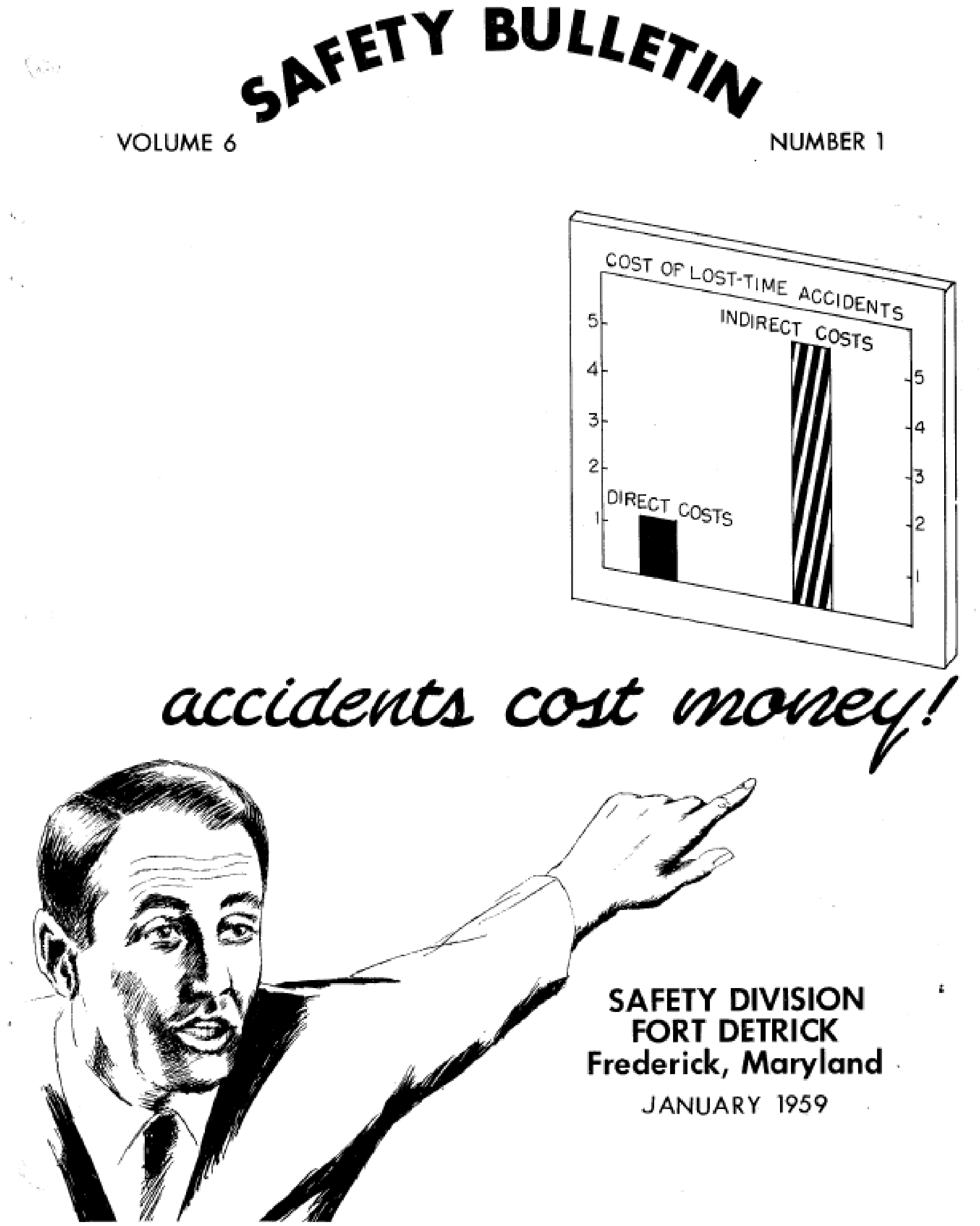

Figure 15: 1959 Safety Bulletin *cover. The message to managers is rather transparent*[525]

[525] *Safety Bulletin* 6 no 1 (1959), Cover Page.

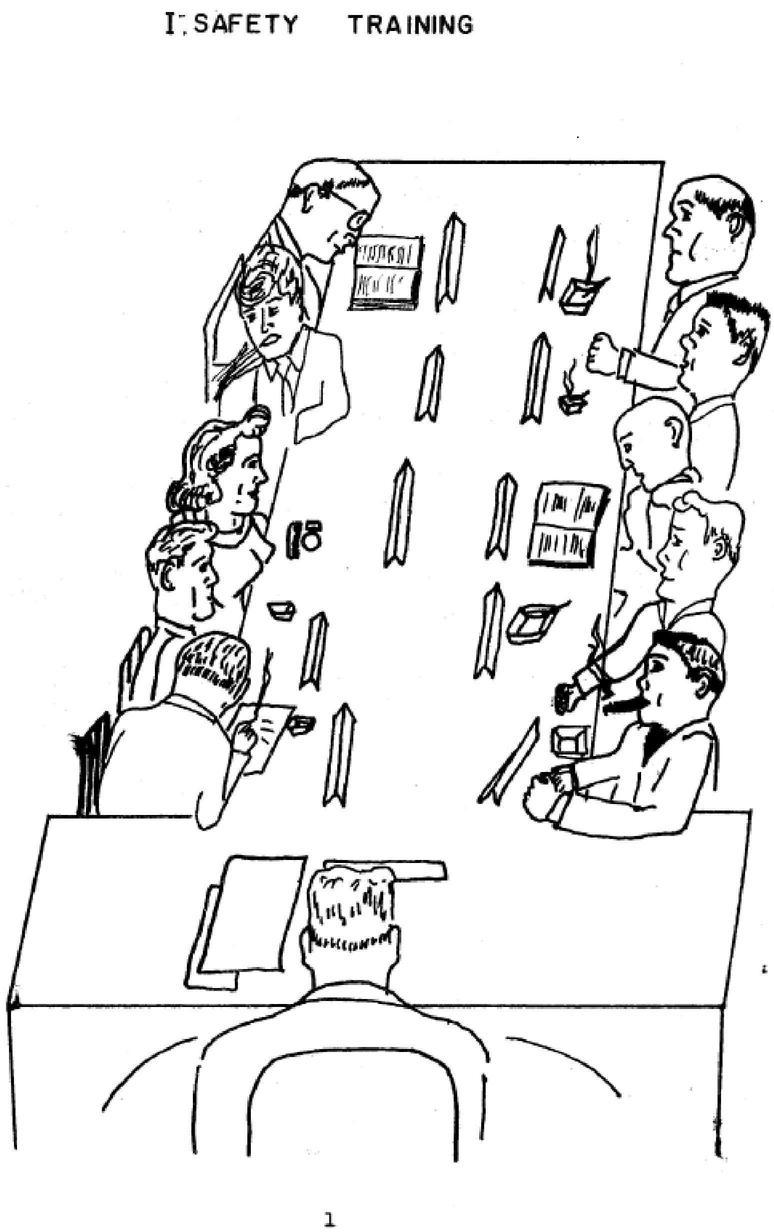

Figure 16: Safety Training Cartoon[526]

In addition to accident forms and investigations, the centralized Safety Division maintained its authority over practice in laboratories by deputizing safety officers in each laboratory at Detrick. These officers were responsible for distributing and enforcing documents like the safety regulations, and were expected to take part in monthly safety

[526] *Safety Bulletin* 7 no 1 (1960), p 1.

meetings where conditions in the laboratories and safety ideas were discussed. These meetings allowed the Safety Division to access 'on the ground' experience from the laboratories, but also provided a socialization function, inculcating the safety officers of each laboratory in the safety system. A similar function was performed by safety training sessions, films, and the like, which were organized and produced by the Safety Division often in conjunction with civilian institutions like George Washington University and the Center for Disease Control.[527] Like citizenship in the modern nation-state, participation in the Detrick-model laboratory as a safe researcher was a set of skills and attitudes which needed to be taught.[528]

Inculcating this new model of laboratory safety was, like the promulgation of new models of citizenship, easier said than done. While it may seem strange to compare skilled scientists and laboratory technicians to recalcitrant peasants, this skill was itself a potential source of resistance for novel Safety Division procedures. A good portion of this skill would necessarily have been tacit knowledge learned from the earliest days of a microbiologist's training. This would have included the reflexive use of some germicidal techniques, like passing a wire loop through an open flame to sterilize it before using it to introduce a culture to a petri dish, and of safety practices, like frequent hand washing, wearing masks and gloves, or taking care when orally aspirating a pipette.[529] Nonetheless, the Safety Division's assertion that a number of basic laboratory operations needed to be done differently, or with different tools in the case of pipettes, would have

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[527] Wedum, "Safety Program at Camp Detrick, 1944-1953," pp 38-45.

[528] Eugen Weber, *Peasants into Frenchmen: The Modernization of Rural France, 1870-1914,* Stanford: Stanford University Press, 1976; Benedict Anderson, *Imagined Communities: Reflections on the Origin and Spread of Nationalism,* London: Verso, 1983.

[529] See Lennette, "Pioneer of Diagnostic Virology with the California Department of Public Health,", pp 85-86 for a discussion of these second-nature practices.

directly challenged this hard-earned skill. In this sense, the Safety Division represented a threat to the professional autonomy of laboratory workers. For microbiologists new to Detrick, like the cycling population of postdoctoral researchers, the unprecedentedly large and active safety organization in their midst must have seemed stifling and bureaucratized, while even workers who had been at Detrick for a considerable part of their careers had experienced a continuous tightening of Safety Division control over their work through technologies, regulations, and increasingly stringent accident reporting standards.[530] This is not to say that researchers would have willfully wanted to do unsafe things, but "safe" and "unsafe" are socially constructed concepts. The moral economy of a laboratory, as Sims notes, can weigh heavily against actions and conditions regarded as unsafe.[531] However, it is not at all clear that the moral economy of laboratory research at Detrick kept pace with the Safety Division's ever-growing list of previously acceptable practices and technologies now deemed unacceptable, and indeed, in light of incidents like the continuing use of oral pipetting techniques into the 1960s, there is good reason to doubt that it did so.[532]

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[530] In the mid-1960s, Detrick researchers could effectively be bifurcated into two groups: those who had been at Detrick for over a decade and a half, having been hired during the Korean or Second World Wars, and those who had been a Detrick for a few years. George H. Nelson and Donald M. Hodge, "Biological Laboratories Communication (Fort Detrick Miscellaneous Publication 13)," Fort Detrick: United States Army Biological Laboratories Technical Information Division, 1965, p 16. See Chapter 2 (above) for more discussion of this source and the prevalence of researchers spending a few years working at Detrick.

[531] Sims, "Safe Science."

[532] Phillips, "Hazards of Mouth Pipetting"; Wedum, "Pipetting Hazards in the Special Virus Cancer Program"; *Safety Bulletin* 5 no 2 (1958), p 1. The continued prevalence of oral pipetting in civilian microbiology probably played a role in this continued flouting of the regulations at Detrick. See W. Emmett Barkley, "Mouth Pipetting: A Threat More Difficult to Eradicate than Small Pox," *Journal of the American Biological Safety Association* 2 no 2 (1997), pp 7-10, which discusses the reluctance of even microbiologists involved in the Asilomar conference (on the safety of recombinant DNA research) to abandon their oral pipettes.

Professional Networks

The Safety Division's authority over recalcitrant laboratory researchers and miserly managers was ultimately rooted in their claims to having superseded the tacit knowledge of ordinary microbiologists with their own expertise. The best way to buttress these clams was to situate them within pre-established communities of professional knowledge, and ultimately to establish a professionalized knowledge community of their own. The former goal can be seen in the Division's adoption of the language of industrial safety experts. For instance, by the mid-1950s Safety Division members commonly discussed Detrick's laboratory infections in terms of lost work time and accidents per worked man-hours that explicitly established Detrick (and the Safety Division) as a peer of similar safety organizations using the same metrics within American industry. The latter goal would consist of bringing this managerial practice and the containment-vision sociotechnological system to the broader civilian world of microbiology. The goals of the work done at Detrick were extraordinary, but the problems of infectious disease research faced there were not. Microbiologists studying the same organisms inside the biological weapons program and outside of it both ran the risk of laboratory infection, and civilian microbiologists faced infectious diseases not studied at Detrick, particularly tuberculosis. As the field of microbiology (measured in people, money, and scope of inquiry) expanded rapidly in the decades after the Second World War, novel entities like genetically modified microbes or hypothetical cancer viruses presented new dangers for researchers. Even as biological weapons research attracted increasing public criticism and was eventually abandoned by the American government in the 1960s, Wedum's community of safety expertise expanded contacts with civilian microbiologists and ultimately developed a broader community of biosafety experts working under the containment vision. These contacts were maintained and expanded despite the military secrecy at Detrick, but the Safety Division which had been started to contain Detrick's secrets was ultimately successful in breaching its own containment to reinforce that of laboratory microorganisms.

Interpersonal links between Detrick and the civilian world of microbiology, often dating from the Second World War, helped pave the way. Microbiology was a small and close-knit field during the early 1940s, and a significant number of microbiologists were connected to the wartime biological weapons program. After the war, most of these microbiologists demobilized and returned to civilian research, but the links they established between one another and with colleagues who remained at Detrick would remain active throughout their careers. As Sanford Elberg, a Berkeley microbiologist who served at Detrick during and shortly after WWII later reminisced, "my service… at Camp Detrick… was a key factor in my professional growth because most of the major figures who were to develop in the American Society of Microbiology were on duty there… it was a tremendous environment of professional skill and expertise, and there were conversations and seminars going all time, informally… Thus, I met and worked with many of the key people in American microbiology."[533] At least some of the researchers who left Detrick in the 1940s took early safety precautions, like containment-oriented laboratory architecture, to heart. Arbovirus researcher Edwin Lennette, for instance, was recruited for a brief stint at Detrick in the immediate aftermath of WWII

[533] Sanford S. Elberg, "Sanford S. Elberg: Graduate Education and Microbiology at the University of California, Berkeley, 1930-1989," Transcript of an interview conducted 1989 by Ann Lage. Oral History Center, The Bancroft Library, University of California, Berkeley, 1990, pp 27-28.

before joining the faculty at Beckley, leaving Detrick with a deep appreciation for the safety implications of the Detrick aerobiological model. Placed in charge of building a new virus laboratory to house his research at Berkeley in the early 1950s, Lennette adopted Detrick architectural styles like UV sterilized ventilation systems and airlocks, later noting that he served as the principal advocate of these safety measures in long battles with the building's skeptical architects, and at a significant increase in the cost of construction.[534] Microbiologists like Elberg and Lennette were thus particularly attuned by their experiences to heed further safety developments coming out of Detrick. Younger microbiologists, in turn, might be exposed to Detrick ideas at conferences like the annual meeting of the Society of American Bacteriologists, or the evangelism of visitors from Detrick like G. Briggs Phillips, who toured and lectured at dozens of laboratories in the US, Japan, and Western-bloc European countries in 1960-1961.[535]

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[534] Lennette, "Pioneer of Diagnostic Virology with the California Department of Public Health,", pp 74, 82-85.
[535] Phillips, "Microbiological Safety in U.S. and Foreign Laboratories."

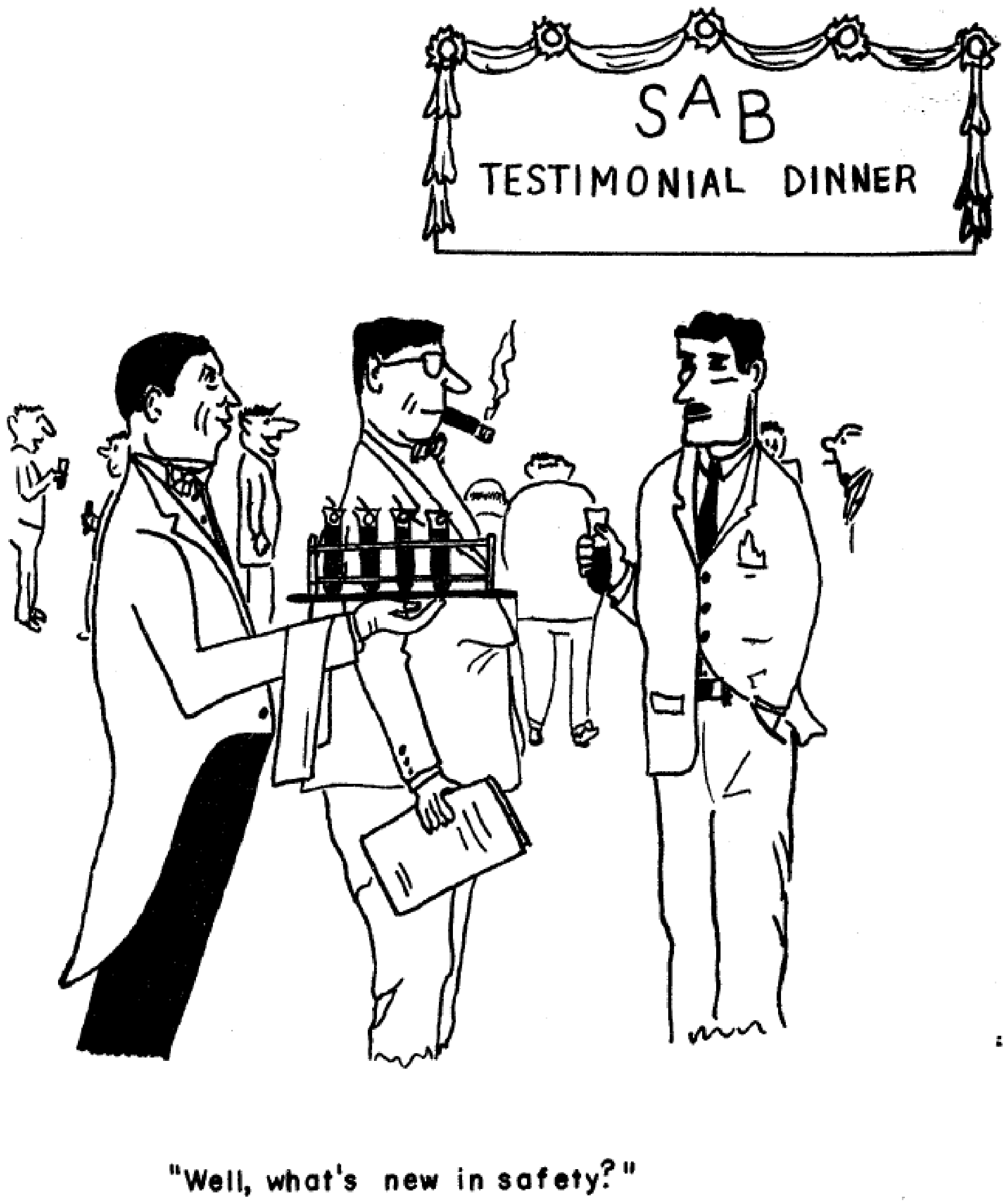

Figure 17: Detrick Safety Bulletin *Cartoon, 1960. "SAB" refers to the Society of American Bacteriologists*[536]

Beyond such interpersonal contacts, one major step in establishing laboratory safety as an area of professionalized expertise was to establish a tradition of published literature. Laboratory infection was a growing area of concern in microbiology when

[536] *Safety Bulletin,* Vol 7 no 1 (1960), p 5

Detrick was founded, particularly in the case of highly infectious microbes like psittacosis and Q fever which came under increased laboratory scrutiny in the 1930s leading up to the Second World War.[537] Indeed, the hypothesis that unexplained laboratory infections might be caused by microbial aerosols, suggested by infectious disease researcher Karl Meyer, helped direct Theodor Rosebury and Elvin Kabat's suggestions about what microbes would make good biological weapons in 1942.[538] In the post-war period, a small literature explicitly focused on the general phenomenon of laboratory infections and their mitigation (rather than infections by one particular pathogen) began to grow. Wedum's group directly contributed to this literature, publishing scientific papers and giving public conference presentations on their aerobiological experiments and safety procedures.[539] The group had the advantage of being an unprecedentedly large and professionalized organization among microbiologists interested in laboratory infections, but almost as importantly, they were based at an unprecedentedly large facility at Detrick. While they could simply publish about their experience protecting (and tacitly studying) hundreds of researchers, civilian microbiologists who were interested in laboratory infections and safety had far less of such data to draw upon in their own (much smaller) laboratories. This had been a major impediment to the systematic study of laboratory safety, and when researchers like Meyer began to publish on laboratory infections by a particular pathogen (in Meyer's case, *Brucella*), it was with surveys of other laboratory directors circulated within interpersonal

[537] See e.g. Burnet and Freeman, "Note on a Series of Laboratory Infections with the Rickettsia of 'Q' Fever"; McCoy, "Accidental Psittacosis Infection Among Personnel of the Hygienic Laboratory."
[538] Meyer and Eddie, "Laboratory Infections Due to *Brucella*"; Rosebury and Kabat "Bacterial Warfare."
[539] See e.g. Anderson, et al, "Potential Infectious Hazards of Common Bacteriological Techniques"; Gremillion, "The Use of Bacteria-Tight Cabinets in the Infectious Disease Laboratory," in *Proceedings of the Second Symposium on Gnotobiotic Technology*; Wedum, "Laboratory Safety in Research with Infectious Aerosols."

networks.[540] By the late 1940s, S. Edward Sulkin and Robert M. Pike of the University of Texas' Southwestern Medical College had emerged the major figures in the study of laboratory infection outside of Detrick, also relying on voluntary surveys of American laboratories to publish a series of papers.[541] With many laboratory directors unwilling to discuss the 'dirty laundry' of researchers' infections, or unable to supply systematic records, Wedum's group at Detrick served as a major source of data for the pair.[542] The Safety Division thus directly and indirectly contributed to the body of literature on laboratory infection and safety which began to grow in the 1950s and 1960s. Open publications would necessarily have to be cleared by Detrick's declassification officer, but the Wedum group was evidently adept at negotiating this process. As safety was a peripheral subject compared to more direct weapons research, the Wedum group was particularly successful at navigating the vicissitudes of military classification, taking advantage of periods of liberalized classification policy like the late 1940s, the late 1950s, and the mid-1960s.[543]

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540 Meyer and Eddie, "Laboratory Infections Due to *Brucella*."
541 S. Edward Sulkin and Robert M. Pike, "Viral Infections Contracted in the Laboratory," *New England Journal of Medicine* 241 no 5 (1949), pp 205-213; S. Edward Sulkin and Robert M. Pike, "Survey of Laboratory-Acquired Infections," *American Journal of Public Health* 41 no 7 (1951), pp 769-781; S. Edward Sulkin, "Laboratory-Acquired Infections," *Bacteriology Reviews* 25 no 3 (1961), pp 203-209; Robert M. Pike and S. Edward Sulkin, "Continuing Importance of Laboratory-Acquired Infections," *American Journal of Public Health* 55 no 2 (1965), pp 190-199; R. M. Pike, "Laboratory-Associated Infections: Incidence, Fatalities, Causes, and Prevention," *Annual Review of Microbiology* 33 (1979), pp 41-66. Their initial study was sponsored by the US Public Health Service. See Robert M. Pike and S. Edward Sulkin, "Occupational Hazards in Microbiology," *The Scientific Monthly* 75 no 4 (1952) p 223.
542 Sulkin and Pike, "Survey of Laboratory-Acquired Infections," pp 769-770 discusses the reluctance of a number of laboratories contacted to share accident information, and specifically thanks Wedum for his help.
543 Manuel S. Barbeito and Richard H. Kruse, "A History of the American Biological Safety Association Part I: The First Ten Biological Safety Conferences, 1955-1965," *Journal of the American Biological Safety Association* 2 no 3 (1997), pp 7-19, 14. By the 1950s, classification policy for biological warfare information revolved around keeping operational capabilities secret and avoiding the political embarrassment of open acknowledgement that the United States was developing biological weapons. A fair amount of scientific information, so long as it was stripped of its overt context within BW research, was relatively acceptable for release. See Chapter 4, below.

The Wedum group also navigated the strictures of military secrecy by establishing a series of annual conferences for laboratory safety specialists within the biological weapons program, and eventually opening them to the unclassified world. These conferences were started by Wedum in 1955 to tie together a biological weapons safety community which had become increasingly dispersed with the growth of the program. Dugway Proving Ground, a WWII-era Chemical Corps facility in Utah, had been reopened in the early 1950s for large-scale field tests of both chemical and biological weapons. With the surge of Army interest in biological weapons research during the Korean War, Dugway swiftly received a full staff and began conducting summer field trials of agents like *Br. suis,* and *B. anthracis* (grown in Detrick's Pilot Plant) in 1951. With infectious agents handled by the kilogram, field aerosol tests of these agents, and laboratory and animal care facilities to analyze these experiments, Dugway soon fostered a safety division comparable to Detrick's. These two sites were in turn joined by a Chemical Corps production facility in Pine Bluff, Arkansas, where beginning in 1953 a biological agent plant intended to produce standardized agents like *Br. suis* was opened.[544] Pine Bluff, too, faced considerable safety challenges (especially given the difficulties experienced at the Detrick Pilot Plant), and also developed a large safety staff, including some experts drawn from Detrick. The result of this expansion by the mid-1950s, then, was a BW program capable of field testing and producing the 'weapons' developed at Detrick, but also of a geographic diffusion of expertise (especially safety expertise) which had previously been concentrated at Detrick.

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[544] US Department of the Army, *U.S. Army Activity in the US Biological Warfare Programs, Volume 1*, Washington, DC: US Department of the Army, 1977, pp 3-2, 3-3, 4-1.

Faced with this geographic diffusion of the community of classified expertise he had developed at Detrick, Wedum paralleled the practices of open science by seeking to build a virtual one. Initially, the Detrick group built a network of correspondence and telephone conversations between the three facilities' safety staff, but soon turned to the established open science practice of holding conferences to solidify their virtual community. This was easier said than done, due to the site-based military secrecy regime under which they operated. Papers presented at such a conference attended by non-Detrick employees, for instance, would need to receive clearance from Detrick's security officer, even if the other attendees were from facilities working on the same body of classified information for the same organization within the same branch of the military. Even then, only those with "Secret"-level clearances or above would be allowed to attend. Nonetheless, despite this inconvenience, the Detrick group organized and hosted an informal conference with Dugway and Pine Bluff safety staff in April 1955, which was followed 6 months later by a conference at Pine Bluff, and by one at Dugway in July 1956.[545]

By the next year, word of these gatherings had percolated through the smaller but growing community of biosafety staff at USDA animal disease facilities, like the National Animal Disease Laboratory in Ames, Iowa. The now-annual conference's organizers began to incorporate unclassified sessions into their plans to allow members of this community to attend, and beginning in 1958, some meetings began to be held outside of Army facilities. Beginning in 1964, the conference's organizers took advantage of a recently liberalized classification policy to shift to an entirely unclassified program,

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[545] Barbeito and Kruse, "A History of the American Biological Safety Association Part I," pp 7-10.

allowing safety experts from other civilian Federal agencies (like the CDC and NIH), and from universities like Wisconsin to attend.[546] In the 1950s, these conferences were an excellent example of the 'parallel world' of classified scientific journals and conferences described by Peter Westwick, but by the next decade they had shifted to become the nucleus for an open-science community of practitioners in what was swiftly becoming a new field.[547] This shift was accelerated by the fall of the offensive BW research program in the late 1960s, with the 1974 meeting, for the first time, held outside of a Federal facility and funded by industry donors, not the government.[548]

The community of "biosafety" expertise that was crystalizing around these conferences was a growing one. With a canon of publications, a conference, and standardized technologies and the manufacturers to provide them (like the S. Blickman company), this area of expertise had an ample supply of resources to base a career upon by the late 1960s. Perhaps more importantly, this 'supply' was matched by an increasing demand from academic, industrial, and non-military governmental institutions that worked with microbes. The early 20th century model of heroic risk-acceptance and laboratory directors' idiosyncratic fiefdoms in microbiology was being superseded by larger grants, bureaucratized institutions, and increased sensitivity to worker's compensation claims.[549] Such institutionalized sensitivity to workplace risk was

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[546] Ibid, p 14.

[547] Peter Westwick, *The National Labs: Science in an American System, 1947-1974*, Cambridge, MA: Harvard University Press, 2003 studies the Atomic Energy Commission's National Laboratory system, including what Westwick describes as a 'parallel community' for physicists working under the strictures of secrecy. The differences between this case and that of the biological weapons program highlights the danger of studying a canonical field like physics as a monolithic exemplar of 'Cold War science.'

[548] Kruse and Barbeito, "A History of the American Biological Safety Association Part II," p 16.

[549] Worker's compensation claims under California law were already an increasing issue for Karl Meyer's George Hooper Foundation by the end of the 1930s. See Karl F. Meyer, "Medical Research and Public Health," Transcript of interviews conducted 1961-1962 by Edna Tartaul Daniel. Oral History Center, The Bancroft Library, University of California, Berkeley, 1976, pp 335-337.

ascendant in mid-century America, culminating in the passage of the 1970 Occupational Health and Safety Act (OSHA). Looming large over the nascent biosafety community at the conferences, OSHA was divisive, with "some view[ing] it as the most important social legislation since social security, or Our Savior Has Arrived; whereas others term it the most unconstitutional freedom-interfering repressive legislation since prohibition."[550] While OSHA was slow to promulgate regulations about laboratory or industrial microbiology, the biosafety community was eager to establish their own standards before what they saw as inevitable regulation happened, paralleling voluntary safety measures to forestall legal regulation by American industry in the Progressive era. A number of the original leaders in the professional society that would rise out of the biosafety conferences in 1984, the American Biological Safety Association, came to their field when their home institutions needed an in-house biosafety officer to meet these concerns of the 1970s.[551]

Supplementing this growing general concern about worker safety, the 1960s and 1970s also witnessed the rise of concern about novel entities like hypothetical cancer-causing viruses, all-too-documented viruses which cause hemorrhagic fevers like the Marburg and Ebola viruses, and bacteria genetically engineered with recombinant DNA.[552] Unlike in the 1930s, when laboratory directors like Karl Meyer researched

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[550] Kruse and Barbeito, "A History of the American Biological Safety Association Part II," p 15.
[551] Richard H. Kruse and Manuel S. Barbeito, "A History of the American Biological Safety Association Part III: Safety Conferences, 1978-1987," *Journal of the American Biological Safety Association* 3 no 1 (1998), pp 11-25.
[552] On American viral oncology research in the mid-20th century, see Robin Wolfe Scheffler, *A Contagious Cause: The American Hunt for Cancer Viruses and the Rise of Molecular Medicine*, Chicago: University of Chicago Press, 2019. The adaptation of containment technologies to research the Marburg virus (named after the German city in which the virus, originally from Africa, was first isolated from the body of a laboratory researcher) is discussed in Kirk, "Life in a Germfree World," pp 262-266.

newly documented microbes like *Coxiella burnetii* with a continuation of the culture of safety through individual skill, containment was invoked as a way to render these unpredictable new entities of the 1960s and 1970s 'safe.' Most famously, the 1975 2nd Asilomar Conference sought to assuage concerns about the safety of genetic engineering by advocating for containment-based voluntary guidelines. Historian of science Susan Wright has argued that the conference drew upon containment technologies and expertise as a 'black box,' effectively legitimating genetic engineering by reducing a universe of ethical questions about risk to the technical question of whether a particular strain of *E. coli* could be reliably contained by a safety cabinet.[553] The logic of Wright's argument can be seen in viral oncology and newly discovered pathogens like the hemorrhagic fevers (or later, in research on the viral cause of AIDS).[554] Novel entities like genetically engineered bacteria or previously-undocumented viruses were by definition unpredictable, and such unpredictability imparted unacceptable risk in the increasingly safety-conscious culture of the 1960s-1980s.[555] Backed up by a gamut of technologies and a professionalized knowledge community to operate them, containment-based biosafety offered a black box to be invoked against those risks. In the literal box of a biosafety cabinet, the dangers of an unknown microbe were reduced to the intelligible risks of separating the laboratory bench and the laboratory researcher.

The growing demand for biosafety expertise can be seen in Wedum's later career. Reflecting growing general concern about laboratory safety, he served on a new

[553] See Susan Wright, *Molecular Politics: Developing American and British Regulatory Policy for Genetic Engineering, 1972-1982*, Chicago: University of Chicago Press, 1994.
[554] Centers for Disease Control, "Acquired Immune Deficiency Syndrome (AIDS): Precautions for Clinical and Laboratory Staffs," *Morbidity and Mortality Weekly Report* 31 no 43 (1982), pp 577-580.
[555] Nicholas Wade, "Microbiology: Hazardous Profession Faces New Uncertainties," *Science* 182 no 4112 (November 9, 1973), pp 566-567.

American Public Health Association Committee on Laboratory Infection (chaired by Sulkin) in the mid-1960s, while still directing Detrick's Safety Division.[556] At the same time, he was recruited as a safety consultant by the National Cancer Institute (NCI) as it prepared to search for a hypothetical leukemia-causing virus.[557] Given the safety requirements which Wedum himself had helped enshrine for NCI research, it is not surprising that Detrick, with its extensive safety cabinet-equipped facilities that virtually embodied the containment ideal, was an attractive research site for the NCI after offensive biological warfare research was abandoned in 1969. By the early 1970s, the Institute and its contractors had become major stakeholders at Detrick, and like many of his colleagues, Wedum left direct government service to work as a safety expert at one such contractor, Litton Bionetics. He served in this newfound (but ultimately, still very familiar) role until his sudden death in 1976.[558] During the 1970s, he also consulted for the NIH on recombinant DNA research, contributing to the NIH's official "Guidelines for Research Involving Recombinant DNA Molecules," which were published shortly after his death in 1976.[559] These NIH standards were emblematic of a wider trend. Formalized regulations and classification schemes had been an integral part of Wedum's Detrick system, and similar systems accompanied containment-based biosafety as it became entrenched in the wider world of microbiology in the 1970s and 1980s. Reflecting the influence of the Detrick system in the growing biosafety community, these systems

[556] Wedum, "Airborne Infection in the Laboratory," p 1673 discusses this committee.
[557] W. Emmett Barkley, "The Contributions of Arnold G. Wedum to the Virus Cancer Program of the National Cancer Institute," *Journal of the American Biological Safety Association* 2 no 1 (1997), pp 10-11.
[558] Barkley, "In Celebration of Dr. Arnold Wedum's Legacy," p 6.
[559] Ibid; A. G. Wedum, "The Detrick Experience as a Guide to the Probable Efficacy of P4 Microbiological Containment Facilities for Studies on Microbial Recombinant DNA Molecules," *Journal of the American Biological Safety Association* 1 no 1 (1996), pp 7-25; U.S. National Institutes of Health, "Recombinant DNA Research Guidelines," *Federal Register* 41 no 131 (1976), pp 27902-27943.

incorporated Detrick practices like dividing safety cabinets into 3 classes and classifying microbes in terms of the minimum appropriate safety technology with which to study them.[560] This basic logic was enshrined in the early 1980s by World Health Organization and Centers for Disease Control publications, with a standardized taxonomy of matching particular microbes with a laboratory of an appropriate "Biosafety Level" that has been canonical to this day.[561]

Conclusion

The Detrick safety system was seen by its proponents as a triumph of rational management and containment technologies over a self-sacrificing "heroic ethos" of microbiological research that accepted the inevitability of some laboratory infections. Rejecting this resignation, the Detrick system sought to eradicate the phenomenon of laboratory infection by establishing total control over the spaces occupied by microbes, paralleling wider contemporary efforts to eradicate entire species of disease-causing organisms. Like disease eradication, this effort to establish total control over laboratory ecological relationships had mixed results. The Detrick system did meaningfully reduce infections in its laboratories as they worked closely with some of the most infectious pathogens known. However, this control was never complete, and laboratory infections were never completely eliminated. Technologies malfunctioned, people rebelled, and microbes escaped, and all of these things conspired against the control sought by the Safety Division.

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[560] See e.g. Jean L. Marx, "The New P4 Laboratories: Containing Recombinant DNA," *Science* 97 no 4311 (September 30, 1977), pp 1350-1352.
[561] World Health Organization, *Laboratory Biosafety Manual*, Geneva: World Health Organization, 1983; John H. Richardson and W. Emmett Barkley (eds), *Biosafety in Microbiological and Biomedical Laboratories*, Washington, DC: Government Printing Office, 1984.

When discussing humans' efforts to shape other organisms into 'technologies,' or to incorporate them into 'technological systems,' it is tempting to view them as essentially passive objects acted upon (with whatever degree of success) by human subjects. If this temptation exists for our fellow animals, it must be still more acute in the case of microbes, so invisible and anonymous that even our most basic knowledge of them and their activities is mediated through scientific instruments and expertise. This is, however, a temptation we must resist. Microbes act, as much as (or more than) they are acted upon, colonizing spaces and growing in ways that humans may not even know about unless they specifically go looking. This agency of the microbes can be seen highlighted in the Detrick safety system. This system, with its containment ideal, was ostensibly arranged to impose complete human control over microbes, confining them to exactly those spaces and bodies where their human masters wanted them to be. The heterogeneities, the complexities, the uncertainties, and the simple and omnipresent failures of this system, however, give the lie to this ideal of microbes as passive domesticates. Often enough, it was human behavior and practice which Wedum's cohort found they needed to regulate and modify, while all the while the unruly microbes they sought to confine found their way into undesired spaces and bodies. It was human researchers, as much or more than the microbes, who were domesticated by the Detrick safety system.

It is this domestication of microbiologists to a new safety regime which proved one of Detrick's most enduring legacies within the wider world of microbiology. With the promulgation of the containment ideal and its technologies in the 1970s, and the attendant rise of professionalized biosafety experts within microbiology laboratories, the Wedum group's vision of a strictly regimented laboratory ecology came increasingly to be the ideal of microbiologists across the world as well. There is considerable irony in this: the Safety Division had been founded in the first place to maintain secrecy at Detrick, yet the widespread promulgation of its ideal of containing microbes represented an ultimate breach of this containment of knowledge. In fact, containing secrets was no simpler than containing microbes. The safety system that Wedum and his group developed, with its recalcitrant subjects, sometimes-contradictory goals, and influence on knowledge production was in a way a microcosm of the military secrecy system in which they operated. It is to this secrecy system that we now turn.

Chapter 4: Bioweapons Scientists and Their Relationship with the Military Secrecy System

Grosse Île was in the middle of nowhere; that is why it was important. Located in the St. Lawrence River a few dozen miles north of Quebec City, the island was used by 19th century authorities as a quarantine station for predominantly Irish immigrants before they were permitted to disembark in the city downriver. This use had ended, but the Second World War brought with it another kind of quarantine for which the island was valued: biological weapons research. Canadian scientists, led by insulin discoverer Sir Frederick Banting, had been interested in the use of germs as weapons from the outbreak of the war, and shortly after the Americans entered the war in December 1941, Canadian bioweapons scientists agreed to collaborate with a similar group within the US' National Academy of Sciences.[562] One of the diseases they agreed to study together was a disease of cattle, not humans: rinderpest. A virus known for causing devastating epizootics in Eurasia and Africa, rinderpest had been heretofore kept out of North America by both countries' veterinary officials. Now, the two bioweapons groups feared that the inadvertent introduction they had long guarded against might be done deliberately, and with this fear, the decades-old search for a safe and effective rinderpest vaccine gained new urgency. Accordingly, one of the first acts of the new collaboration was to recruit prominent Rockefeller Institute virologist Richard Shope to head a program to create such a vaccine (and perhaps eventually, to develop rinderpest to be used offensively

[562] Banting himself died in an aircraft accident earlier that year, returning from a conference with British officials where he sought to convince them to step up their own bioweapons efforts. See Donald Avery, *Pathogens for War: Biological Weapons, Canadian Life Scientists, and North American Biodefense*, Toronto: University of Toronto Press, 2013.

against Axis herds). When the most reliable line of defense against rinderpest available to North American veterinarians was to keep it off the continent, however, such research risked causing the very catastrophe it was supposed to avert. Shope's team was accordingly housed among the abandoned station buildings of Grosse Île, where miles of water between the island and the mainland would hopefully keep the virus from spreading should it escape from the team's laboratories.[563]

Grosse Île's isolation made it a difficult place to work. Shope's team found the island's buildings, left over from its service as a quarantine station, inadequate for the harsh Canadian winter, and their requests for funding for new construction moved frustratingly slowly through the bureaucracies of two countries. Supplies ranging from food to test animals and scientific equipment had to be laboriously sailed out to the island, which the winter again made more difficult with the freezing of the St Lawrence. Finally, any communications between the island and the mainland needed to be either carried by those same boats, or transmitted via short-wave radio. Seeking to rectify this, one of the most urgent construction priorities for the Shope group was the installation of an underwater telephone line from the island to the mainland, which in the fall of 1942 they hoped to have done before that winter's freeze. In principle, the ABC and M-1000 committees agreed with this idea, though in practice, they were still deliberating about it in January 1943, precluding construction before the spring thaw.[564]

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[563] See Chapter 2 of Amanda Kay McVety, *The Rinderpest Campaigns: A Virus, Its Vaccines, and Global Development in the Twentieth Century*, New York: Cambridge University Press, 2018 for a discussion of the Grosse Île program.

[564] These deliberations can be found in the National Academy of Sciences Archives collection "Committees on Biological Warfare, 1941-1948" (NAS BW) Box 8 Folder 7 ("Jewett, Frank B. (Black Book): 1942-1943").

The whole point of having the phone line installed was the inherent insecurity of transmitting any sensitive information over Grosse Île's short-wave radio, which anyone with a set of their own could pick up. In principle, however, the telephone line too was insecure: it could be deliberately tapped by a spy, or simply eavesdropped upon by a manual switchboard operator. Seeking to secure their proposed line further, the Shope group also requested that they be assigned one of Bell Labs' A-3 scramblers. The A-3 was an electromechanical device which transposed particular frequencies of a telephone conversation every 20 seconds, producing a signal of unintelligible gibberish for any listener who did not possess an equivalent descrambler. In the fall of 1942, the A-3 remained an important part of Allied electronic security, used even for trans-Atlantic telephone calls between Franklin Roosevelt and Winston Churchill.[565] The very existence of the Grosse Île project, like the rest of the Allied bioweapons program, was a secret; unsurprisingly, this request for a device to encipher their telephone conversations was initially received favorably.

By January 1943, however, even as plans were being made to finally install the telephone line after the spring thaw, the Grosse Île request for an A-3 scrambler had been firmly denied. The problem, according to military and law enforcement authorities, is that the A-3 could not be trusted to remain truly unintelligible.[566] As NAS President Frank Jewett explained it, the A-3 "is essentially a privacy system which, although guarding against ordinary listening, can readily be broken by experts. If those using circuits

[565] David Kahn, "Cryptology and the Origins of Spread Spectrum," *IEEE Spectrum* 21, no. 9 (1984), pp 70-80.

[566] This assessment was correct: German intelligence had broken A-3 scrambled Anglo-American communications by the fall of 1941, and by 1943 the Allies had developed a superior device, SIGSALY, which they began using for conversations between top-level political and military officials. See David Kahn, *The Codebreakers*, New York: Macmillan, 1967, pp 549-560.

equipped with this privacy system were cryptic in their speech the thing might in effect be reasonably secret. Experience, however, has apparently shown that users have an undue sense of security on circuits thus equipped and so are not cautious in their speech."[567] Instead, E. B. Fred recommended that only "ordinary telephones be used and that conversations be limited to non-secret subjects."[568]

Not absolutely trusting in flawed equipment is logical, but there is something paradoxical in *refraining* from the use of encryption equipment in the name of preserving a secret. Why not install the equipment anyway and censure anyone who broke regulations by speaking of secret matters over the phone? Keeping a secret, like any activity, is an exercise in balancing calculated risks- was the risk of Axis agents knowing enough to tap the phone line, descrambling its signal, and hearing indiscreet conversations greater than that of using a phone line which provided no security against even casual eavesdropping? Perhaps it was, but this question seems not to have been part of the deliberations. In a way, deliberations about the A-3 seemed more concerned about the risk of secrecy regulations being violated than they were about that of secrets actually being revealed to the enemy.

Read in the light of recent work on secrecy and science, this distinction seems like a telling reflection of the distinction between *secrets* and *secrecy* raised by this scholarship. Secrets, information to be concealed, do not exist in a vacuum, instead being upheld by systems of secrecy, practices and technologies intended to maintain this concealment. The Grosse Île decision was a choice about what bioweapons secrecy

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[567] Frank B. Jewett to Charles F. Sise, January 15, 1943, in NAS BW Box 8 Folder 7 ("Jewett, Frank B. (Black Book): 1942-1943").

[568] E. B. Fred to Frank B. Jewett, January 9, 1943, in NAS BW Box 8 Folder 7 ("Jewett, Frank B. (Black Book): 1942-1943").

would look like: would it rely on semi-reliable technological systems, or eschew such systems in the name of disciplining their users more effectively? A great deal of information about Grosse Île was supposed to be a secret, with knowledge ranging from scientific specifics to the very fact that vaccine research was being pursued being seen as worthy of concealment. It is the vagaries of the act of concealment itself, however, that best reveals the presumptions and goals of the military and scientific authorities who deemed this knowledge secret.

The American biological weapons program of 1942-1969, of which Grosse Île was a small part, was steeped in secrecy throughout its existence. Its very existence was a secret throughout the Second World War, and throughout subsequent decades of the Cold War, its activities were shrouded by a thick air of mystery.[569] To understand the bioweapons program requires not only reconstructing a history that took place behind the secrecy system; it requires understanding that system itself. Three major questions present themselves when examining the bioweapons secrecy system. 1) What knowledge was deemed secret, for what reasons, and how did this change over time? 2) How did the sociotechnological system of secrecy which maintained these secrets work? 3) How did this system of secrecy affect the relationship between scientists in the 'closed world' of classified knowledge and the 'open' scientific community? All of these questions, in turn, invite comparison to the better-studied system of nuclear secrecy that existed contemporaneously with the system that existed at Detrick. Unbeknownst to scientists

[569] See John Ellis van Courtland Moon, "US Biological Warfare Planning and Preparedness: The Dilemmas of Policy," in Erhard Geissler and John Ellis van Courtland Moon, eds., *Biological and Toxin Weapons: Research, Development and Use from the Middle Ages to 1945 (SIPRI Chemical & Biological Warfare Studies 18)*, New York: Oxford University Press, 1999, pp 215-254; John Ellis van Courtland Moon, "The US Biological Weapons Program," in Mark Wheelis, Lajos Rózsa, and Malcolm Dando (eds), *Deadly Cultures: Biological Weapons since 1945*, Cambridge, MA: Harvard University Press, 2006, pp 9-46.

working on both weapons projects during the Second World War, they had entered a new world of secret state-sponsored science, which would continue indefinitely after the war's end. State secrecy was a paradigmatic feature of Cold War science, and understanding how that secrecy worked at Detrick gives another perspective to compliment the often-repeated story of the nuclear weapons complex. Both the scientific fields and the government authorities involved in bioweapons research differed from the familiar nuclear physicists and AEC officials of the nuclear secrecy story, and from this difference stemmed a different culture of secrecy. Ultimately, the comparison reiterates the paramount point that any system of secrecy is ultimately an ordering of social relations: that is to say, it is political. The secrecy system of the American bioweapons program is revealing of the politics of that program.

Secrecy and Science

When Allied scientists were working on their biological weapons program, the prevailing view of the secrecy that governed it and other wartime scientific projects was that it was an aberration. Sociologist of science Robert Merton articulated this view canonically, asserting that open publication of data was a basic feature of the norms of science.[570] Scholars of secrecy and science have since made a habit of invoking Merton, either positively to buttress an ethical argument against secrecy in science, or more recently to establish a wrongheaded received wisdom to argue against.[571] In either case,

[570] Robert Merton, "Science and Technology in a Democratic Order," *Journal of Legal and Political Sociology* 1 (1942), pp 115-126. See also David Hollinger, "The Defense of Democracy and Robert K. Merton's Formulation of the Scientific Ethos," *Knowledge and Society* 4 (1983), pp 1-15; Jessica Wang, "Merton's Shadow: Perspectives on Science and Democracy Since 1940," *Historical Studies in the Physical and Biological Sciences*, 30 no 1 (1999), pp 279-306.

[571] For an example of late Cold War STS literature on secrecy (with a heavy ethical undercurrent) see *Science, Technology, and Human Values* 10 no 2 (1985), a special issue on secrecy. See especially Rosemary Chalk, "Overview: AAAS Project on Secrecy and Openness in Science and Technology," *Science, Technology, and Human Values* 10 no 2 (1985), pp 28-35.

however, scholars have generally invoked Merton to argue about a phenomenon- secret science- far more pervasive than the symptom of totalitarianism that he described.[572] Proponents of scientific openness during the Cold War confronted the increasingly apparent reality that continued state sponsorship of science would indefinitely carry with it continued secrecy.[573] Nuclear physicists found their field's results 'born secret' under the Atomic Energy Acts of 1946 and 1954, a new and constitutionally-dubious assertion of government control over nuclear knowledge which superseded wartime schemes to use the preexisting patent system to legally control such knowledge.[574] In other fields of science, this statutory clampdown did not exist, but influxes of military funding still made the attached strings of secrecy all-pervasive in some fields. Michael Aaron Dennis has compared studying the history of Cold War science to examining an archipelago of

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[572] See Judith Reppy (ed), "Secrecy and Knowledge Production," Cornell University Peace Studies Program Occasional Paper #23 (1999); Susan L. Maret and Jan Goldman (eds), *Government Secrecy: Classic and Contemporary Readings*, Westport, CT: Greenwood Publishing, 2009 for two diverse anthologies of writings. Two influential jeremiads to the political consequences of secrecy in a democracy, separated by over forty years, are Edward A. Shils, *The Torment of Secrecy: The Background and Consequences of American Security Policies*, Glencoe, IL: The Free Press, 1956 and Daniel Patrick Moynihan, *Secrecy: The American Experience*, New Haven: Yale University Press, 1998. See also the War on Terror-era retrospective Timothy Ericson, "Building Our Own 'Iron Curtain:' The Emergence of Secrecy in American Government," *The American Archivist* 68 no 1 (2004), pp 18-52.

[573] This is not to say that state secrecy did not exist in American science before its entry into the Second World War. See e.g. Naomi Oreskes and Ronald Rainger, "Science and Security before the Atomic Bomb: The Loyalty Case of Harald U. Sverdrup," *Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics* 31 no 3 (2000), pp 309-369.

[574] Alex Wellerstein, "Patenting the Bomb: Nuclear Weapons, Intellectual Property, and Technological Control," *Isis* 99 no 1 (2008), pp 57-87; Alex Wellerstein, "Knowledge and the Bomb: Nuclear Secrecy in the United States, 1939-2008," PhD diss, Harvard University, 2012; Alex Wellerstein, *Restricted Data: The History of Nuclear Secrecy in the United States*, Chicago: University of Chicago Press, 2021; Simone Turchetti, "Patenting the Atom: The Controversial Management of State Secrecy and Intellectual Property Rights in Atomic Research," in Stathis Arapostathis and Graham Dutfield (eds), *Knowledge Management and Intellectual Property: Concepts, Actors and Practices from the Past to the Present*, Cheltenham, UK: Edward Elgar Publishing, 2013, pp 216-234; William Burr, Thomas S. Blanton, and Stephen I. Schwartz, "The Costs and Consequences of Nuclear Secrecy, in Stephen I. Schwartz (ed), *Atomic Audit: The Costs and Consequences of U.S. Nuclear Weapons Since 1940*, Washington, D.C.: Brookings Institution Press, 1998, pp 433-484. The essential problem with pre-1946 schemes to use the pre-existing American patent system to control atomic knowledge is that by definition, patented knowledge would be made public. British patent law, in contrast, includes provisions for secret patents. See T. H. O'Dell, *Inventions and Official Secrecy: A History of Secret Patents in the United Kingdom*, Oxford: Clarendon Press, 1994.

'open' historical events and actors, with connections between them obscured by a thick 'ocean' of official secrecy.[575] In his study of the AEC's National Laboratory system Peter Westwick argues that for scientists who experienced this growth of permanent secrecy, the way to live with it was to virtually replicate the social structures of 'open' science behind closed doors, complete with classified conferences and peer-reviewed journals.[576] A large government-sponsored world of secret science continues to exist today, and as Peter Galison has influentially argued, the domain of secret knowledge held by the American government alone (produced and concealed in a perverse kind of 'anti-epistemology') is probably several times larger than the domain of 'open' knowledge held in ordinary libraries.[577]

Galison's use of the term "anti-epistemology" both encapsulates a traditional approach to analyzing secret science and summarizes why secrecy is interesting to historians and sociologists of science. Both fields are fundamentally concerned with epistemology, seeking (in a sometimes-direct recapitulation of early modern philosophical debates) to trace the connections between everyday scientific activities like

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575 Michael Aaron Dennis, "'Our First Line of Defense:' Two Laboratories in the Postwar American State," *Isis* 85 no 3 (1994), pp 427-455. For studies of the culture of secrecy in Soviet defense science, see Mark Harrison, "Secrecy," in Mark Harrison (ed), *Guns and Rubles: The Defense Industry in the Stalinist State*, New Haven: Yale University Press, 2008, pp 230-254; Galina Orlova, "Secret Laboratory Life in the USSR, 1940s-1970s," *Cahiers du Monde Russe* 60 no 2-3 (2019), pp 461-492.

576 Peter J. Westwick, *The National Labs: Science in an American System, 1947-1974*, Cambridge, MA: Harvard University Press, 2003. See also Robert W. Seidel, "Secret Scientific Communities: Classification and Scientific Communication in the DOE and DoD," in Mary Ellen Bowden, Trudi Bellardo Hahn, and Robert V. Williams (eds), *Proceedings of the 1998 Conference on the History and Heritage of Science Information Systems,* Medford, NJ: Information Today, Inc., 1999, pp 46-60; Janet Farrell Brodie, "Learning Secrecy in the Cold War: The RAND Corporation," *Diplomatic History* 35 no 4 (2011), pp 643-670; Janet Farrell Brodie, "Radiation Secrecy and Censorship after Hiroshima and Nagasaki," *Journal of Social History* 48 no 4 (2015), pp 842-864.

577 Peter Galison, "Removing Knowledge," *Critical Inquiry* 31 no 1 (2004), pp 229-243. One of the most prominent observers of secrecy in the federal government today is Steven Aftergood of the Federation of American Scientists. See in particular Steven Aftergood, "Reducing Government Secrecy: Finding What Works," *Yale Law and Policy Review* 27 (2009), pp 399-416, as well as his ongoing online newsletter *Secrecy News*.

conducting experiments and debating results and the final production of scientific knowledge.[578] *Secret* scientific knowledge challenges the social structure underlying scientific epistemology by denying the possibility of open peer review and debate. Whatever one's position about how scientists produce knowledge, there is something perverse about pairing this production with such intentional concealment, as Merton encapsulated in his norms or indeed Galison does in his terminology.

More recent scholarship on secrecy has in a sense reversed this viewpoint, privileging the act and consequences of concealment themselves as objects of study, rather than tracing how an open scientific epistemology is violated by secret-keeping.[579] This, for instance, is the self-conscious orientation of scholars of agnotology: the study of ignorance not as a lacuna in knowledge but as an actively produced product of social forces ranging from deliberate corporate obfuscation to a willful forgetting of the origin of subaltern knowledge.[580] Other studies, influenced especially by anthropological and sociological discussions of secrecy, similarly focus on the ritualized act of secret-keeping itself, or on the maintenance of particular spaces privileged as secret.[581] Recent historical

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[578] The most literal example of such recapitulation of early modern debates is Simon Schaffer and Steven Shapin, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life*, Princeton: Princeton University Press, 1985.

[579] The work of philosopher Sissela Bok, with an emphasis on studying active concealment over the passive keeping of secrets, is foundational. See Sissela Bok, *Secrets: On the Ethics of Concealment and Revelation*, New York: Pantheon Books, 1982.

[580] Robert N. Proctor and Londa Schiebinger (eds), *Agnotology: The Making and Unmaking of Ignorance*, Stanford: Sanford University Press, 2008. For an influential study similarly documenting the type of corporate obfuscation of knowledge described in this book, see Naomi Oreskes and Erik M. Conway, *Merchants of Doubt: How a Handful of Scientists Obscured the Truth on Issues from Tobacco Smoke to Climate Change*, New York: Bloomsbury Press, 2010. For a more recent collection developing the theme of ignorance, see Matthias Gross and Linsey McGoey (eds), *Routledge International Handbook of Ignorance Studies*, New York: Routledge, 2015, and the open-access journal *Secrecy and Society*. See also Felicity Mellor and Stephen Webster (eds), *The Silences of Science: Gaps and Pauses in the Communication of Science*, New York: Routledge, 2017.

[581] See e.g. Michael Taussig, *Defacement: Public Secrecy and the Labor of the Negative*, Stanford: Stanford University Press, 1999; Rebecca Empson, "Separating and Containing People and Things in Mongolia," in Amiria Henare, Sari Wastell, and Martin Holbraad (eds), *Thinking Through Things: Theorizing Artefacts Ethnographically*, London: Routledge, 2007, pp 138-171. For a recent review article

treatments of secrecy with this orientation challenge the presumption that science existed in a state of Edenic openness before it tasted the apple of national security, emphasizing the connectedness of openness and secrecy as communications strategies.[582] Mario Biagioli, for instance, describes Galileo as a quintessential Renaissance courtier, managing his relationship with his patrons with calculated revelation, spectacle, and secrecy.[583] Koen Vermier argues that the rise of inductive natural philosophy took place in an early modern context of esoteric knowledge and ritualized secret societies.[584] Secrets were an important cultural currency, ironically advertised in so-called 'books of secrets,' and far from being the opposite of openness, secretiveness thrived on this performativity of revelation.[585] Stephen Hilgartner discusses a similar logic in the mix of

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on the anthropology of secrecy, see Graham M. Jones, "Secrecy," *Annual Review of Anthropology* 43 (2014), pp 53-69. Sociologist Georg Simmel's work is foundational for this literature. See Georg Simmel, "The Sociology of Secrecy and of Secret Societies," *American Journal of Sociology* 11 (1906), pp 441-449. See also Dennis N. Livingstone, *Putting Science in Its Place: Geographies of Scientific Knowledge*, Chicago: Chicago University Press, 2003, for a discussion of the growing importance of space in the historiography of science.

582 A notable example of this literature can be found in a 2012 special issue of the *British Journal for the History of Science* entitled "States of Secrecy." See Koen Vermeir and Dániel Margócsy, "States of Secrecy: An Introduction," *The British Journal for the History of Science* 45 no 2 (2012), pp 153-164. See also Michael Aaron Dennis, "Secrecy and Science Revisited: From Politics to Historical Practice and Back," in Ronald E. Doel and Thomas Söderqvist (eds), *The Historiography of Contemporary Science, Technology, and Medicine: Writing Recent Science*, New York: Routledge, 2006, pp 172-184.

583 See Mario Biagioli, *Galileo, Courtier: The Practice of Science in the Culture of Absolutism*, Chicago: University of Chicago Press, 1993, for the canonical articulation of this thesis. For Biagioli's more recent treatment focusing on Galileo's communication strategies, see Mario Biagioli, *Galileo's Instruments of Credit: Telescopes, Images, Secrecy*, Chicago: University of Chicago Press, 2006. See also María M. Portuondo, *Secret Science: Spanish Cosmography and the New World*, Chicago: University of Chicago Press, 2009 for an examination of state secrecy in an early modern science.

584 Koen Vermeir, "Openness Versus Secrecy? Historical and Historiographical Remarks," *British Journal for the History of Science* 45 no 2 (2012), pp 165-188.

585 Francis Bacon's rhetoric of his philosophical project seeking to reveal the "secrets of nature" was born within this milieu. A rapacious early modern attitude toward a (gendered female) nature has attracted considerable attention from historians of science, particularly in the feminist tradition. Carolyn Merchant, *The Death of Nature: Women, Ecology, and the Scientific Revolution*, San Francisco: Harper & Row, 1980, classically connects this violent Baconian logic to technoscientific environmental degradation, while more recently Katherine Park, *Secrets of Women: Gender, Generation, and the Origins of Human Dissection*, New York: Zone Books, 2006, traces the medieval and Renaissance history of human dissection through a lens of male savants' growing interrogation and subjection of female bodies. See also Carolyn Merchant, "Secrets of Nature: The Bacon Debates Revisited," *Journal of the History of Ideas* 69 no 1 (2008), pp 147-162 for a discussion of scholarly debates about the meaning of Bacon's words, and Katherine Park,

territory-staking open discussion and secret-keeping in the communications strategy of scientists in the biotechnology industry of the 1990s.[586] In both cases, 'openness' and 'secrecy' are both about the selective communication and non-communication of particular pieces of knowledge, existing on a spectrum together rather than diametrical opposites. Other scholars like Pamela Long and Paola Bertucci also challenge the traditional presumption that in contrast to open science, technological or craft knowledge is inherently secretive, instead arguing that craftsmen and engineers have used similarly complex strategies of open revelation and concealment.[587] Brian Balmer, meanwhile, emphasizes the complex nature of even 20th century secret science, with the imperative of concealment producing a practice of science sharply different from its 'open' counterpart.[588] A common theme throughout this scholarly focus on the act of concealment over the thing concealed is a problematization of the neat division between the 'open' world and the 'closed' secret one. Activities conducted under a cloak of secrecy (including science) are not merely their normal selves behind closed doors: they

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"Women, Gender, and Utopia: The Death of Nature and the Historiography of Early Modern Science," *Isis* 27 no 3 (2006), pp 487-495 for a reflective examination of the scholarly impact of Merchant's work.

[586] Stephen Hilgartner, "Selective Flows of Knowledge in Technoscientific Interaction: Information Control in Genome Research," *The British Journal for the History of Science* 45 no 2 (2012), pp 267-280.

[587] Pamela O. Long, *Openness, Secrecy, Authorship: Technical Arts and the Culture of Knowledge from Antiquity to the Renaissance*, Baltimore: Johns Hopkins University Press, 2001; Paola Bertucci, "Enlightened Secrets: Silk, Intelligent Travel, and Industrial Espionage in Eighteenth-Century France," *Technology and Culture* 54 no 4 (2013), pp 820-852. Ann Johnson has likewise analyzed the engineering of anti-lock brake systems in the mid-20th century by studying engineers' knowledge-sharing communities that transcended formal institutional boundaries. Engineering knowledge, while different from other forms of knowledge, is just as socially rooted, she argues. See Ann Johnson, *Hitting the Brakes: Engineering Design and the Production of Knowledge*, Durham, NC: Duke University Press, 2009.

[588] See (most notably) Brian Balmer, *Secrecy and Science: A Historical Sociology of Biological and Chemical Warfare*, London: Ashgate Publishing, 2012 and Brian Balmer and Brian Rappert (eds), *Absence in Science, Security and Policy: From Research Agendas to Global Strategy*, London: Palgrave Macmillan, 2016. As Robert Bud argues, Balmer's subjects, British bioweapons researchers, nonetheless used an ideal of 'open science' as a cultural currency when conversing with their presumed counterparts in the eastern bloc. See Robert Bud, "Biological Warfare Warriors, Secrecy and Pure Science in the Cold War: How to Understand Dialogue and the Classifications of Science," *Medicina Nei Secoli Arte E Scienza* 26 no 2 (2014), pp 451-468.

are fundamentally influenced and altered by the imperative of secrecy surrounding them. Secret-keeping is also inherently social, a process of separating the world into groups of people privy to those secrets and everyone else, and this separation is an inherently political exercise of power. Finally, the maintenance of such a division between knowledge freely known and knowledge controlled is also physical: the control over specific spaces, documents, and artifacts, with sometimes absurd results.

Scientists, Secrecy, and Publication

As the National Academy of Sciences' WBC Committee began to meet in 1941 and 1942 its chairman, the University of Wisconsin's E. B. Fred, continually emphasized "the need for secrecy. NO PUBLICITY IS TO BE GIVEN TO ANY OF THE PAPERS."[589] Biological warfare was seen as both a 'dirty' and a potentially devastating way of making war, and so for both reasons the very fact that the US had such a program was to be kept confidential. For scientists working with the WBC Committee or its NAS-sponsored successors, this meant entering the unfamiliar social world of military secrecy. Though the NAS was a civilian organization (and a curious hybrid of private and public besides), it was acting on behalf of military 'clients' in the form of the War and later Navy Departments, and its committees adopted the War Department system of classifying information in its operations.[590] Documents were to be stamped with classification markings, and anyone privy to them would need to be investigated for a

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[589] "Dr. Fred's Diary, Thursday January 22, 1942," in NAS BW Box 1, Folder 5 ("Chairman: Fred E B 1941-1942").

[590] Major histories of the American secrecy system focus on the period after 1940, but do devote some attention to the 'prehistory' of this system, particularly in the early 20th century. See especially Moynihan, *Secrecy* and Arvin S. Quist, *Security Classification of Information, Volume 1: Introduction, History, and Adverse Impacts* (rev. ed.), Oak Ridge National Laboratory, 2002. This latter work is a report by a consummate "insider" (Quist was a professional declassifier at Oak Ridge), but contains detailed research and valuable analyses.

clearance to see them. Everyone who was cleared was given a copy of the 1917 Espionage Act to read and sign to emphasize the legal consequences of revealing these documents' contents.[591] The scientists accepted the wartime necessity of this secrecy, but it was nonetheless a major culture shock for them. Most microbiologists were used to a culture of open publication of research, complete with discussing their work and exchanging bacterial cultures along networks of correspondence. Some, like many of the University of Wisconsin microbiologists, had experience acting as industrial consultants, but whatever discretion that required of them in that role paled in comparison with a requirement to keep the very fact of their work secret.[592]

The reason secrecy was generally so foreign to the scientists' practice was the importance of publication in their securing professional rewards. This emphasis on publication was not simply a culture of 'openness' in contrast to one of secrecy: instead, it was a differing strategy of concealing and revealing information. Scientific work in progress, which was not yet ready to publish, was well worth concealing from professional rivals. This was facilitated by the small-scale nature of bacteriology research in particular, with research teams typically consisting of only a few individuals.

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[591] NAS BW Box 3 Folder 8 ("WBC Committee: Security Clearance: 1942"). The 1917 Espionage Act was an unprecedented extension of Federal power in the name of national security at the time, was freely used during WWII to suppress anti-war and pro-Axis speech, and is still a fundamental part of the statutory bedrock of the American national security state. See Petra DeWitt, "'Clear and Present Danger:' The Legacy of the 1917 Espionage Act in the United States," *Historical Reflections* 42 no 2 (2016), pp 115-133. Reading and signing the Act, however, was more of a symbolic ritual of initiation than anything legally binding. Indeed, it was not until the mid-1950s that the Act was used to prosecute a leak of classified information to the press (as opposed to espionage on behalf of a specific foreign nation), an example, Sam Lebovic argues, of the "considerable improvisation and contingency that attended the political and legal history of the [American] secrecy regime." Sam Lebovic, "From Censorship to Classification: The Evolution of the Espionage Act," in Kaeten Mistry and Hannah Gurman (eds), *Whistleblowing Nation: The History of National Security Disclosures and the Cult of State Secrecy*, New York: Columbia University Press, 2020, pp 45-68, 46.

[592] Baldwin, for instance, worked as a consultant for the Red Star Yeast Company in the 1930s. See B. A. Bergenthal to Ira Baldwin, in University of Wisconsin Archives (UWA) Ira L. Baldwin Papers (Series 9/10/11), Box 4 Folder 12.

Paradoxically, secret military research contrasted with this culture by insisting on *openness* between researchers which would be premature in normal science. In the first years of American bioweapons research, the NAS' War Research Service sponsored over two dozen research projects based in geographically disparate universities.[593] As usual, these were done by small research teams, but the fact that they could publish nothing meant that for the researchers, the WRS' insistence on regular reports posed the risk of the information they revealed benefiting their rivals. Some researchers resisted this imposition of premature openness. For example, in 1943 Michigan State's I. Forest Huddleson, who served as the WRS' main contract brucellosis researcher, identified and isolated a particularly virulent strain of *Br. suis* which would serve as one of the major strains used in subsequent BW research, and developed a human vaccine. He reported on this research, and provided finished vaccines on request, but he did not reveal enough details for rival brucellosis researchers to replicate his work. By 1944, bioweapons research was undergoing increased militarization and centralization of the program at Camp Detrick, which annexed most of the WRS university projects over the course of the year. When Huddleson found his project turned over to a Detrick team lead by his rival Joseph Franklin Griggs (who, Huddleson asserted, had once previously said to his face that he knew nothing about brucellosis), he was infuriated.[594] He wrote to Fred with a thinly veiled threat to withhold his vaccine from the Detrick team unless he received assurances about retaining sole credit for it after the war. Fred did his best to be diplomatic with Huddleson, noting to his colleague William Sarles that since the

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[593] See list in NAS BW Box 2 Folder 1 ("WBC Committee Projects: 1942").

[594] George W. Merck, "Memorandum for the Chief, Chemical Warfare Service, Subject: Supplemental Research and Development," March 17, 1944, in NAS BW Box 4 Folder 11 ("Chemical Warfare Service Chief: 1942-1944").

government had no patent rights over the vaccine, Huddleson could well follow through on his threat.[595] Huddleson was eventually placated and provided vaccine doses, but apparently continued to conceal crucial details about its production: when the Chemical Warfare Service's official history of the bioweapons program was penned after the war in 1947, Huddleson's technique was still not known to the Army.[596]

For researchers working on the centrally directed projects at Camp Detrick in 1944-1945, this sort of concealment was not possible. They worked as part of an unprecedentedly large institution, with projects whose data was reported directly to Detrick leadership.[597] Fortunately, a Detrick researcher was in the same situation as any of their compatriots, being unable to publish during the war. This control over publication only applied to scientists who actually worked for the bioweapons program, however. The secrecy system affirmatively controlled those who were part of it, but information relevant to biological warfare produced outside the system was not 'born secret' like nuclear knowledge would be after the war. Some topics, like aerobiology and the mass culture of pathogens, were of such niche interest in civilian science that Detrick researchers quickly developed an unmatched body of knowledge, but other topics like antibiotic therapy and plant hormone-based pesticides were active areas of research outside of the bioweapons world. Researchers of these topics, particularly the Detrick plant pathologists who studied the latter, worried about being 'scooped' by scientists not bound by the same strictures of secrecy that they were. Worse yet, they couldn't even

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[595] William Sarles to E. B. Fred, July 17, 1944 in NAS BW Box 4 Folder 5 ("ABC Committee WRS Representative: Fred, E. B.").
[596] See Rexmond C. Cochrane, *History of the Chemical Warfare Service in World War II, Volume 2: Biological Warfare Research in the United States*, Edgewood Arsenal: Historical Section, Office of the Chief, Chemical Corps, 1947, p 295.
[597] See comprehensive monthly statistics about Detrick personnel in Ibid, p 74.

warn off their inadvertent rivals, because the very fact that they were working on the topics that they were was a secret.

By late 1944, these worries came to a head when the plant physiologists caught word that a group of pesticide researchers working for the USDA had inadvertently duplicated some of their work and were planning to publish.[598] Their vehement protests added urgency to the issue for the scientist-advisors working for the newest iteration of the NAS' bioweapons group, the DEF Committee.[599] These scientists were in a strange position. While growing military interest in biological warfare had reduced the control scientists had over the program, they remained an influential part of Detrick's ultimate 'management.' Nonetheless, they were sensitive to the concerns of Detrick researchers, both as colleagues and because they knew what grievances would cause trouble with bench researcher 'labor.' For both reasons, they were eager to establish a system to ensure that publications or at least scientific credit would flow after the war. Furthermore, in the case of pesticides, which were being investigated as a weapon to destroy enemy crops, the interests of scientists who did not want to miss out on credit and Army officials who did not want militarily significant information published coincided.

The DEF Committee drew on a semi-formal preexisting system of voluntary censorship by scientific journals to prevent the USDA group from publishing. The National Research Council Advisory Committee on Scientific Publication was another NAS-affiliated institution staffed by experts in various fields who could serve as judges of how much sensitive information a draft paper's publication could reveal. E. B. Fred,

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[598] George Merck, "Memorandum for Conversation with Dr. Auchter," December 1, 1944 in NAS BW Box 5 Folder 23 ("DEF Committee Chairman: Pepper O H Perry: 1944-1945").
[599] William B. Sarles to O. H. Perry Pepper, December 7, 1944 in NAS BW Box 5 Folder 23 ("DEF Committee Chairman: Pepper O H Perry: 1944-1945").

for instance, served as an official censor for bacteriology papers.[600] The editors of scientific journals could submit papers they had received to this body for official advice on whether to defer publication in the name of the war effort. Unlike the regimented secrecy system governing military research, this system was voluntary, informal, and implicitly temporary, but because it drew on preexisting scientific networks and the public culture of science's sense of emergency patriotism, it was nonetheless fairly effective at supplementing the military secrecy system. Contacting both the would-be authors and likely journal editors through this system, the DEF committee was able to informally forestall the USDA publication. This crisis, however, had brought to a head what Sarles called "the publications problem."[601] By the winter of 1945, it was clear that the war was not going to last too much longer, and even Detrick scientists working on more obscure topics were becoming nervous about securing professional rewards for their hard and hitherto unrecognized work. The common expectation was that their submission to the military secrecy system was a temporary wartime expediency, and that they should be able to publish much of their work after the end of the war. However, as this became a more immediate prospect, the DEF Committee became increasingly concerned with ensuring that this expectation became a concrete and equitable reality.

A major concern was the flattening of time within the secret world of Detrick. Researchers had worked there for almost two years, a timeframe in which under normal circumstances they might have published multiple times as their projects developed. If results were simply published at the end of the war, foundational but old work might be

[600] Herman C. Mason to E. B. Fred, Feb 29, 1944 in NAS BW Box 4 Folder 5 ("ABC Committee WRS Representative: Fred, E. B.").
[601] William B. Sarles to O. H. Perry Pepper, March 15, 1945 in NAS BW Box 5 Folder 23 ("DEF Committee Chairman: Pepper O H Perry: 1944-1945").

superseded by newer work, perversely diminishing the professional rewards reaped by the earlier researcher. Attempting to forestall this concern, the DEF Committee gave a lot of thought in early 1945 to schemes to effectively replicate normal publication behind closed doors.[602] Perhaps, in conjunction with relevant journal editors, researchers could submit their papers to be published after the war, but with a note attached of when the paper was received? Perhaps the NAS itself could receive and archive papers, certifying their priority after the war? As it stood, both schemes came to naught. Drawing editors into a benign kind of priority-preserving conspiracy ran afoul of the strict need-to-know concept governing the secrecy system. The idea that the NAS do the job itself, in turn, foundered on practicalities. Qualified scientists with security clearances were already obviously extremely busy, and if the NAS was to serve as an ersatz editor, what assurance of quality and originality would it plan to provide?[603] Unsatisfactorily, it looked by the summer of 1945 that when secrecy was lifted there would be no system in place to assure priority other than a mad scramble to publish before anyone else did.

In the months following the end of the war, however, it became clear that the presumption that secrecy would in some blanket way 'be lifted' was itself unwarranted. The information-management cultures of science and the military secrecy system were again at odds, as under the later, even information which was admittedly deserving of open release still needed to be affirmatively removed from the system by a process of review and declassification. The natural tendency of documents produced within the secrecy system was for them to remain confidential, whether they contained 'secrets' or

[602] "DEF Committee Meeting of the Executive Committee, 13 February, 1945," in NAS BW Box 5 Folder 26 ("DEF Committee Executive Committee Meetings February 1945").
[603] See "DEF Committee Meeting 9:00 am to 1:00 pm June 13, 1945," in NAS BW Box 6 Folder 1 ("DEF Committee Meetings June 1945").

not. Thus, when the existence of the bioweapons program was revealed to the public at the beginning of 1946 and publication became an option for researchers winding down their service at Detrick, the proverbial rush for the exits took place through the bottleneck of having a paper reviewed and formally cleared by a declassifier.[604] Compounding the bottleneck, this needed to be someone with the security clearance to initially see the (still-secret!) document in the first place, and with the expertise to judge whether the paper revealed anything which was still to remain secret under postwar classification guidelines.[605] Through 1946 and 1947, these guidelines were at least liberal in intent, in theory allowing bioweapons researchers to publish work with all but the most direct military significance. Navigating the bottleneck of declassification, they published 146 papers (and one monograph) in this period, and in 1946 alone presented 21 papers at the meetings of scientific societies like the SAB.[606] Fortune favored the well-prepared: Theodor Rosebury, who directed aerobiology research in Detrick's Cloud Chamber project and who published the solitary monograph, *Experimental Air-borne Infection*, spent the spring of 1946 "working intensively to get the Detrick reports in final form for publication" to secure an early place in the clearance queue.[607] Fortunately, the fact that publication was not possible during the war did not mean that the scientists had not

[604] This bottleneck was compounded by a delay in beginning clearances during early 1946. Rosebury was still waiting for an official pronouncement about when he could submit papers in mid-February, and feared that a straight answer on the question would not soon be forthcoming. See Theodor Rosebury to John A. Lichty, February 14, 1946, in National Library of Medicine (NLM) Theodor Rosebury Papers (MS C 634), Box 3 Folder 11 (Correspondence 'L' 1 of 2).
[605] Commander William B. Sarles to L. P. Eisenhart, May 21 1945, in NAS BW Box 6 Folder 2 ("DEF Committee Meetings October 1944).
[606] Detrick researchers' publications and conference talks to November 1947 are listed in an appendix of Cochrane, *Biological Warfare Research in the United States*, pp 535-555.
[607] Theodor Rosebury, *Experimental Air-borne Infection*, Baltimore: Williams & Wilkins, 1947; Theodor Rosebury to John A. Lichty, May 14, 1946 in NLM Rosebury Papers, Box 3 Folder 11 (Correspondence 'L' 1 of 2).

translated their work into texts. Indeed, 123 authors at Detrick penned 83 "Special Reports," capstone documents on a particular topic or project by early 1947.[608] As in the case of Rosebury, who penned such a report on airborne infection research, the work of writing these reports seems to have provided these authors with a fruitful resource to be adapted for publication. This is particularly true because the bulk of these reports were completed in the immediate post-war months of September 1945-March 1946, just as suddenly-idle scientists would have been penning prospective publications.

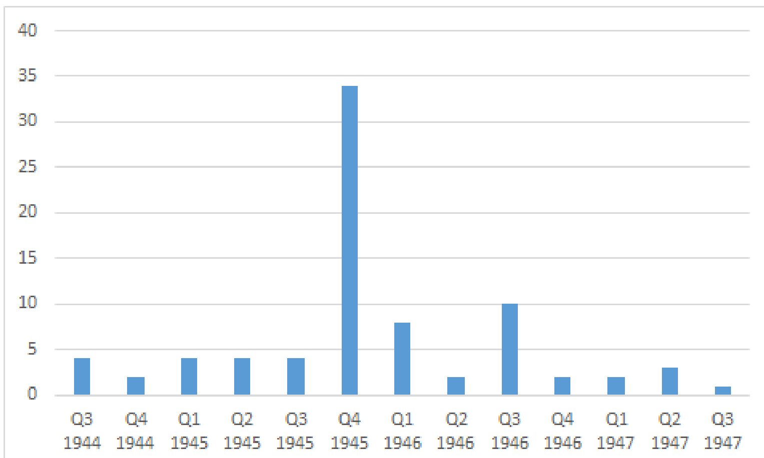

Figure 18: Detrick Reports by Quarter, 1944-1947[609]

These reports also provide us with a statistical window into the postwar publication boom. While not a perfect metric by any means, simply comparing how many pages on a topic were contained in secret reports versus how many were published in this period allows some insight into just how open 1946-1947 was in practice. The key point underlying this comparison is that the secrecy system sought to control texts, not prevent them from being written: if anything, the regimented and unprecedentedly large scale of

[608] A list of these special reports (with titles and page counts) can be found in an appendix to Cochrane, *Biological Warfare Research in the United States*, pp 527-534.
[609] Ibid.

Detrick encouraged the production of texts within the secret world. If a topic had had a great deal written about it in reports but little was published on it, this is a good hint that the secrecy system was particularly unfriendly to releasing information on the topic. Likewise, a high report-to-publication ratio on a topic indicates that the secrecy system was less concerned about keeping control of information about it. A total of 1314 pages of journal articles (and Rosebury's monograph) were published by American bioweapons researchers in 1946-1947, which is 25.6% of the 5132 pages' worth of internal reports that had been produced by 1947.

Table 2: Detrick Reports vs Publications, by Subject[610]

Subject:	Report Pages:	Published Pages:	Percentage:
Antibiotics	34	69	202.94%
Aerobiology	209	246	117.70%
Herbicides	404	199	49.26%
Physiology	37	18	48.65%
Brucellosis	250	87	34.80%
Lab Technique	69	24	34.78%
Neurotropic Encephalitis	12	4	33.33%
Tularemia	418	127	30.38%
Serratia marcescens	38	11	28.95%
Botulinum toxin	466	127	27.25%
Coccidioidomycosis	60	15	25.00%
Psittacosis	305	75	24.59%
Anthrax	845	166	19.64%
Glanders	185	35	18.92%
Plant Diseases	537	61	11.36%
Bacillus globigii	38	4	10.53%
Safety	776	46	5.93%
Munitions	276	0	0%
Production	173	0	0%
TOTAL	***5132***	***1314***	***25.60%***

[610] Statistics calculated from Ibid.

These data can be divided into rough topical categories: 1) munitions and production, topics with no publication, 2) safety, and to a lesser extent, plant diseases and *B. globigii*, with poor publication rates 3) most species-specific topics and laboratory technique (in the 19%-34% range), 4) physiology and herbicides, in the 50% range 5) aerobiology and antibiotics, "supersaturated" topics in the >100% range. Some general trends can be observed in these data. For example, the most immediately 'military' of information was simply not published, but this did not include aerobiological data from Rosebury's Cloud Chamber project, which (thanks to having its work published in monograph form) actually had a higher published page count.[611] As a corollary, Rosebury's monograph was the only one to emerge from Detrick's wartime work, and the high published page count produced by his project probably reflects what he recalled as his group's strong preparation to seize the opportunity presented by the briefly liberal 1946-7 publication regime. The high page count of pesticide work probably represents an equivalently good preparation by that group; it is certainly an ironically good rate of publication for the closest thing to an operationally deployed category of weapon to emerge from Detrick during the war.[612] This is an interesting contrast to plant disease data. Meanwhile, poor safety and *B. globigii* rates contrast unfavorably with their subsequent strong performance over Detrick's *longue durée*: perhaps this simply reflects the relatively large amount of ink spilled on internal reports by Detrick's Safety Division (776 pages, or 10% of Detrick's total)? Finally, roughly a quarter or more of the report page count of research on most organisms (and laboratory techniques) was published: a

[611] Rosebury, *Experimental Air-borne Infection*.
[612] See Barton J. Bernstein, "America's Biological Warfare Program in the Second World War," *Journal of Strategic Studies* 11 (1988), pp 292-317 for a discussion of late war planning to use pesticides for crop-destruction "biological warfare" against Japan.

fairly good ‘return on investment’ for scientists seeking open recognition for wartime research. It is possible, of course, that this number is unrepresentative: if groups which had carefully written up results as special reports were the only ones prepared enough to publish, for example, we would see an unrepresentatively high rate of publication compared to Detrick research as a whole. However, the fact remains that over a hundred individual authors published something coming out of the war. For most of them, this was at least something to show for the war years as they returned to their civilian careers. For some who remained at Detrick, this publication record represented an ongoing link to the ‘open’ scientific community.[613]

This link would be threatened as the 1940s wore on and the Cold War began to chill US-Soviet relations. By the late months of 1947, the relatively liberal paper-clearance regime reigning at Detrick clamped down as government officials came to regard their biological weapons program as a political embarrassment (see below). Fewer than half of the papers published in 1946 and 1947 were published by Detrick researchers during the next two years.[614] “If all of the researchers at Detrick had recognized the importance of publication as I did, and had moved as quickly as I did, once the coast was clear, to get their work ready for publication, more would have been published than actually was,” reminisced Rosebury in 1949.[615] This re-tightening of secrecy threatened

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[613] Rosebury later pointed out to one correspondent an important implication of work being affirmatively secret unless it was published. “If I know of University research which was never published, I can speak of it and perhaps stimulate others to repeat and publish it; with secret research, I cannot mention it, even though there is no conceivable reason for secrecy in regard to it, unless I know that it has been published.” Theodor Rosebury to Walter Gellhorn, July 19, 1949, in NLM Rosebury Papers, Box 4 Folder 19 (Correspondence ‘T’ 2 of 2). In short, open publication was not merely an important link between a Detrick scientist and their larger community, it was veritably the only one.

[614] See Figure 19 below.

[615] Rosebury to Walter Gellhorn, July 19, 1949, in NLM Rosebury Papers, Box 4 Folder 19 (Correspondence ‘T’ 2 of 2).

Detrick researchers' links to the open scientific community, both by making it more difficult to publish and by tying their careers to the secret world because this difficulty publishing also made it more difficult to build a scientific reputation. The degree of secrecy attached to biological weapons was particularly concerning. Virtually all information about biological warfare had been declared Top Secret in 1944.[616] Though the most onerous procedures implied by this classification had been ignored as a matter of policy during the war (see below), they were now in full force, meaning that these scientists' work was now suddenly being held at the highest level of secrecy the government possessed.

So extreme was this *de facto* growth in secrecy that exasperated scientist-advisors working for the military's Joint Research and Development Board, a kind of spiritual successor to the NAS committees, soon began a successful lobbying campaign for these restrictions to be loosened, for both their own sake and that of the researchers. "We recommended a more realistic classification than was then true," recalled Ira Baldwin, former Scientific Director of Detrick and now chair of the JRDB's "Committee X" on biowarfare.[617] "The classification at that moment was "Top Secret" on even the words "BW" and various documents which had been issued publicly had later gotten classified as "Top Secret." The restrictions were so great it was practically impossible to operate. So that the Committee was asked to develop a realistic classification policy; that was developed and did pass this Committee with the recommendation it be adopted. It was adopted in essentially the form that the Committee passes it and is still the policy which

[616] W. Mansfield Clark to Members of the ABC Committee, April 17, 1944 in NAS BW Box 3 Folder 16 ("ABC Committee Chairman Clark, W. Mansfield 1944").
[617] "Committee X" was so named to keep its subject matter obscure.

we have."[618] Besides the practical difficulties they faced in dealing with too many Top Secret documents (see below), the JRDB advisors were concerned that if microbiology was bifurcated into secret and open communities the way physics was, the program at Detrick would ultimately suffer for it. The committee estimated in late 1947 that "between five hundred and six hundred technically trained people would be required to carry on an effective program covering all phases of biological warfare research and development."[619] The "effective program" they described was aspirational, with a nearly doubled research budget (of $4 million, up from $2.2 million), reopened testing and production facilities, and research on multiple candidate organisms being pursued in parallel. Even this optimistic report, however, was skeptical about whether these personnel needs (which roughly mirrored the staffing at Detrick during WWII, when many microbiologists had been willing to take a temporary assignment in what they saw as a wartime emergency) could realistically be met without substantial continued links to the open scientific community. For Detrick to directly hire hundreds of technically trained personnel, it would have to compete with the rapidly expanding universities and state and federal public health research centers with a fairly paltry research budget even in the best case scenario. In the view of the committee, military research had to be made as attractive as possible to scientists who, fundamentally, had options. A more realistic

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[618] Excerpt from Minutes of Meeting of Committee on Biological Warfare, May 15, 1951, p 16, in American Society for Microbiology Archives (ASM), Ira Baldwin Presidential Papers, Box 13-IIAT Folder 2.

[619] Research and Development Board, "Report: Technical Aspects of Biological Warfare," August 26, 1947 p 13, retrieved from Brill Online, "Primary Source Collection: Weapons of Mass Destruction," https://primarysources.brillonline.com/browse/weapons-of-mass-destruction, Retrieved 5/15/2018. Baldwin had argued this line of reasoning since at least 1945, pointing out in that year that it would be "undesirable and impractical to attempt to carry out an adequate peace time BW project under the present security classification" because of "an unjustifiable hardship on personnel." See Ira L. Baldwin, Memo to Capt. L. F. Fothergill, Subject: Considerations in Planning for a Peace Time BW Program, August 25, 1945, p 2 in University of Wisconsin Archives (UWA) Ira L. Baldwin Papers (Series 9/10/11), Box 11 Folder 2.

suggestion than convincing hundreds of new researchers to work at Detrick would be “to increase the amount of work under contract with university and other laboratories,” leaving “the facilities at Camp Detrick… for work that cannot be done outside a military installation.”[620] Research contracts, however, also faced competition from contracts and grants offered by institutions like the NIH and (after 1952) the National Science Foundation.[621] Here as well, too much secrecy would make it hard to secure research talent. In his study of the AEC’s National Laboratories, Peter Westwick discusses similar problems the laboratories faced in attracting physicists due to secrecy, from the petty annoyances of dealing with classified documents to scientists’ concerns about long-term career prospects working on research that was ‘born secret’ under the Atomic Energy Act. These were ultimately not serious impediments however: the laboratories and classified university research thrived in the late 1940s and 1950s, developing what Westwick characterizes as a classified scientific community parallel to the ‘open’ one, complete with its own secret journals and conferences.[622] The crucial reality Committee X faced at the same time, however, was that Detrick’s budget was measured in millions, not billions of dollars, microbiology research was in demand outside of the military, and microbiologists’ work was not ‘born secret’ under a statute that threatened the death penalty for revealing it. Indeed, this last point was a positive one for them, representing a tightrope that microbiologists who advocated biological weapons research always tried to walk between being taken seriously and not being taken *too* seriously (see below). The point, however, was that the National Laboratories described by Westwick carved out

[620] Ibid.
[621] See Toby A. Appel, *Shaping Biology: The National Science Foundation and American Biological Research, 1945-1975*, Baltimore: Johns Hopkins University Press, 2000.
[622] Westwick, *The National Labs*.

effective classified communities because they were in a position to effectively annex whole fields of physics.[623] Detrick was in no such position, and recognizing this, the scientists advising the military always tried to restrain the barriers imposed by secrecy.

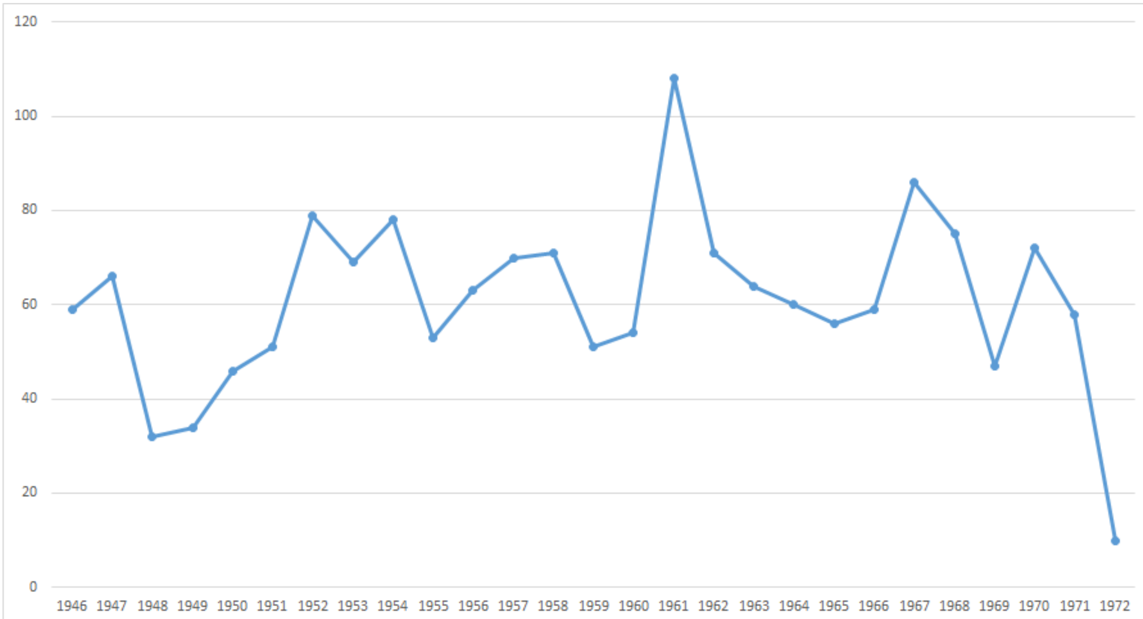

Figure 19: Detrick Publications by Year, 1946-1972[624]

The military took this advice to heart, and ironically beginning in the crisis years of the Korean War, the Detrick publication rate began to recover. For the remainder of the offensive biological weapons program's existence, Detrick's annual publication rates would fluctuate, but typically remained in the area of 55-80 publications a year (see Figure 19, above). Not all topics were fair game: research on airborne infection, for instance, was recognized as militarily significant and little was published on it after 1947

[623] Ibid. Another such field of physics was solid-state physics, whose 'militarization' (through extensive federal funding) is an eloquent case study for Paul Forman. See Paul Forman, "Behind Quantum Electronics: National Security as Basis for Physical Research in the United States, 1940-1960," *Historical Studies in the Physical and Biological Sciences* 18 no 1 (1987), pp 149-229.

[624] Enviro Control, Inc, "Scientific Publications, Fort Detrick 1946-1972," Rockville, MD, 1976, held at ASM Archives.

until a deliberate campaign for openness on the subject began in the early 1960s.[625] Nonetheless, Detrick researchers published a number of remarkably candid papers, on topics ranging from freeze-drying plague to detailed discussions of the course and treatment of respiratory anthrax in experimental animals.[626] Outside researchers collaborating with Detrick on some projects also published information directly relevant to biological warfare planning, such as detailed case histories of Detrick personnel accidentally infected with brucellosis.[627] Such papers did not discuss the military potential of this information, with merely the authors' listed affiliation typically tying them to Detrick. This was a reflection of a tacit compromise in the culture of secrecy surrounding biological warfare (discussed below), in which public acknowledgement of the topic was severely frowned upon while relatively liberal opportunities to publish unvarnished scientific results placated the scientists.

It is unclear what percentage of the total scientific work at Detrick this publication record represents, but it nonetheless seems that it was much less than a majority.

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[625] See Justin M. Andrews, "Report of August 7th meeting in the Surgeon General's Office of representatives of the Army Chemical Corps and the U.S. Public Health Service to discuss aspects of biological warfare research and education," August 11, 1959, in in Johns Hopkins University (JHU) Chesney Archives Collection LanA (Alexander Langmuir Papers), Box 1, Unnumbered Folder Entitled "ADL: Miscellaneous Materials from CDC (Personal)." This campaign of openness was led by an open conference in late 1960, followed by a series throughout the decade. See Walsh McDermott (ed), *Conference on Airborne Infection held in Miami Beach, Florida, December 7-10, 1960. Sponsored by Division of Medical Sciences, National Academy of Sciences-National Research Council*, Baltimore: William & Wilkens, 1961; Naval Biological Laboratory, *First International Symposium on Aerobiology*, Berkeley: Naval Biological Laboratory, 1963; *Bacteriological Reviews* 30 no 3 (Special Issue: Second International Conference on Aerobiology (Airborne Infection), (1966); Ibhar Hall Silver (ed), *Aerobiology: Proceedings of the Third International Symposium held at the University of Sussex, England, September 1969*, London: Academic Press, 1970.

[626] R. J. Heckly, A. W. Anderson, and M. Rockenmacher, "Lyophilization of *Pasteurella pestis*," *Applied Microbiology* 6 no 4 (1958), pp 255–261; W. S. Gochenour, C. A. Gleiser, and W. D. Tigertt. "Observations on Penicillin Prophylaxis of Experimental Inhalation Anthrax in the Monkey," *Journal of Hygiene* 60 no 1 (1962), pp 29-33.

[627] Robert W. Trever, Leighton E. Cluff, and Richard N. Peeler, "Brucellosis I. Laboratory-Acquired Acute Infection," *AMA Archives of Internal Medicine* 103 no 3 (1959), pp 381-397.

Investigative journalist Seymour Hersh, who wrote a book on American CBW research in 1968 (a few years before his Pulitzer Prize-winning reporting of the My Lai massacre), claimed that "only about 15 per cent of the papers produced at Detrick ever get the necessary clearance needed before publishing" but gave no direct source for this claim.[628] Unlike with the WWII era, a comprehensive and declassified list of 'grey literature' reports produced at Detrick does not exist. However, a somewhat cruder metric can be gleaned from a 2014 Army Corps of Engineers environmental impact report on Detrick, which drew in part on archival research with access to still-classified Detrick documents and reports. This report itself is unclassified, with any information in it that was gleaned from classified documents having been declassified before inclusion in it.[629] It thus provides a helpful comprehensive overview of the *existence* of documents which the general public does not have access to, even if details like titles and page counts are not available. Most notable of these is an unclassified but publicly unavailable collection of about 6,150 handwritten Detrick laboratory notebooks from 1943 to 1971 (of which about three-quarters is apparently still extant). These notebooks (averaging about 219 notebooks/year) were sequentially numbered and issued to individual researchers. Detrick researchers produced reams of grey literature as well: writing, from 1943 to 1958, 168

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[628] Hersh mentions "one top government scientist" who he interviewed in April 1967, who told him that "we've been encouraging [Detrick] to loosen up on their information policy" later in the same paragraph, but does not explicitly connect the 15% figure to this source. See Seymour Hersh, *Chemical and Biological Warfare: America's Hidden Arsenal*, Indianapolis: Bobbs-Merrill, 1968, p 273. Hersh, who remains an active journalist today, has been prone in recent decades to making controversial claims often based on anonymous sources, though what this implies about the credibility of his work in the 1960s is a matter of opinion. At the very least, he seems to have developed good access to opponents of biological weapons research in the late 1960s, both inside the Pentagon and within the scientific community. For his correspondence with anti-BW activist Theodor Rosebury, see Seymour Hersh to Theodor Rosebury, May 29, 1967; Rosebury to Hersh, June 5, 1967 and other letters in NLM Rosebury Papers, Box 3 Folder 6 (Correspondence 'H' 3 of 3).

[629] U.S. Army Corps of Engineers St. Louis District, "Archives Search Report Operational History for Potential Environmental Releases Fort Detrick," June 16, 2014.

“Interim Reports,” 29 “Status Reports” (between 1953 and 1955), and 289 sequentially numbered “Special Reports.” After 1958, this system was replaced by an expanded topology of “Technical Reports,” “Technical Studies,” and “Technical Manuscripts,” the last of which most closely corresponded in size and form to a publishable scientific paper.[630] This precession of statistics doesn’t give us much quantitative insight: the page length and scientific significance of all of this writing varies so much that it would be foolish to compare it with Detrick’s open publication record in any detail. The broad point, however, is that the volume of both secret and unclassified but publicly unavailable written work produced by Detrick was at least as large as the base’s volume of open publications, and (given how much longer these reports could be than a published paper) was very probably significantly larger. The impressive rate of publication that did happen, moreover, did not benefit Detrick scientists equally. 1202 individual authors had their names on the 1632 papers published between 1946 and 1971, but 47% of these authors published only one paper stemming from their Detrick career. Just 21 scientists had their names attached to over half of the papers published.[631]

A microcosm of the publication situation can be found in a 1965 Detrick report on a commissioned study of scientific communications at the Fort.[632] This report gives a total of 1,245 “scientist-technician” personnel at Detrick as of October 31, 1964, divided between 150 “Medical Research,” 475 “Biological Research,” 288 “Development,” and 332 “Technical Services” personnel.[633] 60 scientific papers were published by Detrick

[630] These statistics are from Ibid, pp 9-10.
[631] Statistics calculated from “Scientific Publications, Fort Detrick 1946-1972.”
[632] George H. Nelson and Donald M. Hodge, “Biological Laboratories Communication (Fort Detrick Miscellaneous Publication 13),” Fort Detrick: United States Army Biological Laboratories Technical Information Division, 1965.
[633] Table 1 in Ibid, p 11.

employees that year (followed by 56 in 1965). If we assume that most publishable scientific research (rather than, for example, engineering development work) was done by the 625 "Medical" and "Biological Research" personnel, then Detrick published a little under one paper per 10 research scientists in 1964. If we include the 560 "Development" and "Technical Services" personnel as possible paper authors, this number is cut in half, to one paper for every 21 people that year.[634] The number of authors per paper in 1964 was much lower than either figure: averaging 2.3, with no papers having more than six authors.[635] A large majority of authors only published one paper in 1964, and the statistics above heavily imply that most Detrick scientists did not publish anything that year. Furthermore, this does not seem to have been an even distribution by subject: with 9 papers pertaining to laboratory safety, even though the Safety Division was so small that only 2 of the 138 survey respondents listed their specialization as "Safety." The survey also asked the respondents about their publication record over the past 5 years. A substantial number did report authoring journal articles, book chapters, or the like, with only 21 of the 138 explicitly stating that they had not published anything (see Figure 20 below). Assuming no-one gave two contradictory answers for the number of journal articles they had written, 82 respondents of the 138, or 59%, had published an article in the past 5 years. (Incidentally, Detrick employees published 408 items in 1959-1964, including a spike in 1961 associated with the December 1960 aerobiology conference.) However, it is extremely doubtful that all of these publications were done at Detrick: 63, or 45% of the survey respondents, had started work at Detrick in the last 5 years. Almost

[634] It seems highly plausible that this is the case, as the 2 respondents to the survey who listed their specialization as "Safety" were classified as "Technical Services" personnel, and the Safety Division was quite prolific in 1964. See Table 3 in Ibid, p 17.
[635] Statistics taken from 1964 bibliographic entries in "Scientific Publications, Fort Detrick 1946-1972."

certainly some of these publications predated their author coming to Detrick, having contributed to a scientific record that helped get the author the Detrick job in the first place. Perhaps more pertinently, the majority of those who had published had 1-5 papers under their belt, an average of no more than 1 paper a year. In short, these statistics suggest that many Detrick employees in the mid-1960s had an extant but thin publication record, with some of the few papers they had published predating their employment there.

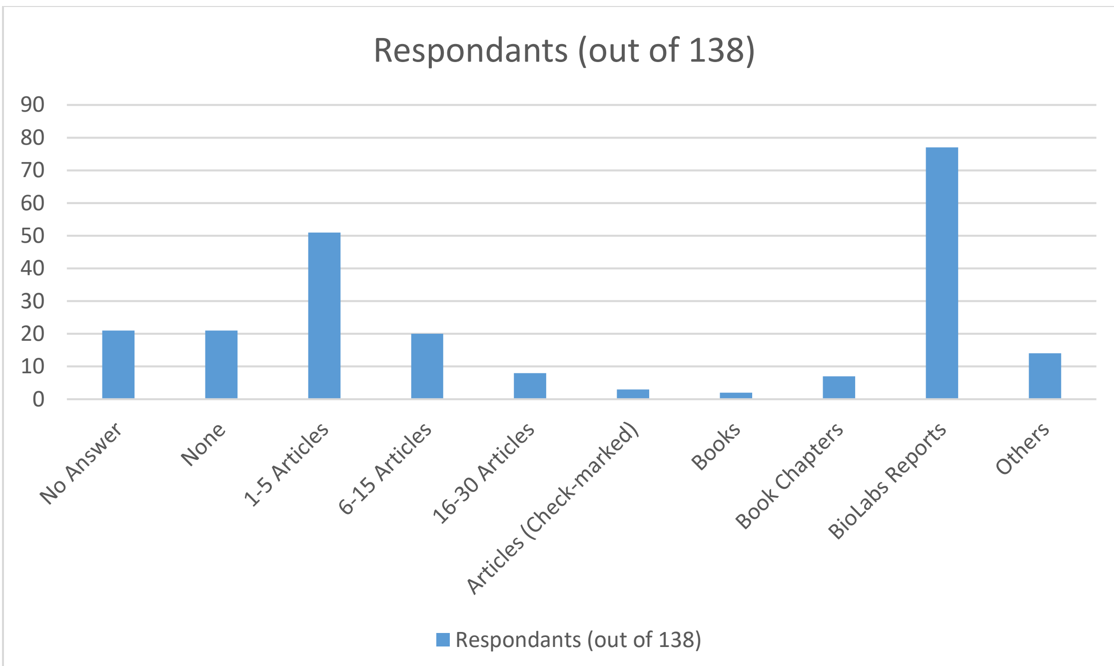

Figure 20: Detrick Publication Activity, 1965[636]

Another point emphasized by the 1965 report and its statistics is how much energy went into internal classified or 'grey literature' documents, with roughly as many respondents (77) having written such reports in the past 5 years as had published open papers. Indeed, as the report authors editorialized, it was "unfortunate" that "many seem

[636] Nelson and Hodge, "Biological Laboratories Communication," p 31.

to place a high value on journal publication because they believe promotion, salary, professional recognition, etc. depend on such publication." After all, the authors argued, "a well-written formal government report can be a meaningful contribution, and can receive very wide announcement and distribution, even exceeding that of certain journals. Surely those who listed security as a reason for not publishing should realize that security considerations do not restrict their publishing formal government reports."[637] The report argued, in short, that the opportunity existed to be a member of a government-sponsored scientific community parallel to the "open" one, like that described among National Laboratory physicists by Westwick.[638] However, the fact of the matter was that such a community was evidently seen as a poor substitute for open publication by Detrick scientists, emphasizing the greater ties they had with their 'open' colleagues. As the report's authors complained, at Detrick "'did not publish' meant to many 'did not publish in journals.'"[639]

This, then, was the uneasy compromise which prevailed at Detrick for almost two decades. The need to have secret work cleared for publication was a serious frustration for many researchers, who would be lucky to navigate the process more than once. The fact that this possibility existed at all, however, reflected the ideal of Detrick retaining its links to the 'open' scientific community, rather than retreating into a classified community like the physicists. Between this ideal and the disappointing reality, Detrick managed to keep itself staffed, fulfilling the original pragmatic purpose of publication. The service to the scientific community embodied by publication was also an

[637] Ibid, p 32.
[638] See Westwick, *The National Labs*.
[639] Nelson and Hodge, "Biological Laboratories Communication," p 32

increasingly useful talking point for its leadership as its existence became more controversial in the late 1960s. Detrick's Scientific Director Riley Housewright, for example, often touted Detrick's record of publication in debates within the American Society for Microbiology (which he also served as the president of in 1966, see Chapter 2, above). For all that the argument that Detrick researchers had been good scientific citizens fell flat in the late 1960s, it reflected a reality of previous years, in which 'open' scientists like Joshua Lederberg had been happy to deal with Detrick scientists like any other colleague.[640] Even after the offensive biological weapons program was ended in 1969, program leaders continued to use this publication record as a rhetorical weapon, for instance commissioning a comprehensive unclassified list of publications from which I have drawn the statistics above.[641]

The Materiality of Secrecy

The clearance process emphasizes how the formalized military secrecy system was not so much about *keeping* secrets as it was *preventing* information from leaving the classified world. All of the reams of internal documents produced by Detrick scientists were *prima facae* secret, regardless of their contents. When these scientists composed papers for publication, the burden was on them to navigate the onerous process of declassification and gain affirmative assurance that the precise words and phrases in the paper could be seen by outside eyes. As observers of American secrecy (even 'insiders'

[640] Lederberg drew upon his preexisting contacts with Detrick researchers to enlist their expertise for space probe sterilization in the early 1960s, for instance. See e.g. Joshua Lederberg to Riley Housewright, July 23, 1959 in Joshua Lederberg Papers, NLM Profiles in Science (NLM ID: 101584906X6189). See also Audra J. Wolfe, "Germs in Space: Joshua Lederberg, Exobiology, and the Public Imagination," *Isis* 93 no 2 (2002), pp 183-205.
[641] "Scientific Publications, Fort Detrick 1946-1972." This report was produced by a contractor for Dugway Proving Ground.

like Arvin Quist) have noted, this arrangement has a strong inertial tendency to keep innocuous information buried, as few of the vast reams of documents produced in the secret world ever reach a declassifier's eyes in the first place.[642] Though often-criticized, however, this division of the world into a walled garden of information to be protected and the unregulated outside of public knowledge has prevailed for over three-quarters of a century, with the proverbial garden growing ever more tangled and packed every year.[643]

There is another way of looking at this system, by remembering that the "information" regulated is embodied in the form of physical texts. Following the example of Matthew Hull's ethnographic scholarship on the materiality of bureaucracy, we can gain a new prospective if we analyze the military secrecy system not as the keeping of information, but as the keeping of documents.[644] Internal texts like the Special Reports were specific pieces of paper held in specific spaces which were trusted to not allow unauthorized eyes in. The process of declassification permitted copies of a specific document to be mailed to journal editors, reviewed by (uncleared) peers, and ultimately read by any member of the unruly public. This focus on materiality is a fine distinction

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642 Quist compares the costs of declassification, a "hidden cost" of the initial classification decision, to those of industrial waste cleanup. See Quist, *Security Classification of Information*, p 151.

643 Galison, "Removing Knowledge." More general scholarship on "classification" in science and technology is more than metaphorically relevant to this conceptual division of the world into secret and non-secret realms. See e.g. Geoffrey C. Bowker and Susan Leigh Star, *Sorting Things Out: Classification and Its Consequences*, Cambridge, MA: MIT Press, 1999.

644 Matthew S. Hull, *Government of Paper: The Materiality of Bureaucracy in Urban Pakistan*, Berkeley: University of California Press, 2012. See also Ben Kafka, "Paperwork: The State of the Discipline," *Book History* 12 (2009), pp 340-353; Matthew S. Hull, "Documents and Bureaucracy," *Annual Review of Anthropology* 41 (2012), pp 251-267. Any discussion of bureaucracy and secrecy must acknowledge the work of Max Weber, who argued that secrecy granted the differential access to information between bureaucrats and the general public that was one of the foundational elements of bureaucratic power. See Max Weber, *Economy and Society: An Outline of Interpretive Sociology* trans. Guenther Roth and Claus Wittich, Berkeley: University of California Press, 1976 (1922). It is precisely this reified view of rationalistic bureaucratic control over information (rather than material documents) that Hull is reacting against, however.

from one on information when focused on a secret space like Detrick, but becomes more important when examining how the secrecy system dealt with more-liminal spaces like the universities in which contract researchers and scientific advisors were housed.[645] These latter scientists, especially, were crucial components both for supporting the bioweapons program within military deliberations and in maintaining ties between the classified and open scientific communities (see Chapter 2). Studying how the secrecy system incorporated these hybrid figures gives us a particularly useful perspective on the practical realities of the system.

The military secrecy system was something of a culture shock for civilian scientists encountering it for the first time during WWII. Initially, the scientists working for the NAS effectively managed their own affairs, and simply kept their mouths shut and their secret documents from prying eyes. As direct military involvement with bioweapons research grew in 1943-1944, however, military practices of secrecy became *de rigor*. Regulations governing the minutiae of how secret documents were to be handled according to a set of classification levels entered the scientists' lives, particularly if they were part of the geographically dispersed network of advisors and contract researchers who continued to work away from the central facilities of Camp Detrick. This encounter with a tightly regulated secrecy system was more of a culture shock than the mere keeping of secrets themselves. The scientists had accepted secrecy as a wartime exigency.

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[645] See Stephen Hilgartner, *Science on Stage: Expert Advice as Public Drama*, Stanford: Stanford University Press, 2000 for a discussion of the maintenance of privileged (and thus secret) spaces through seemingly-mundane "technologies of privacy" like door locks and pass cards. Secret spaces, and differential access to them based on security clearances, also loom large in Hugh Gusterson's ethnography of nuclear weapons research at the National Laboratories. See Hugh Gusterson, *Nuclear Rites: A Weapons Laboratory at the End of the Cold War*, Berkeley: University of California Press, 1996. Secret spaces are likewise the approach taken by Trevor Paglen, *Blank Spots on the Map: The Dark Geography of the Pentagon's Secret World*, New York: Penguin, 2009.

More importantly, the ‘openness’ of science did not mean that they were not acculturated to concealing information- priority and credit could ride on being coy about laboratory results *until* results were complete enough and written up for publication. It was the fact that military secrecy was a different culture of concealment, seemingly more devoted to carrying out a long list of proscribed practices than actually concealing knowledge effectively, that the scientists found vexing. The ritualistic use of classification stamps, for instance, was an effective way of impressing upon those in the system that particular pieces of paper should be treated in a particular way, but as some scientists observed, stamps could also draw increased attention to a document should it fall into unauthorized hands. As Cornell’s William Hagan wryly noted, “Most of the stuff I have would be of no interest to anyone if it were not for the fact that red stamps “SECRET” advertise the fact that they are of special significance. Why wouldn’t a red star or some other symbol, known to the initiated, serve the purpose without exciting the curiosity of others who might, from some kind of accident, come into possession of one of the documents. When you see F.D.R. the next time, tell him that I think his present plan is all wet.”[646] Fortunately for these scientists, some of the secrecy regulations were enforced laxly, either informally or as a matter of policy well into the 1950s. By the late 1950s and 1960s, however, enforcement became more rigorous and proscribed procedures became more stringent. The result was that for scientist-advisors and contract researchers, working within the secrecy system became more onerous and prone to the

[646] W. A. Hagan to E. B. Fred, November 13, 1942, in NAS Box 7 Folder 23 (“Hagan, William Arthur: 1942-1943”).

inconveniences of following the strict letter of often-revised rules over time.[647] This increasingly isolated Detrick just as it most needed civilian allies in the late 1960s.

Words were the atoms of the classified universe, documents the compounds. Particular words or combinations of words could in theory be subject to classification, but in practice, the classification process principally acted upon documents. Those with the authority to classify a document at a particular level theoretically possessed official stamps, which they affixed to the document in question (though in practice, particularly for civilian scientists, the minor crisis of a requisite stamp not being available could occur).[648] Documents were typically classified at the highest level borne by the phrases it contained: for example, an otherwise innocuous document containing a single direct reference to the planned offensive use of biological weapons would probably have been classified Top Secret throughout the 1950s. A document or phrase was classified one of several categories: "Restricted" (a category discontinued in 1954), "Confidential," "Secret," or "Top Secret" (a category introduced during WWII to align with the British classification scheme).[649] Though precise definitions varied with frequent revisions to the scheme, generally speaking information whose revelation risked starting or losing a

[647] Joseph Masco has characterized the similar ritualized strict adherence to clearance regulations at Los Alamos in the late 1990s as "hypersecurity," and has emphasized its importance in reinforcing the culture of the lab. Joseph Masco, "Lie Detectors: On Secrets and Hypersecurity in Los Alamos," *Public Culture* 14 no 3 (2002), pp 441–467.

[648] For example, Baldwin, who had the authority to classify his own documents, did not always have an appropriate stamp on hand, leading him to make a point of having his military correspondents stamp documents "rather than having the classification written with red ink." Baldwin to Lt. Col. L. F. Paul, 10 September, 1947, in UWA Baldwin Papers Box 15, Folder 2. Baldwin sent this letter with an attached document which needed to be stamped, having stamped the letter itself "Secret," (according to a later pencil ms). This was a fairly standard classification for Baldwin's RDB documents, suggesting that the specific details of the document he attached to the letter merited the next-higher "Top Secret" designation, and thus that this was the stamp he did not have. Baldwin was scrupulous about clearing his files of classified documents: the attached document is not within his University of Wisconsin papers, while the letter, as the handwritten ms on it notes, was declassified in 1971 (like all of the other formerly classified documents currently held within his papers).

[649] See Chapter 4 of Balmer, *Secrecy and Science*, pp 57-74.

major war was considered Top Secret, that which could be less- but still seriously damaging was Secret, while Confidential or Restricted information posed less, but non-negligible, risk of harm to American interests.[650] In parallel to this hierarchy after 1946 was nuclear knowledge, "born secret" as so-called "Restricted Information" under that year's Atomic Energy Act. A "Q" clearance, roughly equivalent to a Top Secret clearance, was necessary to access such information.[651] The general secrecy scheme was an inter-organizational and ultimately international system, with attendant compromises and controversies.[652] It was first established by amalgamating Army and Navy secrecy practices in a 1942 executive order, and for specific subjects like biological warfare was soon after aligned with close allies' systems, particularly that of the British. Within this system, individuals held security clearances to view documents on a particular subject up to a particular classification level, which were often administered by specific organizations producing those documents like the US Army Chemical Corps. Appropriate clearance-holders were a virtual audience for a classified document, but this anticipated audience could be disrupted by discontinuities within the system- for example, when the US discontinued the Restricted category, British documents using that category were automatically upgraded to Confidential, significantly restricting their intended American audience.[653] Documents might also be classified strategically to

[650] See Quist, *Security Classification of Information*, for the changing definitions of these categories. See also "Classification of Matter Concerning Biological Warfare," September 4, 1945, in NAS BW Box 6 Folder 4 ("DEF Committee Member: Fred E B: 1944-1945"). The classification status of specific categories of documents in the wartime BW program is explicitly discussed in "DEF Committee Meeting of 12 October 1944, 10 a.m., the National Academy of Sciences," p 7 in NAS BW Box 6 Folder 2 ("DEF Committee Meetings, October 1944").
[651] Wellerstein, "Knowledge and the Bomb."
[652] Balmer, *Secrecy and Science*, p 60; Jeroen van Dongen (ed), *Cold War Science and the Transatlantic Circulation of Knowledge*, Leiden: Brill, 2015.
[653] Balmer, *Secrecy and Science*, p 60.

increase or decrease the size of this anticipated virtual audience.[654] The often-decried and rarely-remedied phenomenon of 'overclassification' similarly impeded documents from reaching their full intended audiences. This phenomenon- in which the original person classifying a document erred on the side of caution by applying the more extreme of two possible ratings- was accentuated by the proliferation of the authority to make such classifications. Individual scientist-advisors like Ira Baldwin, for example, were given the authority to classify documents that they produced.

Scientific advisors on biological warfare often held high enough clearances as a matter of course (typically Top Secret or sometimes Secret) that their community wasn't significantly affected by this impediment. The material realities of the classification scheme, in contrast, made its vagaries ever-present in their lives. The way the secrecy system ultimately assured that a document could only be viewed by people who were cleared was to regulate how documents were physically stored and transmitted. Documents exist in physical space, and repositories of classified documents within circumscribed spaces like the Pentagon or Detrick's Technical Library could in a sense be said to represent the purest ideal of the secrecy system as an ordering of space. Not all classified documents could exist in such spaces, however. Documents needed to be transported between geographically disparate 'secure' spaces, and when civilian scientists who engaged in part-time classified consulting work or contract research were involved, these documents needed to exist in spaces in the 'open' world like university campuses. For all that the secrecy system was based on ritualized trust of specific people like these

[654] For example, one Pentagon committee chose to classify the 1949 "Haskins Report" on the efficacy of biological warfare as "Secret" rather than "Top Secret," despite fitting in the latter category, with the explicit intent of increasing its audience within the government. See Edward Wetter to I. L. Baldwin, June 27 1949, in UWA Baldwin Papers, Box 14 Folder 3.

scientists, this trust did not presume that they guarded their documents at all times. Consequently, rituals of trust in artifacts and buildings accompanied individuals' security clearances, to allow spaces of secrecy to exist as proverbial homes away from home for classified documents.

Anyone who was entrusted with Restricted or Confidential documents was expected to keep them secure, for example in a locked filing cabinet, as well as to sign receipts to establish a chain of custody of the documents.[655] Secret and Top Secret papers, meanwhile, were required to be stored in a safe whose combination was known only to the trusted individuals. This safe and the office in which it was housed were subject to the scrutiny of military officials.[656] Appropriate safes were bulky and expensive, and required additional physical and social infrastructure to support them, like reinforced office floors and university-employed "security officers." Furthermore, these standards of acceptability narrowed over time. The safe the NAS used for BW documents lay unattended in the Academy's offices for two years in the mid-1940s, and the people who knew the combination were scattered to the winds. By the late 1960s, meanwhile, even visitors to an office containing a safe had to be scrupulously logged and reported by a staff guarding it at all times, and Ira Baldwin's safe, which he had used to store Top Secret documents for over twenty years, was declared insufficient protection for even Secret documents in one of the now-biannual inspections.[657]

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655 F. H. Richardson, "Memorandum to Members of Committees and Panels, Subject: Security of Secret Material," January 22, 1948 in UWA Baldwin Papers, Box 15 Folder 2.

656 Ibid. Baldwin and the other members of Committee X from the University of Wisconsin turned in reports about their filing cabinets in response to this memo. See Ira Baldwin, Untitled Statement on Filing Cabinet, February 23, 1948, in UWA Baldwin Papers, Box 15 Folder 2.

657 JAF to Ira Baldwin, July 21, 1967 in UWA Baldwin Papers, Box 16 Folder 8.

These facilities were symbolic reminders for university-bound professors that they were doing secret work, as well as simply being inconvenient. Neither pleased some. For example, Ohio University's N. Paul Hudson, who had worked at Detrick during the war, objected in 1947 when Ira Baldwin tried to rope him back into the classified world as an advisor for the Pentagon's Joint Research and Development Board. "I just don't like the secrecy business," he explained, "both in scientific affairs and in personal responsibilities. We were galled by it during the war, but that was a war necessity that seemed to justify the contradictions to science and personal wishes."[658] When Baldwin pressed him further, however, he admitted that "there is the further practical matter that facilities here do not provide for proper safeguarding of classified papers" which would be an equally major challenge to such an appointment.[659] Baldwin subsequently spent much more time assuring him that "the matter of handling classified documents can be taken care of without difficulty" than he did assuaging Hudson's abstract objection to secrecy.[660] Hudson subsequently accepted the assignment. By the increasingly bureaucratized mid-1960s, workarounds like those proposed for Hudson were apparently no longer available, as Baldwin was informed that his office's "facility clearance cannot be terminated without terminating [his] personal clearance."[661] Turning a corner of one's office into a small slice of the Pentagon, in short, began as an unappealing inconvenience and became increasingly more onerous over time.

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[658] N. Paul Hudson to Baldwin, July 18, 1949, in UWA Baldwin Papers, Box 15 Folder 4.
[659] N. Paul Hudson to Karl T. Compton (cc'd copy w/ ms postscript addressed to Ira Baldwin), July 28, 1949, in UWA Baldwin Papers, Box 15 Folder 4.
[660] Ira Baldwin to N. Paul Hudson, August 5, 1947 in UWA Baldwin Papers, Box 15 Folder 4. Baldwin's two major suggestions were "for you to go to Washington the day before the Committee meeting and study the documents there" or "to send documents to you by special messenger… and then the messenger can take the documents back with him after you have had a chance to study them."
[661] "Memorandum of Security Inspection by Frank W. Flesch," November 17, 1965, in UWA Baldwin Papers, Box 16 Folder 8.

Documents also had to be transported between geographically disparate locations, and it was here that a document's precise classification level had the most practical import for a scientist-advisor. Restricted and Confidential documents could be sent through registered mail almost like any of the other papers and correspondence a working academic could expect to send and receive.[662] For Secret documents, additional special procedures were required, like enclosing documents within two envelopes and formally logging who had possession of them (an increasingly onerous requirement by 1961 as new regulations that uncleared receptionists could not receive such mail were enacted).[663] Top Secret documents, meanwhile, were strictly disallowed from leaving the possession of a trusted bearer. Rather than being entrusted to the mail, they had to be hand-delivered by special messengers or carried to and from Washington by the scientist-advisor who was to hold them.[664] This requirement was at best extremely inconvenient, at worst unworkable. Some advisors would make a habit, as Baldwin had suggested to Hudson, of instead coming early to meetings in Washington to read their assigned documents there (which doubtless impacted their ability to act as independent judges of this hastily-reviewed material). So unworkable was this requirement, that when the category of "Top Secret" was established in the first place during WWII, most bioweapons topics were

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[662] This did preclude mailing such letters when post offices were closed, however. See Elizabeth McCoy to E. B. Fred, July 11, 1943, in NAS BW Box 8 Folder 14 ("McCoy, Prof. Elizabeth F.: 1942-1945").
[663] Donald L. Mcalister to Ira Baldwin, Jan 16, 1961, in UWA Baldwin Papers, Box 16 Folder 8. Baldwin kept meticulous logs of the classified documents he received. These are held in UWA Baldwin Papers, Box 16 Folder 9.
[664] These procedures are described in Frank B. Jewett to E. B. Fred, August 31, 1944 in NAS BW Box 5 Folder 22 ("DEF Committee Beginning 1944"). They were substantially the same a decade later: see William L. Owen to Ira L. Baldwin, February 10, 1954 in UWA Baldwin Papers, Box 16 Folder 1. The Pentagon's Research and Development Board's official security regulations called for "Top Secret" documents to be transmitted "by armed courier" by this time, though given how scientists like Baldwin often carried their own documents to Washington without apparently making an effort to arm themselves, it is unclear how literally this standard was enacted. See Research and Development Board, "Security Regulations, RDB 94/1," August 15, 1952, p 42. This document can be found in UWA Baldwin Papers, Box 16 Folder 2.

upgraded from Secret to the new category, but with the attached dispensation that documents containing this information could still be mailed.[665] That is to say that perhaps the greatest thing practically differentiating Top Secret from Secret documents was effectively ignored in the wartime bioweapons program!

Needless to say, if a document could be reasonably written to not merit such a high degree of classification, it would be much easier to handle.[666] Letters between advisors, in particular, would be particularly difficult to transmit by special messenger, and were commonly written to avoid explicit discussion of Top Secret and Secret topics. This often meant avoiding the use of specific words that made a discussion explicit. Specific names of bioweapons agents, for example, had been usually replaced by code letters in official documents (like "N" for anthrax), but use of these code letters themselves also made a document subject to classification.[667] To avoid having to classify their own letters, scientists used circumspect language instead. This language often drew on shared histories and the close-knit nature of the interpersonal networks in the BW world, which skirted secrecy procedures without violating them (along with reinforcing these social links in a way reminiscent of rituals in secret societies). For example, when Dugway Proving Grounds' temporary scientific director, the University of Wisconsin's Perry Wilson, planned a visit by his colleague Ira Baldwin in the early 1950s, he used interpersonal language to discuss specific pathogens without actually mentioning them.

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[665] W. Mansfield Clark to Members of the ABC Committee, April 17, 1944 in NAS BW Box 8 Folder 6 ("Jewett, Frank B. (Black Book): 1943-1944").
[666] For example, rather than attempting to circulate a Top Secret speech by courier, the Chemical Corps Advisory Council recommended that scientist-advisors read a similar unclassified speech by the same speaker. C. B. Marquand to Advisory Council Members, July 13, 1954 in UWA Baldwin Papers, Box 12, Folder 1.
[667] The use of the code letter "N" dates from 1942. See "Projects Under Study," October 14, 1942 in NAS BW Box 2 Folder 1 ("WBC Committee Projects 1942").

When discussing necessary immunizations for the visit, Wilson could note that "if you can come out in August we will still be working with the material Dr. Huddleson works with and also the type of material that Papenheimer worked with in the black shack at Detrick in 1943. So if you can come in August, the only preparation would be a booster of the Papenheimer type material. However, if your trip to see us is delayed until September, we will be working with the type of material Dr. Foshay works with. That would require a series of three shots at forty-eight hour intervals."[668] To Baldwin, Wilson, or anyone else in their interpersonal network, it was plain that an August visit would involve brucellosis and botulinum toxin, and a booster immunization against the latter, while a September trip would involve potential exposure to tularemia (with more-involved and less-reliable immunization with Foshay's killed whole-cell vaccine).[669] Notably, this circumspect "writ[ing] in riddles" (as Elizabeth McCoy put it in 1943) would have been fairly legible to anyone in the bacteriological community, security clearance or no.[670] Anyone who knew anything about I. Forest Huddleson or Lee Foshay's published work could pretty well guess what "material" was being talked about. The fundamental concern of the secrecy system was demonstrated by such correspondence "subject to a translation that I can say but need not write."[671] Words, not secrets, were what the system most directly acted upon.

Circumspect language was likewise used and even encouraged over the telephone. As the Grosse Île incident in 1943 demonstrated, the secrecy system professed a marked

[668] Perry Wilson to Ira Baldwin, June 21, 1952 in UWA Baldwin Papers, Box 14 Folder 5.
[669] See Eileen M. Barry, Leah E. Cole, and Araceli E. Santiago, "Vaccines Against Tularemia," *Human Vaccines* 5 no 12 (2009), pp 832-838.
[670] Elizabeth McCoy to E. B. Fred, July 11, 1943, in NAS BW Box 8 Folder 14 ("McCoy, Prof. Elizabeth F.: 1942-1945").
[671] W. Mansfield Clark to Frank B. Jewett, October 15, 1942, in NAS BW Box 8 Folder 7 ("Jewett, Frank B. (Black Book): 1942-1943").

suspicion of the telephone, even when privacy-protecting gadgets were used.[672] This was certainly a healthy skepticism: telephone lines could be tapped or even listened in to by manual switchboard operators, and no device to prevent this was really foolproof. More broadly, however, the heightened scrutiny directed at telephones probably also reflected the nature of the secrecy system. It was a system devoted to controlling and tracing specific documents and specific physical spaces in which classified documents and conversations could be held. The telephone, like the transportation of documents, was an anomaly transgressing this order, but unlike documents watched by special messengers, the telephone lines that transmitted information were not easily subject to similar control. The result was an uneasy compromise in which the telephone was used to coordinate the geographically disparate network of BW advisors and contract researchers, but with users being instructed never to discuss specific classified information in their conversations. Whatever could not be conveyed with circumspect, non-classified language, needed to be sent in properly self-classified letters.

Writing these letters and other secret documents implicated often-invisible actors within the mid-20th century office: secretaries and other clerical staff. By the mid-century, secretarial labor was strongly gendered as female, with busy male professionals (like scientists and military officials) often lacking the skill to type or file their own correspondence as much as they lacked the time.[673] Handwritten or poorly typed letters pepper the archives of these men, complete with apologies for his secretary having been

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[672] E. B. Fred to Frank B. Jewett, January 9, 1943, in NAS BW Box 8 Folder 7 (“Jewett, Frank B. (Black Book): 1942-1943”).

[673] Sharon Hartman Strom, *Beyond the Typewriter: Gender, Class, and the Origins of Modern American Office Work, 1900-1930*, Champaign: University of Illinois Press, 1992; Kim England and Kate Boyer, “Women’s Work: The Feminization and Shifting Meanings of Clerical Work,” *Journal of Social History* 43 no 2 (2009), pp 307-340; Craig Robertson, “Learning to File: Reconfiguring Information and Information Work in the Early Twentieth Century,” *Technology and Culture* 58 no 4 (2017), pp 955-981.

out of the office or for having deliberately avoided official secretarial services in the case of particularly candid letters.[674] So too are wry comments about earning the ire of his secretary by bungling filing work, one of those genres of jokes which belied the reality that the subordinates possessed a crucial skill their ostensible superiors did not.[675] Simply put, scientists and their secretaries were a package deal for a geographically-dispersed, correspondence-based network like that of the BW advisors to effectively function.[676] Security clearance of secretaries thus went hand-in-hand with the clearance of a new scientist-advisor or contract researcher, more than doubling the demands placed upon the clearance system. The result during the Second World War was a virtual breakdown in the effectiveness of this system. When the WRC Committee was first organized in 1941, its members' secretaries were supposed to be subjected to the same clearance system and ritualistic signature of the Espionage Act, but as delays mounted in clearing even necessary consultants, secretaries' clearances were delayed further and further. By the end of December, in the heady days after Pearl Harbor, all but the pretense of following the clearance system had been given up, with the War Department agreeing to a system in which, in essence, a cleared scientist could simply vouch for their secretary to have her cleared as well.[677] Supposedly, the secretary's personal history was still supposed to be forwarded for War Department files, should the backlog be cleared or any other concerns

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[674] See e.g. Keith C. Barrons to William B. Sarles, March 12, 1945 in NAS BW Box 6 Folder 9 ("Projects: "LN": Suggestions by Outside Research Workers: 1945"), which Barrons "typed myself…" as "stenographers do have a way of talking at times."
[675] A reasonably close equivalent of this joke in the contemporary American office probably involves earning the ire of members of the IT department, albeit without the gendered connotation of quasi-domesticity.
[676] Harvard's J. Howard Mueller was explicit about this, noting that despite "becoming a fairly expert typist" he "[has] been struggling along with our correspondence without having a secretary cleared to handle it." J. Howard Mueller to E. B. Fred, July 14, 1943 in NAS Box 3 Folder 44.
[677] Frank B. Jewett to E. B. Fred, December 23, 1941 in NAS Box 3 Folder 31.

be raised, but as a practical matter, Fred found that these records were “spotty” at best.[678] Effectively speaking, as the WRC Committee and its contract research projects became fully operational in 1942 more than half of people privy to its secrets had received their clearance essentially by fiat. For scientists like Fred, working with a secretary was a paternalistic “matter of your personal responsibility.”[679] I am aware of no point when a secretary betrayed this trust, but the willingness of the secrecy system to discount the agency of subordinate women so long as they went through the motions of signing loyalty oaths is yet another stark reminder of the distinction between the rituals of secrecy and the actual keeping of secrets.

This emergency system was superseded in subsequent years by a more standard system of individual investigations, but even in the postwar years of the 1940s, secretaries’ clearances were treated fairly lackadaisically. For example, in 1948 Ira Baldwin requested a clearance for Catherine Goddertz, a secretary from a University of Wisconsin administrative office to which he was moving.[680] Within about 6 weeks, effectively on his say-so, she received an interim clearance pending a full background investigation. No full clearance for her ever arrived, however, and it was not until 1951 when Baldwin prodded Pentagon staff to give him some record attesting that she was in fact cleared. This subsequently arrived in the form of an informal letter (but nothing else!) for his files.[681] This informality was a marked contrast with the increased bureaucratization of the next two decades. Audrey Walker, Baldwin’s new secretary in 1959, took months and multiple forms for a clearance process which seems to have

[678] E. B. Fred to T. B. Turner, January 2, 1942, in NAS Box 3 Folder 31.
[679] Irvin Stewart to Edwin Broun Fred, January 5, 1942 in NAS Box 1 Folder 17.
[680] I. L. Baldwin to H. I. Cole, October 13, 1948 in UWA Baldwin Papers, Box 16 Folder 6.
[681] Edward Wetter to Ira L. Baldwin, January 7, 1952 in UWA Baldwin Papers, Box 16 Folder 6.

actually involved an investigation and by 1966, securing clearance for Janet Franke required multiple letters, a number of forms filled out in triplicate, and six months simply to apply.[682] Even earlier cases like that of Goddertz fell afoul of tightening enforcement of regulations. In 1959, ironically just six months before Baldwin was to move offices and thus no longer require her services, a Pentagon audit of its files found no record of her clearance. Despite multiple letters back and forth and Baldwin presenting the earlier correspondence about her, he was instructed to lock her out of the office safe by changing the combination. Despite lamenting that "in view of the fact that I am the world's worst secretary and file clerk, I am sure that this will result in less effective handling of the Chemical Corps business," Baldwin worked without a secretary for much of the year until Audrey Walker's clearance came through.[683] Baldwin's solution in 1959 and 1965 (when the recently-married Audrey Stone resigned her position) was to ask that classified documents simply not be delivered to his office, "though it will undoubtedly make me less effective" as a consultant.[684] Much like the proliferation of logbooks, inspections, and reporting forms, the unwillingness of the secrecy system to simply accept secretaries as extensions of their employers made the system more cumbersome to work with for scientists like Baldwin by the 1960s.

The regulated secrecy system was an irritating reality, with Baldwin complaining about the clearance renewal process that "it does irritate me to have to go through the

[682] I. L. Baldwin to Captain Martha S. Anderson, June 23, 1959; Captain Martha S. Anderson to I. L. Baldwin, August 6, 1959; Anders O. Wiklund to I. L. Baldwin, September 30, 1959; I. L. Baldwin to Chief, Defense Industrial Security Clearance Office, February 2, 1966; I. L. Baldwin to Chief, Defense Industrial Security Clearance Office, May 10, 1966, all in UWA Baldwin Papers, Box 16 Folder 7.
[683] I. L. Baldwin to Captain Martha S. Anderson, March 28, 1959, in UWA Baldwin Papers, Box 16 Folder 6.
[684] I. L. Baldwin to Captain Martha S. Anderson, April 8, 1959, in UWA Baldwin Papers, Box 16 Folder 6; Ira Baldwin to Carl B. Marquand, August 4, 1965 in UWA Baldwin Papers, Box 16 Folder 8.

rigmarole of filling out these blanks once every six months. After all, my finger prints have not changed since the last time a set was taken."[685] However, in the 1940s, this regulatory system could be less draconian in practice than on paper. For instance, chain-of-custody record-keeping helped identify that Baldwin had misplaced a meeting agenda (classified Secret) from a bundle he brought to an RDB meeting in 1948, but it was only after a seemingly unconcerned month that it turned up in his office.[686] It seems improbable that such an omission would have been settled with such an unhurried exchange of informal letters by the paperwork-loving 1960s. By this time, even visitors to the office in which the safe was stored would have to be formally logged and reported to an Army security officer, and the office was subject to inspection by such an office every six months.[687] So onerous had these regulations become that Baldwin made an effort to rid himself of his facilities clearance in 1965, preferring to simply read classified papers during his trips to Washington. Even this proved impossible by this point, however, as he was informed that he could only abandon his faculties clearance by renouncing his security clearance generally.[688] Though he demurred at the time, by 1969 he had had enough and allowed his clearance to lapse. There was a particular irony in one of microbiology's most zealous defenders of Detrick withdrawing from his advisory work the year that the offensive biological weapons program collapsed, but more so in his motivation being excessive security regulations. If Baldwin had remained an advisor through 1969 his voice almost certainly would have made no difference, but it is

[685] I. L. Baldwin to Carl Marquand, May 5, 1954, in UWA Baldwin Papers Box 16 Folder 4.
[686] H. I. Cole to I. L. Baldwin, August 12, 1948; I. L. Baldwin to H. I. Cole, August 19, 1948; I. L. Baldwin to H. I. Cole, September 21, 1948, all in UWA Baldwin Papers, Box 16 Folder 5.
[687] JAF (probably Janet A. Franke) to Ira Baldwin, June 21, 1967; John L. Wise to Ira L. Baldwin, June 25, 1969, both in UWA Baldwin Papers, Box 16 Folder 8.
[688] J. M. Murphy to Ira L. Baldwin, October 15, 1965 and Baldwin to Murphy, October 27, 1965, in UWA Baldwin Papers, Box 16 Folder 8.

emblematic of Detrick's growing isolation from even its allies that the secrecy system pushed him out.

In addition to having an obsession with procedure that scientists found off-putting, the secrecy system had an ambivalent relationship with artifacts. The quest for a 'technological fix' to a problem is a feature of modernity often noted and decried by historians of technology (and seen as particularly stereotypical of the Cold War US military), yet the secrecy system in a way worked against this stereotype.[689] People, rather than artifacts, were the objects of explicit trust within this system, seen in the preference for an unsecured Grosse Île telephone line to encourage better compliance with regulations, or the procrustean use of messengers to transport Top Secret documents. It was only when there was no practical choice but to trust an artifact, as when securing spaces of secrecy with safes, that artifacts were trusted, and even then the people who had access to and inhabited the space featured prominently within the forms that made the space bureaucratically legible. These forms themselves were things as well, of course. People may have been the explicit objects of trust, but *implicitly*, it was their bureaucratic doppelgänger, embodied in specific, physical pieces of paper with which the system really interacted.[690] (Likewise specific pieces of paper, not disembodied 'secrets,' were what the system sought to control.) Hull has rightly pointed out that a trust in paperwork is itself a kind of trust in artifacts over people, seen for instance in the East India Company's obsessive reliance on paperwork over dubiously reliable agents to secure an

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[689] See Sean F. Johnston, "Alvin Weinberg and the Promotion of the Technological Fix," *Technology and Culture* 59 no 3 (2018), pp 620-651. For an influential discussion of the US military's pathological quest for technological fixes in the early Cold War and Vietnam War, see Paul N. Edwards, *The Closed World: Computers and the Politics of Discourse in Cold War America*, Cambridge, MA: MIT Press, 1996.
[690] See Lisa Gitelman, *Paper Knowledge: Toward a Media History of Documents*, Durham: Duke University Press, 2014.

empire half a world away.[691] In this sense, the secrecy system did ultimately place its trust in things. However, while it is helpful to analyze the secrecy system as a material one, I wish to draw a distinction between trust in paperwork and mistrust of other artifacts for two reasons. First, there is something lost in flattening this distinction between obvious gadgets and papers quietly sitting in the background, particularly when the actors concerned would almost certainly have made it if pressed. The materiality of the papers aside, a mistrust of the gadgets is itself remarkable. Second, the universe of papers was not as all-pervasive as it pretended to itself. The bureaucratic doppelgängers of people and spaces ensconced in the secrecy system, for instance, could hold clearances which were by no means warranted under a strict adherence to the system's rules. It was only in the 1960s, decades into the bioweapons program, that an ever-proliferating world of paperwork began to routinely catch such lapses. If the secrecy system did eventually come to represent a primacy of papers over people, it was a long evolutionary process.

Managing Secrecy

Scientists were not merely passive domesticates of the secrecy system. They could resist and subvert it to avoid its most onerous aspects, for example by using circumspect language, or could seek to escape it entirely by leaving classified research positions (as Joshua Lederberg did in the early 1950s, when he abandoned an unclassified Detrick research contract which nonetheless came with security clearances and secrecy procedures attached).[692] Scientific advisors, meanwhile, had still more direct access to the

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[691] Matthew Hull, *Government of Paper*, pp 7-10.

[692] Joshua Lederberg to Herbert F. York, June 21, 1958, in Joshua Lederberg Papers, NLM Profiles in Science (NLM ID: 101584906X19067). One way of avoiding both scientists' objections and unwanted attention to Chemical Corps-sponsored research was to sponsor work without letting the scientists know who they were working for. The most prominent example of this was the Smithsonian Institution's Pacific Ocean Biological Survey Program, which unbeknownst to its researchers was sponsored by the Chemical Corps to prepare for biological warfare testing. See Roy MacLeod, "'Strictly for the Birds': Science, the

structure of the secrecy system itself. Sometimes, advisor groups like the JRDB's Committee X were directly asked to weigh in on secrecy policy; more often, advisors could use their informal role as mediators between research scientists, Chemical Corps officers, and higher-up military officials (see Chapter 1) as avenues for influence. What the advisors tried to do with this influence revealed their profession's complex and sometimes contradictory relationship with secrecy. They were far from passive subjects with rules of secrecy imposed upon them, and they challenged and sometimes changed secrecy policy for a variety of ends. Neither, however, were they simply stereotypical truth-loving scientists in inherent opposition to hidden knowledge. They did not just accept the secrecy surrounding Detrick as the price of influence, they actively supported it, and decried what they saw as the wrong sorts of openness as dangerous. In making this Faustian bargain, however, scientific supporters of bioweapons research faced a conundrum. How could they use their authority as scientists to argue that biological warfare was possible and dangerous without building a consensus to this effect among their colleagues, but conversely, how could they build this consensus while maintaining the system of secrecy which they were a part of? Try as they might, they could never satisfyingly resolve this contradiction, which helped contribute to the downfall of their influence within the larger microbiological community in the late 1960s.

Until the end of the Second World War, the public identity of germ warfare was science-fictional, cohabiting lurid pulp accounts of future warfare alongside rockets and ray guns.[693] It was not until after the war had ended that the American public (and many

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Military and the Smithsonian's Pacific Ocean Biological Survey Program, 1963-1970," *Journal of the History of Biology* 34 no 2 (2001), pp 315-352.

[693] In the fictional future history of Aldous Huxley's 1931 *Brave New World*, for instance, "anthrax bombs" as well as chemical and high-explosive bombs are used in the apocalyptic "Nine Years' War" that ushers in

microbiologists) learned what had been happening at Detrick during the war, through the January 1946 publication of the "Merck report." Penned by pharmaceutical executive and wartime head of the War Research Service George Merck to Secretary of War Robert Patterson with intent to release it in part to the press, this report summarized in extremely general language the aims and actives of the wartime program, and argued that prudence demanded continued defensive biological warfare research into the peace.[694] This document fit into a broad genre of revelatory post-war press releases and reports about technological developments during the war, the most prominent of which was the 1945 Smyth Report, released shortly after the atomic bombing of Hiroshima.[695] Unlike the book-length Smyth Report, which quickly served to frame public discussion of the atomic bomb with its relatively detailed examination of the physics of nuclear fission, however, the publicly released Merck report was 4 pages long, and contained little detail beyond what amounted an official endorsement of the longstanding science-fictional idea that the germ theory of bacteriologists could be turned to war. The result, from the

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the Fordist World State. Aldous Huxley, *Brave New World*, London: Chatto & Windus, 1932. Anticipation of biological warfare was enough a part of the interwar cultural milieu that the Polish delegation to the 1925 Geneva Convention successfully argued for it to be banned alongside gas warfare. See Jerzy Witt Mierzejewski and John Ellis van Courtland Moon, "Poland and Biological Weapons," in Geissler and van Courtland Moon (eds), *Biological and Toxin Weapons: Research, Development and Use from the Middle Ages to 1945*, pp 63-69. This is similar to the 'science fictional' status of chemical warfare and aerial bombing when the 1899 and 1907 Hague Conventions banned them. See I. F. Clarke (ed), *The Tale of the Next Great War, 1871-1914: Fictions of Future Warfare and of Battles Still-to-Come*, Syracuse, NY: Syracuse University Press, 1995.

[694] George W. Merck, "Biological Warfare: Report to the Secretary of War by Mr. George W. Merck, Special Consultant for Biological Warfare," January 3, 1946, in NAS BW Box 6, Folder 5 ("Merck Report to Secretary of War: "Biological Warfare": 1945"), also available on NAS website at this URL: http://www.nasonline.org/about-nas/history/archives/collections/organized-collections/1945merckreport.pdf. Unlike the Smyth Report (see below), the Merck Report was not widely published, though a copy did appear in the *Bulletin of the Atomic Scientists*. See George W. Merck, "Official Report on Biological Warfare," *Bulletin of the Atomic Scientists* 2 no 7-8 (1946), pp 16-18.

[695] Henry D. Smyth, *Atomic Energy for Military Purposes*, Princeton: Princeton University Press, 1945. Another example of a classified wartime project being publicly revealed in the immediate aftermath of the war was the University of Pennsylvania's ENIAC project, heralded in the *New York Times* as one of the war's "top secrets." See Thomas Haigh, Mark Priestley, and Crispin Rope, *ENIAC in Action: Making and Remaking the Modern Computer*, Cambridge, MA: MIT Press, 2016, pp 87-88.

perspective of the BW scientists, was deplorable: "biological warfare" quickly became identified with the atomic bomb as an apocalyptic weapon of World War Three, discussed popular newspaper and magazine articles which were (as geneticist and DEF Committee member J. Howard Muller described one of them) "made up largely of fantasy together with a few facts and certain completely false statements."[696] This was compounded by the diplomatic conflation of the two weapons as "weapons of mass destruction" in negotiations over the international control of atomic energy, with some observers arguing such international control would be useless if it did not encompass biological disarmament as well.[697] Probably as a result of these two developments in the public identity of "biological weapons," an American government policy of avoiding any public discussion of biological weapons was soon tacitly instituted. There was essentially no official discussion of the topic between the early 1946 Merck report and a brief public statement by Secretary of Defense James Forrestal in 1949.[698] This public lacuna was a deliberate policy, most notably constraining what topics Chemical Warfare Service (soon to be Chemical Corps) officers could openly discuss.[699] A new kind of politically-

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[696] J. Howard Mueller to O. H. Perry Pepper, October 11, 1946, in NAS BW Files, Box 5 Folder 21 ("DEF Committee: Advisory Committee on Biological Warfare 1946-1948"). The DEF committee was at the time being wound down in favor of the JRDB and Committee X.

[697] For an examination of the genealogy of the term "weapons of mass destruction," see W. Seth Carus, "Occasional Paper 8: Defining 'Weapons of Mass Destruction,'" National Defense University Center for the Study of Weapons of Mass Destruction, 2006.

[698] "Secretary Forrestal's Statement on Biological Warfare," *Bulletin of the Atomic Scientists* 5 no 4 (1949), pp 104-105. On American government officials' attitude toward biological warfare in the late 1940s, see Chapter 2 of Jacob Darwin Hamblin, *Arming Mother Nature: The Birth of Catastrophic Environmentalism*, New York: Oxford University Press, 2013.

[699] RDB scientists were also warned against answering public questions without consulting first with military officials. See F. H. Richardson, "Memorandum to Expert Consultants, Research and Development Board," February 19, 1948 in UWA Baldwin Papers, Box 15 Folder 2. Theodor Rosebury, who corresponded regularly with friends at Detrick, noted sarcastically in 1949 that "I wouldn't be surprised if they are using secret ink at Detrick at this time, the way the cold war is going." Rosebury to "Jack," November 10, 1949 in NLM Rosebury Papers, Box 3, Folder 21 (Correspondence 'M' 2 of 6).

motivated secrecy thus quickly returned to the topic of "biological warfare," now more in the tacit form of things-not-talked-about than an affirmatively secured military secret.

The United States in the late 1940s was of course a nation increasingly obsessed with secrets and their preservation. Out of canonical documents like the Smyth Report, scholars like David Kaiser have argued, emerged a cultural obsession with "the" atomic secret, monolithic scientific knowledge which must be protected against espionage at all costs if the US was to preserve its atomic monopoly.[700] This viewpoint (which Rebecca Press Schwartz argues was perniciously inaccurate for its privileging of nuclear physics over equally crucial chemistry and tacit engineering knowledge needed to successfully build a Bomb) was a juggernaut in American society, amplifying the Red Scare anxiety already invoked by revelations of Soviet espionage, and leading, in the 1946 Atomic Energy Act, to the constitutionally-dubious status of "born secret" for knowledge produced by virtually the entire field of nuclear physics.[701] The coming of such secrecy to Detrick in 1947 posed a challenge for scientist-advisors like Baldwin. By and large, their *raison d'être* as a community was to use their authority as scientists to argue that biological warfare was plausible and threatening, and thus merited a continued research program. Besides the challenge of convincing military planners to accept Detrick's questionable animal dose data as valid, however, was one of exactly that scientific authority.[702] Many of the presumptions underlying the "BW" concept, particularly that

[700] David Kaiser, "The Atomic Secret in Red Hands? American Suspicions of Theoretical Physicists During the Early Cold War," *Representations* 90 no 1 (2005), pp 28–60.
[701] Rebecca Press Schwartz, "The Making of the History of the Atomic Bomb: Henry DeWolf Smyth and the Historiography of the Manhattan Project," PhD diss, Princeton University, 2008.
[702] Political scientist Frank Smith III argues that the "organizational frame of reference" of military authorities predisposed them to discount knowledge claims from fields (like microbiology) alien to their professional experience with high-explosive weapons. See Frank L. Smith III, *American Biodefense: How Dangerous Ideas about Biological Weapons Shape National Security*, Ithaca: Cornell University Press, 2014. Skepticism of the validity of animal dose data in particular led by the mid-1950s to the systematic

aerosolized microbes could remain airborne, remain alive, and infect their hosts through the lungs even when this was not a natural part of their life cycle, were poorly established and controversial within mid-century medical microbiology. How were Baldwin and his compatriots to have authority to speak as scientists when their own broader community was tepid about their claims?

One solution was to use secrecy as a bludgeon. Scientists who were skeptical of biological warfare claims could be dismissed as either unscientifically emotional, if their critique drew upon any moral arguments, or simply ignorant of secret information which dispelled their concerns. The ever-sharp-tongued Walter Nungester dismissed "microbiologists who do not share this belief" (in the efficacy of biological warfare) as lacking legitimacy, either because "they do not have access to 'Top Secret' information," they were a member of "several interested groups," or they suffered from "the bias of previous ideas based on microbiology as seen in the laboratory or in naturally occurring infections." In short, virtually anyone who disagreed with Nungester was by definition lacking in scientific legitimacy, either because they were not "informed of recent 'T.S.' trials," or because their objectivity was in question. This was a common rhetorical device in early Cold War science, used particularly to dismiss heterodox political positions taken by scientists like Linus Pauling.[703] Nungester went one step further here than dismissing moral critiques of nuclear testing as irrational, however, by targeting scientists who

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use of human test subjects for some more-easily treated biological warfare agents in Project Whitecoat. See U.S. Army Medical Research Institute of Infectious Diseases, "Project Whitecoat: A History" (1974); Robert L. Mole and Dale M. Mole, *For God and Country: Operation Whitecoat: 1954-1973*, New York: TEACH Services, 1998. See also Rebecca Slayton, *Arguments that Count: Physics, Computing, and Missile Defense, 1949-2012*, Cambridge, MA: MIT Press, 2013, for another discussion of one professional community's knowledge claims faring better in military decision-making than another's.

[703] See Audra J. Wolfe, *Freedom's Laboratory: The Cold War Struggle for the Soul of Science*, Baltimore: Johns Hopkins University Press, 2018.

dismissed the "BW" idea on scientific grounds as ill-informed or biased. This was a recurring problem for would-be scientific critics of the bioweapons program, mirroring the dilemma of the pro-BW group: how could they use their authority as scientists to buttress their case when they were arguing against colleagues with access to information they didn't have? As young University of Wisconsin geneticist Joshua Lederberg put it to would-be critics in the Federation of American Scientists in 1949, "I cannot help but feel that the military program holds the ace" in any such discussion, because of the secret information it controlled.[704] Between this dilemma and the threat of political opprobrium amidst a growing Red Scare, few American microbiologists overtly challenged biological weapons research on moral or scientific grounds before the 1960s (see Chapter 5).

However, building scientific consensus in open microbiology required more than silencing dissent. As a result, the community of scientist-advisors consistently turned to increased scientific openness as a solution. For example, in 1959, the NIH's Justin Andrews used a meeting between Chemical Corps and public health officials to argue for a declassification of aerobiology data to convince more scientists of the dangers of biological warfare.[705] "How long, how long have we clamored for this type of approach," his former CDC colleague Alexander Langmuir wrote in a letter supporting the proposal.[706] Langmuir had been a member of advisor community since the mid-1940s, having served as a member of Baldwin's Committee X before joining the CDC to found its Epidemic Intelligence Service, an epidemiological surveillance unit, on the strength of

[704] Joshua Lederberg to Hugo C. Wolfe, June 10, 1949, in Joshua Lederberg Papers, NLM Profiles in Science (NLM ID: 101584906X2400).
[705] Andrews, "Report of August 7th meeting;" in JHU Alexander Langmuir Papers, Box 1, Unnumbered Folder Entitled "ADL: Miscellaneous Materials from CDC (Personal)"
[706] Alexander Langmuir to Justin Andrews, August 21, 1959 in JHU Alexander Langmuir Papers, Box 1, Unnumbered Folder Entitled "ADL: Miscellaneous Materials from CDC (Personal)"

the argument that such an organization would be the first line of defense in detecting a biological attack on the US.[707] Though the EIS' practical mission turned almost immediately to field epidemiological investigations of natural disease outbreaks, Langmuir's use of a biodefense rationale (to use an anachronistic term) represented more than political expediency. He and his subordinates retained close advisory ties to Detrick, and Langmuir made the impassioned argument that epidemiologists and physicians should be concerned with defending against biological attack in two early 1950s papers (one co-authored with Andrews).[708] By the end of the decade, however, Langmuir and other 'true believer' colleagues like Andrews were disappointed that they had failed to enlist much interest from the medical community, and this would not change, Andrews argued, unless Detrick data were more openly discussed in open scientific meetings.[709] The late 1950s were a propitious time for Andrews' proposal, as the Chemical Corps was enjoying a relatively long leash to publicly discuss chemical and biological warfare. The NAS was enlisted to help organize an open conference on aerobiology, which would be dominated by Detrick research, and in turn enlisted the aid of Baldwin.[710] Though this conference was eventually delayed until late 1960, it was indeed unclassified, and the Society of American Bacteriologists quickly published its papers as a special issue of its

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[707] See Elizabeth W. Etheridge, *Sentinel for Health: A History of the Centers for Disease Control*, Berkeley: University of California Press, 1992, pp 36-42; James Colgrove, Amy L. Fairchild, and Ronald Bayer, *Searching Eyes: Privacy, the State, and Disease Surveillance in America*, Berkeley: University of California Press, 2007, p 17; Mark Pendergrast, *Inside the Outbreaks: The Elite Medical Detectives of the Epidemic Intelligence Service*, Boston: Houghton Mifflin Harcourt, 2010.

[708] Alexander D. Langmuir, "The Potentialities of Biological Warfare against Man: An Epidemiological Appraisal," *Public Health Reports* 66 no 13 (1951), pp 387-399; Alexander D. Langmuir and Justin M. Andrews, "Biological Warfare Defense: The Epidemic Intelligence Service of the Communicable Disease Center," *American Journal of Public Health* 42 no 3 (1952), pp 235-238.

[709] Andrews, "Report of August 7th meeting;" pp 3-4 in JHU Alexander Langmuir Papers, Box 1, Unnumbered Folder Entitled "ADL: Miscellaneous Materials from CDC (Personal)"

[710] Per K. Frolich to Ira L. Baldwin, October 8, 1959, in UWA Baldwin Papers, Box 12, Folder 5.

journal *Bacteriological Reviews*.[711] The precedent set by this conference would lead to a series of follow-up aerobiology conferences co-sponsored by the Navy Biological Laboratory, a defensively-oriented and much smaller counterpart to Detrick institutionally hosted by Berkeley's Bacteriology Department.[712] This conscious release of classified information to sway scientific opinion to take biological warfare more seriously seems to have largely been a failure, which should not be a surprise given how little attention even unclassified work on the aerobiology of tuberculosis by the field's founder, William F. Wells, had received in the 1950s.[713] The broader point, however, is that this attempt was made. As is discussed above, the record of open scientific publications from Detrick is ambiguous, but there certainly were a substantial number of them. This owed a great deal to pressure from Baldwin's group, from initial post-war declassification to looser classification policies promulgated by Committee X in the late 1940s to subject-specific lobbying by figures like Andrews and Langmuir. Every paper published on a controversial topic like airborne infection or freeze-drying pathogens was for them a useful weapon to argue that their colleagues should take their claims more seriously. Notably, however, this was an implicit connection. Papers published by Detrick researchers would mention their institutional affiliation, but would otherwise

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711 See *Bacteriological Reviews* 25 no 3 (1961). The proceedings were again published as Walsh McDermott (ed), *Conference on Airborne Infection held in Miami Beach, Florida, December 7-10, 1960. Sponsored by Division of Medical Sciences, National Academy of Sciences-National Research Council*, Baltimore: William & Wilkens, 1961.

712 McDermott (ed), *Conference on Airborne Infection*; Naval Biological Laboratory, *First International Symposium on Aerobiology*, Berkeley: Naval Biological Laboratory, 1963. The practice of universities playing nominal institutional host to geographically removed military-oriented research institutes was a familiar one in the early Cold War, exemplified by Johns Hopkins University's Applied Physics Laboratory. See Denis, "'Our First Line of Defense'"

713 See the retrospective discussion of one of Wells' most prominent disciples in Richard L. Riley, "What Nobody Needs to Know about Airborne Infection," *American Journal of Respiratory and Critical Care Medicine* 163 no 1 (2001), pp 7-8.

leave the implications of their work undiscussed. Even former researchers were subjected to this culture of silence (for example, Theodor Rosebury: see Chapter 5).

This publication of scientific information pertinent to biological warfare without acknowledging this pertinence reflected the crucial reality that for all that pro-BW scientists wanted looser restrictions on scientific publication, they actively supported the culture of public silence on the topic of "biological warfare." This was part of their dislike of popular articles on the topic "made up largely of fantasy" (in Mueller's words), a genre which particularly flowered in the late 1940s.[714] It would be reasonable to argue that such suspicion of popular treatments of scientific ideas stemmed from scientists' jealousy of their expertise more than anything else, but this explanation falters in the face of the tacit policing of fellow scientists. Public discussion of "biological warfare" was undesirable even (or especially) if a scientist did it, as when Rosebury published his 1942 report (see Chapter 5, below). The key to this attitude can be seen in Baldwin's view of lay publicity as inherently "sensationalistic." 'Sensational' publicity painting BW as a superweapon comparable to or more powerful than the atomic bomb risked prompting a public backlash, but more importantly, it could bring the state intrusion experienced by nuclear physicists into microbiology. A world in which infectious disease research was "born secret" like nuclear physics was one in which the field of microbiology would be completely transformed if it could meaningfully exist at all, and had to be avoided at all costs. "Personally," Baldwin wrote along these lines in 1949, "I feel that a disservice had been done to the country by the over-emphasis which the atomic bomb can play in warfare. Among other things this over-emphasis has brought on the Atomic Energy

[714] J. Howard Mueller to O. H. Perry Pepper, October 11, 1946, in NAS BW Files, Box 5 Folder 21 ("DEF Committee: Advisory Committee on Biological Warfare 1946-1948").

Commission and on physicists everywhere a very unfortunate situation with respect to security. As you well know, the situation is so bad that physicists in general are almost at the point of revolt. Certainly we do not want to see that kind of a situation in our own field."[715] This fear that active public discussions of the dangers of biological warfare risked (to put it cynically) an intrusion of democracy, was a commonplace in Baldwin's social world. What if popular revulsion prompted the government to abandon biological warfare research? Worse still, what if such pressure prompted the government to treat microbiology like atomic physics, imposing a regime of extreme secrecy like that in the Atomic Energy Act?[716] For Baldwin, the few understated official discussions of biological warfare, like the 1949 press release by "Secretary [of Defense] Forrestal… to try to put an end to the scare headlines" were not unwelcome, but neither was the habit of official silence which was their backdrop.[717] "I believe there should be discussion of the potentialities of BW… [which] would be very helpful in combatting unreasoning fear," Baldwin wrote in 1954 (when official silence was particularly heightened in the wake of Korean War allegations of American bioweapons use). "On the other hand," he continued, "so long as that unreasoning fear exists, I would doubt the desirability of a public pronouncement on the part of an agency of the United States Government."[718]

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[715] Baldwin to Nungester, November 21, 1949 p 2, in UWA Baldwin Papers Box 15 Folder 4. Baldwin's belief in an "over-emphasis" on the atomic bomb (a position most commonly articulated by the Navy in the late 1940s as part of inter-service budgetary fights with the Air Force, but which he may also have adopted from the Chemical Corps) is particularly noteworthy given the two months of national hand-wringing which had just taken place following President Truman's late September announcement that the Soviet Union had successfully tested a Bomb of its own.
[716] This was not necessarily an unfounded fear, given the role popular perceptions of atomic physicists had played in the security culture imposed upon them. See Lawrence Badash, "From Security Blanket to Security Risk: Scientists in the Decade After Hiroshima," *History and Technology* 19 no 3 (2003), pp 241-256.
[717] Cole to Baldwin, November 20, 1947 in UWA Baldwin Papers Box 15 Folder 1.
[718] Baldwin to Colonel Manford J. Wetzel, November 23, 1954, in UWA Baldwin Papers, Box 12 Folder 1.

Without more open discussion about biological warfare, the BW group faced an uphill battle in being taken seriously even by their colleagues, but if more openness caused the public to take it too seriously this could lead to an unacceptable loss of control over bioweapons policy and perhaps microbiology itself. The chairman of the NRC's Division of Biology and Agriculture, Robert F. Griggs, summed up this dilemma well. On the one hand, in the aftermath of a particularly well-publicized article by popular science writer Gerald Piel, "biologists as a group ought to take some leading part in the formation of public opinion on the subject very much as the atomic scientists have," but "in biological warfare we are playing with something more dangerous than fire. There is a possibility that any public discussion of the matter might clamp down security regulations on biologists in a way that would be tragic."[719] The (unstated) course of action implied by this was what, in fact, most biologists ended up following: if they wished to avoid the extraordinary secrecy being imposed on nuclear physicists by laws like the Atomic Energy Act and institutions like the Atomic Energy Commission, they could not afford to challenge the tacit (rather than legally explicit) culture of secrecy surrounding biological warfare.[720] Fellow scientists, not the public, were the civilian constituency who needed to be convinced of the dangers of biological warfare, and the strategic release of scientific papers only implicitly connected to the topic of BW would have to suffice to engage the former while excluding the latter.

[719] Robert F. Griggs to E. B. Fred, November 18, 1946, in UWA Baldwin Papers, Box 15 Folder 3.
[720] This use of secrecy as a political shield by the microbiologists (in a way more so than government officials!) reflects what political scientist David Gibbs calls an "internal threat" model of secrecy, in which a government's citizens are the main 'audience' for an act of concealment. See David N. Gibbs, "Secrecy and International Relations," *Journal of Peace Research* 32 no 2 (1995), pp 213-228.

Eschewing the publicity surrounding atomic physics for fear of an AEC-like organization being imposed on microbiology came at the cost of being unable to shape a generally-negative public opinion of biological warfare. Chemical Corps officials looked on the AEC with as much envy as the scientists had with horror: "the chief reason the AEC enjoys a favorable press," one Corps memo argued in 1958, "is that it has done an excellent job of convincing John Q. Public that it is helping researchers to make life better and longer provided nations refrain from dropping atomic bombs on one another."[721] Spreading an equivalent message of "Chemical Corps contributions to humanity and better living resulting from Chemical Corps Research and Development" was how the Corps might reverse its own "bad press."[722] Seeking to pursue this strategy, Chemical Corps officers were ironically far more prone to challenge the culture of public silence imposed on them by their Pentagon superiors than the scientists who advised them. Future Corps commander William Creasy often flirted with censure for his public statements about the dangers of chemical and biological attack, while his colleague J. H. Rothschild was so fed up with the official culture of silence that he eventually left the Corps and followed Rosebury in authoring a jeremiad popular book (albeit with an opposing political message).[723] The political winds shifted over time for Corps officials: in the late 1950s, for example, they felt relatively free to lavishly promise the future potential of chemical and biological weapons and warn of a "CBW gap" with the Soviet

[721] C. B. Marquand, "Memorandum, Subject: Participation in the American Chemical Society Committee Meeting of May 1958," April 4, 1958, p 4, in UWA Baldwin Papers Box 12, Folder 2.
[722] Ibid p 3. Creasy particularly chafed at the culture of official silence, noting wryly to Baldwin in 1954 that "I am still in 'hot water' over this speech" on the new Pine Bluff arsenal, "apparently for policy rather than security reasons. I don't suppose I will ever learn how 'security' actually works." Creasy to Baldwin, October 7, 1954, in UWA Baldwin Papers Box 12, Folder 1.
[723] J. H. Rothschild, *Tomorrow's Weapons: Chemical and Biological*, New York: McGraw-Hill, 1964.

Union, while a few years later such public discussions were understood to be verboten.[724] They had little support through all of this from their erstwhile scientific allies, who were much more comfortable with official silence so long as scientific publications flowed out of Detrick. Ultimately, when the topic of chemical and biological warfare finally was lifted from obscurity to public scrutiny during the Vietnam War, the scientists' strategy of supporting the culture of official silence came back to haunt them and the Detrick program.[725] The Nixon administration's decision to abandon offensive biological weapons research as a political embarrassment was predicated on growing protest against such research within the scientific community, and a public presumption that biological weapons were a particularly illegitimate and horrifying way of making war.[726] Both of these factors, in a sense, represent a failure of the Baldwin group, Detrick's most zealous advocates, to make their case over almost a quarter-century. They never resolved the paradox of secrecy confronting them, a contradiction which was their downfall.

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[724] See e.g. Chemical Corps officers' dire testimony in US Congress, House of Representatives, Committee on Science and Astronautics, *Research in CBR: A Report of the Committee on Science and Astronautics*, 86th Cong., 1st sess., 1960, H. Report 815. Under the Kennedy administration, such public discussion was far less welcome. See Moon, "The US Biological Weapons Program," in Wheelis, Rózsa, and Dando (eds), *Deadly Cultures*, pp 9-46.

[725] This Vietnam-era scrutiny, based on publicly available information, is reminiscent of a shift in news reporting about the Hanford nuclear facility described by Daniele Macuglia. Before the 1980s, he argues, the matter-of-fact tone of post-1945 news reporting about accidents with radioactive material at Hanford contributed to a public culture of ambiguous silence about the dangers represented by the site, replaced in that decade by a genre of critical revelatory reporting that attracted increasing public attention. As with Detrick, this reporting did not reveal some grand secret about the existence of Hanford and basic sense of what went on there, but its public disruption of a previous culture of silence was significant nonetheless. See Daniele Macuglia, "Talking About Secrets: The Hanford Nuclear Facility and News Reporting of Silence, 1945-1989," in Felicity Mellor and Stephen Webster (eds), *The Silences of Science: Gaps and Pauses in the Communication of Science*, New York: Routledge, 2017, pp 115-134.

[726] See Jonathan B. Tucker, "A Farewell to Germs: The U.S. Renunciation of Biological and Toxin Warfare, 1969-70," *International Security* 27 no 1 (2002), pp 107-148.

Conclusion

There is a phenomenon that sociologists call elite deviance: the willful, organized violation of community norms by the powerful, shielded by they are from practical repercussions by their power, but also (and far more insidiously) coming to consider themselves exempt from moral opprobrium as well by virtue of their membership in a select and exulted group.[727] In the case of Baldwin and his community, one might well also call it moral arrogance: a belief that their scientific objectivity, as much as their scientific knowledge, made them judges of the morality of their weapons work superior to their fellow-citizens. Baldwin and his compatriots were well aware of the popular perception of biological warfare as a "dirty" way of making war; that is why they tried to hide it. They were basically willing supporters of the secrecy system because it let them shield their moral judgment from "sensational" public discussion and judgment, and avoid the scrutiny of elected officials that this could inspire. It is important not to presume our own moral judgments when we wish to understand the past: the size of the network of Detrick 'friends' makes it clear that their position supporting the use of their science for war was at the very least a defensible one within the microbiological community at the time. That, however, is not really the point. The more salient point is that they sought to enshrine their moral judgment as the only meaningful one at work in public policy, using the military secrecy system. That this hubris proved a weakness of the research they supported and contributed to its downfall is certainly dramatically ironic, but a strong argument can be made that whatever stock one puts in their moral defense of bioweapons research, this hubris is itself worthy of our condemnation.

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[727] See David R. Simon, "White-Collar Crime, Dehumanization, and Inauthenticity: Towards a Millsian Theory of Elite Wrongdoing," *International Review of Modern Sociology* 21 no 1 (1991), pp 93-107.

There is, however, another side to the story of the downfall that followed from this hubris. The 'friends' of Detrick allied themselves with the secrecy system to serve as a shield against the 'sensationalism' of well-informed public discussion, but this shield also necessarily confronted critical scientists like Rosebury who sought to join or even lead such discussions. Quiescence in the face of secrecy may have isolated Detrick from potential allies within the scientific community, but it also kept ammunition out of the hands of enemies like Rosebury. How, after all, could a scientist speak with any kind of credible authority to challenge biological weapons research when they were not in possession of the relevant facts? This dilemma presented by the secrecy system confronted would-be scientific critics of biological weapons research for over two decades between the end of the Second World War and the late 1960s, yet by the end of that period, they had emerged as major voices in the growing tide of public criticism. In the next chapter, we turn to the story of how scientific critics around the world avoided being stymied by the American secrecy system, the different tactics that they used, and the role they ultimately played in ending offensive biological weapons research in the United States.

Chapter 5: Theodor Rosebury, the Pugwash Movement, and the Tactics of Scientific Protest

In 1949, the Federation of American Scientists undertook a report on biological weapons, which like the group's typical focus of the atomic bomb had emerged in the post-WWII public consciousness as weapons of vast potential power.[728] The FAS contacted Joshua Lederberg, then a young professor of bacteriology at the University of Wisconsin, to review this report, citing his expertise in bacterial genetics. Lederberg, who would win the Nobel Prize a few years later for his work on sexual reproduction in *E. coli*, temporalized that he had no particular knowledge of biological weapons beyond having read a widely circulated review article by Columbia University's Theodor Rosebury, but offered several suggestions about the possibility of wheat rust being weaponized. Following Rosebury, he opined that biological weapons were less prone to effective international control than atomic weapons, making them an attractive prospect for smaller nations. He questioned, however, the ultimate value of the FAS report, and declined to publicly endorse it. "I cannot help but feel," he wrote, "that the military program holds the ace. Whatever an outside group might say would necessarily be subject to modification depending on the progress being made in the BW laboratories. Would it not be more useful, therefore, to seek an official endorsement of a report such as this, or to press for a comparable but authoritative statement from the Secretary of

[728] This report, commissioned in 1948, was subsequently reviewed by the FAS' Executive Council in April and November 1949, but does not seem to have been published more widely. See *F.A.S. Newsletter* 1 no 2 (February 1948), pp 2-3; *F.A.S. Newsletter* 2 no 3 (April 1949), p 2; *F.A.S. Newsletter* 2 no 9 (November 1949), p 3; and especially *F.A.S. Newsletter* 4 no 3 (April 1951), p 2, which mentions this report but implies that it was not officially published. The draft report did, however, circulate informally among microbiologists interested in biological weapons issues. A copy provided by military officials can be found in Ira Baldwin's papers. See University of Wisconsin Archives (UWA) Ira L. Baldwin Papers (Series 9/10/11), Box 15, Folder 4.

Defense?"[729] As the FAS noted in reply, however, no such authoritative public assessment was forthcoming from the military.[730]

Lederberg had identified a key dilemma of would-be scientific critics of the biological weapons program: how, in the face of the secrecy surrounding biological weapons research, could critics authoritatively comment on or critique it? Even a former 'insider' like Rosebury, who had directed a major project at Camp Detrick during the Second World War, had to be careful to use only unclassified public information in his *Peace or Pestilence*, a critical discussion of biological warfare, for fear of running afoul of espionage laws.[731] For the next two decades, this problem would confront pro-disarmament scientists like Rosebury, Matthew Meselson, and Lederberg himself, as well as organizations like the Pugwash conferences in which they took part.

Nonetheless, when the incoming Nixon administration initiated the first high-level review of the biological weapons program for two decades in 1969, it was in response to mounting criticism drawn from exactly such 'outsider' assessments. Inter- and transnational organizations like Pugwash, the Stockholm International Peace Research Institute (SIPRI), the World Health Organization (WHO), and the United Nations had all acted to create a body of critical expertise-based assessments of biological warfare of exactly the sort that Lederberg had believed impossible. Lederberg himself had emerged as a public critic of the American biological weapons program, evidently regarding his

[729] Joshua Lederberg to Hugo C. Wolfe, June 10, 1949 in Joshua Lederberg Papers, National Library of Medicine (NLM) Profiles in Science (NLM ID: 101584906X18904). It is worth noting that Lederberg was a recent hire in the Wisconsin Bacteriology Department, which was dominated by Baldwin and a cadre of other 'friends' of Detrick.
[730] Wolfe to Lederberg, June 15, 1949 in Lederberg Papers, NLM Profiles in Science (NLM ID: 101584906X18905).
[731] Theodor Rosebury, *Peace or Pestilence: Biological Warfare and How to Avoid It*, New York: McGraw-Hill, 1949, p 13.

outsider's expertise as a sufficient basis for comment, while Meselson was similarly comfortable in deploying his status as a prominent molecular biologist as a critic in venues from public and scientific meetings to the halls of Congress. Far from being silenced by military secrecy, these scientist-critics were prominent contributors to a rising tide of critique which prompted many officials in the Nixon administration review to recommend ending the program.[732] The Pugwash organization, in particular, served as a meeting space for this multifarious group of critics, acting to connect biologists concerned about biological warfare from various countries and organizations with both each other and with figures in the broader peace movement. Though originally founded by physicists and focused on the medical effects of nuclear fallout, Pugwash's leaders were active policy entrepreneurs and quickly sought to add biological weapons to the organization's disarmament concerns.[733] They sought to actively construct an epistemic community of experts on the dangers of biological warfare, creating a transnational network of microbiologists from countries on both sides of the Iron Curtain. Members of this Pugwash community would in turn serve as links to other organizations like SIPRI and the WHO, as well as venues for activists like Meselson.

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[732] Jonathan B. Tucker, "A Farewell to Germs: The U.S. Renunciation of Biological and Toxin Warfare, 1969-1970," *International Security* 27 no 1 (2002), pp 107-148; David I. Goldman, "The Generals and the Germs: The Army Leadership's Response to Nixon's Review of Chemical and Biological Warfare Policies in 1969," *Journal of Military History* 73 no 2 (2009), pp 531-569.

[733] "Policy entrepreneurs" figure prominently in political scientist John Kingdon's work, which focuses on "agenda-setting" within government decision-making. Kingdon argues that these figures, who somewhat resemble John Law's heterogeneous engineers, severe to construct and advance particular understandings of and potential solutions to an issue long before formal institutional decision-making considers it. This consideration often proceeds from what Kingdon calls a "policy window," a period in which events make decision-makers more receptive to considering the issue. This formal consideration, Kingdon argues, has often already been shaped beforehand by the agenda-setting of the policy entrepreneur. See John W. Kingdon, *Agendas, Alternatives, and Public Policies*, Boston: Little, Brown, 1984.

The tide of critique faced by the Nixon administration was not confined to elite groups, however. Public discontent with American chemical and biological warfare research was also surging in 1969, prompted by the Vietnam War and accentuated by notorious Chemical Corps accidents like the 1968 Skull Valley incident. Likewise, not all scientists focused their activism on elite institutions. Rosebury, for example, consciously sought to engage public protest groups over public officials, employing a language of ethical protest over the technocratic rhetoric of figures like Meselson. This did not represent a surrender to military epistemic authority in his mind: he maintained what he believed to be "the most extensive file of open literature on BW outside of Detrick," served as an expert source for investigative journalists like Seymour Hersh, and billed himself, in his public appearances, as first and foremost a source of scientific information.[734] Indeed, the systematically gathered library open-source publications of which he was so proud represented a similar response to that developed, in institutional form, by elite groups like SIPRI. Rosebury eschewed the tactics of the Pugwash network because he saw them as overly narrow, obsessed with particular technologies, and unwilling to challenge the political status quo. For Rosebury, effective biological disarmament had to be rooted in political transformation: at least, a public mobilized to oppose this research as unethical, and preferably, an alleviation of the superpower conflict that justified such research. With broader ethical and political arguments to be had, he did not despair that "the military held the ace" by holding the precise details of their biowarfare experiments secret; he thought it irrelevant.

[734] Theodore Rosebury to Seymour Hersh, June 5, 1967 in NLM Theodor Rosebury Papers (MS C 634), Box 3 Folder 6 (Correspondence 'H' 3 of 3).

Ultimately, both approaches, technocratic and transformative, elite-focused and populist, were instrumental in persuading the Nixon administration to end biological weapons research. Elite-focused scientists in the Pugwash network had helped generate pressure from American politicians like Representative Richard McCarthy and international institutions like the UN and the WHO. Perhaps more importantly, they had helped build a consensus that biological weapons were militarily questionable and strategically undesirable, an important basis of a generally hawkish administration's willingness to abandon an entire weapons system in the face of public pressure. That pressure, too, was crucial however; serving as an anvil for the hammer of elite protest. As Rosebury demonstrated in the case of biological warfare, or as younger New Left-aligned scientists showed in the March 4th Movement, the public could be just as much an audience for scientific activism as political elites. Both Rosebury and the Pugwash group spoke as scientists, which meant that by the 1960s both had developed effective solutions to Lederberg's military secrecy dilemma. As their diverging tactics demonstrated, however, these solutions did not commit them to any one style of political engagement.

Scientists as Political Actors

It is an oversimplification to assert that political activism by American scientists did not begin until they "knew sin" in the atomic ending of the Second World War.[735] In the early decades of the 20th century, scientists in the United States embarked on a number of political causes across the political spectrum, from campaigns of eugenicists to the left-leaning antifascism of organizations like the American Association of Scientific

[735] "In some sort of crude sense which no vulgarity, no humor, no over-statement can quite extinguish, the physicists have known sin; and this is a knowledge which they cannot lose." Robert Oppenheimer, "Physics in the Contemporary World," *Bulletin of the Atomic Scientists* 4 no 3 (1948), pp 65-86, 66.

Workers (AASW).[736] This activity took place against a backdrop of rising social prestige of science and scientists as harbingers of "modernity," much like the engineers emerging as Progressive-era heroic archetypes.[737] The continuity of the post-WWII sociopolitical role of science with both these earlier activist antecedents and the longstanding growth of scientists' social prestige should thus not be obscured by a rhetoric of post-1945 discontinuity. Nonetheless, the advent of the atomic age certainly increased the political influence and social prestige of scientists, and witnessed a virtually-unprecedented intertwining of science and the American state.[738]

In light of this, it is unsurprising that scientists both found themselves entangled in the politics of Cold War anticommunism and in a position to serve as political actors in the first two decades of the Cold War. The former fact often served as a disciplining force on the latter, particularly in the late 1940s and early 1950s. Maintaining political influence both inside the government (as advisors to both military officials and politicians) and outside of it (with organizations like the FAS) entailed adhering to the anticommunist 'liberal consensus.'[739] Even within moderate circles, anticommunism

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[736] Elizabeth Hodes, "Precedents for Social Responsibility Among Scientists: The American Association of Scientific Workers and the Federation of American Scientists, 1938-1948," PhD diss, University of California, Santa Barbara, 1982; Peter J. Kuznick, *Beyond the Laboratory: Scientists as Political Activists in 1930s America*, Chicago: University of Chicago Press, 1987.

[737] On Progressive-era engineers and their "social responsibility" campaign, see Edwin T. Layton, *The Revolt of the Engineers: Social Responsibility and the American Engineering Profession*, Cleveland: Press of Case Western Reserve University, 1971

[738] The canonical study of pre-1940 Federal sponsorship of science is A. Hunter Dupree, *Science in the Federal Government: A History of Policies and Activities*, Cambridge, MA: Harvard University Press, 1957. Also see Larry Owens, "MIT and the Federal "Angel": Academic R & D and Federal-Private Cooperation before World War II," *Isis* 81 no 2 (1990), pp 188-213; Patrick J. McGrath, *Scientists, Business, and the State, 1890-1960*, Chapel Hill: University of North Carolina Press, 2002; Andrew Jewett, *Science, Democracy, and the American University: From the Civil War to the Cold War*, New York: Cambridge University Press, 2012; John Gascoigne, *Science and the State: From the Scientific Revolution to World War II*, New York: Cambridge University Press, 2019.

[739] See Robert Mason and Iwan Morgan (eds), *The Liberal Consensus Reconsidered: American Politics and Society in the Postwar Era*, Gainesville, FL: University Press of Florida, 2017. On scientists as advisors (particularly in the 1950s), see Gregg Herken, *Cardinal Choices: Presidential Science Advising from the*

could be used as a bludgeon by more 'hawkish' scientists like Edward Teller, as in the infamous 1954 hearings that removed Robert Oppenheimer's security clearance. Further to the left, meanwhile, organizations and individual scientists perceived as excessively critical of Cold War policies risked being decried as dupes or fellow-travelers, which carried with it potential professional sanctions, restricted ability to travel abroad, and a loss of credibility as 'objective' scientists uninfluenced by 'dogmas' like Communism.[740] Even as McCarthyist anticommunism thawed in the late 1950s and early 1960s, 'outsider' scientists like Barry Commoner had to navigate a far narrower course between political activism and retaining the cultural currency of scientific authority than

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Atomic Bomb to SDI, Stanford: Stanford University Press, 1992; Richard V Damms, "Scientists and Statesmen: Eisenhower's Science Advisers and National Security Policy, 1953-1961," PhD diss, Ohio State University, 1993; David L. Snead, *The Gaither Committee, Eisenhower, and the Cold War*, Columbus: Ohio State University Press, 1999; Richard V. Damms, "James Killian, the Technological Capabilities Panel, and the Emergence of President Eisenhower's 'Scientific-Technological Elite,'" *Diplomatic History* 24 no 1 (2000), pp 57-78; Benjamin P. Greene, *Eisenhower, Science Advice, and the Nuclear Test Ban Debate, 1945-1963*, Stanford: Stanford University Press, 2007. On the FAS, see Alice Kimball Smith, *A Peril and a Hope: The Scientists' Movement in America, 1945-47*, Chicago: University of Chicago Press, 1965; Megan Barnhart Sethi, "Information, Education, and Indoctrination: The Federation of American Scientists and Public Communication Strategies in the Atomic Age," *Historical Studies in the Natural Sciences* 42 no 1 (2012), pp 1-29; Patrick David Slaney, "Eugene Rabinowitch, the Bulletin of the Atomic Scientists, and the Nature of Scientific Internationalism in the Early Cold War," *Historical Studies in the Natural Sciences* 42 no 2 (2012), pp 114-142; David Kaiser and Benjamin Wilson, "American Scientists as Public Citizens: 70 Years of the *Bulletin of the Atomic Scientists*," *Bulletin of the Atomic Scientists* 71 no 1 (2015), pp 13-25. Also see S. Waqar H. Zaidi, "Scientists as Political Experts: Atomic Scientists and Their Claims for Expertise on International Relations, 1945–1947," *Centaurus* 63 no 1 (2021), pp 17-31.

[740] Jessica Wang, *American Science in an Age of Anxiety: Scientists, Anticommunism, and the Cold War*, Chapel Hill: University of North Carolina Press, 1999; Lawrence Badash, "Science and McCarthyism," *Minerva* 38 no 1 (2000), pp 53-80; Jessica Wang, "Scientists and the Problem of the Public in Cold War America, 1945–1960," *Osiris* 17 (2002), pp 323-347; Lawrence Badash, "From Security Blanket to Security Risk: Scientists in the Decade after Hiroshima," *History and Technology* 19 no 3 (2003), pp 241-256; David Kaiser, "The Atomic Secret in Red Hands? American Suspicions of Theoretical Physicists During the Early Cold War," *Representations* 90 no 1 (2005), pp 28-60; Jessica Wang, "Physics, Emotion, and the Scientific Self: Merle Tuve's Cold War," *Historical Studies in the Natural Sciences* 42 no 5 (2012), pp 341-388; Paul Rubinson, *Redefining Science: Scientists, the National Security State, and Nuclear Weapons in Cold War America*, Boston: University of Massachusetts Press, 2016; Audra J. Wolfe, *Freedom's Laboratory: The Cold War Struggle for the Soul of Science*, Baltimore: Johns Hopkins University Press, 2018; Mario Daniels, "Restricting the Transnational Movement of "Knowledgeable Bodies": The Interplay of US Visa Restrictions and Export Controls in the Cold War," in John Krige (ed), *How Knowledge Moves: Writing the Transnational History of Science and Technology*, Chicago: University of Chicago Press, 2019, pp 35-61; and Chapter 5 of Julia Rose Kraut, *Threat of Dissent: A History of Ideological Exclusion and Deportation in the United States*, Cambridge, MA: Harvard University Press, 2020.

politically moderate 'insiders' like the Presidential Scientific Advisory Committee's (PSAC) James Killian and George Kistiakowsky.[741] It was not until the late 1960s, with the rise of the 'New Left' and Vietnam-era protest movements, that a predominantly younger generation of scientists began to seriously transform the relationship between scientists and politics that had prevailed since the 1940s.[742]

The world at large offered other opportunities for scientists of various ideological stipes to exercise political influence. Reflecting the increased cultural power of science and scientists in the postwar world, science diplomacy was a favored tool of American officials, in venues ranging from new international bodies under the umbrella of the United Nations, like the International Atomic Energy Agency (IAEA) and World Health Organization (WHO), to within the NATO alliance, to informal personal exchanges.[743] The ideal of the 'universality' of science was a cultural resource underlying both this state use of international scientific meetings and relationships for its ends, and individual

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[741] Michael Egan, *Barry Commoner and the Science of Survival: The Remaking of American Environmentalism*, Cambridge, MA: MIT Press, 2007. On PSAC, see Zuoyue Wang, *In Sputnik's Shadow: The President's Science Advisory Committee and Cold War America*, New Brunswick: Rutgers University Press, 2008. On the ethical evolution of PSAC's membership, see Sarah Bridger, *Scientists at War: The Ethics of Cold War Weapons Research*, Cambridge, MA: Harvard University Press, 2015. See also Ann Finkbeiner, *The Jasons: The Secret History of Science's Postwar Elite*, New York: Penguin, 2006, for a discussion of another scientific advisory group of the post-Sputnik period. For a scientific/political controversy later in the Cold War, see Lawrence Badash, *A Nuclear Winter's Tale: Science and Politics in the 1980s*, Cambridge, MA: MIT Press, 2009.

[742] Kelly Moore, *Disrupting Science: Social Movements, American Scientists, and the Politics of the Military, 1945-1975,* Princeton: Princeton University Press, 2008. Also see John Campbell McMillian and Paul Buhle (eds), *New Left Revisited*, Philadelphia: Temple University Press, 2003; Sigrid Schmalzer, Daniel S. Chard, and Alyssa Botelho (eds), *Science for the People: Documents from America's Movement of Radical Scientists,* Amherst, MA: University of Massachusetts Press, 2018.

[743] See e.g. a special edition of *Osiris* entitled "Global Power Knowledge: Science and Technology in International Affairs," led by John Krige and Kai-Henrik Barth, "Introduction: Science, Technology, and International Affairs," *Osiris* 21 no 1 (2006), pp 1-21; Simone Turchetti, *Greening the Alliance: A History of NATO's Science and Environmental Initiatives*, Chicago: University of Chicago Press, 2018; Greg Whitesides, *Science and American Foreign Relations Since World War II,* New York: Cambridge University Press, 2019; and a 2020 special issue of *Historical Studies in the Natural Sciences* led by Simone Turchetti, Matthew Adamson, Giulia Rispoli, Doubravka Olšáková, and Sam Robinson, "Introduction: Just Needham to Nixon? On Writing the History of 'Science Diplomacy,'" *Historical Studies in the Natural Sciences* 50 no 4 (2020), pp 323-339.

scientists' use of them for theirs.[744] The ability of scientists to act with their own agendas was enhanced by the growth of *trans*national organizations to complement the international institutions of the postwar world. As scholars like Akira Iriye have pointed out, the second half of the 20th century can be seen as a period of growth of inter- and transnational linkages and civil society as much as a zero-sum world of superpower Cold War.[745] While some of the transnational groups that arose after the 1940s were effectively puppets of the superpowers (like the Congress for Cultural Freedom and the World Peace Council) others (particularly in the human rights, antinuclear, and non-Communist-aligned pacifist movements) effectively represented a challenge to the Cold War order.[746] Indeed, scholars like Matthew Evangelista have argued that such groups (including scientists' meetings in the Pugwash conferences) had an outsized influence on moderating and eventually ending the superpowers' Cold War, by establishing channels

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744 See Geert J. Somsen, "A History of Universalism: Conceptions of the Internationality of Science from the Enlightenment to the Cold War," *Minerva* 46 no 3 (2008), pp 361-379. Also see scholarship on transnational exchanges in science, e.g. Simone Turchetti, Nestor Herran, and Soraya Boudia, "Introduction: Have We Ever Been 'Transnational'? Towards a History of Science Across and Beyond Borders," *British Journal for the History of Science* 45 no 3 (2012), pp 319-336; Krige (ed), *How Knowledge Moves*.

745 Akira Iriye, *Global Community: The Role of International Organizations in the Making of the Contemporary World*, Berkeley: University of California Press, 2002.

746 Lawrence S. Wittner, *One World or None: A History of the World Nuclear Disarmament Movement Through 1953 (The Struggle Against the Bomb, Volume I)*, Stanford: Stanford University Press, 1993; Lawrence S. Wittner, *Resisting the Bomb: A History of the World Nuclear Disarmament Movement, 1954-1970 (The Struggle Against the Bomb Volume II)*, Stanford: Stanford University Press, 1997; Lawrence S. Wittner, *Toward Nuclear Abolition: A History of the World Nuclear Disarmament Movement, 1971-Present (The Struggle Against the Bomb Volume III)*, Stanford: Stanford University Press, 2003; Lawrence S. Wittner, *Confronting the Bomb: A Short History of the World Nuclear Disarmament Movement*, Stanford: Stanford University Press, 2009; Sarah B. Snyder, *Human Rights Activism and the End of the Cold War: A Transnational History of the Helsinki Network*, New York: Cambridge University Press, 2011; Akira Iriye, Petra Goedde, and William I. Hitchcock (eds), *The Human Rights Revolution: An International History*, New York: Oxford University Press, 2012; Lisa Rumiel, "Exposing the Cold War Legacy: The Activist Work of Physicians for Social Responsibility and International Physicians for the Prevention of Nuclear War, 1986 and 1992," in Virginia Berridge and Martin Gorsky (eds), *Environment, Health and History*, New York: Palgrave Macmillan, 2012, pp 224-243; Paul Rubinson, *Rethinking the American Antinuclear Movement*, New York: Routledge, 2018; Christoph Laucht, "Transnational Professional Activism and the Prevention of Nuclear War in Britain," *Journal of Social History* 52 no 2 (2018), pp 439-467; Petra Goedde, *The Politics of Peace: A Global Cold War History*, New York: Oxford University Press, 2019.

of communication and common ‘epistemic communities’ across the Iron Curtain.[747] In this context, scientists who opposed biological weapons research had to steer a careful political course, but one that might well bear fruit.

“Mr. B.W.”: Theodor Rosebury in the 1940s

The entry of the United States into the Second World War saw Theodor Rosebury, a 37-year-old microbiologist in Columbia University’s School of Dentistry, eager to join the war effort. “In these times I find it impossible to divide my loyalty suitably between the University and the United States Government,” he wrote in 1942, “I am… unwilling to hide behind the University’s ‘essential list.’”[748] A longstanding antifascist and member of the leftist American Association of Scientific Workers, Rosebury regarded the war against the Axis as an imperative, and chafed when he was rejected for a commission in the Army Dental Corps.[749] Besides regarding his research on the bacteria of the mouth as insignificant to the war, he worried about what military significance the expertise of bacteriologists like him could have. Germany had been a longstanding center of bacteriological expertise since the days of Robert Koch, and under the Nazis was not noted for its ethical scruples. What if German bacteriologists pursued the longstanding science fictional idea of using germs as weapons? What possibilities

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[747] Matthew Evangelista, *Unarmed Forces: The Transnational Movement to End the Cold War*, Ithaca: Cornell University Press, 1999. “Epistemic communities” (which emerged as a concept in political science in the early 1990s) are expert networks whose shared knowledge and presumptions lead them to advocate a particular set of policy goals, often across national boundaries. See Peter M. Haas, “Introduction: Epistemic Communities and International Policy Coordination,” International Organization 46 no 1 (1992), pp 1-35.
[748] Theodor Rosebury, “Note to be attached to Form 3874b (C.S.C.) February 1942 (Dental Enrollment Form for Procurement and Assignment Service), May 12 1942 in NLM Rosebury Papers, Box 5 Folder 7 (Correspondence ‘W’ 1 of 5).
[749] Rosebury held a DDS, but was explicitly rejected on the grounds that his had been a research- rather than practice-oriented career. See Colonel R. C. Craven to Theodor Rosebury, September 21, 1942 in NLM Rosebury Papers, Box 5 Folder 7 (Correspondence ‘W’ 1 of 5).

should the Allies fear? Taking up his pen a frustrated Rosebury, joined by Columbia immunologist Elvin Kabat, wrote a lengthy report for the AASW, arguing that effective biological warfare, particularly using pathogens notorious for causing unexplained laboratory infections, was too serious a possibility to be ignored. Biological warfare had been on the mind of those who, like Rosebury, took an ecological approach to infectious microbes.[750] Australia's F. Macfarlane Burnet had publicly speculated about the subject in his *Natural History of Infectious Disease* a few years before, while in California, Karl Meyer had (unbeknownst to Rosebury) already joined a secret National Academy of Sciences-sponsored group studying the use of germs as weapons.[751] Rosebury and Kabat's suggestion was a welcome one to this NAS group, however, and both scientists were quickly recruited to join the growing biological weapons program.[752]

By 1946, the war had been won, and Rosebury was back at Columbia. He had worked during the war at Camp Detrick, directing research into airborne infections and

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[750] Rosebury later recalled having become increasingly worried about the subject by early 1941, as a part of his membership in the AASW. See Theodor Rosebury, "Experiences at the Columbia Medical Center in Its Early Years," 1972 in NLM Rosebury Papers, Box 7 Folder 27 ("SDOS Lecture Seven").

[751] On the ecological approach of figures like Burnet and Meyer, see e.g. a 2016 special issue of the *Journal of the History of Biology* led by Pierre-Olivier Méthot and Rachel Mason Dentinger, "Ecology and Infection: Studying Host-Parasite Interactions at the Interface of Biology and Medicine," *Journal of the History of Biology* 49 no 2 (2016), pp 231-240; Mark Honigsbaum, "'Tipping the Balance': Karl Friedrich Meyer, Latent Infections, and the Birth of Modern Ideas of Disease Ecology," *Journal of the History of Biology* 49 no 2 (2016), pp 261-309. See also Warwick Anderson, "Natural Histories of Infectious Disease: Ecological Vision in Twentieth-Century Biomedical Science," *Osiris* 19 (2004), pp 39-61; Warwick Anderson, "Nowhere to Run, Rabbit: The Cold-War Calculus of Disease Ecology," *History and Philosophy of the Life Sciences* 39 no 2 (2017), pp 1-18; Mark Honigsbaum, "René Dubos, Tuberculosis, and the "Ecological Facets of Virulence," *History and Philosophy of the Life Sciences* 39 no 3 (2017), pp 1-28. Rosebury's own work on microbes that live on and in the human body is discussed in the larger context of developing ideas about human-microbe relations by Funke Iyabo Sangodeyi, "The Making of the Microbial Body, 1900s-2012," PhD diss, Harvard University, 2014.

[752] E. B. Fred, "Memorandum: Subject: Conference with Prof. A. R. Dochez and discussed the paper on Bacterial Warfare by Drs. Theodor Rosebury, Elvin A. Kabat and Martin H. Boldt," September 11, 1942 in National Academy of Sciences Archives collection "Committees on Biological Warfare, 1941-1948" (NAS BW) Box 7 Folder 19 ("Fred, E.B.: Memoranda (Black Book): 1942-1943")

nearly dying of a laboratory infection of his own in the process.[753] His pessimistic 1942 guess that the microbes that caused diseases like brucellosis, tularemia, and his own case of psittacosis could be deliberately caused through airborne transmission had been seemingly borne out by this work. Worse still, it became clear from his correspondents still working at Detrick that their research program was to remain a permanent fixture of the expanded post-war security state. Just as post-Hiroshima pessimism about a future dominated by the atomic bomb loomed large in public discussion, a similar pessimism about the future of biological warfare had been preoccupying Rosebury.[754] So too did the chilling of American relations with the Soviet Union, which unlike the war against the fascist powers struck him as regressive and unnecessary. "By V-J Day," he later wrote, "I had learned a great deal, and had given much thought to the question of Camp Detrick's legacy to a world at peace."[755] This thought continued through 1946, and culminated in the decision with Elvin Kabat to publish their 1942 report.

The biblical aphorism that "the truth shall make you free" lay at the root of Rosebury and Kabat's decision to publicly discuss biological warfare in this way. The formerly top-secret atomic bomb that had burst into the public imagination in the fall of 1945 was accompanied by an authoritative canon of knowledge about how it worked and what it meant: the book-length "Smyth Report." As historian Alex Wellerstein points out,

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[753] Theodor Rosebury, Harold V. Ellingson, Gordon Meiklejohn and Frank Schabel, "A Laboratory Infection with Psittacosis Virus Treated with Penicillin and Sulfadiazine, and Experimental Data Bearing on the Mode of Infection," *The Journal of Infectious Diseases* 80 no 1 (1947), pp 64-77.
[754] On the early pessimism of American atomic culture, see e.g. 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, 1985.
[755] "Autobiographical Sketch of Theodor Rosebury, Author of 'Peace or Pestilence,'" n.d. (probably December, 1948), in NLM Rosebury Papers Box 7 Folder 10 ("Peace or Pestilence: Contract from McGraw-Hill, Newspaper Clippings, Reviews").

this report was a *selective* release of information, coloring public perceptions of the Bomb and effectively reinforcing the nuclear secrecy system.[756] Nonetheless, the Smyth Report-colored identity of the Bomb was what dominated public discussions about future atomic war, establishing international control over atomic energy, and guarding "the" atomic secret from Red spies.[757] For members of the atomic scientists' movement (like those of the nascent FAS), such public discussion dominated by a physicist's report lent political authority. A would-be bioweapons scientist like Rosebury had no such legitimizing resource to rely upon. The existence of the American bioweapons program had been publicly revealed by the 4-page "Merck Report," but this brief and vague document in effect merely whetted the appetite of public imagination about this other new superweapon.[758] As it became clear that a longer official announcement would not be forthcoming, Rosebury became convinced that he would need to publish an authoritative report himself to lend legitimacy to vague public discussions about controlling biological weapons alongside atomic energy.

It was no accident that no official report appeared after the Merck Report. As much to the disappointment of supporters of Detrick's research as to those (like Rosebury) who wanted such research demilitarized, a tacit culture of official silence on biological warfare emerged after the Merck Report. Scientists who wished to publish

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[756] Alex Wellerstein, "Knowledge and the Bomb: Nuclear Secrecy in the United States, 1939-2008," PhD diss, Harvard University, 2012.

[757] David Kaiser, "The Atomic Secret in Red Hands? American Suspicions of Theoretical Physicists During the Early Cold War," *Representations* 90 no 1 (2005), pp 28–60.

[758] George W. Merck, "Biological Warfare: Report to the Secretary of War by Mr. George W. Merck, Special Consultant for Biological Warfare," January 3, 1946, in NAS BW Box 6, Folder 5 ("Merck Report to Secretary of War: "Biological Warfare": 1945"), also available on the NAS website at this URL: http://www.nasonline.org/about-nas/history/archives/collections/organized-collections/1945merckreport.pdf. Unlike the Smyth Report, the Merck Report was not widely published, though a copy did appear in the *Bulletin of the Atomic Scientists*. See George W. Merck, "Official Report on Biological Warfare," *Bulletin of the Atomic Scientists* 2 no 7-8 (1946), pp 16-18.

work they had done at Detrick already needed to have their papers officially cleared for publication (see Chapter 4), but this clearance became harder to obtain as the 1940s wore on. A former Detrick scientist like Rosebury could not simply publicly discuss the conclusions of the wartime program without such clearance, and given that the whole point of such a discussion would be to subvert official silence, this clearance would not be forthcoming. Publishing the 1942 report verbatim, composed before Rosebury had ever held a clearance or done any secret research, was his solution to this problem. Even this report he submitted for clearance, but besides simple foot-dragging by Detrick clearance officials and a request that no mention be made of his connection to the Army, the eventually conclusion was that he could not legally be prevented from doing so. Rosebury and Kabat submitted their report (with an explanatory note attached) to the *Journal of Immunology* in late 1946, to appear as the bulk of the March 1947 issue.[759]

They do not seem to have anticipated the full gamut of reactions that it would produce. Negative reactions among Ira Baldwin's circle of former and current BW researchers and advisors were immediate and forceful, despite the fact that the paper was scarcely comparable to the 'sensationalistic' *Popular Science* puff pieces that this group despised. "I… feel very strongly that articles like Rosebury's are extremely dangerous, ill-advised and should be nipped in the bud whenever possible," groused Walter

[759] Theodor Rosebury and Elvin A. Kabat, "Bacterial Warfare: A Critical Analysis of the Available Agents, Their Possible Military Applications, and the Means for Protection Against Them," *Journal of Immunology* 56 no 1 (1947), pp 7–96. The publication of this paper was initially held up in the military clearance process, but military censors eventually conceded that they had no grounds to stop publication so long as Rosebury did not list his affiliation with Camp Detrick on the paper. See Oram C. Woolpert to Theodor Rosebury, April 20, 1946, in NLM Rosebury Papers, Box 10 Folder 14 (Microfiche- Correspondence with the War Dept. on delayed publication of the monograph, "Bacterial Warfare.") "References to individual divisions, branches or other units with whom workers were connected" were also verboten (presumably for counterintelligence reasons). See A. E. Hayward to Theodor Rosebury, September 19, 1946, in NLM Rosebury Papers, Box 3 Folder 5 (Correspondence 'H' 2 of 3).

Nungester. "There has been entirely too much loose talk and speculation about B.W. for the good and welfare of all concerned in my opinion."[760] Harvard's J. Howard Mueller "refused to stand idly by and permit this sort of thing to happen," and lobbied to have Rosebury and Kabat fired.[761] In the year and a half since Rosebury had left Detrick, an implicit rule of not explicitly discussing "biological warfare" in public had arisen within this community; a tacit alliance with government secretiveness about the subject intended to forestall political scrutiny (see Chapter 4). Their cardinal sin was not speaking without scientific authority; quite the contrary, it was that violating this implicit culture of silence lent their authority to exactly the sort of lay discussion the Baldwin circle wanted to avoid.[762]

Rosebury compounded this 'sin' by building on the public notoriety the paper brought him. "In the brief hopeful interval between Hiroshima and McCarthy," he later reminisced, "I was Mr. B.W. among all the physicists in the anti-war movement."[763] In the absence of something like a Smyth Report, his article was (just as he had hoped) the major source of information on the potential nature of biological warfare for intellectual communities concerned about weapons of mass destruction.[764] Exemplified by organizations like the FAS, these communities were dominated by concern about atomic

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[760] W. J. Nungester to Ira Baldwin, June 6, 1947, in UWA Baldwin Papers, Box 6 Folder 1.

[761] Quote from J. Howard Mueller to Walter J. Nungester, Stuart Mudd, Thomas Francis, Jr., and Leland W. Parr, June 2, 1947, in Ibid. Rosebury and Kabat had already been called into the office of Dean Willard C Rappleye to be threatened with termination if they published it, before Rappleye eventually relented. See Elvin Kabat, "Getting Started 50 Years Ago: Experiences, Perspectives, and Problems of the First 21 Years," *Annual Review of Immunology* 1 (1983), pp 1-32, 19-23.

[762] Rosebury, *Peace or Pestilence*.

[763] Theodor Rosebury to Edgar Z. Friedenberg, December 23, 1971 in NLM Rosebury Papers, Box 2 Folder 16 (Correspondence 'F').

[764] For an examination of the genealogy of the term "weapons of mass destruction," see W. Seth Carus, "Occasional Paper 8: Defining 'Weapons of Mass Destruction,'" National Defense University Center for the Study of Weapons of Mass Destruction, 2006.

weapons and consequently, by the expertise of physical scientists. With biological warfare invoked in the same breath as atomic in the popular imaginary of futuristic 'super-weapons,' however, Rosebury's expertise was at a premium for such communities. "Mr. BW" was consequently a minor celebrity in such circles, invited to speak publicly by organizations ranging from moderate arms control advocates to radical groups like the AASW, spreading the warning that "the mighty microbe can go to war."[765] Besides this "unofficial peacemongering," Rosebury was the principal source to be consulted about biological warfare for figures ranging from *Scientific American* editor Gerard Piel to secrecy scholar Walter Gellhorn to Prudential Insurance's Edmund Berkeley.[766] By 1949, he expanded his anti-BW "missionary work" even further, publishing a popular book, entitled *Peace or Pestilence*. Much like Hans Zissner's *Rats, Lice, and History* or F. Macfarlane Burnet's *Natural History of Infectious Disease*, this book presented bacteriological concepts at the 'intelligent layman' level, but with a much more explicitly political warning about the potentials of biological warfare and dangers of the chilling Cold War. He walked a fine line with this "missionary work," with the legal imperative of revealing none the of the secrets he had been privy to compounded by the increased scrutiny his activism presumably placed him under.[767] As a former confidant of

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[765] This was the title of a flyer of a talk Rosebury gave on January 15, 1948, for a chapter of the Federation of Architects, Engineers, Chemists, and Technicians, a left-leaning labor union. Flier and attached correspondence in NLM Rosebury Papers, Box 2 Folder 16 (Correspondence 'F).

[766] Theodor Rosebury to Madeleine Brennan, October 3, 1947 in NLM Rosebury Papers, Box 2 Folder 5 (Correspondence 'B' 2 of 5); Theodor Rosebury to Walter Gellhorn, July 19, 1949, in NLM Rosebury Papers, Box 5 Folder 2 (Correspondence 'T' 2 of 2); Edmund C. Berkeley to Theodor Rosebury, April 7, 1948 in NLM Rosebury Papers Box 6 Folder 12 (Group of Future Catastrophe Hazards: A Staff Study by Lt. Col. Barnet W. Beers OSC). Berkeley is best-known to historians as the author of an early popular book about the promise of the electronic computer, *Giant Brains, Or Machines that Think*.

[767] Rosebury's concerns were well-founded, as his books were under CIA scrutiny following their publication, escaping prior restraint because they did "not come under any of the present espionage or security acts." R. H. Hillenkoetter to James A. Hamilton, May 23, 1949, CIA CREST Database (Document Number (FOIA) /ESDN (CREST): CIA-RDP80R01731R003100010053-3).

bioweapons secrets, he was in a sense guilty of revealing those secrets in any assertions he made unless he could affirmatively establish their innocent basis in the open scientific literature. He was consequently scrupulous about citing an open source for each specific claim he made in *Peace or Pestilence*. Claims that his former Detrick colleagues (or he) had previously published were fair game, including one convoluted chain of citation in which he could discuss the existence of a line of aerobiological experimentation he had led (but no other details about it), because he had mentioned it in a previous work, which in turn cited a personal communication with the wartime Safety Division director Gail Dack.[768] With this strategy, the military secrecy system could be as much a shield as a roadblock for him, as everything in a paper that had been cleared for publication effectively carried a military imprimatur.

This strategy only went so far, however, and the chilling of the Cold War eventually caught up with Rosebury's career of activism-through-information. This career had culminated in the summer of 1950, when Rosebury was hired by Otto Frey, a United Nations disarmament official, to compile a complete bibliography of papers published by biological warfare installations. Except for the work of Rosebury himself, a Smyth Report-shaped lacuna continued to confront arms control advocates interested in biological warfare, compounded by the failure of projects like the Federation of American Scientists' 1949 report to even reach the light of day. Carrying Rosebury's

[768] Rosebury explicitly discussed this chain of citation and rationale for it in Rosebury, *Peace or Pestilence*, p 194. He did not give Dack's wartime title, which was the sort of information that military censors had asked him not to publish when they cleared his 1942/1947 paper. "Personal communication" was the citation convention used to cite work that Detrick colleagues intended to publish (despite the fact that these plans were ultimately not always realized, as was the case in this "communication" with Dack). 15 of 67 references in Rosebury's 1947 *Experimental Air-Borne Infection*, for example, were to such "personal communications."

strategy to its logical conclusion with the institutional sponsorship of the UN seemed like a good start toward filling this gap, and he quickly began canvasing former Detrick colleagues for help with the project.[769] Instead, the project was quashed within a few weeks with dark unofficial warnings that a bibliography "of everything that has been published by known BW installations… [would] be both dangerous and misleading." Extending such a bibliography to "known British and Canadian installations… programs Dr. Rosebury may have acquired special knowledge [of] by virtue of his past official capacity," meanwhile, would provoke "a reaction [which] might be quite unfavorable."[770] The threat of Rosebury's prior access to secret matters being used against him was clear, and on his advice, the UN abandoned the project. The fact that he was being threatened for simply compiling a list of publicly available information, meanwhile, highlights the strange double life those papers led. Individually, they reported apparently innocuous scientific research, while *collectively*, they represented the existence of Detrick, an 'open secret' that government officials did not want to be discussed.[771] That same month, the

[769] See e.g. Theodor Rosebury to Dennis Watson, June 28, 1950, in NLM Rosebury Papers, Box 5 Folder 8 (Correspondence 'W' 2 of 5).

[770] Unsigned copy of letter to "Otto" (presumably Otto Frey), July 20, 1950, in NLM Rosebury Papers, Box 4 Folder 7 (Correspondence 'O'). Otto Frey was the UN official who commissioned Rosebury's study, and his correspondent was evidently an American government or UN official who he had previously informally consulted about the project (and who mentioned in turn that he had consulted with "people from whom I could get the necessary advice"). Rosebury evidently took these warnings seriously: on his copy of the letter several of the direst passages, including most of those that I have quoted above, are underlined in red pencil. A few years later, Rosebury sent relevant documents and an explanation of these events to the American Association for the Advancement of Science (AAAS), to serve "as a datum in any compilation on the relations between scientists and U.S. government agencies." Theodor Rosebury to Howard A. Meyerhuff, March 6, 1953 in NLM Rosebury Papers, Box 4 Folder 7 (Correspondence 'O').

[771] This is similar to a phenomenon in formal classification policy, in which a combination of words which themselves are either not secret or deserving of a lower classification can together constitute a greater secret. See Chapter 9 of Arvin S. Quist, *Security Classification of Information Volume 2: Principles for Classification of Information*, Oak Ridge: Oak Ridge National Laboratory, 1993. A crucial distinction, however, is that under a strict ontology of 'open' and 'secret' information, there was no secret to protect here: the existence of an American biological weapons program was publicly known, even if it was rarely publicly acknowledged. Michael Taussig calls such knowledge a 'public secret,' a practice of knowing but not talking about a thing which is constitutive of social order. (The act of talking about the thing thus

outbreak of the Korean War brought a new sense of crisis to the anticommunist politics of the now-frozen Cold War; within six, Rosebury had resigned (for unclear reasons) from the Columbia job Mueller had tried to have him fired from a few years before. The Theodor Rosebury who transplanted his career to Washington University in St. Lewis in 1951 was a seemingly chastised one, later reminiscing about "being very quiet on the subject" of biological warfare during the era of Korea and Joseph McCarthy.[772] "A book [like *Peace or Pestilence*] could hardly be written or published today," he mused in 1954, a week after the Army-McCarthy Hearings began.[773] Rosebury would return to the world of anti-BW activism later in the decade, but by that point his McCarthy-era cynicism about the wisdom of his 1940s informational crusade extended to its efficacy. He had tried being "Mr. B.W.," building opposition to biological warfare with information, and had failed. When Rosebury rejoined the world of activism at the end of the 1950s, it was as part of a larger community of scientists with ideas similar to those he had had, but their viewpoint was no longer his.

Joseph Rotblat and the Pugwash Movement

Across the Atlantic, another dissenting scientist was rebuilding his own career. Originally from Poland, Joseph Rotblat was born to a Warsaw family who were soon left impoverished by the First World War.[774] Largely self-educated and working as an

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serving as an implicit and sometimes existential challenge to that order). See Michael Taussig, *Defacement: Public Secrecy and the Labor of the Negative*, Stanford: Stanford University Press, 1999.

[772] Theodor Rosebury to Jesse Ehrenhaus, January 2, 1975 in NLM Rosebury Papers, Box 2 Folder 15 (Correspondence 'E').

[773] Theodor Rosebury to Frieda Halpern, April 27, 1954 in NLM Rosebury Papers, Box 7 Folder 10 (Peace or Pestilence: Contract from McGraw-Hill, Newspaper Clippings, Reviews).

[774] The two principal (albeit highly effusive) biographies of Rotblat are Martin Underwood, *Joseph Rotblat: A Man of Conscience in the Nuclear Age*, Sussex: Sussex Academic Press, 2009; Andrew Brown, *Keeper of the Nuclear Conscience: The Life and Works of Joseph Rotblat*, Oxford: Oxford University Press, 2012.

electrician before securing entrance into the Free University of Poland in 1929, he emerged late in the decade as a prominent figure in the growing experimental field of uranium fission. He found himself a war refugee in the UK in 1939, separated from his wife, who would later die in a German concentration camp. Having conceived of the military potential of his uranium work, he became part of the British atomic bomb project, Project MAUD, before transferring to Los Alamos, New Mexico for the American Manhattan Project in 1943. Rotblat chafed against the security restrictions at Los Alamos, and as it became increasingly apparent that the German bomb he feared and had been working to anticipate would not materialize, he resigned from the project in 1944 and returned to England. This prompted considerable suspicion of pro-Communist espionage by American counterintelligence officials, which would continue to dog him without apparent evidence in his subsequent career. Following the atomic bombings in Hiroshima and Nagasaki, Rotblat switched fields to medical physics (sacrificing a probable Royal Society appointment in the process), working at St. Bartholomew's Hospital, in London, on the biological effects of radiation. He also helped found the Atomic Scientists' Association (ASA), a British counterpart to the American FAS.[775] As a 'repentant' Manhattan Project researcher and an expert on radiation poisoning, he was naturally interested in the notorious 1954 case of the *Daigo Fukuryū Maru,* a Japanese fishing boat whose crew was afflicted by fallout from an American hydrogen bomb test,

See also the festschrift Reiner Braun, Robert Hinde, David Krieger, Harold Kroto, and Sally Milne (eds), *Joseph Rotblat: Visionary for Peace*, Weinheim, Germany: Wiley-VCH Verlag GmbH & Co. KGaA, 2007.

[775] On the ASA, see Christoph Laucht, "Atoms for the People: The Atomic Scientists' Association, the British State and Nuclear Education in the Atom Train Exhibition, 1947-1948," *British Journal of the History of Science* 45 no 4 (2012), pp 591-608. See also the introduction to the special issue of *BJHS* on British nuclear culture in which this article appears. Johnathan Hogg and Christoph Laucht, "Introduction: British Nuclear Culture," *British Journal for the History of Science* 45 no 4 (2012), pp 479-493.

and made a name for himself deducing the classified design of the American bomb from publicly available information about the fallout it produced.[776]

In the aftermath of this incident, Rotblat also joined in signing the Russell-Einstein Manifesto. Written by philosopher Bertrand Russel and nominally co-authored by a dying Albert Einstein in 1955, this manifesto pointed to nuclear fallout to decry the folly of thermonuclear war, and called for an international meeting of scientists from both sides of the Iron Curtain to foster peace. This call was the latest in a failed decade of similar proposals by 'dovish' Western scientists, such as those advanced by physicist-turned-biologist Leo Szilard in the 1940s.[777] These calls had been resoundingly rebuffed by scientists in Stalin's Soviet Union, officially condemned as "cosmopolitanism" which could only serve to legitimate bourgeois liberalism. Communist-aligned Western organizations like the World Federation of Scientific Workers (WFSW) and the World Peace Council toed this ideological line as well. With the death of Stalin and liberal reforms of Nikita Khrushchev in the mid-1950s, however, this Soviet position began to reverse. WFSW leaders like French physicist Frédéric Joliot-Curie, now willing to work with former ideological rivals in the ASA and FAS, responded favorably to the Manifesto's call. Presented with this Communist thaw and the declining fortunes of McCarthyism in the US, Rotblat and the FAS' Eugene Rabinowitch began to seek to

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[776] Alison Kraft, "Dissenting Scientists in Early Cold War Britain: The "Fallout" Controversy and the Origins of Pugwash, 1954–1957," *Journal of Cold War Studies* 20 no 1 (2018), pp 58-100. The ship was known as the *Lucky Dragon* in English-language reporting.
[777] See William Lanouette, *Genius in the Shadows: A Biography of Leo Szilard, The Man Behind the Bomb, rev. ed.*, New York: Skyhorse Publishing 2013, pp 363-384.

organize such a meeting in earnest, eventually securing funding from financier Cyrus Eaton, an outspoken critic of the Cold War.[778]

Scientists from 10 nations attended the meeting, held in 1957 near Eaton's boyhood home of Pugwash, Nova Scotia, including (to the surprise of the Anglo-American organizers), several high-level members of the Soviet Academy of Sciences like Aleksandr Topchiev. As these scientists were high-ranking advisors within the Soviet government, they were taken by many Western observers to constitute a semi-official Soviet presence, lending a sense of legitimacy to the meeting's solemn prognostications against the nuclear arms race.[779] With the meeting's results publicized in the *Bulletin of the Atomic Scientists* (whose editor, Eugene Rabinowitch, was a meeting participant) and the Soviet press, further meetings were planned.[780] For the next two years, several more meetings took place, sponsored variously by Eaton and the Austrian Körner Foundation. The Vienna Declaration produced at the third meeting in 1958, was also widely publicized, serving as a central document in what was becoming a movement within the scientific community.[781] This ad hoc series of conferences crystalized around the organizational leadership of Rotblat and a central governing Continuing Committee

[778] Joseph Rotblat, *Scientists in the Quest for Peace: A History of the Pugwash Conferences*, Cambridge, MA: MIT Press, 1972, pp 1-7. On Eaton, see the neigh-hagiographic M. Allen Gibson, *Beautiful Upon the Mountains: A Portrait of Cyrus Eaton*, Windsor, Nova Scotia: Lancelot Press, 1977.
[779] This presumption of close ties between the Soviet delegation and their government was correct: see Matthew Evangelista, *Unarmed Forces: The Transnational Movement to End the Cold War*, Ithaca: Cornell University Press, 1999, pp 32-35.
[780] Eugene Rabinowitch, "Pugwash- History and Outlook," *Bulletin of the Atomic Scientists* 13 no 7 (1957), pp 243-248.
[781] The 3rd meeting took place in close temporal and geographic proximity with the first meetings of the Eighteen Nation Conference on Disarmament and the International Atomic Energy Agency, and included some of the same participants. See Elisabeth Röhrlich, "An Attitude of Caution: The IAEA, the UN, and the 1958 Pugwash Conference in Austria," *Journal of Cold War Studies* 20 no 1 (2018), pp 31-57 for a discussion of the sometimes-fought relationship between these international meetings and the transnational Pugwash conference.

established in 1957. The central organization that developed was in many ways an extension of Rotblat himself. Serving as the Secretary (later General-Secretary, at Soviet members' insistence) of the organization, Rotblat acted as the center of a network of correspondence, using his London home as the group's official headquarters through much of the 1960s. The core of the Continuing Committee drew upon prominent figures from the FAS and ASA, along with Topchiev, who would be an enthusiastic leader in the Pugwash movement until his unexpected death in 1962.[782] While the group's membership was characterized by its national and ideological heterogeneity, it was this inner cadre of leaders, especially Rotblat, who effectively guided what was becoming known as the Pugwash movement.[783]

Pugwash was dominated by physicists, and the thought community that coalesced in these early meetings primarily developed an expertise on the effects of nuclear weapons and the technopolitics of nuclear disarmament. However, the ambition of Rotblat and the other core of leaders was to develop an extragovernmental, transnational community of elite scientists able to speak authoritatively on essentially all issues of

[782] Rotblat, *Scientists in the Quest for Peace*, pp 33, 88.

[783] Pugwash histories can be divided into two categories: memoirs and official histories (written by Rotblat), and a small but growing body of secondary scholarship which has developed since Pugwash and Rotblat shared the 1995 Nobel Peace Prize. For the former, see Rotblat, *Scientists in the Quest for Peace*. For the latter, see Evangelista, *Unarmed Forces* (in which the Pugwash network is one of the major actors), a 2018 special issue of the *Journal of Cold War Studies* which is introduced by Alison Kraft, Holger Nehring, and Carola Sachse, "The Pugwash Conferences and the Global Cold War: Scientists, Transnational Networks, and the Complexity of Nuclear Histories," *Journal of Cold War Studies* 20 no 1 (2018), pp 4-30; and the recent edited volume (by the same research team) Alison Kraft and Carola Sachse, *Science, (Anti-)Communism and Diplomacy: The Pugwash Conferences on Science and World Affairs in the Early Cold War*, Boston: Brill, 2020. Pugwash is featured and place in context in Lawrence S. Wittner's magisterial history of global antinuclear activism. See Lawrence S. Wittner, *Resisting the Bomb: A History of the World Nuclear Disarmament Movement, 1954-1970 (The Struggle Against the Bomb Volume II*, Stanford, Stanford University Press, 1997. Pugwash also appears as an actor in more general works on Cold War science. See e.g. Rubinson, *Redefining Science*; Wolfe, *Freedom's Laboratory*; and, as the title suggests, Gerson S. Sher, *From Pugwash to Putin: A Critical History of US–Soviet Scientific Cooperation*, Bloomington: Indiana University Press, 2019.

great power conflict and armaments. Much like the anti-war physicists Rosebury collaborated with in the 1940s, these leaders sought to incorporate expertise on chemical and biological weapons into their community. As British biologist and Pugwash member John Humphrey would later confide to the prominent British scientific advisor Sir Solly Zuckerman, "the physicists, who predominate, are worried about BW as something of which they do not have the measure (by contrast with nuclear warfare, concerning the possibilities of which enough is known and discussed to make its consequences- within limits- predictable)."[784] To build similar expertise, the Pugwash leadership enlisted the aid of their few biomedical members, for whom building a Pugwash BW community offered a way to legitimate earlier individual activism. The most prominent of these was former WHO General Secretary Brock Chisholm, a Canadian psychiatrist who had been an opponent of biological weapons since the end of the Second World War.[785] He was joined by Martin Kaplan, an American epidemiologist who served as head of the WHO's Communicable Diseases section.[786] Under Chisholm, the WHO had been the major international institution to pay attention to the dangers of biological warfare, and both Chisholm and Kaplan had encountered the problems of discerning deliberate biological

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[784] John Humphrey to Solly Zuckerman, June 8, 1965, p 1, in Cambridge University Churchill Archives Center GBR/0014/RTBT (Joseph Rotblat Papers) (RTBT), Series 5/2/5/3 Folder 4. This remarkable admission, in a letter to Zuckerman regarding the participation of the British BW research site at Porton Down in the Pugwash inspection experiment (see below), should not be read as rhetorically neutral. Zuckerman was an influential part of British government decision-making, and Pugwash's typical rhetorical strategy was to emphasize the unimportance of BW as a weapons system- which made it a better disarmament 'test case.'

[785] Chisholm had spontaneously raised the topic of biological warfare at the 3rd Pugwash conference in Austria. See Joseph Rotblat to Brock Chisholm, January 22, 1959 in RTBT 5/2/5/3 Folder 13. On Chisholm, see John Farley, *Brock Chisholm, the World Health Organization, and the Cold War*, Vancouver: University of British Columbia Press, 2008.

[786] Margalit Fox, "Martin Kaplan, 89, Health Official Who Fought the Spread of Disease, Dies," *The New York Times*, November 21, 2004, Section 1, p 47. See also a collection of personal reminiscences by Pugwash luminaries about Kaplan (who served as the organization's Secretary-General in 1976-1988), collected shortly after his death in 2004 in RTBT 5/4/3/12.

warfare from naturally occurring disease during the Korean War, when China alleged that the US had used BW against its troops (a charge the US hotly denied).[787] Kaplan volunteered his services to Rotblat at the Vienna conference, and was placed in charge of organizing the CBW conference.[788] For Kaplan and (after some initial skepticism) Chisholm, this represented an opportunity to build a transnational community of scientists sharing their concerns about biological warfare. They enlisted Rosebury, who had been publicly silent on the topic since 1950, to help construct a bibliography.[789]

Besides Rosebury himself, the group of invitees Kaplan put together had generally not been involved with biological warfare research. Some participants noted that their expertise was only general, including geneticist and Pugwash continuing committee member H. Bentley Glass, who Rotblat included on the program "to have some sort of continuity with previous Pugwash conferences."[790] The presence of Glass, and indeed, non-biologist Pugwash leaders like Rotblat and Patricia Lindop did serve exactly this purpose, shaping the new participants into a Pugwash community.[791] This dearth of 'insiders' was both a weakness and a strength. As several participants noted, this lack called their assessments into question, and even the knowledge of Rosebury and

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787 See Martin M. Kaplan, "The Efforts of WHO and Pugwash to Eliminate Chemical and Biological Weapons: A Memoir," *Bulletin of the World Health Organization* 77 no 2 (1999), pp 149-155 for a discussion of the WHO and the Korean War allegations.

788 Ibid, p 150. Pugwash Continuing Committee to Martin Kaplan, March 14, 1959 in RTBT 5/2/1/5 Folder 15.

789 Joseph Rotblat to Theodor Rosebury, June 23, 1959 in RTBT 5/2/1/5 Folder 14.

790 Joseph Rotblat to Bentley Glass, June 30, 1959, in RTBT 5/2/1/5 Folder 13.

791 Glass had, however, previously publicly spoken out against biological warfare, delivering a speech on Baltimore radio in 1949. See Bentley Glass, "Biological Warfare- A Sober Estimate," April 1, 1949 in American Philosophical Society Archives (APS) Mss.Ms.Coll.105 (Bentley Glass Papers), Box 109 Unnumbered Folder Entitled "Biological Warfare- 1949." Glass drew upon Rosebury's expertise in preparing for this speech. See Bentley Glass to Theodor Rosebury, March 9, 1949 in NLM Rosebury Papers, Box 3 Folder 21 (Correspondence 'M' 2 of 6).

Chisholm was over a decade out of date.[792] On the other hand, it meant that unlike Rosebury, whose Second World War clearance colored his freedom to criticize BW in subsequent years, the other participants did not have to worry about revealing classified information that they did not have access to. This seems to have been a deliberate strategy: Joshua Lederberg was invited, despite having only nominally held a clearance for Detrick contract research years before, but when he demurred and suggested that recent ex-Detrick bacterial geneticist Werner Braun with his "far more relevant experience" be invited instead, the Pugwash leaders did not take this advice.[793] Ultimately, the fact that the Pugwash group successfully came into existence and produced an assessment they considered satisfactory served to demonstrate to Pugwash's leadership that their model of open-source scientific discussions of security issues could be applied to BW. The conference was thus important in dispelling the rhetorical power of secrecy for would-be BW opponents. The Pugwash group's very existence asserted that biomedical scientists, without access to specific classified information, were qualified to make assessments about the dangers and wisdom of biological warfare research.

Over half of the invitees (almost all of whom were western, with the Soviet Academy of Science providing a delegation without input from Pugwash) declined their invitations (as Lederberg did). Pugwash was still a largely unfamiliar and politically

[792] "Minutes of Second Meeting," August 24, 1959 in RTBT 5/2/1/5 Folder 8.
[793] Lederberg to Rotblat, March 31, 1959, ms postscript, in RTBT 5/2/1/5 Folder 4. Braun's name is not present on any of the lists of prospective attendees or invitation letters in RTBT 5/2/1/5 Folder 3-4. Another reason he may not have been invited is his continued support for Detrick's work. For example, see a memorandum to this effect that he signed a few years later, "Confidential memorandum for Dr. C. McLeod to be used at his discretion," November 29, 1961 in ASM 13-IIAT (Baldwin Presidential Papers), Folder 54.

suspect organization at the time where, as an anonymous American participant put it later, one ran the risk of "los[ing] all one's 'clearances'" for attending.[794] While one must account for hyperbole in this statement, a reluctance to associate with a group with suspect political loyalties may have made potential participants more reticent. Additionally, while Pugwash's American and British physicists largely came out of a preexisting tradition of organizations critical of nuclear weapons like the FAS and ASA, biological weapons had not inspired similar political organization among biologists and medical professionals. In this sense, then, Kaplan, Rosebury, and Rotblat faced the unenviable task of building such a political community of scientists from scratch. However, even scientists potentially willing to take on the political risks of activism did not always agree that Pugwash's elite-focused secretive ethos was an appropriate foundation for such a community. Harvard's René Dubos, for example, initially accepted his invitation, but subsequently demurred as he became more aware of this ethos. "In my opinion," he wrote to Rotblat, "the first need is to make the general public aware of the problem and perhaps to acquaint the scientific community with some of its public aspects. I do not see how either of these ends would be served by a 'private' meeting." For Dubos, the confidentiality which accompanied Pugwash's orientation away from public opinion was misguided, and "might create suspicion and ill will by making scientists appear as conspirators."[795]

Those who did agree to attend met at Pugwash in September 1959, funded once again by Cyrus Eaton. The conference participants were marked by a notable diversity of

[794] "American Participant," quoted in Solly Zuckerman, "Report: Conference on Science and World Affairs (8th Pugwash Meeting), Vermont, September 11-16, 1961," in RTBT 5/1/3/7.
[795] René Dubos to Joseph Rotblat, April 21, 1959, in RTBT 5/2/1/5 Folder 3.

viewpoints, disagreeing about the military logic of biological warfare, the efficacy of germs as weapons and even whether biological weapons were notably abhorrent.[796] The assessment they finally produced leaned heavily on a language of possibility, befitting the non-privileged nature of the group's expertise. Based on the state of the art in virus research, for instance, the group agreed that viral and rickettsial diseases were a greater BW threat than they had been a decade before. Control, however, was more doubtful, with the group expressing skepticism that militarily useful predictions could be made. The participants were especially 'optimistic' about the ability of bioweapons researchers to use modern lab techniques to modify existing pathogens into more militarily useful forms. As Kaplan noted, however, such novel organisms would be even more epidemiologically unpredictable than unmodified pathogens, as the only data to predict virulence, symptoms, and casualties would be the necessarily limited (generally animal) data generated by secret experiments. Biological warfare, in short, had great and growing destructive potential (whatever the exact state of secret research happened to be), though the group opined that such weapons ultimately posed far less danger to the world than did thermonuclear weapons.[797]

Cyrus Eaton, who evidently saw BW as the ultimate technoscientific perversion by the Cold War system, took great exception to this conclusion by 'his' scientists and unsuccessfully tried to have this assessment changed.[798] This direct attempt at interference would contribute to the growing break between him and the Pugwash

[796] Minutes of the conference can be found in RTBT 5/2/1/5 Folders 8-12.
[797] "Statement of Pugwash International Conference of Scientists On Biological and Chemical Warfare," August 29, 1959 in RTBT 5/2/1/5 Folder 2.
[798] This incident is discussed in Joseph Rotblat, "Notes taken at the Meeting of the Pugwash Continuing Committee, Tuesday morning, June 21st," (1960), p 7 in RTBT 5/3/1/6 Folder 3.

organization, as Rotblat, Kaplan, and the other leaders sought to assert their political independence. In subsequent years, the Pugwash conferences themselves underwent a metamorphosis in meaning if not in form, as they solidified as channels for informal contact and diplomacy between politically influential scientists of both superpowers, and as leaders like Rotblat correspondingly sought to excise their earlier tinge of radicalism by marginalizing critical 'outsiders' like Russell.[799] As early as 1958, Soviet and American scientists were using Pugwash meetings as a backchannel for nuclear test ban negotiations, and by 1960 Pugwash leaders (seeking to encourage such informal diplomacy at their meetings) delayed a planned Moscow meeting until after the American presidential election to enable scientists working on the Kennedy campaign to attend.[800] This trend of political acceptability continued with the next year's conference, held in the US and well-attended by American 'insiders' to the world of government scientific

[799] Rotblat explicitly discusses distancing Pugwash from Russell, whose Campaign for Nuclear Disarmament had just launched a controversial civil disobedience campaign, in Rotblat to William M. Swartz, October 5, 1960 in RTBT 5/4/1/20 Folder 6. Russell remained the titular leader of the organization for several more years, before being formally deposed in the mid-1960s. See Joseph Rotblat to Lord Bertrand Russell, October 2, 1967 in RTBT 5/4/12/15 Folder 2. See "Notes Taken at the Meeting of the Pugwash Continuing Committee on Wednesday Afternoon, June 22nd," (1960) pp 5-6 in RTBT 5/3/1/4 Folder 1 for a discussion by Pugwash leaders about how to distance themselves from Eaton without irreparably offending him. Rotblat and his moderate faction's remaking of Pugwash extended to official histories which members of left-leaning organizations like the World Federation of Scientific Workers saw as diminishing such organizations' role in founding the conferences in the first place. See e.g. E. H. S. Burhop, "The World Federation of Scientific Workers and the Origin of the Pugwash Movement," n.d. (ca. 1966) in RTBT 5/1/4/6, a retrospective which sharply challenges the first official history, Joseph Rotblat, *Pugwash- The First Ten Years: History of the Conferences of Science and World Affairs*, New York: Humanities Press, 1967.

[800] Most notable among these 1960 attendees were incoming Kennedy administration advisors Walt Rostow and Jerome Wiesner, who informally discussed a nuclear test ban treaty with Soviet officials on the sidelines of the conference. See Rubinson, *Redefining Science*, p 106. The Pugwash meetings were serving as an informal conduit between politically connected Soviet and American scientists as early as the 1958 3rd conference in Austria, where Soviet geophysicist Academician Evgeniy Fedorov, returning from the US-Soviet Geneva Conference of Experts (called to discuss verification of a nuclear test ban), candidly discussed Soviet political divisions about the test ban with American physicist Victor Weisskopf, who in turn shared these insights with Presidential Science Advisor James Killian. See Wang, *In Sputnik's Shadow*, pp 131-132. On these internal Soviet debates about the desirability of a nuclear test ban and the role of Soviet scientists in advocating such a ban, see Evangelista, *Unarmed Forces*, pp 51-67.

advising. As one of these attendees anonymously put it to British scientific advisor Solly Zuckerman, while “before, if one went to these meetings, one lost all one’s ‘clearances’; to come to this one, one practically needed a special pass to show that you were a trusted man.”[801] Rotblat’s vision of the Pugwash conferences serving as neutral ground for informal contacts between the superpowers was firmly entrenched by the mid-1960s, offering the political influence the early movement had craved at the price of its pretentions to unfettered ‘outsider’ erudition.

The attention of Pugwash leaders was largely devoted to this transformation, and to meddling in high diplomacy with events like the Moscow conference and a 1963 symposium on seismology, intended to facilitate ongoing test ban negotiations.[802] Between the 1959 group’s conclusion that thermonuclear warfare endangered the world less than biological, and the continued dominance (with the notable exception of Martin Kaplan) of physical scientists among Pugwash leaders and membership, the group generally neglected biological warfare in the years following the 1959 meeting. This began to change in the mid-1960s with the impetus of a small cadre of biologist members of Pugwash rather than the organization’s leadership. This push for further Pugwash community-building in biological arms control emerged at the 1963 11th Pugwash conference, led by Czechoslovakian microbiologists Ivan Málek and Karel Raška. At this conference, Málek and Raška argued that both the optimism of the 1959 conference (in asserting that thermonuclear weapons were a more dangerous technology than biological weapons) and its pessimism (in asserting that biological arms control was essentially

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801 “American Participant,” quoted in Solly Zuckerman, “Report: Conference on Science and World Affairs (8th Pugwash Meeting), Vermont, September 11-16, 1961,” in RTBT 5/1/3/7.
802 See correspondence and materials from this meeting in RTBT 5/2/16/1.

impossible to achieve) were misplaced. Rather, they argued, the destructive potential of biological weapons was highly uncertain and potentially on the order of nuclear weapons, while such weapons offered much smaller countries than the Cold War superpowers nuclear-like means of mass destruction. Nonetheless, they argued, biological disarmament could be buttressed by the expertise of an international scientific organization like Pugwash, by developing techniques, technologies, and institutions to identify and counter biological attacks.[803] Their paper should not be seen in a vacuum: as Doubravka Olšáková has argued, Czechoslovakian Pugwash participation in the 1960s, like that of other Communist-bloc countries, was often closely controlled by the Soviet Academy of Sciences and the Party, through the sometimes-unenthusiastic Czechoslovakian Academy of Sciences. She has briefly argued that Málek and Raška's BW paper should thus be seen as much as a semi-official diplomatic initiative as their personal project.[804] In any event, their initiative was convincing to Western attendees like Glass and Kaplan, who had both participated in the 1959 conference, as well as the American molecular biologist Matthew Meselson, a recent convert to the cause of biological arms control after a stint as a consultant at the Arms Control and Disarmament

[803] I. Málek and K. Raška, "Some Problems of Disarmament in the field of Biological Warfare," in *Proceedings of the Eleventh Pugwash Conference on Science and World Affairs: "Current Problems of Disarmament and World Security"*, London: Pugwash Continuing Committee, 1963, pp 194-198. See also J. P. Perry Robinson, "The Impact of Pugwash on the Debates over Chemical and Biological Weapons," *Annals of the New York Academy of Sciences* 866 no 1 (1998), pp 224-252.

[804] Doubravka Olšáková, "Pugwash in Eastern Europe: The Limits of International Cooperation Under Soviet Control in the 1950s and 1960s," *Journal of Cold War Studies* 20 no 1 (2018), pp 210-240. It would nonetheless be a mistake to interpret the two men as mere Party stooges: they both navigated Party bureaucracy to advance their scientific interests and political goals in the liberalizing period of the 1960s, and both suffered loss of influence and virtual confinement within Czechoslovakian borders in the wake of the 1968 Soviet invasion that suppressed the Prague Spring. Epidemiologist Walter Holland recalls Raška, for instance, going out of his way to warn him that their conversations would be eavesdropped upon when the two met in Prague in 1971. See Walter W. Holland, "Karel Raška- The Development of Modern Epidemiology. The Role of the IEA," *Central European Journal of Public Health* 18 no 1 (2010), pp 57-60.

Agency.[805] The convert's fervor of Meselson may have played a part in this friendly reception, but the credibility of Málek was also likely a factor. An internationally prominent researcher in continuous culture fermentation techniques, a line of research pursued by bioweapons research establishments like Porton Down, Málek possessed what Glass, Kaplan, and Meselson did not: prominence as an expert in a body of research directly relevant to biological warfare.[806]

Joined by a network of European biologists, most notably Carl-Göran Hedén of Sweden's Karolinska Institute, these scientists organized themselves into a "Biological Weapons Study Group," meeting separately from the regular conferences but still sponsored by Pugwash leadership.[807] Unlike a regular Pugwash conference, whose attendees were numerous and inconsistent, and whose formal proceedings had principally

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[805] On Glass' reaction to Málek and Raška's paper, see Bentley Glass, "The Role of Ivan Málek in the Pugwash Conferences on Science and World Affairs," July 1968 in APS Glass Papers, Box 120 Unnumbered Folder Entitled "Role of Ivan Malek in the Pugwash Conferences on Science and World Affairs." Befitting the social world of moderate American defense-connected scientists from which he came, Meselson's membership in the incipient BW group (a subject with which he had one summer's professional experience) came after attending his first Pugwash conference to present a paper on European security (about which he had even less professional knowledge). On Meselson's ACDA 'conversion,' see Joel Primack and Frank von Hippel, *Advice and Dissent: Scientists in the Political Arena*, New York: Basic Books, 1974, pp 147-148, 161. This book on the role of American scientists in policymaking, by physicists Primack and von Hippel, includes a chapter on Meselson and his anti-CBW activism, based extensively on interviews with Meselson himself.

[806] Alan T. Bull, "Ivan Málek [1909-1994]: A Tribute," *Journal of Chemical Technology and Biotechnology* 86 no 5 (2011), pp 621-624. "Continuous culture" referred to growing microorganisms (such as pathogens) in an apparatus with continuous inputs and outputs, as opposed to older "batch culture" techniques. Continuous culture involved considerable challenges in 'quality control' of the microbes in question (for instance, avoiding the outcompetition of the desired strain by undesirable mutants), and entailed considerable tacit knowledge on the part of research groups at the liminal space between biology and engineering. This research tradition flowered in the 1950s and '60s but failed to achieve practical commercial success. Málek and the Porton group were conscious members of an international community of such continuous culture researchers, holding a conference on the topic in Czechoslovakia in 1958. On this conference and continuous culture research at Porton, see Robert Bud, *The Uses of Life: A History of Biotechnology*, New York: Cambridge University Press, 1993, pp 111-116; Robert Bud, "Biological Warfare Warriors, Secrecy and Pure Science in the Cold War: How to Understand Dialogue and the Classifications of Science," *Medicina Nei Secoli Arte E Scienza* 26 no 2 (2014), pp 451-468.

[807] Like Málek, Hedén was a prominent expert in fermentation and evangelist for biotechnology. Bud argues that despite a lack of formal engineering training, Hedén's skill at constructing biotechnological apparatus embodied an "instrumental" tradition of early 20th century Swedish biochemistry, exemplified by Svedberg's development of the ultracentrifuge in the 1920s. See Bud, *The Uses of Life*, pp 97-99.

devolved to the negotiation of the text of a final statement, the "Study Group" model was based on establishing a small group of subject-specific experts concentrating on a particular arms control problem. This model was a favorite of Rotblat's, who had pioneered it in early 1963 by organizing a conference on hypothetical "black box" seismographic technology capable of detecting covert underground tests.[808] Rotblat's logic was fundamentally technocratic: such tests had been a major impasse in the superpowers' test ban negotiations, so establishing a consensus about the feasibility of such technology could break it. Undeterred by the partial treaty which emerged later in the year, he continued to seek to influence political elites by presenting them with hypothetical technological solutions to security problems.[809] The Biological Weapons Study Group offered a prime opportunity to do so, and Rotblat enthusiastically sponsored it.

The group saw a flowering of early activity, with its first meeting on the sidelines of the 1964 conference swiftly followed by several other meetings that set the Group's

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808 See correspondence and materials from this meeting in RTBT 5/2/16/1; Rotblat, *Scientists in the Quest for Peace*, p 33. There is a large historiography on the test ban negotiations, situated at the junction between diplomatic history, environmental history, and the political role of scientists in the negotiations. See e.g. Robert A. Divine, *Blowing on the Wind: The Nuclear Test Ban Debate, 1954-1960*, New York: Oxford University Press, 1978; Per Fredrik Ilsaas Pharo, "A Precondition for Peace: Transparency and the Test-Ban Negotiations, 1958–1963," *The International History Review* 22 no 3 (2000), pp 557-582; Greene, *Eisenhower, Science Advice, and the Nuclear Test Ban Debate*; Paul Rubinson, "'Crucified on a Cross of Atoms:' Scientists, Politics, and the Test Ban Treaty," *Diplomatic History* 35 no 2 (2011), pp 283-319; Simone Turchetti, "'In God We Trust, All Others We Monitor': Seismology, Surveillance, and the Test Ban Negotiations," in Simone Turchetti and Peder Roberts (eds), *The Surveillance Imperative: Geosciences During the Cold War and Beyond*, New York: Palgrave Macmillan, 2014, pp 85-104; Toshihiro Higuchi, *Political Fallout: Nuclear Weapons Testing and the Making of a Global Environmental Crisis*, Stanford: Stanford University Press, 2020.

809 Rotblat's pursuit of influence over elite arms control negotiations was challenged by American Pugwash leader Eugene Rabinowitch, who argued that the organization should instead focus on addressing wider causes of conflict, like international development inequities. Both leaders' views were enacted in subsequent years, with several conferences in the late 1960s revolving around Rabinowitch's vision of focusing on broader problems of international development. See Rubinson, *Redefining Science*, pp 118-127.

major strategies. Most notable of these was the decision to focus on biological weapons alone, neglecting the issue of chemical weapons disarmament.[810] While the 1959 conference had treated the two as inextricably linked, separating them allowed the biologists to keep their group small and focused. Perhaps more importantly for the Pugwash focus on influencing policymakers, this decision allowed them to problematize an unproven and poorly institutionally entrenched type of weapon without simultaneously attacking one which had been part of military planning since the First World War. To accomplish this, the group embarked on three major projects.[811] One of these was to build a body of authoritative open-source information to challenge the secrecy which surrounded biological weapons research, the longstanding goal of scientific critics of biological warfare dating back to Rosebury in the 1940s. The other two projects also sought to subvert the secrecy system, but they were far more focused on technocratic elite politics. First, paralleling the 'black box' conference, the group sought to establish a consensus on the feasibility of early warning technologies to detect biological attack, presuming that this would replicate research in the classified world. Second, they sought to establish a system for inspecting civilian laboratories for biological weapons work. By developing the right combination of techniques and technology, they hoped to ensure that secrecy would not subvert a future arms control agreement. The group agreed that they should cultivate contacts with military-connected scientists and government officials, feeling (as Hedén put it) "that our recommendations will be anemic if we do not

[810] "Report of the Third Session of the Pugwash Study Group on Biological Warfare, Trieste, 8-10 April, 1965," p 1 in RTBT 5/2/5/2 Folder 1.
[811] These projects and assignments for who was to lead them are outlined in "Summary of Assignments for Members of Special Study Group on BW-Control (Geneva, 31 January 1965)," in RTBT 5/2/5/2, Folder 1.

introduce the new blood of the political and military-strategical serotypes."[812] They spent much of 1965 and 1966 sharply disagreeing about how to pursue such contacts, with Hedén trying to incorporate defense officials from his native Sweden into the community, while Rotblat and Kaplan tried to cultivate ties with British officials while keeping them at arms' length.[813]

Swedish ties proved particularly fruitful for the group after 1966, when parts of their projects were absorbed by the nascent Stockholm International Peace Research Institute (SIPRI).[814] Inspired by Sweden's 150th year of peace in 1964, this institute was principally the brainchild of prominent Swedish diplomat Alva Myrdal, who seized on Prime Minister Tage Erlander's vague proposal for a government-funded peace research institute to commemorate the occasion. Myrdal herself was a member of Pugwash, and the institution she helped organize followed the Pugwash style of technocratic, 'insider'-oriented peace advocacy, rather than the 'outsider' focus on structural psychological and socioeconomic causes of conflict embodied in Johan Galtung's contemporaneous Peace Research Institute, Oslo.[815] Like Pugwash conference reports, the reports of the Swedish International Peace Research Institute (soon renamed to emphasize the institute's

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812 Hedén to Kaplan, July 9, 1965 in RTBT 5/2/5/3 Folder 2.

813 See e.g. Meselson to Rotblat, July 9, 1965; Meselson to Rotblat, October 12, 1965; Hedén to Málek, September 6, 1965, all in RTBT 5/2/5/3 Folder 3; Martin Kaplan to David W. W. Henderson, January 21, 1965 in RTBT 5/2/17/28. Rotblat and Kaplan's British efforts were imperiled by what they saw as the meddling of Matthew Meselson, leading Rotblat to clash with him. See Rotblat to Meselson, October 28, 1965, in RTBT 5/4/12/11 Folder 2.

814 See *SIPRI, Continuity and Change, 1966-1996*, Stockholm: Stockholm International Peace Research Institute, 1996.

815 On the Peace Research Institute, Oslo see Johan Galtung, "Twenty-Five Years of Peace Research: Ten Challenges and Some Responses," *Journal of Peace Research* 22 no 2 (1985), pp 141-158. The theoretical orientation of the Oslo institute can be seen in the first decade of Galtung's *Journal of Peace Research*, with studies of the nature of "interests," the mentality of arms control negotiators, and Galtung's concept of "structural violence." See Halvard Buhaug, Jack S Levy and Henrik Urdal, "50 Years of Peace Research: An Introduction to the 'Journal of Peace Research' Anniversary Special Issue," *Journal of Peace Research* 51 no 2 (2014), pp 139-144.

purported independence from the Swedish government which funded it) would be oriented toward an audience of arms control professionals and diplomats, especially those from smaller nations without the epistemic infrastructure of the superpowers to draw upon. Embodying these similarities, Rotblat, Ivan Málek and Rolf Björnerstedt of the Swedish Pugwash chapter were appointed members of SIPRI's governing board, and another Pugwash member, British economist Robert Neild, was appointed as the institute's first director.[816]

Given these intimate connections, it is no surprise that SIPRI adopted the Pugwash literature review study as one of its first projects in 1967. Hedén had already published an initial 1967 review article on the military potential of biological weapons and potential defenses against them, drawing on the Pugwash network to solicit bibliographies from European, American, and Soviet scientists.[817] He now continued his work as a SIPRI consultant, aided by a number of scholars hired by SIPRI from both sides of the Iron Curtain, most notably British chemist Julian Perry Robinson, and Czech microbiologist Theodor Němec, an associate of Málek's.[818] This study attempted to paint a picture of the probable military potential and epidemiological impact of a wide gamut of potential biological warfare agents, and the probable research and development being conducted in various nations, solving the problem of unknown classified information by

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[816] D. S. Greenberg, "Peace Research: SIPRI, in Sweden, Is Making a Role for Itself," *Science* 162 no 3861 (December 27, 1968), pp 1465-1466. Rotblat discusses the Pugwash role in organizing SIPRI and the prevalence of Pugwash members on the governing board in Joseph Rotblat to Philip J. Noel-Baker, May 7, 1968 in RTBT 5/4/4/18.

[817] Carl-Göran Hedén, "Defenses Against Biological Warfare," *Annual Review of Microbiology* 21 (1967), pp 639-676.

[818] Joseph Rotblat to H. Alfven, July 7, 1965 in RTBT 5/2/5/3 Folder 4; "Report of the meeting of the Pugwash Study Group on Biological warfare held in Stockholm, September 4-6, 1966," in RTBT 5/2/5/4 Folder 1.

systematically drawing on as many open sources as possible. The SIPRI project also reversed the Pugwash decision to separate chemical and biological warfare. The 6-volume study that they produced was published between 1971 and 1975, during which time the Biological Weapons Convention was negotiated with a sole focus on biological warfare.[819] However, the study was not entirely quixotic, as in 1969 SIPRI had contributed drafts and researchers to the production of influential reports on biological warfare sponsored by the UN and the World Health Organization (see below).

SIPRI also collaborated with the Pugwash group on their long-delayed inspection project. This project had been most immediately inspired by preexisting Western European Union inspections of West German chemical plants, to verify that they were not producing chemical weapons.[820] Like this inspection regime, the Pugwash project was European in its focus, explicitly excluding American and Soviet microbiological facilities, but within the European continent, it deliberately transcended Cold War boundaries. Members of the Pugwash Study Group, led especially by Hedén and Málek, drew upon a network of personal contacts in European industrial microbiology to organize model site visits in NATO, Warsaw Pact, and non-aligned European countries, and with SIPRI funding of $11,000, the inspection group visited 14 laboratories and industrial microbiological plants in late 1968 and early 1969.[821] This experiment left the

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[819] Stockholm International Peace Research Institute, *The Problem of Chemical and Biological Warfare*, 6 vols, New York: Humanities Press, 1971-1975.
[820] Robinson, “The Impact of Pugwash on the Debates over Chemical and Biological Weapons,” p 234; C-G Hedén to Agence pour le Contrôle des Armements, Union de l'Europe Occidentale, April 22, 1966 in RTBT 5/2/5/4 Folder 2. For more general background on the relationship between the WEU and West German arms, see Michael H. Creswell and Dieter H. Kollmer, “Power, Preferences, or Ideas?: Explaining West Germany’s Armaments Strategy, 1955-1972,” *Journal of Cold War Studies* 15 no 4 (2013), pp 55-103.
[821] T. Němec, “SIPRI CBW-Study,” in RTBT 5/2/5/6 Folder 1.

SIPRI group convinced of the efficacy of on-site inspection, as the extra safety measures required for growing pathogens (like waste air sterilization, positive-pressure containment rooms, and worker immunization) would be difficult to disguise from a skilled inspection team.[822] True to Pugwash's technocratic vision of arms control, the group also proposed hypothetical 'black boxes' to match or supersede human inspections. These 'black boxes' were to be tamper-proof data recorders, which would allow verification of that facility's activities without the expense or intrusion of being monitored in person, and without otherwise spying on confidential procedures (like trade secrets). Automatic air samplers, devices to record the changing heat signatures of bioreactors, or even site visits by experienced and keen-eyed experts could help instill confidence in a negotiated ban on biological weapons, the group opined.[823]

A similar technocratic logic was behind the Pugwash group's attempts to organize a conference on the rapid detection of airborne microorganisms. Much like the growing consensus against ballistic missile defense growing in the main meetings, the Study Group's logic was based on a structuralist technological vision of world politics.

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[822] They were likely aided in this conclusion by Hedén's own experience growing pathogenic bacteria in bulk since the late 1950s, albeit for medical research rather than military purposes. On Hedén's project to grow "kilogramme quantities of pathogenic microbes for study in many European research institutes," see G. Hamer, "Carl-Göran Hedén," *MIRCEN Journal of Applied Microbiology and Biotechnology* 2 no 1 (1986), p 3. This paper was a tribute written by Geoffrey Hamer, a colleague of Hedén's at the Karolinska Institute in the 1960s, upon Hedén's retirement. This fermenter (though not the delivery of pathogens to other European research institutes) is also discussed in Nigel Calder, "Sweden: New Institute to Focus on Applied Microbiology," *Science* 160 no 3823 (April 5, 1968), pp 54-56. The lines between medical and military work were blurry, however, as Hedén and his "Bioengineering Unit" turned this fermenter work to open-source work under the auspices of the Swedish government-sponsored Medical Research Council after 1960. See Calder, Ibid, p 54. Examples of Hedén's open-source publication of his pathogen-growing work include C.-G. Hedén and B. Malmgren, "Equipment for Cultivation of Microorganisms," *Industrial and Engineering Chemistry* 46 no 9 (September 1954), pp 1747-1751; C.-G. Hedén, "Pulsating Aeration of Microbial Cultures," *Nature* 179 (February 9, 1957), pp 324-325; H. Billaudelle, C.-G. Hedén, B. Malmgren, "Problems in large-scale culture of *H. pertussis*," *Journal of Biochemical and Microbiological Technology and Engineering* 1 no 2 (1959), pp 173-184. This last study, on growing the whooping cough bacterium, was sponsored by the US Army.

[823] C.-G. Hedén, "Frequency of Inspections," January 12, 1969 in RTBT 5/2/5/6 Folder 2.

Biological attacks could be defended against with airtight respirators or shelters if their targets were forewarned, but any such timely detection was nigh-impossible with conventional laboratory diagnostic techniques. Therefore, biological weapons favored surprise, dangerously inclining a military which possessed them to use them offensively, the logic went. Much like the hypothetical 'black boxes' of the nuclear test ban debate, however, the Pugwash group thought that the solution to this security problem was technological: the development of automatic devices to rapidly detect and warn against biological attack. Such devices would strengthen defenses against biological attack, and if their feasibility could be publicly demonstrated, the group reasoned, biological weapons would look less militarily attractive and an international ban on them more feasible for the countries that possessed them. Some members, led by Matthew Meselson, even hoped that such technologies could be used to detect clouds from open-air biological tests from hundreds or thousands of miles away, which could provide further technical means to underlie a BW ban.[824] (However, this was far from a unanimous viewpoint, and Meselson clashed with Rotblat in trying to impose it on the rest of the group).[825]

The problem, once again, was that of the world's militaries "holding the ace" of classified knowledge. The Pugwash group presumed with very little discussion that major bioweapons programs like those of the US, UK, and USSR were actively researching such technology, and that the cutting edge of scientific knowledge underlying such

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[824] See Kaplan to Rotblat, December 7, 1966, p 1, in RTBT 5/4/1/12.

[825] Meselson to Rotblat, June 1, 1967 and Rotblat to Meselson, June 8, 1967, in RTBT 5/3/1/20 Folder 5; Meselson to Rotblat, August 21, 1967, in RTBT 5/4/4/16.

devices was part of those countries' classified worlds.[826] How, then, could the Pugwash group meaningfully opine when their open-source information was presumably out-of-date? They adopted two tacit strategies. The first was to focus on possibilities rather than the precise details of how a rapid detection device would work. This enabled the Study Group to conclude in a 1966 meeting that new techniques like immunofluorescence were a promising basis for rapid detection devices with further research.[827] The fact that this additional research had not been published in the open literature was not a problem for this claim, because such research was presumed to be classified. Essentially, this rhetoric of possibility coopted the "ace" of secrecy, allowing the Pugwash group to buttress the credibility of their claims with a presumed body of classified military knowledge. This use of *uncertainty* as a basis for scientific authority is strikingly similar to that of British biological weapons researchers (described by Brian Balmer), who used the unprovable presumption that secret Soviet bioweapons research was proceeding at a strong pace to buttress their arguments for continued support of their own program. As Balmer notes, this is the reverse of the typically studied rhetoric of science, in which scientific authority, both epistemic and funding-justifying, derives from expressions of certainty.[828] For both British BW researchers, and the Pugwash group, a secrecy-produced lacuna in knowledge was used to buttress their programs *as if* that lacuna were filled by the maximum progress imaginable on the part of the secretive group. Ironically, just as in

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[826] The group, for instance, "doubt[ed]" whether "good scientists could be obtained to do [a laboratory research] study, because it may involve a duplication of work already carried out in secret defense work in some of the large countries." See "Noted on Informal Discussion in Geneva, 9 May 1965, On Pugwash BW Study Group Reports," May 25, 1965, pp 1-2 in RTBT 5/2/5/2 Folder 1.

[827] "Report of the meeting of the Pugwash Study Group on Biological warfare held in Stockholm, September 4-6, 1966," pp 3-4 in RTBT 5/2/5/4 Folder 1.

[828] This is the major topic of Chapter 5 of Brian Balmer, *Secrecy and Science: A Historical Sociology of Biological and Chemical Warfare*, New York: Ashgate Publishing, 2012, pp 73-87.

British fears of Soviet capabilities, the Pugwash group's estimation of military detection work was significantly overblown.[829] The American Chemical Corps, for example, considered the detection of airborne biological agents to be an intractable problem in the 1960s, with only cursory work devoted to it in response to pressure from high-level policymakers concerned with strengthening CBW defense.[830] It was not until the late 1970s that the Chemical Corps fielded an experimental biological agent detector, which took so long to analyze a sample, had such high power requirements, and had such a high false-positive rate that the device was canceled in the early 1980s.[831]

Not knowing this, the Pugwash group's second strategy was to probe the margins of secret knowledge by enlisting bioweapons-connected scientists to share their unclassified knowledge in a symposium. In 1965, Martin Kaplan and British biologist John Humphrey drew on pre-existing personal contacts with D. W. Henderson, scientific director of the British research establishment at Porton Down, to try to secure the cooperation of Porton researchers, while the rest of the group discussed enlisting former Detrick researcher LeRoy Fothergill.[832] His help was not forthcoming, however, and the

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829 For a discussion of the anemic Soviet biological weapons program of the first two decades of the Cold War, see Milton Leitenberg and Raymond A. Zilinskas, *The Soviet Biological Weapons Program: A History*, Cambridge, MA: Harvard University Press, 2012, pp 34-50.

830 Albert J. Mauroni, *America's Struggle with Chemical-Biological Warfare*, Westport, CT: Praeger, 2000, pp 99-100. Mauroni's book is a brief and partisan "insider's" history of the Army Chemical Corps (Mauroni served 14 years in the Chemical Corps before becoming a civilian Pentagon CBW consultant), focusing on its institutional attempts to rebuild itself after the budgetary cuts of the early 1970s. It includes brief topical discussions of equipment R&D before and during this period, including detection equipment.

831 Ibid, p 101. Mauroni also describes smaller but still extant technical challenges and lack of funding emphasis for automated chemical weapons detection in the 1950s and '60s. Further evidence for the Army's failure to develop a biological aerosol detector lies in the fact that basic field testing procedures for such a device were not articulated at the Dugway Proving Ground until 1968 (probably in response to the high-level pressure for such a device described by Mauroni). See U.S. Army Test and Evaluation Command, "Commodity Engineering Test Procedure: Alarms, Biological," Dugway Proving Ground, January 31, 1968. Unclassified; retrieved from DTIC 9/7/2018.

832 See Kaplan to Henderson, January 21, 1965, in RTBT 5/2/17/28, and Kaplan to Rotblat, March 12, 1965, in RTBT 5/4/4/13. Humphrey continued these personal contacts with Porton leadership after Henderson's retirement. See J. H. Humphrey to Rotblat, January 13. 1971, in RTBT 5/4/1/9. Fothergill was

Porton overtures received a lukewarm reception, leaving the question of a symposium dormant for several years. It was not until the end of the decade, as both institutions became increasingly politically insecure, that the bioweapons community began to treat the Pugwash group more seriously with Kaplan, for instance, traveling to Detrick to meet with its scientific director Riley Housewright in September 1969.[833] With the Nixon administration's renunciation of biological weapons research a few months later (see below), Detrick's position became still less secure, and Kaplan continued to push Housewright to contribute to a symposium, perhaps even to be held at Detrick itself.[834] Housewright was initially open to these overtures, likely attracted by the possibility of demonstrating Detrick's participation in important biomedical research when its future was in jeopardy.[835] As it was, however, the administrative chaos in which the facility found itself precluded a meeting there, and even Detrick researchers invited to the symposium when it was eventually held in Switzerland in 1971 failed to receive administrative clearance to attend.[836] Director Gordon-Smith of Porton likewise demurred, questioning the planning of the symposium, though "not unsympathetic" to the

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listed among potential future members of the Study Group after the first meeting. See "Summary of Assignments for Members of Special Study Group on BW-Control (Geneva, 31 January, 1965)," p. 4 in RTBT 5/2/5/2 Folder 1.

[833] Kaplan also met with Benjamin Warshowsky, head of Detrick's Detection Branch, and recruited him to join a Pugwash meeting on airborne detection pending clearance. This clearance was not forthcoming when the meeting did take place, but Warshowsky nonetheless remained engaged with the Pugwash group in seeking it (see below). See Kaplan to Bennett, December 15, 1970, in RTBT 5/4/1/12.

[834] Ivan Bennett was much less enthusiastic about this idea. See Ivan Bennett to Bernard T. Feld, May 11 and 25, 1970, both in RTBT 5/4/12/19 Folder 1. Rotblat, however, was receptive to the idea of holding the meeting at Detrick, and solicited Soviet views through Continuing Committee member Academician Millionshchikov [sic]. See Rotblat to Kaplan, January 9, 1970, in RTBT 5/4/1/12. Despite his objections, Bennett subsequently aided in unsuccessful efforts to recruit Fort Detrick researcher Benjamin Warshowsky. See Kaplan to Rotblat, December 29, 1970, in RTBT 5/4/1/12.

[835] Kaplan to Rotblat, January 5, 1970. Housewright's reply is not in the archives, but was evidently positive, given the tone of Kaplan to Housewright, July 14, 1970, both in RTBT 5/4/1/12.

[836] Benjamin Warshowsky, head of Detrick's Detection Branch was the principal researcher invited. See Warshowsky to Rotblat, November 16, 1970, in RTBT 5/4/1/24. Warshowsky's clearance to attend apparently remained an open issue until the last minute. See Warshowsky to Kaplan, December 17, 1970 and Rotblat to Kaplan, January 13, 1971, in RTBT 5/4/1/12.

group's basic aims, and a subsequent request from Kaplan for Solly Zuckerman to lobby for attendees from Porton Down (pointing to the probable attendance of defense researchers from a number of countries, including the US and USSR, as compelling reasons for a British presence) ultimately failed.[837] The 1971 symposium which did eventually take place in Switzerland had thus failed at its most fundamental goal of directly enlisting bioweapons expertise in the Pugwash "black box" vision, but this did not particularly deter the group's confident proceedings. Like the 1966 meeting, the 1971 symposium reached a favorable consensus about the feasibility of rapid detection technology, but as its participants noted, this conclusion had already been largely superseded by events.[838] The American renunciation of its offensive BW program and the active Biological Weapons Convention negotiations then ongoing seemed even to Kaplan to render the symposium's findings largely academic, though he emphasized their importance should technical verification emerge as a concern in BWC negotiations.[839] This did not occur, however, and much like Pugwash's earlier attempts to buttress the credibility of "black box" seismographs in the nuclear test ban debate, political compromise and tacit trust rather than technical verification served as the basis of the BWC signed in 1972.

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[837] Gordon-Smith to Kaplan, May 13, 1970, in RTBT 5/4/12/19 Folder 1; Kaplan to Zuckerman, December 3, 1970, in RTBT 5/4/1/12.
[838] See "12th Pugwash Symposium on 'Rapid Detection and Identification of Microbiological Agents," in RTBT 5/2/2/12 Folder 12.
[839] "Rapid Detection and Identification of Microbiological Agents, Report from 12th Pugwash Symposium;" Joseph Rotblat to William Epstein, April 28, 1971, both in RTBT 5/4/10/9.

Theodor Rosebury's Ethical Protest

Theodor Rosebury, too, was busy in the 1960s. The 1959 Pugwash meeting was a return to anti-biological warfare activism for him, after a hiatus through most of the 1950s. At the height of the McCarthy years he had even declined invitations to speak for chapters of the World Federation of Scientific Workers, of which he was under normal circumstances an enthusiastic member.[840] By the end of the 1950s, however, with the political climate thawing and with a new series of "Pugwash" conferences growing up under the partial tutelage of friendly World Federation leaders like Eric Burhop, he evidently felt more secure, and he promptly accepted Rotblat's invitation to the 1959 meeting. The meeting itself was a mixed bag for him, enabling him to meet most of the other attendees for the first time, but confronting him with a group in which "as [he] feared… nobody… knows their stuff." He was particularly unimpressed by Martin Kaplan, the tacit leader of the group, whose paper was nothing more than "a bowdlerized Rosebury-Kabat Report as of 1949," which furthermore was "a good try but amateurish, & he hadn't read us enough." Regarding himself (with some justice) to be the sole expert in a room full of such amateurs, he "did a great deal of talking, some of it critical" throughout the meeting.[841] Though he felt that this criticism shaped the discussion and subsequent meeting report positively, he did not come away from the meeting with much respect for the Pugwash group. What he did come away with, however, was a renewed interest in anti-bioweapons activism, just as sympathetic editors like the *Bulletin of*

[840] W. Terwiel to Theodor Rosebury, February 27, 1954 and Theodor Rosebury to W. Terwiel, March 16, 1954, both in NLM Rosebury Papers, Box 5 Folder 2 (Correspondence 'T' 2 of 2).
[841] Theodor Rosebury, Untitled ms journal notes on "Pugwash Conference of International Scientists on Biological and Chemical Warfare" letterhead, August 23-25, 1959, in NLM Rosebury Papers, Box 7 Folder 23 (Pugwash 5: a proof copy of the paper "Biological Chemical Warfare-An International Symposium").

Atomic Scientists' Eugene Rabinowitch were rediscovering their own interest in the topic.[842]

The result, for Rosebury, was a renaissance of his 1940s-era "missionary work." Besides publishing a paper critical of Fort Detrick's deep ties to the broader microbiological community in *Perspectives in Biology and Medicine*, he began accepting speaking engagements on biological warfare again; even suffering a serious automobile accident on the way to one in 1960.[843] Unlike the Pugwash model he had experienced, in which elite scientists sonorously discussed world affairs behind explicitly closed doors, Rosebury spoke widely to groups critical of the arms race ranging from the Congress of Scientists on Survival to Unitarian congregations.[844] He presented himself in such talks as more of an informational resource than a leader, reflecting the reality that protests against bioweapons research in the early 1960s were spearheaded by non-scientist groups like the American Friends Service Committee, who led a year-long 'Vigil at Fort Detrick' between 1959 and 1960.[845] It also reflected his own growing pessimism about the role of science in society. His own 'missionary work' and publications remained the best public source on biological warfare, but none of his educational efforts had apparently borne any fruit, as the seeming juggernaut of a technologically driven arms race barreled on. "I am

[842] Rosebury's Pugwash paper was published in a special section of the *BAS* devoted to the 1959 meeting. See Theodor Rosebury, "Some Historical Considerations," *Bulletin of the Atomic Scientists* 16 no 6 (1960), pp 227-236.
[843] Theodore Rosebury, "Medical Ethics and Biological Warfare," *Perspectives in Biology and Medicine* 6 no 4 (1963), pp 512-532. Rosebury discusses the accident in Theodor Rosebury to Cyrus Eaton, November 18, 1960 in NLM Rosebury Papers, Box 5 Folder 23 (Eaton-Royon: 9th Pugwash conference).
[844] Christian Andreasen to Theodor Rosebury, October 3, 1960 in NLM Rosebury Papers Box 2 Folder 3 (Correspondence 'A' 3 of 3); Harry H. Lerner to Theodor Rosebury, May 8, 1962 in NLM Rosebury Papers Box 3 Folder 12 (Correspondence 'L' 2 of 2).
[845] Papers relating to the Vigil are held in the Swarthmore College Archives Peace Collection (Identifier: SCPC-CDG-A-Appeal and Vigil at Fort Detrick), along with those of several leaders in the movement. See also Moore, *Disrupting Science*, pp 83-84.

not convinced that Progress is inevitable, or that we shall all be happy when fully automated," he declared in 1964, reflecting the contemporary technological pessimism of a Jacques Ellul or Herbert Marcuse.[846]

Rosebury's technological pessimism was deepening just as the Pugwash movement, which had originally been a politically and philosophically heterogeneous group partially led by his WFSW correspondents, was being shaped by Rotblat into a self-consciously politically moderate organization inclined toward advocating technological fixes to political problems. It is therefore unsurprising that little love was lost between him and the new Pugwash's Study Group on Biological Warfare. I have found no indication in Pugwash records that Rosebury was even considered for membership in the group, which while dominated by Europeans did include Americans like Matthew Meselson on its roster. It is doubtful that Rosebury would have been interested in joining in any event. "The problem [of biological warfare]," he argued in a letter to Rotblat critiquing the Study Group, "is political, not scientific," and "an impression of concerted 'scientific' activity… will necessarily make all the more difficult any genuine effort toward a solution of the problem."[847] He attended his final general Pugwash meeting in 1967 to present a jeremiad paper arguing that without an ethical and political shift in world politics, any form of disarmament, biological or otherwise, was futile.[848] Even this trip was consciously quixotic, undertaken as much to take a side-trip

[846] Theodor Rosebury to James E. Aiguier, February 10, 1964 in NLM Rosebury Papers, Box 2 Folder 3 (Correspondence 'A' 3 of 3). For a general overview of this 1960s technological pessimism, see Chapter 13 of Eric Schatzberg, *Technology: Critical History of a Concept*, Chicago: University of Chicago Press, 2018.
[847] Theodor Rosebury to Joseph Rotblat, October 22, 1966, in RTBT 5/4/1/19, Folder 2.
[848] Rosebury had previously attended the 10th Pugwash meeting in 1962, without making much of an impression. For the 1967 paper see T. Rosebury, "Technology and Biological Disarmament," in *Proceedings of the Seventeenth Pugwash Conference on Science and World Affairs, Ronneby, Sweden,*

to visit fellow WFSW member Ivan Málek in Prague as it was to argue with the Pugwash group.[849] As one of Rosebury's correspondents summarized their mutual sentiment, the Pugwash "scientists [will] continue to look for some gimmick… which will supposedly lead to disarmament instead of facing up to the causes of international conflict."[850] "You are of course right… that I am unlikely to convince the scientists," Rosebury agreed, "but… we do what we can, and I am strategically placed to do a critical job."[851] At the very least, he would represent "a U.S. voice guaranteed to be independent of the CIA."[852] He did not wash his hands of the organization completely (remaining an occasional correspondent with Rotblat into the mid-1970s), but emblematic of his impatience with the direction that Pugwash had taken under Rotblat, he remained a far more enthusiastic correspondent with Cyrus Eaton.[853]

With the escalation of the Vietnam war in the late 1960s, Rosebury found himself and his model of public engagement more "strategically placed" than ever. Growing opposition to the war itself and to the Cold War system in general provided him with

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September 3-8, 1967. Reflecting Rosebury's pessimism about technological solutions, this paper focused far more on examining the failed campaign for international control of atomic energy in 1946 than on anything to do with biological warfare in particular.

849 Theodor Rosebury to Ivan Málek, July 30, 1967 in NLM Rosebury Papers, Box 4 Folder 3 (Correspondence 'M' 5 of 6).

850 Ira A. Kipnis to Theodor Rosebury, July 3, 1967 in NLM Rosebury Papers, Box 3 Folder 10 (Correspondence 'K' 3 of 3).

851 Theodor Rosebury to Ira A. Kipnis, July 16, 1967 in NLM Rosebury Papers, Box 3 Folder 10 (Correspondence 'K' 3 of 3).

852 Theodor Rosebury to Fred Kraus, March 15, 1967 in NLM Rosebury Papers, Box 3 Folder 10 (Correspondence 'K' 3 of 3). The apparent perception within Rosebury's leftist circles that American attendees of the Pugwash conferences in the mid-1960s often had ties to state institutions was prescient. See Wolfe, *Freedom's Laboratory*, pp 123-134, 170.

853 Excising Eaton and his influence had been a major part of Rotblat's consolidation of power over the organization, particularly in the wake of Eaton's own attempt to influence the deliberations of 'his' scientists at the 1959 conference. See Footnote 798, above. Examples of the Rosebury-Eaton correspondence can be found in NLM Rosebury Papers, Box 5 Folders 21-24. Examples of Rosebury's continued interest in Pugwash into his later years include Rosebury to Rotblat, August 14, 1974, and Rosebury to Rotblat, November 22, 1971, both in RTBT 5/4/1/19, Folder 2.

increasingly receptive lay audiences to warn of the dangers of biological warfare. Reflecting this generalized antimilitarism, he commonly spoke to groups whose leaders expressed concern about the war, anti-ballistic missile systems and CBW (chemical and biological warfare) in the same breath.[854] CBW represented more than just another transgression of the military-industrial complex in the Vietnam era, however. The use of tear gas in combat and the mass spraying of herbicides by American forces fighting the war represented arguable instances of such warfare, further increasing Rosebury's attractiveness as a guest speaker.[855] By the end of the decade, college campuses had become a particularly prevalent destination, with the rise of politically radical groups like the March 4th movement. This movement was exemplified by a 1969 day of radical speeches and teach-in events at MIT protesting everything from CBW to the war to the university's entanglement with military affairs, but similar events occurred on dozens of campuses across the country.[856] Rosebury spent March 4, 1969 at Case Western Reserve, speaking against biological warfare and sharing a stage with the likes of Benjamin

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[854] See e.g. postscript of Blaine Kennedy to Theodor Rosebury, February 21, 1968, in NLM Rosebury Papers, Box 3 Folder 10 (Correspondence 'K' 3 of 3). "ABM" had by the late 1960s become an object of public contention, with defense 'doves' opposing such defenses as destabilizing to the nuclear balance of terror and representative of the bellicose excesses of the military-industrial complex. This counterintuitive identification of a defensive technology as destabilizingly aggressive originated in the 1960s among moderate American defense intellectuals of the sort that Rotblat's Pugwash sought to cater to. Indeed, Pugwash served as an important venue for exchanging these ideas across the Iron Curtain. A commonly-repeated anecdote in Pugwash circles was that when ARPA engineer Jack Ruina first presented an argument against missile defense at the 1964 conference, his Soviet listeners presumed that his criticism of this defensive technology had been mistranslated. Drawing on political scientist Peter Haas' concept of epistemic communities, Evangelista argues that Pugwash's status as such a community helped establish common presumptions about ABM and national security, a crucial precondition for subsequent US-Soviet diplomatic negotiations to limit ABM technology. See Rebecca Slayton, *Arguments that Count: Physics, Computing, and Missile Defense, 1949-2012*, Cambridge, MA: MIT Press, 2013, pp 90-91; Peter M. Haas, "Introduction: Epistemic Communities and International Policy Coordination," *International Organization* 46 no 1 (1992), pp 1-35; Evangelista, *Unarmed Forces*, pp 123-140.

[855] Roger Eardley-Pryor, "Better to Cry than Die?: The Paradoxes of Tear Gas in the Vietnam Era," in James Rodger Fleming and Ann Johnson (eds), *Toxic Airs: Body, Place, Planet in Historical Perspective*, Pittsburgh: Pittsburgh University Press, 2014, pp 50-76.

[856] Bryce Nelson, "M.I.T.'s March 4: Scientists Discuss Renouncing Military Research," *Science* 163 no 3872 (March 14, 1969), pp 1175-1178; Moore, *Disrupting Science*, pp 137-146.

Spock.[857] Such public denunciations of biological warfare had reached a crescendo by the spring of 1969, with Matthew Meselson giving a similar talk at the MIT events on March 4.[858] Popular books and news documentaries raised public awareness of the American CBW program, with later Pulitzer Prize-winner Seymour Hersh drawing heavily on Rosebury's expertise to publish a leading title among such exposés in 1968.[859] Even Joshua Lederberg, by this time a Nobel laureate with a weekly *Washington Post* column entitled "Science and Man," had joined in this public criticism of biological warfare research.[860] Eschewing his earlier caution about "the military hold[ing] the ace," of secret knowledge, Lederberg now used a rhetoric of possibility to argue that unforeseen ecological consequences and the danger of accidents made biological warfare research too dangerous to be worth pursuing.[861] Just as Rosebury had hoped to do and Lederberg had despaired of doing in the 1940s, public engagement was drawing popular scrutiny of biological weapons research ever closer to Detrick; literally in the case of a late 1968 visit to Hood College in the neighboring town of Frederick, where Rosebury spoke to "a

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[857] Theodor Rosebury to Mrs. Bruce Kendrick, March 5, 1969 in NLM Rosebury Papers, Box 3 Folder 10 (Correspondence 'K' 3 of 3); Theodor Rosebury to Doris Bolef, March 5, 1969 in NLM Rosebury Papers, Box 4 Folder 3 (Correspondence 'M' 5 of 6).

[858] "Schedule of Events at M.I.T.," *The Harvard Crimson*, March 4, 1969, p 4.

[859] See Seymour Hersh, *Chemical and Biological Warfare: America's Hidden Arsenal*, Indianapolis: Bobbs-Merrill, 1968; Seymour Hersh to Theodor Rosebury, May 29, 1967; Rosebury to Hersh, June 5, 1967 and other letters in NLM Rosebury Papers, Box 3 Folder 6 (Correspondence 'H' 3 of 3).

[860] Despite Lederberg's expressions of sympathy toward the anti-BW activism of organizations like the FAS and Pugwash, he also had close links with Detrick contacts and had even briefly maintained a security clearance in the early 1950s. On Lederberg's use of Detrick expertise for his guidance of space probe sterilization in the early days of NASA, see Audra J. Wolfe, "Germs in Space: Joshua Lederberg, Exobiology, and the Public Imagination," *Isis* 93 no 2 (2002), pp 183-205. By the time he came out publicly against biological warfare in the mid-1960s, he had the security of a highly established scientific career to draw upon, but his willingness to burn previously fruitful bridges with Detrick can also be seen as a bellwether of attitudes in the Vietnam era. Open criticism of biological weapons research was ultimately more expedient by 1966-1968, with increased scrutiny and a souring public attitude toward chemical and biological warfare, than it would have been in 1958 or 1949.

[861] See e.g. Joshua Lederberg, "A Treaty Proposal on Germ Warfare," *Washington Post*, September 24, 1966; Joshua Lederberg, "Congress Should Examine Biological Warfare Tests," *Washington Post*, March 30, 1968; Joshua Lederberg, "Swift Biological Advance Can Be Bent to Genocide," *Washington Post*, August 17, 1968.

full house" of "about 350," reporters from NBC, *Science*, and various newspapers, and "old friends from Detrick," which "sent a delegation" to meet him.[862]

Elite Politics and the Nixon Announcement

By the end of the 1960s, this growing popular consciousness was joined by pressure from the kind of elite institutions that the Pugwash Study Group focused on influencing, most notably the United Nations and World Health Organization. The openly acknowledged existence of American and British CBW programs had been a useful weapon for Communist governments in Cold War rhetorical battles since the late 1940s, making both international bodies reluctant to criticize CBW too strongly, for fear of entangling themselves in these Cold War propaganda battles. For instance, despite strong personal concern about the dangers of biological warfare of Brock Chisholm, the first WHO director, the WHO was prevented from investigating Chinese and North Korean allegations that the US had used biological weapons in the Korean War, which led to an unofficial (and generally Communist-aligned) group of scientists under Joseph Needham investigating these allegations instead.[863] Likewise, the UN had quickly backed away from its collaboration with Rosebury when American officials made their displeasure felt

[862] Theodor Rosebury to Beatrice Rosenfeld, November 21, 1968 in NLM Rosebury Papers, Box 5 Folder 4 (Correspondence 'V' 1 of 3).
[863] On Needham, see Tom Buchanan, "The Courage of Galileo: Joseph Needham and the 'Germ Warfare' Allegations in the Korean War," *History* 86 no 284 (2001), pp 503-522. The Korean War allegations have remained a minor controversy in the historiography of biological warfare for decades, with a post-Cold War scholarly consensus that they were fabricated challenged by heterodox figures like sinologist Stephen Endicott. See e.g. Stephen Endicott and Edward Hagerman, *The United States and Biological Warfare: Secrets from the Early Cold War and Korea*, Bloomington: Indiana University Press, 1998; Milton Leitenberg, "Resolution of the Korean War Biological Warfare Allegations," *Critical Reviews in Microbiology* 24 no 3 (1998), pp 169-194; Milton Leitenberg, "A Chinese Admission of False Korean War Allegations of Biological Weapon Use by the United States," *Asian Perspective* 40 no 1 (2016), pp 131-146.

a few years earlier.[864] This reluctance to antagonize the Americans increased as their use of tear gas and herbicides in the Vietnam War made CBW a still more charged weapon in Cold War politics, as Hungary introduced a UN resolution condemning American chemical warfare in Vietnam soon after the first reports arrived in 1966.[865] This resolution was predictably defeated as part of the back-and-forth riposte of Cold War diplomacy, but continued reports of this warfare as the American military increased its use of riot control agents for combat combined with general growing sentiment against the war in Vietnam continued to raise the issue throughout the 1960s. By 1968, this issue came to a head in the United Nations with the UK even advancing a Working Paper on a biological weapons ban at the Eighteen-Nation Disarmament Conference.[866] In this climate, the UN General Assembly finally took the step (which had previously been too controversial) of commissioning a formal study of the potential of biological and chemical warfare.[867] The next year, the World Health Organization, too, commissioned a study on the health implications of these weapons.[868]

It was in this context of Cold War politics that both international institutions turned toward the *trans*national Pugwash-SIPRI network for expertise. Like the Needham

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[864] Unsigned copy of letter to "Otto" (presumably Otto Frey), July 20, 1950; Theodor Rosebury to Howard A. Meyerhuff, March 6, 1953, both in NLM Rosebury Papers, Box 4 Folder 7 (Correspondence 'O'). See Footnote 770, above.

[865] See SIPRI, *The Problem of Chemical and Biological Warfare Volume IV: CB Disarmament Negotiations, 1920-1970*, New York: Humanities Press, 1971, pp 234-242.

[866] For a detailed discussion of the British government deliberations behind this proposal, see Chapter 5 of John R. Walker, *Britain and Disarmament: The UK and Nuclear, Biological and Chemical Weapons Arms Control and Programmes 1956-1975,* Farnham, UK: Ashgate, 2012, pp 49-72. For a more general examination of the negotiations that subsequently led to the BWC, see Marie Isabelle Chevrier, "The Politics of Biological Disarmament," in Mark Wheelis, Lajos Rózsa, and Malcolm Dando (eds), *Deadly Cultures: Biological Weapons since 1945,* Cambridge, MA: Harvard University Press, 2006, pp 304-328.

[867] United Nations Group of Consultant Experts on Chemical and Bacteriological (Biological) Weapons, "Chemical and Bacteriological (Biological) Weapons and the Effects of Their Possible Use: Report of the Secretary General (UN Report A/7575/rev. 1)," New York: United Nations, 1969.

[868] "Health Aspects of Chemical and Biological Weapons: Report of a WHO Group of Consultants," Geneva: World Health Organization, 1970.

commission in the 1950s, the Pugwash group had no member governments to be beholden to, but unlike Needham's commission, also had the prestige of a prominent scientific membership and a carefully cultivated appearance of disinterested political neutrality in the Cold War context. The UN study fell under the auspices of Canadian lawyer and Director of the UN Disarmament division William Epstein, who outmaneuvered Swedish Pugwashite and SIPRI consultant-turned UN employee Rolf Björnerstedt for the role.[869] Nonetheless, through informal channels with Björnerstedt, Meselson, and Bernard Feld, Rotblat maintained a view of UN deliberations, and successfully offered the services of several members of the Study Group as prospective experts.[870] The subsequent UN panel included a number of members, including recent recruits like the American physician Ivan Bennett and Swedish defense researcher L. E. Tammelin.[871] The WHO panel was even more directly connected to the Pugwash Study Group, having been organized by Martin Kaplan, who was himself a WHO official in addition to his role in the Pugwash Study Group. Of 19 members, 8, including Málek, Meselson, and Kaplan himself were Study Group members, and the report they produced was, in Kaplan's words, the "culminat[ion]… [of] the Pugwash initiative begun in 1959."[872] Besides this direct recruitment, Kaplan followed Rotblat's example of informal influence-peddling among scientists and officials connected to the Pugwash network. Most notably, he shared pre-release drafts of both reports with Soviet epidemiologist and

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869 Rolf Björnerstedt to Joseph Rotblat, January 10, 1969 in RTBT 5/2/5/6 Folder 7.

870 See Feld to Rotblat, December 10 and 12, 1968 in RTBT 5/4/12/16 Folder 3, and 5/4/12/18 Folder 3. See also "Pugwash Submission to U.N. Special Committee on C.B.W. 1969," in RTBT 5/4/10/8. Epstein subsequently continued to draw upon Pugwash resources for other UN projects. See e.g. Epstein to Rotblat, December 22, 1970, in RTBT 5/4/12/18 Folder 3.

871 See list of consultants in "Chemical and Bacteriological (Biological) Weapons and the Effects of Their Possible Use," pp xiii-xiv.

872 List of consultants in "Health Aspects of Chemical and Biological Weapons," p 8. Quote from Martin Kaplan to Joseph Rotblat, December 5, 1969 in RTBT 5/3/1/28.

former Deputy Minister of Health Victor Zhdanov, and tried (unsuccessfully) to recruit him to act as a formal reviewer of these reports alongside Western scientists like Meselson and John Humphrey.[873] In effect, this was an attempt to secure Soviet support for the reports, as the Pugwash group had treated Zhdanov as an informal point of contact for the presumed-but-never-acknowledged Soviet biological weapons program since 1965.[874]

The UN report was published in the summer of 1969, to be followed the next year by the WHO report. Both reports argued that biological weapons were inherently unpredictable, both limiting their military utility and making them potentially dangerous to civilian populations, and advocated for an international convention banning biological warfare like that proposed by the British.[875] This focus on questioning the military utility of biological weapons (rather than their morality) was essentially a reflection of the consensus that had developed among the Pugwash group over the decade, to which attendees of the conferences (including the new Nixon administration's National Security Advisor Henry Kissinger) would have been exposed. Even Rosebury, who certainly did want to address morality, was impressed by the UN report when he acquired a copy,

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[873] See Kaplan to Zhdanov, March 27, 1969, in RTBT 5/4/1/12.

[874] For example, the second Study Group meeting assigned Hedén the task of "establish[ing] the <u>official USA (Housewright) and USSR (Zhdanov) policy with regard to the borderline between open and classified efforts in the BW area</u>." "Summary of Assignments for Members of Special Study Group on BW-Control (Geneva, 31 January 1965)," p 2 in RTBT 5/2/5/2, Folder 1 (emphasis in original). Housewright was well-known to the group as the scientific director of the American Fort Detrick; their equation of him with Zhdanov is telling. This presumption that Zhdanov (who was best known abroad as an organizer of the WHO's smallpox eradication project) has connections to military research was correct. See Leitenberg and Zilinskas, *The Soviet Biological Weapons Program*, pp 68, 155-156.

[875] "Chemical and Bacteriological (Biological) Weapons and the Effects of Their Possible Use," pp 87-88; "Health Aspects of Chemical and Biological Weapons," pp 19-21.

calling it “a magnificent piece of work” which “may just achieve its objective and get us some real CBW disarmament.”[876]

Rosebury was more right than he knew at the time, as Richard Nixon would publicly renounce offensive biological weapons research within a few months. Declassified documents surrounding this decision have been studied in the past two decades by historians Johnathan Tucker and David Goldman, both of whom have argued that the unprecedented decision to renounce an entire category of weapon was a result of military interests being out-maneuvered by civilian policymakers behind the scenes.[877] Months before the UN report was released, the political embarrassment presented by its very existence (alongside growing popular protest) had impelled the incoming administration to undertake the first high-level review of American CBW policy in years. Led by Kissinger, a National Security Council Political-Military Group was organized in May, with representatives from the State and Defense Departments, the CIA, and the Arms Control and Disarmament Agency (ACDA), and delivered its final report to Nixon’s desk by September. Tucker and Goldman have described these deliberations as a case of bureaucratic outmaneuvering, with Kissinger guiding the committee toward a position essentially like that of the Pugwash Study Group: that biological weapons lacked much military utility, that even if they did have mass destructive potential this would present a proliferation problem with smaller powers, and that the United States, in addition to lacking an interest in developing the ‘poor man’s Bomb,’ could deter biological attack with its nuclear arsenal rather than retaliation in kind. To buttress this

[876] Theodor Rosebury to Ivan L. Bennett, August 12, 1969, in NLM Rosebury Papers, Box 2 Folder 7 (Correspondence ‘B’ 4 of 5).
[877] See Tucker, “A Farewell to Germs;” Goldman, “The Generals and the Germs.”

reasoning, Kissinger commissioned a panel drawn from PSAC (a body which had already issued a generally-ignored report critical of CBW policy in 1966), which strongly concurred with these views.[878] These deliberations largely cut out military officers in favor of civilian officials, and when Army leaders became seriously involved in the process, they swiftly retreated from advocating a CBW expansion to offering the BW program as a sacrificial lamb to preserve chemical weapons capability (seen as a deterrent against Soviet chemical warfare in Europe, and a quiet affirmation of Vietnam riot control policy).[879] It was the NSC group's recommendation, that the US renounce its offensive BW program without reference to its chemical arsenal, that Nixon adopted without much comment and announced on November 25, 1969.[880]

The Pugwash community was of course not explicitly involved in this decision-making process the way they were with the UN and WHO reports, but several indirect influences on American decision-making are apparent. Most obviously, Kissinger himself had been a Pugwash member before joining the administration, having attended a number of conferences through the 1960s as one of the new generation of politically connected social scientists who joined in those years.[881] While he had not participated in the BW Study Group, he had comparable experience on its sibling Study Group on European Security, and had been exposed to the BW group's ideas and members at general Pugwash conferences. This is of course not to say that Kissinger was doing anything

[878] On PSAC and CBW under Johnson, see Wang, *In Sputnik's Shadow*, pp 261-264.
[879] Because of the delay between infection and illness in a biological attack, NSC group dismissed their battlefield utility and focused on BW as a class of *strategic* weapons, deterrable by the strategic nuclear arsenal. Conversely, the Army argued convincingly to the committee that chemical weapons could offer a compelling *tactical* advantage in an otherwise "limited" war with the Warsaw Pact, implying that a deterrent in kind was needed. Goldman, "The Generals and the Germs," p 560-561.
[880] Tucker, "A Farewell to Germs," pp 126-130.
[881] Evangelista, *Unarmed Forces*, p 147.

more than using Pugwash as a site to curry contacts and influence (as he did while still a Harvard professor in 1967, opening contacts with North Vietnam through the Pugwash network), but it should be noted that Kissinger was precisely the sort of policy-connected figure the BW Study Group sought to court with its national interest-based arguments.[882] While Kissinger formally disassociated himself from the organization after gaining political power, he also continued to invoke informal international contacts, like those he developed in the Pugwash network, to buttress his position in NSC deliberations, citing a conversation with a "Czech chemist," for instance, as evidence that small powers like Czechoslovakia were pursuing CBW research.[883] The Pugwash network likewise maintained "quite good contacts" with the Arms Control and Disarmament Agency through ACDA consultants like Meselson and Continuing Committee member Frank Long.[884] Finally, the PSAC report featured direct Pugwash connections, with the commission that wrote it having been organized and led by New York University professor Ivan Bennett, a physician who had been recruited to Pugwash Study Group by Meselson earlier in the year.[885] Bennett was joined on this panel by Meselson, Doty, and Richard Garwin, all members of Pugwash, and the report they fashioned for Kissinger

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[882] Indeed, Hedén had explicitly argued for bringing Kissinger (along with the likes of Herman Kahn and retired Chemical Corps General Rothschild) into the 1965 Study Group meeting in Stockholm. See C.-G. Hedén, "Tentative Plan for Stockholm Meeting (Wenner-Gren Center, April 1965)," in RTBT 5/2/5/3 Folder 2. On Kissinger's 1967 "Pennsylvania Channel" through Pugwash, see Jonathan Colman, *The Foreign Policy of Lyndon B. Johnson: The United States and the World, 1963-1969*, Edinburgh: Edinburgh University Press, 2010, pp 59-60.

[883] It is not clear who this chemist was, or if this conversation took place in connection to Pugwash, but it is noteworthy that participation in the Pugwash meetings (including one in Czechoslovakia) would have put Kissinger in personal contact with a number of Czechoslovak scientists. See Document 97, "Minutes of Review Group Meeting, October 30, 1969," in M. Todd Bennett (ed), *Foreign Relations of the United States, Vol XXXIV, National Security Policy, 1969-1972*, Washington, DC: Office of the Historian, 2011, pp 322-323. On Kissinger leaving Pugwash, see Evangelista, *Unarmed Forces*, p 147.

[884] The names of 17 PSAC members appear on the list of pre-1972 Pugwash meeting attendees in Rotblat, *Scientists in the Quest for Peace*. On Pugwash's "quite good contacts" with the ACDA, see Rotblat to Carl-Göran Hedén, June 28, 1968, in RTBT 5/4/1/9 Folder 1.

[885] Meselson to Rotblat, February 27, 1969, in RTBT 5/2/17/31.

apparently substantively agreed with the NSC group's logic (the report itself has not yet been declassified).[886] Martin Kaplan, at least, believed that his compatriots' network had made its influence felt. "I am convinced," he wrote to Rotblat, "that without the Pugwash effort we would not have arrived at the great, although still incomplete, victory of Nixon's declaration last week."[887]

Conclusion

By the beginning of the 1970s, scientific opponents of biological weapons research had made immense strides toward achieving their goals. The Americans and British had formally renounced their offensive research programs, and they had joined with the Soviet Union and the 15 other countries in the UN's Conference of the Committee on Disarmament in negotiating a treaty to ban such weapons entirely. Fellow scientists, as well as politicians, were shifting to a default stance that germs were a 'dirty' means of making war as organizations like the American Society for Microbiology endorsed the Nixon announcement, and the 1970 International Congress of Microbiology adopted a resolution condemning bioweapons research.[888] Twenty years after Lederberg had warned about the epistemic power of military secrecy, it seemed clear from the success of their critical reports, talks, and conferences that the military did not, in fact,

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886 Leitenberg and Zilinskas, *The Soviet Biological Weapons Program*, p 527. Garwin briefly discusses the PSAC report in a 1987 oral history interview with the American Institute of Physics. See "Interview of Richard Garwin by Finn Aaserud on June 8,1987," College Park, MD: Niels Bohr Library & Archives, American Institute of Physics, www.aip.org/history-programs/niels-bohr-library/oral-histories/4622-3

887 Martin Kaplan to Joseph Rotblat, December 5, 1969 in RTBT 5/3/1/28.

888 The Pugwash group had planned this coup at the Congress years in advance. See C.-G. Hedén, I. Málek, et al, to Pugwash Continuing Committee, August 25, 1966, p 3, in RTBT 5/4/12/14 Folder 3. As it stood, it was anticlimactic in the wake of the Nixon announcement, with even the previously pro-BW leadership of the American Society for Microbiology joining in sponsoring the resolution as part of a post-1969 reversal in course.

"hold the ace" when scientists criticized them. By the 1960s, scientists like the Pugwash group or Lederberg himself used a rhetorical strategy of possibility which made the specific details of secret information irrelevant. Lederberg, for instance, could warn of the ecological unpredictability of introducing novel microbes into an environment without knowing specifically what experiments Detrick researchers were conducting.[889] The Pugwash study group even used the secrecy of biological weapons researchers as a buttress for their claims about rapid detection technology. Comprehensive open-source reports like those produced by SIPRI, the UN, and the WHO, meanwhile, could draw upon the open publication record of the British and American programs to paint a rough picture of what might be possible in biological warfare. Rosebury, meanwhile, drew upon a combination of personal experience and his own library of open-source information to speak authoritatively about biological warfare in public. In Rosebury's case as well, knowing the particular details of cutting-edge classified work were not necessary when he made an argument condemning biological warfare research in general on ethical grounds.

One thing permitting this proliferation of scientists confidently making claims with incomplete information was the changing social atmosphere of the 1960s, with open criticism of Cold War weapons research proving more acceptable than in the chilled atmosphere of the 1950s. More to the point, however, it had become increasingly clear that Lederberg had been wrong about one major thing in the late 1940s: if military officials "held the ace" of authoritative secret information, it was a card that by and large they could not play. All biological weapons programs labored in one form or another

[889] Lederberg, "Congress Should Examine Biological Warfare Tests," *Washington Post*, March 30, 1968.

under a politically motivated culture of silence; the Americans and British acknowledged the existence of their programs but discouraged officials and scientists from explicitly discussing it and the Soviet Union did not even acknowledge the widely-presumed existence of its program. As the previous chapter shows, the American secrecy system permitted the publication of less-militarily relevant scientific results, crucial to maintaining recruiting links to the 'open' scientific community, but generally did not permit either scientists or military officials to explicitly discuss "biological warfare" in public. One Chemical Corps officer found this system so stifling that upon retirement he wrote a popular philippic that, like an inverse of Rosebury's *Peace or Pestilence*, extolled the dangers of underfunding chemical and biological warfare research.[890] In this culture of official silence, it was extremely difficult for members of the biological weapons program to use their knowledge to refute the claims of outsiders, or indeed to build political support (to their detriment when the NSC group began to meet). Military secrecy, in short, was ultimately as much of an impediment as a shield for supporters of the biological weapons program. It was this frustration which underlay the professional gatekeeping of Riley Housewright, who a decade after these events dismissed Meselson as "not a microbiologist" and a "a certified expert in ½ truths & outright lies re: BW."[891]

Both Rosebury and the Pugwash group (which included Meselson) had found by the 1960s that military secrecy was in fact not a meaningful impediment to activism, but the approaches they adopted were widely divergent. Rosebury explicitly rejected what he

[890] J. H. Rothschild, *Tomorrow's Weapons: Chemical and Biological*, New York: McGraw-Hill, 1964. This book, like the open scientific publications, served as an informational resource for groups like SIPRI.
[891] Riley D. Housewright, Untitled ms (Memo on 1950 San Francisco *Serratia marcescens* tests), n.d. (ca 1981, attached to a note dated March 16, 1981), pp 1-2, in ASM Series 13-IIBP ("Presidential Papers: Riley D. Housewright"), Folder 26 "BW Materials- *Serratia marcescens*." See also Footnote 388, above.

saw as the overly technocratic focus of the Pugwash group, while the Pugwash culture which had developed under Rotblat implicitly rejected Rosebury-style public protest and ethical condemnation in its quest to cultivate behind-the-scenes influence with political elites. These divergent strategies, representing in microcosm contemporary activists' debates between 'outsider' moral absolutism and 'insider' compromise and incrementalism, raise the question of which, in retrospect, was ultimately more successful in bringing about the downfall of American biological weapons research. The answer seems to be that, loathe as they would have been to admit it, neither group could do without the other. The growing public pressure created by public journalism and teach-ins of the sort that Rosebury contributed to was integral to a group like the NSC considering whether to discontinue an entire category of weapons in the first place. On the other hand, increasing elite pressure from the UN and WHO reports and arguments (very reminiscent of those promulgated by the Pugwash group) that it was foolish for a nuclear power like the United State to develop cheaper means of mass destruction played an important role in the NSC group reaching the conclusion that it did when it was convened. Both the push of popular protest and the pull of elite politics were behind the decision to end bioweapons research in 1969.

The activist career of Matthew Meselson is illustrative of how these two approaches were not so incompatible as they seemed. A member of the Pugwash group, Meselson was certainly interested in influencing political elites, but these attempts included the inherently public pressure of testifying before Congress and organizing a discussion on chemical and biological warfare to be held at the December 1969 AAAS meeting.[892] On March 4, 1969, when Rosebury was at a teach-in at Case Western University, Meselson was speaking before a crowd of protesting students and faculty at MIT.[893] Meselson's subsequent career as an expert on biological weapons and avoiding their proliferation continued to be double-edged: he alternately acted as an 'outsider' (for instance, publicly questioning American intelligence officials' assertions that a 1979 outbreak of anthrax in the Soviet city of Sverdlovsk represented an accident from an illicit biological weapons program) and as an 'insider' (joining an early 1990s inspection trip to Sverdlovsk when the existence of this accident and illicit program was confirmed).[894] SIPRI had likewise been an enduring part of the epistemic infrastructure of biological arms control, with the original multi-volume report that appeared in the early 1970s serving as a canonical source for scholars and with an ongoing tradition of reports pertaining to biological arms control. Ironically, one of the most enduring legacies of early Cold War biological warfare research has been the careers and institutions established by the scientists who set out to oppose it.

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[892] William T. Kabisch to J. R. Porter, November 21, 1969 in ASM 8-1A Folder 1. A copy of the speaker list for the symposium is attached to this letter. Along with Meselson, this symposium included Ivan Bennett, and Victor W. Sidel of Physicians for Social Responsibility.

[893] "Schedule of Events at M.I.T.," *The Harvard Crimson*, March 4, 1969, p 4.

[894] Susan Walton, "Clouds of Doubt: 'Germ Warfare' Violation Hard to Pin Down," *BioScience* 30 no 7 (1980), pp 485-487; M. Meselson, J. Guillemin, M. Hugh-Jones, A. Langmuir, I. Popova, A. Shelokov, and O. Yampolskaya, "The Sverdlovsk Anthrax Outbreak of 1979," *Science* 266 no 5188 (November 18, 1994), pp 1202-1208; Michael D. Gordin, "The Anthrax Solution: The Sverdlovsk Incident and the Resolution of a Biological Weapons Controversy," *Journal of the History of Biology* 30 no 3 (1997), pp 441-480; Jeanne Guillemin, *Anthrax: The Investigation of a Deadly Outbreak*, Berkeley: University of California Press, 1999.

Epilogue: The Legacy of Detrick

The post-1969 legacy of the American biological weapons program is an ambiguous one. In one sense, its end was the harbinger of a safer world. American stockpiles of pathogens like anthrax and Venezuelan Equine Encephalitis were destroyed in the wake of Nixon's announcement, and were joined after another clarifying announcement by stockpiles of biologically-produced toxins like saxitoxin and ricin.[895] The budget and personnel cuts that accompanied the end of offensive weapons research at Detrick were regarded by ASM leaders as a professional catastrophe. It was exactly this fact, representing a degradation of expertise akin to that faced by nuclear weapons laboratories in the wake of the Cold War, that made the formal end of offensive research at Detrick mean something.[896] New Anglo-American support for an international treaty to ban biological warfare outright opened negotiations in earnest for what would become the Biological Weapons Convention (BWC) of 1972, the first Cold War agreement to ban an entire class of weapons outright. Declaring biological warfare to be "repugnant to the conscience of mankind," the Convention filled the gaping loopholes left by the 1925

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[895] See John Ellis van Courtland Moon, "The US Biological Weapons Program," in Mark Wheelis, Lajos Rózsa, and Malcolm Dando (eds), *Deadly Cultures: Biological Weapons since 1945*, Cambridge, MA: Harvard University Press, 2006, pp 9-46. A list of stockpiled agents to be destroyed is appended to "Memorandum for the President from Secretary of Defense Melvin Laird, Subject: National Security Decision Memoranda 35 and 44," July 6, 1970, held as Document 22 in the National Security Archive's Electronic Briefing Book No. 58, "Volume III- BIOWAR," https://nsarchive2.gwu.edu/NSAEBB/NSAEBB58/. The data in this source are replicated on pp 37-39 of *Deadly Cultures*.

[896] On the potential loss of nuclear knowledge after the Cold War (and American weapons scientists' campaign to avoid it) see Donald MacKenzie and Graham Spinardi, "Tacit Knowledge, Weapons Design, and the Uninvention of Nuclear Weapons," *American Journal of Sociology* 101 no 1 (1995), pp 44-99; Benjamin Sims and Christopher R. Henke, "Repairing Credibility: Repositioning Nuclear Weapons Knowledge After the Cold War," *Social Studies of Science* 42 no 3 (2012), pp 324-347.

Geneva Convention's ban on the *use* of such weapons by explicitly banning their development and stockpiling as well.[897]

In a sense, this moment could not have come too soon. Just as the Convention was being negotiated, recombinant DNA was being transformed from a laboratory curiosity to a practical means to insert foreign genes into bacteria. In the West, genetic engineering based on techniques like this emerged in the late 1970s and 1980s as a potentially revolutionary (and lucrative) industry, but because of the events of a decade before, this newfound ability to manipulate microbial genomes did not translate into an equivalent military revolution.[898] Genetic engineering techniques have only gotten more sophisticated in subsequent decades. Looking back today, in the age of CRISPR, it seems fortuitous that essentially by historical accident the use of microbial weapons came to be outlawed just at the beginning of these developments. This is not to say that the Convention has fully rid the world of the specter of biological warfare, as some nations' surreptitious programs and contemporary fears of bioterrorism attest. What it has done, however, is enshrine a norm against such weapons that by and large endures in global politics today. The covert nature of national programs that have violated the Convention are in a sense the exception that proves the rule. They stand in stark contrast to open American sponsorship of Detrick's research in the 1950s. Using germs as weapons, or

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[897] For examinations of the BWC and its legacy, see Marie Isabelle Chevrier, "The Politics of Biological Disarmament," and Nicholas A. Sims, "Legal Constraints on Biological Weapons," both in Wheelis, Rózsa, and Dando (eds), *Deadly Cultures*, pp 304-328, 329-354.

[898] A foundational (but dated) history of "biotechnology" (including the evolving meanings of the word) is Robert Bud, *The Uses of Life: A History of Biotechnology*, New York: Cambridge University Press, 1993. A more-recent useful synthesis is Hallam Stevens, *Biotechnology and Society: An Introduction*, Chicago: University of Chicago Press, 2016. Sally Smith Hughes, *Genentech: The Beginnings of Biotech*, Chicago: University of Chicago Press, 2011 examines one of the leading companies in the 'revolutionary' 1970s-1980s period.

even conducting research to prepare to do so, has been relegated to the domain of terrorists and outlaw nations, with some prominent observers even calling for such acts to be regarded as international crimes akin to torture or piracy.[899]

We can also take a far less sanguine view of Detrick's legacy, however. True, offensive research at Detrick ended in the wake of the Nixon announcement, and much of the subsequent research there has been conducted for non-military projects like the National Cancer Institute in the 1970s.[900] Military research on biological weapons has by no means ceased there, however. While the US renounced *offensive* biological warfare research (intended to produce the knowledge, technologies, and microbial cultures to produce a militarily useful biological weapons system), *defensive* research (intended to develop the knowledge and technologies to defend against biological attack) has been an active pursuit at Detrick for the past half-century. As many critics have noted, there is an extremely fine line between the two avenues of research, as both might (for instance) entail enhancing the virulence of pathogens and investigating optimal techniques to spread them through the air, either to undergird a weapon or to prepare to defend against an enemy's weapon.[901] The fact that offensive research is distinguished from defensive more in intent than in capability has been an enduring source of ambiguity since the BWC (which only bans offensive research) was signed. Critics of American defensive

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[899] See e.g. Michael P. Scharf, "Clear and Present Danger: Enforcing the International Ban on Biological and Chemical Weapons Through Sanctions, Use of Biological and Chemical Weapons Through Sanctions, Use of Force, and Criminalization," *Michigan Journal of International Law* 20 no 3 (1999), pp 477-521; Matthew Meselson, "International Criminalization of Biological and Chemical Weapons," *Bulletin of the American Academy of Arts and Sciences* 54 no 2 (Winter, 2001), pp 38-42.
[900] See Robin Wolfe Scheffler, *A Contagious Cause: The American Hunt for Cancer Viruses and the Rise of Molecular Medicine*, Chicago: University of Chicago Press, 2019.
[901] See e.g. discussion of the distinction (or lack thereof), in Milton Leitenberg, *The Problem of Biological Weapons*, Stockholm: Swedish National Defense College, 2004.

research at Detrick, particularly in the 1980s, pointed out that such research could well be seen as a covert offensive program.[902]

A number of Soviet officials agreed, justifying a vast, covert, and explicitly *offensive* program of their own in part as a precaution against American perfidy. Biological weapons research in the USSR in fact surged in scale *after* the Nixon announcement and BWC negotiations, took full advantage of developments in genetic engineering, and continued until the fall of the Soviet Union in the early 1990s.[903] Moreover, until 1992, when defectors' revelations forced the new Russian government to belatedly acknowledge the program's existence, it remained an at least plausibly deniable secret.[904] While American officials did accuse the Soviets of maintaining a covert program in the 1980s, pointing to examples like the outbreak of anthrax in the closed city of Sverdlovsk in 1979, this assessment was based on ambiguous enough information to be disputed by prominent Western observers like Matthew Meselson.[905] It was not until the 1990s that the truth of the Sverdlovsk incident was fully documented (by a team that included Meselson among its members) as having indeed been an accidental release of anthrax from a military laboratory.[906] A norm and legal prohibition against openly conducting bioweapons research is all well and good, but we may well ask what good

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[902] See e.g. Susan Wright, "New Designs for Biological Weapons," *Bulletin of the Atomic Scientists* 43 no 1 (1987), pp 43-46; Seth Shulman, "Funding for Biological Weapons Research Grows Amidst Controversy," *BioScience* 37 no 6 (1987), pp 372-375.

[903] For a comprehensive examination of this program, see Milton Leitenberg and Raymond A. Zilinskas, *The Soviet Biological Weapons Program: A History*, Cambridge, MA: Harvard University Press, 2012.

[904] Kanatjan Alibekov, the most prominent of these defectors, later published a memoir of his experiences in the Soviet program (under his anglicized name). See Ken Alibek, *Biohazard: The Chilling True Story of the Largest Covert Biological Weapons Program in the World- Told from Inside by the Man Who Ran It*, New York: Random House, 1999.

[905] Susan Walton, "Clouds of Doubt: 'Germ Warfare' Violation Hard to Pin Down," *BioScience* 30 no 7 (1980), pp 485-487

[906] Jeanne Guillemin, *Anthrax: The Investigation of a Deadly Outbreak,* Berkeley: University of California Press, 1999.

they do if a superpower could covertly pursue such research with little hindrance. The covert programs of smaller nations like Iraq and South Africa in the 1980s, which were revealed only after the fact in the wake of major disruptions like the Gulf War and fall of the apartheid government, similarly call the credibility of the post-1972 order into question.[907] Perhaps most problematically, as a number of observers have noted, the BWC lacked and continues to lack a mechanism for verifying compliance (of the sort that the Pugwash group had considered *de rigueur* for an effective treaty in the 1960s). Regular review conferences between the signatories have failed to resolve this issue, blocked in part by the concerns of countries like the United States that a binding inspection provision could impact the intellectual property of the biotechnology industry.[908]

Since the end of the 20th century, this worrying weakness in the BWC system has been coupled with growing concerns about the use of biological weapons by non-state actors like terrorist groups or biocriminals. Fears of biological sabotage, as much as overt military attack, had been deeply rooted in the WWII and Cold War-era among the network of 'friends' of the US biological warfare program, overtly justifying (for instance) Alexander Langmuir's establishment of the Epidemic Intelligence Service within the CDC. With the end of the Cold War, however, fears of Communist agents spreading disease began to be replaced by fears of terrorist organizations answering to no

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[907] See Graham S. Pearson, "The Iraqi Biological Weapons Program," and Chandré Gould and Alastair Hay, "The South African Biological Weapons Program," both in in Wheelis, Rózsa, and Dando (eds), *Deadly Cultures*, pp 169-190, 191-212.

[908] Susan Wright and David A. Wallace, "Varieties of Secrets and Secret Varieties: The Case of Biotechnology," *Politics and the Life Sciences* 19 no 1 (2000), pp 45-57; Nicolas A. Sims, *The Evolution of Biological Disarmament (SIPRI Chemical & Biological Warfare Studies 19),* New York: Oxford University Press, 2001.

state. Such fears were not entirely hypothetical: the apocalyptic Japanese cult Aum Shinrikyo had actively tried to develop their own biological weapons before settling for using sarin gas to attack the Tokyo subway system in 1995, while new revelations about covert state-sponsored programs, especially the Soviets', raised the prospect of expertise or even microbial cultures falling into the hands of an unscrupulous highest bidder.[909] Such fears dovetailed with developing worries in the wake of the AIDS crisis that unprecedented and unpredictable 'emerging infectious diseases' would be a major public health concern in the 21st century.[910] Faced with these prospects of deliberately and inadvertently spread diseases in the future, American policymakers in the Clinton administration began to adopt a securitized rhetoric of public health, promoting 'biosecurity' as a major national priority.[911] As was the case a half-century before, scientist-advisors were major actors in constructing these concepts as threats, and in the case of Joshua Lederberg there was direct continuity between the community of scientists concerned about biological warfare in the 1940s-1960s and these advisors of the 1990s. Acting as a major figure in developing and promoting the emerging infectious diseases concept, and serving as a government advisor on the emerging issue of biodefense, Lederberg represented in one career the ever-changing but often-thickening relationship between microbiologists and the American security state that had developed since the 1940s.

909 William Rosenau, "Aum Shinrikyo's Biological Weapons Program: Why Did It Fail?," *Studies in Conflict & Terrorism* 24 no 4 (2001), pp 289-301.
910 On the development of the 'emerging infectious diseases' idea, see Nicholas B. King, "The Scale Politics of Emerging Diseases," *Osiris* 19 (2004), pp 62-76.
911 Susan Wright, "Terrorists and Biological Weapons: Forging the Linkage in the Clinton Administration," *Politics and the Life Sciences* 25 no 1/2 (2006), pp 57-115.

This coupling of security and public health has only accelerated in the 21st century, with ballooning budgets for biodefense and a growing endeavor of 'health security' diplomacy, in the wake of the only major bioterrorist attack on American soil.[912] Shortly after the 9/11 attacks in 2001, letters containing weapons-grade anthrax and threatening death in the name of Allah began to appear at the media and Congressional offices to which they were addressed.[913] 5 people ultimately died, and another 17 were infected in the addressee offices and postal facilities that had handled the letters. Despite the letters' use of al Qaeda-like rhetoric, however, the germs themselves were apparently American in origin: the highly infectious Ames strain of anthrax, isolated and studied by American defensive biological warfare researchers at Detrick in the 1980s. Not only the strain but the physical characteristics of the anthrax powder itself pointed to the perpetrator's sophisticated expertise in 'weaponization' like that which still prevailed at Detrick. (Two of the letters had undergone the difficult process of fine milling into the optimal size for deep inhalation without killing the bacteria, a process that required specialized equipment and expertise found in very few places). A vast FBI investigation using traditional law-enforcement techniques soon began to focus on the theory of a 'lone wolf' terrorist, rather than a sophisticated international network. Traditional techniques of interviewing witnesses and profiling suspects were joined by an equally extensive effort focused on the microbes themselves. Over subsequent years, civilian and government scientists working on this investigation developed novel technologies that examined the genome of the bacilli in the letters with a very high resolution, with the hope of matching

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[912] Andrew Lakoff, *Unprepared: Global Health in a Time of Emergency*, Oakland: University of California Press, 2017.

[913] See Jeanne Guillemin, *American Anthrax: Fear, Crime, and the Investigation of the Nation's Deadliest Bioterror Attack*, New York: Times Books, 2011.

it to a particular culture at one of the relatively few laboratories that held the Ames strain (including Detrick).[914] The novel field of 'microbial forensics' that they were pioneering was a hybrid one, with the scientific goal of achieving this highly discriminating genomic view serving a legal goal of attributing responsibility to a particular individual in a way that would be admissible in a court of law.[915] By 2007, this work had narrowed the origin of the anthrax bacilli down to a particular flask held at Detrick in the lab of microbiologist Bruce E. Ivins. This evidence, coupled with a traditional criminal investigation, convinced the FBI that Ivins had been the culprit. In 2008, with an indictment pending, Ivins committed suicide. Though the general presumption of Ivins' guilt (which was obviously never proved in court) has been controversial, the more general point that the only fatal bioterrorist attack in American history was perpetuated with Detrick anthrax bacilli bears repeating. Detrick researchers had isolated and weaponized Ames strain anthrax in the name of post-BWC defensive research (highlighting again how such research could well be regarded with suspicion), and when misused these microorganisms ultimately killed five people. Among the ambiguities of Detrick's legacy is the fact that in its later life as a biodefense center, it was the most important source of the threat it was tasked with guarding against.

In the fall of 2001, before the Detrick connection was established and in the emotionally charged atmosphere following major terrorist attacks, American policymakers took away a different lesson: that the growing crescendo of dire warnings

[914] Susan D. Jones, *Death in a Small Package: A Short History of Anthrax*, Baltimore: Johns Hopkins University Press, 2013, pp 239-246.
[915] For an influential examination of this sort of meeting of two different knowledge domains, with scientists' studies being influenced by legal standards of evidence-handling and proof, see Sheila Jasanoff, *Science at the Bar: Law, Science, and Technology in America*, Cambridge, MA: Harvard University Press, 1995.

about biological attack should be heeded with top-priority spending and legal powers.[916] Besides vast budget increases for biodefense research and extensive law enforcement powers granted by laws like the USA PATRIOT Act, the new official attention paid to biological threats brought to the fore another ambiguous legacy of the mid-20th century biological weapons program: questions about secrecy and the control of information.

As this dissertation has argued, the secrecy system which surrounded work at Detrick between the 1940s and the 1960s was often an *ad hoc* affair, influenced by groups (like scientists and military officers) with often widely differing interests. Though many questions about if and how particular areas of knowledge should be controlled were tacitly raised in this period, more often than not any answer was equally tacit and contingent. Aerobiological research is an excellent example. In the immediate wake of the Second World War, studies of the airborne transmission of disease like those conducted by Theodor Rosebury's Cloud Chamber project were among the most significant of the scientific findings to come out of the wartime program at Detrick. In a climate that presumed that results like this would be published, a number of papers and an entire monograph by Rosebury were indeed published by 1947.[917] For the next 15 years, however, there was a strict clampdown on publishing further work like this, which was after all among the most pertinent areas of study for would-be bioweaponeers. This policy was in turn partially reversed in the 1960s, however, with a series of conferences organized to showcase declassified Army work to enlist the interest of the civilian

[916] Biological weapons also gained an ambiguous status as a *casus belli* on a state possessing them in this period, with purported evidence that Iraq has retained its earlier program serving as a major component of the Bush administration's justification of war in the name of eliminating "weapons of mass destruction" stockpiles.

[917] See Chapter 4. The monograph was Theodor Rosebury, *Experimental Air-Borne Infection*, Baltimore: Williams and Wilkins Co, 1947.

scientific community in biological warfare.[918] Over a 20-year span, then, results which would be useful for another bioweapons program were eagerly published, kept tightly controlled, and then again published with an eye to convincing skeptics of the efficacy of disseminating airborne microbes. These sharp reverses in policy were generally guided more by concerns like political embarrassment and enlisting more scientific support than with what an adversary might do with such information.

By the turn of the 21st century, still more information from the biological weapons program had been declassified, including scientific reports from the early years at Detrick. Historians and other scholars of biological warfare had begun to use such documents in their work, paralleling similar historical bonanzas like the mass release of information in the 1990s on contemporaneous human radiation experiments under the Manhattan Project and AEC.[919] In the charged atmosphere after 2001, however, these nearly 60-year-old documents took on a new social meaning. No longer mere historical curiosities, their existence was now discussed in a series of media reports in early 2002 as "cookbooks" for biological weapons freely accessible to all.[920] In a partial reversal of previous administrations' policy on whether declassified documents could be *re-*

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[918] See Chapter 4. Examples of these conferences included Walsh McDermott (ed), *Conference on Airborne Infection held in Miami Beach, Florida, December 7-10, 1960. Sponsored by Division of Medical Sciences, National Academy of Sciences-National Research Council*, Baltimore: William & Wilkens, 1961; Naval Biological Laboratory, *First International Symposium on Aerobiology*, Berkeley: Naval Biological Laboratory, 1963.

[919] Examples of histories written with access to these declassified documents include Barton J. Bernstein, "America's Biological Warfare Program in the Second World War," Journal of Strategic Studies 11 (1988), pp 292-317; Ed Regis, *The Biology of Doom: The History of America's Secret Germ Warfare Project*, New York: Henry Holt Co., 1999. The declassification of human-subjects radiological research in the 1990s is discussed in Jonathan D. Moreno, *Undue Risk: Secret State Experiments on Humans*, New York: W. H. Freeman, 2000; Lisa Martino-Taylor, *Behind the Fog: How the U.S. Cold War Radiological Weapons Program Exposed Innocent Americans*, New York: Routledge, 2017.

[920] The most prominent of these appeared on the front page of the *New York Times*. See William J. Broad, "A Nation Challenged: The Biological Threat; U.S. Is Still Selling Reports on Making Biological Weapons," *The New York Times*, January 13, 2002, pp 1, 15.

classified, the Bush administration set about removing this free access, sometimes with formal reclassification of documents and sometimes by simply removing permission for members of the public to view them at the National Archives.[921] This was one part of a more general wave of reclassifications in the post-9/11 period, some of which were seemingly far more for political than any legitimate national security purposes.[922] We might well put cynicism aside in this case, however, and honestly ask if it isn't simply more prudent to keep this old research restricted, perhaps indefinitely? Few observers at the time disagreed with the principle that sufficiently dangerous and specific knowledge shouldn't be freely shared, but a number echoed their predecessors in the 1940s by pointing to the potential public health uses of the newly restricted research. Others, such as secrecy scholar Steven Aftergood, warned of an established pattern of abusing classification authority, arguing that it would only be made worse by enshrining a power to reclassify.[923] Then-ASM president Ronald Atlas summed up one of the most obvious objections to restricting material that had been publicly available for years or decades: "once the cat's out of the bag, can you ever really put it back?"[924] This entire episode is certainly an illustrative case study in the ambiguous and socially constructed nature of

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[921] Steven Aftergood, "Secrecy News," January 15, 2002, https://sgp.fas.org/news/secrecy/2002/01/011502.html; Matthew M. Aid (ed), "Declassification in Reverse: The U.S. Intelligence Community's Secret Historical Document Reclassification Program," *The National Security Archive*, February 21, 2006, https://nsarchive2.gwu.edu/NSAEBB/NSAEBB179/; Scott Shane, "U.S. Archives Making Public Data Secret Again," *The International Herald-Tribune*, February 21, 2006, p 2; Joseph Masco, "'Sensitive but Unclassified:' Secrecy and the Counterterrorist State," *Public Culture* 22 no 3 (2010), pp 433-463.

[922] Perhaps the most infamous example was the reclassification of a CIA report from late 1950, which assessed a very low probability that China would intervene in the ongoing Korean War anytime soon. Chinese troops entered North Korea 12 days later. The document in question had been declassified and had been known to scholars for years. As National Security Archive researcher Matthew M. Aid noted at the time, it is hard to interpret this action as anything more than an attempt to use secrecy to obscure an organizational failure. See Aid (ed), "Declassification in Reverse."

[923] William J. Broad, "A Nation Challenged: The Biological Threat; U.S. Is Still Selling Reports on Making Biological Weapons," *The New York Times*, January 13, 2002, pp 1, 15.

[924] Quoted in Ibid.

secrecy (in which historical curios were transmuted into dangers, and esoteric 'open' documents could with some degree of effectiveness be made secret again by simply ceasing to provide copies). It also represented another legacy of Detrick. Under these new standards, much of the knowledge that had been produced there might never safely see the light of day.

More fundamental than the question of decades-old research reports, and pertaining to more than American national security policy was the general question of what was to be done with potentially dangerous microbiological knowledge. In the late 1940s, scientist-advisors like Baldwin tried to discourage public discussion of biological warfare to forestall a debate about whether, if his field's knowledge could produce weapons of such claimed potency, it should be legally controlled.[925] The explicit analogy for these scientists was the Atomic Energy Act, under which the knowledge produced by *all* nuclear physicists was considered to have been 'born secret,' regardless of whether the government had had any hand in its production. The legal novelty and constitutional issues with this law aside, it represented at the broadest scale a consensus of American society coming out of the Second World War: if esoteric scientific knowledge had enabled the power of the Bomb, society had an interest in controlling that knowledge.[926] For the subset of microbiologists whose tacit business was claiming that their own esoteric knowledge had equivalent destructive potential, the idea that a similar conclusion might emerge from robust public debate was a reasonable fear. Microbiology did not, ultimately, suffer the legal fate of atomic physics, but this did not so much resolve the

[925] See Chapter 4.
[926] For an expanded discussion of the Atomic Energy Act (of 1946, and its 1954 replacement), see Alex Wellerstein, *Restricted Data: The History of Nuclear Secrecy in the United States*, Chicago: University of Chicago Press, 2021.

question of whether it should have as leave it in abeyance. With the exception of the controversy in the 1970s stemming from safety concerns about recombinant DNA techniques, questions about whether microbiologists' work should face legal scrutiny and regulation largely remained out of the public eye throughout the 20th century.[927] Nor, even as genetic engineering techniques grew in sophistication, did microbiologists spend much energy debating whether they should have a professional ethic of withholding knowledge in the name of security.

It was not until the 21st century, steeped as it was in a growing biosecurity mentality, that such questions began to come to the fore again. An early example came in early 2001, months before the events of that fall, when the work of two Australian mousepox researchers attracted media attention.[928] While researching mousepox as part of an effort to control Australia's invasive mouse population, they inserted a new gene in the hopes that the genetically engineered virus would induce infertility in its hosts. Instead, they found that they had enhanced the virulence of the virus, which killed even mice vaccinated against naturally occurring strains. What attracted media attention was not so much what they had done, however, as the fact that they published their work.[929] Mousepox is a close relative of the smallpox virus- eradicated in the wild but still extant in American and Russian laboratories, and increasingly feared as a potential bioterrorist weapon. What if this technique for enhancing the virulence of mousepox, so freely

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[927] On this debate, see Susan Wright, *Molecular Politics: Developing American and British Regulatory Policy for Genetic Engineering, 1972-1982*, Chicago: University of Chicago Press, 1994.
[928] For a retrospective interview of the researchers about their work and this incident, see Michael J. Selgelid and Lorna Weir, "The Mousepox Experience: An Interview with Ronald Jackson and Ian Ramshaw on Dual-Use Research," *EMBO Reports* 11 no 1 (2009), pp 18-24.
[929] Rachel Nowak, "Disaster in the Making: An Engineered Mouse Virus Leaves Us One Step Away from the Ultimate Bioweapon," *New Scientist* 169 no 2273 (January 13, 2001), pp 4-5; William J. Broad, "Australians Create a Deadly Mouse Virus," *The New York Times*, January 23, 2001, p 6.

published, was applicable to smallpox as well? Should ethical scientists or ethical journal editors have published such research? Should legal authorities have been involved?

Though moot at the time (as the research had already been published), questions like these began to sink into the culture and institutions of science, and their influence can be seen in another controversy from a decade later. In 2005, the US government founded the National Science Advisory Board for Biosecurity (NSABB), an institution intended to set policy on potentially dangerous research conducted by or in collaboration with Federal agencies, and specifically addressing "dual-use research of concern," or scientific work which might be used for nefarious purposes by a third party. Six years later, when two teams (one at the University of Wisconsin, the other, partially sponsored by the American NIH, at the Erasmus Medical Center in the Netherlands) submitted their prospective papers on influenza research, the NSABB made headlines by recommending that the work not, in fact, be published.[930] The two teams had been engaged in gain-of-function research, investigating how easily the H5N1 influenza's airborne transmissibility could be enhanced, which raised the concern that this research into possible natural developments could also be used by a would-be bioterrorist. The teams reluctantly agreed to refrain from publishing their work (the Dutch team also being restrained by the Dutch government, which declared that publishing their potentially dangerous work would require an export license), until an expert panel convened by the WHO opined that their work could in fact be safely published and the American and Dutch institutions relented. Shortly afterward, the NIH released clarifying guidelines for institutional biosafety

[930] Scholarship examining this episode includes Kathleen M. Vogel, "Expert Knowledge in Intelligence Assessments: Bird Flu and Bioterrorism," *International Security* 38 no 3 (Winter 2013/2014), pp 39-71; Andrew Lakoff, "A Fragile Assemblage: Mutant Bird Flu and the Limits of Risk Assessment," *Social Studies of Science* 47 no 3 (2017), pp 376-397.

committees on which types of research constituted “dual-use research of concern” and thus merited circumspection about publishing. The question of whether certain biological knowledge is dangerous enough to merit either ethically motivated self-censorship or regulatory control has remained contested among microbiologists, biosecurity experts, and government officials over the past decade, and likely will continue to do so into the future. What is clear from the very existence of this debate, however, is that in a biosecurity-conscious world, microbiology is now being confronted with questions of openness and secrecy that it largely avoided in previous decades. Another legacy of Detrick (or more accurately the network of ‘friends’ which surrounded it) was effectively to kick the proverbial can of how to treat dangerous knowledge down the road.

There are major presumptions built in to classifying a research paper as “dual-use,” or an old report as a “cookbook” for biological weapons, most notably an idealized view that information can hold power somewhat irrespective of social context. Beyond a minimum threshold of competence, any chef can use a cookbook’s guidance to produce an acceptable product; by implication, a “cookbook” for producing finely powdered anthrax is tantamount to a weapon in the hands of any competent microbiologist. Security studies scholars like Kathleen Vogel and Sonia Ben Ouagrham-Gormley have questioned this presumption, however.[931] They point to scholarship on the crucial importance of tacit knowledge in accomplishing technical tasks and argue that developing useful biological weapons is far more difficult that simply following a “cookbook’s” recipes. An effective biological nonproliferation regime, they argue, would focus far more on awareness of and

[931] See e.g. Kathleen M. Vogel, *Phantom Menace or Looming Danger?: A New Framework for Assessing Bioweapons Threats*, Baltimore: Johns Hopkins University Press, 2013; Sonia Ben Ouagrham-Gormley, *Barriers to Bioweapons: The Challenges of Expertise and Organization for Weapons Development*, Ithaca: Cornell University Press, 2014.

control over skilled individuals than the nigh-Sisyphean task of keeping information truly secret amidst an information age. Political scientist Frank Smith III makes a somewhat similar argument about the social context of knowledge, asserting that the institutional 'frame' (implicitly shared presumptions about the world) in which biodefense preparations take place is largely determinative of how effective those preparations ultimately are.[932] Information alone, in Smith's view, is not sufficient without the proper institutional context to support it. Broadly speaking, such scholarship is influenced by the same traditions of studying the social component of knowledge that has led to various historiographical 'turns' in the history of science and have undergirded studies of openness and secrecy over the past few decades. Seen in this light, this study of how openness and secrecy worked in biological weapons research in the Second World War and early Cold War not only illuminates a crucial period in history but can provide insights for the present moment as well.

We live in an increasingly biosecurity-conscious age (particularly in the wake of the COVID-19 pandemic) and a decent amount of our culture's concern is focused not just on the emergence of new infectious diseases, unforeseen consequences of manipulating living organisms, or the accidental release of dangerous microbes, but also on the malice of nebulously defined human actors. Perhaps, in a counterfactual world that had never flirted with using microbiology for military purposes in the age of openly pursued total war, this concern would still have emerged as our attempts to control living things increased, but in our world the idea of germs as weapons is as much a legacy of

[932] Frank L. Smith III, *American Biodefense: How Dangerous Ideas about Biological Weapons Shape National Security*, Ithaca: Cornell University Press, 2014.

that age of total war as stockpiles of radioactive waste. Managing that legacy entails responsible scientific ethics and effective international governance, but another substantial component lies in establishing effective and reasonable standards for controlling information. Many of the questions about how to do this that confront us today confronted Detrick researchers, their network of 'friends,' and the military officials who sponsored them, and one of the legacies they have left us with is their failure, by and large, to satisfactorily answer them. Perhaps their most fundamental legacy, however, lies not in a secret but in an openly shared idea: that "the mighty microbe can go to war." Only time will tell where that idea takes us.

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