RADIATION ECOLOGIES: BOMBS, BODIES, AND ENVIRONMENT DURING THE ATMOSPHERIC NUCLEAR WEAPONS TESTING PERIOD, 1942-1965

by

Emory Jerry Jessee

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APPROVAL

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Emory Jerry Jessee

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Timothy LeCain (Co-chair)

Michael Reidy (Co-chair)

Approved for the Department of History and Philosophy

David Cherry

Approved for The Graduate School

Dr. Ronald W. Larsen

STATEMENT OF PERMISSION TO USE

DEDICATION

For Teresa, with love.

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ABBREVIATIONS

AAAS	Association for the Advancement of Science
ACBM	Advisory Committee for Biology and Medicine
AEC	Atomic Energy Commission
AEP	Atomic Energy Project
AFL	Applied Fisheries Laboratory
AFSWP	Armed Forces Special Weapons Project
BEAR	Biological Effects of Atomic Radiation Committees
CNI	Committee for Nuclear Information
DBM	Division of Biology and Medicine
DoD	Department of Defense
ESA	Ecological Society of America
FRC	Federal Radiation Council
HASL	Health and Safety Laboratory
HASP	High Altitude Sampling Program
JCAE	Joint Committee for Atomic Energy
MED	Manhattan Engineering District
NAS	National Academy of Science
NTS	Nevada Test Site
PHS	Public Health Service
r	Roentgen
WMSCRI	Western Montana Scientists' Committee for Radiation Information

LIST OF ARCHIVES AND COLLECTIONS ABBREVIATIONS

These are abbreviations for the special collections and archives libraries cited in the in the footnotes. Short titles to designate individual collections are included in the parentheses in the right hand column.

LANL-RL	Los Alamos National Laboratory Research Library Los Alamos, NM Online access at http://tang.lanl.gov
LOC	Library of Congress Manuscript Reading Room Washington D.C.
NAA	National Academies Archives Washington D.C. (CBEAR) Committees on Biological Effects of Atomic Radiation (MET) Meteorology Committee (OaF) Oceanography and Fisheries Committee
NARACP	National Archives and Records Administration at College Park College Park, MD (RAEC) Records of the Atomic Energy Commission Record Group 326 (RDNTRA) Records of the Defense Threat Reduction Agency Record Group 374
NTA	Nuclear Testing Archive Las Vegas, NV
SIA	Smithsonian Institution Archives Washington D.C. (Schultz Papers) Leonard P. Schultz Papers Record Unit 7222
SIOA	Scripps Institution of Oceanography Archives La Jolla, CA (SIODR) Scripps Institution of Oceanography, Office of the Director (Revelle) AC 16

LIST OF ARCHIVES AND COLLECTIONS ABBREVIATIONS - CONTINUED

	(SIOSF) Scripps Institution of Oceanography, Subject Files MC6A
UCLASC	University of California Los Angeles, Charles E. Young Research Library, Department of Special Collections Los Angeles, CA (Warren Papers) Stafford Leak Warren Papers Collection 987
UCLAUA	University of California Los Angeles, Charles E. Young Library, University Archives Los Angeles, CA (Warren AF) Administrative Files of Stafford Warren Record Series no. 300
UGA- Hargrett	University of Georgia Hargrett Rare Book and Manuscript Library Athens, GA
UMSC	The University of Montana Maureen and Mike Mansfield Library Archives and Special Collections Missoula, MT (Chessin Papers) The Meyer Chessin Papers Ms 630
UWSC	University of Washington Libraries Special Collections Seattle, WA (LRD Papers) The Lauren Donaldson Papers Accession no. 2932-007 (LRER) The Laboratory of Radiation Ecology Records Accession no. 00-065
WHMC	Western Historical Manuscripts Collection University of Missouri, St. Louis, MA (CEIR) Committee for Environmental Information Records

ABSTRACT

From 1945 to 1963 the United States Atomic Energy Commission detonated over 200 nuclear weapons tests at its Nevada and Pacific test sites, irradiating every living thing on the planet. Much of the historical scholarship on the period has focused on the scientific debate over the health effects of low-level radiation exposure or on determining what and when the Atomic Energy Commission knew about the health effects fallout. This dissertation, however, argues that the growth of ecological thinking about the health effects of fallout exposure in environmental sciences such as ecology, oceanography, and meteorology dramatically reshaped what was known about radiological risk and provided the scientific foundation for the Limited Test Ban Treaty. By highlighting the ways that radiation traveled beyond the boundaries of the test sites and became incorporated into critical human food chains, this ecological way of perceiving fallout largely replaced previous approaches to fallout risks derived from the discipline of health physics that focused on external forms of radiation exposure and ideas of spatial containment.

This dissertation, however, also argues that fallout radiation proved much more than a menacing pollutant. Because environmental scientists can utilize radiation as a tool to trace out structure and function of the ecosystem, as well as oceanic and atmospheric motions, it also emerged during this period as a critical scientific practice. In tracing radiation as it moved through the environment, environmental scientists not only made legible the connections between the health of human bodies and the irradiated environment, but also demonstrated empirically that the earth was a spatially integrated biosphere. Such realizations, this dissertation concludes, formed an important footing the nascent environmental movement and helped establish the authority of the environmental sciences in matters of environmental pollution and regulation.

INTRODUCTION

When Rachel Carson opened her seminal book *Silent Spring* she chose to begin not with the dangers of DDT (her main topic), but rather with a seemingly more menacing pollutant: radioactive fallout produced by nuclear weapons tests. "Strontium 90," she wrote, "released through nuclear explosions into the air, comes to earth in rain or drifts down as fallout, lodges in the soil, enters into the grass or corn or wheat grown there, and in time takes up its abode in the bones of a human being, there to remain until his death. Similarly, chemicals sprayed on croplands or forests or gardens lie long in soil, entering into living organisms, passing from one to another in a chain of of poisoning and death."[1] This linkage was by no means coincidental. Carson's comparison of DDT to the effects of fallout surely resonated with an American public already attuned to the bodily threat that environmental radiation posed.[2] Indeed, when the book was published in 1962 humans across the entire globe were exposed to as much nuclear fallout in that single year than had accumulated from all prior tests.

The rise of "ecological thinking" in the United States during the 1950s and growing concern regarding the dangers of fallout from nuclear weapons testing bear a mutually causative relationship. From 1947 to 1963, the United States tested over 200 nuclear weapons in the atmosphere at the Pacific and Nevada test sites. At the inception of the postwar weapons testing program in 1946, the AEC initially designed their fallout program based upon a model of industrial safety inherited from the Plutonium Project of

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[1] Rachel Carson, *Silent Spring*, 40th anniversary ed. (New York: Houghton Mifflin, 2002), 6.
[2] Ralph H. Lutts, "Chemical Fallout: Rachel Carson's Silent Spring, Radioactive Fallout, and the Environmental Movement," *Environmental Review* 9 (1985).

the Manhattan Engineering District (MED) during the war. "Health Physics," as the specialized practice of radiation safety was dubbed, was born out of the exigencies of the atomic bomb project, but its core practices originated in the industrial hygiene and toxicology programs of early-twentieth century factories. Focusing on what they called "good housekeeping," health physicists ensured safety at the Manhattan Engineering District production facilities by carefully shielding worker bodies from radiation and "sanitizing" the workplace environment through engineering controls. Health physicists' experience protecting workers in this simplified environment resulted in the idea that fallout in environments outside of the factory was also subject to this same kind of modernist technoscientific control and could thus be similarly protected. That is, the control elicited over the environment of the factory imbued health physicists with the notion that the environment was largely passive and thus irrelevant when it came to protecting human health. The result was that radiological protection from fallout was expressed in terms of limiting exposure to short-lived *external* gamma radiation, a focus which vastly underestimated the complex ways that the environment might mediate the *internal* ingestion of the less abundant, but longer-lived, alpha- and beta-emitting radionuclides. Similarly, health physicists assumed that like the laboratory or factory, fallout radiation would be largely confined within the boundaries of the test sites. To be sure, they understood that the bomb tests would spew significant amounts of radiation into the atmosphere. But they assumed that the atmosphere too was passive—nothing more than a vertical radioactive sink. Radiation safety in the early years of nuclear testing was thus based on the idea of boundedness; human bodies, the environment, and geographical space were discrete entities with few material linkages.

Yet this supposed boundedness that characterized bodies, environments, and geographical space gradually eroded in the 1950s. The advent of thermonuclear weapons, which pumped ever larger amounts of radiation into the environment, coupled with the development of radiotracer tools in environment sciences such as ecology, meteorology and oceanography demonstrated empirically that fallout was travelling globally and accumulating at high levels in living things. Work conducted under the new field of radioecology by Lauren Donaldson at the University of Washington Applied Fisheries Laboratory, Richard Foster at Hanford Site, and Eugene Odum at the University of Georgia, for example, illustrated how specific radionuclides became incorporated into ecosystems, biomagnified through food chains, and deposited inside human bodies. Oceanographers associated with Scripps Institute of Oceanography and the Applied Fisheries Laboratory used nuclear fallout to show how dynamic processes in the ocean distributed radiation far beyond the Pacific test sites. Meteorologist Lester Machta of the United States Weather Bureau used nuclear fallout to study atmospheric dispersal mechanisms to counteract the AEC's claim that the stratosphere reduced and limited the amount and rates of thermonuclear injected radioactive debris that could fall back to Earth. As a result of this work, the hazards of nuclear fallout no longer seemed to be confined to the test sites or limited simply to the external dose. Instead, fallout became an environmental problem that depended on knowing the atmospheric mechanisms by which significant levels of radiation could travel offsite and the ecological pathways that mediated the internal ingestion of particularly worrisome radionuclides like strontium-90 and iodine-131. The evaluation of fallout hazards now owed as much to the expertise of environmental scientists as the health physicists.

The growing awareness of these environmental aspects of nuclear fallout was the driving force behind the fallout controversy during the late 1950s and led to the establishment of a number of proto-environmentalist anti-testing groups like the Greater St. Louis Committee for Nuclear Information, SANE, and Women Strike for Peace. Facing growing pressure from groups such as these to end the tests, the United States, Soviet Union, and Great Britain signed the Limited Test Ban Treaty in 1963 ending all atmospheric, underwater, and outer space tests, which effectively closed the fallout controversy.

Interestingly, most studies of the fallout controversy have centered their analysis on the scientific and political debate regarding the risk to humans from exposure to low-levels of fallout radiation, downplaying or virtually ignoring the role of ecology and ecological thinking in shaping the dispute.[3] Scholarship in environmental history, conversely, has paid scant attention to nuclear weapons testing, despite the fact that

[3] Barton C. Hacker, *Elements of Controversy: The Atomic Energy Commission and Radiation Safety in Nuclear Weapons Testing, 1947-1974* (Berkeley: University of California Press, 1994); Carolyn Kopp, "The Origins of the American Scientific Debate over Fallout Hazards," *Social Studies of Science* 9, no. 4 (1979); J. Samuel Walker, *Permissible Dose: A History of Radiation Protection in the Twentieth Century* (Berkeley: University of California Press, 2000). Bruno and Masco are the only sources that I have found that explore explicitly the environmental work conducted during fallout and explored its effect on the fallout controversy or the Limited Test Ban Treaty. See Laura A. Bruno, "The Bequest of the Nuclear Battlefield: Science, Nature, and the Atom During the First Decade of the Cold War," *Historical Studies in the Physical and Biological Sciences* 33, no. 2 (2003); Joseph Masco, "Bad Weather: On Planetary Crisis," *Social Studies of Science* 40, no. 1. Another approach has been to look at disciplinary differences. See J. Christopher Jolly, "Thresholds of Uncertainty: Radiation and Responsibility in the Fallout Controversy" (PhD diss., Oregon State University, 2004).

fallout comprised the first great modern global environmental crisis.[4] This fact is even more puzzling given that studies of nuclear power development abound in the field.[5]

This dissertation endeavors to apply recent theoretical and methodological insights in environmental history, the history of science and technology, and historical geography to explore how new scientific practices in the environmental sciences produced knowledge about fallout hazards, and thus new conceptualizations of risk, which led ultimately to the end of atmospheric nuclear weapons testing. It argues that the core of the debate over nuclear fallout hinged on emerging holistic concepts, based in material scientific practices, of the dynamic and integrated relationship between human bodies, irradiated environments, and the spaces where the tests were conducted. In short, this dissertation aims to demonstrate how the Limited Test Ban Treaty originated in the fusion of nuclear physics with the environmental sciences.

Yet, this dissertation also argues that the story of nuclear fallout explains more than simply why nuclear tests went underground. As suggested by the quote from Carson's *Silent Spring*, the fallout controversy also reveals how many of the ideological, scientific, and social elements that made up environmental movement in the late 1960s were forged in the widespread protests to nuclear fallout. New ecological ways of "seeing" the holistic relationships between bodies, the environment, and space was one such critical link. It is not coincidence, for example, that new views of the Earth as an

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[4] There were, of course, other global environmental crises, particularly with regard to epidemics. However, I use "modern" here to denote the fact that this was the first global crisis that was recognized in contemporary thought as an environmental crisis.

[5] Brian Balogh, *Chain Reaction: Expert Debate and Public Participation in American Commercial Nuclear Power, 1945-1975* (Cambridge: Cambridge University Press, 1991); Thomas R. Wellock, *Critical Masses: Opposition to Nuclear Power in California, 1958-1978* (Madison: University of Wisconsin Press, 1998); John Wills, *Conservation Fallout: Nuclear Protest at Diablo Canyon* (Reno: University of Nevada Press, 2006).

integrated biosphere or ecosphere were beginning to be articulated just as fallout was emerging as a planetary crisis. It was during this period too that many of the leading figures of the environmental movement first emerged as voices for the environment. Barry Commoner, for instance, cut his teeth as an environmental activist during the fallout controversy when he founded the science information movement.

There were still other lasting legacies of the fallout controversy. Historians of science have spent that last thirty years analyzing the growth and impact of federal and military patronage on the concepts, practices, and even the very content of the sciences during the Cold War. Although much of this work has centered on sciences of obvious instrumental value in the conduct of war, statecraft, and diplomacy (i.e. physics), historians have also begun to turn their attention to the ways that environmental sciences have contributed to and been shaped by national security imperatives. This dissertation seeks to contribute to this work by exploring how the fallout research played a critical role in the development and professionalization of environmental sciences such as ecology, meteorology, and oceanography. On the one hand, the AEC's need for new knowledge about the environmental movement of radiation dramatically improved the funding possibilities for the environmental sciences. In ecology, for example, new centers of ecological research (specifically in ecosystems ecology) were established at the University of Georgia, Oak Ridge National Laboratory, and the University of Washington directly as a result of concerns about radioactivity. On the other hand, radiation profoundly shaped the way these scientists went about their business. In addition to being a source of contamination, radiation also, ironically, proved to be an extremely valuable tool for tracing out and making legible environmental mechanisms that prior to the advent of the atomic age had largely been foreclosed to them. This development served to "harden" these traditionally field-based sciences, putting their scientific claims on equal footing (or nearly so) with laboratory science, and thereby enhancing the status and authority of the practitioners of these disciplines. Indeed, the tremendous growth and usefulness of the environmental sciences in the 1960s and beyond for identifying and regulating other environmental pollutants (such as DDT, for example) was driven in great measure by the resources (financial and technological) that were accrued during the fallout controversy. The fact that radiation served as an important tool in field practice also provides an opportunity to explore the development of two very different strains of ecological thinking that emerged during this period: the popular ecology of Carson and Commoner and the technocratic ecology of professionals like Eugene Odum.

By placing fallout radiation at the center of my story and analysis, this dissertation endeavors to show how radiation as both an agent of disease and tool in scientific practice animated the environment in profoundly new ways and in the process shaped the character of the dissent against fallout, helped lay important ideological and infrastructural foundations for the environmental movement, and fueled the growth of the environmental sciences. In so doing, I argue that radiation played an important role in the changing ways we have come to understand nature and our place in it.

Nuclear Fallout and History

Although the signing of the Limited Test Ban Treaty in 1963 largely quelled the controversy over fallout, questions over the safety practices of the AEC and its scientists have persisted. In the late 1970s, for example, Congressional hearings were initiated after allegations from Marshall Islanders, "downwinders," and "atomic veterans" surfaced accusing the government and the military of negligently and deliberately exposing them to dangerous levels of fallout, which, they claimed, resulted in the high cancer rates among their populations.[6]

This renewal of what might be termed the "second fallout controversy" sparked a flurry of historical, though largely journalistic, accounts that have been mired in debates over what precisely the AEC knew about the health risks associated with fallout and whether the Commission acted sufficiently to protect those exposed. One camp insists that the AEC and its scientists knew what the dangers associated with exposure to fallout were when weapons testing began in the late 1940s, and that the AEC knowingly put workers and the public at undue risk.[7] Others contend that the risks were not well known and even supposing that they were, the fallout dispute raised political and ethical

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[6] The most famous of these was perhaps House Committee on Interstate and Foreign Commerce, Subcommittee on Oversight and investigation, *"The Forgotten Guinea Pigs": A Report on Health Effects of Low-Level Radiation Sustained as a Result of the Nuclear Weapons Testing Program Conducted by the United States Government* 96th Congress, 2nd sess., 1980.

[7] For example, Howard Ball, *Justice Downwind: America's Atomic Testing Program in the 1950's* (New York: Oxford University Press, 1986); Daniel F. Ford, *The Cult of the Atom: The Secret Papers of the Atomic Energy Commission* (New York: Simon and Schuster, 1982); John G. Fuller, *The Day We Bombed Utah: America's Most Lethal Secret* (New York: New American Library, 1984); Richard L. Miller, *Under the Cloud: The Decades of Nuclear Testing* (New York: Free Press, 1986); Harvey Wasserman and et al, *Killing Our Own: The Disaster of America's Experience with Atomic Radiation* (New York: Delacourt Press, 1982). For a more contextualized account that places the decision to test in light of cold war imperatives see A. Constandina Titus, *Bombs in the Backyard: Atomic Testing and American Politics*, vol. 2nd (Reno: University of Nevada Press, 2001).

questions that scientific evidence could not adjudicate.[8] This historical debate has hardly been settled and continues to flare up as new information and incidents are brought to light.[9]

Given the enduring controversy over the atmospheric weapons testing program it should come as no surprise that the literature has been dominated by these kinds of legalistic “what” and “when” questions regarding the state of contemporaneous scientific knowledge about the health effects of radiation. Yet, as the interpretive polarization in respect to AEC culpability suggests, these may be the wrong questions to ask. Within the last two to three decades, for instance, historians and sociologists of science have focused far more on *how* scientists know rather than on simply *what* they know. This shift in analysis has been a reflection of the turn toward scientific practice and culture that has largely replaced the history of ideas that so dominated the field until the 1970s. As a result, scholars have increasingly focused their attention toward the sites of knowledge production and on the ideological, material, and social resources employed in creating knowledge. One of the consequences of this practical turn has been a growing awareness that the content of scientific knowledge has been in significant part "constructed" fundamentally by the contexts in which it was made (social, cultural, or political, for example).

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[8] Catherine Caufield, *Multiple Exposures: Chronicles of the Radiation Age* (Chicago: The University of Chicago Press, 1989); Robert A. Divine, *Blowing on the Wind: The Nuclear Test Ban Debate, 1954-1960* (New York: Oxford University Press, 1978); Barton C. Hacker, *The Dragon's Tail: Radiation Safety in the Manhattan Project, 1942-1946* (Berkeley: University of California Press, 1987); Hacker, *Elements of Controversy: The Atomic Energy Commission and Radiation Safety in Nuclear Weapons Testing, 1947-1974*; Robert Seidel, "Books on the Bomb," *Isis* 81, no. 308 (1990); Walker, *Permissible Dose: A History of Radiation Protection in the Twentieth Century*.

[9] For example, the controversy the 1990s sparked by Eilleen Welsome’s articles on the governments plutonium injections in the 1940s. Eileen Welsome, *The Plutonium Files: America's Secret Medical Experiments in the Cold War* (New York: Dial Press, 1999).

In the more radical strain of this scholarship, social relations, not nature, determine what counts as scientific knowledge.[10]

These insights regarding the "social construction" of scientific knowledge carry obvious implications for studies of health during nuclear weapons testing. Nuclear Regulatory Commission historian J. Samuel Walker, for example, has written that "even if the data had been more definitive, the fallout debate raised philosophical, moral, and political questions that scientific evidence alone could not resolve."[11] To be sure, Walker is right that the fallout controversy raised serious ethical and political questions. Nonetheless, he too neatly separates the production of scientific knowledge from social values and politics. On the one hand, his argument seems to imply a dichotomy between a putatively "pure" science and "corruptive" politics, thereby limiting the ways that politics influence science. On the other hand, it assumes that scientific knowledge about fallout effects played little to no role in how either policymakers or citizens considered the ethical implications of testing.

One of the core arguments I make in this dissertation is that shifting conceptual models for understanding the relationship between human health and environmental radiation shaped how scientists and the American public conceived of the risks associated with nuclear fallout. In particular, I argue that the production of knowledge about fallout hazards and risk was tied to cultural and scientific assumptions about the relationship between human bodies, the environment, and geographical space. Health physics, for

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[10] Two classics in the "social construction of science" school are David Bloor, *Knowledge and Social Imagery*, vol. 2nd (Chicago: University of Chicago Press, 1991); Steven Shapin and Simon Schaffer, *The Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life* (Princeton: Princeton University Press, 1985).

[11] Walker, *Permissible Dose: A History of Radiation Protection in the Twentieth Century*, 28.

example, was a discipline based on earlier toxicological practices that tended to limit the power of the environment to mediate radiation exposures. As a result, health physics in the early years of nuclear weapons testing operated under the assumption that short-lived external gamma exposures represented the greatest threat to human health. Throughout the 1950s, however, more ecologically-minded thinking about radiation exposure surfaced, revealing how significant amounts of radiation were travelling outside the proving grounds, assimilating into ecological food chains, and accumulating inside the human body. In other words, *what* was known about about the risks of nuclear testing was determined in part by *how* one understood the relationship between human bodies, the environment, and space. Moreover, it was precisely this realization that one could not escape fallout and that it was contaminating food supplies that, for many, challenged the morality of testing.

The history of the health effects of nuclear fallout need not be reduced to culture, social relations, or politics, despite my argument for a greater awareness of the historicity of scientific concepts and practices during the period. Nor does attention to these contextual issues reduce risk to mere perception—"a bit like aesthetic judgment"—as some have argued.[12] Rather, I wish to demonstrate the critical role that the environment played in shaping the development of more ecological ways of seeing the problem of fallout. Similarly, I put radiation in the forefront of this story because as both a source of worry and as a tool for making the environment legible it was also an important actor in shaping knowledge and risk. In this way, this dissertation is influenced by recent

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[12] Mary Douglas and Aaron Wildavsky, *Risk and Culture: An Essay on the Selection of Technological and Environmental Dangers* (Berkeley: University of California Press, 1982), 186.

scholarship in the history of science and environmental history that has endeavored to historicize scientific knowledge while also taking the materiality of nature quite seriously. Yet, it also aims to show how radiation and the fallout controversy reveal productive new ways of thinking about these issues. First, a review of this literature and how I situate my work within it is in order.

Bridging the Gap between the History of Science and Technology and Environmental History: Agency, Nature, and Fallout

One of the critiques leveled at the social constructionists has been the epistemological implications of their work, namely its relativism arising out its reduction of scientific knowledge to social relations. Actor-Network Theory (ANT), for example, provides a model for understanding the production of knowledge that treats social forces and material objects as co-actors that mutually constitute the other.[13] ANT scholars, therefore, tout their theory as anti-essentialist since scientific knowledge can be reduced to neither social relations nor material reality.

Although ANT has been criticized for ascribing agency to non-human actors and its apparent inattention to politics, the theory has been influential in the history of science. Yet, even without the contributions of ANT, many historians of science working at the level of practice have operated under the intuitive logic that nature matters in the construction of scientific knowledge. During the height of social constructivist approach

[13] Bruno Latour, *Reassembling the Social: An Introduction to Actor-Network Theory* (Oxford: Oxford University Press, 2005).See also Bruno Latour, *Science in Action: How to Follow Scientists and Engineers through Society* (Cambridge: Harvard University Press, 1987); Bruno Latour, *We Have Never Been Modern* (Cambridge: Harvard University Press, 1993); John Law, "Technology and Heterogeneous Engineering: The Case of Portuguese Expansion," in *The Social Construction of Technological Systems: New Directions in the Sociology and History of Technology*, ed. Wiebe E. Bijker, Thomas P. Hughes, and Trevor J. Pinch (Cambridge: the MIT Press, 1987).

to the field during the 1980s, a number of works persuasively demonstrated how experimental traditions and the materiality of scientific objects themselves function to "constrain" and thus limit the number of ways that evidence can be manipulated toward theoretical or ideological ends.[14] More recently, histories of the development of standardized organisms such as fruit flies and mice have pointed to the ways that "biological histories" of these organisms have shaped scientific concepts and laboratory practices.[15]

It has only been in the last decade or so, however, that historians of science have turned to the natural environment as an agent in the formation of scientific theories and practices. This shift has grown in significant part out of the spatial turn within the field. Initially, however, much of the the spatial work conducted by historians of science was driven by the social constructionist school and, not surprisingly, focused on the sites of knowledge production, especially the laboratory.[16] Although some recent work has moved beyond the laboratory to investigate the role of mapping and other forms of geographic knowledge in the spatial construction and ordering of earth's surface (usually

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[14] Peter Louis Galison, *How Experiments End* (Chicago: University of Chicago Press, 1987); Latour, *Science in Action: How to Follow Scientists and Engineers through Society*; M. J. S. Rudwick, *The Great Devonian Controversy: The Shaping of Scientific Knowledge among Gentlemanly Specialists* (Chicago: University of Chicago Press, 1985). On the more technological side, see Gabrielle Hecht, *The Radiance of France: Nuclear Power and National Identity after World War II* (Cambridge: MIT Press, 1998).

[15] Robert Kohler, *Lords of the Fly: Drosophila Genetics and the Experimental Life* (Chicago: University of Chicago Press, 1994). See also Karen Rader, *Making Mice: Standardizing Animals for American Biomedical Research, 1900-1955* (Princeton: Princeton University Press, 2004).

[16] Thomas F. Gieryn, *Cultural Boundaries of Science: Credibility on the Line* (Chicago: University of Chicago Press, 1999); Bruno Latour, "Give Me a Laboratory and I Will Raise the World," in *Science Observed: Perspectives on the Social Study of Science*, ed. Karen Knorr-Cetina and Michael Mulkay (London: Sage Publications, 1983); Steven Shapin, "Placing the View from Nowhere: Historical and Sociological Problems in the Location of Science," *Transactions of the Institute of British Geographers* 23, no. 1 (1998); Susan Leigh Star and James R. Griesemer, "Institutional Ecology, 'Translations' and Boundary Objects: Amateurs and Professionals in Berkeley's Museum of Vertebrate Zoology, 1907-39," *Social Studies of Science* 19, no. 3 (1989).

in the service of European imperialism), the analysis has in large part remined centered on the role of scientific actors in production of space and place, rather than the environment.[17]

Robert Kohler's work on lab-field border practices, however, offers an intriguing analysis of the ways that place has mattered in the conduct of field work.[18] According to Kohler, field sciences such as ecology and natural history underwent a period of soul searching in the late nineteenth and early twentieth centuries as a result of the growing epistemological prestige ascribed to laboratory work in the nineteenth and early twentieth centuries. But rather than abandon the field in favor of the lab, he argues that field scientists engaged in a kind of hybrid lab-field practice—what he calls "practices of place." That is, instead of adopting laboratory practices for field work, field scientists read natural landscapes and indentified "natural processes [that] can be interpreted and understood as experiments—not as our experiments, but as 'Nature's experiments'."[19] In this sense, practices of place enabled scientists to conduct lab-like experiments in the field, but without reducing the complexities of the place itself. The material environment, in other words, is not a passive object of scientific study. Rather, it actively

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[17] For general treatments of the historical geography of science that see place as rooted in the social and cultural aspects of science see David N. Livingstone, "The Spaces of Knowledge: Contributions toward a Historical Geography of Science," *Environment and Planning D: Society and Space* 13 (1995); David. N. Livingstone, *Putting Science in Its Place: Geographies of Scientific Knowledge* (Chicago: University of Chicago Press, 2003); Simon Naylor, "Introduction: Historical Geographies of Science - Places, Contexts, Cartographies," *British Journal for the History of Science* 38, no. 1 (2005). On the connection between mapping, science, and empire see D. Graham Burnett, *Masters of All They Surveyed: Exploration, Geography, and a British El Dorado* (Chicago: University of Chicago Press, 2001); Michael S. Reidy, *Tides of History: Ocean Science and Her Majesty's Navy* (Chicago: University of Chicago Press, 2008).
[18] Robert E. Kohler, *Landscapes and Labscapes: Exploring the Lab-Field Border in Biology* (Chicago: University of Chicago Press, 2002); Robert E. Kohler, "Place and Practice in Field Biology," *History of Science* 40, no. 2 (2002). See also Arn M. Keeling, "Charting Marine Pollution Science: Oceanography on Canada's Pacific Coast, 1938-1970," *Journal of Historical Geography* 33 (2007).
[19] Kohler, *Landscapes and Labscapes: Exploring the Lab-Field Border in Biology*. Quote is on page 214.

shapes the very way scientists go about their business and the knowledge that is produced as a result. It is important to note, however, that Kohler's work does not return to a normative view of universal nature. What counts as nature is still considered an outcome of the social, scientific, and material practices that went into its making. Nonetheless, his argument that place has shaped scientific practice has led Paul Sutter to refer to it as an "environmental history of science."[20]

Not coincidentally, as historians of science have been more willing to put the materiality of nature back into its stories, environmental historians have turned away from the normative view of nature that drove so much of its early scholarship. Much of this cultural turn can be attributed to William Cronon's famous essay "The Trouble with Wilderness," published nearly twenty years ago.[21] While the essay is remembered chiefly for Cronon's declaration that "there is nothing natural about the concept of wilderness," his political and methodological message that the field's focus on wilderness directed attention away from lived environments has had an equally lasting impact. In the years since its publication, the field had witnessed a proliferation of monographs and articles centered on urbanization, technology, built landscapes, and, critically, public health.[22]

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[20] Paul S. Sutter, "Nature's Agents or Agents of Empire? Entomological Workers and Environmental Change During the Construction of the Panama Canal," *Isis* 98, no. 4 (2007): 728.

[21] William Cronon, "The Trouble with Wilderness or, Getting Back to the Wrong Nature," *Environmental History* 1, no. 1 (1996). See also the introduction and volumes in William Cronon, ed., *Uncommon Ground: Rethinking the Human Place in Nature* (New York: W.W. Norton & Co., 1995).

[22] The literature is now lengthy. But see Michael Bess, *The Light-Green Society: Ecology and Technological Modernity in France, 1960-2000* (Chicago: University of Chicago Press, 2003); Andrew Isenberg, *Mining California: An Ecological History* (New York: Hill and Wang, 2005); Matthew Klingle, *Emerald City: An Environmental History of Seattle* (New Haven: Yale University Press, 2007); Martin V. Melosi, *Effluent America: Cities, Industry, Energy, and the Environment* (Pittsburgh: University of Pittsburgh Press, 2001).

There is some irony that health has become such an important topic in environmental history in last ten years given that field was founded in part by work on the relationship between disease ecology and global historical events found in works such as Alfred Crosby's *The Columbian Exchange* and William McNeill's *Plagues and Peoples* in the 1970s.[23] Perhaps because these works have been severely criticized for being environmentally determinist, environmental historians have shied away from health topics until recent years.[24] In any event, scholarship in the turn toward health has been far more careful about granting historical agency to non-human actors as if they were somehow independent of social, economic, political, and cultural circumstances. As Gregg Mitman explains in his recent historiographical and methodological treatment of health and environmental history:

> Unlike past scholarship that relied upon disease ecology to animate microbes as universal agents of empire, we need to examine more critically the ways in which on-human actors came into being and acquired agency as disease becomes a more central analytic category within environmental history. The actions of nonhuman actors such as rats, lice, or fleas are connected to, but not independent of, histories of knowledge production through which such objects gain new meaning and power in the world.[25]

For Mitman, the problem for environmental historians is not that nature or the environment is socially constructed and thus relative. Rather, the traditional method in the field of reading current scientific knowledge back on to past to grant agency to the

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[23] Alfred W. Crosby, *Ecological Imperialism: The Biological Expansion of Europe, 900-1900* (Cambridge: Cambridge University Press, 1986); William H McNeill, *Plagues and Peoples*, reprint ed. (New York: Anchor Books, 1998). For a more recent but similar take, see Jared M. Diamond, *Guns, Germs, and Steel: The Fates of Human Societies* (New York: W.W. Norton & Company, 1997).

[24] That is, focusing on the environment as the sole cause of global events erases the social, political, economic, and cultural that also explain, for example, imperialism. See David Arnold, *The Problem of Nature: Environment, Culture and European Expansion* (Cambridge: Blackwell, 1996).

[25] Gregg Mitman, "In Search of Health: Landscape and Disease in American Environmental History," *Environmental History* 10, no. 2 (2005): 193.

environment ignores the ways in which historically situated knowledge and experience of health and disease has shaped the landscape and been a product of environmental change. Nonhuman agency, Mitman concludes, is therefore "dependent upon a relational network of people, things, and forces at any given historical moment in time. Nature is an outcome, not the cause of changes in the land."[26]

One of the ways in which this relational view of materiality and ideas has influenced the turn toward health in the field has been the insistence that environmental knowledge has been a product of embodied ways of experiencing health and disease. In his book *Breathing Space*, Mitman, for example, ties in the way that Americans' bodily experience of airborne allergens since the nineteenth century has fueled environmental perceptions and been an agent and product of material changes in the landscape.[27] Similarly, Linda Nash, Conevery Bolton Valencius, and Michelle Murphy have explored the ways that embodied environmental knowledge has counteracted the tendency in modern medical practice to locate disease etiology within the body rather than the environment.[28]

My point in discussing the confluence of the history of science and environment history in the last few years is not simply to direct the reader to the ways that the fields have reached a kind of "middle ground" on the question of the agency of nature. Instead,

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[26] Ibid.
[27] Gregg Mitman, *Breathing Space: How Allergies Shape Our Lives and Landscapes* (New Haven: Yale University Press, 2007). See also, Gregg Mitman, "Hay Fever Holiday: Health, Leisure, and Place in Gilded-Age America," *Bulletin of the History of Medicine* 77, no. 3 (2003).
[28] Michelle Murphy, *Sick Building Syndrome and the Problem of Uncertainty: Environmental Politics, Technoscience and Women Workers* (Durham: Duke University Press, 2006); Linda Nash, *Inescapable Ecologies: A History of Environment, Disease, and Knowledge* (Berkeley: University of California Press, 2006); Conevery Bolton Valencius, *The Health of the Country: How American Settlers Understood Themselves and Their Land* (New York: Basic Books, 2002).

my aim is to point to how this work has profoundly altered how we think about historical agency. As Linda Nash and Timothy Mitchell have argued, when we interrogate the relationship between natural and human actors, we are questioning not only the agency of nature, but the nature of agency.[29] That is, what these works that I have discussed above show is that the mind-body (or subject-object) dualism inherited from the Enlightenment, which has provided the foundation for how we think of human agency as being tied to constraining "structures," has outlived its usefulness. Thus, when we speak of the agency of nature we are not talking about how the environment structures human actions. Instead, human and non-human actions and intentions arise out of engagement with the natural or material world. As Mitchell has argued, "agency, like capital, is a technical body, is something made."[30]

I too am interested in moving beyond traditional conceptions of historical agency to address the ways that human and non-human actors intersected to produce new knowledge about fallout risks, the environment, and scientific practices. And similar to many of the works I have discussed, I also narrate this story of shifting perceptions of fallout risks as an instance where ideological predisposition met with the materiality of nature to create new knowledge. Yet, by placing fallout at the center of my analysis I seek to show how radiation as both an "artificial" pollutant and scientific tool formed a critical nexus or node through which ideas and the materiality of the environment came into being and acquired agency. Let me explain.

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[29] Timothy Mitchell, *The Rule of Experts: Egypt, Technopolitics, Modernity* (Berkeley: University of California Press, 2002); Linda Nash, "The Agency of Nature or the Nature of Agency," *Environmental History* 10, no. 1 (2005). See also, Sutter, "Nature's Agents or Agents of Empire? Entomological Workers and Environmental Change During the Construction of the Panama Canal."
[30]Mitchell, *The Rule of Experts: Egypt, Technopolitics, Modernity*, 53.

Although much of my dissertation explores the shifting ways that scientists perceived the relationship between human bodies and the environment, it is not strictly an embodied environmental history. To be sure, there were instances where populations exposed to fallout in southern Utah engaged in a kind of popular epidemiology in response to the presence of cancer clusters in their neighborhood. But the long latency period in which exposure to low levels of radiation is expressed as biological effect has generally complicated this aspect.[31] Indeed, it is not surprising that the fallout period received renewed attention twenty years later when radiation had sufficient time to manifest itself in a range of health effects.

Rather, my approach owes more to Paul Sutter's recent work that situates the focus on health in environmental history within the literature on the interactions between scientific practice and place. In his article "Nature's Agents or Agents of Empire?," Sutter seeks to highlight the agency of the material environment by exploring the ways that scientific observation and experimentation by "entolomological workers" produced new knowledge about mosquito vectors that at once enabled U.S. construction of the Panama Canal but also subverted the dominant imperial thinking of the time that the "tropics" were unfit for white Europeans and thus needed to be dominated or "sanitized." By showing how efforts to control and sanitize tropical environments produced landscape changes that in fact encouraged the spread of mosquitoes and disease, they called into question the very idea of conquering nature. According to Sutter, "material influence can be seen quite clearly at the points of tension between ideological predisposition and

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[31] Philip Fradkin has written about the cancer clusters in southern Utah. Notably, these did not arise until the 1970s although fallout was suspected as a source of some health issues in these communities earlier. Phillip L. Fradkin, *Fallout: An American Nuclear Tragedy* (Tucson: University of Arizona Press, 1989).

empirical observation and that scientists are particularly fruitful subjects for examining such tensions."[32] The materiality of the environment, in other words, spoke through scientists to alter subtly ideologies of environmental triumphalism.

While my dissertation is framed similarly to Sutter (in the sense that I too explore the ways that material scientific practices in the environment subsequently shaped ideological preconceptions), I argue that the materiality of nature became apparent not simply through the scientists observing or experiencing the environment, but through the radiation. Ascribing agency to chemical pollutants is not a novel concept in environmental history. After all, we often highlight the agency of nature by the unintended environmental consequences that arise when industries pump chemicals into the environment. Yet, radiation proved much more than an agent of illness and death. It was also a tool through which scientists made the complex environmental processes that connected human bodies, the environment, and geographical space together legible. When scientists utilized fallout radiation or unique radioactive tags to trace out atmospheric motions, food chain dynamics, and oceanic currents, older health physics approaches to radiological safety no longer appeared reasonable or safe. Indeed, it is this duality of radiation as a contaminant and as a scientific tool that underlaid the irony of ecologist Eugene Odum's perceptive comment in 1957 that “the atomic age can well provide the means of solving the very problems it creates…the use of radioactive tracers in the environment offers unlimited opportunities.”[33] Radiation, the very source of so

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[32] Sutter, "Nature's Agents or Agents of Empire? Entomological Workers and Environmental Change During the Construction of the Panama Canal," 729.
[33] Eugene P. Odum, "Ecology and the Atomic Age," *The ASB Bulletin* 4, no. 2 (1957): 29.

much anxiety during the fallout controversy, was a tool that mediated scientists' interaction with and understandings of nature.

The fact that radioactive tools have provided scientists with a powerful means by which we have come to know the environment should give environmental historians pause to reflect upon the conceptual divides that we often place between the natural and the artificial, the environmental and the technological. Scholarship in the subfield of envirotech, for example, has explored the ways that the natural and the technological are deeply intertwined; what is technological is built out of nature, while the natural feeds back on the technological.[34] More recently, envirotech scholars have begun to realize that the creation of natural knowledge is in fact profoundly technical.[35] By exploring the ways that radiation has mediated the construction of knowledge of the environment, this dissertation builds on this work. Yet, by ascribing agency to radiation as both a deadly pollutant and a tool in the making of natural knowledge it also slaughters another sacred cow in environmental history—the tendency to see chemical pollutants as inherently unnatural. In so doing, it seeks to move beyond the declensionist narratives that have dominated the field. The story of radioactive fallout is in fact a story of progress. It

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[34] See, for example, Timothy J. LeCain, *Mass Destruction: The Men and the Giant Mines That Wired America and Scarred the Planet* (New Brunswick: Rutgers University Press, 2009); Sara Pritchard, *Confluence: The Nature of Technology and the Remaking of the Rhône* (Cambridge: Harvard University Press, 2011); Martin Reuss and Stephen H. Cutcliffe, eds., *The Illusory Boundary: Environment and Technology in History* (Charlottesville: University of Virginia Press, 2010); Edmund Russell, "Evolutionary History: A Prospectus for a New Field," *Environmental History* 8, no. 2 (2003); Jeffery K. Stine and Joel A. Tarr, "At the Intersection of Histories: Technology and the Environment," *Technology and Culture* 39 (1998).

[35] Etienne Benson, *Wired Wilderness: Technologies of Tracking and the Making of Modern Wildlife* (Baltimore: The Johns Hopkins University Press, 2010); Robert Gardner, "Constructing a Technological Forest: Nature, Culture, and Tree Planting in the Nebraska Sand Hills," *Environmental History* 14 (2009); Reuss and Cutcliffe, eds., *The Illusory Boundary: Environment and Technology in History*.

illuminates the role of radiation in producing environmental knowledge and how that knowledge put a close to a global environmental crisis.

Bodies and Space in Environmental History

As I noted above, one of the central themes in the literature on human health in environmental history has been the exploration of the how the tensions between embodied and universal scientific knowledge (particularly toxicological) has shaped the ontology of disease. Linda Nash's work on the history of pesticide poisoning in California agricultural fields, for example, convincingly demonstrates that toxicological practices for determining the "reality" of pesticide-induced disease were founded in modernist ideas about the discreteness and impermeability of bodies and environments. Such conceptualizations, she argues, effectively rendered the environmental pathways by which pesticides might be consumed by orchard workers invisible. As a result, when workers complained that the orchards were making them sick they were routinely ignored. Eventually, state health officials did take the bodily knowledge of the worker's seriously and identified some of the key environmental pathways that were exposing workers bodies to pesticides. Similar studies on bodies and environmental pollution have focused on different environments (the indoor workplace environment, for example) and national contexts.[36]

What is striking about Nash's description of toxicology is how practitioners in the new discipline of health physics during the Manhattan Engineering District shared similar

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[36] Murphy, *Sick Building Syndrome and the Problem of Uncertainty: Environmental Politics, Technoscience and Women Workers*; Brett Walker, *The Toxic Archipelago: A History of Industrial Disease in Japan* (Seattle: University of Washington Press, 2009).

assumptions, methodologies, and practices. This was hardly coincidental.[37] Although the birth of health physics arose out of the demands of protecting MED workers from wholly new toxic substances produced by nuclear fission, its core practices were inherited from the radiation toxicology programs of the 1930s radium dial painting studios. From the beginning, therefore, radiation protection from fallout was based on practices tried and true in the factory. One of the consequences of envisioning fallout as a factory problem, as Nash shows, was a general environmental myopia.[38] In the case of health physicists, this shortsightedness was reflected in the overweening emphasis they placed on the effects of *external* gamma exposure as opposed to the *internal* ingestion of alpha and beta particles. As Karl Z. Morgan who led the health physics section at Oak Ridge later reflected, “We gave little consideration to internal dose from body intake of radionuclides because almost nothing had been published on the subject except some information on high-level exposure to radium. We focused primarily on preventing the radiation syndrome (acute death) from very high exposures. Our secondary concern was to prevent acute external radiation damage, such as skin erythema. Unfortunately, we accepted the threshold hypothesis: that so long as we avoided the skin-reddening threshold dose, all of us were safe.”[39] Yet, the problem was not simply lack of adequate knowledge as Morgan’s fellow health physicists J. Newell Stannard perceptively noted in his massive digest of the scientific literature on radioactivity and health. The lack of

[37] J. C. Jolly, "Linus Pauling and the Scientific Debate over Fallout Hazards," *Endeavour* 26, no. 4 (2002).
[38] The factory approach of health physicists is noted in Jacob Darwin Hamblin, *Poison in the Well: Radioactive Waste in the Oceans at the Dawn of the Nuclear Age* (New Brunswick: Rutgers University Press, 2008), 56. Linda Nash has written that "toxicology reproduces the world in the image of the early twentieth-century factory." See Linda Nash, "Purity and Danger: Historical Reflections on the Regulation of Environmental Pollutants," *Environmental History* 13, no. 4 (2008): 565.
[39] Karl Z. Morgan and Ken M. Peterson, *The Angry Genie: One Man's Walk through the Nuclear Age* (Norman: University of Oklahoma Press, 1999), 21.

environmental approaches to fallout early in the atmospheric weapons testing program, he argued, was due to “the fact that man [as] ‘an integral part of his environment’ had not yet become generally appreciated.”[40] Thus when health physicists set out for Alamogordo New Mexico in July of 1945 to make preparations for protecting the surrounding area from the very first atomic explosion, they saw in the New Mexico desert not a dynamic and complex environment, but rather something more akin to a factory.

Yet, as the move from the factories where the bombs were built to the sites where they were to be field tested suggests, matters of scale and spatial relations were also tied to how health physicists perceived radiation hazards. Tim LeCain's history of mining in Montana, for instance, offers an intriguing analysis of the spatial dynamics involved in the engineering approach to the environment and health hazards. As he richly demonstrates, Anaconda Company mining engineers' control of the subterranean space of the mine fostered a deep arrogance about their ability to control pollution in the terrestrial environment. Mining engineers assumed that they could apply the engineering skills they learned in the simplified environment of the mine to the complex landscape of the surface by building ever taller smokestacks so that smelter pollution would remain in the atmosphere longer. The result was the Washoe Smokestack, the largest freestanding masonry structure in the world, and also a spectacular failure in preventing the contamination of the downwind Deer Lodge Valley.[41]

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[40] J. Newell Stannard, *Radioactivity and Health: A History* (Richland, WA: Pacific Northwest Laboratory, 1988), 768.
[41] LeCain, *Mass Destruction: The Men and the Giant Mines That Wired America and Scarred the Planet*.

Something very similar was going on with the health physicists' approach to fallout; their experience protecting workers in the simplified and tightly controlled environment of the bomb building factories imbued them with the confidence that the environment outside the factory walls was similarly amenable to engineering practices. As Scott Kirsch has shown in his work on the nuclear bomb excavation projects of the late 1950s and early 1960s (Project Plowshare), this kind of engineering approach to fallout protection tended to spatialize the bomb tests as bounded discrete events.[42] That is, through technical control the AEC assumed that the majority of fallout would be contained within the boundaries of the test site. This notion of containment was contested when Plowshare moved from the Nevada Test Site to the first feasibility studies in Alaska where the AEC hoped to "dig" a harbor. There the AEC ran against a counter spatialization from ecologists hired to perform ecological studies of the affected area. Thinking more ecologically, these scientists maintained that the radiation produced by the bombs could not be contained within the prescribed "fallout sector" because once in the environment, the radiation would be subject to whims of nature, which knew no boundaries.

In this sense, Kirsch's discussion of space and the environment is reminiscent of Mark Fiege's work on weeds in Montana. As mobile and uncontrolled nature, weeds knew no bounds, moving in and across the fences that abstractly divided the landscape into property. "Transboundary movement," contends Feige, "could be profoundly unsettling to the desired spatial order, but it also carried enormous potential for spatial—

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[42] Scott Kirsch, *Proving Grounds: Project Plowshare and the Unrealized Dream of Nuclear Earthmoving* (New Brunswick: Rutgers University Press, 2005).

and social—transformation. When weeds spanned boundaries, they put at risk the fiction that the grid separated one unit of land from another."[43] What each of these works show is that conceptualizations of the environment are deeply connected to notions of space. Indeed, one of the main arguments that I am making in this dissertation is that modernist ideas about bodies and the environment were inextricablyconnected to technoscientific ways of seeing space; all were intimately tied to the concept of boundedness. As the boundaries between human bodies and the environment eroded through the 1950s, so too did space emerge as interconnected.

In the case of nuclear fallout, boundary control applied as much to the vertical axis as the horizontal.[44] Fallout, of course, was falling and knowing how and where it was going to fall was of paramount importance. Nonetheless, with the advent of thermonuclear weapons testing, the AEC assumed that the majority of radioactive debris injected into the stratosphere from these tests would remain there for upwards of 10-12 years. According to AEC Commissioner Willard Libby, the stratosphere functioned as a global boundary layer, helping to protect humans from fallout by storing testing radiation, thereby ensuring adequate time for radioactive decay, and enabling even distribution throughout the globe. Yet during a Congressional hearing on fallout in 1959, Weather Bureau meteorologist Lester Machta proved that stratospheric fallout was in fact falling much faster and indeed concentrating in the most populated regions in the earth—the

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[43] Mark Fiege, "The Weedy West: Mobile Nature, Boundaries, and Common Space in the Montana Landscape," *Western Historical Quarterly* 36, no. 1 (2005): 25.

[44] That is also one of the points made in LeCain, *Mass Destruction: The Men and the Giant Mines That Wired America and Scarred the Planet.*. See also Bruce Braun, "Producing Vertical Territory: Geology and Governmentality in Late Victorian Canada," *Ecumene* 7, no. 4 (2000); Rosalind Williams, *Notes on the Underground: An Essay on Technology, Society, and the Imagination* new ed. (Cambridge: The MIT Press, 2008).

northern hemisphere. Thus the stratosphere, rather than being the vertically bounded atmospheric sink that Libby claimed, was in fact a far more dynamic and vertically integrated space than previously imagined.

Fallout and the Roots of Environmentalism

In tracing the emergence of new ideas about the holistic relationship between bodies, the environment, and space, I also endeavor to demonstrate how these realizations formed a critical cultural and scientific footing for the environmental movement in the late 1960s and early 1970s. And in keeping the central theme of this dissertation, I want to show how some of the ideas, actors, and social and scientific practices that made up the environmental movement coalesced around fallout radiation.

As noted in the opening quotation from *Silent Spring,* Carson deliberately likened DDT to strontium-90 to set up her argument about the ecological consequences of the indiscriminate release of pesticides in the environment. Ralph Lutts has argued that one of the primary reasons for the tremendous success and reception of *Silent Spring* hailed from the fact that the fallout controversy "pre-educated" Americans about the ways that chemicals enter into the food chain and accumulate in human bodies.[45] Similarly, Linda Nash has described Carson's book as being a touchstone for the reemergence of popular notions of the "ecological body," which she argues formed a major component of the environmental movement. This dissertation seek to meld and extend both of these insights by highlighting the critical importance of fallout as a means by which the global

[45] Lutts, "Chemical Fallout: Rachel Carson's Silent Spring, Radioactive Fallout, and the Environmental Movement."

public became aware of the interconnections between their bodies and environmental pollution. Although I do not wish to give the impression that the emergence of the ecological thinking about bodies and environments arose solely out of the fallout controversy, it is worth emphasizing that the global public was all too aware that nuclear testing radiation touched and was inscribed into the biological matrix of every human (and indeed every living thing) on the planet.

Indeed, the fact that fallout was a global phenomena also points to another lasting influence on the environmental movement—global spatial interconnectedness. While we often think of this kind of holistic or ecological view of the Earth as having arisen out the notion of "Spaceship Earth" or the famous Apollo 11 image in the late 1960s, I argue that the origins of this kind of thought can be better traced to the growing realization that seemingly disconnected nuclear events in one part of the world could and did have serious repercussions for the rest of the globe. As the advent of thermonuclear weapons testing raised the specter of global fallout, so too did notions of the global biosphere or ecosphere emerge as ways to organize and systematically link the earth into an integrated whole. This idea of spatial ecospheric connectedness would eventually form a critical cornerstone in environmentalist thought when, for example, Barry Commoner argued that the "first law of ecology" was the notion that "everything is connected to everything else" in *The Closing Circle*.[46]

As Commoner would further relate in *The Closing Circle*, the growth of his environmental thought stemmed from his unease about nuclear fallout—"I learned about

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[46] Barry Commoner, *The Closing Circle: Nature, Man, and Technology*, 1st ed. (New York: Alfred A. Knopf, 1971), 29.

the environment from the United States Atomic Energy Commission in 1953."[47] Yet, Commoner's influence on environmentalism proved more than ideological, as Michael Egan has recently shown.[48] As a founder of the Committee for Nuclear Information (CNI), he helped provide the foundation for what would later become an important social and political mechanism by which environmental information would be communicated to the public, namely the science information movement. CNI was based on Commoner's conviction that a central problem in the controversy over fallout was due to the fact that the technocratic AEC controlled knowledge about fallout levels and effects. By collecting and distributing fallout information to the public, CNI empowered citizens on a grass-roots level to weigh in on critical public policies. With the end of atmospheric testing, however, CNI would move on to other environmental issues and its fallout newsletter, *Nuclear Information,* would in 1969 become *Environment* magazine.

By pointing to the influence of the fallout controversy on the emergence of this environmentalist triumvirate—i.e. ecological bodies, ecological geographies, and democratic distribution of scientific knowledge—this dissertation contributes to a growing body of literature that has complicated the classic treatment of environmentalism that locates the movement in postwar affluence and consumption.[49] Yet, as important, it

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[47] Ibid., 45.

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

[49] The original thesis is Samuel P. Hays, *Beauty, Health, and Permanence: Environmental Politics in the United States* (New York: Cambridge University Press, 1987). For a sampling of critiques see Robert Gottlieb, *Forcing the Spring: The Transformation of the American Environmental Movement* (Washington D.C.: Island Press, 2005); Karl Jacoby, *Crimes against Nature: Squatters, Poachers, Thieves, and the Hidden History of American Conservation* (Berkeley: University of California Press, 2003); Adam Rome, *The Bulldozer in the Countryside: Suburban Sprawl and the Rise of American Environmentalism* (Cambridge: Cambridge University Press, 2001); P. S. Sutter, *Driven Wild: How the Fight against Automobiles Launched the Modern Wilderness Movement* (Seattle: University of Washington Press, 2004).

also argues against the tendency to see environmentalism as a predominantly non-scientific or non-expert-driven movement. Linda Nash, for example, has credited Rachel Carson for sparking popular notions of the ecological body, but identifies her unique way of connecting bodies and the environment to older holistic notions of nineteenth century Hippocratic medicine rather than to her solid background in environmental field science.[50] Similarly, Donald Worster has downplayed professional ecosystems ecologists as a force in environmentalism owing to their managerial ethos toward the environment and technocratic tendencies.[51] But, as Christopher Sellers and Stephen Bocking have shown, these arguments too neatly separate the popular from the technocratic or the expert from the public; depending on certain institutional or cultural contexts, scientists working in industrial hygiene or ecology have provided critical empirical insights that have formed the basis for ecological critiques of production.[52]

In this dissertation I build on the insights of Sellers and Bocking by pointing to the ways that the environmentalist ideas that I describe above were rooted in scientific practice and dissent during the fallout controversy. On the one hand, I emphasize the role of the scientists such as Commoner outside the AEC in the science information movement for disseminating ecological and biological knowledge about fallout effects and the ways in which they marshaled that knowledge to create the conditions for

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[50] Nash, *Inescapable Ecologies: A History of Environment, Disease, and Knowledge*, 156-60.
[51] Donald Worster, *Nature's Economy: The Roots of Ecology* (San Francisco: Sierra Club Books, 1977), 311-3.Worster Nature's economy, 311-3. Ironically, Worster also notes that scientists also formed a major front of criticism about environmental pollution on page 22.
[52] Stephen Bocking, *Ecologists and Environmental Politics: A History of Contemporary Ecology* (New Haven: Yale University Press, 1997); Christopher Sellers, "Factory as Environment: Industrial Hygiene, Professional Collaboration and the Modern Sciences of Pollution," *Environmental History Review* 18, no. 1 (1994); Christopher C. Sellers, *Hazards of the Job: From Industrial Disease to Environmental Health Science* (Chapel Hill: University of North Carolina Press, 1999).

scientifically authoritative grass-roots level dissent. Although the science information movement was at its most basic level informed by populist sentiment, I argue that scientists formed a critical element in the intellectual leadership of fallout activism and later the environmental movement. On the other hand, I want to further muddy the public-expert divide, by similarly emphasizing the role of AEC environmental scientists in the production of fallout knowledge and the ways in which they dissented from official Commission positions. Rather than see these scientists as mere reflections of the technocratic order and ethos of the AEC, I argue that they formed a three-pronged scientific attack that fundamentally altered how the AEC assessed and regulated fallout radiation. Some like Lauren Donaldson, Eugene Odum, and John Wolfe operated largely within the AEC bureaucracy. Others still, such as meteorologist Lester Machta, aired their grievances in Congressional hearings. But no matter how they expressed their disagreement with AEC policy, their work and advocacy played a fundamental role in alerting both the AEC and the public of the environmental aspects of human fallout effects.

Yet, their work had an impact that went beyond the AEC and the fallout controversy. To focus solely on their efforts to alert the AEC of the connections between environmental radiation and human health is to miss the critically important ways in which the new radiotracer practices that they helped develop provided a firm scientific footing through which the environment became legible. By illuminating food chain dynamics such a biomagnification, atmospheric dispersal mechanisms, or ocean current dynamics, they contributed materially to the production of new ideas about the connections between human health and the environment and the global ecosphere.

To be sure, few of these scientists would turn out to be environmental advocates in the traditional sense of the word. Radiation for them proved much more than a contaminant. It was also a tool—a new practice—that enabled them to study the environment in profoundly new ways. It is for this reason that I want to suggest a possible explanation for why they never quite adopted the environmentalist mantle. Because they understood radiation as technological means by which they unveiled Nature, they were perhaps not as beholden to the human-nature divide that has so animated environmentalist thought. Technology, even something seemingly sinister like radiation, was a productive force in the creation of natural knowledge, not simply an agent of Nature's destruction. Thus managing technological progress rather than dismantling it, provided the surest means for ensuring technological progress and sustainability. This is perhaps best reflected in Eugene Odum's quotation above that radioactive tools provided a means by which humans can solve environmental problems. Theirs was thus a technocratic branch of ecological thinking, but it was ecological nonetheless.

Radiation and the Environmental Sciences

While the story of science during the Cold War has been told largely from the perspective of physics, more recent approaches have explored the ways that the environmental sciences have shaped and been shaped by postwar national security concerns. This dissertation builds on this literature to discuss the important ways that the research on nuclear fallout laid some of the early ideological and material infrastructure that would contribute the development of the modern environmental sciences. In particular, the history of testing fallout helps illuminate four critical shifts in environment science concepts and practices: the redefinition of the environment, including notions of the global biosphere; the shift in views of the Earth from a national security space to a fragile globe; the development of new scientific tool kits and practices; and the field's growing scientific authority in matters related to environmental governance and regulation.

One of the central analytical foci in the history of Cold War environmental sciences has centered on the U.S. military and government's growing need for environmental knowledge as the Cold War reordered and imaginatively remapped the Earth as global military space. As never before, environmental sciences such as geology, oceanography, and meteorology emerged as critical instruments in the endeavor to map the globe in order to render it legible and thus controllable in the service of waging and winning the Cold War.[53] Although much of this research has focused on how military patronage shaped the content and character of environment science, new scholarship has

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[53] See, for example, the articles in the special issue "Earth Science in the Cold War," *Social Studies of Science* 33, no. 5 (2003). See also Jacob Darwin Hamblin, *Oceanographers and the Cold War: Disciples of Marine Science* (Seattle: University of Washington Press, 2005); Hamblin, *Poison in the Well: Radioactive Waste in the Oceans at the Dawn of the Nuclear Age*; Ronald Rainger, "Constructing a Landscape for Postwar Science: Roger Revelle, the Scripps Institution and the University of California, San Diego," *Minerva* 39 (2001); Ronald Rainger, "Patronage and Science: Roger Revelle, the U.S. Navy, and Oceanography at the Scripps Institution," *Earth Sciences History* 19, no. 1 (2000).

explored the ways that this global purview redefined the environment in militarized terms, in particular the notion of the biosphere.[54]

This dissertation seeks to expand on this research by exploring the ways in which research in ecology, oceanography, and meteorology on nuclear fallout played a critical role in the emergence of biospheric concepts. It does so on two levels. On the one hand, by taking the "long view" on fallout research, this dissertation provides a lens into the ways that the environment emerged as an actor in the Cold War and explores, subsequently, how environmental knowledge of radiation effects contributed to ideas about the environment. That is, it traces the history of fallout from an incidental environmental byproduct of the arms race to a critical research program in its own right that illuminated environmental processes and mechanisms that would have lasting value to the environmental sciences. On the other hand, this dissertation also provides needed insight into the ways that the environmental sciences "scaled up" from field sciences beholden to specific research sites to become truly global sciences. Whereas previous scholarship has shown how computer technologies, global satellite data and images, and international scientific institutions contributed to making the environmental sciences global, I argue that radiotracer technologies also played a critical role.[55] Fallout, as I

[54] Matthias Dorries, "The Politics of the Atmospheric Sciences: 'Nuclear Winter' and Global Climate Change," *Osiris* 26 (2011); Masco, "Bad Weather: On Planetary Crisis."

[55] Paul N. Edwards, *The Closed World: Computers and the Politics of Discourse in Cold War America* (Cambridge: The MIT Press, 1996); Paul N. Edwards, "Meteorology as Infrastructural Globalism," *Osiris* 21 (2006); Paul N. Edwards, *A Vast Machine: Computer Models, Climate Data, and the Politics of Global Warming* (Cambridge: The MIT Press, 2010); James Rodger Fleming, "Polar and Global Meteorology in the Life of Harry Wexler, 1933-62," in *Globalizing Polar Science: Reconsidering the International Polar and Geophysical Years*, ed. Roger D. Launius, James Rodger Fleming, and Davis H. DeVorkin (New York: Palgrave Macmillan, 2010); Katherine C. Harper, *Weather by the Numbers: The Genesis of Modern Meteorology* (Cambridge: The MIT Press, 2008). See also Sheila Jasanoff and Marybeth Long Martello, eds., *Earthly Politics: Local and Global in Environmental Governance* (Cambridge: The MIT Press, 2004).

show in chapter 6, for example, fundamentally transformed meteorological practice by making it possible to trace aerosols, wind patters, and other atmospheric phenomena on a global level. Similarly, weapons tests afforded new opportunities in oceanography to study large-scale physical and biological phenomena in the oceans.

Yet, even more "place-based" sciences such as ecology owed a debt to radiotracer technologies for expanding the purview of the discipline. Historians of ecology have noted how important radiotracer methods were for "hardening" ecological practice and transforming the discipline from the descriptive toward the physical or metabolic.[56] Less well attended to, however, is the way that radiotracers, as an important practice in the emergence of the ecosystems concept, also helped ecologists to more freely move between spatial scales. In chapters 3 and 4, for example, I show how ecologists associated with early atomic energy development were enamored with testing sites such as Bikini Atoll because those isolated places offered them unique opportunities to study discrete, self-contained spaces—what they called "natural laboratories"—where natural processes could be investigated in experimental lab-like conditions.[57] With the advent of radiotracers, however, ecologists were able to do the converse: bring the lab to the field. One consequence of this shift in practice was that ecologists were no longer as beholden to such practices of place since radiotracers provided the kind of physical practice that the

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[56] Bocking, *Ecologists and Environmental Politics: A History of Contemporary Ecology*; Joel B. Hagen, *An Entangled Bank: The Origins of Ecosystem Ecology* (New Brunswick: Rutgers University Press, 1992); Chunglin Kwa, "Radiation Ecology, Systems Ecology and the Management of the Environment," in *Science and Nature: Essays in the History of the Environmental Sciences*, ed. Michael Shortland (Stanford in the Vale: British Society for the History of Science, 1993). See also Robert P. McIntosh, *The Background of Ecology: Concept and Theory* (London: Cambridge University Press, 1985); Peter J. Taylor, "Technocratic Optimism, H.T. Odum, and the Partial Transformation of Ecological Metaphor after World War II," *Journal of the History of Biology* 21, no. 2 (1988).

[57] That is, these were "practices of place" in the sense of Kohler. Kohler, *Landscapes and Labscapes: Exploring the Lab-Field Border in Biology*.

search for bounded ecological spaces were originally designed to convey. Yet, more importantly, because radiotracers made it possible to track the flow of energy—the foundation of the ecosystems idea—it was now possible, theoretically at least, to define and thus work at various "ecosystemic" levels: species interactions, the region, or indeed the entire biosphere. This was the idea that Francis C. Evans put forward when he wrote in 1956 that:

> Each population can be regarded as an entity in its own right, interacting with its environment (which may include other organisms as well as physical features of the habitat) to form a system of lower rank that likewise involves the distribution of matter and energy. In turn, each individual animal or plant, together with its particular microenvironment, constitutes a system of still lower rank. Or we may wish to take a world view of life and look upon the biosphere as a gigantic ecosystem. Regardless of the level on which life is examined, the ecosystem concept can appropriately be applied…All ranks of ecosystems are open systems, not closed ones. Energy and matter continually escape from them in the course of of the processes of life, and they must be replaced if the system is to continue to function."[58]

I am not arguing that rad tools de*placed* ecology. Nor do I contend that after the fallout controversy ecology became concerned solely with global phenomena. Rather, I am suggesting that radiotracers proved a critical tool for ecologists to adapt to the complexities of natural environments (specifically issues of spatial complexity) in ways that had been previously foreclosed to them. By exploring the development of radiotracer methodologies during the era of nuclear weapons testing, this dissertation, then, provides some insight into these early spatial changes in ecological practice when the discipline was attempting to transform itself into a Big Science with such projects as the International Biological Program.

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[58] Evans 1127.

If military efforts to map the globe and the arms race played an important role in the formation of global environmental consciousness, it should come as little surprise that one of the unintended consequences of the biospheric concept was the growing sense that the Earth was under threat not only from Communism, but from industrial production as well. Popularly, this is best reflected in *Silent Spring*, as I have discussed above. Yet, similar bifurcations were happening among environmental scientists as well. Ronald Doel, for example, has argued that "by the 1960s, two distinct 'environmental sciences' had emerged: one biology-centered, focused on problems in ecology and populations studies, and funded in part by agencies and managers concerned about human threats to the environment; the other geophysics-centered, focused on the physical environment, and responsive to the operational needs of the military services that supported it."[59] While this dissertation renders such distinctions problematic (meteorologist Lester Machta, as well as physical oceanographers, for instance, played a key role in the dissenting from the AEC), recent scholarship has born out his argument that institutional contexts have played a key role in how scientists "see" global dangers.[60] In building on Doel's thesis, Joseph Masco has argued that "within these two sciences were also two concepts of planetary threat, one focused increasingly on issues of global environment and the cumulative effects of industrial civilization, while the other continued to focus on how nature could be militarized for the benefit of the US national security state."[61]

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[59] 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): 563.
[60] Dorries, "The Politics of the Atmospheric Sciences: 'Nuclear Winter' and Global Climate Change."; Jasanoff and Martello, eds., *Earthly Politics: Local and Global in Environmental Governance*; Masco, "Bad Weather: On Planetary Crisis."
[61] Masco, "Bad Weather: On Planetary Crisis," 17.

Furthermore, according to Masco, the best example of the link between global national security and global environmental concerns, as well as the seed of their eventual division, can be found in the Limited Test Ban Treaty—"the first nuclear arms control treaty and the first international environmental protection treaty."[62] I agree. Although my dissertation does not extend its analysis to the late 1960s and early 1970s when these competing visions of global political and environmental threat would materialize more fully, by connecting the growth and importance of the environmental sciences to the evaluation of fallout effects it provides needed context and history for how new ideas about the environment emerged within the Cold War and how that knowledge subsequently shaped the Limited Test Ban Treaty. In this way, this dissertation argues that the fallout controversy helps explain both the growth of global environmental consciousness popularly and among environmental scientists.

Finally, the fallout controversy also proved a critical period that enabled environmental scientists to enhance their status as experts and garner political power. The connection between fallout studies and the growing prestige and importance of the environmental sciences is not a new topic. Jacob Hamblin and Stephen Bocking, for instance, have explored the ways in which oceanographers and ecologists attempted to wrest away power from health physicists in order to, as Hamblin puts it, "assert a place at the nuclear table."[63] Similarly, and critically for the purposes of this dissertation, Ronald Rainger has described how the development of radioactive tools conferred authority upon

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[62] Ibid.

[63] Stephen Bocking, "Ecosystems, Ecologists, and the Atom: Environmental Research at Oak Ridge National Laboratory," *Journal of the History of Biology* 28, no. 1 (1995); Hamblin, *Poison in the Well: Radioactive Waste in the Oceans at the Dawn of the Nuclear Age*, 6.

oceanographers by providing them with a powerful means by which to address problems of environmental radiation. This dissertation, in line with this research, argues that radiation proved to be a critical actor in organizing environmental science practices, research lines, and helped environmental scientists working in the field achieve a level of expertise and authority on par with laboratory science. Yet, I also extend this research by not only including meteorology in the mix, but also by exploring how environmental science transformed how the AEC conceived of risk and how their new practices, in a sense, subverted the dominance of the laboratory within the Commission and in western culture more generally.

A Chapter Roadmap in Space and Time

Because this dissertation treats radiation as the central actor in the history of the fallout controversy, I have organized its chapters in accordance with Bruno Latour's methodological dictum to "follow the actors." As such, it is organized spatially and broadly chronological. It begins in the laboratories and factories where radiation was discovered and industrialized and proceeds to follow it as it travelled as nuclear fallout throughout the atmosphere, biosphere and finally into human bodies.

Chapter 1, "A Laboratory Prologue," explores the cultural and scientific contexts in which radiation was discovered and industrialized. I argue that the ethos of technological enthusiasm and the primacy of the toxicological laboratory for delineating the health effects of radiation in the first half of the twentieth century shaped the way that practitioners in health physics in the coming years would conceive of fallout risks by rendering the environment as a passive force in the mediation of human health. In Chapter 2, "In the Factories of the Bomb," I continue this line of reasoning to the Manhattan Engineering District where and when the first nuclear bomb was built and field tested. I argue that health physicists charged with radiation safety during the MED approached the Trinity Test Site as if it were a de facto factory and in the process created a model of fallout protection that assumed that bodies and environments were separate and that, like a factory, fallout would be contained within the boundaries of the test site. Chapter 3, "The Greatest Laboratory Experiment in History," further continues this argument by exploring how the U.S. Navy in the first postwar weapons test, Operation Crossroads, conceived of Bikini Atoll as a laboratory for the testing of nuclear weapons. I also discuss the beginning of scientific work in the environmental sciences associated with nuclear fallout. Chapters 4-6 ("The Atomic Age and the Age of Ecology," "Following the Bravo Footprint," and "Bounding the Atmosphere"), individually focus on ecology, oceanography, and meteorology and explores the growth of these sciences in relation to fallout and how their attention to the environmental mechanisms of fallout exposure changed the way fallout risks were conceived by illuminating the holistic relationships between human bodies, the environment, and geographical space. I also discuss how radioactive tracers played a major role in much of this work and how, as result, new ideas about the biosphere emerged. Finally, in chapter 7, "A Radioactive Democracy," I discuss the work of the Western Montana Scientists Committee on Radiation Information as a case study to explore the growing importance of the science information movement for illuminating ecological fallout hazards and the ways in which scientific groups outside the AEC formed a critical dissenting voice in the fight against nuclear fallout.

A LABORATORY PROLOGUE: HEALTH AND RADIATION PROTECTION IN THE FIRST HALF OF THE TWENTIETH CENTURY

In the first decades of the twentieth century, the United States underwent a dramatic technological and environmental transformation. From rapid industrialization across the continent, growing mass consumption, and urbanization, the United States was developing into a massive technologically advanced nation. Where Americans had once agonized over the "machine in the garden" in the nineteenth century, they were in these triumphant decades celebrating a new nature and a new America founded on technological progress.[1] Scientific advancement too accompanied this transformation. By the late 1930s, American universities were benefiting from the mass emigration of leading scientists fleeing the specter of German National Socialism. Moreover, the professionalization of medical practice and development of related laboratory-based medical fields like bacteriology promised to rid the world of diseases that had plagued humans since time immemorial. Such progress was not unalloyed, however. Industrialization brought with it wanton destruction of whole landscapes, dirtier air and water, and toxic workplaces.

In this chapter, I situate the discovery of radiation and institution of radiation protection standards within this expansive period to set the foundation for how scientists within the Manhattan Project approached the problem of environmental radiation exposure from the Trinity nuclear weapon test in 1945. I focus in particular on the establishment of the toxicological laboratory as the authoritative site for making the

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[1] See, for example, David E. Nye, *America as Second Creation: Technology and Narratives of New Beginnings* (Cambridge: The MIT Press, 2003).

chemical hazards of the factory legible. Borrowing from recent scholarship on bodies and health in environmental history, I argue that toxicological assumptions about the separateness of human bodies and environments blinded toxicologists to the complex ways that the environment mediated chemical exposures. In this chapter, however, I add a spatial element to this analysis. The power of toxicology rested not only on its practitioner's extraordinary ability to isolate and define chemical effects in the laboratory, I argue, but equally on their capacity to manage those effects outside of the carefully controlled environment of the lab. To ensure safety required closely approximating the conditions of the factory to those of the laboratory. Moving from the lab to the factory, I show, was accomplished with the help of industrial engineers who were able to create technological devices to "sanitize" the workplace. In the main, this effort at control worked remarkably well. Yet as I allude to in at the end of this chapter, this confidence in environmental control inside of the factory fostered an ethos that environments beyond the workplace could be similarly controlled. The development of radiation protection standards was a driver and product of this toxicological approach to human health.

The Discovery and Industrialization of Radiation: Technological Enthusiasm

On November 8, 1895, German professor Wilhelm Roentgen was sitting in his darkened laboratory at the University of Wurzburg experimenting with tubes.[2] Vacuum tubes had been something of a scientific wonder in the nineteenth century owing to their curious fluorescence when filled with gas and given an electrical charge. The “cathode

[2] This account is largely drawn from Lawrence Badash, "Marie Curie: In the Laboratory and on the Battlefield," *Physics Today* 52 (2003); Caufield, *Multiple Exposures: Chronicles of the Radiation Age*.

ray," as the fluorescence in the tubes was termed, fascinated Roentgen and prompted him (and a number of other scientists) to delve into its mysterious properties. While experimenting with the cathode tube on the 8th, he fortuitously stumbled on a particularly interesting phenomenon. Having covered the tube with a lightproof black paper as he applied the electrical current, Roentgen noticed a glow emanating from a nearby table. Quickly lighting a match, he realized that the glow was coming from a screen coated with barium platino-cyanide. Curious, Roentgen waved his hand between the darkened tube and the glowing screen. Observing not only that the glowing dimmed, he could also see a projection of the bones in his hand.[3] The luminescence, he thought, must have been the product of some invisible ray emanating from the tube and penetrating its lightproof covering. Not quite sure of what this new penetrating ray was he settled on calling it, appropriately enough, the "X"-ray. Following a few more weeks of further experimentation, he published his findings after the first of the year in an article that included an X-ray photograph of his wife's hand that strikingly pictured the outline of her bones and wedding ring.[4]

Roentgen's announcement of the X-ray prompted a wave of excitement within scientific circles and the public. One scientist who had been particularly intrigued by this new development was French scientist Henri Becquerel. Becquerel, upon hearing Roentgen's discovery, wondered if luminescent materials—materials that emit light when exposed to the sun—might also emit X-rays.[5] To test this idea, he placed various

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[3] Roentgen seeing the bones in his hand is noted in Richard Rhodes, *The Making of the Atomic Bomb* (New York: Simon & Schuster, 1988), 41.
[4] W.C. Roentgen, "On a New Kind of Rays," *Nature* 1369, no. 53 (1896).
[5] Lawrence Badash, "The Discovery of Radiation," *Physics Today* 49 (1996).

luminescent substances on a photographic plate sealed with lightproof paper and exposed them to the sun. Hoping that when the plates were developed there might be some evidence of X-ray exposure, his experiments failed him until he tried uranium salts. Upon developing the plate, Becquerel noticed that the uranium salts did indeed show faint ray emission. Becquerel had, however, mistakenly assumed that the uranium had emitted X-rays just as had Roentgen's tube. What happened next is one of the most fascinating examples of serendipity in the history of science. Wishing to repeat the experiment, Becquerel set up his plate again but upon seeing the gray clouds of Paris outside his window decided to put it in his desk drawer and wait for a sunnier day. Coming back to the plate a few days later, Becquerel decided to go ahead and develop it at any rate to see if anything might have happened to the film while in the dark. To his astonishment, the developed plate clearly pictured the outline of the uranium salts. Becquerel concluded that this was sufficient evidence that the uranium was emanating a new kind of penetrating energy. Whether the uranium was producing the energy by itself or absorbing it from some atmospheric source, Becquerel did not know. Although he didn't call it as such, Becquerel had discovered radiation. He published his findings in the middle of March 1896, just months after Roentgen announced his discovery of X-rays.

Although Roentgen's X-rays and the eerie images they produced of the human body certainly captured the largest share of public (and medical) fascination with these new discoveries, Becquerel's findings set off a chain of events that would help to thrust the study of radioactivity to the forefront of physics research. Still, a full decade would pass before the study of radiation would be taken seriously as a research project in its own right. As historian of science Lawrence Badash has noted, once the novelty of radiation had worn off, it became “more like a dead horse; there it was, but no one knew what to do with it.”[6] By 1898, for example, even Becquerel had largely ceased working on problems associated with this new ray he had discovered; he had, in fact, run out of ideas.[7] The revolutionary implications that radiation would pose to classical physics did not become apparent until scientists like the Curies in France, and more significantly, Ernest Rutherford in Canada and Great Britain, could provide some explanation for its underlying causes.

Rutherford began studying uranium, and another newly discovered radioactive material, thorium, the same year that Becquerel published his last paper on the subject in 1898.[8] Finding that the emissions of these substances exhibited varying penetrative properties, Rutherford concluded that there were at least two types of radiation present in these materials: the less penetrating alpha radiation and the more penetrating beta radiation. Frenchman Paul Villard would add to that list in 1900 by identifying a third, more penetrating radiation—gamma radiation. But Rutherford’s real claim to scientific fame rested on his, and chemist Frederick Soddy’s, explanation that radiation was a product of elemental transmutation; it was the product of atomic disintegration whereby one elemental atom emitted part of itself (radiation) to become a wholly other substance. The immense amount of energy released during this radioactive “decay,” Rutherford and

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[6] L. Badash, *Radioactivity in America: Growth and Decay of a Science* (Baltimore: Johns Hopkins University Press, 1979), 11.
[7] Badash, "The Discovery of Radiation," 25.
[8] Ibid; Lawrence Badash, *Scientists and the Development of Nuclear Weapons: From Fission to the Limited Test Ban Treaty, 1939-1963* (Atlantic Highlands: Humanities Press International, 1995).

Soddy demonstrated, was inherent to the thing itself, not from some external source. By moving from describing the properties of radiation into its mechanisms of energy release, they had completely transformed radioactivity from a curious novelty into a fully fledged research paradigm that allowed physicists to probe into the basic building blocks of the world—the atom.[9]

While Rutherford was just beginning his research, Marie and Pierre Currie had begun their own intensive studies on Becquerel's rays.[10] Marie Curie, in particular, was interested in quantifying the effect of Becquerel's rays by means of their capacity to ionize air, rather than merely qualify their presence by photographic detection. Working with pitchblende (uranium oxide ore), Curie detected radiation emissions which were far more active than uranium was known to be. Over the next couple years, Marie spent countless hours isolating this new radioactive element and measuring its radioactivity, a term she coined in 1898. By 1902, after treating eight tons of pitchblende, she had isolated and refined a tenth of a gram of what she would call radium.[11] Even at such an infinitesimal quantity, the radium was so active that it glowed! The next year, the Curies and Becquerel shared the Nobel Prize in Physics for their discoveries, garnering considerable acclaim.

Upon their discovery, X-rays and radiation provoked awe and wonder at their marvelous attributes. X-rays produced astonishing images of the human body. Radium glowed without any perceptible cause. Americans at the turn of the century flocked to

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[9] Badash, "The Discovery of Radiation."
[10] Ibid; Badash, "Marie Curie: In the Laboratory and on the Battlefield."
[11] Curie also isolated another new element, Polonium, which she named after Poland, her country of origin. The eight ton figure comes from Edward R Landa, "The First Nuclear Industry," *Scientific American* 247 (1982): 184.

newsstands to read about these miraculous discoveries. But Americans not only devoured the stories of these discoveries as cultural artifacts characteristic of their burgeoning modernity, they quite literally *consumed* them. Within months of Roentgen's announcement, commercially produced X-ray devices were already widely available on the market. The inventor Thomas Edison, who had also been working with cathode tubes, produced and patented the first fluoroscope which he introduced to Americans at an electrical exposition in New York City in May of 1896. Functioning quite similar to Roentgen's initial experimental design, Edison's fluoroscope consisted of an X-ray tube positioned opposite a screen covered with barium platino-cyanide. Visitors to the exposition were encouraged to place various body parts between the tube and the screen to witness their underlying structure and movement.[12] Medical diagnosis, however, was the most obvious application of X-rays. Doctors and hospitals almost immediately began to employ crude X-ray devices to diagnose broken bones and other injuries. In 1896, physicians at McGill University in Montreal, for example, subjected a man to a 45 minute X-ray in order to remove a bullet from his leg.[13] By World War I, X-rays had so transformed diagnostic practices that they were widely available for use in the battlefield.

Given the difficulties that Marie Curie encountered with the extraction process for radium, it is no small wonder that it took years before the radioactive element could be refined to quantities large enough to be incorporated into industrial processes and mass produced. Indeed, according to historian of science Claudia Clark, between 1914 and 1921 the market price for a single gram of radium demanded anywhere from $89,000 to

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[12] Jack Schubert and Ralph E Lapp, *Radiation: What It Is and How It Affects You* (New York: Viking Press, 1957), 13-14.
[13] Caufield, *Multiple Exposures: Chronicles of the Radiation Age*, 8.

$125,000.[14] Nevertheless, as new extraction methods were developed in the 1920s, radium was introduced into the market in greater quantities.

Although X-rays were also promoted for their supposed curative power, Radium was perceived by many as a superlative cure-all for a host of maladies. From tonics to vaginal jellies, radium-laced products were introduced and promoted to consumers throughout the 1920s and 1930s by charlatans and medical physicians alike. *Radithor*, a radium tonic developed by a swindler named William J.A. Bailey, was advertised as a "Fountain of Youth."[15] *Radithor* was prescribed widely by doctors and promised not only youthful rejuvenation, but could also repair failing gonads so that couples might have "the courage and power to carry through."[16] Even Frederick Soddy believed that radium gases (more properly radon, a daughter product of radium) might provide a panacea for tuberculosis sufferers. The construction of "inhalatorias" and "emanatorias" to treat the disease soon followed Soddy's suggestion.[17] Food production too was not immune from this kind of nostrum. At least one chicken farmer recommended that radium be mixed with chicken feed to produce self-cooked eggs. Radium-laced fertilizers were also marketed toward similar fantastic ends.[18]

But aside from its more fringe applications, many physicians became interested in radium as a powerful internal medicine. Early handlers of radium, most notably the Curies, had personally witnessed the destructive power to bodily tissues that prolonged

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[14] Claudia Clark, *Radium Girls: Women and Industrial Health Reform, 1910-1935* (Chapel Hill: The University of North Carolina Press, 1997), 47.
[15] Ibid., 172-73.
[16] Ibid.
[17] Badash, *Radioactivity in America: Growth and Decay of a Science*, 131.
[18] Ronald L. Kathren, *Radioactivity in the Environment: Sources, Distribution, and Surveillance* (Chur: Harwood Academic Publishers, 1984), 17.

exposure to radium wrought. While physicians assumed that at low doses radium produced hormetic (or potentially beneficial) effects on the body, they also quickly realized that at elevated and concentrated doses, the high-energy radiation produced by the element might prove useful in destroying tumors and cancerous cells. Even as early as 1916, doctors at Memorial Hospital in New York had treated 424 malignant tumors with success in at least 120 cases.[19] Still, as radium treatment required substantial amounts of the quite scarce and expensive element, large-scale application of radiation in cancer research would have to wait until radioisotope-producing cyclotrons were developed in the 1930s.

By far the greatest application of radium in the early years of the twentieth century was in the luminous paint industry. Non-radioactive luminescent paints had been marketed for a number of years in both America and Europe.[20] As most of these paints required prior exposure to light in order to create phosphorescence, radium promised continual illumination irrespective of an external energy source. Moreover, despite meager supplies, its intensity of energy emission meant that radium could be diluted to minute amounts in paint and still luminesce. World War I provided the primary impetus for the development of the radium paint industry as luminous watches, gunsights, and aircraft instruments were in demand by soldiers fighting in the dark battlefields of Europe. Prior to American involvement in the Great War, radium supplies appeared to have been divided equally between medical uses and paint. But by the end of the war, roughly ninety-five percent went into the dial painting industry and in subsequent years

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[19] Caufield, *Multiple Exposures: Chronicles of the Radiation Age*, 27.

[20] Uranium was a relatively common constituent in pain and clays because gave off a yellowish hue. It didn’t luminesce and no one realized it was radioactive.

demand remained constant. By 1920, over four million watch and other clock dials had been painted with radium.[21]

The widespread application of radium and radium products to consumers was not, of course, without consequence for peoples' health. Marie Curie suffered badly scarred and inflamed fingers from handling refined radium and died in 1934 at the age of sixty-six from leukemia, no doubt as a result from her prolonged exposure. E.M. Byers, a wealthy Pittsburg industrialist and well-known dandy, died in 1932 from radium poising arising from his daily *Radithor* drinking habit. There were in fact early suspicions among the medical community (and popularly as well) that radium might prove hazardous, but these warnings were overshadowed by the element's putative therapeutic value. The widespread realization of radium's toxicity emerged in the 1920s not as a public health issue arising from radium-laced consumer products, ironically, but from the growing occupational diseases plaguing radium dial painters. Given the rise of the industrial manufacturing during the period—of which newly created chemicals formed the backbone—it should come as no surprise that new toxic illnesses should first materialize among workers who were exposed daily to various chemicals on factory floors.

The discovery and eventual recognition of radium's toxicity, of which I will have more to say in the next section, offers a classic example of how Americans grappled with modernity in the early decades of the twentieth century. As an object that was at once cultural and material, radium occupied a liminal space that mediated between these spheres; it reflected Americans' hopes and desires for technological progress, but also carried grave consequences for bodies harboring the radioactive element. Historian of

[21] Badash, *Radioactivity in America: Growth and Decay of a Science*, 148.

technology Thomas Hughes has described the period from roughly the 1870s to the end of World War Two as the era of "technological enthusiasm."[22] Americans no longer looked to America as frontier, where wildness and unspoiled nature abounded. By 1900, Hughes has written, Americans had "reached the promised land of the technological world, the world as artifact."[23] This was America newly constituted—a second creation: No longer Nature's nation, America had become Technology's nation.[24] This shift was reflected not only in a changing landscape where skyscrapers, dams, canals, factories, and railroads dotted what had once been forests and open fields, but also penetrated deep into the American psyche. Americans had become modern, and to become modern meant, among other things, having a deep confidence in technical control.

But the increasing technical control that Americans brought to bear on the material world produced all manner of unintended consequences. One of those consequences was disease. And given the technological enthusiasm of the age, Americans not unexpectedly turned to a new breed of technocratic and scientific elite to identify, treat, and prevent the diseases that plagued the nation. Bacteriologists and sanitation engineers were the first to apply modern laboratory and engineering practices to disease control. But in the face of the growing reliance on industrial chemicals and the diseases that they wrought, Americans looked to industrial hygienists and toxicologists to cure them of their ills. These experts would first learn the tools of their trade in factories. Their experiences and the practices developed in the workplace would convince them that

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[22] Thomas P Hughes, *American Genesis: A Century of Invention and Technological Enthusiasm, 1870-1970* (New York: Penguin Books, 1990).
[23] Ibid., 1.
[24] See also Nye, *America as Second Creation: Technology and Narratives of New Beginnings*.

disease-control was indeed possible, which, in turn, gave them the confidence that similar techniques might also be employed in more complex, yet still quite amenable, environments outside the factory. It is this rise of industrial models of health that we turn to next.

The Origin of Radiation Protection Standards: From Factory to Toxicology Laboratory, or There and Back Again

At the turn of the twentieth century, Americans found themselves in rather peculiar circumstances. With the supposed closing of the "frontier" in 1890 and the rise of industrial manufacturing and urbanization, Americans seemed to be on the cusp, if not fully immersed, in modernity. This was a break from the past, the contours of which required reexamination and redefinition. One need not look further than the enormously popular world's fairs of the period to find evidence that Americans were redefining not only what it meant to be American, but what it meant to be modern—a difference that would become increasingly difficult to tease apart. According to historian Robert Rydell, between 1876 and 1916 over 100 million Americans flocked to World's Fairs at St. Louis, Chicago and other Americans cities to witness firsthand the changes being wrought by the material progress of American modernity.[25] Alongside exhibits on electricity and turbines, visitors to the fairs were also exposed to various displays of "traditional" and "primitive" people, a stunning counterposition testifying, to borrow

[25] The statistics are in Robert W. Rydell, *All the World's a Fair: Visions of Empire at American International Expositions, 1876-1916* (Chicago: University of Chicago Press, 1984), 2.

from historian of technology Michael Adas, that machines were indeed the "measure of men."[26]

But if machines were one element that mediated how Americans defined the "Otherness" of less-developed peoples, "dirtiness" might be another. The rise of bacteriological theories of disease revolutionized public health and medicine at the turn of the century.[27] Largely replacing older Hippocratic notions of disease etiology that centered on the effects of climate and landscape on bodily health (miasmas, for example), Americans and medical practitioners alike were readily applying bacteriology to solve a host of health problems from cholera to typhoid.[28] As bacteriology refocused the cause of disease from the environment to specific pathogens identified under the microscope, bodies became the primary site for medical intervention. "Dirty" immigrant bodies within this framework were prime suspects; immigration threatened not only the health of American democracy (the body politic), but the health of American bodies too.[29]

Just as bacteriology was transforming how medical doctors were re-envisioning the place and ultimately the cause of disease, it equally transformed how they prevented the onset of disease. Although the environment was no longer considered a disease-causing agent, it still remained, despite its passivity, a space through which dangerous

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[26] Michael Adas, *Machines as the Measure of Man: Science, Technology, and Ideologies of Western Dominance* (Ithaca: Cornell University Press, 1990).

[27] Paul Starr, *The Social Transformation of American Medicine* (New York: Basic Books, Inc., Publishers, 1982).

[28] Holistic understandings of the body and environment were not completely replaced. See Christopher Lawrence and George Weisz, *Greater Than the Parts: Holism in Biomedicine, 1920-1950* (New York: Oxford University Press, 1998).

[29] On threat of immigration to American Democracy during the period see Matthew Frye Jacobson, *Barbarian Virtues: The United States Encounters Foreign Peoples at Home and Abroad, 1876-1917*, 1st ed. (New York: Hill and Wang, 2000). On the threat of immigrant bodies on health see Mitman, "In Search of Health: Landscape and Disease in American Environmental History."; Nash, *Inescapable Ecologies: A History of Environment, Disease, and Knowledge*.

pathogens might cross to infect the body. Intervention into the environment to prevent disease, then, shifted from balancing the relationship between bodies and the environment to create more "salubrious" conditions, to wholesale "sanitation" of the environment.[30] Such efforts at control, naturally, demanded a more engineering-like approach to the environment and sanitation engineers emerged as the prime movers in ridding it of bacteria. Garbage disposal, street cleaning, and water purification, for example, were thus technical solutions to disease prevention and medical professionals weighted these efforts in equal importance alongside their own interventions into the human body.[31] Coupled with the rise of state power during the progressive era, the science and engineering aspects of bacteriology and sanitation recast the urban landscape and the body in terms of its cleanliness and in the process attenuated the relationship between the two. As historian Gregg Mitman has written, with "expanding industrialization, escalating immigration, and the closing of the frontier, Americans came to regard nature, not as part of themselves, but as a place apart from the decadence and decay of urban landscapes and the human bodies that inhabited them."[32] Nevertheless, despite the myopia that resulted from bacteriology, it proved immensely useful for preventing disease as the increase in life expectancy during the period demonstrates.

Ironically, despite the success of bacteriological theories of disease, protection of laborers from chemicals in factories was surprisingly long in coming. The first state-

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[30] On nineteenth century approaches to health see Valencius, *The Health of the Country: How American Settlers Understood Themselves and Their Land*.
[31] Martin V. Melosi, *Garbage in the Cities: Refuse Reform and the Environment* (Pittsburgh: University of Pittsburgh Press, 2004); Martin V. Melosi, *The Sanitary City: Urban Infrastructure in America from Colonial Times to the Present* (Baltimore: Johns Hopkins University Press, 2000).
[32] Mitman, "In Search of Health: Landscape and Disease in American Environmental History," 195.

sponsored progressive reforms into workplace hazards in the first two decades of the twentieth century were largely geared toward industrial "accidents." The infamous Triangle Shirtwaist Fire of 1911, where 143 women were burned to death or died from jumping out of the factory windows to escape the conflagration, was perhaps the most gruesome of these accidents. Such shocking events prompted many progressive reformers to urge for a greater governmental role in protecting workers from unsafe working conditions and as a result many laws were enacted toward that end. But protecting workers from newly created chemicals in industrial processes remained largely absent from these early reforms. Part of the problem stemmed from the fact that chemicals often required months or even years before their apparent effects on worker health could be perceived. Nonetheless, corporate unwillingness to investigate worker claims that might affect their bottom line by opening them up to litigation equally explains this lacuna.

Not surprisingly, then, it was labor groups and progressive reformers that were the first to seriously consider the role of factory work in causing worker diseases.[33] Owing to their close association and sympathies for the plight of workers, groups like the American Association for Labor Legislation and Hull House organized their own studies and conferences to publicize worker complaints. One of the first issues to arise was a condition known as "phossy jaw" (jaw necrosis) among workers employed in factories manufacturing phosphorus matches. Lawmakers eventually taxed phosphorus so heavily that manufacturers turned to other chemicals to make matches in 1912, thereby

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[33] This account is largely drawn from Sellers, *Hazards of the Job: From Industrial Disease to Environmental Health Science*.

eliminating the problem.[34] But for other emerging diseases arising from chemical exposures from substances like lead (and radium as we shall see) were not so easily disposed of. Lead, for example, was a widely used chemical in a number of industrial processes and its elimination from paint and other products (without a sufficient alternative) would have meant a serious curtailment of needed industrial commodities; regulating lead out of existence was simply not an option for most Americans. Such reliance on chemicals highlighted the central tension between worker health and corporate profits. In an era where labor strife was perhaps the dominant political issue of the day, questions about how to define and thus regulate an occupational disease was of chief importance as the ontological status attributed to a disease frequently fell along labor-managerial lines. It was within this larger context of labor-management politics that the scientific discipline of toxicology emerged as the preeminent approach to the study of occupational illnesses.

Toxicology was a fledgling discipline in the 1920s and 1930s and its professionalization hinged on its practitioners' ability to successfully position themselves between labor and management. By exhorting their putative disinterestedness in political matters and laying claim to special knowledge about the nature of industrial illnesses, toxicologists found a fruitful environment in which to secure their authority over determining industrial hazards while simultaneously ensuring steady sources of funding. But such an outcome was no easy matter. Academic programs in toxicology throughout the U.S. in institutions like Harvard were beholden to corporate capital to finance their studies and questions regarding the manner in which toxicologists could maintain their

[34] Ibid., 66.

autonomy in the face of this corporate patronage were paramount. Ultimately, a gentleman's agreement was struck. Toxicologists would receive funding without strings attached and industry would have to abide by their findings whether showing harm or not. Corporations in return, as Christopher Sellers has written, could rely on the underlying message of toxicology: "industrial chemicals need not be suspected or feared… their bodily effects were coming within the realm of the knowable and controllable."[35] As toxicologists increasingly dominated the field of occupational health, their research might limit or restrain how corporations used chemicals, but in return industry could continue to employ them so long as their use was grounded in an objective and disinterested science. The question of banning certain chemicals was never broached.

But toxicologists' claim to expertise and objectivity in matters of occupational health relied on more than rhetorical devices. The authority of their claims rested on new practices they developed in the laboratory. A common misperception in popular understandings of scientific practice has assumed that laboratories have always been the premier and preferred site of scientific knowledge production. Yet in the early part of the twentieth century, the establishment of laboratory practices and cultures had hardly gained the kind of epistemological capital that we ascribe to them today.[36] The first efforts at describing, identifying, and protecting workers from occupational diseases were largely based on factory shop surveys and anecdotal evidence from workers, and labor advocates and progressive-minded reformers, not scientists, performed much of that

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[35] Ibid., 183.
[36] Kohler, *Landscapes and Labscapes: Exploring the Lab-Field Border in Biology*.

work. Thus the move toward the laboratory to answer questions about the nature of industrial toxins was hardly inevitable. Nevertheless, the growing cachet of laboratory practices among scientists coupled with the remarkable success of bacteriology, pointed to a potentially productive avenue of research through which to address the problems of the factory.[37]

On the one hand, moving the site of research from the factory floor to the toxicological lab, offered toxicologists a safe space through which to work. Sequestered from the potentially prying eyes of corporate managers, toxicologists felt that the laboratory provided a space through which to pursue research questions unhindered. On the other hand, the laboratory offered unique opportunities to isolate and control the chemicals under study by toxicologists outside of the messy contingencies of the factory floor. The beauty of the lab, as historian of science Robert Kohler has argued, rests on its inherent placelessness.[38] In the field, scientists must deal with variability—place matters. In the lab, however, the environment is stripped of all locality. Deprived of "white noise," labs environments allow scientists to control single chemicals to such an extent that their effects can be studied in near total isolation. Laboratories as placeless places then gain credibility precisely by their exceptional facility in keeping nature out.[39]

With the environment kept at bay, toxicologists focused much of their attention on the internal bodily environment. Invariably, toxicologists understood pathology as a function of chemical imbalances within the body. Healthy bodies, they thought, were in

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[37] On the influence of bacteriology on toxicology see Nash, *Inescapable Ecologies: A History of Environment, Disease, and Knowledge*.
[38] Kohler, *Landscapes and Labscapes: Exploring the Lab-Field Border in Biology*; Kohler, "Place and Practice in Field Biology."
[39] Ibid.

a state of homeostasis; bodies ravaged by external chemicals were in disequilibrium.[40] The trick for toxicologists was in identifying the physiological mechanisms by which chemical influences outside the body disrupted the metabolic regulation of chemicals inside a "normal" body. By framing the "normal" and the "pathological" in terms of regulation, toxicologists, not surprisingly, set out to determine the concentration levels at which toxic chemicals effected imbalances within the body. In the laboratory, the measurement and determination of chemical effects was greatly enhanced by virtue of the controlled conditions which allowed toxicologists to isolate and dose specific toxins. Atmospheric exposure chambers and direct intravenous injection were the typical tools employed toward that end depending on the suspected environmental route of entry (i.e. inhalation or ingestion).[41] In the exposure chamber, for example, toxicologists placed model organisms like rats or dogs in a sealed box and introduced specific chemicals at controlled concentrations to measure the effect on the animals. These practices resulted in dose-response curves for specific toxins and formed the foundation for the establishment of exposure standards which could be generalized and applied across various industries to protect workers. As a result, the "maximum concentration level" or the "threshold limit value" became the prime scientific product produced in the labs. The notion of a threshold value was based on a long-held assumption that the "dose makes the poison." Chemical exposures below a threshold level supposed that such levels were safe, or at the very least, demonstrated the absence of harm. Once set, threshold values

[40] Mitman, "In Search of Health: Landscape and Disease in American Environmental History."; Sellers, *Hazards of the Job: From Industrial Disease to Environmental Health Science*.
[41] For an excellent discussion of the air chamber see Murphy, *Sick Building Syndrome and the Problem of Uncertainty: Environmental Politics, Technoscience and Women Workers*; Sellers, *Hazards of the Job: From Industrial Disease to Environmental Health Science*.

formed the basis for protection standards in the factory. By 1945, in one historical estimate, toxicologists had established threshold values for over 130 chemicals.[42]

But the move back to the factory to ensure adequate protection for workers was fraught with difficulties. In the factory, toxicologists had little formal jurisdiction; suggestions for limiting occupational exposures were just that, mere suggestions. But more importantly, even in cases where toxicologists found industries willing to enact changes in industrial practices, when toxicologists moved back to the factory they encountered head-on the variety of local particularities that they had worked so hard in the laboratory to control. How did toxicologists extrapolate the knowledge produced in tightly controlled laboratories to the messy complexities of the workshop floor? Engineering and sanitation provided the answer.

In the external environment, toxicologists were just as influenced by concepts of balance and equilibrium as they were in their determination of chemical effects in the body. And in the same way, they interpreted the relationship between the body and the factory environment in physiological and chemical terms; health was a function of balancing the interactions between chemicals in the workplace with chemicals in the body.[43] Engineering and sanitation offered solutions to balancing those interactions by controlling the pathways through which chemicals might enter the body. Toxicologists were particularly concerned with controlling two routes of exposure in particular: inhalation and ingestion. Given the threshold values determined in the laboratory, toxicologists were not interested in completely sanitizing the work environment. Within

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[42] Nash, *Inescapable Ecologies: A History of Environment, Disease, and Knowledge*, 142.
[43] Sellers, *Hazards of the Job: From Industrial Disease to Environmental Health Science*, esp. 175.

the ecology of the workplace, the best they could hope for was limiting exposures to levels below what they considered harmful thereby balancing the body and environment. Proper ventilation was a chief consideration in preventing inhalation of toxic chemicals. Ventilation hoods for individual work stations and centralized air conditioning units for the larger factory became standard instruments for cleaning the workplace atmosphere throughout the 1930s and 1940s. Limiting the ingestion of toxic chemicals, however, required considerable collaboration with workers as much as it demanded a technological fix. Routine cleaning of work stations was a typical requirement, but so too were work rules prohibiting workers from eating or smoking without first thoroughly washing their hands. Toxicology, then, was at least in part geared toward controlling workers as much as it was the factory environment.[44]

In the end, the alliance of laboratory science with industrial engineering, allowed toxicologists to more closely approximate the conditions of the factory to those of the laboratory.[45] And in the main, this toxicological approach to occupational disease worked remarkably well. The toxicological practices developed in the lab supplied the means through which to make hazardous chemicals in the workplace perceptible and the engineering of the workplace enabled toxicologists to control, and thus prevent, over-exposures to chemicals that might induce the kinds of diseases that toxicologists identified in the laboratory. With its blend of laboratory and engineering practices, the

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[44] For more on the ways that the rise of expertise in occupational medicine led to greater control of workers see the collections in David Rosner and Gerald Markowitz, eds., *Dying for Work: Worker's Safety and Health in Twentieth-Century America* (Bloomington: Indiana University Press, 1989).

[45] See especially Sellers, "Factory as Environment: Industrial Hygiene, Professional Collaboration and the Modern Sciences of Pollution."

factory constituted a border zone that mixed the pure and applied.[46] Steeped in quantifiable and standardized units, threshold values provided industries with sound footing on which to base their protection protocol. But without the kind of technical know-how that engineering offered, it is hard to imagine a scenario where toxicologists could have performed the scaling-up required to turn the factory into a laboratory.[47]

To be sure, the toxicological approach to occupational illnesses greatly improved working conditions in the factories. The development of quantifiable standards for exposure limits helped to more clearly define the nature of industrial health hazards. Yet by garnering the authority to define occupational diseases, toxicologists as a matter of course contributed to the growing decline of labor power.[48] Toxicologists had indeed established themselves as the arbiters of what counted as a hazard. But in the process of creating an exclusive knowledge that only they had special competence for, their expertise dramatically undercut laborers' personal bodily knowledge. Put simply, a disease was only a disease if toxicologists could identify the chemical culprit; subjective labor knowledge did not count.[49]

The origin of radiation protection standards was both a legacy and a spur for toxicology. X-ray standards were the first protection guidelines established and the practices developed by radiographers in the first decades of the twentieth century would

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[46] Kohler, *Landscapes and Labscapes: Exploring the Lab-Field Border in Biology*.
[47] Although he does not use this precise language, Bruno Latour nicely describes this scaling-up in Latour, "Give Me a Laboratory and I Will Raise the World."
[48] Again see Rosner and Markowitz, eds., *Dying for Work: Worker's Safety and Health in Twentieth-Century America*.
[49] This point is nicely made by Nash in her discussion of farm workers in Linda Nash, "The Fruits of Ill-Health: Pesticides and Workers' Bodies in Post-World War II California," *Osiris* 19 (2004). It was also of no small coincidence that the scientization of occupational hazards occurred alongside the growing importance of scientific management of the factory floor.

influence the thinking of toxicologists in the ensuing years. The suspicion of internal radium poisoning, however, did not emerge until the late 1920s and into the 1930s and only after toxicologists identified the mechanisms by which it damaged human tissues did radium poisoning become "real."

The potentially hazardous effects of protracted x-rays exposure were apparent almost immediately. Thomas Edison, who as I described earlier had invented the fluoroscope just months after Roentgen's discovery, suffered a series of skin rashes and eye sores resulting from his experimentation with x-ray tubes. It was his laboratory assistant, Clarence Dally, however, who more clearly bore the evidence of x-ray damage. Dally repeatedly exposed himself to Edison's fluoroscope and suffered severe burns and eventually died from his injuries in 1905, prompting Edison to wisely abandon his x-ray studies.[50] As Edison and Dally's experience suggests, the most obvious effect of x-ray exposure were skin burns and innumerable x-ray technicians were permanently impaired or died from prolonged exposure in these early years.[51] X-ray protection was hampered initially by the almost total lack of quantitative knowledge about radiation measurements.[52] Unstandardized biological indicators like hair loss (epilation) and reddening of the skin (erythema) were the primary means by which most radiologists detected overexposure.

Arthur Mutscheller, a physicist for an x-ray manufacturer, proposed the first semi-quantifiable dose-effect standard for x-rays in 1924. Mutscheller was mainly interested in establishing the amount of shielding required to adequately protect x-ray

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[50] Schubert and Lapp, *Radiation: What It Is and How It Affects You*, 14.
[51] This was especially true following the growing use of x-ray diagnosis during World War I.
[52] Lauriston S. Taylor, *Radiation Protection Standards* (Cleveland: CRC Press, 1971), 11.

diagnosticians. But before calculating shielding requirements, Mutscheller first had to determine the x-ray intensities (based upon x-ray voltages) necessary to produce a biological response and for that he relied on the most readily observable effect, erythema. Mutscheller based his calculation of the "erythema dose" on observations of physicians and technicians working with x-rays, rather than pure experimentation. Having calculated what he thought were the intensities required to produce erythema, he then set the "tolerance dose" at 1/100 of the erythema dose, a figure he arbitrarily assumed would be safe.[53] As a fraction of the clinically observed erythema dose, the tolerance dose was based upon the assumption that exposures kept below that threshold would produce no biological harm. The tolerance dose was, in its most salient aspects, equivalent to the concept of the threshold values enumerated by the toxicologists discussed above. Flawed though it was (Mutscheller's tolerance dose was based upon the absence of harm, not quantifiable biological changes), Mutscheller's "tolerance dose," as radiologist Lauriston Taylor has written, "is still the basis of our protection standards of today."[54] By 1934, Mutscheller's tolerance dose was adopted and formalized by the Advisory Committee on X-Ray and Radium Protection, a division of the U.S. Bureau of Standards.[55]

While the biological effects of over-exposure to x-rays were readily apparent, the realization of radium toxicity had a decidedly more convoluted and protracted history. To be sure, Marie Curie's rotting fingers provided ample evidence that handling highly

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[53] This was based upon surveys taken of technicians at "good installations." See Hacker, *The Dragon's Tail: Radiation Safety in the Manhattan Project, 1942-1946*, 15. See also Jolly, "Thresholds of Uncertainty: Radiation and Responsibility in the Fallout Controversy".

[54] Quoted in Caufield, *Multiple Exposures: Chronicles of the Radiation Age*, 21.

[55] This same body also adopted and formalized the roentgen (r) as the unit of x-ray measurement. Unlike Mutschller's use of x-ray voltage, the roentgen was a unit of the ability of x-rays to produce ionization within a given amount of air. See Ibid., 18. Taylor, *Radiation Protection Standards*.

concentrated samples of radium could produce grisly biological effects. But the discovery of radium toxicity and the development of protection standards governing its uses required recognition of its internal bodily dangers. The appreciation for internal modes of biological effects was further compounded by the rather long latency period between initial exposure and the onset of disease which served to muddy the clear cause and effect relationship that had so typified x-ray damage.[56] For internalized radium damage, there were no immediate biological indicators like erythema. The discovery of internal radium poisoning was, not unexpectedly, a product of the toxicological lab and the first hints of its bodily effects occurred in their province of expertise, the factory.[57]

The first inkling that radium might prove hazardous appeared shortly after a series of deaths among radium dial painters in the early 1920s.[58] Employing mostly women, the radium industry was centered initially at least in a plant owned by U.S. Radium in West Orange, New Jersey. Between 1922 and 1924, nine workers in the plant died and the coroners who examined their bodies listed a variety of causes from syphilis to necrosis of the jaw.[59] All of the women prior to their death, however, showed similar signs of anemia and jaw rot. Doctors did not suspect radium initially largely because of the therapeutic value that they had ascribed to the substance.[60] Nevertheless, the isolated and

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[56] Moreover, many medical physicians were convinced of radium hormetic effect in the body.
[57] Claudia Clark in her study of radium poisoning among dial painters discusses the problems associated with the notion of "discovery." One main thesis of her book is that the discovery of toxic illnesses owes as much to politics as pure scientific discovery. I agree, but use the term as shorthand for the general process. Clark, *Radium Girls: Women and Industrial Health Reform, 1910-1035.*
[58] I am heavily indebted to Clark and Nugent for this section on the dial painters. Ibid; Angela Nugent, "The Power to Define a New Disease: Epidemiological Politics and Radium Poisoning," in *Dying for Work: Workers' Safety and Health in Twentieth-Century America*, ed. Gerald Markowitz and David Rosner (Bloomington: Indiana University Press, 1987).
[59] Nugent, "The Power to Define a New Disease: Epidemiological Politics and Radium Poisoning," 178.
[60] Ibid.

similar nature of their illnesses (especially the necrosis of the jaw) suggested to labor advocates, at least, that the women's deaths might have been occupational in origin. Katherine Wiley of the New Jersey Consumers' League, for example, was convinced that radium was indeed the culprit and fought steadfastly to win its recognition. Wiley quickly pieced together anecdotal evidence from workers and their personal doctors to publicize the epidemic among dial painters and used this evidence to steadily pressure U.S. Radium to fund and enact an independent toxicological study of the West Orange factory. From the start, the company relied on the uncertain nature of the women's deaths to delay recognition of the hazard. In the face of growing pressure from advocates like Wiley, however, U.S. Radium increasingly feared the potential for legal action if they did not at the very least attempt a study. In 1924, they did just that by hiring the toxicology team at Harvard.[61]

The Harvard group's study of the factory conditions and of the health of the workers provided rather stark clinical evidence that radium was behind the workers' ills. Cecil Drinker, the leader of the Harvard team, found while examining workers in a dark room that "their hair, faces, hands, arms, necks, the dresses, the underclothes, even the corsets of the dial painters were luminous."[62] In fact, every space within the factory was coated with radium dusts. Moreover, blood tests on twenty-two employees revealed abnormalities in every case.[63] Although Drinker suspected that inhalation was the critical route of entry into the women's bodies (and thus the cause of their jaw necrosis), a

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[61] Wiley had an ally at Harvard, Alice Hamilton. Hamilton was a preeminent toxicologist and the first woman faculty member at Harvard but what also associated with the New Jersey Consumers' League.
[62] Quoted in Caufield, *Multiple Exposures: Chronicles of the Radiation Age*, 32.
[63] Ibid. Drinker also did animal studies at the behest of U.S. Radium which confirmed their findings. See Nugent, "The Power to Define a New Disease: Epidemiological Politics and Radium Poisoning," 179.

follow-up study by Frederick Hoffman, a statistician for an insurance company who was brought in to study the plight of the radium dial painters by Wiley, demonstrated that the workers habit of pointing their brushes with their mouths was the more likely and damaging point of entry.

Despite the overwhelming clinical evidence, questions remained regarding the biological mechanisms by which radium exposure induced injury. Hoffman and Drinker's conclusions were educated guesses at best and in light of the paucity of information of the biological impact of internally deposited radium, the full realization of radium poisoning would not emerge until the 1930s and 1940s.[64] Nevertheless, radium dial painting studios soon prohibited the practice of lippointing, mitigating (even if they didn't quite understand how) the symptoms of the dial painters.

Harrison Martland, a local county medical examiner, began research on the etiology of radium poisoning among dial painters in early 1925. Martland had become increasingly interested in the widely publicized cases of the dial painters, and after being contacted by Katherine Wiley, embarked upon an epidemiological study of the women, the core of which would be based upon clinical and autopsy studies.[65] The first autopsies Martland performed plainly illustrated what many had already begun to suspect: internally deposited radium was a bone seeker. Sadly, Martland would find no shortage of bodies to study as dial painters were dying in increasing numbers throughout the decade. Sarah Maillefer, a dial painter with seven years of experience in the West Orange studio, for example, was referred to Martland by her dentist when she began to

[64] The problem was compounded by the fact that dial painting studios in Connecticut didn't show similar incidences of necrosis of the jaw.
[65] Nugent, "The Power to Define a New Disease: Epidemiological Politics and Radium Poisoning," 182.

show classic symptoms of the ills that were plaguing dial painters—fatigue, leg pains, and jaw problems. Martland performed a series of tests before and after she died in 1925 which showed high concentrations of radiation throughout her body, but mostly concentrated in the skeleton.[66] Martland also discovered that he could detect radon (a gaseous radium "daughter product") in the breath of living dial painters. This was particularly worrisome for Martland because its presence meant that radon was circulating throughout the blood stream, constantly inundating the blood-forming centers of the body with radiation.[67] Radon breath analysis, not incidentally, proved a useful tool for the detection of radium deposition, even though it indicated a qualitative presence and could not be used for determination of absolute exposure values. Through Martland's studies, the mechanisms behind the radium poisoning were becoming increasingly clear: radium deposited in bone caused the skeletal pain and jaw rot and the decay of radium within bone was producing radon which affected the normal operation of blood formation producing anemia.

By 1933, the etiology of radium poisoning of the dial painters had progressed sufficiently enough to warrant a special article surveying the topic. Robley Evans, the author of the survey, was a relative newcomer to the subject at the time, but would soon establish himself as the preeminent expert on radium toxicology in the coming years. Evans was first introduced to the problem of radium poisoning while a graduate student at Cal Tech in Pasadena in 1932. While working on a PhD investigating ways to measure the radium content of rocks, Evans was approached by a Los Angeles County Medical

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[66] Clark, *Radium Girls: Women and Industrial Health Reform, 1910-1035*, 103.
[67] Hacker, *The Dragon's Tail: Radiation Safety in the Manhattan Project, 1942-1946*, 22.

Examiner who, alarmed by the recent death of E.B. Byers and wanting "no radium scandal in sunny California," asked him for help.[68] Evans was intrigued and embarked on a "quantitative study of the uptake, metabolism and excretion rate of radium in living persons," an interest that he would carry with him as he established himself as a professor of physics at M.I.T. in 1934.[69]

Evans's first studies on radium poisoning centered on uptake experiments in rats, but he grew increasingly frustrated by the poor reliability of the animals for extrapolating data to human subjects. As Evans recalled years later, the "*proper subject for the study of man is man*," and the key to understanding radium toxicity, he thought, was in the development of more precise quantitative means for measuring the total radium body-burdens for exposed individuals.[70] Breath analysis was a useful means of detecting radium exposure, but because of the complexities and different types of radiation emissions posed by radium and its daughter products it could only measure a fraction of the total radioactive body burden. In each step of the decay chain of radium, its daughter products each have their own specific radioactive properties (i.e. alpha, beta, gamma, or a combination of the three) and each compounds the metabolic effect of the others. Evans thus referred to radium as merely "the first bullet from a repeating gun."[71] Breath analysis measured only a fraction of the radium daughter bullets that managed to make their way into the bloodstream and eventually the lungs. For Evans, to determine the

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[68] Robley D. Evans, "Inception of Standards for Internal Emitters, Radon and Radium," *Health Physics* 41, no. 3 (1981): 438.
[69] Ibid.
[70] Evans, "Inception of Standards for Internal Emitters, Radon and Radium," 441.
[71] Robley D. Evans, "Protection of Radium Dial Workers and Radiologists from Injury by Radium," *Journal of Industrial Hygiene and Toxicology* 25, no. 7 (1943): 234.

total radioactive emission present he had to develop a means of counting the total activities present in the whole body. Without such a quantitative total measure, he understood, the establishments of a permissible body burden (or tolerance dose) for radium would have been impossible. With this problem in mind, Evans developed the "meter-arc technique" which became the basis for the first radiation whole-body counter in 1934. With this new tool, Evans turned his attention back to the dial painters themselves. By measuring the body burden of twenty-seven dial painters and comparing their burdens to the observable pathologies, Evans was able to begin to approximate a permissible level of exposure.

Despite Evans's breakthrough investigations and development of the whole-body counter, radium exposure standards were not established until 1941. The impetus was the growing shadow of World War II. The Navy, in particular, was interested in expanding the radium dial paint industry to meet the growing need for luminescent ships instruments. As Evans recalled some years later, he was approached by a Navy officer who threatened him with enlistment if he didn't provide a maximum permissible dose for radium.[72] Apparently persuaded, Evans assembled a team of investigators (including Martland) and based upon his research on the body burdens of dial painters set the maximum permissible dose of radium at 0.1 microcuries of exposure.[73] This, he concluded, was "such a level that we would feel perfectly comfortable if our own wife or

[72] Evans, "Inception of Standards for Internal Emitters, Radon and Radium," 443.

[73] The "curie" is a unit for all radioactive substances. It denotes not air ionization, but the total activity in terms of energy. One curie is equivalent to the amount of radioactivity emitted by one gram of radium. A microcurie, then, is one millionth of a curie. These were purely physical units. Units based on biological damage would be developed during and shortly after the MED.

daughter were the subject."[74] The team published their recommendations in National Bureau of Standards Handbook 27, and for the Navy, just in time.

Even without the establishment of a permissible dose, had his wife been a dial painter, Evans could have found comfort in the dramatic transformations that the dial painting factories underwent throughout the 1930s. The dial painting companies of the 1920s, as noted by Angela Nugent, based the painting workspace, quite naturally, on artists' studios.[75] As one might expect in a studio, management of workers was rather loose and casual. But by the 1940s, radium dial painting was a tightly controlled and supervised enterprise [see Figures 1 and 2]. The growing evidence of radium toxicity coupled with labor and state pressure on dial painting companies to ameliorate working conditions that contributed to exposure had transformed the studio into a toxicological laboratory. Dial painters now worked individually in discrete work booths equipped with their own ventilation system and were encouraged to maintain routines that would guarantee personal and workspace "hygiene." Proper "housekeeping" was the guiding principle.

As Evans remarked in an article on radium protection in 1943, "hoods facilitate good housekeeping and personal cleanliness, which are the essence of protection against radium ingestion, and also provide forced suction ventilation for the removal of radon liberated from the paint stocks and finished work under the hood."[76] To make certain that hoods were adequately drawing any potentially hazardous dusts away from workers, periodic atmospheric monitoring of the factory air was also implemented as a control

[74] Evans, "Inception of Standards for Internal Emitters, Radon and Radium," 443.
[75] Nugent, "The Power to Define a New Disease: Epidemiological Politics and Radium Poisoning," 186.
[76] Evans, "Protection of Radium Dial Workers and Radiologists from Injury by Radium," 256.

device. But monitoring focused more keenly on worker bodies. Factory managers required dial painters to routinely provide breath samples and toxicologists or company medical physicians performed blood tests at least twice a year to monitor for biological changes in blood counts.[77] Evidence of over exposure frequently resulted in workers being barred from radium work, and in some cases firing. By the 1940s, dial painters, then, had little power to influence protection standards—that was the province of toxicologists and engineers.

The recourse to toxicology held other consequences for health protection in the factory and health more generally, however. The move to the laboratory enabled toxicologists to study single chemicals under ideal conditions. As Linda Nash has written, scientific reductionism made it possible for toxicologists "to think about precisely measuring the amount of a given chemical that must be present before it caused consistent biological effects."[78] For radium and a host of other chemicals, this method was crucial for making chemical toxicities perceptible.[79] Tolerance, threshold, or permissible values, as a result, were an almost inevitable outcome of the move to the laboratory. But this kind of "threshold thinking" also carried with it new ways of thinking about the environment.[80] By its very nature, the turn toward the laboratory closed the door on the environment; that seemed to be the only way to identify and

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[77] Nugent, "The Power to Define a New Disease: Epidemiological Politics and Radium Poisoning," 186. See also, Evans, "Protection of Radium Dial Workers and Radiologists from Injury by Radium."
[78] Nash, *Inescapable Ecologies: A History of Environment, Disease, and Knowledge*, 142.
[79] Murphy, *Sick Building Syndrome and the Problem of Uncertainty: Environmental Politics, Technoscience and Women Workers*.
[80] The term "threshold thinking" is Hacker's. See Hacker, *The Dragon's Tail: Radiation Safety in the Manhattan Project, 1942-1946*; Hacker, *Elements of Controversy: The Atomic Energy Commission and Radiation Safety in Nuclear Weapons Testing, 1947-1974*.

measure the particular chemicals under study.[81] Toxicologists understood that when they applied that knowledge gained in the laboratory to “real” world occupational illnesses they would encounter the complexities, irregularities, and contingencies that encompassed the material world of the factory floor. Nevertheless, their experience in the laboratory suggested to them that just as they had eliminated the problem of place in the laboratory, they could do the same for the factory. In short, they had to make the factory into a laboratory. To enact this delicate spatial move they needed control and for that they turned to the exceptional faculties of industrial engineers who devised the instruments necessary to eliminate the intrusion of the environment. By so controlling the factory, the environment was rendered passive; chemicals, not the environment, is what really mattered when it came to disease.[82]

Occupational illnesses were the unintended consequences of America’s rapid industrialization in the late nineteenth and early twentieth centuries. Although many Americans were wary of the technological transformations taking place during the period, few could have anticipated the impact that large-scale industrialization of toxic chemicals would have for human health. Ironically, the increasing visibility and publicity of chemically-induced diseases did not temper Americans enthusiasm for technological progress. Engineers were not to blame, they were ones destined to find the technological fix. The death of the women dial painters were merely a glitch in the upward trajectory of American modernity and the technological optimism that Americans had in

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[81] Kohler, *Landscapes and Labscapes: Exploring the Lab-Field Border in Biology*; Nash, *Inescapable Ecologies: A History of Environment, Disease, and Knowledge*.
[82] Mitman, "In Search of Health: Landscape and Disease in American Environmental History."; Murphy, *Sick Building Syndrome and the Problem of Uncertainty: Environmental Politics, Technoscience and Women Workers*; Nash, *Inescapable Ecologies: A History of Environment, Disease, and Knowledge*.

engineering control gave them confidence that engineers and toxicologists could do the same for environments outside the factory walls.

That confidence would soon be tested. In a twist of historical coincidence, just as the first tolerance standards for radium were being published in 1941, America was about to embark on the largest technological enterprise ever devised up to that time—the building of an atomic weapon. Encompassing dozens of sites throughout the United States, staffed with thousands of scientists and workers who would be exposed to a host of new radiological hazards, the Manhattan Engineering District posed protection problems on a scale never before witnessed. But armed with the successful methods of toxicology and the experiences gained in studying the dial painters, scientists charged with radiation safety were convinced that they could prevent undue injury and death. As AEC industrial hygienist Merill Eisenbud recalled in 1959:

> Historically speaking, the New Jersey cases…were a most valuable accident. If they had occurred twenty years later, we might have gone into our wartime atomic-bomb project without knowing how boneseekers do their work. Our Manhattan District might have proved to be a tremendous booby trap. We might not have foreseen the internal-emitter problem in all its seriousness, and even if we had, we might have been five years arguing it. With the evidence before us, however, there was no room for debate. If it hadn't been for those dial painters, the project's management could have reasonably rejected the extreme precautions that were urged on it—the remote control gadgetry, the dust-dispersal systems, the filtering of exhaust air—and thousands of Manhattan District workers might well have been, and might still be, in great danger.[83]

The hazards presented by the building the bomb, then, were merely toxicological problems writ large.

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[83] Daniel Lang, "A Most Valuable Accident," *The New Yorker*, May 2, 1959, 15-16.

Figure 1 - Dial painting studio circa 1920s Source: Public domain.

Figure 2 - Dial painting studio circa 1940s. Source: Robley Evans, "Protection of Radium Dial Workers and Radiologists from Injury by Radium." Journal of Industrial Hygiene and Toxicology 25, no. 7 (1943): 253-69. Copyright unknown.

IN THE FACTORIES OF THE BOMB: KEEPING HOUSE

> "*It is in the nature of radioactivity not to end but to continue*." – Peter Bacon Hales.[1]

At roughly 8:00 on the morning of July 16, 1945, radiation monitor Arthur Breslow was driving east on New Mexico Highway 380 toward searchlight station L-8. Breslow, like his some forty-four fellow radiation monitors, was charged with tracking, measuring, and reporting the whereabouts and intensity of radioactive fallout from the world's first nuclear explosion—Trinity—which had detonated approximately two and a half hours earlier. Breslow no doubt looked rather ridiculous to many of the residents of the local ranching communities that he passed on the highway toward L-8, protected as he was with hardly a piece of skin exposed to the morning sun gleaming through his windshield. He was, in fact, dressed head to toe in standard Manhattan Engineer District (MED) radiation protection garb: coveralls, cap, booties, gloves, and respirator (an iconography that along with the mushroom cloud would come to symbolize the atomic age). Out of radio contact at his previous position at station L-7, he was anxious to reach L-8 in order to report back to Trinity base camp the meager gamma radiation readings picked up by his Victoreen model 247 ionization chamber. "Up until this time," he would later report, "there were no indications of any danger from radiation." As Breslow reached the top of a pass ten miles outside of Bingham, however, he caught sight of a "strata of sand-like dust" covering the valley floor below. Fearing what likely lay in that

[1] Peter Bacon Hales, *Atomic Spaces: Living on the Manhattan Project* (Urbana: University of Illinois Press, 1997), 325.

dust, Breslow immediately closed all the windows to his vehicle and reached for his respirator in order to more fully seal his body from the potentially deadly dust. Unable to locate his respirator which he had left in haste back at L-7, he quickly improvised and found a slice of bread that he managed to breathe through as he traveled through the valley toward his destination.[2]

Breslow's fellow radiation monitors found a number of areas throughout central New Mexico that exhibited alarmingly high concentrations of fallout radiation. John Magee, who was monitoring in a canyon along the Chupadera Mesa, recorded radiation levels that were in excess of 20 roentgens per hour. (The roentgen (r) was an internationally adopted unit that measured the amount of radiation required to produce a certain amount of ionization in the air. Prior to the war, the standard was 0.2r per day. An acute dose of 500r was considered lethal.) Stafford Warren, the chief medical officer in charge of the entire medical section of the MED, later estimated that "Hot Canyon," as the area was dubbed thereafter, likely received between 212-230 total roentgens of radiation.[3] Fortunately, MED personnel thought, Hot Canyon was uninhabited.

But Hot Canyon *was* inhabited. While driving through the canyon the following morning, two high-ranking medical officers from Los Alamos happened upon a little ranch that had not been accounted for in the radiation monitors' maps. Situated less than a mile from the canyon, the Raitliff ranch was home to an older couple, their ten year old

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[2] Joseph G. Hoffman, "Nuclear Explosion 16 July 1945: Health Physics Report on Radioactive Contamination Throughout New Mexico Following the Nuclear Explosion, Part C: Transcript of Radiation Monitor's Field Notes. Film Badge Data on Town Monitoring," December 13, 1945, Nuclear Testing Archive, Las Vegas, Nevada (hereafter NTA), Accession # NV0059839. See also Ferenc Morton Szasz, *The Day the Sun Rose Twice: The Story of the Trinity Site Nuclear Explosion, July 16, 1945* (Albuquerque: University of New Mexico Press, 1984).

[3] Ibid.

grandson, and a swarm of cattle. According to the two Los Alamos officers, the family appeared to be unaffected by the high radiation levels found on the ranch. The cattle, however, showed classic signs of radiation damage: skin burns, ulcerations, and loss of hair.[4] Two days later at Los Alamos, the Trinity radiation monitors gathered to discuss, among other things, what to do about the Raitliffs. Given the rapidly decaying levels of external gamma radiation recorded over the last few days in the area and in light of the apparent healthy status of the family, they decided against evacuation. Monitors, they agreed, would periodically check the Raitliffs over the coming months for signs of injury. They did not consider the cattle to be an immediate or long-term danger to the Raitliffs' health.[5]

Recent studies of the MED's radiation safety program during Trinity have noted how little attention MED health officials paid to the risks associated with the internal ingestion of fallout radiation.[6] The only measurements that MED personnel took of the Raitliffs, for example, were for external gamma radiation. Why didn't the MED consider the Raitliff's livestock a potentially dangerous pathway of radiation exposure? Undoubtedly, the Raitliffs drank the milk from these livestock and butchered and consumed them, but the MED did nothing to remove these animals or the Raitliffs from this contaminated food chain. The failure to consider these ecological pathways was not

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[4] Hacker, *The Dragon's Tail: Radiation Safety in the Manhattan Project, 1942-1946*, 105; Hales, *Atomic Spaces: Living on the Manhattan Project*, 329.

[5] Louis Hempelmann, "Events in Camp Immediately Following Shot—July 16, 1945," n.d., in L.H. Hempelmann, "Nuclear Explosion 16 July 1945: Health Physics Report on Radioactive Contamination Throughout New Mexico, Part B: Biological Effects," July 3, 1947, Report # LA-638, Los Alamos National Laboratory Research Library (hereafter LANL-RL), http://tang.lanl.gov/, 13.

[6] Stannard, *Radioactivity and Health: A History*; Thomas E. Widner and Susan M. Plack, "Characterization of the World's First Nuclear Explosion, the Trinity Test, as a Source of Public Radiation Exposure," *Health Physics* 98, no. 3 (2010).

limited to the Trinity test. Consideration of the ecological dimensions of fallout were glaringly absent in the U.S. government's fallout risk assessments well into the middle of the 1950s when testing reached its greatest heights.[7]

What follows is a close look into the foundational scientific practices that informed how scientists within the MED (and subsequently, the Atomic Energy Commission) considered the risks associated with fallout. This chapter focuses on the emergence of a new scientific discipline within the MED. “Health Physics,” as this new discipline was termed during the war, grew out of a concern for the special hazards that working with heretofore unknown substances like plutonium engendered. Although health physics was born out of the exigencies of the atomic bomb project, its core practices were an outgrowth of earlier developments in the industrial hygiene and toxicology programs of early twentieth century factories. Following my discussion of the development and professionalization of toxicology in the previous chapter, I argue that health physicists excluded the environment as an agent affecting health when considering fallout because they believed that human bodies and the environment were bounded entities with few material linkages.[8] This inherited inclination regarding the nature of the

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[7] Much of the historical literature analyzing radiation safety regarding fallout has focused on scientific debates about linear versus threshold theories of radiation exposure. Although valuable, none of these works have analyzed why health physicists consistently excluded environmental radiation as a hazard, even though ecological concerns formed the core of the fallout controversy in the late 1950s. See Hacker, *The Dragon's Tail: Radiation Safety in the Manhattan Project, 1942-1946*; Hacker, *Elements of Controversy: The Atomic Energy Commission and Radiation Safety in Nuclear Weapons Testing, 1947-1974*; Jolly, "Thresholds of Uncertainty: Radiation and Responsibility in the Fallout Controversy"; Walker, *Permissible Dose: A History of Radiation Protection in the Twentieth Century*.

[8] Environmental historians have recently begun to look closely at the ways in which toxicological practices have shaped the regulation of chemicals in the environment. See, for example, Nancy Langston, *Toxic Bodies: Hormone Disruptors and the Legacy of DES* (New Haven: Yale University Press, 2010); Gerald Markowitz and David Rosner, *Deceit and Denial: The Deadly Politics of Industrial Pollution* (Berkeley: University of California Press, 2003); Mitman, "In Search of Health: Landscape and Disease in American Environmental History."; Gregg Mitman, Michelle Murphy, and Christopher Sellers, *Landscapes of*

relationship between bodies and environments, I show in this chapter, predisposed health physicists into downplaying the risks of the internal ingestion of fallout radiation caused by the contamination of food chains. There were other repercussions too. The concept of bounded bodies was complimented by a similar view that the environment of the Trinity test site was also bounded.[9] Unlike previous toxicological experience, the testing of the atomic bomb presented health physicists with problems involving wholly new geographic scales. This much health physicists understood. Nevertheless, they assumed that the radioactivity produced by the bomb would be contained within the boundaries of the Trinity test site, just as the radiation in the factories largely remained within its walls. As I demonstrated in the previous chapter, protection of workers in factories was predicated on toxicologists approximating the environmental conditions of the factory to those of the toxicological laboratory, where the environment was tightly controlled. When it came to testing the bomb, health physicists adopted an analogous spatial technique: the confidence gained in protecting workers in the bomb-building factories convinced health physicists that they could control the environment outside the factory in similar ways. The resulting double blindness produced a radical simplification of the environment. Not only did health physicists miss the critical environmental pathways that contributed to

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Exposure: Knowledge and Illness in Modern Environments, *Osiris* (Chicago: University of Chicago Press, 2004); Murphy, *Sick Building Syndrome and the Problem of Uncertainty: Environmental Politics, Technoscience and Women Workers*; Nash, *Inescapable Ecologies: A History of Environment, Disease, and Knowledge*; Sellers, *Hazards of the Job: From Industrial Disease to Environmental Health Science*.

[9] My analysis of spatial containment has been shaped by Fiege, "The Weedy West: Mobile Nature, Boundaries, and Common Space in the Montana Landscape."; David Harvey, *Justice, Nature, and the Geography of Difference* (Cambridge: Blackwell Publishers, 1996); Kirsch, *Proving Grounds: Project Plowshare and the Unrealized Dream of Nuclear Earthmoving*. See also Thomas F. Gieryn, "Boundaries of Science," in *Handbook of Science and Technology Studies*, ed. S. Jasanoff, et al. (Beverly Hills: Sage, 1994); Gieryn, *Cultural Boundaries of Science: Credibility on the Line*; Reidy, *Tides of History: Ocean Science and Her Majesty's Navy*.

internal exposure, they also overlooked the environmental mechanisms (primarily meteorological) that could produce significant off-site fallout. In short, as health physicists left the factory and looked upon the New Mexico desert they did not see a complex environment requiring special scientific consideration, they saw a factory.[10]

In the first section of this chapter, I explore the establishment of health physics within the MED. Like toxicology, health physics began in the laboratory and I focus on the research programs instituted to understand the toxicity of the multitude of new radioactive products being produced by atomic reactors. I then go on to examine the radiological detection instruments devised by technicians and the "good housekeeping" practices the MED utilized to engineer a sanitary workplace environment. In last section of the chapter, I analyze how health physicists applied the practices and concepts they developed in the factories to the environment in and around the Trinity Site. In particular, I pay close attention to the fallout models and monitoring plans that MED scientists employed to protect off-site populations. I conclude by calling attention to the repercussions that these early safety practices had for the Atomic Energy Commission in the postwar nuclear weapons testing period.

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[10] Scholars in both environmental history and the history of technology have recently begun to redefine how we understand factories. Much of this has centered on industrial practices embedded in environments outside of the factory. Although works in labor history have for some time analyzed the industrial-like control of workers in agricultural fields, these works tend to focus on environmental control outside of the factory. For labor control, see, Carey McWilliams, *Factories in the Field: The Story of Migratory Farm Labor in California* (Santa Barbara: Peregrine Publishers, 1971); Don Mitchell, *The Lie of the Land: Migrant Workers and the California Landscape* (Minneapolis: University of Minnesota Press, 1996). For the environmental control, see, David Igler, *Industrial Cowboys: Miller & Lux and the Transformation of the Far West, 1850-1920* (Seattle: University of Washington Press, 2001); Nash, "Purity and Danger: Historical Reflections on the Regulation of Environmental Pollutants."; Russell, "Evolutionary History: A Prospectus for a New Field." See especially, Timothy J. LeCain, *Mass Destruction: The Men and Giant Mines That Wired America and Scarred the Planet* (New Brunswick: Rutgers University Press, 2009).

Manhattan Engineering District: "Good Housekeeping"

On January 16, 1939 Niels Bohr, the eminent Danish physicist, was disembarking from a lengthy cruise to the United States to spend several months at Princeton University in part to discuss theoretical problems with Albert Einstein.[11] Shortly before he left Europe, however, Bohr received astonishing news. Earlier in December, Otto Hahn and Fritz Straussman while experimentally bombarding uranium with neutrons discovered, quite unexpectedly, trace quantities of barium in the resulting uranium sample. The presence of barium seemed to suggest that the uranium had somehow been split leaving behind two fragments (one being barium) and an appreciable amount of energy. Not quite sure what to make of this, Hahn contacted his colleague and recent German refugee Lise Mietner in Sweden to make some sense of the physics behind this chemical reaction. Meitner along with her nephew Otto Frisch confirmed the fragmentation of the uranium and reasoned that the splitting was a product of the extra energy provided by the added neutron to an already packed atomic nucleus. They termed the splitting fission and demonstrated that the energies observed were a result of part of the original uranium mass being converted to energy, so much energy in fact that Frisch calculated that splitting one uranium atom could make a grain of sand jump.[12] Frisch hastily wrote up two pages of his and Metiner's work and handed them to Bohr as he was

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[11] H. D. Smyth, *Atomic Energy for Military Purposes: The Official Report on the Development of the Atomic Bomb under the Auspices of the United States Government, 1940-1945* (Princeton: Princeton University Press, 1945), 24.

[12] My account of the events leading up to the establishment of the Manhattan Engineering District is drawn Richard Rhodes, *The Making of the Atomic Bomb* (New York: Simon and Schuster, 1986).

taking the train to the harbor to catch his ship. When Bohr arrived in the United States, he had news indeed, news of this new discovery and, more ominously, of political events in Europe.

The German invasion of Poland lay many months away from Bohr's arrival in January 1939, but the specter of German National Socialism cast a menacing pall over the news of atomic fission. The United States and Great Britain by the late 1930s had already witnessed a remarkable flood of scientific talent fleeing the grip of Hitler's anti-Jewish and anti-intellectual policies. Albert Einstein was the most notable of the European émigrés, having arrived in the U.S. in 1933. A number of physicists soon followed him, many of whom would play decisive roles in the building of the atomic bomb including Enrico Fermi, Hans Bethe, Edward Teller, and Leo Szilard. No wonder, then, that when Bohr's news hit American shores, the thoughts of European émigrés turned almost immediately to the military potential of nuclear fission. All were convinced that if provided a fission weapon, Hitler would almost surely use it. That conviction coupled with the fact that both Hahn and Straussman were German made the prospects of a German atomic weapon program an alarming near certainty.[13]

Nevertheless, despite the theoretical possibilities, the practical and technical aspects of producing an atomic weapon seemed nearly insurmountable. For one thing, splitting a few uranium atoms in a laboratory did not imply that a self-sustaining nuclear reaction could be reached. Leo Szilard had in the early 1930s considered the possibility of a nuclear chain reaction, going so far as to patent his idea in 1936. But that was before Hahn and his colleagues verified the possibility of fission and long before anyone

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[13] Ibid.

understood how to control a potential chain reaction. Questions also surrounded uranium. Naturally occurring uranium deposits are made up of six different isotopes. Which isotopes of uranium could be fissioned? Once that was determined, how could one separate enough of the material to create a critical mass without resorting to a massive industrial undertaking?[14]

These questions and more loomed in 1939.[15] Nevertheless, physicists in the United States and Great Britain quickly delved into theoretical and experimental research investigating the possibility of achieving a self-sustaining chain reaction. Research progressed rapidly and as the prospect of achieving a chain reaction appeared all the more feasible, political questions about the balance of power in Europe should Germany build an atomic weapon became manifest. Leo Szilard, perhaps the physicist with the keenest mind for the political consequences of fission, convinced his colleagues that at the very least the American government should be notified of the potential for an atom bomb. Nearly a year and a half before official U.S. entry into World War II and a month before Germany invaded Poland, Szilard along with Albert Einstein wrote to President Roosevelt warning of the feasibility and destructive potential of an atomic bomb. Roosevelt formed an exploratory committee (Advisory Committee on Uranium) on the potential for atomic weaponry two months later, where the idea essentially languished for nearly two years. In mid-1941, the American effort was galvanized by a British report

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[14] By the end of the war, the Manhattan Engineering District possessed more industrial capacity than General Motors. See Welsome, *The Plutonium Files: America's Secret Medical Experiments in the Cold War*, 8.

[15] For a technical history of the atomic bomb building project see Lillian Hoddeson et al., *Critical Assembly: A Technical History of Los Alamos During the Oppenheimer Years, 1943-1945* (Cambridge: Cambridge University Press, 1993); Rhodes, *The Making of the Atomic Bomb*.

suggesting that as little as five kilograms of uranium isotope 235 (U235) could provide the needed critical mass to provide a chain reaction and explosion. The British report and American entry into World War II in December 1941, thrust the atomic bomb project to the forefront of the American war effort.

But where to begin? Achieving a controlled chain reaction seemed the first priority and, fortunately, that effort was already underway at Arthur Compton's Metallurgical Laboratory (Met Lab) at the University of Chicago where Enrico Fermi was busy working on building a nuclear reactor. Chicago became the center of the atomic bomb program for the next two years until the army formally took control of the project under the umbrella of the Manhattan Engineer District.

Not long after Fermi and his colleagues began construction of the nuclear reactor (or "pile"), questions concerning protecting the scientists, technicians, and workers began to surface. As Met Lab director Arthur Compton wrote in his memoirs in 1956, "Our physicists became worried. They knew what had happened to the early experimenters with radioactive materials. Not many of them had lived very long. They [the physicists working on the pile] were themselves to work with materials millions of times more active than those of these earlier experimenters."[16] The early experimenters were scientists like Marie Curie whose fingers had rotted from handling radium. She eventually died from leukemia, no doubt induced by her radiation exposure. She was not the only casualty. As physician Robert Stone, who would in the coming months direct the Health Division of the Met Lab, noted after the war, the real worry was in the

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[16] Arthur Holly Compton, *Atomic Quest: A Personal Narrative* (Oxford: Oxford University Press, 1956), 177.

experience of the radium dial painters.[17] By the early 1940s, roughly about a kilogram of radium had been refined and isolated for medical and industrial use in paint.[18] The nuclear pile that Fermi was building would produce radioactive material on a wholly different order of magnitude. If such a relatively small amount of radium could wreak havoc among the dial painters, what would "fission products whose radioactivity was equivalent to thousands of grams of radium" do to workers in laboratories and the production plants?[19] The problem was further compounded by the fact that most of the radioactive byproducts of fission were altogether new substances of which radiologists, physicians, and toxicologists had little knowledge. Radium was a bone seeker. That much they knew. Yet, as Stone acknowledged, "Where these new radioactive elements—some of which were gases, other volatile materials, and all others capable of being dust—would go in the body and what they would do there was unknown."[20]

Ironically, Compton did not recruit scientists such as Robley Evans or Harrison Martland who had spent the last decade and a half researching the dial painters into the Met Lab's Health Division. Nor did he seek out many of the qualified researchers within his own university. Most of the staff of the Health Division were brought in from the west coast, particularly those who had pioneered the use of radiological therapies and tracer methodologies while working with the cyclotron at the Radiation Laboratory (Rad Lab) in Berkeley.

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[17] Robert S. Stone, "The Plutonium Project," *Radiology* 49, no. 3 (1947).
[18] The 1 kg figure comes from Jos. G. Hamilton, "The Metabolism of Fission Products and the Heaviest Elements," *Radiology* 49, no. 3 (1947): 325.
[19] Stone, "The Plutonium Project," 364.
[20] Ibid.: 365.

Physicist Ernest Lawrence developed the cyclotron at Berkeley in the early 1930s, primarily to study the behavior of subatomic particles. Colloquially known in the press as an “atom smasher,” the Berkeley cyclotron was one of the first high-energy particle accelerators. Although the cyclotron has been commonly associated with nuclear physics, the philanthropists whom Lawrence recruited to finance the expensive machines were much more keenly interested in its biomedical uses.[21] As was mentioned in the previous chapter, physicians had long expressed hope in developing a radium palliative or cure for cancer and, at least since the 1920s, biomedical scientists conducted experimental therapies toward that end to varying degrees of success. The cyclotron promised to expand biological knowledge and cancer radiotherapy owing to the machine’s capability of transforming ordinary elements into radioactive forms, or radioisotopes. The radioisotopes produced in the cyclotrons were chemically identical to their non-radioactive counterparts and therein lay their utility. When administered to the body (intravenously, paranatally, or via ingestion), these radioisotopes biologically behave exactly as their non-radioactive forms.[22] And due to their radioactivity, biologists could easily trace these radioisotopes as they metabolized in the body with the aid of Geiger-Muller counters or through autoradiographic techniques.[23]

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[21] Timothy Lenoir and Marguerite Hays, "The Manhattan Project for Biomedicine," in *Controlling Our Destinies*, ed. Phillip R. Sloan (South Bend: University of Notre Dame Press, 2000). See also Waldo E. Cohn, interviewed by Thomas Fisher, Jr. and Michael Yuffee, January 18, 1995, Human Radiation Studies: Remembering the Early Years, http://hss.energy.gov/HealthSafety/ohre/roadmap/histories/0464/0464toc.html.

[22] That is, provided they were administered in a tracer-sized dose low enough to prevent a biological response from their radioactive emissions.

[23] A radioautograph is an image on an x-ray film produced by the pattern of radioactive emissions emerging from the body or post-mortem section.

The boon for biomedical research was nearly endless. As physician and MED veteran Joseph Hamilton remarked after the war, "These momentous developments have given the biologist probably the most useful tool for research since the discovery of the microscope, because almost all of the elements and compounds present in biological systems can be 'tagged' with the aid of artificial radio-elements and their course in living structures directly studied."[24] Once the metabolic pathways of the various radioisotopes were determined, clinical therapies were developed. Hamilton, for example, developed techniques for treating thyroid diseases by employing radioiodine which tended to bioconcentrate in the gland.[25] Berkeley-centered physicians experimented with similar therapies throughout the 1930s for bone cancer (radiostrontium) and leukemia (radiophosphorus), all based upon a similar hope that given their particular physiological characteristics, these radioisotopes might acts as "magic bullets" targeting specific geographies of the body where malignancies might lay.[26] In no small way, to paraphrase an argument made by Timothy Lenoir and Marguerite Hays, the development of radioactive tools in biomedicine up to and during the years of the war transformed medicine from an art to a fully-fledged science built on the sound footing of quantifiable research and experimentation.[27]

Robert Stone, a physician at the University of California Medical School (UCSF) who Compton hired to head the Health Division in August of 1942, pioneered many of

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[24] Joseph G. Hamilton, "The Use of Radioactive Tracers in Biology and Medicine," *Radiology* 39 (1942): 542.
[25] Cite the Hamilton papers on the radioiodine issue.
[26] "Magic bullets" comes from Welsome, *The Plutonium Files: America's Secret Medical Experiments in the Cold War*, 24.
[27] Lenoir and Hays, "The Manhattan Project for Biomedicine," 34.

the clinical radioisotope techniques and therapies developed at Rad Lab.[28] A Canadian by birth, Stone fought in World War I and joined the faculty of UC San Francisco in 1928. A firm believer in radiation therapy, Stone was a frequent visitor to the Rad Lab and throughout the decade of the 1930s developed a number of clinical experiments using radiosodium and neutron bombardment for deep seated tumors.[29] Compton, whose interest in cancer research kept him in touch with Stone's experimental work at Berkeley, felt him uniquely qualified to handle the hazards posed to workers from the nuclear pile Fermi was building. "Stone's exceptional qualification," Compton wrote in his memoirs, "for this work on which the very lives of our workers depended was evident."[30] For the physicist Compton, what better person to choose to head your Health Division than someone who had wide experience with a variety of radioisotopes and approached radiation much as he and his colleagues did—as a danger yes, but also as a tool.[31]

Despite the academic setting, Compton organized the Met Lab along industrial lines. There were no "departments" to speak of. Instead, there were divisions and, underneath them, special sections and groups. Stone's was the Health Division, and he created three sections, "each having its own problems."[32] The Medical Section largely confined itself to monitoring general personnel health and clinical tests checking for radiation damage. To that end, they instituted routine physicals to monitor for erythemic

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[28] K.G. Scott, "Robert Spencer Stone (1895 - 1966)," *Radiation Research* 33, no. 3 (1968).
[29] For a fuller description of his work see Welsome, *The Plutonium Files: America's Secret Medical Experiments in the Cold War*, 25-27.
[30] Compton, *Atomic Quest: A Personal Narrative*, 177.
[31] Kenneth Scott, himself a product of the Berkeley group, wrote of Stone in an obituary, "As one of the pioneers in the investigation of ionizing radiation, he was one of the first to bridge the gap between the basic sciences and clinical medicine." Scott, "Robert Spencer Stone (1895 - 1966)," 675.
[32] Robert S. Stone, *Industrial Medicine on the Plutonium Project* (New York: McGraw-Hill, 1951), 2. There was also a fourth short-lived "military section."

effects and blood changes to safeguard personnel. The Health Physics Section was "concerned mainly with detecting, measuring, and protecting against radiations and with interpreting the results in terms of health hazards."[33] Drawn from the Met Lab's Engineering Division and recruits from radiology departments in hospitals on the west coast, the Health Physics Section was largely devoted to the development of radiation detection instruments. The Biology Section was formed to conduct animal experiments to investigate the biological effects of radiation exposure. Each of the three sections were geared toward the one all important goal of protecting scientists and workers from the radiations that were to become apparent when Fermi tested his nuclear pile in the coming months. Although Stone differentiated between the biological, medical, and radiological aspects of the Health Division, health physics became the catch-all term for radiation protection whether such investigations were basic or applied.

In reality, however, the line between the pure and applied was never clearly drawn.[34] When Stone organized the Health Division, he said as much to his fellow colleagues in a Met Lab report: "It must be remembered that the whole clinical study of the personnel is one vast experiment. Never before has so large a collection of individuals been exposed to so much irradiation."[35] While the Medical Section might confine itself to blood monitoring, or the Health Physics Section to instrument development, Stone felt that the vast scale of the atomic bomb project coupled with the extensive new hazards posed by the fission process placed nearly every researcher in

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33 Ibid.

34 This point is also made in Hacker, *The Dragon's Tail: Radiation Safety in the Manhattan Project, 1942-1946*, 39-43.

35 R.S. Stone, "Health Division Program," May 10, 1943, Met Lab Report # CH-632, NTA, 1-2.

uncharted territory. Yet, given the dearth of clear toxicological data on the biological effect of ionizing radiation, Stone more keenly sensed the need for fundamental biological research into the mechanisms of radiological damage. If protection was the overriding concern, then the question that naturally followed was protection from what? Standards for external gamma radiation exposure, which appeared to be the most critical immediate threat to workers, could be, initially at least, extrapolated from the International X-Ray and Radium Protection Committee's maximum permissible concentration (mpc) standard for x-rays. Likewise, the research on the radium dial painters offered valuable experience and data to the Health Division, not the least of which was the mpc for radium that Robley Evans helped develop just prior to the war. But, as Stone argued in an article after the war, the "scientific data on which it was based was found to be very sketchy [because] the experimental material was usually too limited, the data given by one worker were not comparable with those of others, the doses were often unsatisfactorily stated, and few, if any, fundamental facts emerged."[36] By "fundamental facts," Stone meant the biological mechanisms of radiation damage from both internal and external sources. If those could be found, then, "many of the problems would have been much simpler."[37] If one could not know all the precise effects of the various fission products (to say nothing of uranium, and later, as we will see, plutonium), one could have a basic understanding of the action of radiation in the body and from there

[36] Stone, "The Plutonium Project," 364-5.
[37] Stone, *Industrial Medicine on the Plutonium Project*, 4.

determine tolerance doses. What was needed, he argued, was a direct approach and that meant pure research.[38]

Outside of humans, animal experimentation seemed the most direct and productive place to begin. Kenneth Cole, who headed the Biology Section, conducted some of the experiments. Stone's former colleague at the Rad Lab in Berkeley, Joseph Hamilton, conducted most of the interesting experimental work, however. Hamilton, as we have seen, had extensive experience with the radioisotopes produced in the Berkeley cyclotron and Stone thought him the perfect candidate to perform the metabolic work that he hoped would provide some answers to the biological mechanism question. The project was daunting. As Hamilton wrote just after the war, "fission products can produce injury as an external source of radiation, or, if they gain entry into the body, by acting as an internal radioactive poison, quite analogous to radium poisoning."[39] If the case of the radium dial painters was any indication, he reckoned, the quantities of fission products required to produce injury inside the body were likely to be far less than the amount needed from external sources. The severity of the internal radioactive emitter problem was further compounded by the fact that each radionuclide produced in the pile was likely to exhibit different metabolic properties once inside the body. Prior to the war, only two had been studied at any length—radioiodine and radiostrontium. In each of those cases, the radionuclides behaved quite dissimilarly, one depositing in and irradiating the thyroid gland and the other in bone, respectively. "The nature of the metabolic characteristics of the other fission products at that date [i.e. World War II],"

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[38] Stone, "The Plutonium Project," 365.
[39] Hamilton, "The Metabolism of Fission Products and the Heaviest Elements," 325.

Hamilton later reported, “was essentially a completely unknown quantity.”[40] What Hamilton and his group needed to do, as a colleague of his perceptively put it, was to “chew our way through the periodic table.”[41]

Hamilton’s metabolic program functioned largely independently from the Met Lab, although results of the experiments were reported directly to Stone in Chicago. With the help of a handful of researchers, many of whom would go on to scientific and medical acclaim, Hamilton worked his way through the major long-lived fission products by experimenting mainly with rats.[42] The first studies were conducted using only tracer quantities of cyclotron-produced radionuclides so as to not affect any biological changes that might skew normal metabolic processes in the rats. In each of the tracer studies, special attention was paid to the assimilation, distribution, retention, and excretion of the radionuclides. Following the euthanization of the rats, radioautographs were made of organs displaying high degrees of radioactive selectivity and accumulation.[43]

By spring of 1943, the first results of Hamilton’s experiments were coming in.[44] The picture that emerged showed that the radionuclides exhibiting the greatest retention were the alkaline and rare earths like strontium, iodine, and cesium. With the exception of iodine, all tended to concentrate in bone and were easily absorbed through the digestive track which marked them for particular scrutiny as an internal radioactive

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[40] Ibid.: 326.

[41] The quote is from Hamilton's colleague Patricia Durbin. See Stannard, *Radioactivity and Health: A History*, 399.

[42] Patricia Durbin and Kenneth Scott were pioneers in nuclear medicine.

[43] Hamilton, "The Metabolism of Fission Products and the Heaviest Elements."; Joseph G. Hamilton, "The Metabolic Properties of the Fission Products and Actinide Elements," *Reviews of Modern Physics* 20, no. 4 (1948); Stannard, *Radioactivity and Health: A History*, 302-12.

[44] For example, see Robert S. Stone, “Health Division Program,” May 10, 1943, Met Lab Report # Ch-632, NTA, NV0717325.

hazard. Although from this initial data tolerance standards for each of these radionuclides could not be made, at the very least, the realization that their metabolism was quite similar to radium (which was also a bone-seeker) put the workers protection on firmer ground because standards for it already existed.[45]

This development could not have happened soon enough. On December 2, 1942, Fermi's reactor pile underneath the University of Chicago football stands went online, turning the problems of the Health Division from the "mainly theoretical" into concrete reality.[46] The "realities" posed by the running reactor were complex. Prior to the war, the hazards associated with radium were relatively easily controlled. The reactor, however, presented not only a host of new radionuclides to contend with, but also greater combined energies. Shielding the reactor, and eventually the uranium and plutonium production and processing plants, would afford some protection, but for a host of technical reasons, some radiation would undoubtedly escape. To protect workers, Stone reasoned, it was "not enough to simply know that radiations are present in a given area." Instead, before physicians and biologists could determine the nature and severity of certain radiological injuries, they had to "define the characteristics of the physical situation where and when such biological changes occurred."[47] That required sufficient instrumentation to measure and identify the radiations present.

Prior to the war, however, few instruments were commercially available and those that were had solved only part of the overall problem. Pocket ion chambers had been

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[45] See previous chapter.
[46] Stone, *Industrial Medicine on the Plutonium Project*, 5.
[47] Robert S. Stone, "Health Protection Activities of the Plutonium Project," *Proceedings of the American Philosophical Society* 90, no. 1 (1946): 13.

marketed by the Victoreen Instrument Company since about 1940. About the size of a fountain pen that could be worn in shirt pockets, the device consisted of a tube with a charged wire running through its center [Figure 3]. When the tube was exposed to ionizing radiation, the charge was gradually negated and to figure out the exposure one merely had to measure (with a separate device) the difference between the initial charge and the subsequent one. Pocket dosimeters were often unreliable, so health physicists usually required workers to wear two at a time.[48] Photographic film badges were another extant technology immediately adopted by the Health Division [Figure 4]. Unlike pocket chambers, which could provide nearly instantaneous exposure readings, film badges required development before revealing the extent of exposure. That was an important drawback, as was its largely qualitative properties since there was little way to quantifiably express what a particular dose was from the blackened marks on the film. If the film was black, overexposure was a certainty. Nevertheless, film was a reasonably reliable way of monitoring accumulated doses over periods of time ranging from days to weeks.[49]

But personal monitoring was only one aspect of the overall monitoring program. While film badges and pocket chambers gave good indication of the external (mostly gamma) radiation exposure to workers, they could not provide data on exposure to alpha particles or slow and fast neutrons emanating from the reactor pile. Alpha-emitting

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[48] William P. Jesse, *The Role of Instruments in the Atomic Bomb Project,* vol. Report # MDDC-289 (Oak Ridge: United States Atomic Energy Commission, 1946), 4. See also, Hacker, *The Dragon's Tail: Radiation Safety in the Manhattan Project, 1942-1946*, 36.

[49] Ibid. For more on the development of photo badges see Ronald L. Kathren, "Before Transistors, Ic's, and All Those Other Good Things: The First Fifty Years of Radiation Monitoring Instrumentation," in *Health Physics: A Backwards Glance. Thirteen Original Papers on the History of Radiation Protection*, ed. Ronald L. Kathren and Paul L. Zimmer (New York: Pergamon Press, 1980).

particles, as evidenced by radium, constituted perhaps the gravest long-term threat to the workers' bodies because of their insidious effect on the internal organs should they be inhaled or ingested. Area and survey monitoring thus provided a crucial element of the overall protection scheme. Because few alpha particle counters existed, or because of their size were unfit for portable or ease of use, the Health Physics Section made it a priority to improve existing or develop new instruments.[50]

Area monitoring equipment consisted of two types: fixed and portable survey equipment. Health physicists at the Met Lab found that ion chambers or Geiger-Muller tubes could be adapted for use as fixed monitoring instruments, often with an added alarm mechanism should radiation reach exceed certain levels.[51] Portable monitoring instruments were another matter. Health physicists, along with a considerable number of physicists, had to develop these instruments from scratch and, if the names they ascribed to these new instruments were any indication, they had a rather good time doing it. All the instruments were designed for a specific purpose and their names, however colorful, reflected that. Pluto, like the famous Disney dog, was a hand-held instrument that could be run over work benches to "sniff out" alpha particles. Sneezy was a modified vacuum fitted with a special removable filter used to monitor radiation in the air. For corners and hard to reach places, health physicists used the fish-pole meter [Figure 5]. Not all the portable monitoring equipment was geared toward the workplace. Some were designed to monitor clothes and parts of the human body. Poppy, named for the popping sound it made when it registered alpha particles, was a proportional counter affixed with a long

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[50] Jesse, *The Role of Instruments in the Atomic Bomb Project*.

[51] H. M. Parker, "Health Physics, Instrumentation and Radiation Protection," *Health Physics* 38, no. 6 (1980): 984. This is a reprint of a report Parker originally distributed in 1943 within the MED.

narrow probe that could be used to monitor clothes or even the nose. Later, when the plutonium-producing reactors were built at Hanford, health physicist Herbert Parker devised a stationary hand and foot counter.[52]

Knowing that certain radiations or radioactive particles were present was only part of the technical complexities inherent to protecting workers during the building of the bomb. Once detected, one needed to ensure that the workplace environment was sanitized of all radioactive material—or at least as much as was technically feasible. While the best practices for limiting exposure to external forms of radiation was proper shielding, preventing the entry of radioactive particles into the body required a more engineering-like approach, and for that health physicists turned to industrial hygiene for guidance.

The industrial hygiene program at the Met Lab, and later for the entire Manhattan Engineering District, was modeled chiefly on the radium dial painting studios. And like the case of radium protection, the key driving principle behind the sanitization of the workspace was proper housekeeping. As Stafford Warren, soon to be chief of the entire medical section once the military subsumed the Met Lab under the Manhattan Engineering District, wrote in his account of the health protection activities of the bomb project, "Plant personnel were told frankly that certain hazards existed but could be prevented; that practices could be instituted which, if sedulously observed, would prevent harm to them; and that the precautions necessary were really quite simple and chiefly

[52] Karl Z. Morgan, "Instrumentation in the Field of Health Physics," *Proceedings of the IRE* 75, no. 1 (1949): 81; Parker, "Health Physics, Instrumentation and Radiation Protection," 984.

based upon *good housekeeping*."[53] The most basic practices were geared toward the worker's bodies themselves [Figure 6]. At various sites throughout the MED, health physicists installed showers and wash basins, and instituted strict prohibitions against eating, drinking, or smoking in quarantined or contaminated areas.[54] Worker clothing was closely monitored for radioactivity and, if contaminated, was laundered so as to avoid spreading radioactive materials beyond the confines of the workplace.[55]

Proper housekeeping also required steadfast attention to the workplace and it was here that the greatest effort was placed to engineer a sanitary environment. Health physicists believed that inhalation was the primary route through which radioactive substances might enter the human body. Four possible precautions preventing or minimizing the hazard were considered: 1) supplying separate air to the worker; 2) enclosing the radioactive material in an air-tight containers; 3) filtering the air; or 4) directing the flow of air in the immediate workspace so that the air flows from the direction of the worker to the radioactive material.[56] Health physicists implemented all four possibilities to varying degrees. Fitting plastic hoods over worker heads with a separate air supply system was one preventative method. The hoods, however, were clumsy and reduced the efficiency of the worker thereby limiting their use to areas of high contamination. Efficiency, of course, was paramount given the military imperative to beat a possible German atomic bomb. The need for speed thus shaped the technical

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[53] Stafford L. Warren, "The Role of Radiology in the Development of the Atomic Bomb," in *Radiology in World War II*, ed. Kenneth D.A. Allen (Washington: Office of the Surgeon General Dept. of the Army, 1966), 869-70. Emphasis mine.
[54] Ibid. See also J.J. Nickson, "Protective Measures for Personnel," in *Industrial Medicine on the Plutonium Project*, ed. Robert S. Stone (New York: McGraw-Hill, 1951), 86.
[55] Ibid., Nickson, 91.
[56] Paraphrased from Ibid., 81.

choices that health physicists made. The two more common devices implemented to reduce inhalation of radioactive materials were ventilation hoods and "dust boxes." Ventilation hoods, as I discussed in the previous chapter, had been developed for use in the radium dial painting studios. At the Met Lab, ventilation hoods were installed over almost every work bench where radioactive materials were being handled. Experience had shown health physicists that to be effective the ventilation hoods needed to move at least one hundred linear feet of air per minute. That requirement resulted in a number of engineering problems, not the least of which was that when all the ventilation hoods were in operation, the air in the room was exchanged every four minutes, making heating of the rooms difficult. In addition to the work bench hoods, most buildings were also equipped with electrostatic precipitation filters to pick up stray ambient radioactivity. Dust boxes were employed for excessively dry radioactive materials. Consisting of a closed box or cylinder and fitted with gloves sealed in two arm holes, the dust box was an early forerunner of the modern glove box commonly found in scientific laboratories today. The dust box afforded considerable efficiency and protection from handling alpha-active materials because it allowed workers to handle the materials closely, but behind the protection of a layer of glass. Dust boxes were less effective when dealing with gamma or beta radiation owing to their more highly penetrating properties.[57]

The Met Lab would not be the only site where such instruments and protective machinery would be devised and utilized. By September of 1942, the U.S. military formally took charge of the bomb building project and the Met Lab was subsumed under General Leslie Groves and the Manhattan Engineering District. Military control of the

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[57] Ibid., 84-5.

project and of the Met Lab greatly expanded scale and scope of the atomic bomb project. The decision, mainly by Groves, between October 1942 and February 1943 to build two more reactors turned the project from a small scale academic effort into a rapidly expanding industrial enterprise.[58] The Clinton Engineer Works at Oak Ridge, Tennessee was the first to break ground and the Met Lab was charged with running the site. Oak Ridge, as the site became known after the war, housed the electromagnetic separation plants which were designed to manufacture weapons-grade uranium—the fuel in the bomb detonated over Hiroshima. The other site, Hanford Works in Washington State, was the site of the plutonium processing plant and Groves contracted the du Pont Company to run the site. Both were massive in scale and each devised their own health physics section based upon the organization and initial developments made at the Met Lab.

The entrance of the army into the project also brought with it new organizational structures, including strict compartmentalization and other security apparatuses. It also brought about a reorientation of the basic biological research program instituted by Stone in Chicago. Groves initially asked Stone to head the Medical Section of the entire MED project, but Stone demurred, preferring the comfort of academic life at Chicago to the strictures of the military since he would have been required to enlist in the army.[59] Groves next offered the job to University of Rochester radiologist Stafford L. Warren

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[58] Hacker, *The Dragon's Tail: Radiation Safety in the Manhattan Project, 1942-1946*, 45. The expansion of industrial capacity was a function of the extraordinary difficult and time-consuming process required to extract fissionable isotopes of uranium and plutonium from uranium ores. Because there were two possible methods for processing, Groves decided to go with both to ensure that enough fissionable material could be produced in a timely matter.

[59] Hymer Friedell, interview by J. Newell Stannard, May 27, 1981, NTA, NV0702814, 4. See also, Welsome, *The Plutonium Files: America's Secret Medical Experiments in the Cold War*, 60.

who accepted and was promptly given the rank of colonel. Stone and Warren would not see eye to eye on many issues over the next couple years. Their main conflict centered on the nature of Stone's basic biological research.

Warren was, in many ways, Grove's ideal person for the job. At Rochester, Warren served as a consultant for the Eastman Company which made him intimately familiar with the imperatives of industrial production. Because speed and efficiency were Grove's primary concerns in directing the MED, Warren was equipped to see to it that those values were embedded in every aspect of the Medical Section.[60] As Warren wrote to a high-ranking MED official, that meant biological research "which cannot come to fruition for long periods of time should not be undertaken."[61] Expediency did not mean that worker's should be brazenly put in undue harm. Warren's concern was not so much the worker, per se, but the "medico-legal" problems that might arise should a worker later claim that he was injured by radiation while working on the bomb. "For consideration of the medical legal aspects, including necessary biological research," Warren argued, "it should be pointed out…that investigations should be conducted only insofar as they will augment the data and information available for protecting personnel and from the operating facilities and should be limited to those features which are deemed most likely to embarrass the Government."[62] Both the scaling down of biomedical research and Warren's concern for the legal aspects of injury claims infuriated Stone. Upon receiving a copy of Warren's letter, Stone fashioned a lengthy

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[60] Hales, *Atomic Spaces: Living on the Manhattan Project*, 278.
[61] Stafford L. Warren to Colonel K.D. Nichols, April 17, 1945, "Purposes and Limitations of the Biological and Health-Physics Program," NTA NV0728644.
[62] Ibid.

rebuttal in a memo to his files so that his opposition to Warren might be documented. "I do not believe that any research should be done to 'strengthen the Government's interests' from the medico-legal point of view. Research should be done to establish the facts. If these are in the employees interests they should be done equally as much as if they are in the Government's interests, because the two interests should be the same."[63] Warren won. Research on acute effects was germane to the mission of the MED. Long-term research on low-level effects was not; that research might threaten to slow production.[64]

The partial exception to Warren's rule was plutonium. University of California chemist Glenn Seaborg discovered (produced would be the better term since plutonium was a known possibility) the substance by bombarding uranium in the Berkeley cyclotron. After tedious chemical processing, in early 1941 Seaborg managed to isolate a small fleck of plutonium. The military importance of plutonium was recognized immediately because the substance was assumed to be highly fissionable. Seaborg confirmed that a few weeks later.

Plutonium as an occupational hazard for workers emerged rather late in the bomb-building enterprise. The tardiness in considering its toxicity lay in the fact that so little of it existed until roughly 1944. External gamma hazards and the fission radionuclides seemed to warrant health physicists' primary attention, initially at least.[65] By early 1944,

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[63] Robert S. Stone, Memo to Files, August 17, 1945, "Col. Stafford L. Warren's Memo Entitled 'Purposes and Limitations of the Biological and Health-Physics Program," NV00715874.
[64] Hales has analyzed the Warren/Stone controversy at some length. See, Hales, *Atomic Spaces: Living on the Manhattan Project*, 278-82.
[65] W.H. Langam and J.W. Healy, "Maximum Permissible Body Burdens and Concentrations of Plutonium: Biological Basis and History of Development," in *Uranium, Plutonium, Transuranic Elements*, H.C. Hodge, J.N. Stannard, J.B. Hursch eds. 1973, 572.

however, Seaborg had become particularly worried about plutonium after reading a recent article by Robley Evans on the radium poisoning among the dial painters. Like radium, plutonium is a heavy metal and primarily an alpha emitter. If such small quantities of radium could wreak such havoc, what might plutonium do to the body if ingested? That question prompted Seaborg, who had moved to join the Met Lab in 1942, to write to Stone in the first week of January 1944 suggesting that a biological research program be set up to determine the element's toxicity. "It has occurred to me," Seaborg wrote, "that the physiological hazards of working with plutonium and its compounds may be very great. Due to its alpha radiation and long life it may be that the permanent location in the body of even very small amounts, say one milligram or less, may be very harmful…In the handling of the relatively large amounts soon to begin here and at Site Y [Los Alamos], there are many conceivable methods by which amounts of this order might be taken in unless the greatest care is exercised…I would like to suggest that a program to trace the course of plutonium in the body be initiated as soon as possible."[66] Stone responded a few days later telling Seaborg that a metabolic study of plutonium had been on the radar of the Health Division for some time and that Hamilton's group at Berkeley would be the prime choice to conduct such studies. Unfortunately, Stone informed Seaborg, "Sufficient quantities to study the chemical toxicology will not be available for some time." The studies would have to wait for at least a couple months.[67]

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[66] G.T. Seaborg to R.S. Stone, January5, 1944, "Physiological Hazards of Working with Plutonium," NTA, NV0708058.

[67] R.S. Stone to G.T. Seaborg, January 8, 1944, "Health Hazards of Working with Plutonium," NTA, NV0708278.

Still, Stone was alarmed enough to mention the plutonium hazard at a high-level meeting at the Clinton Engineer Works (Oak Ridge) the next week. At the meeting, Stone noted the poisonous nature of the "product." Although, plutonium was assumed to be a bone seeker like radium, the fact that the radioactive element emitted almost purely alpha particles meant that detection of it within the body was impossible because it did not emit gamma or beta rays or decay into a substance that did. Compton suggested that despite its similarity to radium, plutonium was, in the worst case, probably less hazardous by about a factor 50, owing to the fact that the energy output per alpha decay of plutonium was less than an equivalent decay of a radium alpha. If the prewar radium body burden standard was 1 microgram, Stone and Compton reasoned, then the tolerance dose for plutonium would be roughly 5 micrograms.[68]

Hamilton received his first shipment of plutonium on February 10, 1944 and promptly began a study of the element's metabolism in rats. The results were not quite as comforting as Stone and Compton predicted. As expected, plutonium tended to concentrate in bone and could be expected to cause bone cancers. But unlike radium, the elimination rate of plutonium once deposited was quite slow. Despite the different alpha energies exhibited between the two substances, plutonium was as biologically effective as radium because it stayed in the body longer. That wasn't the only problem. The principle means through which plutonium became absorbed into the body was through the lungs, not the digestive track. Given the emphasis of the Health Division on inhalation hazards, that fact marked plutonium for special consideration and health

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[68] Project Council – Policy Meeting, January 19, 1944, NTA NV0729214.

physicists at Hanford quickly developed air concentration standards to limit airborne exposure.[69]

By 1944 and early 1945, Hanford was not the only MED project site faced with the prospects of plutonium contamination. In 1943, construction began at an old private boy's school northwest of Santa Fe, New Mexico where much of the actual bomb building enterprise would be researched and undertaken. Until roughly the beginning of 1944, radiation hazards at Los Alamos, as the site was named, were not dissimilar to what the other MED sites had experienced. Soon, however, shipments of plutonium began to arrive for experimentation and contamination of workspaces quickly followed. Initially, Los Alamos was not slated for a pure biological research program on plutonium or any other radioactive hazards. J. Robert Oppenheimer, Groves's scientific advisor and head of Los Alamos, preferred that the research conducted at the lab should focus on the bomb itself. As Oppenheimer wrote in a laboratory memo, "We are not equipped for biological experiments."[70] Most of the Los Alamos health section, as a result, confined their activities to routine monitoring and decontamination of workers and personnel. An accident in August of 1944 changed everything.

Don Mastick, a chemist at Los Alamos, inadvertently snapped a vial containing ten milligrams of plutonium suspended in liquid. As the vial broke, the contents spewed out of the bottle and splattered against a wall, some of it ricocheting back in Mastick's face. In the ricochet, Mastick involuntarily swallowed some of the spray. How much

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[69] Joseph Hamilton, "Metabolism of Product, NTA, NV0180070. See also, W.H. Langam and J.W. Healy, "Maximum Permissible Body Burdens and Concentrations of Plutonium: Biological Basis and History of Development."

[70] Quoted in Hacker, *The Dragon's Tail: Radiation Safety in the Manhattan Project, 1942-1946*, 63.

plutonium he happened to inhale or ingest, Mastick had no clue. Neither did, Louis Hempelmann, the head of Los Alamos' medical section. Without a way to detect the amount of plutonium that Mastick may have ingested or inhaled, Hempelmann was at a loss as to how to assess the potential biological damage. To be sure, Hamilton's rat data put plutonium hazards on par with radium (i.e. a tolerance dose of one microgram), but without a system to measure the actual dose, Hempelmann was groping in the dark when it came to actually monitoring plutonium exposure for workers.[71] The best Hemplemann could do to prevent exposures was to perform nose-swipes, but even then, this was at best a guess at the potential lung exposure.[72]

With the problem of plutonium detection in mind, Hempelmann approached Oppenheimer to request permission to begin a biological research program at Los Alamos. The aim, he wrote Oppenheimer, was to develop a satisfactory method for determining the plutonium content in urine and feces.[73] Oppenheimer agreed and within a year one of Hempelmann's scientists in the medical section, Wright Langham, developed a long and tedious process for detecting plutonium which involved dry-ashing the workers' stool and urine.[74] When Hempelmann and Langham began trial runs of the new detection method, they found that many of the workers' samples showed that they

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[71] For more information on the Mastick incident see, Ibid; Welsome, *The Plutonium Files: America's Secret Medical Experiments in the Cold War*.
[72] Hacker, *The Dragon's Tail: Radiation Safety in the Manhattan Project, 1942-1946*, 68.
[73] L.H. Hempelmann to J.R. Oppenheimer, "Health Hazards related to Plutonium," August 16, 1944, NTA, NV0701491.
[74] J.R. Oppenheimer to L.H. Hempelmann, "Your Memorandum of August 16, 1944," August 16, 1944, NTA, NV0701494; Wright H. Langham and Elizabeth Maxwell, "Determination of Plutonium in Human Feces," August 29, 1945, Report # LA-376, LANL-RL.

were already carrying an above tolerance dose of plutonium.[75] But that was only the excreted plutonium. How much had the workers actually inhaled or ingested? What was their actual dose? What Hempelmann needed to know was whether there was an excretion rate from which they could extrapolate the initial dose. Hamilton's previous work on rats was of little help. It seemed to conflict with other studies performed on dogs; and there were the ever-present difficulties inherent in extrapolating animal data to humans.[76] They needed a human subject to test the reliability of the urine-fecal analyses.[77]

All told, MED scientists injected eighteen patients with plutonium. None were informed of the nature of the injections and not all who were injected were terminally ill patients; at least one of the patients lived until the early 1990s, while others lived to their 70s and 80s. The injections occurred at various MED installations including Berkeley, Chicago, Oak Ridge, and Rochester, New York. Nearly every high level medical officer employed or associated (i.e. Hamilton) with the MED was involved with the secret research project.

As a result of the human experiments, MED physicians learned that once deposited within the body, plutonium managed to stay put with remarkable efficiency. "The retention of plutonium," Hamilton wrote of one experimental human subject in a 1946 report, "… is so great that the loss of this material can be considered negligible.

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[75] Hacker, *The Dragon's Tail: Radiation Safety in the Manhattan Project, 1942-1946*, 68; Welsome, *The Plutonium Files: America's Secret Medical Experiments in the Cold War*, 73.
[76] William Moss and Roger Eckhart, "The Human Plutonium Injection Experiments," *Los Alamos Science* 23 (1995): 194.
[77] Ibid; Welsome, *The Plutonium Files: America's Secret Medical Experiments in the Cold War*.

The half time of plutonium excretion is probably greater than fifty years."[78] Ultimately, Los Alamos officials determined that roughly 8.7 per cent of a single plutonium dose was excreted in a five year period. From those initial estimates, a daily excretion rate of 0.01 per cent was assumed to constitute a lifetime permissible exposure to plutonium (i.e. one microgram). By July 1945, an analysis using the 0.01 per cent excretion rate on Los Alamos workers revealed that five workers met or exceeded the body burden tolerance dose for plutonium. They were promptly removed from further work with the element.[79] The human experiments may have helped to prevent undue harm to those workers, but at a terrible ethical price.

If by the spring of 1945, the plutonium injections had illuminated much about the plutonium hazard to workers, Los Alamos health officials soon learned that new hazards loomed on the horizon. As the final stages of the bomb building projected moved toward completion, new questions emerged about radiation safety once the bomb was to be field tested. Fallout, a word that heretofore had not yet been uttered on anyone's lips, soon preoccupied safety planners minds. Experience would soon tell whether the concepts, methods, and practices developed at various sites within the MED to protect workers could be similarly applied to the world outside the bomb-making factories.

<u>Trinity: Turning the Jornada del Muerto Valley into a Factory</u>

In the spring of 1945, Los Alamos physicists Joseph Hirschfelder and John Magee rather awkwardly stumbled on the problem of fallout. As Magee told historian Ferenc

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[78] Hamilton quoted in Moss and Eckhart, "The Human Plutonium Injection Experiments," 199.
[79] Ibid.: 213.

Szasz, Hirshfelder rushed into Magee's office, after an apparent epiphany, and exclaimed, "What about the radioactivity?"[80] While studying the formation of the bomb fireball, Magee and Hirschefelder realized that ground level debris would be sucked up in the updraft of the explosion and become radioactive as it mixed with the gases and particles in the fireball. If the height of the fireball reached upwards of 12,000 feet, what would be the fate of those radioactive materials as they traveled through the atmosphere and what effect would they have on surrounding areas should they rain back to ground level?[81] As early as October of the previous year, the head of the implosion design team George Kistiakowski wrote to Oppenheimer informing him that the Trinity bomb test tentatively scheduled for June would undoubtedly produce significant airborne radiation and that tracking their movement would be essential.[82] Kistiakowski, however, was concerned largely with the implications that such movement would have for military application of the bomb. Hirschfelder and Magee, by contrast, worried about the test itself and its potential impact on MED personnel and surrounding civilian communities.

Initially, as Kistiakowski's memo to Oppenheimer demonstrates, safety played a small part in the early plans for the Trinity test. Before the fallout question was even broached, the site for testing the bomb had already been chosen in the fall of the previous year. Although General Groves in his postwar recollections listed fallout as one of the factors influencing his decision to test the bomb in the Jornada del Muerto Valley near

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[80]Quoted in Szasz, *The Day the Sun Rose Twice: The Story of the Trinity Site Nuclear Explosion, July 16, 1945*, 63.
[81] Joseph O. Hirschfelder, "The Scientific and Technological Miracle at Los Alamos," in *Reminiscences of Los Alamos*, ed. Lawrence Badash, Joseph O. Hirschfelder, and Herbert P. Broida (Boston: D. Reidel Publishing Company, 1980), 73-75.
[82] G.B. Kistiakowski to J.R. Oppenheimer, "Activities at Trinity," 13 October, 1944, NTA, NV0120975.

Alamogordo, New Mexico, in reality, security imperatives dominated his thinking at the time.[83] As Kenneth Bainbridge, director of the Trinity test wrote in a contemporary report, "The main consideration [in choosing a test site]…was the question of security and complete isolation of the activities of the test site from activities at Project Y [Los Alamos]." To be sure, the decision to select a remote site helped to ensure that "Ranches and settlements [w]ould be distant to avoid possible danger," but that consideration was subservient to security imperatives; it just so happened that the need for security and safety held similar requirements.[84] Technical imperatives too were placed above safety. "The idea was to explode the thing...We weren't terribly concerned with the radiation," Hymer Friedell, a high-ranking MED medical official, recounted in the 1980s.[85] Still, even if concerns about radiation protection never outweighed other testing goals, once the possibility of fallout emerged in technical discussions about the potential effects of the bomb, MED officials did take it seriously. In fact, the MED marshaled some of its best health physicists and medical officers from Los Alamos and elsewhere to begin planning for the test.

Safety planning for Trinity began in March just as Hirschfelder and Magee were beginning to study the fallout problem. The initial plans materialized in two meetings of a self-appointed committee which included Hirschfelder, Hempelmann, and several

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[83] Lt. Gen. Leslie Groves, "Some Recollections of July 16, 1945," *The Bulletin of Atomic Scientists* 26, no. 6 (1970): 24.

[84] Quotes are in K.T. Bainbridge, "Trinity," May 1976, Report number LA-63000-H, LANL-RL, 3. This report is a compilation of Trinity reports, memos, and documents written during the MED.

[85] Quoted in Hacker, *The Dragon's Tail: Radiation Safety in the Manhattan Project, 1942-1946*, 84-5.

physicists.[86] The immediate concern centered on the Trinity rehearsal test known as the 100-ton test scheduled for early May. The idea behind the 100-ton test was to explode one hundred tons of explosives so physicists could calibrate their instruments and practice making blast measurements in a smaller and more controlled explosion. Yet that was not the only reason behind the trial run. Physicists also planned on placing a dissolved, but still quite radioactive, reactor slug from Hanford in the middle of the assembled explosives. As physicists Herbert Anderson wrote in a memo to Kenneth Bainbridge, the addition of the slug to the test would provide an opportunity to learn more about the distribution of radioactivity during an atomic explosion. "What happens to the radioactivity; what fraction deposits superficially over the ground nearby, what fraction is mixed with the earth thrown up from the crater, and what fraction is essentially lost through having been thrown up in the air and dispersed over a much wider area?"[87] The 100-ton test, then, provided an opportunity to put Hirschfelder and Magee's theoretical concerns on a sound empirical footing.

The need for empirical data was acute because much of the radiation monitoring activities at the Trinity site and in surrounding areas during the final atomic test depended on a "working model of the explosion and of [the] cloud of fission products."[88] During the initial planning meetings in March, the Trinity safety committee chiefly considered

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[86] See "History of the Preparation of the Medical Group for Trinity Test II" in L.H. Hempelmann, "Preparation and Operational Plan of Medical Group (TR-7) for Nuclear Explosion 16, July 1945," June 13, 1947, Report # LA-631, LANL-RL; L. Hempelmann and R. Watts, "Hazards of Trinity Experiment," April 12, 1945, NTA, NV0059747.

[87] H.L Anderson to Mr. K.T. Bainbridge, "The 100 Ton Shot Preparations - Addition to No. 7 to Project TR Circular," April 19, 1945, NTA, NV0059793.

[88] "History of the Perparation of the Medical Group for Trinity Test II" in L.H. Hempelmann, "Preparation and Operational Plan of Medical Group (TR-7) for Nuclear Explosion 16, July 1945," June 13, 1947, Report # LA-631, LANL-RL.

immediate hazards to MED personnel. These included dangers from the blast, blast fragments, heat, light, gamma radiation exposure, and radioactive particles.[89] For the first five hazards, theoretical calculations gave a reasonable picture of what to expect during the detonation. Predictions of the radioactive particle problem were far less certain. The dilemma centered in part on the projected efficiency of the atomic explosion. Efficiency was a function of the amount of plutonium that underwent fission. If the bomb was efficient, most of the plutonium would fission into other radionuclides and, given the greater magnitude of the explosion, would be thrust high into the atmosphere where, presumably, it would disperse. A less efficient explosion was considered the greater hazard because most of the plutonium would remain close to the ground in a cloud of radioactivity. In that event, they figured, within a six mile radius of the test, even with the aid of respirators, it would be hazardous to breathe in the dust for more than eight minutes. For the nearest community roughly twenty miles away, it would be dangerous to breathe in this cloud for more than five-hundred minutes.[90] In either case, the problem was primarily meteorological and safety planners assumed that given the right conditions the atmosphere would sufficiently disperse the cloud and dilute its radioactivity so that serious overexposures might be avoided.

Given the importance of meteorology, Bainbridge immediately hired meteorologist Jack Hubbard to provide weather forecasts for the upcoming tests.[91] Hubbard's first assignment was the 100-ton test and Bainbridge had specific weather

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89 L. Hempelmann and R. Watts, "Hazards of Trinity Experiment," April 12, 1945, NTA, NV0059747.

90 Ibid.

91 K.T. Bainbridge to Mr. J.M. Hubbard, April 19, 1945, NTA, NV0123760.

conditions he wanted met. In particular, he wanted clear skies and low wind velocities so that the various measurements and cloud tracking could be conducted under the best possible conditions.[92] Hubbard promptly began weather observations on April 20 and was given nearly full authority for selecting the date of the shot.[93] Based on his forecast, Hubbard selected May 7th and at 4:30 in the morning of that day the 100-tons of explosives was detonated.

As Hubbard predicted, the weather proved ideal for the test and the blast went off as expected. Stafford Warren, who was stationed twelve miles away from the Trinity site, heard the explosion and witnessed a "red-brown to orange light" arising out of the Trinity site.[94] At 6:30, Hempelmann and the safety team entered the explosion site to begin taking radiation measurements. All were wearing respirators and booties, except for Oppenheimer who followed the team. His was the only "infraction of the rules," Hempelmann reported, and "he took full responsibilities for his actions." Luckily, Hempelmann considered the hazards to be "relatively slight." In the center of the crater produced by the explosion, the radioactivity readings were well below what they considered hazardous—about 0.7r per 24 hours. Hempelmann held similar views regarding the off-site radiation readings. Although the radioactive materials placed in the explosion were almost entirely contained in the resulting cloud, Hempelmann felt that "there was little likelihood of any contamination ever reaching the earth since there has been shown to be a dilution of 10,000 times for every 2,000 feet vertical decent of such

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92 Ibid
93 J.M. Hubbard, "100-Ton Test Meteorological Report," June 7, 1945, Report# LA-285, LANL-RL.
94 Stafford L. Warren to Major General L.R. Groves, "Visit to Sites M and Y on 6-10 May. inc. 1945 During Test I," May 16, 1945, NTA, NV0026160.

clouds."[95] The test appeared to have confirmed many safety planners speculation that setting the bomb off under proper meteorological conditions would help reduce the hazards posed by fallout.

Such optimism did not pervade all who were involved in the 100-ton test, however. Hubbard was the first to sound a note of caution regarding the limitations of forecasting. "The question of dissipation of the trailing column and ball of smoke," he wrote Bainbridge nearly three weeks after the 100-ton test, "is not primarily a problem of forecasting."[96] Forecasting could give safety planners a general idea of the types of weather to be expected on a given day, but they gave little sense of how localized atmospheric phenomena might affect the distribution of fallout. In his letter to Bainbridge, Hubbard discussed a number of mechanisms which might affect the track of the cloud during the Trinity test including daytime solar heating and terrain effects. While Hubbard maintained that the observed phenomena during the 100-ton test seemed to suggest that these mechanisms prevented the cloud from descending back to the earth sooner than expected, his point was clear: local conditions matter. More pessimistic conclusions drawn from the 100-ton test were elaborated by Hirschfelder and Magee on June 16th, a month before the scheduled Trinity test. "There is a definite danger," they wrote Bainbridge, "of dust containing active material and fission products falling on

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[95] L.H. Hempelmann to File, "Hazards of 100 Ton Shot at Trinity," May 18, 1945, NTA, NV0059884.
[96] J.M. Hubbard to Mr. K. Bainbridge, May 26, 1945, in L.H. Hempelmann, "Preparation and Operational Plan of Medical Group (TR-7) for Nuclear Explosion 16, July 1945," June 13, 1947, Report # LA-631, LANL-RL,

towns near Trinity and necessitating their evacuation."[97] The problem, according to Hirschfelder and Magee, centered on the size of the particles that would be picked up from the ground during the explosion. Large particles were likely to fall back to earth near the Trinity site owing to their mass. Mid-sized particles with diameters ranging from 74 to 149 microns, however, were likely to fall back to earth within thirty minutes to two and a half hours. Assuming the cloud moved along at an average pace of thirty miles per hour, a person living in a nearby town might receive approximately 22r of gamma radiation in the first day and, depending on the efficiency of the bomb, the plutonium hazards might be an order of magnitude worse. Still, Hirschfelder and Magee concluded, there might be a technological fix. "The obvious solution," they wrote, "is to take steps to prevent such dusts from getting there."[98] The best way to do that would be to pave the Trinity site in concrete and oil. Ultimately, Bainbridge decided to do just that.

Hirschfelder and Magee's conclusions were met with some skepticism. Hempelmann and his fellow medical colleague, James Nolan, thought Hirschfelder and Magee's conclusions much too pessimistic. Assuming that Hirschfelder and Magee were correct, they maintained, the actual danger from radiation to the exposed populations would be minimal. The problem was not so much in the meteorological projections, but in the actual biological hazards posed by the radiation exposures, their special province of expertise. The danger from inhaling plutonium was "nil," they argued, because larger

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[97] J.O. Hirschfelder and John Magee to K.T. Bainbridge, "Danger from Active Material Falling from Cloud - Desirability of Binding Soil Near Zero with Concrete and Oil,: June 16, 1945, NTA, NV0059784. This memo can also be found in in L.H. Hempelmann, "Preparation and Operational Plan of Medical Group (TR-7) for Nuclear Explosion 16, July 1945," June 13, 1947, Report # LA-631, LANL-RL,
[98] Ibid.

diameter particles would be "filtered out completely by the nose." Ingestion, too, would be unlikely as an individual would have to consume an amount of plutonium "distributed over 5000 sq. feet of surface." The danger from fission products presented a more real hazard, but even those exposures "would seem extremely improbable...in the case of people not previously exposed to radiation." In the case of external gamma radiation, the estimated dose of 68r in the first fourteen days would not result in any permanent injury. In fact, they maintained, a "normal person could probably stand two or three times this amount without sustaining permanent bodily damage." The dangers from the ingestion of fission products were similarly negligible for much the same reason as plutonium: a person would have to ingest approximately five hundred square centimeters of material distributed over the ground surface. Even then, "it is probable that ten to thirty times the...amount could be safely ingested" because most of these radionuclides would be short-lived.[99]

There the matter stood for three weeks. On July 6th, Hirschfelder and Magee wrote to Bainbridge with revised figures for the fallout problem. The subject line of the memo, "Improbability of Danger from Active Material Falling from Cloud," summed up their new conclusions. In their previous memo of June 16th, they had assumed that all of the fission products would condense onto particles picked up from the ground during the explosion. In their revised estimation, they figured that much of the radioactivity would form very small particles that would remain suspended in the atmosphere until it would become diffuse. The difference was considerable as it "would seem to indicate that the

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[99] L.H. Hempelmann and James F. Nolan to K.T. Bainbridge, June 22, 1945, "Danger to Personnel in Nearby Towns Exposed to Active Material Falling from Cloud," NTA NV0062737.

amount of active material sedimenting onto a nearby town may be less by a factor of from 2 to 10."[100] This was reasonably good news seeing that the Trinity test was ten days away.

As the above discussion demonstrates, the fallout calculations that MED scientists conducted were in the main theoretical. To be sure, the 100-ton test provided some empirical evidence for how the post-shot fireball might function. If successful, however, the Trinity test might yield an explosive power one-hundred times greater than the 100-ton test. That fact alone put safety planning outside the range of experience. Still, even if scientists like Hirschfelder and Magee could not state with any certainty what the fallout picture might look like after Trinity, safety planners did draw on previous experience in radiation toxicology to develop plans to protect MED personnel and off-site communities should they be exposed to radiation. After all, medical officers at Los Alamos, Oak Ridge, and the Met Lab had spent the last three years researching the toxicity of radiation and instituting protective measures inside the bomb-building factories to ensure that workers were not injured.

It was precisely that kind of toxicological experience that helps explain why Hempelmann and Nolan largely dismissed Hirschfelder and Magee's fallout predictions. When medical officers began looking at the problem of fallout, they pictured circumstances and conditions that were not unlike those of the factory. Consider, for example, how Hempelmann and Nolan in their critique of Hirschfelder and Magee downplayed the hazards associated with internal exposures. Neither inhalation nor

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[100] J.O. Hirschfelder to K.T. Bainbridge, "Improbability of Danger from Active Material Falling from Cloud," July 6, 1945, NTA, NV0059903.

ingestion appeared to be much of a concern because they could not imagine how an individual would be able to inhale or consume an above-tolerance dose if the fallout particles were evenly distributed over a wide swath of ground. None of the safety planners addressed the possibility that over exposures could occur from the contamination of water or foods. This blindness to internal exposures seems ironic given that medical officials in the factory placed so much emphasis on the ingestion route of exposure. Yet, when one considers the assumptions that MED health officials made about the role of the environment in mediating exposure to radiation, the discrepancy does not seem so illogical. In the factory, the potential ingestion of radioactive particles was direct; workers who failed to assiduously attend to the housekeeping rules of the factory floor risked exposing themselves simply by smoking or eating. For health physicists, the combination of worker "hygiene" and workplace engineering to "sanitize" the factory environment ensured safety. Yet, the unrecognized assumption undergirding this approach to health and safety was that the environment was passive and fixed. Thus Hempelmann and Nolan's under-appreciation of the potential internal exposure pathways was shaped by the notion that place did not matter; an individual could consume a dangerous amount of radiation only if they ate it *directly off the ground*.

Hempelmann and Nolan were not the only ones to downplay internal exposures. Health and safety officials generally held that external gamma radiation exposures posed the greatest danger to MED personnel and off-site populations. One of the main questions left as the Trinity date swiftly approached was how much gamma radiation safety officials would permit individuals to be exposed. That question was raised during one of the final Trinity hazards meetings on July 10th which included Hirschfelder, Stafford Warren and Oppenheimer. Warren informed the group that he thought a total dose of "60r in two weeks as safe." Even then, he added, "100r would not be harmful provided there would be no further exposure to radiation." Still, in the event of serious fallout, the safety team would "make measurements for several hours and consider evacuation if [the] total dose reached [a] final total of 60-100r."[101] As I mentioned earlier in this chapter, such figures were extraordinarily excessive, even by the standards of the day. The external radiation dose standard adopted by the International Commission on Radiation Protection in 1934 was .2r per day, or 36.5r per year.[102] Given the imperative of ensuring a successful test, the risks seemed worth taking. Tellingly, however, the question of internal hazards was never broached at the meeting.

Discussion of Hirschfelder and Magee's fallout calculations at the July 10th meeting comprised the extent of any environmental considerations for the Trinity test. There were many unknowns in Hirschfelder and Magee's fallout models to be absolutely sure of what the fallout situation might look like after Trinity. Yet, even then, safety planners assumed that, given the right meteorological conditions, most fallout would remain within the boundaries of the test site or become diffuse as it travelled through the atmosphere. At worst, if the right conditions were met, the danger would end "after about 2 1/2 hrs."[103] But that all depended on the weather. Hubbard, in due course,

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[101] "Conference About Contamination of Countryside Near Trinity With Radioactive Materials, July 10, 1945, in L.H. Hempelmann, "Preparation and Operational Plan of Medical Group (TR-7) for Nuclear Explosion 16, July 1945," June 13, 1947, Report # LA-631, LANL-RL,
[102] Szasz, *The Day the Sun Rose Twice: The Story of the Trinity Site Nuclear Explosion, July 16, 1945*, 118.
[103] "Conference About Contamination of Countryside Near Trinity With Radioactive Materials, July 10, 1945, in L.H. Hempelmann, "Preparation and Operational Plan of Medical Group (TR-7) for Nuclear Explosion 16, July 1945," June 13, 1947, Report # LA-631, LANL-RL,

developed a list of best conditions for the test. The list included, in part, high visibility, humidity below eight-five percent, little or no inversion between 5,000 and 25,000 feet, low-level winds, and above all, no precipitation.[104] All these conditions were predicated on the understanding that they afforded the greatest opportunity for atmospheric control. Hubbard also developed long-range forecasts for the test and, in early July, determined that the most favorable dates for detonating the bomb would be between July 18th and 20th.[105]

The question of weather also affected plans for the radiation monitoring teams that were to be placed in strategic positions through the surrounding region. Joe Hoffman, who headed the off-site monitoring teams, developed a flexible plan that would enable monitors to move freely to track the Trinity cloud.[106] Accordingly, each team was equipped with a car or jeep. Each team was also required to wear protective clothing (including gas masks or respirators) and carry various rad-monitoring devices to measure the radiation. Hoffman placed four monitoring teams in Roswell, Socorro, Fort Sumner, and Carrizozo, effectively creating an umbrella north of the test site, the expected trajectory of the Trinity cloud.[107] From these positions, monitors were to take readings in town and along highways after the test and report by radio to the Trinity base camp where Stafford Warren was located. Some of these towns were located more than fifty miles

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[104] Adapted from J.M. Hubbard, "July 16th Nuclear Explosion: Meteorological Report," September 10, 1945, Report # LA-357, LANL-RL.
[105] Szasz, *The Day the Sun Rose Twice: The Story of the Trinity Site Nuclear Explosion, July 16, 1945*, 71.
[106] J. Hoffman, "Changes and Supplement to Town Monitoring," July 7, 1945, NTA, NV0059901.
[107] n.a., "Final Plans for Monitoring and Evacuation N.E. and N.W. Regions as of 14 July 1945," in in L.H. Hempelmann, "Preparation and Operational Plan of Medical Group (TR-7) for Nuclear Explosion 16, July 1945," June 13, 1947, Report # LA-631, LANL-RL. See also K.T. Bainbridge, "Trinity," May 1976, Report number LA-63000-H, LANL-RL, 33-5.

from the test site, which seemed more than adequate to Hempelman because, he felt, it is "probable that the danger area will not extend beyond thirty miles."[108]

Unlike the 100-ton test, meteorologist Jack Hubbard was not given authority to determine the test date for Trinity. Instead, political considerations intervened. President Truman was scheduled to meet with Joseph Stalin and Winston Churchill on July 16th at Potsdam, a suburb outside of Berlin, to discuss the future of eastern Asia once hostilities ended. Worried about Soviet designs in the region following a Japanese surrender, Truman wanted confirmation of a successful test of the bomb. If the bomb worked as expected, Truman hoped to use the news of the weapon as a negotiating tool against Stalin. Political imperatives thus took priority over safety issues. The date of the Trinity test would be July 16th, weather permitting or not.

The problem was that on July 6th Hubbard had forecast thunderstorms beginning July 13th which would likely last for four days.[109] The date of the 16th, then, promised the worst of all scenarios—rain. The fixing of the date forced Hubbard to reconsider the relationship between weather and the test; if testing the bomb under "best" conditions was no longer a priority, then Hubbard would have to determine the "minimum specifications under which the operation could be conducted."[110] Fixing the date also meant, especially considering the possibility of rain, that Hubbard would have to provide

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[108] L.H. Hempelmann to K.T. Bainbridge, "The Influence of Meteorological Conditions on the Monitoring and Evacuation Plans of the Medical Group," July 14, 1945, NTA, NV0123770.

[109] J.M. Hubbard, "July 16th Nuclear Explosion: Meteorological Report," September 10, 1945, Report # LA-357, LANL-RL, 7.

[110] Quoted in Szasz, *The Day the Sun Rose Twice: The Story of the Trinity Site Nuclear Explosion, July 16, 1945*, 71.

hour-by-hour forecasts. If the weather provided even the slimmest window that met the minimum requirements, they would have to take it.[111]

Just as Hubbard had predicted, in the early hours of the 16th the Trinity Site was awash in heavy thunderstorms. As scientists were loading the plutonium core into the bomb which was situated on top of a hundred-foot tower, cloud-to-cloud lightening lit up the sky. Despite the rain, Hubbard phoned Bainbridge notifying him that by 5:30 a.m. conditions would be more favorable for detonating the bomb. When Hubbard met with Groves shortly after phoning Bainbridge, Groves threatened to hang him if his forecast was wrong.[112] He wasn't. "Sky now broken becoming scattered at 5:15," Hubbard reported.[113] Although conditions were far from perfect (humidity was still above eighty percent), the bomb was detonated fifteen seconds short of 5:30 a.m [figure 7].

The explosion produced a brilliant light the equivalent of almost twenty suns.[114] Enrico Fermi, who was five and a half miles from the explosion, estimated after experiencing the blast wave that the yield of the bomb was roughly 10,000 tons of TNT. He was almost exactly right. All who witnessed the event were awestruck. Oppenheimer famously quoted from the Bhagavad-Gita, "I am become death, the shatterer of worlds." Most MED personnel, however, had little time for reflection. After a quick calculation, Stafford Warren estimated that the cloud from the bomb rose 70,000 ft in roughly fifteen minutes.[115] However much awe might have been experienced by the radiation monitors,

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[111] J.M. Hubbard, "July 16th Nuclear Explosion: Meteorological Report," September 10, 1945, Report # LA-357, LANL-RL,

[112] Szasz, *The Day the Sun Rose Twice: The Story of the Trinity Site Nuclear Explosion, July 16, 1945*, 77.

[113] J.M. Hubbard, "July 16th Nuclear Explosion: Meteorological Report," September 10, 1945, Report # LA-357, LANL-RL, 9.

[114] Szasz, *The Day the Sun Rose Twice: The Story of the Trinity Site Nuclear Explosion, July 16, 1945*, 83.

[115] Warren, "The Role of Radiology in the Development of the Atomic Bomb," 884.

seeing the massive fireball would have certainly alerted them to their more pressing post-shot responsibilities—tracking the cloud and monitoring fallout [Figure 8].

The early radiation readings from off-site monitors recorded very little fallout radiation. The first report that Warren received back at Base Camp was from one of the searchlight crews who had been stationed at strategic positions to illuminate the cloud in the dark early morning hours. Around 8:00, they radioed Warren a reading of 0.11r per hour. which was significantly low. It was just about that time, however, that Arthur Breslow encountered the "strata of sand like dust" and as a precaution began breathing through a slice of bread. He was traveling along highway 380 about 20 miles north of the Trinity Site toward the L-8 searchlight station near Bingham. When Breslow arrived at L-8, the crew stationed there informed him that "there was a danger of an immediate evacuation and the count was rapidly rising."[116] Hirschfelder and Magee had in fact just been at L-8 only to find the crew there grilling up steaks in celebration of the bomb. Then, almost immediately, dust particles began to rain down on the men and the steak. Hirschfelder and Magee ordered them to bury their steaks and evacuate.[117] Radiation levels were 2.0r per hour and Breslow promptly left to go evacuate L-7.[118]

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[116] Arthur Breslow to J.G. Hoffman, "Radiation Monitoring and Itinerary," u.d., in Joseph G. Hoffman, "Nuclear Explosion 16 July 1945. Health Physics Report on Radioactive Contamination Throughout New Mexico Following Nuclear Explosion. Part C: Transcript of Radiation Monitor's Field Notes. Film Badge Data on Town Monitoring," December 31, 1945, NTA, NV0059839.

[117] Hirschfelder, "The Scientific and Technological Miracle at Los Alamos," 77. See also "L-8 Search Light Crew," July 16, 1945, in Joseph G. Hoffman, "Nuclear Explosion 16 July 1945. Health Physics Report on Radioactive Contamination Throughout New Mexico Following Nuclear Explosion. Part C: Transcript of Radiation Monitor's Field Notes. Film Badge Data on Town Monitoring," December 31, 1945, NTA, NV0059839.

[118] Arthur Breslow to J.G. Hoffman, "Radiation Monitoring and Itinerary," u.d., in Joseph G. Hoffman, "Nuclear Explosion 16 July 1945. Health Physics Report on Radioactive Contamination Throughout New Mexico Following Nuclear Explosion. Part C: Transcript of Radiation Monitor's Field Notes. Film Badge Data on Town Monitoring," December 31, 1945, NTA, NV0059839.

Magee stayed near L-8 to take more readings. At 8:30, he recorded levels as high as 20r per hour just a few miles from L-8.[119] Meanwhile, Hoffman found radiation readings of approximately 15r along highway 380. He radioed Base Camp notifying them that this area would reach ninety percent of tolerance dose.[120] That was Hoffman's last radio communication; in the course of tracking the fallout, he was moving out of radio range.[121]

At 10:30, Hempelmann left base camp "in a car with good radio" and met up with Hoffman, Hirschfelder, and Magee in Bingham around noon. At the conference, Hoffman and the others determined that the dangers from alpha particles were negligible based upon recent measurements. The real worry was the gamma measurements made by Magee. It was decided that some members of the monitoring teams would continue surveying areas throughout the region while Hoffman would go to the "hot" area reported by Magee to confirm his findings.[122] The subsequent survey of ranches near Magee's hot fallout zone showed relatively low-level radioactivity. Yet, gamma measurements taken in a steep gorge through which a rural road ran corroborated Magee's initial readings. Rad-monitors dubbed the gorge "Hot Canyon." Because none of the monitors' maps indicated that there were any inhabitants near Hot Canyon, they discontinued surveying

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[119] Szasz, *The Day the Sun Rose Twice: The Story of the Trinity Site Nuclear Explosion, July 16, 1945*, 126.

[120] Joseph G. Hoffman, "Nuclear Explosion 16 July 1945. Health Physics Report on Radioactive Contamination Throughout New Mexico Following the Nuclear Explosion. Part A: Physics," February 20, 1947, Report # LA-626, LANL-RL, 40.

[121] "Events in Camp Immediately Following Shot—July 16, 1945," n.d., NTA, NV0039287. This documents can also be found in L.H. Hempelmann, "Preparation and Operational Plan of Medical Group (TR-7) for Nuclear Explosion 16, July 1945," June 13, 1947, Report # LA-631, LANL-RL,

[122] Joseph G. Hoffman, "Nuclear Explosion 16 July 1945. Health Physics Report on Radioactive Contamination Throughout New Mexico Following the Nuclear Explosion. Part A: Physics," February 20, 1947, Report # LA-626, LANL-RL, 12-13. See also "Events in Camp Immediately Following Shot—July 16, 1945," n.d., NTA, NV0039287.

the area and and moved on.[123] By the end of the day, despite some of the shocking levels of fallout, monitors and MED officials decided that evacuation of any surrounding communities was unwarranted; radiation levels were quickly decaying and there was no indication that any bystanding persons were exposed to above-tolerance doses.[124]

The next day, however, Hempelmann and Hymer Friedell drove out to Hot Canyon to inspect the area. About a mile away from Hot Canyon, they stumbled upon an adobe house occupied by the Raitliff family. The Raitliff ranch was not on any of the monitors maps. Hempelman and Friedell took some gamma radiation measurements and decided against evacuating the family because the intensities at that time were low. The next day, Warren, Hempelmann, and Friedell left for Los Alamos.[125] The Raitliff's remained on the radiation monitors' minds, however. At a monitors meeting on the 19th, they discussed the family's exposure and decided definitively against evacuation. Instead, they agreed to monitor the family over the next six months to "determine whether their health had been affected."[126]

The first follow-up on the Raitliffs occurred in mid-August. When Warren and Hempelmann met with the family on the 17th, they began to piece together the radiation levels that the family was likely exposed to. The family consisted of a couple over fifty years of age and their ten year old grandson. On the morning of the Trinity test, the Raitliffs were completely unaware of the explosion. The first to learn of the explosion

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[123] Joseph G. Hoffman, "Nuclear Explosion 16 July 1945. Health Physics Report on Radioactive Contamination Throughout New Mexico Following the Nuclear Explosion. Part A: Physics," February 20, 1947, Report # LA-626, LANL-RL, 13.
[124] Hacker, *The Dragon's Tail: Radiation Safety in the Manhattan Project, 1942-1946*, 104.
[125] "Events in Camp Immediately Following Shot—July 16, 1945," n.d., in L.H. Hempelmann, "Preparation and Operational Plan of Medical Group (TR-7) for Nuclear Explosion 16, July 1945," June 13, 1947, Report # LA-631, LANL-RL, 13.
[126] Ibid., 14.

was the grandson who had left the ranch early in the morning by horseback, probably to attend school in nearby Bingham. Because he was in town during the shot and indoors at night, Hempelmann concluded that the boy "missed most of the heavy exposure of the first day." Mr. Raitliff, however, spent most of his day outside. When talking to Hempelmann, Mr. Raitliff complained of nervousness, bad teeth, and tightness in in chest. Hempelmann judged these to be of little interest radiologically because they "are not new symptoms."[127] Animals on the ranch displayed classic signs of severe radiation exposure. The cattle and dogs all showed burns on their backs, likely from exposure to beta particles. One of the dogs suffered from a bleeding foot.[128] Judging from the gamma radiation measurements, Hempelmann figured that over the course of two weeks since the Trinity test, the family exceeded the tolerance dose by a factor of thirty-three.[129] Still, the family was not ordered to evacuate nor were they told of their radiation exposures. No connection was made between the irradiated cattle and the health of the family. Nor was there any discussion of the potential internal doses to the cattle who would have surely ingested a considerable amount of radiation by grazing on contaminated grasses. MED officials later learned of other families not initially

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[127] "Itinerary of Trip Made by Colonel Warren, Captain Whipple and L.H. Hempelmann on 12 August 1945," August 17, 1945 in L.H. Hempelmann, "Nuclear Explosion 16 July 1945: Health Physics Report on Radioactive Contamination Throughout New Mexico, Part B: Biological Effects," July 3, 1947, Report # LA-638, LANL-RL, 8.

[128] Hacker, *The Dragon's Tail: Radiation Safety in the Manhattan Project, 1942-1946*, 105.

[129] L.H. Hempelmann, "Itinerary of Trip made by Colonel Warren, Captain Whipple and L.H. Hempelmann on 12 August, 1945," August 17, 1945, in L.H. Hempelmann, "Nuclear Explosion 16 July 1945: Health Physics Report on Radioactive Contamination Throughout New Mexico, Part B: Biological Effects," July 3, 1947, Report # LA-638, LANL-RL 9.

accounted for on July 16th. The Wilson ranch, located near the Raitliff's, experienced similar exposure levels. No evacuation order was given to them either.[130]

By October, all seemed well. None of the families that MED health officials monitored appeared in poor health and none showed signs of radiation sickness. The same could not be said for some livestock that were grazing in the heaviest zone of fallout. On October 11, the Red Canyon Sheep Company filed suit against the government for damage to the company's cattle.[131] Red Canyon's cattle showed injuries similar to the Raitliff's—loss of hair and burns on the back and sides. Over 600 cattle were affected and eventually the army bought seventy-five of the most injured and shipped them to Los Alamos for study.[132] Robert Stone inspected the animals while on his way to California from Chicago. He estimated that the cattle were likely exposed to "between 4,000 roentgens and 50,000 roentgens, probably about 20,000 roentgens"—enough gamma radiation to kill a human being within a single day.[133] Still, no correlation was made between the cattle and people; the health of the remaining irradiated cattle not purchased by the army ostensibly held little or no danger to beef consumers. Subsequent post-mortem analyses of a couple euthanized cattle paid no attention to the possibility that the cattle might have ingested significant beta or alpha particles. Instead, the autopsies focused on the whether the internal organs appeared

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[130] Hacker, *The Dragon's Tail: Radiation Safety in the Manhattan Project, 1942-1946*, 104-5.
[131] L.H. Hempelmann to Trinity Files, "Cattle Northeast of the Alamogordo Bombing Range Allegedly Damaged by the Nuclear Explosion 16 July 1945," in L.H. Hempelmann, "Nuclear Explosion 16 July 1945: Health Physics Report on Radioactive Contamination Throughout New Mexico, Part B: Biological Effects," July 3, 1947, Report # LA-638, LANL-RL, 13.
[132] Hacker, *The Dragon's Tail: Radiation Safety in the Manhattan Project, 1942-1946*, 106.
[133] LHM to Trinity Files, "Estimation of dose of beta radiation received by cattle," February 10, 1946, NTA, NV0090365.

normal; no bioassays measuring the bioconcentration of radiation in specific organs were conducted.

Los Alamos held onto the animals until 1952 when they were shipped to Oak Ridge for long-term study. Thomas Shipman, the head of the Health Division of the recently created Los Alamos Scientific Laboratory, lamented the transfer. "I certainly had my mouth all set for a nice piece of barbecued beef, and it now looks as though that pleasure will be enjoyed in Tennessee."[134] Five years into the atmospheric nuclear weapons testing period, the cattle still seemed benign from a human exposure standpoint.

Conclusion: The Engineering of Simplified Ecologies

When Stafford Warren wrote an account of the activities of the MED Health Division in 1966, he did not even devote a single page to the Trinity fallout. What he did write is revealing, nonetheless. MED officials, he informed his readers, assumed that most of the fallout produced by the Trinity test would remain within the boundaries of the test site. During the test, however, there was a "skip" in the fallout pattern. "The failure to detect this skip in the first test" he noted, "gave the false impression that the fallout had been restricted to the test area and also produced the impression that it was less dangerous than subsequent investigations proved it to be." This "detection" failure resulted in significant radioactive contamination of "Uninhabited upper stretches of the Chupadera Mesa…about 20 to 30 miles from point zero." Warren knew full well, of course, that Chupadera Mesa was not "uninhabited"; Chupadera Mesa was home to "Hot

[134] T.L. Shipman to Carroll L. Tyler, "Cattle for Division of Biology and Medicine," September 5, 1952, NTA, NV0079097.

Canyon"—and the Raitliffs. Despite Warren's prevarication, his acknowledgment of widespread fallout beyond the boundaries of the Trinity Site is telling. The assumption that fallout could be bounded within the arbitrary confines of the test site reflected the simplistic picture of the environment that MED officials held when considering the hazards associated with the Trinity test. As Warren's reflections suggest, such human constructions failed to account for the meteorological realities that structured the unbounded movement of fallout. That fallout might cross the human boundaries constructed by the MED was nearly unthinkable. Hirschfelder noted after the war that "very few people believed us when we predicted radiation fallout from the atom bomb, [but] they did not dare to ignore the possibility."[135] The possibility was there, it just was not a likely one.

Other considerations of the radiological hazards associated with Trinity equally reflected this simplistic view of the environment. Why, for instance, did health officials like Warren and Hempelmann focus so keenly on the dangers of external gamma exposure and not internal exposures from ingested beta or alpha particles? Scholars have noted the delicate balancing act health officials had to negotiate between military pressures to build the bomb quickly while concomitantly ensuring radiation safety.[136] As Barton Hacker has argued, "Radiological safety…competed with other test goals; it ranked higher than most, perhaps, but not highest."[137] To be sure, prioritizing military concerns above safety helps to explain why Warren established such high standards for

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[135] Hirschfelder, "The Scientific and Technological Miracle at Los Alamos," 75.
[136] Hacker, *The Dragon's Tail: Radiation Safety in the Manhattan Project, 1942-1946*; Hacker, *Elements of Controversy: The Atomic Energy Commission and Radiation Safety in Nuclear Weapons Testing, 1947-1974*; Walker, *Permissible Dose: A History of Radiation Protection in the Twentieth Century*.
[137] Hacker, *The Dragon's Tail: Radiation Safety in the Manhattan Project, 1942-1946*, 85.

external radiation doses to off-site communities. It does not, however, clarify why MED officials put such great emphasis on the external gamma dose hazard and so little on *internal* doses.

In this chapter, I have traced the development of the nascent field of health physics within the MED to explain how the foundational structure of radiation safety (health physics) during the atmospheric nuclear weapons testing period was assembled out of core toxicological concepts, methods and practices. I have argued that by adopting toxicology as the wellspring of radiation safety, health physicists delimited the material interactions between human bodies and the environment. Because toxicology was a laboratory-based discipline, its practitioners were predisposed to thinking that what mattered ultimately in protecting health was not the environment, but the particular chemicals under study. As Robert Kohler has perceptively argued, laboratories derive their epistemological power precisely by their ability to repress the messy complexities of the environment.[138] For toxicologists, toxic chemicals only become agents of disease when their effects can be clearly and directly delineated in the laboratory. Yet, as Linda Nash has written, such an approach to human health has historically rendered environments outside the laboratory as "homogenous spaces...with no agency of its own in the production of disease."[139] For the health physicists charged with protecting off-site populations during the Trinity test, this kind of environmental blindness shaped their assessment of the radiological hazards from fallout; external exposure from gamma radiation was the more risky fallout danger. Internal exposures were only a threat if

[138] Kohler, *Landscapes and Labscapes: Exploring the Lab-Field Border in Biology*.
[139] Nash, *Inescapable Ecologies: A History of Environment, Disease, and Knowledge*, 13.

persons were to consume radiation directly from the ground. The possibility that irradiated animals, vegetation, or the vagaries of atmospheric conditions might mediate radiation exposure were distant concerns, if they were concerns at all. In short, health physicists assumed bodies to be discrete bounded entities.

The notion of a bounded body, however, was accompanied by a view that the environment could be similarly bounded. Although much of the scholarship on toxicology within environmental history has focused on the modernist separation of human bodies and environments, I have argued that the idea of spatial containment and boundedness was equally a legacy of the toxicological approach to human health. As Warren's comments above demonstrate, MED officials initially assumed that the most dangerous fallout from the Trinity test would be spatially contained within the boundaries of the test site. The core problem that health physicists faced when they moved from laboratory studies of radiation effects to the institution of protection standards in the factories was how to control the environmental variables that would undoubtedly complicate human exposures. Engineering those spaces so that the factory more closely approximated the controls established in the laboratory, as I have argued, provided the answer. Ventilation hoods, dust boxes, personal hygiene, were all key protective methods and devices designed to ensure good factory housekeeping. The experience protecting workers in this simplified environment, however, reinforced the idea that radiation in environments outside of the factory was also subject to this same kind of modernist technoscientific control and could thus be similarly protected.[140] This "high-

[140] My thinking here has been influenced by LeCain, *Mass Destruction: The Men and Giant Mines That Wired America and Scarred the Planet*.

modernist" faith in technoscientific rationality resulted in health physicists radically simplifying the environment of the Jonada del Muerto Valley, as weather apparently could not violate boundaries.[141] When health physicists looked at Trinity and the surrounding landscape they saw a factory. And like a factory, there were walls at Trinity. They just happened to be virtual—inked lines on maps showing the borders of the test site.

Despite the problems associated with the Trinity test, health physicists never wavered in their confidence in environmental control. For roughly the next ten years, risk assessments of the post-war nuclear weapons testing program habitually downplayed internal radiation exposures and health physicists continued to rely on the discourse and practices of technoscientific control to assure Americans (and the world) that weapons testing was safe.

In the next chapter, I will explore the establishment of a new test site at an atoll in the Marshall Islands which scientists dubbed the "greatest laboratory in the world."

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[141] For more on high-modernism, see James C. Scott, *Seeing Like A State: How Certain Schemes to Improve the Human Condition Have Failed* (New Haven: Yale University Press, 1998).

Figure 3 - Robley Evans pocket dosimeter circa 1932. Photo courtesy of Health Physics Historical Instrumentation Collection. http://www.orau.org/ptp/museumdirectory.htm

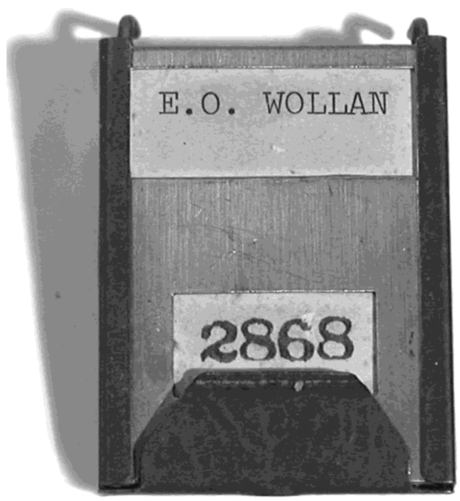

Figure 4 - Oak Ridge health physicist Ernest Wollan's film badge. Photo courtesy of Health Physics Historical Instrumentation Collection. http://www.orau.org/ptp/museumdirectory.htm

Figure 5 - Fish pole meter. Photo courtesy of National Radiation Instrument Catalog http://national-radiation-instrument-catalog.com/.

Figure 6 - 1947 Oak Ridge health physics poster. Note the hand and foot counter and the importance of "hygiene." Photo courtesy of Health Physics Historical Instrumentation Collection.

Figure 7 - Trinity. Source: Wikipedia. Public domain.

Figure 8 - Trinity radiation monitors. Note gas masks. Photo courtesy of National Security Administration/Nevada Site Office.

"THE GREATEST LABORATORY EXPERIMENT OF HISTORY": OPERATION CROSSROADS AND THE BEGINNING OF POSTWAR NUCLEAR WEAPONS TESTING

> "Test *is something of a misnomer when it comes to nuclear bombs. A test is controlled and contained, a preliminary to the thing itself, and though these nuclear bombs weren't being dropped on cities or strategic centers, they were full-scale explosions in the real world, with all the attendant effects*." — Rebecca Solnit.[1]

In late December of 1946, Rear Admiral William Parsons delivered a speech before the American Association for the Advancement of Science in Boston entitled "Results of Operation Crossroads." Crossroads was the first postwar nuclear weapons test and was designed to measure and evaluate the effect of the atomic bomb on naval warships. Conducted at Bikini Atoll in the Marshall Islands the previous summer, the operation consisted of an air and underwater detonation within the atoll's lagoon where the Navy had anchored a mothball fleet of obsolete American, German, and Japanese ships and submarines. Parsons served as deputy commander for the tests. Crossroads, however, came at an inauspicious time. Less than a year previous, the United States had dropped two atomic bombs on Hiroshima and Nagasaki, signaling the advent of the atomic age and ushering in a profound and lasting anxiety about the destructive power of "the Bomb." With two single bombs, the United States had seemingly brought the Japanese to their knees. Yet it was not only the sheer devastation wrought by the bomb that worried the world; stories of the effect of the bomb's radiation on the Japanese

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[1] Rebecca Solnit, *Savage Dreams: A Journey into the Hidden Wars of the American West* (Sierra Club Books, 1994), 5.

equally consumed the macabre imagery elicited by this new technological terror.[2] The bomb in many people's mind seemed to symbolize science and technology run amok. Like a spoiled child soon grown unwieldy, the bomb born in the laboratory had similarly outgrown its parent's control. Indeed, during the operation debates raged domestically and internationally about the future of atomic energy. In part, Parson's speech was intended to quell such fears by reasserting the Navy's technoscientific control over the Crossroads tests. As Parson's emphasized at the conclusion of his speech:

> I would summarize Operation Crossroads by saying that it was the largest *laboratory experiment in history*, conducted by a Task Force of 40,000 men, 4,000 miles west of the West Coast. Not one man was injured either by the blast or radio activity [sic] from the bomb experiment…tens of thousands of people have seen two atomic bombs set off under *controlled conditions* and have thereby gone part of the way toward substituting a rational fear of the known for an irrational fear of the unknown in this Atomic Age.[3]

Parsons' invocation of Crossroads as an experimental test within the laboratory of Bikini is telling. Although laboratories, then as now, have been associated popularly with scientists robed in lab coats and fiddling about with beakers and test tubes, they were also understood as self-contained and discrete spaces in which scientists studied and controlled phenomena, free from external influences. That is, interesting things happened within their walls, but not outside them—they were perfect vehicles for controlling the world. Indeed, that is the kind of cultural resonance evoked by Parsons' speech. The bomb could not have elicited widespread effects or injured anyone precisely because they

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[2] See Paul S. Boyer, *By the Bomb's Early Light: American Thought and Culture at the Dawn of the Atomic Age* (New York: Pantheon, 1985), 188-9. The allusion to the death star is intentional.

[3] William Parsons to AAAS, December 27, 1946, SIO Subject Files Records, 1890-1981, AC6 (hereafter SIOSF), Scripps Institute of Oceanography Archives (hereafter SIOA), box 18, folder 5. Emphasis mine.

were set off under *controlled conditions*. The entire phenomena, in other words, was localized within the "walls" of the Bikini laboratory.

Parsons' reference to Bikini as laboratory and Crossroads as an experiment proved more than rhetoric, however. He was not wrong in describing it as fundamentally scientific in character. Among the 40,000 men he referred to who participated in the tests, were a number of scientists and technicians from the military and academe. Not all, nor even a majority, were physicists. Most hailed from what would later be called the environmental sciences (both physical and biological) at universities and institutions as diverse as the University of Washington, Stanford University, the Smithsonian National Museum, the Fish and Wildlife Service and Scripps Institute of Oceanography, to name but a few. Their job during Crossroads was to survey and study the effect of the bomb on Bikini's geology, oceanographic processes, and organisms. As head of the Crossroads scientific team Roger Revelle noted, "in order to gain a maximum technical value from the weapons tests, a thorough understanding of the physical and biological environment of the proving ground was desirable."[4] It just so happened that study of the bomb effects also afforded an opportunity for fundamental scientific work under the patronage of the U.S. Navy. Indeed, by the mid-1950s, Revelle could boast in all seriousness that Bikini Atoll was "perhaps the most thoroughly known atoll on earth."[5]

What is striking about the Crossroads scientific expedition (and the Bikini Scientific Resurvey a year later) was not only the new kinds of military patronage networks that were being forged in the early postwar years, but that the scientists

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[4] Roger Revelle, "Foreword," in *Geology of Bikini and Nearby Atolls: Part 1, Geology*, ed. Kenneth O. Emery, Jr. J.I. Tracey, and H.S. Ladd (Washington D.C.: U.S. Government Printing Office, 1954), iii.
[5] Ibid.

participating in the operation also considered Bikini to be an ideal laboratory. Across the spectrum of scientific disciplines that the Crossroads scientists represented, nearly all believed that Bikini was a "great natural laboratory." Although Bikini didn't have walls, its relative disconnectedness from pelagic processes rendered it as a relatively self-contained space, not unlike a modern laboratory. Here, they believed, one could produce authentic scientific knowledge epistemologically on par with the most reductionist kind laboratory science. The motivations for representing Bikini as a laboratory were thus qualitatively different from the more overtly political imperatives of the Navy. Nonetheless, the practical result was the same: Bikini was a bounded space where phenomena and effects could be studied in isolation.

In this chapter, I build upon my analysis of the spatial and environmental assumptions that informed how scientists assessed the health risks associated with nuclear fallout during the Manhattan Engineering District to the immediate postwar period. In particular, I focus on Operation Crossroads and the Bikini Scientific Resurvey to show how the concept of a nuclear proving ground as a bounded space was reinforced and instantiated in the discourse of Bikini as a laboratory. As with Trinity, such ideas reflected and reproduced the idea that fallout effects would be localized within the proving ground. I also investigate how the basic science conducted as part of the surveys reinforced the notion of Bikini as a laboratory space and how the postwar testing program opened up new places, opportunities, and patronage relationships for the environmental sciences. It was during the early postwar period that knowledge of the environment as a strategic tool in military affairs was first elaborated. Finally, I pay close attention to the biological work of Lauren Donaldson and the Applied Fisheries Laboratory. Their research on the ecological uptake, movement, and circulation of fallout at Bikini marked a critical first step in realizing the dangers of ingested radioactivity. Their work also demonstrated the profoundly useful ways that radiation could be used as a tracer to delineate the structure and metabolism of aquatic ecosystems. Yet, as I point out in the the conclusion, these insights were soon lost on the successor of the postwar weapons testing program, the Atomic Energy Commission. They continued to view the hazards of fallout radiation as a factory problem, despite the fact that the plankton and other organisms within the atoll were telling them otherwise. The world and indeed science was at a crossroads in 1946 and 1947, but understanding the environmental aspects of fallout was not.

Operation Crossroads: Back to the Laboratory

The dust had hardly settled over Hiroshima and Nagasaki when suggestions for pursuing nuclear weapons testing began to surface. As early as August 25, less than three weeks following the bombing of Nagasaki, Connecticut Senator Brien McMahon delivered a speech calling for a series of tests to determine “just how effective the atomic bomb is when used against the giant naval warships.”[6] McMahon was not alone in fearing the the potential repercussions of the atomic bomb on conventional military might. Days after McMahon's speech, Commanding General of the Army Air Forces Henry "Hap" Arnold brought before the Joint Chiefs of Staff (JCS) a similar proposal to

[6] Quoted in W. A. Shurcliff, *Bombs at Bikini: The Official Report of Operation Crossroads* (New York: Wm. H. Wise & Co., Inc., 1947), 10. This book is the source for this paragraph.

test the effectiveness of the bomb on surviving Japanese vessels leftover from the war. From there, the proposal was sent to a special subcommittee for study. By January, the basic operational outlines of the tests were set and the JCS submitted its recommendations to President Truman for approval. The proposal called for a joint Naval and Army task force to conduct two simulated atomic attacks (one an air drop, the other an underwater burst) on a naval array.[7] A site would soon be selected too: Bikini Atoll, an island in the Marshall Islands recently put under trust by the United States government. Admiral William Blandy was designated commander of the operation and a May target date was selected.

When Blandy went before the Senate later in the month to announce the tests, his decision to codename the Operation Crossroads reflected the perceived stakes confronting the world in the wake of the advent of atomic weaponry. "Seapower, airpower, and perhaps humanity itself—are at the crossroads," Blandy explained portentously.[8] In retrospect, Blandy may have wished he had not flavored his words with so much salt. Few needed to be reminded that awesome power of the atomic bomb changed the future conduct of war and, for some, diplomacy. Indeed, the Navy and Truman faced challenges from many scientists and statesmen who questioned the wisdom of conducting atomic bomb tests while debates raged in regard to the future control of atomic energy. While plans were being made for Crossroads, Congress was busy negotiating legislation that would eventually result in the Atomic Energy Commission. Likewise, proposals for an international body to regulate atomic energy were being

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[7] A third planned deep underwater test was subsequently cancelled.
[8] Jonathan M. Weisgall, *Operation Crossroads: The Atomic Tests at Bikini Atoll* (Annapolis: Naval Institute Press, 1994), 32.

submitted to the newly formed United Nations. With regard to the latter debate, it is no small surprise that Crossroads was immediately held suspect as an instance of saber rattling, or atomic diplomacy.[9] As Caltech President Lee DuBridge asked in an article in the *Bulletin of Atomic Scientists*, "Are international relations to be improved by these tests?" He thought likely not: "One can do target practice with a gun (even a 16 inch gun) in his own 'backyard' without foreign complications. But brandishing atomic weapons is in a different class."[10] Yet, perhaps more importantly, the advent of atomic weaponry elicited a feeling of existential dread. Before the Japanese atomic bombings, the world had, of course, witnessed total destruction with the firebombing raids of Dresden and Tokyo. Still, never before had such devastation been wrought by a single weapon. For many, atomic weaponry shattered previously held notion of destructive scale—and with that the ability to control such a weapon as the atomic bomb. Thus, when Blandy waxed on about the world being at a crossroads, he touched a particularly sensitive nerve among many and not surprisingly faced criticism that suggested that the tests would wreak widespread environmental havoc.

Although some of these fears centered on the fantastic such as the possibility of the tests creating fissures in the earth's crust or perhaps igniting the ocean, other concerns were decidedly more plausible.[11] When the Navy consulted Pacific fisheries industry officials about testing at Bikini, for example, they expressed grave concerns that the

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[9] See, for example, Gregg Herken, *The Winning Weapon: The Atomic Bomb in the Cold War, 1945-1950, with a New Preface* (Princeton: Princeton University Press, 1988), 175-6. For an alternative view, see Lloyd J. Graybar, "The 1946 Atomic Bomb Tests: Atomic Diplomacy or Bureaucratic Infighting?," *The Journal of American History* 72, no. 4 (1986). Also, Lloyd J. Graybar, "Bikini Revisited," *Military Affairs* 44, no. 3 (1980).
[10] Lee A. DuBridge, "What About the Bikini Tests?," *Bulletin of Atomic Scientists* 1, no. 11 (1946): 8. L. A. DuBridge, *The New York Times*, May 5 1946.
[11] See Weisgall, *Operation Crossroads: The Atomic Tests at Bikini Atoll*, 63-5.

bomb might kill millions of fish (including whales and tuna) living beyond the atoll and permanently harm the economically valuable fisheries stock of the Pacific Ocean.[12] To assuage the fisheries industry, Blandy wrote to the Fish and Wildlife Service for technical advice on migration and spawning around the Marshall Islands. The day before Blandy went before the Senate to announce the tests, FWS director Ira N. Gabrielson wrote Blandy confirming the suitability of Bikini. From the data available, Gabrielson assured Blandy that Bikini was "not in a critical area with respect to tuna propagation, nor does it lie in important migration or nursery areas of other valuable food fishes." Therefore, Gabrielson concluded, "not only has an area for the *experiment* been chosen where the fisheries resources are negligible, or are of extremely *local* importance, but that in view of the safeguards which will be employed the fisheries of the Pacific Ocean at large, and particularly those of our Pacific coast, will suffer no appreciable damage whatever from the undertaking."[13] In other words, Bikini's isolation and remoteness precluded the possibility of any biological effects beyond the atoll. The Navy reiterated much the same argument when it justified its choice of Bikini as the test site. As one admiral put it in *Newsweek* after Blandy's announcement, Bikini was the safest place to conduct Crossroads because any potential bomb effects would "be directed over the great expanse of empty ocean." [Figure 9][14]

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[12] Shurcliff, *Bombs at Bikini: The Official Report of Operation Crossroads*, 19-20. See also Chief of Bureau of Ships to Director Navy Electronics Laboratory, "History of Oceanographic Section JTF-1—Forwarding of," May 27, 1946, SIOSF, SIOA, box 18, folder 5, pg 11.
[13] Letter reproduced in Special Senate Committee on Atomic Energy, *Hearings on Atomic Energy*, 79th Congress, 2nd sess., January 24 1946, 459-60.
[14] W. V. Pratt, "How Bikini Became the Bomb-Testing Ground," *Newsweek* 1946, 60.

Yet, as geographer Jeffrey Sasha Davis has perceptively argued, the remoteness of Bikini Atoll was not an a priori property inherent to Bikini. Rather this aspect of the apparent suitability of Bikini as a "place worthy of nuclear destruction" was, in part, discursively produced.[15] The most glaring problem with conducting Crossroads at Bikini was the fact that one hundred and sixty two native Bikini islanders inhabited the atoll—all of whom would have to be relocated for the operation. But rendering Bikini suitable for nuclear weapons testing demanded more than simply removal. To be "worthy of nuclear destruction" the atoll had to be spatialized as a marginalized "non-place"—in effect, a deserted isle unfit for human habitation. As Davis notes, the Navy's portrayal of Bikini as non-place took on the form of two related representations. For one, the Navy highlighted the few food resources available to Bikinians on the atoll, suggesting that Bikini was unhealthful and thereby justifying their removal to the seemingly more salubrious Kwajelein atoll. A similar tact utilized by the Navy stressed the supposed cultural inferiority of the Bikinians in comparison to the technological sophistication of the United States. That is, by virtue of American technological modernity the Navy held greater claim to Bikini than natives who used the atoll for merely subsistence purposes. In both representations, the Navy effectively delegitimized the Bikinian way of life and in the process marginalized them, the atoll, and their claim to their homeland.

Interestingly, Davis's analysis of the spatialization of Bikini as non-place was similarly presented to Americans to help sooth fears about possible widespread effects of

[15] Jeffrey Sasha Davis, "Representing Place: 'Deserted Isles' and the Reproduction of Bikini Atoll," *Annals of the Association of American Geographers* 95, no. 3 (2005): 614.

testing beyond the boundaries of the atoll. In this case, however, the Navy employed an equally powerful discursive strategy (to Western eyes, at least)—the language of experimentation and the laboratory. From its inception, the Navy portrayed the Crossroads tests as scientific experiments to be conducted within the laboratory-like conditions of Bikini atoll. When Blandy announced the tests, for example, he was at pains to convince the Senators that Crossroads was nothing more than a weapons-effects experiment. "I wish to emphasize," he told the Senate, "that this undertaking is not a combined or international operation, but rather a *scientific experiment* by the United States Government alone."[16] Throughout the months leading up to the tests, Blandy would repeat constantly a similar refrain: "This operation is joint scientific-military venture. It is not a military operation with scientific advisors."[17] By April, even the *New York Times* journalist William L. Laurence was praising the upcoming bomb tests as the "most stupendous single set of experiments in history."[18] I'll have more to say about the various surveys and field work attached to the Crossroads operation below, but it is worth considering how conceptions of the bomb tests as experiments and the two hundred and forty square mile Bikini as laboratory served the Navy's political ambitions.

As I noted above, the announcement of the tests generated considerable alarm about potential widespread destruction. Fears that the bomb might ignite the ocean or

[16] *Hearings on Atomic Energy*, 459.

[17] Quoted in Weisgall, *Operation Crossroads: The Atomic Tests at Bikini Atoll*, 117.

[18] William L. Laurence, "Stars' Secrets May Unfold to Myriad Guages at Bikini," *The New York Times*, April 24 1946, 1. The fact that Laurence played up the tests is of no surprise. He witnessed both the Trinity test and the Japanese bombings as a reporter for the *New York Times*. Although he won the Pulitzer Prize for his reporting on Trinity, recent scholarship has shown how he was a lackey for atomic energy interests, especially the military. In particular, he downplayed or suppressed news about radiation effects. See Beverly Deepe Keever, *News Zero: The New York Times and the Bomb* (Monroe: Common Courage Books, 2004).

produce tsunamis were all reflections of a generally held feeling following the bombings of Japan that atomic energy opened up a new kind of power that humans were incapable of controlling. Set within this context, Blandy and others' utilization of scientific language to describe tests conducted in the field as laboratory-style experimentation begins to make sense.

At least since the later part of the nineteenth century, westerners had been culturally conditioned to view labs as the premier space for the production of scientific knowledge. They were understood as isolated spaces where scientists studied natural objects without social or physical intrusion from the outside world. That is, by being cut off from the world (non-place), labs enabled scientists to control and isolate the particular object or phenomenon that they wished to study. Isolation and control were indeed synonymous with the lab itself.[19]

To be sure, western culture has been saturated with subversive representations of this ideal with the recurring motif of the "mad scientist" and the laboratory experiment gone awry (Frankenstein's monster immediately comes to mind). The zenith of those representations (Godzilla, the ants in THEM!, for example), however, were still a few

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[19] For sociological and historical analysis of laboratory-style knowledge production see Jan Golinsky, *Making Natural Knowledge: Constructivism and the History of Science* (Cambridge: Cambridge University Press, 1998); Karin Knorr-Cetina, "The Couch, the Cathedral, and the Laboratory: On the Relationship between Experiment and Laboratory in Science," in *Science as Practice and Culture*, ed. Andrew Pickering (Chicago: The University of Chicago Press, 1992); Karin Knorr-Cetina, "Laboratory Studies: The Cultural Approach to the Study of Science," in *The Handbook of Science and Technology Studies*, ed. S. Jasanoff, et al. (Thousand Oaks: Sage Publications, 2001); Kohler, *Landscapes and Labscapes: Exploring the Lab-Field Border in Biology*; Latour, "Give Me a Laboratory and I Will Raise the World."; Bruno Latour and Steve Woolgar, *Laboratory Life: The Construction of Scientific Facts* (Princeton, N.J.: Princeton University Press, 1986).

years away.[20] Moreover, one cannot simply dismiss the ways in which the western world, and perhaps particularly Americans, have defined themselves through their material and technological progress and by their ability to control the natural world. Indeed, the the factory and the laboratory were powerful symbols of the West's mastery of their environment at least since the first decades of the twentieth century.[21] This was perhaps even more apparent in the immediate postwar world, especially given the American advent of the atomic bomb. For example, in the widely read Smyth Report in 1945, which described the scientific and technical developments that led to the atomic bomb, the idea that the bomb was a product of the laboratory would have been lost on no one. Throughout the report, reference is made to the numerous laboratories where the the components of the bomb were studied, developed, and experimented on. Smyth, in fact, dubbed Los Alamos the "atomic-bomb laboratory": "In the choice of a site for this atomic-bomb laboratory, the all-important considerations were secrecy and safety. It was therefore decided to establish the laboratory in an isolated location and to sever unnecessary connection with the outside world."[22] The laboratory intimated control and control implied safety. That is precisely what Blandy hoped to effect when he described the Crossroads tests as laboratory experiments.

The Navy was not alone in thinking of Bikini as a laboratory or an experimental system, however. In addition to the studies on blast effects on naval ships, Crossroads

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[20] Notably, these monsters were products of genetic mutation caused by radiation exposure, especially after the disastrous Bravo shot at Bikini in 1954.

[21] Adas, *Machines as the Measure of Man: Science, Technology, and Ideologies of Western Dominance*; Hughes, *American Genesis: A Century of Invention and Technological Enthusiasm, 1870-1970*; Nye, *America as Second Creation: Technology and Narratives of New Beginnings*; David E. Nye, *American Technological Sublime* (Cambridge: The MIT Press, 1994).

[22] Smyth, *Atomic Energy for Military Purposes: The Official Report on the Development of the Atomic Bomb under the Auspices of the United States Government, 1940-1945*, 207.

also contained a number of field studies to investigate other physical and biological effects within the atoll. Comprised of scientists from various academic and private outlets as well as from disciplines as varied as fisheries science, geology, and oceanography, the Crossroads surveys were designed to yield knowledge of immediate relevance to naval imperatives and also knowledge of a more fundamental value to scientists. Still, for the scientists attached to Crossroads, the allure of Bikini was not simply the opportunity to study a little known atoll. Instead, they saw in Bikini a rare chance to work in a natural field laboratory capable of producing insights into larger theoretical questions of interest to all who worked in the geophysical and biological sciences.

The Crossroads Surveys: Field Science in the Mid-Twentieth Century

As early as October of 1945, as the first testing proposals were circulating throughout military circles, physicist Norman J. Holter and oceanographer Roger Revelle of the Navy's Bureau of Ships suggested that a program of wave measurement investigations accompany the blast effect studies [Figure 10].[23] When the Navy approved that proposal later in November, Revelle was able to secure additional support for a much larger set of oceanographic, geological, and biological studies (pre- and post-shot surveys) as part of the Oceanographic Section, which would be led by himself.[24] Comprised of military and civilian scientists, the expedition suited the desires and needs

[23] "History of the JTF-1 Oceanography Section," SIOSF, SIOA, box 18, folder 5, 3; "History of Director of Ship Material: Operation Crossroads," Nuclear Testing Archive, Las Vegas, Nevada (hereafter NTA), accession # NV0701380.
[24] Ibid.

of both groups. In fact, Revelle designed the surveys as a hybrid of fundamental and applied scientific work; in studying the physical and biological effects of the atomic bombs on Bikini's lagoon, organisms, and geophysical features, the Navy would gain data on blast effects while as "a corollary" scientists would learn more about biological structures, coral formation, and other related scientific issues.[25]

Revelle was perfectly suited and situated for developing and leading such a military-scientific enterprise. His background was in academic oceanography, having earned a PhD in the subject from the University of California in the 1930s. During the war, however, he engaged in oceanographic work directly related to war effort. He started first at the University of California Division of War Research, an academic-military hybrid research institution at Point Loma, but by 1942 had accepted a commission at the Navy's Hydrographic Office and the Bureau of Ships. Revelle's studies during the war on aquatic acoustic sounding and other methods of charting underwater topographical features of the ocean bottoms was of immense strategic value to the Navy in the conduct of undersea warfare. It was also useful to oceanographers. If the war taught Revelle anything, it was that the Navy's pursuit of knowledge of the ocean environment could work at dual purposes. On the one hand, it aided the war effort. On the other, it generated fundamental insights of interest to oceanographers. As historian of science Roger Rainger has written, "Studies of ocean bottoms, surface layers, coastlines, and almost any other topic other than biological oceanography were, at once, both

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[25] See Rainger, "Constructing a Landscape for Postwar Science: Roger Revelle, the Scripps Institution and the University of California, San Diego."; Rainger, "Patronage and Science: Roger Revelle, the U.S. Navy, and Oceanography at the Scripps Institution."; Ronald Rainger, "Science at the Crossroads: The Navy, Bikini Atoll, and American Oceanography in the 1940s," *Historical Studies on the Physical and Biological Sciences* 30, no. 4 (2000).

intellectually meaningful to oceanographers and operationally useful to the Navy."[26] That was during the war, however. To maintain and further develop the alliance between the Navy and oceanography that he had forged during the war, he would need to persuade the Navy that "in order to gain a maximum technical value from the weapons tests, a thorough understanding of the physical and biological environment of the proving ground was desirable." This he managed to accomplish on two levels. First, he convinced the Navy that knowledge of physical oceanographic mechanisms of wave generation and the like would yield useful knowledge on blast effects. Second, he highlighted the possible effects that radiation might have on biological forms. The purpose of the oceanographic section, he wrote later was to "carry out an integrated investigation of all the aspects of the natural environment within and around the atoll: the currents and other properties of the ocean and lagoon waters, the surface geology, the identity, distribution and abundance of living creatures, and the equilibrium relationship among all these. It was believed that these relationships might be disturbed by the nuclear-fission explosions."[27] With one foot in academic oceanography and the other firmly planted in the Navy, Revelle was better positioned to couple the instrumental needs of the Navy and the basic interests of oceanography than perhaps any other person.[28]

Still, the marriage that Revelle brokered between oceanography and the Navy was not without its difficulties. As I'll discuss presently, military priorities trumped scientific

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[26] Rainger, "Science at the Crossroads: The Navy, Bikini Atoll, and American Oceanography in the 1940s," 366.
[27] Revelle, "Foreword," iii.
[28] Revelle's long term plans for this marriage was spelled out in a long memo to the Chief of Naval Research. See Revelle to Chief of Naval Research, undated, "Oceanography in the Navy Post-war Program," SIO Office of the Director (Revelle), 1848-1964 Records (hereafter SIODR), SIOA, box 12, folder Operation Crossroads, 1946-47.

ones when it came to the conduct of the surveys. Nonetheless, oceanographers, fisheries scientists, and geologists were quite willing to put up with indifference or outright hostility from naval officers with regard to the more "pure" aspects of the scientific studies. Why? To be sure, the scientists of the oceanographic section were interested as much as Revelle in securing a lasting patronage relationship with the Navy, even if it meant doing military work. Yet, patronage ties only partly explains the interest of geophysical and biological scientists in the Crossroads operation. Just as important as patronage, scientists saw in Bikini a wonderful and unique *place* to study. Bikini, for the scientists attached to the Crossroads surveys, was more than simply a testing space. Instead, they viewed the atoll, almost literally, as a scientific proving ground to test hypotheses and theories with general and universal relevance. Bikini was a natural field laboratory [Figure 11].

The concept of Bikini as a laboratory stemmed from the assumption that the atoll represented a discrete, isolated, and hence closed system. As Alistair Sponsel has shown, for example, Revelle and others (notably geologist Harry Ladd) saw Bikini as a proving ground to test the Dana-Darwin hypothesis of atoll formation, which posited that atolls were the result of the upward growth of corals on dead volcanoes that had slowly subsided back into the ocean.[29] Yet it was among biologists that the notion of Bikini as a laboratory was held strongest. As Marston Sargent and Thomas Austin wrote in their pioneering study of the organic productivity of the Bikini ecosystem as part of Crossroads, atolls provide a perfect place to study ecological relationships because "coral

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[29] Alistair William Sponsel, "Coral Reef Formation and the Sciences of Earth, Life, and Sea, C. 1770-1952" (PhD diss., Princeton University, 2009). See chapter 5.

structures remote from any terrigenous influence maintain themselves both as geographical features and as biotic communities supporting marine populations considerably denser than those of the surrounding waters."[30] Or as taxonomist Leonard Schultz of the Smithsonian put it simply, Bikini was a "great natural laboratory." "Open ocean waters are relatively barren," Schultz wrote in a report on his taxonomic studies of Bikini fishes. "Organic matter and dissolved nutriments in the sea are very slight. An atoll, however, is a very rich area of living plants and organisms. It is an oasis in an aquatic desert."[31] Bikini as a lab-like atoll was reinforced by the operation plan for the biological investigations which also called for surveys of nearby atolls for "purposes of comparison and to serve as 'control areas'."[32] Furthermore, although the surveys were largely taxonomic (Sargent and Austin's work notwithstanding), the plans couched the work in the language of experimentation. The biological survey "involves a quantitative inventory of flora and fauna at and near (1) Bikini (location of burst), (2) a secondary experimental point likely to be affected by the burst, as Eniwetok, and (3) a point unlikely to be affected by the burst as Rongerik. These inventories to be made (1) prior to the explosion of the bombs, (2) as soon after explosion as safe and feasible, (3) at varying

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[30] Marston C. Sargent and Thomas S. Austin, "Organic Productivity of an Atoll," *Transactions of the American Geophysical Union* 30, no. 2 (1949): 245. Not incidentally, Eugene and Howard Odum's equally pioneering study of ecological metabolism was conducted at Eniwetok Atoll eight years later. See Howard T. Odum and Eugene P. Odum, "Trophic Structure and Productivity of a Windward Coral Reef Community on Eniwetok Atoll," *Ecological Monographs* 25, no. 3 (1955).
[31] Leonard P. Schultz, "The Biology of Bikini Atoll, with Special Reference to the Fishes," in *Annual Report of the Board of Regents of the Smithsonian Institution* (Washington D.C.: Smithsonian Institution, 1948), 314.
[32] "History of the JTF-1 Oceanography Section," SIOSF, SIOA, box 18, folder 5, 12. Schultz also references the control atolls in Ibid., 302.

intervals for a considerable time subsequently in order to estimate long period effect and the rate of repopulation."[33]

Although reference to these seemingly closed biological systems as laboratories appears to have been a relatively new phenomenon among biologists or ecologists of the time, the search for bounded and discrete ecological units or communities amenable to observational and experimental scrutiny had a rather lengthy history prior to Crossroads, especially among limnologists. In 1887, for example, pioneer ecologist Stephen Forbes extolled lakes as being "microcosms" of larger ecological processes. With the lake, Forbes argued, "all the elemental forces are at work and the play of life goes on in full, but on so small a scale as to bring it easily within the mental grasp."[34] Forbes' description of the lake as a microcosm essentially treated certain bodies of water as a whole "organismal" entity (including biotic and abiotic factors). Forbes work, in fact, prefiguring the concept of the ecosystem coined by Arthur Tansley nearly sixty years later. Lakes would continue to preoccupy limnologists interested in community dynamics well throughout the late 19th and mid-20th century, precisely because their boundaries were so easy to define.[35] (Such was not the case with terrestrial ecology where discrete ecological communities, plant or otherwise, proved much more elusive. This conundrum perhaps explains why studies of ecological productivity on land were so slow to develop).[36] Even as late as 1942, Raymond Lindemann, in his classic paper

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[33] Revelle to Rear Admiral T.A. Solberg and Dr. Ralph a Sawyer, February 15, 1946, "Oceanographic Survey Program for Operation CROSSROADS—Summary of," SIOSF, SIOA, box 18, folder 3.

[34] Stephen A. Forbes, "The Lake as a Microcosm," in *Foundations of Ecology: Classic Papers with Commentaries*, ed. Leslie A. Real and James H. Brown (Chicago: The University of Chicago Press, 1991), 14.

[35] See McIntosh, *The Background of Ecology: Concept and Theory*, 120-7.

[36] Ibid., 130-1..

signaling the turn towards the trophic-dynamic viewpoint and ecosystems theory in ecology, argued that the lake is the proper unit of analysis "since all the lesser 'communities' ... are dependent upon other components of the lacustrine lake [definition: of a lake] food cycle for their very existence."[37] Indeed, it was exactly the closed bounded system of Cedar Bog Lake that enabled Lindemann to measure the energy flow through an ostensibly complete ecosystem. As Robert Kohler has written, Lindemann's paper marked watershed in ecological practice because it utilized a mixed bag of laboratory-style practices (that is, physiological in the sense that Lindemann measured ecological "metabolism") and old-school descriptive field practices ("practices of place").[38]

The idea of Bikini as an outdoor natural laboratory was an extension of this preoccupation with closed aquatic systems. Again Kohler's work on the borders between laboratory and field practices helps explain why. With the growing epistemological prestige ascribed to laboratory-based science in the late nineteenth century, scientists within natural history acutely felt the perceived weaknesses of field work. As Kohler argues, laboratories were exemplars of scientific practice precisely because they eliminated the problem of place, which was the essence of field science. Laboratories as "simplified and standardized, stripped of all context and environmental variations" exemplify "places *apart* from the world—placeless places. It is this odd spatial quality that gives knowledge produced in labs its credibility."[39] Field sciences, however, cannot ignore place. Instead, place-making becomes a central practice among field scientists in

[37] Raymond L. Lindeman, "The Trophic-Dynamic Aspect of Ecology," *Ecology* 23, no. 4 (1942): 399.
[38] Kohler, *Landscapes and Labscapes: Exploring the Lab-Field Border in Biology,* 274-83.
[39] Ibid., 191. Emphasis mine.

order to make the field more lab-like. "Rather than eliminate the element of place," Kohler contends, "field biologists take advantage of it to do in their own way what lab biologists achieve by standardizing animals and protocols—and without sacrificing the authenticity of work in nature."[40] Here, then, we can begin to better understand how and why the Crossroads survey scientists emphasized the lab-like features of Bikini. Their lab discourse neatly bounded Bikini. As an "oasis"—a space opposed to the "barren" sea—they maintained the exceptionality of Bikini, depicting it as a discrete space, sheltered, as it were, from outside influences. By making Bikini lab-like (i.e. stressing the atoll's placeness), they in essence protected their scientific authority by maintaining that the knowledge produced at Bikini was indeed genuine knowledge.

Thus the portrayal of Bikini as a laboratory by both the Navy and the civilian scientists conducting the surveys served to render the atoll as a bounded laboratory, worthy of destruction on the one hand and credible as a site of knowledge production on the other. Each group had their own particular reasons for representing Bikini as a lab, even if the end result was the same. For the Navy, bounding Bikini through laboratory discourse served to meet their larger testing goals—proving that nuclear weapons testing was under their technoscientific control and thus safe. For scientists, representing Bikini as a laboratory protected their epistemological authority over the knowledge produced there. Nonetheless, the one enabled the other and Bikini was thus made into a military-scientific space; where one began and the other ended is not clear. Still, it is worth noting again the different motivations that each group had for portraying Bikini as a laboratory. As we will glimpse in this chapter and see in later ones, knowledge produced in this

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[40] Ibid., 192.

laboratory turned out to be a double-edged sword for the military and soon the Atomic Energy Commission. As scientists began to discern the movement of radiation beyond the confines of the test sites, the spatialization of Bikini or the Nevada Test Site as bounded laboratory-like spaces would soon give way to more holistic understandings of space and hence radiological risk. The difference would prove to be an even greater boon to the environmental sciences, but a death knell for atmospheric testing. During Crossroads, nonetheless, the isolation of Bikini coupled with its unique geological and geographic features made it a promising site through which to ensure safety from fallout and enhance field research. Descriptions by scientists of Bikini as a laboratory, therefore, was not simply political rhetoric, but was based in solid material practices.

Ironically, the investigation of biological effects of fallout on Bikini's organisms was not part of the mission of the Oceanographic Section. Whereas the Oceanographic section's biological surveys were devoted to determining changes in population numbers, radiological effects on fishes and other organisms fell to the Radiological Section, headed by Manhattan Engineering District veteran Stafford Warren [Figure 12].[41] Warren had been part of the Crossroads planning with Revelle from the beginning. Early on, he and Revelle decided to separate out the ecological investigations from the radiological effects studies.[42] Although the the reasons for this division are not entirely clear, it may be that Revelle considered the radiological effects studies to be part of mundane monitoring

[41] The radiation safety activities of the Radiological Section and Warren's role in setting exposure standards and protection protocols is discussed at length in Hacker, *The Dragon's Tail: Radiation Safety in the Manhattan Project, 1942-1946*, chapter 6. See also Stafford Leak Warren, "An Exceptional Man for Exceptional Challenges," interview by Adelaide Tusler, 1983, University of California at Los Angeles Special Collections Library, Los Angeles, CA (hereafter UCLASC).
[42] "History of the JTF-1 Oceanography Section," SIOSF, SIOA, box 18, folder 5, 13.

duties rather than a fundamental research program in its own right. In any event, Warren quickly organized an ad hoc conference in San Francisco in late January with representatives from the MED, Scripps Institute of Oceanography, the California Academy of Sciences, and the University of Washington School of Fisheries to begin planning for a study of radiological effects.[43] Following the meetings, it was determined that radiological monitoring of Bikini's organisms would be conducted by a team of fisheries experts from the University of Washington Applied Fisheries Laboratory (AFL) under the direction of lab head Lauren Donaldson [Figure 13].

Donaldson was an apt choice to lead the radiological surveys. During the war, Warren asked Donaldson to set up an experimental program on the university's campus to investigate the effects of x-rays on salmanoid fishes. With construction of the plutonium production piles steadily underway at Hanford upriver from Seattle, Warren was keen on learning as much as he could on the potential biological effects of the reactor effluent on Columbia River fishes once the site went online. Accordingly, Warren arranged for a contract to be let between the university and the Office of Science and Research Development to establish the misleadingly named Applied Fisheries Laboratory. Even Donaldson was not told of the true nature of the work he was being asked to undertake. When Donaldson did learn of Hanford and the bomb project around 1944, he convinced Warren to set up a small onsite laboratory under the direction of his graduate student Richard Foster to study the effluent problem more closely to the site of contamination

[43] "History of Director of Ship Material: Operation Crossroads," NTA, NV0701380, 281-2.

itself.[44] As the x-ray study suggests, the fisheries investigations reflected the MED's emphasis on the hazards of external radiation since x-rays were considered to have equivalent kinds of energies and biological effects as gamma radiation. Moreover, the work conducted both on the UW campus and at Hanford were solely laboratory-based; no field studies were conducted during the war. For example, fish were exposed to x-rays or radioactive effluent in ponding troughs or basins, but no work was conducted on biological uptake or in the river itself.[45] For the Crossroads operation, however, Donaldson jumped at the chance to study radiological effects in the field itself, even if the work was primarily monitoring.

There is no documentary evidence to suggest that Donaldson foresaw in Bikini an opportunity for long-term ecological study utilizing the lingering radiation from the bombs as a tool to trace out community structure and function when he signed on to participate inCrossroads. Nonetheless, as we will see, the detonation of the bombs (especially the underwater shot, Baker) would soon impress upon the scientists participating in the survey the ineluctable assimilation of radiation into every crevice and organism of the Bikini environment. At first, this contamination elicited alarm. Yet, soon enough scientists would begin to view this lingering radiation as a tool to study

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[44] Neal O. Hines, *Proving Ground: An Account of the Radiobiological Studies in the Pacific, 1946-1961* (Seattle: Univ. of Washington Press, 1962), chapter 1. For more on the establishment of the AFL in the archival record, see The Laboratory of Radiation Ecology Records, 1944-1984 accession # 00-065 (herafter LRER), The University of Washington Special Collections, Seattle, WA (hereafter UWSC), box 3, folder 23 Stafford Warren, 1944-51.

[45] Kelshaw Bonham et al., "The Effect of X-Ray on Martialitym Weight, Length, and Counts of Erythrocytes and Hematopietic Cells in Fingerling Chinook Salmon, *Oncorhynchus Tschawytscha Walbaum*," *Growth* 12 (1948); Kelshaw Bonham et al., "Lethal Effect of X-Rays on Marine Microplankton Organisms," *Science* 106, no. 2750 (1947); Richard F. Foster et al., "The Effect on Embryos and Young of Rainbow Trout from Exposing the Parent Fish to X-Rays," *Growth* 13 (1949); Arthur D. Welander et al., "The Effects of Roentgen Rays on the Embryos and Larvae of the Chinook Salmon," *Growth* 12 (1948). Early AFL reports on these experiments under the UWFL report series can be found in LRER, UWSC, box 9.

complex biological and physical processes that, by and large, had previously been foreclosed to them. Indeed, by the time Donaldson sailed home to Seattle following his work at Crossroads, he began to ponder the potential long-term research possibilities at Bikini. First, though, he would witness the Crossroads bomb detonations and fret over the radiation left in their wake.

Crossroads: Able and Baker

Members of the oceanography section sailed into Bikini lagoon aboard the U.S.S. *Bowditch* on March 10, well ahead of the anticipated first test scheduled for May 15. The biological, geological, and oceanographic pre-test surveys began shortly thereafter. From the beginning, however, the admixture of military and fundamental science that Revelle envisioned for Crossroads proved fleeting—military imperatives, it soon became clear, took priority over scientific ones. The members of the biological surveys perhaps most keenly felt this general lack of regard for the fundamental research aspects of Crossroads. Leonard Schultz and Joseph Morrison, both curators at the Smithsonian, for example, complained bitterly throughout the expedition of the terrible working conditions, lack of logistical support, and poor cooperation of naval officers attached to the surveys.[46] At one point, Schultz and his party were forced to camp-out with meager provisions for a couple days when the Navy failed to pick them up following a collecting trip. When the

[46] Pamela M. Henson, "The Smithsonian Goes to War: The Increase and Diffusion of Scientific Knowledge in the Pacific," in *Science and the Pacific War: Science and Survival in the Pacific, 1939-1945*, ed. Roy M. MacLeod (Dordrecht: Kluwer Academic Publishers, 2000), 42-3. See also correspondence in the Schultz Papers, SIA, box 23, folder 4; Schultz, "Log of Crossroads Project," Schultz Papers, SIA, box 23, folder 1. Complaints by Schultz and other scientists can also be found in correspondence contained in the Waldo LaSalle Schmitt Papers, Record Unit 7231, SIA, box 8, folder Crossroads Project.

first test was delayed and rescheduled for June or July, Schultz was so flustered by the expedition that he began making arrangements for a replacement and a trip back home.[47] Although the the oceanographers and geologists fared much better with the Navy (ostensibly because their work on ocean floor mapping and current studies were more closely aligned with naval priorities), they nonetheless lamented some of the lost opportunities afforded to them for advancing basic science. Scripps oceanographer Walter Munk, for example, bemoaned that "no one here... is interested in applying physical principles to the interpretation of data."[48]

Conditions aboard the U.S.S. *Haven* for the radiological section proved much more comfortable and Warren and Donaldson's relations with the Navy appeared to have been cordial, initially at least. The *Haven* reached Bikini on June 12, months after the oceanography section. On the 14th, Donaldson began leading fishing expeditions throughout the lagoon and coral reefs to "determine the normal population and to secure 'control' material."[49] Unlike Schultz and his colleagues, Donaldson had few problems securing boats or the assistance of officers.[50] From the 14th to the 29th, Donaldson and his crew collected some 1,926 fish specimens obtained by trolling (for deeper water

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[47] Ibid.

[48] Quoted in Rainger, "Science at the Crossroads: The Navy, Bikini Atoll, and American Oceanography in the 1940s," 367.

[49] Dr. Lauren Donaldson et al., undated, "Appendix XIV, Radiobiological Studies Bikini Atoll June 12 to August 14," LRER, UWSC, box 6, folder Bikini 1946-47. This document can also be found with Warren's report in the Stafford Warren Leak Papers, Collection Number 987 (hereafter Warren Papers), UCLASC, box 76, folder 5.

[50] Lauren Donaldson log of Operation Crossroads, The Lauren R. Donaldson Papers, Record Group 2932 (hereafter LRD Papers), UWSC, box 11, folder 28. This document can also be found in the UW Digital Collection at http://content.lib.washington.edu/index.html.

species) or poisoning (for near-shore fishes) using derris root powder, or rotenone.[51] Larger (and tastier) samples of captured fish found their way to the kitchen, perhaps explaining, in part, the different experiences between the two survey sections.[52]

Both the oceanographic and radiobiological surveys ended on the 30th when trial runs for Able, the air drop, were initiated. It had been a long wait for everybody involved in the operation. The Able detonation, as I noted above, had been scheduled for May 15, but President Truman decided to delay the test until June in an effort to decouple the operation from the ongoing negotiations about the future control of atomic energy.[53] Blandy further postponed the target date as weather conditions in the South Pacific failed to cooperate. When the bomb was finally dropped on July 1 at 9:15 a.m., Warren and the other survey scientists could not have been happier. Delays in the operation not only exacerbated the inconveniences and problems associated with pre-surveys, but also threatened to derail the post-operations since many of the scientists would soon need to make their way back to the States to commence their academic appointments for the new school term. This was a particularly vexing problem for Warren who had already encountered considerable difficulty in securing adequate manpower for the radiation monitoring activities.[54]

Unlike Trinity, Able neither awed the some 42,000 observers safely witnessing the shot outside the lagoon nor taxed the radiological monitoring team afterwards. Among the special guests invited to view the detonation were diplomats, Congressmen,

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[51] Donaldson, "Appendix XIV" LRER, UWPC, box 6, folder Bikini 1946-47. For fishing techniques see ibid.
[52] Ibid.
[53] Weisgall, *Operation Crossroads: The Atomic Tests at Bikini Atoll*, chapter 10.
[54] Hacker, *The Dragon's Tail: Radiation Safety in the Manhattan Project, 1942-1946*, 124-5.

and scores of newspaper journalists who quickly ran off stories via the wire to report what they had seen. The news reports were decidedly blasé. As one blunt journalist put it, "There was the same high hope of a great thrill, and the same momentary disappointment when the bomb burst that one feels when a circus act proves less exciting than its billboard poster."[55] Distance appears to have been the mitigating circumstance preventing the observers from experiencing the flash and heat effects of the bomb. The damage wrought on the ships, once the mushroom cloud had dissipated and ship inspection commenced, was equally disappointing. While the explosion managed to sink five of the ships in the array outright, it only produced serious damage on about ten ships out of a fleet of ninety-five.

Radiological monitoring began almost immediately following the shot with drone air- and water-craft. Airplanes manned with rad-safe personnel were also directed to make radiation measurements of the mushroom cloud, first at a safe distance and later directly over the target area.[56] Manned boats soon followed, but only skirted along the edges of the lagoon.[57] It was not until the next day that radiological monitoring within the lagoon began in full force. Patrols entered the lagoon early in the morning to determine whether radiation levels had sufficiently decayed to start salvaging operations. When the lagoon was declared safe later in the day, monitors then took readings on the ships. Geiger "sweet" ships were safe to work on. "Sour" ones were designated as unsafe. Most ships proved "sweet" and none of the workers who boarded them exceeded

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[55] Quoted in Weisgall, *Operation Crossroads: The Atomic Tests at Bikini Atoll*, 187.
[56] David Bradley, *No Place to Hide* (Boston: Little, Brown and Company, 1948), 55-61.
[57] Lauren Donaldson log, LRD Papers, UWSC, box 11, folder 28, 68.

tolerance doses (Warren set the tolerance dose to at 0.1 roentgens/day, a relatively conservative standard when compared to Trinity).[58]

On Able day plus 2, Donaldson and his crew began their radiobiological survey. From the 3rd to the 24th, Donaldson collected another 1,800 fish from the lagoon and along the reefs. Despite the large number of fish caught, Donaldson and his crew did not find any dead fish or seriously irradiated ones. As he wrote in his final report, there "was no evidence uncovered that fish, or other marine organisms were injured by irradiation during the Able Day explosion."[59] By and large, the Able test went off about as well as hoped. It was largely uneventful and produced no immediately detectable radiological effects on humans or fish.

The next shot (Baker) was anticipated to be much worse, radiologically at least. Although no one knew for sure what kind of effects the bomb might produce if detonated underwater, it was generally agreed that the majority of the bomb's radiation would not be vented into the atmosphere, but instead become entangled within the lagoon itself. In one prediction, a Los Alamos scientist conjectured that the "water near a recent surface explosion will be a witch's brew [where] there will probably be enough plutonium near the surface to poison the combined armed forces of the United States at their highest wartime strength. The fission products will be worse."[60] Stafford Warren held similar concerns. As an atmospheric shot, Able was not dissimilar from Trinity. As you will recall from chapter 2, the problems encountered with offsite radiation during Trinity

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[58] For more on the target ships boarding parties and exposure levels see Hacker, *The Dragon's Tail: Radiation Safety in the Manhattan Project, 1942-1946*.
[59] Lauren Donaldson log, LRD Papers, UWSC, box 11, folder 28, 4.
[60] Weisgall, *Operation Crossroads: The Atomic Tests at Bikini Atoll*, 216.

prompted Warren to warn Leslie Groves that future tests be conducted at least 150 miles away from any settled bystanding populations. With the removal of the native Bikinians, Bikini atoll, of course, more than filled that requirement. Yet because Baker's radiation was sure to envelop Bikini's lagoon, it "was going to be the real mean one."[61]

Warren's concerns about Baker reflected a shift in his thinking about radiological hazards. Throughout the preparations and conduct of Crossroads, Warren had displayed a good deal more cautiousness and apprehension in regards to the potential radiation hazards of the testing series. In part, the difference between war- and peace-time operations explains this change in attitude. Trinity, after all, was deemed by all to be the definitive weapon in ending the war and ensuring peace; lax radiological standards seemed worth the perceived benefits of completing the bomb and winning the war. Still, as I argued in chapter 2, MED officials felt that they acted safely during Trinity, even if in hindsight their lack of environmental considerations left a gaping hole in the potential hazards posed by testing. In fact, it was precisely this kind of environmental thinking that better explains Warren's more precautious approach to radiological hazards during Crossroads. During early planning with Revelle, for example, Warren pointed out that "radiological safety during the tests, for both near and distant observers, was basically a problem in meteorology and oceanography."[62] It was also a problem in biology. Warren's decision to have Donaldson and members of his Applied Fisheries Laboratory team brought on board for Crossroads speaks to his turn toward environmental considerations of radiation exposure. Of all the scientists mobilized to study radiation

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[61] Quoted in Ibid., 214. Quote is taken from Stafford L. Warren, "An Exceptional Man for Exceptional Challenges," interviewed by Adelaide G. Tusler, 1966-1968, transcript, Oral History Program, UCLASC.
[62] "History of Director of Ship Material: Operation Crossroads," NTA, NV0701380, 267.

effects during the MED, Donaldson was one of the very few researchers investigating biological effects of radiation on non-human animals under field-like conditions.[63] Although his work conducted as part of the MED did not contain any firm conclusions as to environmental hazards, Donaldson's work impressed upon Warren the need to at least study the problem closely. As Neal Hines has written, Warren was "interested in the problems that had produced the program at Hanford [and] was eager to see the problems pursued at Bikini."[64] Donaldson's research at the AFL, then, convinced Warren to include fundamental radiobiological studies as part of his team. To understand the effects of Baker, Warren and Donaldson would need to study the environmental behavior of radiation in the field. Indeed, it was becoming clearer to Warren that the problems posed by the bomb were not merely factory problems. What Warren and Donaldson found following Baker would soon confirm Warren's initial apprehensions and point to other potentially more insidious challenges posed by atomic energy development.

Baker was detonated approximately 90 feet below the surface of lagoon on July 25 [Figure 14]. In nearly all aspects, it was a markedly different kind of detonation than Able. Whereas the explosion from Able was a spectacular disappointment, the Baker blast evoked a profound sense of wonder at the magnitude and destructive power of the bomb. Few of the civilian spectators who witnessed Able, however, stuck around for Baker. Upon detonation, a bright flash appeared in the lagoon and was immediately followed by a huge column of water roughly a half mile wide that soon reached an

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[63] The other scientists working on what might be termed environmental questions during the MED were Louis Jacobson and Roy Overstreet of the University of California. They did studies on plant uptake of fission radionuclides as part of the Joseph Hamilton's rat metabolism studies.
[64] Hines, *Proving Ground: An Account of the Radiobiological Studies in the Pacific, 1946-1961*, 41.

altitude of 1,800 feet. At that point, according to radiological monitor David Bradley who rode in one of the rad-safe planes, the "great geyser... stood there as if solidifying for many seconds, its head enshrouded in a tumult of steam. Then the pillar began to break up. At its base a tidal wave of spray and stream arose, to smother the fleet and move on toward the islands."[65] Donaldson perhaps best summed up the shock of Baker compared to Able: "The one July 1 was awe-inspiring and in many ways beautiful, but the one today just frightened the very daylights out of one...it appeared as if the entire lagoon were up in the air."[66] The destruction to the naval flotilla was equally shocking. Although many of the ships remained afloat following the detonation, several were badly damaged. Still worse was the radiation. Initial estimates of the radioactive burden of the lagoon put the figure as equivalent to hundreds of tons of radium.[67] (A microgram of ingested radium was considered above tolerance dose). Patrol boats entered lagoon within a couple hours of the detonation to measure the radioactivity of the ships and to delineate the extent of radiation in the lagoon.[68] By all accounts, the lagoon and nearly all the ships were severely contaminated. So contaminated, in fact, that entrance into most parts of the lagoon was declared off-limits for the first day of the boarding and salvaging operations.[69]

The unexpected extent and severity of the radiation threatened the very purpose of the operation since evaluation of the damage would have to wait until the ships were safe

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[65] Bradley, *No Place to Hide*, 93..
[66] Donaldson Log Book, LRD Papers, UWSC, box 11, folder 28, 56.
[67] Shurcliff, *Bombs at Bikini: The Official Report of Operation Crossroads*, 167-8; Weisgall, *Operation Crossroads: The Atomic Tests at Bikini Atoll*, 227-8.
[68] Hacker, *The Dragon's Tail: Radiation Safety in the Manhattan Project, 1942-1946*, 139; Weisgall, *Operation Crossroads: The Atomic Tests at Bikini Atoll*, 227-8.
[69] Hacker, *The Dragon's Tail: Radiation Safety in the Manhattan Project, 1942-1946*, 138-9.

to board. As a result, the contamination from Baker pit the operational objectives of Crossroads (and thus the naval command) against radiation safety and Warren, who rightly saw the incident as a disaster. Throughout the next month as crews attempted to decontaminate the ships, Warren found himself worrying at ever greater lengths about the amount of radiation salvage crews were being exposed to and confronting naval officials who resisted the protocols and prohibitions of his radiological safety plan. Eventually, Warren succeeded in convincing Blandy to cancel the clean-up attempts, but not before a number of military men received doses of radiation above tolerance standards.[70] The ships capable of staying afloat were subsequently towed back to the naval ship yard at Hunter's Point in the San Francisco Bay for further decontamination and study.[71]

In the meantime, at Baker day plus 4 Donaldson's general monitoring duties ended and he and his fellow AFL colleagues began collecting samples of various Bikini organisms for radiobiological analysis. Dead fish were readily apparent immediately following Baker, most of which were killed presumably by the blast. Other living samples collected in the first few days showed high levels of radioactivity in the skin and gills. That much wasn't too surprising. Along the edges of the lagoon, however, fish began displaying concentrated levels of radiation in their digestive tracts. Contaminated algae and plankton was immediately suspected as the source of the radiation and soon confirmed. Samples of algae taken from the lagoon, for example, revealed that they "were excellent absorbers of fission products."[72] The algae and plankton, in fact,

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[70] Weisgall, *Operation Crossroads: The Atomic Tests at Bikini Atoll*.
[71] Ibid.
[72] Stafford Warren, September 25, 1946, "Nuclear Radiation Effects in Tests Able and Baker —Preliminary Report of," Warren Papers, UCLASC, box 76, folder 5, 14.

displayed levels of radiation that exceeded the levels detected in the surrounding waters. The radioactive algae adhering to the target fleets' hulls were so hot that they were contributing to the external radiation levels aboard the ships.[73] Plankton too were contributing to the radiation levels of the ship hulls and indeed the general radiation situation throughout the lagoon; at night, when plankton would rise to the surface of the lagoon, external levels of radioactivity rose markedly.[74]

While the radiobiology crew's basic purpose was to determine the biological effect of radiation of Bikini's marine organisms, it was the phenomena of uptake and food chain dynamics that soon preoccupied Donaldson. As a result, the group became much more selective in their sampling and began paying special attention paid to certain geographic areas of the lagoon and specific ecological relations that they thought might reveal the dynamics behind the cycling of radiation in the ecosystem.[75] Donaldson too began to make radioautographs of algae and fishes in an effort to better understand the concentration of radiation within fish bodies as well as clams and corals [Figure 15].[76]

Although the practices were crude by later standards, by and large these efforts revealed the radiation from Baker had insinuated itself into nearly every component of Bikini's biological system. That fact prompted Arthur Welander, an AFL scientist and member of the radiobiology division, to issue a warning against eating local food sources. "In no case were any of the biological specimens taken within the lagoon entirely free of

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[73] Hacker, *The Dragon's Tail: Radiation Safety in the Manhattan Project, 1942-1946*, 141.
[74] Hines, *Proving Ground: An Account of the Radiobiological Studies in the Pacific, 1946-1961*, 45, 48.
[75] Ibid., 47.
[76] "Appendix XIV" LRER, UWPC, box 6, folder Bikini 1946-47. See also, Stafford L. Warren, "An Exceptional Man for Exceptional Challenges," interviewed by Adelaide G. Tusler, 1966-1968, transcript, Oral History Program, UCLASC, 930.

radioactive contamination" Welander cautioned in a report issued during operations. "In general, the algae proved to have the highest activity and in consequence fish and other animals feeding on the algae also show high activity...Because of the highly poisonous nature of the fission products, and the increasing difficulty of detecting alpha emitters in the tissues of these marine animals, it is recommended that no fish, mollusk, or other marine animal taken within 100 miles of Bikini lagoon be used as food."[77] It was a remarkable statement. Whereas the biological surveys had been initiated to alleviate concerns that blast and radiological effects would not harm commercially valuable fisheries stock, the radiobiological work undertaken at Bikini revealed that it was conceivable that the irradiation of the environment from nuclear fallout posed a direct threat humans themselves.

Yet as Donaldson acknowledged in his final report after the radiobiology division left Bikini in late August, there were still many unknowns. First, his lab needed time to study the nearly 4,500 specimens collected. But more importantly he noted, "Additional collections should be made at Bikini. (a) Collections in a period of about six months... (b) Collections the year after the blast..." Moreover, "Laboratory experimental work is needed to provide a backlog of information. (a) Information is needed on the absorption path and the retention of active material by various classes of aquatic organisms. (b) Basic information is very much needed on the absorption-retention chain among aquatic animals. (c) A special study should be made of the 'fouling' organisms on ship's bottoms

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[77] Dr. Arthur Welander "Recommendations resulting from the Radiobiological Monitoring of Bikini Lagoon Subsequent to Baker Day," LRER, UWSC, box 6, folder Bikini 1946-47. Welander's report is also quoted in Hines, *Proving Ground: An Account of the Radiobiological Studies in the Pacific, 1946-1961*, 44-5.

and their relationship to activity accumulation and its retention."[78] Donaldson, in other words, was making a plea for a relatively long-term study of the cycling of radioactivity within the Bikini lagoon and its potential health repercussions for humans should testing resume.

The import of the Baker radiation on the Bikini ecosystem and its potential effect on naval operations in the event of a nuclear war was not lost of the Navy either. “It is too soon to attempt an analysis of all the implications of the Bikini tests,” a preliminary report by the Joint Chiefs of Staff in early-August declared. “But it is not too soon to point to the necessity for immediate and intensive research into several unique problems posed by the atomic bomb. The poisoning of such large volumes of water presents such a problem.”[79]

Despite the Navy's acknowledgement of problems encountered with Baker, the let down of Able and the their insistence that the bombs were conducted under laboratory-like conditions had helped produce the political and social effects that the Navy, and the military in general, desired—soothing fears of the atomic bomb in the wake of the Hiroshima and Nagasaki. “I would summarize Operation Crossroads,” to quote William Parsons again from the opening of this chapter, “by saying that it was the largest laboratory experiment in history…Not one man was injured either by the blast or radio activity [sic] from the bomb experiment. The tests were conducted on schedule and were both valid.” Because “tens of thousands of people have seen two atomic bombs set off under controlled conditions,” he continued, the tests have “gone part of the way toward

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[78] "Appendix XIV" LRER, UWPC, box 6, folder Bikini 1946-47.
[79] Joint Chiefs of Staff Evaluation Board. *Preliminary Statement by the Evaluation Board on Test B*. Reprinted in Shurcliff*, Bombs at Bikini: The Official Report of Operation Crossroads*, 199.

substituting a rational fear of the known for an irrational fear of the unknown in this Atomic Age."[80] It is no small wonder, then, as the historian Paul Boyer has argued, that the "short-term effect of the 1946 test series…was to dampen fears of the atomic bomb."[81] Reference to the Bikini laboratory was a critical part of this endeavor.

Still, the "unique problems" presented by the Baker radioactivity demanded answers. Accordingly, the Navy soon began planning for a resurvey the following summer. This time, the kind of fundamental science that Revelle had envisioned for Crossroads would be more fully realized. And Donaldson, for his part, would begin to make plans for a long-term study in the perfect natural laboratory of Bikini.

Unanswered Questions and Opportunities: Preparing for a Resurvey

Donaldson was not alone among the fisheries scientists attached to Crossroads to recommend a resurvey of Bikini to evaluate potential long-term effects of the Baker radiation on the atoll's marine organisms. Leonard Schultz and Earl Herald, who replaced Schultz when he left for Washington following the Able blast, warned the Navy in their final report in September of 1946 of the need for further surveys if the "overall effect upon the reef fish populations is to be known." During Crossroads, Schultz and Herald had focused primarily on taxonomic changes in the relative abundance and make-up of Bikini fishes due to the immediate effects of blast, heat and external radiation. Donaldson's finding that radiation from Baker was moving throughout the Bikini ecosystem and concentrating in its organisms, however, alerted them to the possibility of

[80] See note 2.
[81] Boyer, *By the Bomb's Early Light: American Thought and Culture at the Dawn of the Atomic Age*, 84.

latent effects among local populations. In particular, Schultz and Herald emphasized the food chain dynamics in the lagoon: "herbivorous fishes feeding upon algae and other plants that are radioactive may concentrate the radioactive materials. These fishes may in turn be fed upon by the larger carnivorous animals imparting their radioactivity to their predators. As soon as any fish shows the slightest debility it is immediately preyed upon by the carnivorous fishes or other marine animals. Thus radioactive substances could be concentrated in certain fishes and theoretically the carnivors [sic] are the last to be affected."[82] One had to wait until the cycling of the radiation reached a balance or climax stage, in other words, for the effects to become recognizable.

Algae, specifically, was singled out as the critical link in the general pattern of radiation circulation and cycling in the Bikini ecosystem. Schultz wrote Donaldson the month after he submitted his report asking if he happened to know of any research of the effect of radiation on algae. He also noted that he had been in contact with Revelle "about the necessity of having a full understanding of these algal radioactivity selectivity factors especially as they concern the feeding habits of the reef fishes."[83] Early in November, Schultz had Revelle deliver a letter to Stafford Warren that sketched out a rough plan for a resurvey coordinated with Donaldson for the following year.[84] He likewise submitted a proposal to Fish and Wildlife Service, Division of Fishery Biology head Elmer Higgins for a resurvey, which he knew would be received appreciatively

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[82] Leonard P. Schultz and Capt. Earl S. Herald, "Preliminary Report on the Reef Fishes of the Crossroads Project," September 1946, Schultz Papers, SIA, box 23, folder 6.
[83] Schultz to Donaldson, October 15, 1946, Schultz Papers, SIA, box 23, folder 4. This same document can be found in the LRER, UWSC, box 1, folder 21.
[84] Schultz to Warren thru Commander Revelle, November 1, 1946, Leonard P. Schultz Papers, SIA, box 23, folder 4.

since the FWS had suggested follow-up studies when it initially agreed to participate in the Crossroads tests.[85] Eventually, he talked to Warren by phone in regards to the survey, but was not sure "whether anything will become of it."[86]

In fact, the Navy had already been contemplating a return to Bikini for the following summer. On November 1, the Joint Task Force in charge of the Crossroads tests ended and was replaced by the Joint Crossroads Committee under the direction of William Parsons. The purpose of the committee was to collect and finalize the reports of the various sections of the operation.[87] As the reports came in, however, "immediate consideration was given to the desirability of making a follow-up investigation."[88] The impetus was the Baker radiation. As Schultz and Donaldson made clear in their reports, the lingering radiation left in the wake of Baker raised questions "concerning the persistence of radiation in water, soil, metal, and rock; and the ultimate effects of radiation upon the survival, genetic structure, distribution, and the ecological relationships of aquatic and terrestrial plants and animals." Yet perhaps more importantly, the contamination of the lagoon prevented the salvaging crews and other strategic-oriented operations from recovering various instruments and hampered observations of structural damage to sunken ships.[89] A resurvey, then, would ensure that all of the objectives initially outlined in the Crossroads operation would be met. It might

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[85] Schultz to Higgins, November 6, 1946, "Proposal for Resurvey of Pelagic Fishes at Bikini," Schultz Papers, SIA, box 23, folder 4.
[86] Schultz to Donaldson, November 13, 1946, Schultz Papers, SIA, box 23, folder 4.
[87] "History of Director of Ship Material: Operation Crossroads," NTA, NV0701380, 303. For the makeup, purpose, and organization of the Joint Crossroads Committee see Carson to the Joint Crossroads Committee, November 26, 1946, "Memorandum No. 4-46," NTA, NV0767517.
[88] "Bikini Scientific Resurvey: Operations, Volume I," (Washington D.C.: Armed Forces Special Weapons Project, 1947), 1.
[89] Ibid.

also, not incidentally, provide proof that the effect of the Baker radiation was an ephemeral phenomenon.

Again, Revelle was the point man for organizing the expedition. Whereas the fundamental aspects of the Crossroads surveys had been hampered by military imperatives, Revelle worked hard within the corridors of the naval establishment to ensure that the resurvey would better meet the pure scientific goals. His position as head of the new Office of Naval Research geophysics branch helped toward that end.[90] This time around, however, instead of seeking opportunities for scientific work as part of naval expeditions or operations, Revelle married the two. One of the major objectives of the Bikini resurvey, for example, was to settle a long standing scientific debate about the formation of coral atolls (the Dana-Darwin debate). The project involved a deep drilling project into the core of the atoll. It also required underwater soundings and seismic refraction studies of the underwater geology and structure of the atoll. On its face, this seemed to have little to do with the conduct of sea warfare. But upon closer examination, this scientific work produced immensely important information for planning submarine operations and amphibious landings.[91] The tie that Revelle bound geophysical science with the Navy was the environment. That is, producing knowledge about the ocean environment was not simply the purview of oceanographers and geologists, but was a weapon itself. As Revelle noted at the time, "There are two main reasons why we conduct expeditions today. One for scientific purposes, to discover new principles which

[90] For correspondence between Revelle and the academic scientists who he recruited to design and conduct the resurvey see Records of the Defense Threat Reduction Agency, Record Group 374 (hereafter RDTRA), Entry 47 B, NARACP, box 156, folder Operation Plans. DTRA was the successor agency to the Armed Forces Special Weapons Project (AFWSP).

[91] See note 24.

control our environment and new natural resources, and two, for the purposes of waging war. In some cases these two purposes are entirely inseparable. It has become apparent that the society which knows the most about its environment and how to turn it to account, is going to be the more likely to win the next war."[92] As we will see in later chapters, Revelle's marriage was a double-edged sword; environmental knowledge might benefit the United States' ability to wage war, but it would also provide a foundation for a trenchant critique of the military-industrial-academic complex in the 1960s.

Ironically, however, biology played a small part in Revelle's long-term plans for geophysical sciences, despite the fact that biological questions largely drove the need for a resurvey. Donaldson, for one, was disturbed by Revelle's downplaying of the potential radiation hazards and biological investigation in general. Following a planning meeting for the resurvey in May, Donaldson wrote Warren complaining that:

> It seems there is a great difference of viewpoint as to the purpose of the expedition back to Bikini. They [Revelle and E.S. Gilfillan, his eventual successor as project director] are of the opinion that radiation is a very secondary problem at Bikini, in fact, they seem to question the presence of active materials in other than trace quantities. They seem much more interested in geological experiments on the formation and growth of coral atolls than the measurements of the effects of the tests last summer.[93]

Revelle and Donaldson also apparently engaged in a bit of a turf war. Even though Revelle considered the radiobiological aspects of the resurvey to be of relatively little interest, he nonetheless moved to ensure that Donaldson had as little autonomy as possible in the resurvey. "It is considered essential..." Revelle wrote in a memo outlining the organization of the resurvey to Parsons, "that all scientists shall work as a team under

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[92] Quoted in Rainger, "Science at the Crossroads: The Navy, Bikini Atoll, and American Oceanography in the 1940s," 369.
[93] Donaldson to Warren, May 27, 1947, LRER, UWSC, box 1, folder 33.

the coordination of the project officer [Gilfillan] and the technical director...It will seriously jeopardize the success of the expedition if a group such as Dr. Donaldson's from the University of Washington considers that it has special responsibilities and information which are not to be shared with other members of the scientific survey."[94] It is not clear what sparked Revelle to make a formal note of this to Parsons, but it seems all the more puzzling considering that little to no evidence exists in Donaldson or his laboratory's papers that he was ever perceived as a non-team player or withheld (unclassified) information. The resurvey, however, marked the beginning of a long and fractious relationship between the two men.[95]

In any event, Donaldson was eager to participate. In late January, he forwarded a general outline for a resurvey of Bikini to Warren. The plans were relatively modest, he noted to Warren, owing to the uncertainty of the scope of investigation that the Navy was willing to devote to a resurvey. As a result, his program was "directed toward very practical ends." The plans were, in fact, not dissimilar from what had been done during Crossroads. Donaldson called for the collection of marine animals from all sectors of the atoll in order to study the absorbed radiation in relation to the distribution of radioactivity within the lagoon. Collected samples would then be analyzed for genetic and population changes or abnormalities. "Food-cycle studies are of paramount importance and should

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[94] Revelle to Parsons, June 18, 1947, "Memorandum for Rear Admiral W.S. Parsons, U.S. Navy," RDTRA, NARACP, box 156, folder Operation Plans. The quote is in the attachment, "Enclosure 'A': Organization of Scientific Team at Bikini and Interchange of Information Between Scientific Personnel."
[95] Glimpses and more direct evidence of their dislike for each other can be found throughout Donaldson's papers and the Laboratory of Radiation Ecology Records at the University of Washington Special Collections Library. The precise nature of their distrust and dislike is not, however, apparent.

receive every possible attention and study," he added, but did not elaborate further than that.[96]

Donaldson's tentativeness with regard to his initial plans for a resurvey also stemmed from new institutional circumstances. The same month that he wrote Warren, the Atomic Energy Commission came into being and inherited all the facilities of the MED, including the Applied Fisheries Laboratory. Both Warren and Donaldson served on the Commission's Interim Medical Committee, which was charged with developing a comprehensive medical research program on the hazards associated with atomic energy development. Although the final report of the committee proposed that the AFL continue its studies related to Hanford operations, it mentioned the Bikini studies only in passing and recommended that the lab take a fifty percent cut in its budget.[97] The AFL cut was by far the most drastic proposed.[98] Not incidentally, the lab was also the only program devoted to questions of environmental radiation effects. With few links to the Navy and his lab's uncertain future with the AEC, Donaldson was understandably cautious in his approach to the resurvey and made sure his proposal reflected technical rather than fundamental needs.

Still, despite the budget cutting, Donaldson had a powerful ally in Warren, who saw the value of Donaldson's work and, as the chairman of the interim committee, was in a position to advocate for his support. In April, for example, Warren wrote to the AEC requesting that AFL's budget be increased and that it "take the leadership in both the

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[96] Donaldson to Warren, January 28, 1947, LRER, UWSC, box 6, folder Bikini 1946-47. The plans were attached to the letter as "Suggestions for a Follow-up of Radiobiological Program at Bikini."
[97] Stafford Warren, "Report of the 23-24 January 27 1947 Meeting of the Interim Medical Committee, U.S. Atomic Energy Commission," enclosed in Warren to Nolan, January 29, 1947, NTA, NV0727195.
[98] See Lenoir and Hays, "The Manhattan Project for Biomedicine," 36-9.

Japanese and Bikini areas where its own interests are directly involved, before such information is lost."[99] That same month, Donaldson forwarded a detailed equipment list and budget to the AEC.[100] By May, the Navy and the AEC were finalizing plans for the AFL's return to Bikini.[101]

Although Donaldson couched the resurvey in term of general radiological monitoring, in actuality he had a more long-term experimental program in mind. In part, Donaldson was worried about the implications of environmental contamination from radiation. "It has been implied," he wrote in early 1949, "that radiation exposure is from external sources,—but a more specific problem is the self-contained radiation that living forms may absorb directly or pick up in the food chain. Such contained radioactive materials act chemically as any other organic or inorganic substance, but the energy released has a greater impact than that from external sources because of the immediate contact with tissues. And then one can't just walk away and put distance,— the best protection—between the contaminated and non-contaminated portions of his anatomy."[102] Warren too had been thinking along similar lines.[103] Given the extreme contamination from the Baker shot, Bikini was a perfectly place through which to evaluate this least understood aspect of radiation exposure. Because Bikini was "a shallow saucer of water in the Pacific, with water flowing over it and circulating through

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[99] Warren to Wilson, April, 7, 1947, NTA, NV0726688.
[100] Donaldson to Warren, April 1, 1947, LRER, UWSC, box 6, folder Bikini 1946-47. The original request from the AEC occurred on March 10. See Buettner to Donaldson, ibid.
[101] Donaldson forwarded his plans to Parsons on May 3 in ibid Documents on the Navy and AEC negotiations can be found in The Records of the Atomic Energy Commission, RG 326, Entry A1-67, NARACP, box 67, folder Bikini-Radiological Resurvey of Bikini and Enewetok Atolls, 1947-48.
[102] Lauren R. Donaldson, January 26, 1949, "Implications of the Atomic Problem," LRD Papers, UWSC, box 17, folder Radiation Biology, 4.
[103] See Stafford L. Warren, "An Exceptional Man for Exceptional Challenges," interviewed by Adelaide G. Tusler, 1966-1968, transcript, Oral History Program, UCLASC.

it," it offers an excellent opportunity to "explain what has happened and will take place in the future."[104] It was, in other words, a perfect natural laboratory to study long-term radiological effects.

Yet, Bikini was also a perfect laboratory through which to investigate more fundamental questions in ecology. As we have seen earlier in this chapter, the reference to Bikini as a scientific laboratory was not unique to Donaldson; both physical and biological scientists during Crossroads were enamored with the lab-like qualities of Bikini's features. There was one critical difference between the Bikini lab of 1946 and the one they were about to enter a year later—it was radioactive. Revelle perhaps best summed up the importance of radioactivity to field science when he referred the the atomic bomb as a "wonderful oceanographic tool."[105] Because radiation was relatively easily detectable with geiger-counters and other assorted radiological equipment, it made a useful tracer tool for understanding oceanographic processes such as aquatic circulation and mixing. Revelle and Warren, in fact, had anticipated its usefulness when they had originally planned to conduct a diffusion experiment prior to the Crossroads tests by distributing several curies of radiation throughout the lagoon.[106] They ended up cancelling the experiment, but it did not matter because it was was ultimately carried out

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[104] Donaldson, "Implications of the Atomic Problem," LRD Papers, UWSC, box 17, folder Radiation Biology, 2, 3.
[105] Quote can be found in John J. Slacum, "Crossroads Scientific Dividends," Schultz Papers, SIA, Box 23, folder 4. See also Henson, "The Smithsonian Goes to War: The Increase and Diffusion of Scientific Knowledge in the Pacific."; Ronald Rainger, "'A Wonderful Oceanographic Tool': The Atomic Bomb, Radioactivity and the Development of American Oceanography," in *The Machine in Neptune's Garden: Historical Perspectives on Technology and the Marine Environment*, ed. Helen M. Rozwadowski and David K. van Keuren (Sagamore Beach: Science History Publications/USA, 2004).
[106] Parsons to Technical Director, February 8, 1946, "Instrumentation for Crossroads," Warren Papers, UCLASC, Box 75, folder 1.

on a grander scale with the Baker shot.[107] Donaldson was just as excited as Revelle about the the potential of this new tool and he hoped the Bikini resurvey would prove to be the start of a long-term study of ecological processes. As Donaldson's colleague Neal Hines wrote, the Applied Fisheries people "began to see Bikini, in 1947, as a tracer laboratory."[108] What Donaldson had in mind was not dissimilar from what medical physicians were doing with their metabolism studies using cyclotron radiation in the 1930s and during the MED. Because radioactive isotopes behave in precisely the same way as their non-radioactive counterparts, they can provide information on fundamental biological processes. In Donaldson's case, they shed light on ecological processes of "growth, diet, movement, [and] reproduction."[109] Donaldson was not alone in seeing the potential of radiotracer techniques to ecology. Famed ecologist G. Evelyn Hutchinson conducted a radiotracer experiment in 1947 using radiophosphorous obtained by the AEC to study the phosphate metabolism of Lindley Pond. Hutchison too was taken by the relatively closed-systems of lakes common to limnology, but soon lost interest in the radiotracer project despite its potential for ecology.[110] For Donaldson, however, Bikini afforded the best opportunity for fundamental work in ecology. It fit the prescription of an aquatic closed system and it contained a uniquely valuable tool for studying it. It was, in short, as close to a natural field laboratory as he was like to find.

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[107] The cancelation of the tracer experimented was noted in W.H. Munk, G.C. Ewing, and R.R. Revelle, "Diffusion in Bikini Lagoon," *Transactions of the American Geophysical Union* 30, no. 1 (1949).
[108] Neal O. Hines, "Bikini Report," *The Scientific Monthly* 72, no. 2 (1951): 107.
[109] Ibid.: 105.
[110] G. Evelyn Hutchinson and Vaughan T. Bowen, "A Direct Demonstration of the Phosphorus Cycle in a Small Lake," *Proceedings of the National Academy of Sciences* 33 (1947). Nancy Slack discusses this experiment and Hutchinson's passing interest in radiotracer techniques in Nancy G. Slack, *G. Evelyn Hutchinson and the Invention of Modern Ecology* (New Haven: Yale University Press, 2010), chapter 9. See also Hagen, *An Entangled Bank: The Origins of Ecosystem Ecology*, 112-3.

Still, these ideas for further research were nascent in Donaldson's mind. Much depended on what he and the radiobiology division found in the Resurvey. It also hinged on convincing the AEC and the Navy of the import of field studies.

The Bikini Scientific Resurvey: Returning to the Laboratory

The Bikini Scientific Resurvey left San Diego for Bikini on July 1 aboard the U.S.S. *Chilton* with some forty scientists [Table 1]. By all accounts, the conditions aboard the *Chilton* proved far superior to what the Crossroads scientists had to endure. The sleeping quarters were adequate, there was room for laboratories, and the Navy officers were well disposed to the scientific nature of the expedition.[111] Morale was so improved, in fact, that while en route the scientists turned the ship into an ersatz college. From the first day of the voyage, project officer Captain C.L Engleman organized an advisory board (which included Donaldson and Schultz) to advise him on scientific and administrative problems, allocate laboratory space aboard the ship and ashore, and set up a series of seminars.[112] Lectures and seminars were held frequently and covered a range of topics from biology to radiochemistry.[113] Press releases covering the activities of the scientists were also issued from the *Chilton*. In an early press release dispatched when the ship entered Pearl Harbor for resupply on July 7, for example, Engleman described the purpose of the resurvey and scientists involved in the expedition. "Last

[111] Henson, "The Smithsonian Goes to War: The Increase and Diffusion of Scientific Knowledge in the Pacific." Leonard P. Schultz, “Notes by Leonard P. Schultz while attached to the U.S.S. *Chilton* (APA-38),”entry dated July 7, 1947, Schultz papers, SIA, box 25, folder 7.

[112] Engleman to Schultz, July 1, 1947, Schultz Papers, SIA, Box 26, folder 3. See also Hines, *Proving Ground: An Account of the Radiobiological Studies in the Pacific, 1946-1961*, 57.

[113] Short summaries of some of these seminars can be found in Bikini Scientific Resurvey Press Release No. 3, ibid.

summer Bikini lagoon," he was quoted as saying, "provided the laboratory for one of mankind's most significant scientific experiments." The Resurvey, Englemann continued, consisted of a "group of scientists returning to that laboratory to determine, record, and evaluate long-range results of the two CROSSROADS atomic bomb explosions." He also noted that the expedition "provides an unusually good example of the high degree of cooperation that now exists between military and scientific groups."[114]

They reached Bikini on July 15. Technical Director Gilfillan quickly assembled a monitoring party to measure the external radiation levels on the beach to determine whether it was safe to begin the survey operations. Gamma and beta levels on the beach were roughly the same level as normal background. Some of the detritus that had washed ashore from the previous years test showed higher levels, but Gilfillan judged the hazard insignificant and approved launching the operation.[115] Donaldson's Radiobiology group began collecting material soon after [Figure 16]. Overall, the group collected specimens from some fifty-five selected locations along the rim of the atoll and in the lagoon . Collection sites were chosen by their likely representativeness of species within the atoll.[116] As Donaldson was particularly interested in ecological relationships, it was "necessary to collect a wide variety of plants and animals from as many locations as possible."[117] That meant not only fishes, but arthropods and other invertebrates as well.

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[114] Bikini Scientific Resurvey Press Release No. 5, Schultz Papers, SIA, ibid.
[115] L. H. Berkhouse et al., "Operation Crossroads, 1946," (Washington, D.C.: Defense Nuclear Agency, 1984), 150. See also Bikini Scientific Resurvey Press Release No. 10, July 15, 1947, Schultz Papers, SIA, ibid.
[116] Hines, *Proving Ground: An Account of the Radiobiological Studies in the Pacific, 1946-1961*, 62. "Bikini Scientific Resurvey: Report of the Technical Director, Volume II," (Washington D.C.: Armed Forces Special Weapons Project, 1947), 16.
[117] Lauren Donaldson, December 1947, "Radiobiological Resurvey of Bikini Atoll During the Summer of 1947," Report number UWFL-7, NTA, NV0149057, 9.

Collections along the reefs were made using the poisoning technique utilized during Crossroads. Poisoning was used in deeper waters too, but was supplemented by hook and line fishing for larger species.[118] Divers inspecting sunken ships also made collections of radioactive sediment at the bottom of the lagoon for radiological analysis.[119]

Once collected, the radiobiology group and other fisheries scientists conducted taxonomic identification of the various species. Some specimens were collected whole and preserved while others were sent back to the ship to be ashed for radiological analysis. In many cases, samples were dissected in the field (organs of particular interest, specifically) and preserved for later study back in Seattle.[120] By the time the Resurvey left Bikini on August 29, a total of 5,883 organisms were collected. Of those, nearly 750 were analyzed in the field and the rest preserved.[121]

When Donaldson wrote up his initial findings for the resurvey technical report, he confirmed the earlier phenomena detected during Crossroads. Fish and other organisms were concentrating significant, but not necessarily hazardous, levels of radioactivity. "Fission products were found to occur in fish, and in invertebrates such as clams, snails, oysters, corals, sponges, octopods, crabs, sea urchins, sea cucumbers, spiny lobsters, and shrimps. They were also represented in the algae found about the lagoon. Concentration of active substances in fish was greatest in the spleen, liver, and feces...Other tissues extensively sampled, including gills, skin, bone, and muscle contained fission products in

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[118] Ibid. "Bikini Scientific Resurvey: Report of the Technical Director, Volume II," 16.
[119] Hines, *Proving Ground: An Account of the Radiobiological Studies in the Pacific, 1946-1961*, 70-1.
[120] Lauren Donaldson, December 1947, "Radiobiological Resurvey of Bikini Atoll During the Summer of 1947," Report number UWFL-7, NTA, NV0149057, 11.
[121] Ibid.

lesser amounts."[122] This wasn't an insignificant find, as Donaldson elaborated in a speech following the resurvey. "Fouling organisms on the ship [i.e. plankton and algae]," he noted, "had absorbed and concentrated it [radiation] to some 10,000 times the activity we had found in the water. So here, a year later, we still find selective absorption...this, again, is a fundamental concept we musn't [sic] forget."[123] The real surprise of the resurvey was the distribution of radioactivity. As Donaldson wrote in his report, the "Data collected...indicate a very widespread distribution of radioactive substances in the organisms in and about Bikini Lagoon. In fact, some activity was found in organisms taken from every part of the Bikini area that was sampled."[124] In particular, however, high levels of radiation were found concentrated off of Enyu Island at the entrance of the lagoon. This area, more than any other, should have been nearly entirely free of radioactivity since it was subject to the greatest dilution from pelagic currents outside the lagoon. The difference, Donaldson surmised, was the circulation of plankton, which was spreading radioactivity throughout the atoll ecosystem and drifting along with the currents to Enyu.[125]

Although Donaldson did not offer any editorial comments on the possible long-term repercussions of his findings to either the Navy or the AEC, the fact that organisms were taking in radiation into their bodies and concentrating it seemed of tremendous

[122] "Bikini Scientific Resurvey: Report of the Technical Director, Volume II," 18.
[123] Lauren Donaldson, Speech at the Atomic Energy Project, University of California Los Angeles, LRER, UWSC, box 13, folder speeches and writings, 10.
[124] "Bikini Scientific Resurvey: Report of the Technical Director, Volume II," 18.
[125] Lauren Donaldson, Speech at the Atomic Energy Project, University of California Los Angeles, LRER, UWSC, box 13, folder speeches and writings, 8.

import. That much was true for the Navy at least, even if the conclusions drawn in the resurvey technical report were somewhat ambivalent. As was noted in the report,

> At the present time worms and sea cucumbers are burrowing actively in and eating the highly radioactive bottom mud. Most of this passes right through them, and some of the feces are left on top, where bacteria compost them, returning most of the active material to the mud. However, some is left available to plants, which grow on the altered material. These plants are eaten by small fish, which pass almost all the radioactive material through the gut. Small fish with the small fraction of radioactive material they have retained in their tissues are in turn eaten by large fish, which again eliminate most of the radioactivity, carrying some of it to distant parts of the lagoon and some even outside. Plants remote from the explosion center get traces of radioactive material in this way, and the cycle is continued.[126]

Nonetheless, as the author of the report suggested in the first line of the report's summary, the potential effect of radiation on the Bikini's organisms was determined before the resurvey even took place. "The principle result of the BIKINI SCIENTIFIC RESURVEY *was to show* that the atomic explosions caused only minor, transient disturbance to the plant and animal populations of the area, the effects of which have almost completely disappeared after one year's [sic] time."[127] Even if Donaldson and his radiobiological monitoring party had discovered more profoundly disturbing levels of radioactivity in the ecology of the Bikini environment, given the apparent premeditated conclusions drawn in the final report, one wonders whether it would have made a significant difference.

In any event, the potential effects that this concentrated radioactivity in the bodies of fishes and other organisms might have for humans should testing continue was lost on

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[126] U.S. Armed Forces Special Weapons Project, "Bikini Scientific Resurvey: Report of the Technical Director, Volume II" (Armed Forces Special Weapons Project, December 1947): 102 (3)
[127] Ibid., 102 (1).

nearly everybody, Donaldson and Warren not withstanding. Revelle, for instance, discussed the radiobiological findings of the Resurvey in a paper presented at the National Academy of Sciences. “Although fishes and other marine animals from the entire lagoon area were often found to contain slight amounts of radioactivity," he announced, "no large-scale changes were observed in population density of reef or pelagic animals or in the relative abundance of different species.”[128] While true, such pronouncements failed to acknowledge the very questions that drove Donaldson's interest in the Resurvey—potential long-term effects of the cycling of radiation within the Bikini environment and its implications for human health. It wasn't that Revelle was purposefully downplaying these possible effects as much as a general blindness to the material connections between human beings and their environment. Yes, humans could conceivably accumulate some level of radiation by eating contaminated food stuffs, but most still considered the environment to be largely irrelevant with regard to human exposures. What mattered before and after Crossroads was external exposure. As the author of the widely read and influential sourcebook *The Effects of Atomic Weapons* put it in 1950, "The chances of radioactive material entering the system following an atomic explosion are believed to be extremely small...Even when there is considerable contamination of the ground, due to fission products, plutonium or uranium, it would be a matter of great difficulty for an appreciable quantity to enter the blood stream."[129]

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[128] Roger Revelle, "Bikini Revisited: Preliminary Results of the Scientific Resurvey During the Summer of 1947," *Science* 106, no. 2761 (1947). Revelle’s talk at the NAS was reported in the *New York Times*. See W.L.L., "Marine Life Survives on Bikini," *New York Times*, November 23, 1947.
[129] Samuel Glasstone, ed., *The Effects of Atomic Weapons* (Los Alamos: Los Alamos Scientific Laboratory, 1950), 9.

Despite Donaldson's work demonstrating the uptake and cycling of radiation among Bikini's organisms, environmentally mediated internal exposures still seemed unlikely.

Warren proved the partial exception, in public discourse at least. While the scientists were busy completing the resurvey at Bikini, *Life* magazine published a lengthy pictorial and written record of the activities of the biological surveys of the previous year.[130] Among the numerous glossy photographs included in the story were two of Donaldson's radioautograph images of a radioactive coral and fish, depicting vividly the pronounced uptake of radioactivity in the organisms. A short essay by Warren entitled "Conclusions: Tests Proved Irresistible Spread of Radioactivity" closed the feature. The purpose of the piece was to explore the question of whether there was "any effective protection for people involved in an atomic war" in the wake of the five bombs detonated to date.[131] The radiological effects on human beings from all the bombs prior to Baker, Warren argued misleadingly, were relatively minor owing to atmospheric dilution. Baker, however, posed a wholly "new danger of atomic warfare." Recounting the trouble with the contaminated ships, Warren emphasized that the radioactivity from Baker had "penetrated into every crevice." More alarming, however, was that as the "radioactivity of the fission products lessened, a more insidious hazard was discovered. The area of slight contamination was spreading outside the target area. The algae in the water, moreover, had absorbed radioactive particles and passed them on to little fish...Before the crews returned home, the safety section was monitoring almost every bite of food, every drink of water, every piece of laundry, the handrails of the ships, the beaches where the

[130] "What Science Learned at Bikini: Latest Report on Results," *Life*, August 11, 1947.
[131] That is, Trinity, the bombs dropped on Japan, and the two Crossroads detonations.

men went swimming." The radioactivity from Baker had enveloped every aspect of the Bikini environment, including its living inhabitants. Luckily, Warren concluded, the "deadly range of radioactive products from the atomic bomb had been clearly demonstrated under controlled conditions. The contamination of Bikini could have no serious consequences because the atoll is isolated from human habitation."[132] Warren's essay, in other words, opened up the possibility of environmental contamination with serious implications in the event of further testing, but foreclosed the possibility of radiological effects beyond the Bikini laboratory. Nature could affect the pathways of internal exposure, but held little significance for offsite exposures. To be sure, Warren had good reason to feel confident that the Crossroads tests harmed no one offsite; Bikini was indeed remote and little radiation was detected beyond the confines of the atoll. Nonetheless, such notions of spatial circumscription would imbue AEC officials with a deep seated hubris that similar forms of technoscientific control could be insinuated at test sites closer to home or with weapons of exponentially greater magnitude. What else are laboratories good for if they cannot control nature within their boundaries?

Conclusion

The success of Operation Crossroads and the reassurances that the radiation from Baker was not harming the organisms of Bikini Atoll formed the basis for the justification of the postwar nuclear testing program as the Cold War began to heat up in the late 1940s. Indeed, the absence of any local or widespread blast or radiological

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[132] Stafford L. Warren, "Conclusions: Tests Prove Irresistible Spread of Radioactivity," *Life*, August 11 1947.

effects on the biology of the atoll seemed sure confirmation of the Navy's technical control over the bomb. As a result, Crossroads helped effect a general attenuation of fears of the atomic bomb in the wake of the Japanese bombings and shaped the benefit/risk calculus of nuclear testing already being weighed in the immediate postwar years; following Crossroads, the benefits of creating an effective atomic deterrence against the Soviets seemed worth the potential health risks from fallout. So long as the bombs were detonated within the laboratory, all seemed well and good.

The relaxation of atomic tensions benefited the Atomic Energy Commission when the civilian agency took control over all of the facilities of the Manhattan Engineering District in January of 1947. Concerns about bomb testing soon turned to the possibilities of atomic energy. The bomb, the world was told, would help find a cure for cancer as radiation therapies were to be developed. And soon, atomic power generation "too cheap to meter" would replace fossil fuels. All of these anticipated futures soon overshadowed the deep fears elicited by the Crossroads tests, even as the United States expanded the nuclear testing program and the Soviets were racing toward completing a bomb of their own. Fear of an atomic war, of course, preoccupied many, but the hazards from testing were largely out of the minds of the general public in these early years. Although descriptions of the test sites as laboratories would wane in the early years of the 1950s, the assumptions that undergirded safety were nonetheless ever present. From 1947 until the middle years of the 1950s, the AEC confidently reassured the American public that the majority of fallout from nuclear tests was confined within the proving grounds and that any offsite exposures were due to short-lived external radiation that could be effectively defended against by simply remaining indoors. That the food people were eating might be contaminated from fallout was not even considered as a hazard among the general populace. The genie was effectively bottled up in the test site laboratories.

In the next three chapters, I explore the development and contribution of three environmental sciences to the understanding of fallout effects on human health from the end of the Bikini Scientific Resurvey to the ending of above ground nuclear weapons testing in 1963. Research supported by the Atomic Energy Commission in ecology, oceanography and meteorology in the 1950s and early 1960s would profoundly shape how the world assessed the risks of nuclear fallout. By pointing to the connections between human bodies, the environment, and geographical space, each of these sciences in their own way laid a critical cornerstone in the scientific foundation that justified the Limited Test Ban Treaty. Ironically, much of this work was enabled by the use of nuclear fallout as a tool to trace out these connections. And while many of these scientists in the early years were as wedded to the idea of the test sites as localized discrete laboratories as the Navy was, in tracing and tracking fallout as it moved throughout the world they would come to see the globe as a deeply integrated and holistic system.

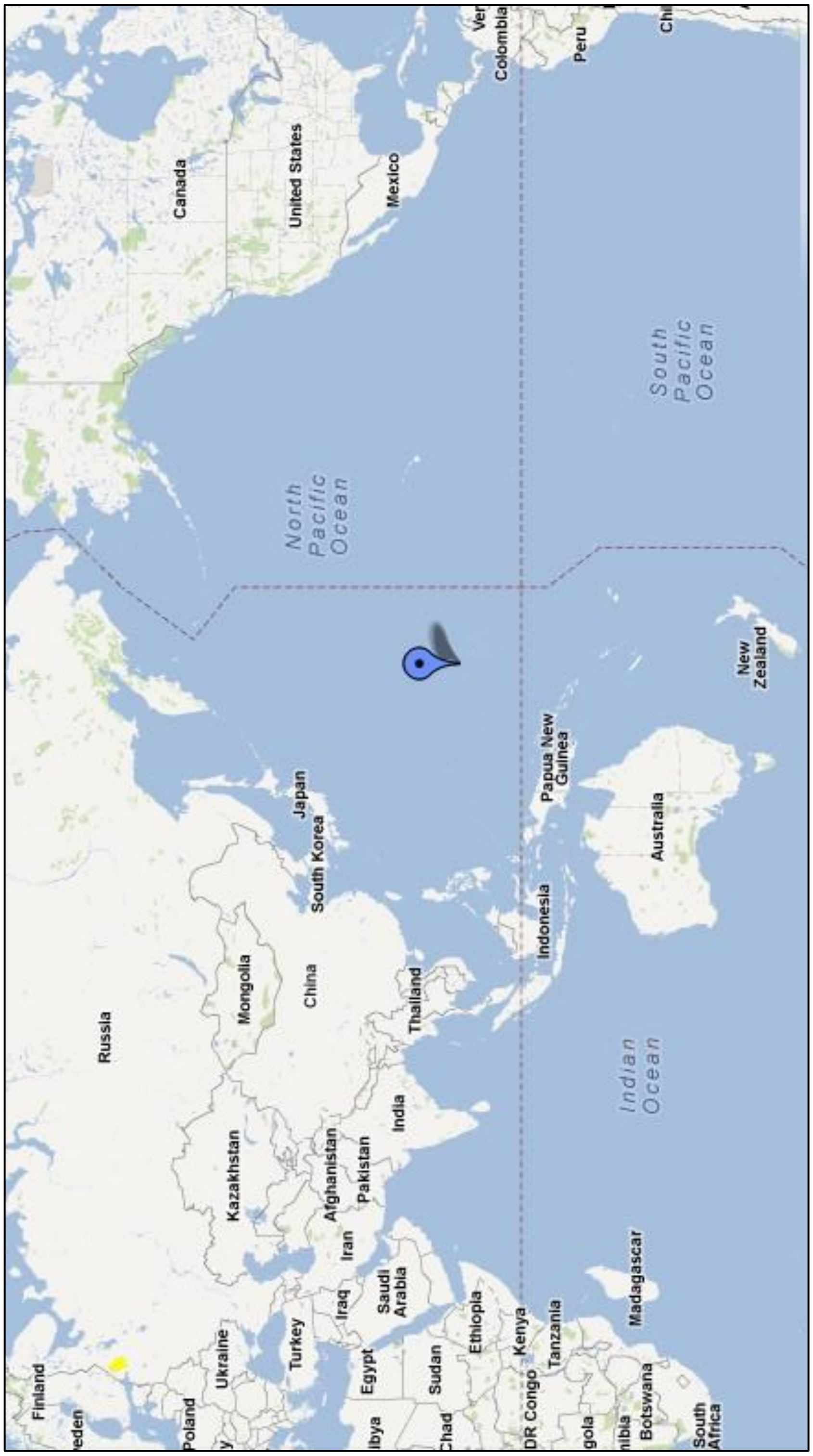

Figure 9 - Bikini Atoll. Source: Google Maps.

Figure 10 - Roger Revelle and Jeff Holter during Operation Crossroads. Copyright unknown. Source: Scripps Institute of Oceanography Library, Digital Collections.

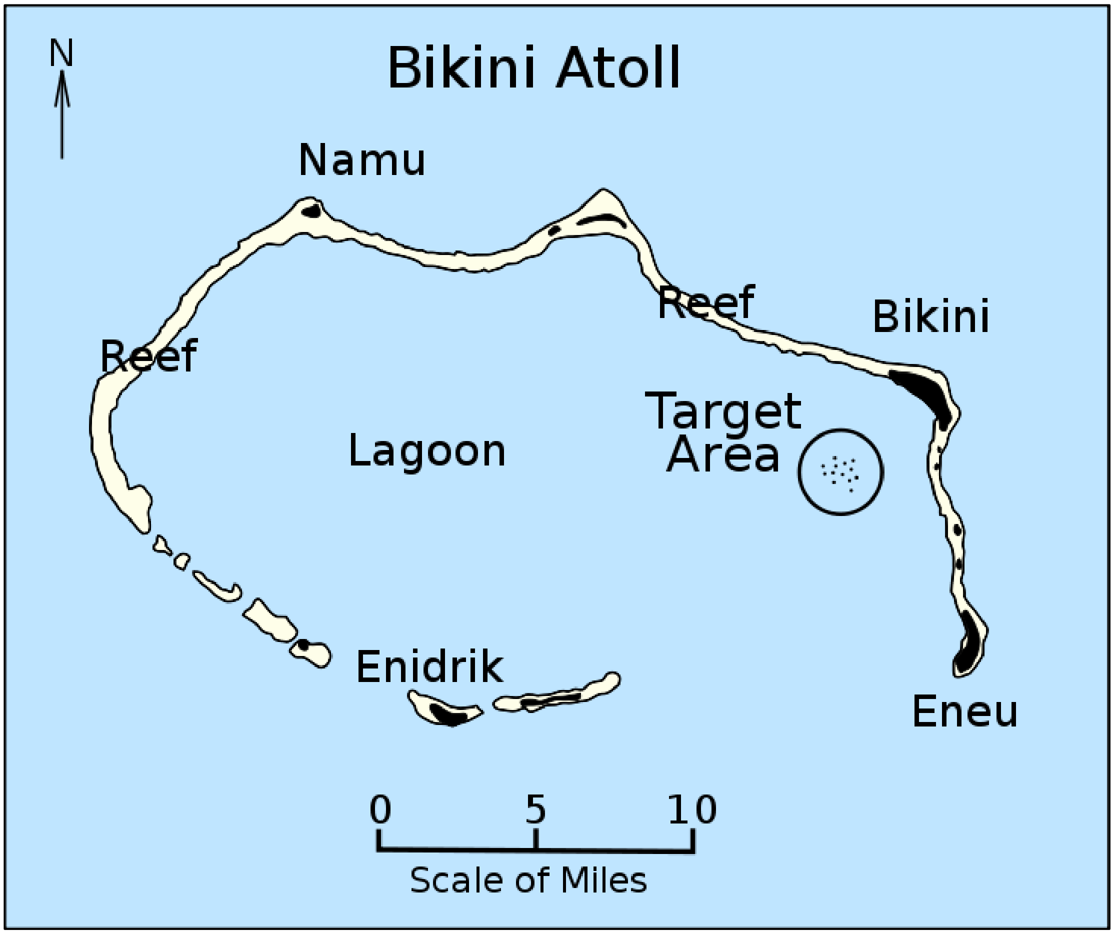

Figure 11 - Bikini Atoll. Note bounded-like features. Source: wikipedia. Public domain.

Figure 12 - Stafford Warren during Operation Crossroads. William Parsons is in the foreground. Source: Wikipedia. Public Domain.

Figure 13 - Lauren Donaldson,1947. Reproduced by permission of the University of Washington, Special Collections Library.

Figure 14 - Baker shot. Source: Wikipedia. Public domain.

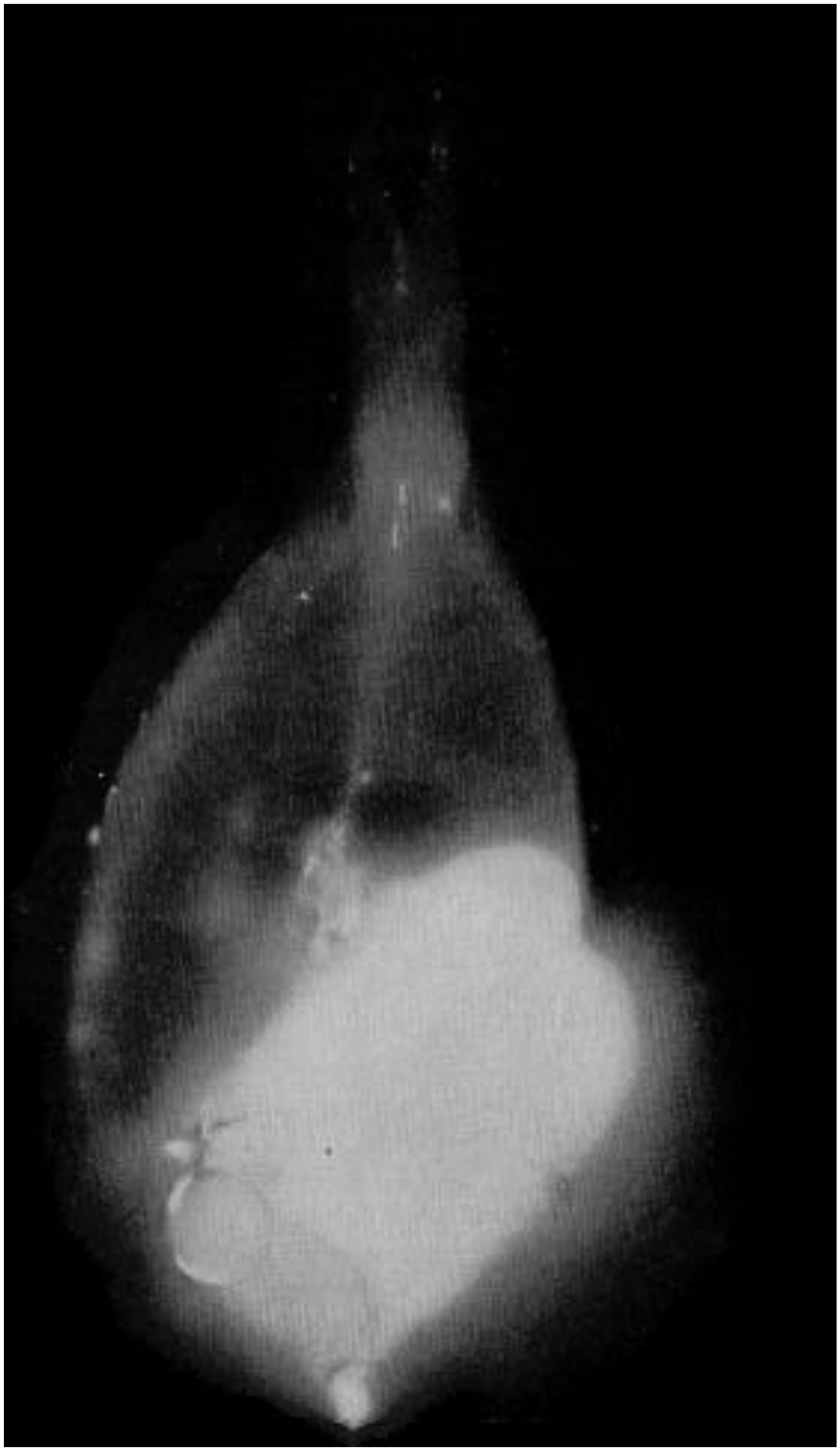

Figure 15 - Radioautograph of Bikini fish following Baker.
Source: Wikipedia. Public domain.

Figure 16 - Members of the Radiobiology Section. Reproduced by permission, University of Washington, Special Collections Library.

Geology	H.S. Ladd, U.S. Geological Survey
	J. Harlan Johnson, U.S. Geological Survey
	G.G. Lill, Office of Naval Research
	J.I. Tracey, U.S. Geological Survey
	J.W. Wells, Ohio State University
	R.D. Russell, Navy Electronics Laboratory
	E.H. Shuler, Navy Electronics Laboratory
Fisheries	V.E. Brock, U.S. Fish and Wildlife Service
	R.W. Hiatt, University of Hawii
	R.T. Tuiasosop, U.S. Fish and Wildlife Service
	J.C. Marr, U.S. Fish and Wildlife Service
	G.S. Myers, Stanford University
	O.R. Smith, U.S. Fish and Wildlife Service
	L.P. Schultz, U.S. National Museum, Smithsonian Institution
Biology	D.M. Whitaker, Stanford University
	L.R. Blinks, Hopkins Marine Station, Stanford University
	P.M. Brooks, Stanford University
	W.A. Gortner, Scripps Institute of Oceanography
	G.M. Smith, Stanford University
	J.P.E.Morrison, National Museum, Smithsonian Institution
	F.M. Bayer, National Museum, Smithsonian Institution
	A.C. Cole, University of Tennessee
Radiochemistry and Radiophysics	R.R. Williams, University of Notre Dame
	D.M. Black, Massachusetts Institute of Technology
	W.H. Hamill, University of Notre Dame
	J. Schulert, University of Minnesota
	L.F. Seatz, University of Tennessee
	J.H. Roberson, Clinton Laboratories, Oak Ridge
Radiobiology	L.R. Donaldson, Applied Fisheries Laboratory, University of Washington
	R.F. Foster, Biological Laboratory, Hanford Engineer Works
	C.F. Pautzke, Chief Biologist, Department of Game, State of Washington
	J.P. Pflueger, University of Washington
	F.H. Rofenbaugh, University of Washington
	A.H. Seymour, Applied Fisheries Laboratory, University of Washington
	A.D. Welander, Applied Fisheries Laboratory, University of Washington

Table 1- List of major scientific figures participating in the Bikini Scientific Resurvey

THE ATOMIC AGE AND THE AGE OF ECOLOGY:

FALLOUT RISKS AND THE RISE OF ECOLOGICAL THINKING

In an oral history conducted in the late 1970s, Lauren Donaldson, director of the University of Washington Applied Fisheries Laboratory, reflected on his lab's contribution to studies of fallout radiation in the environment for the Atomic Energy Commission (AEC) during the atmospheric nuclear weapons testing period. "I think we came along in a difficult period of time," Donaldson said.

> Really ahead of the environmental era when people began to look at the whole ecosystem as a unit, if you wish, or look at whole life cycles…We didn't have people [in the AEC] who were thinking outside laboratories. They weren't thinking in terms of total systems…When the testing program started in the Pacific, the concept was to go out, shoot a big firecracker, run back home and it was all over with. We were essentially alone in insisting that, No!—What happens this year, next year, ten years from now? We really could never get support for that.[1]

Indeed, throughout the AEC's stewardship of the nuclear weapons testing program from 1948 to the late 1950s, the focus of radiation protection centered on the external gamma dose, much as it had during the Manhattan Engineering District and Operation Crossroads. As J. Newell Stannard notes in his massive technical history on biological and medical research related to the health effects of radiation, "The external dose predominates in the early phases of fallout even more than with reactor fission products, a fact that underlies some of the apparent neglect of internal doses in considering

[1] Lauren Donaldson, interviewed by J. Newell Stannard, May 31, 1979, transcript, Nuclear Testing Archive, Las Vegas, NV (hereafter NTA), accession # NV0026916, 2.

weapons fallout that has become a source of recent concern."[2] Donaldson's remarks indicate a source for the AEC's predilection for short-term effects of gamma radiation: the best path for understanding radiation effects, the AEC thought, went through the toxicological laboratory, not the field. As a result, basic ecological considerations of environmentally-mediated exposures to the inside of the human body were left relatively unexplored.

Throughout the 1950s, however, Donaldson and other ecologists such as John Wolfe, Stanley Auerbach and Eugene Odum would press the Commission to fund ecological studies related to atomic energy development. Although their proposals fell on deaf ears in the early years of the decade, their persistence paid off in the late 1950s and early 1960s as they managed to turn the focus of the AEC toward the potential health effects of lingering environmental radiation. In the process, they helped enact a radical shift in how the risks of nuclear fallout were evaluated. Through the use of fallout or unique radioactive tags as ecological tracers, they demonstrated empirically the critical ecological pathways that linked environmental radiation to the health of the human body

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[2] Stannard, *Radioactivity and Health: A History*, 882-3. Other scientists involved in one way or another with atomic energy have noted the emphasis on the external gamma exposure as well. Merril Eisenbud, head of the AEC's Health and Safety Laboratory and developer of the offsite fallout monitoring network, for example, told Stannard in an interview that "when they [AEC] set up the weapons testing program, the only attention was to external radiation… They had no concept there might be problems due to internal radiation." Merril Eisenbud, interviewed by J. Newell Stannard, July 9, 1979, transcript, NTA, NV0702766, 4. Karl Z Morgan, the "father" of health physics similarly argued that "We gave little consideration to internal dose from body intake of radionuclides because almost nothing had been published on the subject except some information on high-level exposure to radium. We focused primarily on preventing the radiation syndrome (acute death) from very high exposures. Our secondary concern was to prevent acute external radiation damage, such as skin erythema. Unfortunately, we accepted the threshold hypothesis: that so long as we avoided the skin-reddening threshold dose, all of us were safe. We erroneously thought that the system of macrophages found in human bone marrow, liver, and spleen would repair any injury within a few days. Radiation-induced cancer, lens cataracts, life shortening, and genetic damage never occurred to us as a possibility except at very high doses." Morgan and Peterson, *The Angry Genie: One Man's Walk through the Nuclear Age*, 21.

and showed how the environment, through the biomagnifying mechanisms of the food chain, could deliver radiation to the inside of the body greater than the external dose. It was only by seeing fallout as an environmental problem—"thinking ecologically"—that potential internal emitting radionuclides such as strontium-90 and iodine-131 were conceived as hazards and genuine risks.

Ecological thinking also helped produce more holistic views of geographic space. As I described in the previous chapter, a common ecological practice in aquatic ecology entailed finding closed or self-contained places where outside influences could be limited or controlled, enabling ecologists to study whole communities in situ. This practice continued throughout the 1950s. Donaldson, for example, continued to refer to Pacific atolls as laboratories throughout the 1950s as he had during the Bikini Scientific Resurvey. So too did Eugene Odum and Oak Ridge ecologist Stanley Auerbach privilege closed spaces as ideal natural laboratories for ecological study. For ecologists, working in these natural labs combined the best of both laboratory and field practices. Their boundedness allowed ecologists a good measure of lab-like control, but did not reduce the importance of place. But throughout the late 1950s, the idea that the ecosystem could be so easily delimited (especially in the terrestrial realm) gradually began to fade. Ecologists would move freely between different ecosystem scales, usually without much epistemological difficulty. In fact, the development of radiotracer technologies and mathematical modeling served to harden ecological practice in such a way as to reduce the need for the natural laboratory. But perhaps more importantly, tracking radiation as it moved through the environment showed that radiation could not be so neatly bounded.

As fallout was tracked throughout the globe, the idea of the ecosystem likewise became global. Thus in hindsight, it seems no mere coincidence that as outmoded ideas of the boundedness of bodies and environment gave way to "porosity," to use Linda Nash's term, so to did the boundaries of the test sites erode into the ecosphere. Ecological thinking entailed breaking down the boundaries between human bodies, the environment, *and* geographical space.

This story of the rise of ecology and its effects on how fallout risks were perceived has not been explored extensively in the historical literature. Most studies investigating the fallout controversy during the period, for example, focus almost exclusively on the debate over the health effects of low-level radiation. As a result, they have played down or virtually ignored the the critical role that new knowledge about the contamination of important food staples in the human diet via the food chain drove concerns about fallout by linking the human body to irradiated environments.

Historians of ecology have similarly failed to address adequately the influence of ecosystems ecology on AEC safety practices. Typically, they focused on the impact of AEC support of ecosystems ecology by exploring the ways that radiotracer technologies and computer-based modeling "hardened" and thus professionalized the discipline.[3] The history of ecosystems ecology at mid-century, then, has been geared primarily toward explaining how AEC patronage created the context and conditions that enabled the growth of "new" or "systems" ecology. Although it has been noted that AEC interest in

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[3] For example, Bocking, *Ecologists and Environmental Politics: A History of Contemporary Ecology*; Bocking, "Ecosystems, Ecologists, and the Atom: Environmental Research at Oak Ridge National Laboratory."; Hagen, *An Entangled Bank: The Origins of Ecosystem Ecology*; Kwa, "Radiation Ecology, Systems Ecology and the Management of the Environment."

ecosystems ecology was driven by the need to provide "a basis for the management of radioactive contamination of the environment," there is little sense of how the emergence of ecology fundamentally restructured how the AEC went about doings its regulatory business in the ways that I have described above.[4]

In this chapter, I focus on the influence of ecosystems ecology (the first prong in the environmental science triumvirate that I discuss) on the ways that the AEC assessed the risks of nuclear fallout. I begin by exploring the ecological research on fallout conducted by two different groups in two different environments. First, I discuss the research conducted on radionuclide and plutonium cycling by the UCLA Atomic Energy Project Radioecology Division at the NTS and Trinity. Second, I return to Lauren Donaldson and the Applied Fisheries Laboratory's efforts to study environmental radiation at the Pacific test sites. Throughout both of these discussions, I emphasize the pioneering ecological research performed by these groups and the difficulties they encountered in garnering AEC support for their work. I also explore the continued focus on the gamma dose by NTS and AEC officials, despite the work of the AEP and AFL, and its effect on how fallout risks were evaluated. In the next section, I analyze the development of radioecology in the AEC in the mid-1950s, focusing specifically on new radiotracer practices and the lobbying of individuals like Odum, Donaldson, and Wolfe to secure a place for ecology within the Commission. I pay close attention to the ways that work conducted in the new discipline of radioecology began to clarify the importance of ecological factors in radiation exposures for the AEC. In the last section, I discuss the

[4] Bocking, "Ecosystems, Ecologists, and the Atom: Environmental Research at Oak Ridge National Laboratory," 44. See also Bruno, "The Bequest of the Nuclear Battlefield: Science, Nature, and the Atom During the First Decade of the Cold War."

emergence of the radioiodine hazard in the early 1960s. In particular, I focus on the story of Harold Knapp's efforts to reconstruct the radioiodine doses to downwind NTS populations that the AEC had failed to account for during the majority of the atmospheric nuclear testing period. Finally, I conclude this chapter with some thoughts on the lasting influence of ecological thinking during the fallout controversy on the environmental movement and what this story tells us about the intersections of scientific ideas and practices, technology, and the material environment.

Radioactive Deserts: Trinity Revisited, the Nevada Test Site, and the UCLA Atomic Energy Project

In 1947 as Lauren Donaldson and the Applied Fisheries Laboratory were participating in the Bikini Scientific Resurvey, Stafford Warren sent out a similar biological survey party from his recently established Atomic Energy Project at the University of California at Los Angeles Medical School to assess the radiological situation at Trinity. All in all, the Manhattan Engineering District had proven to be a boon for Warren's professional career. He entered the MED as an experienced, but not particularly well-known, radiologist and left it with unmatched credentials in administering a large-scale radiobiological research program. Given his experience in directing the Health Division of the MED and the ostensibly clean track record his radiation safety efforts, he was quickly offered the directorship of the Division of Biology and Medicine when the Atomic Energy Commission went online in January of 1947. He declined. Instead, as the chairman of the medical advisory committee to the AEC, he succeeded in founding the medical and biological research program of the Commission in the nation's universities. One such university program was the Atomic Energy Project (AEP) at the newly established UCLA Medical School, which hired Warren to serve as its first dean while he sat on the advisory committee.[5] One of the first orders of business for Dean Warren was a survey of the Trinity Site.

For Warren, because the Trinity area had been subjected to fallout only once, it offered one of the best opportunities for studying radiation effects in a terrestrial environment if the Commission were to expand the nuclear testing program or in the event of future nuclear power production. Moreover, Warren felt that such studies were a necessary part of the Commission's statutory duties as a regulator of atomic energy. Nonetheless, when he wrote to the Commission reminding them that Alamogordo region was a "direct responsibility of the Atomic Energy Commission" and urging them to "set up, as soon as possible, [a] proper survey," he faced stiff resistance.[6] As he later recalled, "Of course, there was the first test at Alamogordo; there was Bikini test area; and then there was Japan. These were very important areas, in my opinion, where surveys and continuous observations would give us a lot of information of what had happened, and the mechanisms of the after effects. I soon found that Shields Warren [the director of the DBM] was truly a pathologist and not interested in industrial medicine and environmental things. Of course, this field of work was environmental to a great extent." Warren also chalked up the AEC's reticence to support the Trinity survey to the fear of finding evidence of severe contamination, thereby opening up the possibility of legal action. His reference to Shields Warren's pathology background was closer to the mark. The

[5] Lenoir and Hays, "The Manhattan Project for Biomedicine."
[6] Warren to Carroll Wilson, April 7, 1947, NTA, NV0726688.

majority of funds for medical or biological research on radiation effects in the early years of the Commission were devoted almost exclusively to laboratory-based disciplines. What few field-based research programs the AEC did fund, such as the AFL, were not supported well.[7] Indeed, the funding the AEC provided to the AEP (and hence the UCLA Medical School) was motivated by the Commission's interest in nuclear medicine. Field studies, in short, were the farthest thing from AEC officials' minds. Their first priority was promoting the atom for basic medical research.[8] A far distant concern was the health effects of environmental radiation. Warren, however, did manage to scrape together funds for a series of surveys at Trinity during the summers of 1947-1950.

Warren himself did not participate in the surveys, busy as he was with building the medical school. AEP scientists Albert Bellamy and Kermit Larson, both of whom had participated in Operation Crossroads, instead led the Trinity Survey Program. The initial survey in August of 1947 was relatively broad, encompassing collection and analysis of local mammals, birds, plants, and soil within the site and the surrounding area. The results of the survey confirmed what radiation monitors had detected shortly after the Trinity detonation: alarmingly high, in some cases higher, radiation levels (or hot spots) were found offsite than at ground zero. The Chupadera Mesa, used for cattle grazing, was particularly radioactive. As a result, the survey party sampled grasses and cattle

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[7] Stafford Warren, "Report of the 23-24 January 27 1947 Meeting of the Interim Medical Committee, U.S. Atomic Energy Commission," enclosed in Warren to Nolan, January 29, 1947, NTA, NV0727195. Note that Warren (chairman of the advisory committee) was the author of the report. The fact that he signed off on these budget priorities suggests the kind of fierce resistance he met in trying to establish environmentally-focused studies.

[8] Radioisotope distribution to medical researchers was one early priority. See Angela N. H. Creager, "The Industrialization of Radioisotopes by the U. S. Atomic Energy Commission," in *Science-Industry Nexus: History, Policy, Implications* (Sagamore Beach, Maine: Science History Publications/USA, 2005, 2005); Angela N. H. Creager, "Nuclear Energy in the Service of Biomedicine: The U.S. Atomic Energy Commission's Radioisotope Program, 1946-1950," *Journal of the History of Biology* 39, no. 4 (2006).

feces for radioactive content. The plants demonstrated marked uptake of fission products and alpha emitters (presumably unfissioned plutonium). The cattle feces too were radioactive. While none of the cattle exhibited any obvious sign of injury, some birds showed morphological abnormalities in their feet and claws.[9] Larson and Bellamy declined to draw any firm conclusions as to their findings. The implications for human health, however, were obvious and they recommended that further surveys be conducted in the near future. The subsequent surveys in the summers of 1948-1950 focused on particular problems encountered during the initial 1947 investigation. The following summer, for example, a more intensive investigation of the contaminated soils and local organisms was conducted along a transect edging along the Chupadera Mesa to a distance of roughly a hundred miles [Figure 17].[10] In this survey, Larson and Bellamy were able to draw firmer, if still somewhat ambivalent, conclusions: "This report includes data indicating that the present concentrations of radioactive fission products are not great enough anywhere in the contaminated region...to present a significant immediate hazard to man or his domestic animals from total body exposure to beta gamma irradiation." That much was "gratifying," the authors noted, but cautioned that "it would be rash to conclude, in the absence of specific information, that now, no hazards associated with products of the bomb detonation exist in this area, the harmful effects of which may not appear for a number of years." In that respect, the authors were thinking of potential

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[9] Stannard, *Radioactivity and Health: A History*, 928-9; Szasz, *The Day the Sun Rose Twice: The Story of the Trinity Site Nuclear Explosion, July 16, 1945*, 136-7.

[10] Larson, Kermit et al., "The 1948 Radiological and Biological Survey of Areas in New Mexico Affected by the First Atomic Bomb Detonation," Atomic Energy Project Report UCLA 32. This report can be found online at the Los Alamos National Laboratory Research Library website http://library.lanl.gov/. Figure is reproduced from this source. It is in the public domain.

effects due to ingested radioactivity, even as they concluded that "a human being would have to go to some trouble to expose himself to the minimum permissible daily dose of beta-gamma irradiation."[11] The Trinity Survey team failed to find any immediate internal bodily hazards, but they were beginning to ask the right questions.

There were other interesting findings in terms of field practice in subsequent surveys. During the 1950 survey, Bellamy encountered a number of heavy dust storms while out collecting material. "The entire valley—some 3,000,000 acres—is on the move," he wrote to Warren in one letter. The shifting environmental conditions created by the dust storms impressed upon Bellamy the need to reconsider how they approached the field surveys. "We, or some of us," he emphasized to Warren, "have been worshipping too much the laboratory tradition of 'controls'. In the unique and rapidly changing conditions in and around the Crater Region [ground zero], there can be no control. One only has to experience the five heavy dust storms (so far); the one rain of cloudburst proportions and two other heavy rains and numerous showers, to realize that controls can exist, for the area, only in the imagination."[12] One could not, in other words, simply ignore place. As both Larson and Bellamy were beginning to realize, environmental conditions mattered with regard to the distribution of radioactive material and the pathways of internal exposure.

Unfortunately, the Trinity surveys went largely unnoticed by the AEC and were not reported in the open literature for over a decade. Even then, because the reports were

[11] Ibid., 99-100.
[12] This letter, dated August 22, 1950, is reproduced in Larson, Kermit, et al., "Alpha Activity Due to the 1945 Atomic Bomb Detonation at Trinity, Alamogordo, New Mexico," AEP Report UCLA-108, 40. This report can be found in NTA, NV0008975.

classified secret, access to the reports was limited. Furthermore, for reasons that remain unclear, only thirteen copies of their reports were ever printed.[13] Had the reports been better circulated it may not have improved the Commission's appreciation of internal hazards in any event. Shortly following the final 1950 survey, Warren wrote to Bellamy warning him that the AEC was likely to terminate the survey program. "There is some reason to be apprehensive about future budgets for the Alamogordo Program...I'm afraid that you may have to face the elimination of the program altho [sic] they [the AEC] may allow us enough to keep a technician or two."[14] The apprehensiveness of the AEC stemmed not from budget constraints but rather disinterest. Historian Ferenc Szasz, who interviewed Stafford Warren and Larson extensively for his book on Trinity, noted that one reason behind the cancellation of the Trinity surveys was due to the fact that the "head of the Division of Biology and Medicine [Shields Warren] was never sympathetic to field studies."[15]

One more survey was conducted later in 1955, but by and large, investigations of the environmental radiation found at Trinity remained tabled until the late 1960s. Still, the lessons learned from the surveys were not lost on the AEP team, especially Larson. In a summary of the Trinity radioecology studies that Larson published in the early 1960s, he concluded that:

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[13] Ferenc. M Szasz, "The Impact of World War II on the Land: Gruinard Island, Scotland, and Trinity Site, New Mexico as Case Studies," *Environmental History Review* 19, no. 4 (1995): 25.

[14] Warren to Bellamy, September 29, 1950, "Alamogordo Program," The Administrative Files of Stafford Warren, Record Series number 300, (hereafter Warren AF), Department of Special Collections, University Archives, Charles E. Young Research Library, the University of California at Los Angeles (hereafter UCLA-UA), Los Angeles, CA, box 36, folder 5 AEC Personnel 1950.

[15] Szasz, *The Day the Sun Rose Twice: The Story of the Trinity Site Nuclear Explosion, July 16, 1945*, 104-1.

> Even at this time (1950) it was apparent from the data available from 1947-1951 that no hazard from external total body exposure to ionizing (gamma) radiation existed anywhere outside the Fenced Area [ground zero]. However, we did not assume that an "internal emitter" problem might not exist. It was clear that the entire area was in a state of flux with respect to distribution and biological availability of radioactive fission products and plutonium. Evidence was accumulating from the annual biological surveys and correlated laboratory that many years might pass before a biological equilibrium with respect to residual contamination could be established.
>
> These findings indicated that surveys of animal populations must take into account the climatology, the topography of the country, the soil properties, the food chains available to the animal in the specific area, the habits of the animals, and finally the time required for all these factors to create the conditions whereby the animal might consume, absorb, and deposit the radioactive materials from the fallout. It was believed that the data reported demonstrated the complex and time-consuming interaction of these factors.[16]

To be sure, Larson's comments (particularly the last sentence in the quotation) reflected a good deal of hindsight with respect to the recognition of the internal emitter problem. Nonetheless, he accurately described the upshot of the early AEP Trinity surveys; gamma radiation measurements, while good for the immediate evaluation of fallout hazards, were not adequate for judging long-term internal hazards. Long-term impacts were a function of specific fission radionuclides and comprehending their distribution and effects depended on a thorough knowledge of the environment. The problem was that no one in the upper echelons of the AEC administration was listening. Fallout was a factory problem, not an environmental one.

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[16] Kermit Larson, "Continental Close-in Fallout: Its History, Measurement and Characteristics," in *Radioecology: Proceedings of the First National Symposium on Radioecology Held at Colorado State University, Fort Collins, Colorado, Spetember 10-15, 1961*, ed. Vincent Schultz and Alfred W. Klement Jr. (New York: Reinhold Publishing Corporation, 1963), 21.

The overweening emphasis on external radiation undergirded the safety practices at the Nevada Test Site (NTA) when it was established in December of 1950, a few months after the AEP's final Trinity survey.[17] As with the original Trinity safety plan, the feasibility of testing in the southern Nevada desert stemmed from the idea that fallout from the tests would be contained within the test site and that the primary danger to consider for off-site populations was exposure to external gamma radiation.[18] During a meeting at Los Alamos to evaluate the suitability of the NTS shortly before President Truman approved the test site, for example, a panel of radiation experts concluded that a "tower-burst bomb having a yield of 25 kilotons [i.e., a trinity-type weapon] could be detonated without exceeding the allowed emergency tolerance dose of 6-12 r[oentgens] outside a 180° test area sector 100 miles in radius."[19] The panelists considered the 6-12 emergency roentgen dose, which is a measure of external exposure, as exceptionally conservative. Although the question of potential ingested doses was brought up during the meeting, it was dismissed as highly improbable. As one panelist noted, the data from prior tests suggested a person "would have to ingest a kilogram of the material immediately under the shot tower in order to ingest enough plutonium to cause physical damage."[20] No discussion of indirect ingestion of radioactive material via plant uptake or other food chain dynamics was broached during the meeting.

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[17] The Nevada Test Site was originally named the Nevada Proving Grounds.
[18] I discuss the notion of fallout containment and the atmospheric assumptions that guided this idea more fully in chapter 6.
[19] [19] Frederick Reines, September 1, 1950, "Discussion of Radiological Hazards Associated with A Continental Test Site for Atomic Bombs," Los Alamos report # LAMS-1173, NTA, NV0030434, 24.
[20] Ibid., 7.

Through nearly the entire period of atmospheric nuclear weapons testing at the NTS (1951 to roughly 1957) direct radiological monitoring was confined to a radius of 200 miles from the test site and exposures to offsite populations expressed solely in terms of the the gamma dose.[21] Responsibility for monitoring in the early years of the test site shifted between personnel within the health division at Los Alamos under the direction of Thomas Shipman and the military. Eventually, the Public Health Service took over most monitoring duties in 1953. Radiological monitoring typically involved groups of Rad-Safe (radiation safety) personnel roving throughout the desert tracking, as nearly as possible, the movement of the radioactive cloud as it passed over head. Measurements of gamma radiation were taken with standard Geiger-Muller counters and personal radiation exposure was monitored through film badges, which were subsequently analyzed once the group made their way back to headquarters. Exposure standards for civilian populations near the test site were set at 3.9 roentgens for a ten week period. Deemed conservative by the standards of the time, the 3.9 dose rate measured only external forms of radiation.[22]

Periodically, the radiation safety monitors and AEC officials would meet to reconsider safety practices and off-site exposure criteria. In 1953, for example, a committee (dubbed the Committee to Study the Nevada Proving Ground) comprised of scientists associated with the NTS as well as the new AEC's Division of Biology director

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[21] Off-site monitoring (that is, farther than the 200 mile radius) was established after the first testing series in 1951 when fallout was detected in upstate New York. The resulting continental monitoring system, however, was not a direct method of monitoring fallout and was not intended to inform monitors of possible countermeasure actions in the event of serious contamination off-site. This aspect of fallout monitoring is treated more fully in chapter 6.

[22] The organization and activities of the Rad-Safe groups throughout the atmospheric testing era can be found in Hacker, *Elements of Controversy: The Atomic Energy Commission and Radiation Safety in Nuclear Weapons Testing, 1947-1974.*

John Bugher convened to reconsider the safety practices at the test site. The primary motivation for the committee was a series of fallout episodes during the recent Upshot-Knothole tests, where civilians in southern Utah were exposed to radiation levels that exceeded the 3.9 r tolerance dose. In light of the overexposures, the AEC wanted to revaluate the potential health costs of testing and balance their findings against the perceived national security benefits of testing.

The eventual report, issued February 1, 1954, was lengthy and wide-ranging, including reports addressing public relations, radiological safety and the apparent necessity for nuclear testing. With the executive summary alone running over sixty pages, along with the twenty-four supplementary reports from various members of the committee, the document was the most complete and thorough consideration of the practicability of continental nuclear testing to date.

The committee's findings were hardly groundbreaking, however. Acknowledging the inherent risk associated with nuclear testing, the committee concluded that the "potential hazard [was] low and fully acceptable in view of national necessity, and that it can be justified to the Nation."[23] The justification was based largely on previous measurements of offsite gamma exposures. "At any point outside of NPG, radiation exposures can be produced by five different mechanisms," the report claimed. These pathways included, "Passage of the cloud over-head; fallout on the ground; fallout on the skin; inhalation of aerosol; and fallout in water supplies. All five contribute to interference with commerce and industry. Only the second and third are likely to be

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[23] n.a., "Report of the Committee to Study the Nevada Proving Grounds," February 1, 1954, NV0061646, pg.2 of abstract.

important in most situations to human and animal health."[24] Bugher concluded much the same in his report to the committee. His comments are worth quoting at length for they acknowledge the potential for internal exposure from strontium-90, but reveal a rather short-sighted view of of the total environmental factors involved.

> We have no evidence at the present time which would indicate that *where the requirements expressed in terms of gamma exposure* have been met that there need be concern with regard to inhalation or the ingestion of contaminating material in drinking water. It is likely, for both water and air contamination, that the important isotopes are actually Strontium^{89} and Strontium^{90} which appear to be relatively soluble and thus capable of early transport to bone from either system concerned. In no case does the likelihood of acquiring anything like the permissible limit of these isotopes appear significant.[25]

No mention was made of other potentially critical environmental pathways such as the food chain. Another section of the Committee's report by Howard L. Andrews of the National Institutes of Health reaffirmed Bugher's identification of the main risks posed by fallout. "In analyzing off-site radiological hazards only two… appear to be of major importance: Whole body gamma radiation received from particles deposited on the ground and beta radiation from particles deposited on the skin."[26] Accordingly, the 3.9 roentgen exposure standard stood.

There were some indications to the contrary, however. Following the Trinity surveys, the AEC contracted the UCLA AEP group to conduct field- and laboratory-based research on radionuclide contamination within the boundaries of the NTS.

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[24] Ibid., 31.
[25] John C. Bugher, "Interpretation of the Standards of Radiological Exposure," Attachment to the Report of the Committee to Study Nevada Proving Grounds, September 8, 1953, NV0061647, 4. Emphasis mine.
[26] Capt. Howard L. Andrews, "Residual Radioactivity Associated with the Testing of Nuclear Devices within the Continental Limits of the United States," Attachment to the Report of the Committee to Study Nevada Proving Grounds September 13, 1953, NV0061647, 7.

According to Kermit Larson, who led the effort, the AEP studies were not dictated directly by the AEC. Instead, his group functioned primarily as a "research unit." "We chose the shots that we thought we wanted to work," Larson later remembered.[27] As a result, their early studies were not necessarily associated with a particular shot and were thus not part of the general Rad-Safe operations plan. AEC budgets for the AEP research at the NTS were accordingly limited and the field work performed only intermittently.[28] Nonetheless, much of the work performed by the AEP pointed to radionuclide contamination and the ingestion route of exposure as perhaps the controlling factor in protecting human health from fallout.

Early AEP studies saw Larson and his colleagues tramping throughout the test site collecting native flora and fauna. Initially, collections were made close to ground zero for the shots. But as Larson remarked later, "we got a little more extensive each time we were pushing for distance. We started with 50 miles maximum, because, after all, fallout wasn't going to go beyond that, we were told."[29] When the AEP group—which by 1953 had begun calling itself the Division of Radioecology—conducted collections further afield with the Upshot-Knothole tests, what they found surprised them. Plants and animals sampled close to ground zero all showed significant levels of uptake of various fission products. But at distances of up to 130 miles or so, they showed, in many cases, higher levels than at ground zero. According to AEP member R.C. Lindberg, during one of these early field trips "animals collected 31 miles from GZ [ground zero] contained

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[27] "Report of the Monitors Meeting, Volume III," June 27, 1980, NTA, NV011586, 6.
[28] Stafford L. Warren, "An Exceptional Man for Exceptional Challenges," interviewed by Adelaide G. Tusler, 1966-1968, transcript, Oral History Program, University of California Los Angeles, Charles E. Young Research Library, Department of Special Collections, Los Angeles, CA, 1117.
[29] Report of the Monitors Meeting, Volume III," June 27, 1980, NTA, NV011586, 6.

greater amounts of radioactive material than animals sampled 16 miles from GZ, although the environment at 31 miles received much less contamination. Data from four stations between Yucca Flat [on the test site] and St. George, Utah, revealed that the accumulation of radiostrontium in animal bones as measured 1 year after contamination was approximately five times greater in the St. George Area (130 miles distant) than within 5 miles of GZ."[30] By Operation Teapot in 1955, they had discovered a similar phenomenon with radioiodine concentrations Jackrabbit thyroids.[31] At the time, radioiodine with its half-life of eight days had been almost universally regarded as an unlikely hazard.

Despite the AEP data, no concentrated effort was put forth to measure the radiostrontium or radioiodine exposures to people living in downwind communities such as St. George, Utah. Their risk level was still determined by the measurements of external dose. As AEC Chairman Lewis Strauss commented during a Congressional hearing in 1955, "Generally speaking, the exposure experienced by the American people from the current Nevada tests have been less than the radiation they normally receive every few days from natural sources."[32] That may have been true, but only if one only considered the external dose.

It would not be until 1957 that the AEC would provide adequate support to the AEP for ecological studies in or near the Nevada Test Site. Unfortunately, increased funding and prioritization of the AEP radioecology studies proved to be a double edged

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[30] R.G. Lindberg et al., "Factors Influencing the Biological Fate and Persistence of Radioactive Fall-Out, Operation Teapot, February-May 1955," NTA, NV0014436, 16.
[31] Ibid., 70-1.
[32] Quoted in Titus, *Bombs in the Backyard: Atomic Testing and American Politics*, 83.

sword. Throughout the late 1950s and early 1960s, the AEP Radioecology Division underwent a series of reorganizations in response to operational needs at the NTS. As a result, although AEP personnel such as Larson were producing valuable ecological data of importance to the internal emitter problem, much of the work remained under-analyzed and under-reported.[33] Some of the data would find its way into various AEC reports, but the most comprehensive and important reports by the AEP on NTS fallout would not be published until well after the Limited Test Ban Treaty.[34]

In any event, the story of fallout from the NTS in the four or five years leading to the LTBT was largely about radioiodine exposures. And the realization of the radioiodine hazard involved another set of actors, as we will see later. In the meantime, we need to return to the Pacific and pick up on the activities of the Lauren Donaldson and the Applied Fisheries Laboratories since the Bikini Scientific Resurvey. Their story, it turns out, was not dissimilar from the AEP's. They too ran into resistance from the AEC when they sought to pursue and expand ecological studies of radionuclide cycling and internal exposure pathways at the Pacific proving grounds.

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[33] Information on the reorganization of the AEP Radioecology Division and the AEC's reviews of the program can be found in National Archives and Records Administration, College Park (hereafter NARACP), Records of the Atomic Energy Commission, Record Group 326, Entry Number 73b (hereafter RAEC), box 16, folder NTS Environmental Programs; and Warren-AF, UCLA-UA, box 32, folders AEP general 1957.

[34] For example, Kermit Larson et al., July 26, 1966, "Distribution, Characteristics, and Biotic Availability of Fallout, Operation Plumbbob," report number WT-1448. This report was issued nine years *after* Operation Plumbbob. An example of the AEP's work being used in AEC reports or other publications can be found in G. M. Dunning, "Radiation Exposures from Nuclear Tests at the Nevada Test Site," *Health Physics* 1, no. 3 (1958). Gordon Dunning, it should be noted, was often responsible for making sense of the research related to fallout and compiling various reports. He has often been scorned for what many view as his sugarcoating, or in some cases his dissembling, of fallout effects. See especially, Fradkin, *Fallout: An American Nuclear Tragedy*. Dunning penned a response to these accusations in a book-length untitled manuscript in 1990. See NTA, NV339939.

Radioactive Waters: Ecology, the AFL, and the Pacific Proving Grounds

As I noted in the previous chapter, when Donaldson learned of the nature of the MED project sometime in 1944 he convinced Stafford Warren to establish a small onsite laboratory at Hanford to investigate problems directly related to the potential biological effects of reactor effluent on fishes in the Columbia River. To lead the lab Donaldson chose one of his graduate students, Richard Foster. Although Foster's lab would eventually be assumed under the health physics section of Hanford Works, we begin with his work because in the latter years of the 1940s he stumbled upon an ecological concept that would prove critical for not only studies of environmental radiation but ecological studies of pollution in general: biomagnification.

Foster's initial studies at Hanford were not dissimilar to what Donaldson was doing back at the University of Washington campus, the main difference being that the fish were exposed to reactor effluent as opposed to x-rays. And like Donaldson's first foray into radiological effects on fish, Foster's studies in the early years were not in a strict sense ecological. The lab consisted of a Quonset hut near the reactors for easy access to effluent and a series of toughs for the experiments [Figure 18]. Early practices centered on exposing fish (mature and fingerling, as well as eggs) in the troughs to various levels of diluted effluent so that "conditions which existed in the river could be duplicated as nearly as possible" [Figure 19].[35] Specific attention to individual radionuclides present in the effluent was not considered. Instead, the focus remained

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[35] Richard F. Foster, August 31, 1946, "Some Effects of Pile Area Effluent Water on Young Chinook Salmon and Steelhead Trout," report no. HW-7-4759, NTA, NV717097, 1.

exclusively on gross effects to growth and mortality. The initial results of Foster's experiments were heartening: although the fish exposed at low dilutions showed varying degrees of mortality, there was nothing present in the affected fish to suggest that fish might become hazardous to humans so long as the radiation levels in the water remained low.[36] Erring on the safe side, Foster sampled water downstream in the Columbia to verify whether the river was sufficiently diluting the reactor effluent to levels which his experiments suggested were not hazardous. By this standard, the concentration of radiation in the water proved harmless.[37]

It was not until 1946 that Foster conducted any field studies on fish exposures in the Columbia itself. Even then, he only thought of the studies as "augmenting the laboratory program," rather than a justified research program in its own right.[38] What he found shocked him. As Foster later recalled, "it turned out that the activity in fish that we got from the river was substantially greater than that which we got from fish in the laboratory, in spite of the fact that the ones in the laboratory were being exposed to much more concentrated pile effluent water than existed in the river!"[39] Moreover, the fish also appeared to have been concentrating specific radionuclides not found in the hatchery populations. Foster was soon able to narrow down the cause of the different exposures levels to a surprising environmental factor not capable of being reproduced in the laboratory: while laboratory fishes were fed commercial food, the Columbia River fish,

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[36] Ibid.
[37] R. F. Foster, "The History of Hanford and Its Contribution of Radionuclides to the Columbia River," in *The Columbia River Estuary and Adjacent Ocean Waters*, ed. A. T. Pruter and D. L. Alverson (Seattle: University of Washington Press, 1972), 8.
[38] Richard Foster, interview by J. Newell Stannard, June 11, 1979, transcript, NTA, NV0702900, 10.
[39] Ibid., 11.

he found, were consuming highly contaminated plankton and sponges in the river.[40] When Foster made similar collections later in the spring of 1947, he discovered that the "Concentration of active materials in tissues or organs ranged to a maximum of several thousand times that of an equal weight of river water."[41] That same year, Hanford health physicist Herbert Parker reported to the AEC that the "Concentration of activity in algae or in colloidal materials with its possible utilization by fish, later used for food, presents a chain of events of great consequence to the public health."[42] Foster had stumbled upon the process on bioaccumulation in aquatic food chains—a chain in which humans stood squarely at the top.

Over the next few years, Foster would continue to study the phenomena by looking at specific radionuclides and their "concentration factors" in the aquatic environment. Despite the implications that bioaccumulation held for radiation in the terrestrial environment (and hence fallout), Foster's work went largely unnoticed within the closed community of classified researchers in the AEC. Likewise, because his work was classified, professional ecologists outside the agency would not learn of Foster's bioaccumulation studies until 1955 (seven years later) when he presented a paper on the subject at the first Atoms for Peace Conference in Geneva, Switzerland.[43] There were

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[40] Ibid.

[41] Herde, K.E., May 14, 1947, "Radioactivity in Various Species of Fish from the Columbia and Yakima Rivers," Report HW-3-5501, NTA NV0906220.

[42] Michelle Stenehjem Gerber, *On the Home Front: The Cold War Legacy of the Hanford Nuclear Site*, 2nd ed. (Lincoln: University of Nebraska Press, 2002), 116. Original quote is in Parker, "Health Physics, Instrumentation and Radiation Protection."

[43] R. F. Foster and J. J. Davis, "The Accumulation of Radioactive Substances in Aquatic Forms, Volume 13 Legal, Administrative, Health and Safety Aspects of Large-Scale Use of Nuclear Energy," in *Proceedings of the International Conference on the Peaceful Uses of Atomic Energy* (New York: United Nations, 1956). See also J. J. Davis and R. F. Foster, "Bioaccumulation of Radioisotopes through Aquatic Food Chains," *Ecology* 39, no. 3 (1958).

also similar studies conducted at Hanford around this same time on radioiodine uptake in terrestrial mammals, the reports of which also appear to have been lost within the bureaucracy or deemed inconsequential.[44] I will discuss this work at more length later in the chapter.

At the time of Foster's discovery, his laboratory had been subsumed under the direction of General Electric, the operating contractor of the Hanford Site. Donaldson and the AFL still maintained close research ties with Foster and continued to conduct active research on questions of the radiological effects of the Hanford reactors on the Columbia. Most of Donaldson's attention around this time, however, was devoted to addressing his concerns and interests in a long-term ecological study of the Pacific test sites, which in 1948 also included Eniwetok Atoll.

When Donaldson approached the AEC about intensifying the AFL's biological studies out in the Pacific, the Commission demurred. Although Donaldson and the lab conducted some smaller-scale surveys and collecting trips at Bikini and Eniwetok atolls during the last years of the decade, by 1950 the AEC had all but cut off support for AFL activities in the Pacific. On June 5, 1951, for example, Paul Pearson, the chief of the AEC's Biology Branch of the Division of Biology and Medicine (DBM) wrote to Donaldson informing him that a survey following the upcoming Greenhouse series would not be warranted and urged him to continue with the laboratory-based studies on campus. Donaldson wrote back the following week expressing his desire to continue field studies and proposed that the AFL initiate a special radiotracer study at the Pacific atolls. "We should like to explore an area—new for us—the use of specific isotopes for tagging foods

[44] Gerber, *On the Home Front: The Cold War Legacy of the Hanford Nuclear Site*, see chapter 4.

and in natural food cycles by aquatic animals. These studies would take two general directions: a. The role of essential food elements such as phosphorus, calcium, iron, etc., in the food cycles of natural waters and the possibility of dilution of radioactive materials by the addition of non-active salts. b. The exploration of the synthesis of vitamin B12 by insects and its subsequent absorption by fishes using Co60 as a tracer."[45] This proposal didn't satisfy Pearson either; that summer, members of the AFL went out instead to the NTS to help with radiological monitoring.[46]

In mid-August, Pearson visited Donaldson in Seattle to further discuss the future work of the lab. During the course of their discussions, Pearson posed seven questions to Donaldson designed to justify the need for continued surveys. Two of the more pointed questions asked "What advantages have field studies over laboratory studies?" and "Is the radiation level sufficiently high to be of interest to the Commission?"[47] Donaldson was so taken aback by Pearson's short-sighted view of the fallout problem that he wrote to AFL-friendly Stafford Warren to complain. "We can answer the questions with statements we think are adequate, but we feel the entire problem is much broader than simply answering the questions presented to us. It involves the entire concept of contamination of a biotic community."[48] Later the next year, Donaldson prepared a report that directly addressed Pearson's questions, especially as pertained to the critical

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[45] Quoted in Hines, *Proving Ground: An Account of the Radiobiological Studies in the Pacific, 1946-1961*, 127-8.
[46] Ibid., 126.
[47] Lauren R. Donaldson to Stafford L. Warren, August 31, 1951, University of Washington Special Collections (hereafter UWSC), Laboratory of Radiation Ecology Records (hereafter LRER), box 3, folder 23. For more on Warren's support for the AFL see his oral history Stafford L. Warren, "An Exceptional Man for Exceptional Challenges," interviewed by Adelaide G. Tusler, 1966-1968, transcript, Oral History Program, University of California Los Angeles, Charles E. Young Research Library, Department of Special Collections, Los Angeles, CA, especially 535 and 981.
[48] Ibid.

need for field studies. "*Laboratory experiments in themselves*," Donaldson wrote, "*cannot substitute for direct observations in the field*. The total ecological situation is of such a complex nature that only comparatively minute segments can be duplicated under controlled laboratory conditions. Which segments deserve priority can and should be determined from results obtained in field studies...It is essential to the understanding of the atomic energy testing program that studies evaluating biotic contamination keep pace with the changes in weapon designs, materials used, and efficiencies obtained."[49] In 1952, Pearson lukewarmly supported an AFL survey as part of the first thermonuclear test (Ivy Mike) scheduled for fall. In reality, the survey was nothing more than a spot-check biomonitoring operation—the AEC simply had no interest in a "total ecological" study.[50]

By 1953, things were beginning to look up for the AFL. That year John Bugher took over the reins of the DBM and began spurring interest in field-based biological and ecological work. In November, he visited the lab for program review and organized a conference with the AFL scientists to air out frustrations about the direction of biological research on fallout effects over the last five years. The transcript notes of the conference reveal Bugher's nascent appreciation of the internal emitter problem:

> Doc [Donaldson]: We have a feeling that much of the failure to organize [ecological studies of fallout] in the Division dates back to the Navy's comments early in the plans that 'there is no problem' [sic].

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[49] Lauren R. Donaldson, October 1952, "The Need for Continuation of Studies of Radiation Contamination of Biotic Forms at the Bikini and Eniwetok Testing Grounds," Applied Fisheries Laboratory report UWFL-28, NTA, NV0050010, 5-6. Emphasis in original.

[50] Pearson attempted to have the APL provide radiological monitoring for Roger Revelle's Scripps Institute of Oceanography oceanographic survey. Donaldson, however, resisted this idea not only because he wanted to conduct more fundamental work, but also because he despised Revelle. The fact that the AEC (and Navy) had more interest in sponsoring physical studies of ocean currents reveals the depth to which they neglected long-term environmental effects of radiation.

> Bugher: I don't think that the particular fault has been that of the Division of Biology and Medicine, but that the staff has been drawn from physics, industrial medicine, etc.
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> Doc: Those problems are, of course, more acute.
>
> Bugher: Not particularly more real, however. There has been that bias. There has not been an understanding of the marine biology field. I wanted to do something about that. We begin more and more to realize that we are dealing with the small end of a large subject...There has been a certain lack of request [for ecological studies] due to lack of comprehension of what needed to be done. We have a situation where the expert knowledge is here in your laboratory."[51]

By the end of the conference it became clear what was motivating Bugher's interest in the environmental aspects of nuclear fallout—strontium-90. Strontium-90 had recently emerged as a particular fallout particle of concern within the DBM owing to its long half-life and chemical similarity to calcium making it uniquely bioavailable to the tissues of plants and animals. The radionuclide was also known to concentrate in the bone if ingested resulting potentially in bone cancers. Yet as Bugher noted to the AFL scientists, "The uptake of strontium depends largely on whats [sic] going on. We don't know too much about it in relation to marine life. We don't know too much about it from the standpoint of land animals either."[52] Strontium-90 was thus an ecological problem; to know the nature of the hazard was to understand the ecological pathways through which it moved through the environment and ultimately deposited in the human body. Unbeknownst to Donaldson at the time, the AEC had already initiated a top-secret strontium-90 study. It was not organized along the ecological lines that Donaldson had

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[51] "Notes on Conference of Applied Fisheries Laboratory Staff Members with Dr. John C. Bugher...," November 1, 1953, UWSC, LRER, box 1, folder 39, 2.
[52] Ibid., 6.

been advocating, but it did represent a subtle turn in the Commission toward the potential health effects of internal emitters.

The Thermonuclear Age: Project Sunshine, Strontium-90, and the H-Bomb

Strontium-90 first emerged as a plausible fallout threat following a top secret meeting of a highly select group of AEC scientists and contracted affiliates at Rand headquarters in the summer of 1953. Led by future Nobel Prize winner and soon to be AEC Commissioner Willard Libby, the conference was an outgrowth of an earlier small-scale AEC study initiated in 1949 codenamed Project Gabriel. Gabriel's objective was to determine the number of bombs required to so saturate the environment with radiation as to make it hazardous to human health. It wasn't a question of testing fallout, but rather nuclear war: how many atomic bombs could the U.S.S.R. and U.S. exchange before the environment reached the point of no return? The project naturally focused on long-lived fallout particles and consequently singled out strontium-90 owing to its long half-life and chemical similarity to calcium, which made it available for incorporation into the biosphere.[53] Based on Project Gabriel, the AEC concluded that roughly 100,000 nominal Trinity-type weapons could be detonated before strontium-90 levels in the environment reached a doomsday scenario.[54]

The primary issue that the Rand conference attendees grappled with was that the advent of thermonuclear weapons shattered the notion of a nominal bomb.

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[53] AEC Division of Biology and Medicine, July 1954, "Report on Project Gabriel," NTA, NV0720894.
[54] Hacker, *Elements of Controversy: The Atomic Energy Commission and Radiation Safety in Nuclear Weapons Testing, 1947-1974,* 182.

"Conventional" Trinity-type atomic bomb tests reached yields of twenty kilotons; thermonuclear weapons such as the first H-bomb test, Ivy Mike in 1952, generated 10.4 *mega*tons of explosive energy—or roughly 500 times more powerful. Such an exponential increase in explosive yield necessarily corresponded to a concomitant rise in radioactive particle production. Thermonuclear weapons testing over the long-term, thus, raised the specter that the world might witness doomsday irrespective of a full-scale nuclear war. The problem, however, was that the severity of the hazard was clouded in uncertainty. For one, the rate of fallout from the atmosphere for the Ivy Mike test was virtually unknown. If stratospheric-reaching radioactive debris remained suspended in the upper atmosphere for perhaps tens of years, the potential effect of strontium-90 in the biosphere would be ameliorated.[55] For another, no one knew what the current levels of strontium-90 in the soil, plants and animals, or humans were. Although the AEC had established a continental fallout sampling network at various Weather Bureau sites across the U.S. in 1951, the resulting samples were reported in terms of total beta activity and were not radiochemically analyzed for specific fission radionuclides like strontium-90. What was needed to better understand the problem, the Rand attendees agreed, was a comprehensive biological monitoring program in plants, animals, and humans.[56] Thus was born Project Sunshine.

Coordinated and headed by Libby, Sunshine was conducted in secret until late 1956 when public concern over fallout compelled the AEC to reveal the nature of the project to assuage fears about nuclear testing. From the AEC's perspective, they had

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[55] See chapter 6.

[56] Arnold Kramish, "Worldwide Effects of Atomic Weapons: Project Sunshine," (The RAND Corporation, 1953), see chapter 5.

good reason to do so; the project's seemingly benign title masked one of its more macabre objectives—obtaining human samples (especially children and stillborns) for analysis of strontium-90 body burdens.[57] Yet human sampling was only one, albeit critical, element in the project. As Libby remarked in a follow-up meeting to the Rand conference a year and a half later, "what we are aiming at [with Project Sunshine] is a complete *worldwide assay* for strontium 90, not only in the biosphere, but in the lithosphere itself. We must have the whole world assay for strontium 90 with the objective of finding out its effect on human life."[58] In short, Libby announced, "We are aiming at an equation which shows that everything checks."[59]

Sunshine, then, was a relatively comprehensive monitoring project that sought to measure strontium-90 levels at every known link in the chain of events that culminated in human exposures. And it did so on a global scale. (Because strontium-90 was approached as a thermonuclear problem, questions of NTS fallout was not necessarily germane to Sunshine). Toward that end, Sunshine consisted of research (primarily laboratory-based) at various links in the environment: Lamont Geological Laboratory at Columbia University was responsible for bone analysis; the AEC's Health and Safety Laboratory for fallout monitoring; Lyle Alexander at the U.S. Department of Agriculture Beltsville Station for soil analysis; and the U.S. Weather Bureau for aerial trajectories, for example. Nevertheless, as Libby's reference to "assaying" suggests, the scientific scope of the project was limited; most research was restricted to biomonitoring, not

[57] Children presented a particular fear for strontium-90 uptake because of the the metabolic need for calcium in growing bones.
[58] January 18, 1955, "Transcript of Biophysics Conference," NARACP, RAEC, box 8, folder Biophysics Conference, 11-12. Emphasis mine.
[59] Ibid., 11.

fundamental field-based research. The primary assumption guiding Sunshine pivoted on the notion that to understand the hazard was to know radiation concentrations at given levels, not necessarily the environment itself. That is, Sunshine scientists did not consider research on the ecological mechanisms in a total environmental system relevant to the strontium-90 problem. As a result, the Sunshine Project signified an important turn towards the environmental connections between lingering radiation and human bodies, but was not approached in the kind of ecological manner that Donaldson had proposed back in the late 1940s. Sunshine scientists, in other words, had identified the problem, but failed to appreciate why any study of strontium-90 effects should be predicated first on understanding the environment. Project Sunshine consequently presented a grossly simplified picture of a complex environmental problem: assaying the environment, not studying it, would ensure that "everything checks."

Although Project Sunshine remained a secret until the spring of 1956, events following the Castle Bravo thermonuclear test in March of 1954 pushed concerns regarding fallout to the forefront of public debate about the risks of nuclear weapons testing. With an explosive power of over 15 megatons, Bravo far exceeded its expected yield. The test also generated enormous amounts of fallout throughout the Pacific Ocean. In addition to servicemen stationed on nearby atolls, a crew of Japanese fishermen and natives on Rongelap Atoll were exposed to lethal amounts of fallout radiation [Figure 21]. The Rongelapese were eventually evacuated from their home atoll, two days after being exposed. The Japanese fishing vessel made its way back to Japan two weeks later, but not without difficulty. All of the crewmembers displayed classic symptoms of acute radiation sickness, and one fisherman died shortly thereafter.

While the incident was widely reported in the press, the AEC remained relatively tight-lipped about the nature and extent of the Bravo fallout. AEC reticence to divulge the particulars of the radiation exposures, however, only served to fuel suspicions that the AEC was not forthcoming about fallout hazards. As a result of Bravo, a space was opened up in public discourse for new scientific voices outside the Commission to weigh in on the critical issue.[60] And, as Americans would soon learn, Bravo held consequences for not only peoples in the vicinity of the test sites, but for the entire world, especially as questions about strontium-90 began to surface.[61] Yet, and perhaps equally as important, the fallout controversy following Bravo also sparked a subtle shift in how the AEC approached the problem of fallout. The kind of ecological thinking that Donaldson and others had been advocating for would gradually take hold within the Commission and reshape how the risks of fallout were perceived and provide a critical source of funding for ecosystems research in ecology.

Radioecology and the AEC

Donaldson, as I've argued above, had been sounding the alarm of environmental radiation from fallout, with respect to human health and as a research project in ecology, as far back as the Bikini Scientific Resurvey in 1947. That call had, by and large, gone unnoticed among professional ecologists. By around 1955, however, ecologists had

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[60] See chapter 7.
[61] Divine, *Blowing on the Wind: The Nuclear Test Ban Debate, 1954-1960*.

begun to perceive in atomic energy development new opportunities to contribute not only to a better understanding of radiation effects on biotic populations and communities, but to practices, theories, and concepts within ecology itself. Led by ecosystems pioneer Eugene Odum and Ohio State University ecologist John N. Wolfe, this movement sought to capitalize on new radiotracer methodologies to delineate the structure and function of ecosystems as well as to secure AEC patronage to solve environmental problems. As Odum argued in 1957, the use of radiotracers in the emerging field of radioecology "can well provide the means of solving the very problems it creates."[62]

For Odum and the other ecologists the radiation problem was twofold. First was the question of human impacts from environmental radiation. Research on food chain dynamics and critical pathway studies by which environmental radiation reached humans were essential for evaluating the risks of fallout and future nuclear power development. A related and under appreciated problem from their perspective was the question of ecosystems-level effects. Throughout the latter half of the 1950s, ecologists working with the AEC lamented the tendency within the Commission to play down larger questions of total environmental or ecological effects. As Oak Ridge ecologist George Woodwell wrote in 1963, "It has generally been assumed that if man could be protected from the harmful effects of ionizing radiation, the other living things around him, particularly plants, would be safe by a wide margin."[63] For the ecologists, this kind of thinking exemplified by Project Sunshine embodied all that was wrong with how the

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[62] Odum, "Ecology and the Atomic Age," 29.

[63] George M. Woodwell, "The Ecological Effects of Radiation," *Scientific American* 208, no. 6 (1963): 40. See also George M. Woodwell, "Bravo Plus 25 Years," in *Environmental Sciences Laboratory Dedication*, ed. S.I. Auerbach and N.T. Millemann (Oak Ridge: Oak Ridge National Laboratory, 1980).

AEC had attacked the hazards of lingering environmental radiation. While the levels of radiation in a particular ecosystem might not pose a direct threat to human health, ecologists argued that any changes to the balance of an ecosystem due to radiation could have indirect and potentially more serious implications for humanity's essential well-being. Moreover, as the quote from Odum above evocatively expresses, radiation provided ecologists with a powerful new tool to unveil ecological mechanisms and knowledge of the environment that could be mobilized to solve environmental problems—problems, Odum implied, that arose not simply from atomic energy development, but from modern progress in general. The problem with the AEC, in other words, was that spot-check monitoring provided only partial answers to the radiation question. Knowledge of radiation effects stemmed from knowledge of the environment. Radioecology held the key to unlocking radiological health effects and nature's secrets, as we will see.

Eugene Odum had been associated with the AEC as far back as 1951 when he submitted a proposal to the Commission to develop a comprehensive study of ecological changes arising from the soon to be constructed plutonium production site at Savannah River, South Carolina.[64] At the time, Odum had been a well-established ecologist at the University of Georgia with a growing interest in the concept of the ecosystem as a unifying principle in ecology. For Odum, the proposed Savannah River Site offered an unparalleled opportunity to study long term ecosystem level effects that would accompany the construction and eventually radioactive releases at the plant. Not

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[64] For biographical information on Odum see Betty Jean Craige, *Eugene Odum: Ecosystem Ecologist and Environmentalist* (Athens: University of Georgia Press, 2001). Her discussion of the origination of the Savannah River ecological project can be found on pages 48-54.

surprisingly, his proposal to the AEC reflected this broad view of the problem. "The total ecological complex is to be studied," he explained, "since no one can predict what problems may arise in the future."[65] Yet, as Donaldson experienced that same year, the DBM's Paul Pearson rejected Odum's proposal. Ecology, Pearson explained to Odum, was not necessarily a Commission concern since it seemed only tangentially related to the projects they typically supported in the fields of biology and medicine. Nevertheless, Pearson did inform Odum that if he or the University had a future biological study "that may come within the scope of interest of the Commission...we would be glad to have an opportunity to consider a modest proposal."[66]

The following month, Odum did just that when he submitted an abstracted version of the initial proposal that centered solely on a study of terrestrial "old-field" ecological succession. Because the the Savannah River Site was to be built on recently abandoned agricultural fields, Odum couched the study in the kind of language that would surely appeal to the reductionist Pearson: "A vast *field laboratory* for the study of the dynamics of succession will thus be provided as a by-product of Savannah River Operations use, with almost controlled conditions."[67] Thus, like Bikini for Donaldson, the Savannah Site offered the ecologist a near perfect place where ecological processes and radiological

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[65] The University of Georgia, Athens, "An Ecological Study of the Savannah River AEC Installation Area," University of Georgia Hargrett Rare Book and Manuscript Library (hereafter UGA-Hargrett), Athens, Georgia, Eugene P. Odum Papers, MS-3257 Box 81 folder 24. Quote is on page 1. The proposal can also be found in Appendix "A" in Eugene P. Odum, "Early University of Georgia Research, 1952-1962," in *The Savannah River and Its Environs*, ed. J.C. Corey (Aiken, South Carolina: E.I. du Pont de Nemours & Company, 1987), 60.
[66] Paul B. Pearson to Eugene P. Odum, April 26, 1951, UGA-Hargrett, Eugene P. Odum Papers, Series III Correspondence, MS-3257. The correspondence section of these papers was uncataloged as of Fall, 2010.
[67] Second proposal entitled "A Proposal for an Ecological Study of Land-Use, Succession, and Indicator Invertebrate and Worm-Blooded Vertebrate Populations of the Savannah River Operations Area" can be found in Appendix "B" in Odum, "Early University of Georgia Research, 1952-1962."

effects could be studied in laboratory-like conditions. The AEC accepted this proposal, but at a much reduced level of financial support—$10,000 as opposed to the initial request of $150,000.

When Odum started the Savannah River project, it is not clear whether he fully appreciated the human health implications of his ecological work any more than the AEC. His interest in the ecological studies at Savannah River stemmed from his interests in long-range ecosystems level processes. Meeting the operational and programmatic needs of the AEC was a tertiary concern. In a briefing Odum delivered to SREL's environmental director Karl Herde, for example, he emphasized the fundamental character of the work. "Since energy is the common denominator for all components of an ecosystem," he told Herde, "we propose to use energy flow through biological food chains as a means of linking plants to animals."[68] At the time, Odum was keen on using radiotracers for that kind of work, but had not felt competent to undertake such studies.

In the course of learning more about radiotracer methodologies, he came across Foster's work at Hanford, which at the time was still classified. Foster's work struck Odum as critical for ecological investigation, especially as they pertained to ecosystem studies, on two levels. First, the notion of bioconcentration clearly impacted human health. As Odum wrote in the second edition of his *Fundamentals of Ecology* textbook in 1957, bioconcentration of radionuclides through food chains revealed that "we could give 'nature' and apparently innocuous amount of radioactivity and have her give it back to us

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[68] Ibid., 51.

in a lethal package!"[69] Yet for Odum, human effects from contaminated food chains were but one aspect of larger ecosystems-level processes. Focusing exclusively on any one element or chain in an ecosystem, in Odum's mind, was reductionist. What ecologists needed to do was to think holistically. That is, they needed to ask themselves what effects radiation might induce on entire ecosystems. Such a focus necessarily demanded that evaluation of radiological effects begin with a thorough knowledge of ecosystem processes. "Before the total effects of radiations can be assayed and practical methods developed for determining the tolerance levels for entire ecosystems," he wrote in 1955, "it is necessary that we find methods for the measurement of total community structure and function."[70] In essence, Odum was arguing that the AEC had approached the problem of fallout and radioactive wastes from the entirely wrong direction: they began in the laboratory, studying radiation effects on mice and such. What they needed to do was begin with the environment.

In 1953, Odum and his brother Howard (also an ecologist) had in fact taken an important first step in measuring the total function of an ecosystem. That year, the AEC built a small field-based laboratory at Eniwetok Atoll and the DBM was encouraging university biologists to utilize the lab for fundamental biological research. In April, Karl Wilbur of the DBM approached Howard and inquired whether he might be interested in using the lab. AEC financial support for biological studies would be limited, Wilbur

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[69] Eugene P. Odum, *Fundamentals of Ecology*, 2nd ed. (Philadelphia: W.B. Saunders Company, 1957), 467.
[70] Eugene P. Odum, "Consideration of the Total Environment in Power Reactor Waste Disposal," in *Proceedings of the First International Conference on the Peaceful Uses of Atomic Energy, Volume 13 Legal, Administrative, Health and Safety Aspects of Large-Scale Use of Nuclear Energy* (New York: United Nations, 1956), 350.

informed Howard, but "Fundamental studies will be encouraged and will, of course, contribute to the program of the AEC in Eniwetok even though they may have no direct connection with the atomic tests program."[71] Howard jumped at the chance and began coordinating with his brother to begin planning an ecological study of the metabolism of Eniwetok's windward reef in the summer of 1954.

The opportunity of conducting an ecological study at Eniwetok intrigued the Odums. Although the reef ecological system at Eniwetok was complex, it was relatively small, self-contained and, critically, a seemingly stable climax community. As Joel Hagen has argued, the Odums viewed the Eniwetok reef as an isolated system or "black box," whose "total inputs and outputs of energy were being measured."[72] In that sense, Eniwetok fascinated the Odums for precisely the same reasons that Donaldson had been initially attracted to Bikini: it constituted a natural field laboratory where the Odums could measure the metabolic processes that contributed to the stability of a relatively complete ecosystem. There were other benefits. Eniwetok had also been the site of atomic testing and still exhibited trace amounts of radioactivity. While the Odums conducted very little radiotracer research, the presence of radiation in the reef enabled "critical assays of the effects of radiation due to fission products *on whole populations and entire ecological systems in the field*."[73] The Odums' Eniwetok reef study, therefore, afforded them a unique opportunity to study not only ecosystems level processes, but also

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[71] Karl M. Wilbur to Howard Odum, April 6, 1953, UGA-Hargrett, Eugene Odum Research Files: Eniwetok Atoll, Old-Field Plant Specimens, Southern Nuclear Task Force, etc. 1950-1970, UGA-06-032, Box 1 folder 10.
[72] Hagen, *An Entangled Bank: The Origins of Ecosystem Ecology*, 101. See also Craige, *Eugene Odum: Ecosystem Ecologist and Environmentalist*, 61.
[73] Odum and Odum, "Trophic Structure and Productivity of a Windward Coral Reef Community on Eniwetok Atoll," 291. Emphasis in original.

provided a kind of baseline assay of total function through which to compare "normal" and" balanced" reef ecosystems against irradiated ones.

The Odums' Eniwetok reef study marked a watershed in ecosystems research. By measuring the energy flow at the ecosystem level, it solidified the gradual shift in the discipline, begun by Lindemann in the early 1940s, away from studies of species or populations to macro-processes that integrated all the biotic and abiotic into a single complex system. Yet, aside from the study's more fundamental contributions, it also signaled a growing interest among ecologists in the ecological effects of environmental radiation. In late 1954, a few months after the brothers returned from Eniwetok, Northwestern University ecologist Orlando Park wrote to Eugene Odum inquiring whether he might be interested in participating in a new Ecological Society of America committee on the "effects of radioactivity on natural populations."[74] Odum agreed enthusiastically and suggested to Park that a "conference of persons working on ecological problems on various AEC experimental areas would be helpful as work in progress in one area seems to be almost unknown in another."[75]

Odum's conference idea appealed to Park. For most of that year, Park had been working as a consultant to Oak Ridge health physicist Karl Z. Morgan, who was interested in developing an ecology program at the lab to investigate the environmental effects of reactor effluent. Radioactive waste, of course, was not a new phenomenon to Oak Ridge. Circumstances as of 1954 had changed, however; as the prospects of

[74] William Hamilton to Eugene Odum, November 24, 1954, UGA-Hargrett, Eugene Odum Correspondence, 1945-1982, 97-044, Box 1 Folder 26.
[75] Eugene Odum to Orlando Park, November 26, 1954, UGA-Hargrett, Eugene Odum Correspondence, 1945-1982, 97-044, Box 1 Folder 26

commercial nuclear power development loomed on the horizon, Morgan understood that radioactive contamination of the environment from nuclear power reactors would pose the primary limiting factor in the technology's development. Park and Morgan consequently began pressing the AEC to support ecological research at Oak Ridge, but ran head long into the kind of environmental blindness that had plagued Donaldson and Odum. As Morgan later recalled, “We told the AEC that it had a responsibility to protect arthropods, bacteria, fungi, trees, and animals exposed to our radioactive wastes. The committee did not take us seriously. Even once the AEC realized how determined we were, one official commented, 'Man is the thing we should be interested in protecting; we should protect him and forget about these microorganisms and other forms of life. After all it would be a good thing if radiation destroyed all these microorganisms.'"[76] When Morgan brought up the issue of bioconcentration of radionuclides in plants and animals, he was equally greeted with skepticism; protecting humans was a matter of limiting exposure to external radiation.[77] As one AEC administrator argued, the "argument that we do not know the consequences of radiation damage to the environment is not a valid argument for support of such a program."[78] For Park, a meeting between professional ecologists and AEC administrators and researchers conducting ecologically-related work might prove fruitful for countering this kind of thinking and establishing some traction for ecology within the Commission.

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[76] Morgan and Peterson, *The Angry Genie: One Man's Walk through the Nuclear Age*, 85.
[77] Morgan discusses bioconcentration in Ibid., 85-6. The anonymous administrator that Morgan referred to responded by pointing out that it took 100,000 roentgens of exposure to kill some microorganisms, a sure sign of the external focus and inattention to the internal dose within the AEC.
[78] Quote in F. Ward Whicker and Vincent Schultz, *Radioecology: Nuclear Energy and the Environment*, 2 vols. (Boca Raton, FL: CRC Press, 1982), 5.

The meeting was eventually held over two days in late spring of the following year at AEC headquarters in Washington. The list of attendees was impressive: included among professional ecologists were Odum, Park and Paul Sears; on the AEC side, Foster from Hanford, Larson from the AEP, and Edward Held and Frank Lowman from the AFL. Donaldson could not make the meeting. The conference consisted mainly of presentations by the AEC ecologists. Odum presented his recent work on Eniwetok; Held and Lowman on the Pacific surveys; and Foster on the Hanford aquatic studies. Much of this was news to DBM officials. Paul Pearson remarked toward the end of the conference that, "We have learned a great deal about what is going on within our own organization with which we were not so thoroughly familiar before."[79] That was a bit of an understatement for Pearson who, as I have shown, held ecology and field work, in general, in low esteem. There were some dramatic moments at the conference too. As future head of the Oak Ridge ecology program, Stanley Auerbach, remembered, the non-AEC ecologists held an executive session of sorts during the conference and came back and informed the AEC that they were displeased with the Commission's lack of support for ecological studies. "We're not happy," Auerbach recalled the ecologists saying, "you've got fallout problems, and you've got other problems, and you've got radiation and nobody knows anything in depth."[80] Although it is not clear whether the DBM had been anticipating this reaction, at the end of the last session Pearson notified the conference group that the AEC had recently opened up a position for an ecologist on the staff and

[79] "Transcripts of Proceedings: Ecology Conference," May 20 & 21, 1955, NTA, NV402771, 234.
[80] Staley Auerbach, interviewed by J. Newell Stannard, April 19, 1979, transcript, NTA, NV0702881, 6. There is no record of an executive session during the conference, but Pearson noted in the transcript of the meetings that members of the DBM and the ESA Committee met to discuss the AEC ecological programs late into the night of the first day's sessions. See Ibid., 239.

hired Ohio State University ecologist John Wolfe to fill it. Wolfe, Pearson explained, would "sort of pull together [the] ends and fill up the gaps that we folks who are pretty much unfamiliar with ecology in some respects may have overlooked."[81] Wolfe would begin in September.

The appointment of Wolfe to the DBM was considered a positive step from the ESA radiation committee's perspective. Wolfe was a well-established ecologist in the discipline and generally regarded as trustworthy with the best interests of the profession in mind. After Pearson made the announcement, Park congratulated the Commission for the position and Wolfe's hiring. Yet Park also outlined further steps that he and the ESA radiation committee felt the AEC ought to take. These included, better and more intensive training of radiation ecologists, the creation of a joint program in ecology and genetics, and an expansion of the ecology program to study the impact of radiation on all major environmental types where nuclear reactors might be situated.[82]

Park's suggestions were at the forefront of Wolfe's mind when he started the position and began visiting the various AEC installations and labs such as Hanford, the AFL, Savannah River, and Oak Ridge for review.[83] In December, Wolfe was invited to report to the Commission's Advisory Committee on Biology and Medicine (ACBM) meeting on current ecological program and needs. His report reflected Park's initial proposition that the Commission extend the geographical scope of their ecology program,

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[81]Ibid., Ecology Conference, 235.
[82] "Transcripts of Proceedings: Ecology Conference," May 20 & 21, 1955, NTA, NV402771,238-40. Park also reported these suggestions to the ESA directly. See W.D. Billings et al., "Report of the Committee on the Effects of Radioactivity on Natural Populations," *Bulletin of the Ecological Society of America* 37, no. 1 (1956).
[83] See, for example, Wolfe to Odum, October 25, 1955, UGA-Hargrett, The Eugene Odum Collection, Correspondence, 97-044, Box 1, folder 30.

but he also heavily emphasized the need for extensive field work and a holistic approach to radiation problems. At the meeting, he proposed that DBM expand the ecology program along six lines of inquiry:

> 1. *The study of whole communities* in essential equilibrium with the environment (primeval areas) as well as domesticated communities in various climatic areas of the U.S. in term of fallout, reactor wastes, experimental techniques with isotopes,
> 2. The support of *fundamental biological surveys* in the field in various climatic regions of the U.S., anticipating the wide-spread establishment of reactors (or future technological developments) in these areas. These areas would require long-time protection.
> 3. The study of high mountain habitats in region tests, aquatic and Terrestrial [sic]; in term of fallout, natural dynamics of biological cycles, plans and animal populations in naturally radioactive areas...
> 4. Exploration of the the possibility of Weather Bureau stations and Agricultural Experiment Farms Cooperatives in carrying out certain phases of the program. *But it is suggested that field studies be major, laboratory investigations supplemental*.
> 5. A major objective could well be the development of an "awareness of responsibilities" among ecologists by utilization of scientific meetings, conferences, and publication of material germane to ecological researchers.
> 6. The training of radio-biologists in the field techniques remains a problem.[84]

Unfortunately, most of Wolfe's suggestions mostly fell on deaf ears; none of his ideas regarding the geographical expansion of ecological research into different environment types were heeded. Still, Wolfe was successful in garnering support for a small-scale ecological program at Oak Ridge, under the direction of Stanley Auerbach.

As I mentioned previously, Karl Morgan had been interested in initiating an ecology program at Oak Ridge, but had faced resistance from AEC administrators who failed to perceive how environmental studies of radiation effects pertained to the

[84] "Minutes of the 53rd Meeting of the Advisory Committee for Biology and Medicine, NTA, NV0709147, 7-8.

Commission's responsibility of protecting human health. In part, Wolfe and Auerbach were able to establish an ecology program at Oak Ridge because administrators there had decided to drain White Oak Lake, a containment pond that the lab had used to as a waste dump since the Manhattan Project days. Owing the heavy saturation of radionuclides in the lakebed and its relative boundedness, Auerbach, Morgan, and Wolfe managed to convince the AEC that the lake would be a perfect natural laboratory setting for the study of the movement and recycling of radiation in the environment.

Auerbach's program began somewhat modestly. As the lake was drained, Auerbach initially focused on plant succession and radioactive assays in soil and plants to create baseline profiles in the newly exposed bed. Soon, however, he had begun direct experimentation in the bed in response to growing questions about the environmental behavior of critical fallout radionuclides. In 1957, he and his colleagues planted corn and beans in the dried lake to measure the uptake and redistribution of strontium-90 and cesium-137. The results of these experiments demonstrated the importance of the ecological processes for concentrating radiation; plants grown in the lake bed, on average, nearly exceeded the maximum permissible level for strontium-90 intake.[85] Yet as two of Auerbach's colleagues D.A. Crossley and Henry Howden noted in one of their publications arising out of the investigations, the White Oak Lake experiments also resulted in fundamental ecological knowledge. "The resulting ecosystem on White Oak Lake," they wrote, "may be envisioned as a gigantic tracer experiment, which can yield

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[85] S.I. Auerbach and D.A. Crossley Jr, "Strontium-90 and Cesium-137 Uptake by Vegetation under Natural Conditions," in *Proceedings of the Second United Nations International Conference on the Peaceful Uses of Atomic Energy, Volume 18: Waste Treatment and Environmental Aspects of Atomic Energy* (Geneva: United Nations, 1958), 498.

information of interest to both ecologists and health physicists."[86] As a mixture of the lab and field, the White Oak Lake studies appealed to the reductionist tendencies in health physics while simultaneously meeting the larger environmental interests of the ecologists. As a result, it signaled an alliance of sorts between health physicists and ecologists, which in the coming years would become solidified under the umbrella of radioecology.

In the meantime, Eugene Odum had also been actively promoting the need for radioecology and ecological views about environmental radioactivity. In 1955, both he and Foster attended the first international Atoms for Peace conference in Geneva. For Foster, the conference marked the first opportunity that he had to describe his research on the bioaccumulation of radiation in aquatic organisms to a public audience. Odum too took advantage of the unique setting to express the need for a total ecosystems-level view of environmental radiation problems.[87] The following year, Odum sought to capitalize on the Atoms for Peace conference by organizing a special radioecology symposium at the annual American Institute of Biological Sciences meeting in Storrs, Connecticut. Shortly after the conference, he had also been granted fellowship from the National Science Foundation for the 1957-1958 academic year to visit, among other places, Hanford and the NTS to learn more about radiotracers, which he had hoped would form the basis for a chapter on radioecology in a revised edition of his *Fundamentals of Ecology* textbook.

All of this was occurring at a time when the AEC was facing increased scrutiny about the health effects of fallout. In the wake of the Bravo incident back in 1954, scientists outside the Commission like Ralph Lapp and Linus Pauling had begun to

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[86] Quoted in Angela Creager, *Life Atomic: Radioisotopes in Science and Medicine* (Chicago: University of Chicago Press, forthcoming).
[87] Odum, "Consideration of the Total Environment in Power Reactor Waste Disposal."

express publically their frustration regarding the AEC's lack of candor about the health risks of fallout, especially strontium-90.[88] The question of fallout, in fact, had become such a hot button issue in the middle years of the decade that it formed a central tenet in Adlai Stevenson's presidential platform to unseat Eisenhower in the 1956 election. Stevenson would lose, but the growing uncertainty of fallout effects remained. Accordingly, in the spring of 1957 Congress decided to hold a series of special hearings on fallout to, as Robert Divine has written, "try to compel the scientists to come forth with a satisfactory explanation of the radiation problem."[89]

The hearings, which were held over four days in early June, marked an important milestone in the public's awareness of Project Sunshine and the global contamination of food chains from strontium-90. In all, more than forty witnesses appeared, mostly scientists from the AEC, but many from the academic world as well. Although neither Donaldson, Odum, or the other ecologists I have mentioned testified, Congress and the public heard plenty about the uptake of strontium-90 in critical food supplies like wheat and milk. In his testimony on Project Sunshine, Willard Libby maintained that current and future levels of strontium in food would not pose a hazard to humans. Yet, as even Libby admitted, there were profound uncertainties; variability in the uptake of strontium-90 in different plants and animals, in soil types, and deposition in bone, for example, complicated efforts to ascertain exactly how environmental radioactivity affected human health. Although Libby did not acknowledge it, these environmental complexities problematized his conviction that an "equation that shows that everything checks," could

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[88] Divine, *Blowing on the Wind: The Nuclear Test Ban Debate, 1954-1960*.
[89] Ibid., 129.

be found. Indeed, following the hearings, the Joint Committee on Atomic Energy began pressuring the the Commission to do more in the environmental field to reassure the public that strontium-90 levels were safe. As New Mexico Congressman Chet Holified remarked in a speech after the hearings, "We are now dealing with a global health problem."[90]

The hearings also impressed Odum. In a letter to John Wolfe, he commented, "With all the fuss about fallout it looks as if ecological work should be better appreciated!"[91] It would; later that summer, the AEC created a special Environmental Studies Branch within the DBM to deal specifically with the oceanographic, meteorological, and ecological aspects of radiation. John Wolfe would lead the new division.

The Ecological Crusade I: Project Sunshine, Rongelap Atoll, and the AFL

Wolfe had been out of the Commission for a year when he agreed to accept the position as director of the Environmental Branch. Missing academic life and frustrated at the AEC's lack of support for ecology, he had returned to Ohio State in 1957. When he again returned to the AEC, as his obituarist noted, Wolfe initiated a "crusade for ecology" within the Commission.[92] His first strike at the infidels began with Project Sunshine.

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[90] Ibid., 138.
[91] Eguene P. Odum to John Wolfe, June 7, 1957, UGA-Hargrett, Eugene Odum Correspondence, 1945-1982, 97-044.
[92] George Sprugel Jr, "John N. Wolfe, 1910-1974," *Bulletin of the Ecological Society of America* 56, no. 3 (1975): 21.

Following the 1957 Congressional hearings, the DBM began to reconsider the aims and methods of Sunshine. Was the project geared only toward understanding the strontium-90 hazard? Did it deal solely with the global aspects of fallout? Was it simply a monitoring effort or a fundamental research program? And, critically, did Sunshine deal with radiation effects on whole biological systems, or just a single chain from the environment to humans?[93] When Wolfe was briefed on the history and current status of Sunshine in March of 1958, he was shocked to learn that some in the DBM still considered Sunshine as simply a monitoring effort. In this manner of thinking, the main objective to improve the project centered on more thorough sampling and collection of data. Wolfe, however, pressed an ecological perspective to not only Sunshine, but fallout studies in general. Later that month, Wolfe wrote to Environmental Branch scientist Hal Hollister who was currently drafting a white paper on the Sunshine problems for the DBM. In the memo, Wolfe lamented that, from a biological point of view, the monitoring aspects of Sunshine left much to be desired and requested that Hollister "should [keep] this in mind as you develop your staff paper."[94] In his white paper, however, Hollister played down the need for fundamental biological and ecological field work. "Good research," he wrote, "implies pursuit of understanding, i.e. mechanisms. An estimate of hazard, however, *is more engineering in approach* than science." Therefore, Hollister concluded, "One of the pressing questions facing Sunshine is to ask

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[93] Hal Hollister to C.L Dunham et al., January 22, 1958, "Discussion of 'Project Sunshine,' NARACP, RAEC, box 10, folder Administrative Files-Fallout Program Policy. See also, Hal Hollister to Dunham et al., February 27, 1958, "Project Sunshine Scientific Objectives," NTA, NV0070866; Hal Hollister, April 7, 1958, "Project Sunshine: Its Aims and Operation: A Report to the Director, Division of Biology and Medicine," NARACP, RAEC, box 10, folder Administrative Files-Fallout Program Policy.
[94] John Wolfe to Hal Hollister, March 27, 1958, "Sunshine Program," NTA, NV0026680.

to what extent *we will be forced* to measure the environment to satisfy the public *even though sound engineering estimates of hazard* could be made without so much activity of that sort."[95] Put another way, the problem of fallout remained a factory problem; engineering rendered the environment immaterial.

There the matter rested for nearly a year. In the spring of 1959, however, as a new round of Congressional hearings on fallout were being organized, the debate over Sunshine was renewed. On the 21st, Wolfe wrote a biting memo to Hollister demanding answers as to why ecological considerations were not being addressed in Sunshine. "I recognize [fallout] as an ecological problem," he wrote, "and while I am aware that it has never been attacked ecologically, there are questions directed to this Branch which require answers." Specifically, Wolfe appended fifteen questions to his memo, the answers to which he intended to forward to the radioecology committee of the ESA currently being chaired by Odum. In the main, Wolfe's questions centered on resolving why Sunshine had not supported field studies or focused specifically on the redistribution and concentration of radionuclides in ecosystems.[96] Although Hollister drafted a riposte of sorts to Charles Dunham, director of the DBM, he opted not to send it.[97]

The questions Wolfe raised, however, would not go away. During the Congressional hearings later that spring, the environmental aspects of fallout contamination again dominated the discussion and, once more, the AEC found itself

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[95] Hal Hollister, April 7, 1958, "Project Sunshine: Its Aims and Operation: A Report to the Director, Division of Biology and Medicine," NARACP, RAEC, box 10, folder Administrative Files-Fallout Program Policy. Emphasis mine.
[96] John N. Wolfe to Hal Hollister, April 21, 1959, "Questions on Atomic Fallout Resulting from Nuclear Detonations and their Biological Implications," NARACP, RAEC, box 10, folder Administrative files-Fallout Program Policy.
[97] Hal Hollister to C.L. Dunham, April 24, 1959, "Memo from John Wolfe Dated April 21, 1959," NARACP, RAEC, box 10, folder Administrative files-Fallout Program Policy.

under severe criticism that it was not doing enough to understand this aspect of the problem. In response, the AEC formed a special Fallout Studies Branch in late 1959. The creation of the Branch effectively ended the debate about Sunshine; the new branch would combine both the monitoring aspect of Sunshine and expand research into fallout effects in the total environment, including the atmosphere, oceans, and terrestrial ecosystems.[98] This environmental focus was reflected in an early conference sponsored by the Branch in 1961, which contained a number of papers on ecological distribution and recycling of fission products in ecosystems.[99] The formation of the Branch also resulted in a split between operational and pure aspects of ecological research within the Commission; the Environmental Sciences Branch under Wolfe would dedicate most of its efforts to the fundamental ecological studies while the Fallout Studies Branch would deal directly with fallout matters.

Another salvo in Wolfe's ecological crusade targeted the Pacific. As I mentioned above, the Bravo shot in 1954 had exposed the natives living on Rongelap Atoll to lethal levels of fallout radiation. While the AEC and DoD had belatedly evacuated Rogelap, the question remained whether or not the natives could be safely repatriated back to the atoll in the future. For the AEC, that question hinged largely on the levels of strontium-90 in the atoll's ecosystem.[100] Accordingly, the Commission directed the Applied Fisheries Laboratory to conduct a series of small-scale collection visits to Rongelap to

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[98] C.L. Dunham to A.R. Luedecke, July 7, 1959, "Proposal to Strengthen AEC's Radioactive Fallout Studies Program," NTA, NV0025208.
[99] "Radioactive Fallout from Nuclear Weapons Tests: Proceedings of a Conference," ed. Alfred R. Klement (Washington D.C.: Atomic Energy Commission, Fallout Studies Branch, November 15-17, 1961). This can also be found in NTA, NV0065133.
[100] "Conference on Long Term Surveys and Studies of Marshall Islands," NTA, NV0410246.

measure radiation levels throughout 1955 and 1956.[101] While the AFL's surveys were not comprehensive by any means, the analysis of the samples of soil, as well as plants and animals commonly used as food, suggested that the atoll was seriously irradiated. Crabs, in particular, had bioaccumulated levels strontium-90 that exceeded the maximum permissible concentration standard.[102] There were some surprises too. As Donaldson emphasized in his report on the surveys to the AEC, "<u>It appears likely then that although maximum fallout occurred at the north end of the atoll, the radioactive material is being redistributed throughout the atoll</u>."[103] With these findings in mind, Donaldson in early 1957 began to advocate, as he had done with Bikini, that the AEC initiate a long-term ecological study of Rongelap to determine whether the atoll was safe for human habitation.

In January of 1957, Donaldson prepared an outline of prospective ecological studies for John Wolfe who was scheduled to visit the AFL on the 19th and 20th. The proposal listed two possible avenues of study. The first was simply a continuation of spot-check surveys for the purposes of biological monitoring. The second was a detailed ecological research program "to better understand the many intricate problems of

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[101] Lauren Donaldson, "A Radiological Study of Rongelap Atoll, Marshall Islands, During 1954-1955," Report number UWFL-42, UWSC, LRER, box 9.
[102] Gordon M. Dunning, *Radioactive Contamination of Certain Areas in the Pacific Ocean from Nuclear Tests; a Summary of the Data from the Radiological Surveys and Medical Examinations* (Washington D.C.: U. S. Atomic Energy Commission, 1957), 49-50. See also Allyn H. Seymour to Dr. Lauren R. Donaldson, December 4, 1957, UWSC, LREC, Box 1, folder 43.
[103] Applied Fisheries Laboratory, December 30, 1955, "Radiobiological Resurvey of Rongelap and Ailinginae Atolls Marshall Islands, October-November 1955," UWSC, LRER, box 9. Emphasis in original.

radiation in the natural environment."[104] Donaldson, in particular, was interested in studying the entire radiological cocktail present in the Rongelap's ecosystem, not just Strontium-90. "Since the dissemination of similar mixtures of radioisotopes is more likely to be repeated than the contamination of a particular ecosystem," Donaldson explained, "information of more general value might be obtained by giving priority to careful study of several radionuclides in a limited number of ecological situations rather than trying to cover numerous situations and limiting studies to strontium 90."[105] Moreover, he argued, emphasis should be placed "primarily with the processes involved in the movement of radionuclides."[106] The implications of Donaldson's proposal were clear: spot-checks and basic radiological monitoring were not sufficient to determine whether it was safe to repatriate the Rongelapese.

Yet, during his discussions with Wolfe, a decision had been made at the highest levels of the AEC in February to return the natives home, based largely on the AFL's surveys conducted the previous two years. Although the AEC acknowledged the problem of strontium-90 levels in land crabs, they argued that certain restrictions to the Rongelapese diet would reduce their internal exposure to radionuclides to below the maximum permissible concentration levels.[107] That summer, the Rongelapese boarded a navy vessel and returned. The AEC, it was decided, would continually monitor the

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[104] Lauren Donaldson, January 19 and 20, 1957, "Suggested Study Areas for Rongelap Atoll for Discussion with Dr. John Wolfe, United States Atomic Energy Commission, Division of Biology and Medicine," UWSC, LRER, Box 2, folder 1.
[105] Ibid., 14-5.
[106] Ibid., 15.
[107] Dunning, *Radioactive Contamination of Certain Areas in the Pacific Ocean from Nuclear Tests; a Summary of the Data from the Radiological Surveys and Medical Examinations*, 50. See also W.B. McCool, February 6, 1957, "Note by the Secretary, Return of the Rongelapese to Their Home Island," NTA, NV0726362. This report was also written by Dunning. To be fair, before writing these reports Dunning had advocated for further survey work before repatriation.

health of Rongalapese, but ecological investigations of the irradiated Rongelap environment were put on hold.[108]

Wolfe, however, continued to press the AEC on the need for an ecological study to compliment the medical surveys. In March, he and Allyn Seymour, an AFL marine biologist currently on loan to the DBM, submitted a report to the Advisory Committee on Biology and Medicine on plans for conducting a long-term ecological study at Rongelap.[109] Nevertheless, as of that summer, the study remained grounded.[110] By September, Wolfe had managed to round up roughly $80,000 from the DBM and directed Donaldson to write up detailed revised proposal for Rongelap, which he completed in October. The proposal was broad and reflected Donaldson's long-held conviction that the evaluation of radiological hazards demanded a total long-term approach to the problem of fallout. It also marked the long association between the AFL and the study of the health effects of Bravo fallout on the Rongalapese.

The Rongelap repatriation and the ecological and medical studies periodically conducted there have not been without controversy. One of the major reasons the AEC had for supporting the long-term study was that the atoll harkened back to Donaldson's long interest in studying atolls for their laboratory-like qualities. As Donaldson remarked in a working draft of his ideas for the Rongelap ecological study intended for John Wolfe, "In spite of the hundreds of man-days spent in the field and laboratories, much remains to be done, for it is very evident that Rongelap is an excellent laboratory for radiation

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[108] Ibid, McCool, February 6, 1957. Robert Conard of Brookhaven National Laboratory, who had directed the medical surveys since the Bravo incident, was to continue the medical surveillance program.
[109] Minutes of the 61st Meeting of the A.E.C. Advisory Committee on Biology and Medicine, March 14-16, 1957, NTA, NV0712127.
[110] Allyn H. Seymour to L.R. Donaldson, June 20, 1957, UWSC, LRER, Box 2, folder 2.

biology."[111] There was a major difference between Donaldson's plans for the Bikini laboratory and Rongelap; Rongelap was home to hundreds of native Rongelapese, all of whom had been seriously exposed to fallout radiation. Not surprisingly, with all of the studies that have been conducted on their atoll and on their bodies, many of the Rongelapese who inhabit the atoll today consider themselves as unwitting experimental subjects.[112] Although some Rongelapese have charged that the AEC deliberately rained fallout down on their homeland in order to study biological effects, the fact that the AEC sought to capitalize on their plight in order to investigate long-term effects is beyond doubt. DBM director Charles Dunham, who approved the studies, said as much to Acting High Commissioner of the Trust Territory of the Pacific Islands, Delmas Nucker in a 1959 letter. "A very unusual opportunity exists at Rongelap to study ecological relationships in a relatively undisturbed area which has been contaminated only once with radioactive fallout. The radioactive materials provide tracers for study of the movement of minerals in the environment and in organisms on a scale that would be impossible under any practical experimental conditions. The measurement of possible environmental imbalance caused by the fallout radioisotopes will contribute to estimates of long term hazards to human beings and to an evaluation of the recovery period following single nuclear detonations."[113] Donaldson himself was not unaware of the tricky ethical issues involved in the study, but saw the shift in the AEC priorities toward

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[111] Applied Fisheries Laboratory, January 19-20, 1957, Working Copy of "Suggested Study Areas for Rongelap Atoll For Discussion with Dr. John Wolfe," UWSC, LRER, box 2, folder 1, 2.
[112] Holly M. Barker, *Bravo for the Marshallese: Regaining Control in a Post-Nuclear, Post-Colonial World* (Belmont, CA: Wadsworth/Thomson Learning, 2004); Barbara Rose Johnston and Holly M. Barker, *Consequential Damages of Nuclear War: The Rongelap Report* (Walnut Creek, CA: Left Coast Press, 2008).
[113] Charles Dunham to Delmas Nucker, June 26, 1959, UWSC, LRER, box 3, folder 19.

ecology as profoundly important. As he told J. Newell Stannard in a 1970s interview, "Well, unfortunately, the Rongelap experience is an extremely valuable laboratory, field laboratory, involving all aspects of living things. I think to ignore that or even to play down that, as bitter as that experience was, is to miss the point of what the subject's all about. We hate to drag those people [AEC officials] back in and point to their (mistakes), but it's [sic] (the residual activity) there!"[114] What the subject was all about for Donaldson was the biological effect of environmental radiation—a field of study that the AEC had belatedly supported only after a disaster and one that should have been investigated ten years earlier when he had first proposed it.

The results of the Rongelap studies in the early years were just about all that Donaldson had hoped they would be. Lab parties that went to Rongelap between 1957 and 1961 had sufficient resources and time to trace out particular fission radionuclides of interest. The results of these studies showed that although none of the levels of radiation detected in the local flora and fauna at the time appeared to exceed permissible levels for consumption, there were interesting factors redistributing radiation at work. Terns, in particular, appeared to have been accreting radionuclides from contaminated food sources at sea and contributing to Rongelap's radiation burden through their droppings.[115] Comprehensive diagrams were produced to illustrate these ecological relationships. In 1961, for example, Edward Held, the leader of the project, produced a "circuit" diagram of the radiation cycling within the Rongelap ecosystem of the kind that Howard Odum (Eugene's brother) had pioneered in his radioecological study of Silver Springs, Florida.

[114] Lauren Donaldson, interviewed by J. Newell Stannard, May 31, 1979, transcript, NTA, NV0026916, 13.
[115] Hines, *Proving Ground: An Account of the Radiobiological Studies in the Pacific, 1946-1961*, 267-8.

Yet it was precisely this kind of technocratic ecology that ultimately failed Donaldson. Much of Donaldson's interest in ecosystems ecology and radioecology, as Mathew Klingle has shown, stemmed from his desire to rationalize the environment in order to better manage and improve fisheries stocks.[116] The same year that Donaldson was approved to begin the Rongelap study, the AEC also supported his Fern Lake project. Through radioecology, Donaldson hoped to turn Fern Lake, a relatively barren lake in Washington State, into a fertile spawning ground for depleted salmon stocks. He failed, despite years of study and work. Donaldson's systems method of disassembling nature into component parts (as graphically depicted in the circuit diagram) in order to arrange them to manage salmon runs failed to account for the inherent complexities of the environment. His ecological studies, for all they did to make the connections between the environment and human health legible, were steeped in the kind of technocratic managerial ethos that defined the AEC's attitude toward nature. He was not alone among his fellow AEC ecologists as I'll discuss in the conclusion to this chapter.

The Ecological Crusade II: the NTS, Harold Knapp, and Reconstructing the Ecology of Iodine-131

The AEC did not fully appreciate the radioiodine hazard until after the 1959 Joint Committee on Atomic Energy hearings. Until then, the Commission's focus on the internal emitter problem centered primarily on the strontium-90 question, even as the radiation safety practices at the NTS remained wedded to gamma radiation measurements as the indicator for total exposure. The lack of attention to radioiodine did not stem from

[116] Matthew W. Klingle, "Plying Atomic Waters: Lauren Donaldson and the 'Fern Lake Concept' of Fisheries Management," *Journal of the History of Biology* 31, no. 1 (1998).

the AEC's ignorance of the radionuclide's biological effect in the body. The tendency of iodine-131 to bioaccumulate in thyroid glands had been well established at least since the metabolic studies at Berkeley in the 1930s.[117] But radioiodine was a short-lived fission nuclide (8 day half-life) and, as the focus on strontium-90 suggests, the AEC concerned itself with the accumulation of long-lived fission products in the environment. The short half-life of radioiodine, therefore, coupled with the Commission's predilection for monitoring external exposures effectively obscured the hazard. AEC and NTS officials simply could not conceive how a short-lived fission product like radioiodine could become an internal danger.

Nonetheless, there were indications in a few early studies supported by the AEC that radioiodine in fallout was a genuine hazard.[118] Studies at Hanford pointed to the potential seriousness of radioiodine exposure, including data gathered as a result of the infamous Green Run experiment in which operators at the site deliberately released radioactive gases from an insufficiently "cooled" irradiated uranium slug (hence they were "green").[119] In fact, by the early 1950s, Hanford health physics understood the basic ecological behavior of radioiodine: radioiodine deposits on foliage (it does not become incorporated in the plant itself, but settles on it), is eaten by animals (for example dairy cows), and is subsequently bioconcentrated in milk.[120] The Hanford radioiodine studies did not apparently apply to a weapons testing scenario, however, especially concerning offsite exposures. As a result, during the atmospheric weapons testing era at the NTS the

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[117] See chapter 2.
[118] The AEP, as I have noted, was one of those. For a discussion of others, see Merril Eisenbud, *Environmental Radioactivity*, 2d ed. (New York: Academic Press, 1973), 385-9.
[119] Gerber, *On the Home Front: The Cold War Legacy of the Hanford Nuclear Site*, 90-8.
[120] Ibid., 73-4.

AEC did not monitor for radioiodine in milk anywhere in the United States. Only at the start of the testing moratorium in 1958 did the Public Health Service establish a comprehensive milk monitoring program as part of its regular monitoring duties at the NTS.[121]

There was one notable exception to the lack of milk monitoring data. A group of Public Health Service scientists measured fission product concentrations (including radioiodine) in milk from five milksheds across the nation (Sacramento, Salt Lake City, St. Louis, Cincinnati, and New York City) from May 1957 to April 1958. Although the results of the study, which were published in 1959, did not apparently generate much interest, E.B. Lewis, a Caltech geneticist with a long association with the debate over radiation hazards, used their data to make a calculation of the radiation dose to the thyroid of those who drank an average amount of the contaminated milk. From his calculations, Lewis figured that an adult on average would receive roughly twice the amount of radiation to his thyroid from milk than from natural sources. The real shock, however, came from his estimates of exposure to children, who naturally consumed more milk than adults. Children in these milksheds, he calculated, likely received fifteen to twenty times the dose to their thyroids than adults did.[122] Lewis published his findings in June of 1959, shortly after he testified before Congress on the subject during the latest round of fallout hearings.

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[121] Hacker, *Elements of Controversy: The Atomic Energy Commission and Radiation Safety in Nuclear Weapons Testing, 1947-1974*, 198, 210.
[122] U.S. Joint Congressional Committee on Atomic Energy, *Fallout from Nuclear Weapons Tests*, 86th Congress, 1st session, 1959; E. B. Lewis, "Thyroid Radiation Doses from Fallout," *Proceedings of the National Academy of Sciences of the United States of America* 45, no. 6 (1959): 1552-4.

Based in part on his testimony and research, the Joint Committee on Atomic Energy requested that the AEC produce a report on the current levels and potential effects of short-lived fission products. Although it took over a year to produce the report, the results confirmed the hazard.[123] Harold Knapp, a little known mathematician in the Fallout Studies Branch, wrote the report. From this point on, radioiodine replaced strontium-90 as the chief internal radiation hazard. Yet because the report was based on a paucity of observed milk data, and the fact that it was issued during the moratorium, the radioiodine problem was considered in the abstract. That would soon change when the Soviets resumed testing in August of 1961, followed shortly by the Americans a few months later.

The new round of tests from 1961 to 1963 produced more than twice the fallout than had been injected into the atmosphere prior to the moratorium.[124] Not surprisingly, the PHS milk monitoring network immediately began to note the rise of radioiodine in milk. This came as somewhat of a shock considering that the Soviet tests were conducted on the other side of the globe and the NTS tests were conducted partially underground. Detection of high levels of radioiodine in milk samples from Salt Lake City following the Sedan shot in 1962, for example, led Utah's public health director to urge milk producers to use dry feed for their animals in hopes of reducing the level of the radioactive

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[123] Harold A. Knapp, June 6, 1960, "The Contribution of Short Lived Isotopes and Hot Spots to Radiation Exposure in the United States from Nuclear Test Fallout, NTA, NV00019168.
[124] Congress, *Fallout, Radiation Standards, and Countermeasures,* 10.

substance in milk.[125] Similar high levels were detected across the nation, renewing and intensifying the fallout controversy as Americans learned more about the new hazard.[126]

The radioiodine controversy proved to be the straw that broke the AEC's back. With little faith in the AEC or their Soviet and British counterparts to protect public health, most in the world had had enough of fallout. In 1963, under tremendous pressure to end fallout, the three nations agreed to the Limited Test Ban Treaty. Although the LTBT did not end the arms race or nuclear testing, by ending above ground testing it effectively ended the fallout controversy.

As the final touches were being put on the treaty, however, a new controversy erupted within the corridors of the AEC. With radioiodine emerging as the principal controlling factor in the contamination of the environment, the question remained as to the extent of the iodine exposure to Americans before the 1958 moratorium. It was a tricky problem. Because milk had not been monitored for radioiodine until 1958, any attempt to reconstruct the internal radioiodine dose to nearby downwind populations required extrapolation from the sole source of information collected in the past: gamma radiation. In 1962, the author of the previous radioiodine report, Harold Knapp, attempted to do just that. What he found was disheartening indeed and pointed to the vast underestimation of the fallout problem that had occurred when the AEC failed to consider the environmental mechanisms that contributed to overall exposure of Americans to fallout.

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[125] Hacker, *Elements of Controversy: The Atomic Energy Commission and Radiation Safety in Nuclear Weapons Testing, 1947-1974*, 221.
[126] Ibid; "Radioactive Content of Milk Found Sharply Higher in Utah," *The New York Times*, August 2 1962.

In order to reconstruct the internal dose from radioiodine, Knapp correlated the external gamma exposures from the 1962 tests with the measured levels of radioiodine in milk and applied the ratio to the external radiation figures compiled in the early 1950s.[127] His determination of the ratio between external gamma exposure and internal exposure were startling: "Comparison of the thyroid dose due to internal I-131 [radioiodine] from a single incident of fresh fallout…with the calculated, lifetime whole body dose due to external gamma radiation from the same fallout, leads to the conclusion that for a 1 year old child, the *internal* thyroid dose can lie in a range *60 to 240 times that of the external thyroid dose*."[128] Moreover, he continued, "These calculations also indicate that [high] milk levels…could occur when external gamma levels due to fallout are too low to distinguish from natural background by routine field survey methods."[129] Knapp's results suggested that despite the AEC's contention that the public never experienced undue harm from nuclear testing because external gamma levels were within permissible guidelines was fatally flawed. According to Knapp, external gamma levels and internal levels did not bear even a one-to-one correlation. Thus, when Knapp applied his ratio to the external gamma measurements taken following the infamous Harry shot in 1953, he concluded that the "doses to the thyroid of an infant which drank 1 liter of this milk per day for three weeks following the test would lie in the range of 120 to 440 rads."[130]

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[127] H.A. Knapp, "Iodine-131 in Fresh Milk and Human Thyroids Following a Single Deposition of Nuclear Test Fallout," Fallout Studies Branch, Division of Biology and Medicine, AEC, June 1, 1963, NV0001758, 2.

[128] Ibid., 35. Italics mine.

[129] Ibid.

[130] Harold A. Knapp to Mr. Conway, September 9, 1963, NV0004909. The "rad" is an expression of the absorbed dose. Roentgens simply measure exposure from ionized air, not dose. Nevertheless, the two measurements are essentially equivalent. See table 1 for the possible biological injury from these doses.

These levels far exceeded the AEC's established maximum permissible limit of 3.9r/quarter.

Knapp's report proved highly controversial for it expressly called into question the putative safety of the public and the safety practices of the agency during the era of atmospheric nuclear testing in the 1950s. Gordon Dunning, a high ranking scientist within the Commission, sharply criticized Knapp's findings. Yet what appeared to concern Dunning most was the potential public reaction:

> In response to a question asked as to the motive for publishing the paper, a member (not the author) of the Commission's staff said, 'The Commission has been telling the world for years that it has been conducting its operations safely, now it appears that this may not be so'. If a member of the staff says this about the paper [i.e. Knapp's], what reaction may we expect from the press and the public?[131]

Knapp acidly responded to this in a later memo, "I expect somebody (not the author) might want to hang Gordon Dunning from a sour apple tree."[132] For Knapp, the issue was all too clear: "It looks mightily silly to suddenly become so doubtful just because when the theory is applied, we suddenly realize we have all been missing the obvious in a haystack for over 10 years…."[133]

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131 Harold A. Knapp to Dr. Dunham, June 27, 1963, NV0004884.
132 Ibid.
133 Ibid.

Conclusion: The Ecosphere, Technocracy, and Popular Ecology

Knapp eventually published a version of his report in the journal *Nature* in 1964.[134] Since then, numerous studies have been conducted to reconstruct the "hidden doses" that Knapp first brought to light in the early 1960s.[135] Owing to the many uncertainties inherent in estimating exposures to radioiodine after the fact, the conclusions reached by these studies have likewise been marked by uncertainty. In 1997, for example, the effort to reconstruct the radioiodine dose to Americans during the atmospheric testing period culminated in a National Cancer Institute report entitled "Study Estimating Thyroid Doses of I-131 Received by Americans from Nevada Atmospheric Nuclear Bomb Tests."[136] In addition to reconstructing the probable dose to persons living in every county in the United States, the report also made an attempt to evaluate one's likelihood of one contracting thyroid cancer. Included on the website is a dose reconstruction calculator that anyone can use to estimate their radioiodine dose. Punching in some figures into the calculator demonstrates the uncertainties inherent in dose reconstruction efforts. A male born July 4, 1950 in Gallatin County, Montana who drank one to three glasses of milk a day, for example, received anywhere from 10 to 190 rads of radiation to their thyroid, with a hypothetical average of 30 rads. At the average dose, the NCI estimates that this person, as of August 23, 2012, would have a 3 in 1,000 chance of developing thyroid cancer.

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[134] H.A. Knapp, "Iodine-131 in Fresh Milk and Human Thyroids Following a Single Deposition of Nuclear Test Fall-Out," *Nature* 202 (1964). See also S. Kirsch, "Harold Knapp and the Geography of Normal Controversy: Radioiodine in the Historical Environment," *Osiris* 19 (2004).

[135] The term "hidden dose" is apparently John Gofman's. See Stannard, *Radioactivity and Health: A History*, 1015-6.

[136] The entire report can be found online at http://www.cancer.gov/cancertopics/causes/i131/nci-reports.

Despite the profound uncertainties of these dose reconstruction studies, one thing has remained clear since the Knapp report: the external dose measurements taken during the atmospheric nuclear weapons testing period was not a reliable indicator of total exposure. By ignoring the complex ways that the environment mediated human exposure to fallout, the AEC dramatically underestimated the risks to nuclear fallout.

Since the beginning of the atmospheric nuclear weapons testing period the AEC had largely conceived of the health effects of fallout radiation as a factory problem. As such, efforts at understanding fallout were best approached in the laboratory where radiation effects could be studying in isolation and without the intrusion of the environment. This approach to the problem blinded AEC administrators to the complex ways that the environment mediated the internal ingestion of long-lived radionuclides; direct exposure from external gamma radiation appeared to be the controlling factor in human health, not the environmentally-mediated internal exposures. Yet, even as concern grew within the Commission about the contamination of strontium-90 in food chains, the Commission still regarded field-based and holistically-oriented study of the environment as, at best, a related, but not fundamental, aspect of human health. This was perhaps best exemplified by Libby's Project Sunshine, which eschewed ecological field work in favor of laboratory-based analysis and spot-check monitoring.

In addition to public and Congressional pressure, a major factor motivating the AEC to reevaluate and prioritize the internal emitter question and fund research in ecology was lobbying from within the Commission itself by the likes of Donaldson, Wolfe, and Eugene Odum. To be sure, concerns about human health played a small, perhaps even minor, role in someone like Odum's case. His interest laid more in fundamental and theoretical questions in ecosystems ecology. Nonetheless, the development of radioecology, the backbone of ecosystems practice, provided a "hard" material practice that linked preconceived ideas of fallout as an engineering problem to the material environment. Indeed, the fallout controversy illustrates the ways that scientific ideas have been shaped by the material environment; through radiotracer techniques, ecologists animated complex ecological processes and mechanisms that demonstrated the critical role of the environment for delivering and concentrating radiation to human bodies. There was a good measure of irony in all of this, of course. The very thing that caused so much anxiety was also the instrument that made the environment legible—fallout radiation, a potentially lethal product of one of the most powerful technological objects ever devised, was also a tool that mediated scientists' interaction with and understanding of nature. We have been culturally wired to think of nature and technology as mutually exclusive, usually in terms of technology as humankind's tool to dominate nature. But as the story of ecology and nuclear fallout show, such simple dichotomies fail to acknowledge the the more complex interactions between technology and nature. In fact, the utilization of radiation as a tool that become incorporated in nature and mimicked "natural" environmental processes—a kind of "second nature"—suggests that the framework for conceiving of nature and technology as two discrete realms or systems might have outlived its analytical usefulness.[137]

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[137] For a related example of how technology mediated knowledge of the environment, see Gardner, "Constructing a Technological Forest: Nature, Culture, and Tree Planting in the Nebraska Sand Hills."; Reuss and Cutcliffe, eds., *The Illusory Boundary: Environment and Technology in History*.

The rise of ecological thinking about fallout also held consequences for how ecologists and the AEC viewed geographic space and spatial relations. A related aspect of the engineering approach to fallout was the tendency to see the spaces where the tests were conducted as discrete and disconnected from the rest of the landscape. The notion that fallout could be contained within arbitrary boundaries (the fallout sector) was a reflection of this kind of modernist engineering-like control. Although I discuss this idea more fully in chapter 6, it is worth discussing the role of ecology during this period in demonstrating the uncontrolled transboundary movement of nature that linked the spaces of the test site to inhabited spaces across the globe.

As I've argued here and in the previous chapter, a critical practice in aquatic (and eventually terrestrial) ecology involved the search for environments that, as closely as practicable, exhibited closed-system or self-contained ecological features. Before the use of radiotracers, such practices helped solidify and "harden" ecology by making field work more lab-like. The search for the perfect natural laboratory exemplifies what Robert Kohler has termed practices of place. Selecting the right place, one that could be neatly bounded and external influences controlled, helped to ensure that the work ecologists conducted in the field would produce authentic knowledge on par, or perhaps even superior, to laboratory work. Thus Donaldson extolled the Bikini Laboratory; Odum the terrestrial field laboratory at Savannah River; Auerbach the virtues of White Oak Lake. By the late 1950s, as radiotracers were being developed and used extensively, the idea of the ecosystem as a discrete self-contained space gradually began to erode. It is no coincidence that just as fallout emerged as a global problem, so too did the concept of the ecosystem become associated with the entire biosphere. As Odum's colleague Frank Golley noted in his book on the history of the ecosystem concept, Odum moved freely between these various scales of the ecosystems unit without necessarily defining what their boundaries were. In part, this shifting of ecosystem scales was a product of ecosystems theory and radiotracers practices: if energy flow was to define the ecosystem, it proved impossible to limit its boundaries because there always occurred slippages of energy between seemingly self-contained systems. That was the basic argument set forth by ecologist LaMont Cole who in 1958 coined the term ecosphere, a combination of the ecosystem with the all-encompassing biosphere.[138] Such ideas, of the globe as an interconnected biosphere did not compel ecologists to abandon smaller-scale field work and turn exclusively to global phenomena, but the implications were obvious: what happened in one part of the globe could have profound impacts elsewhere. The ecological idea that bodies were connected to the environment, then, was seamlessly interwoven with holistic ideas of space. That is, as the boundaries between bodies and environment broke down during the atmospheric nuclear testing period, so too did space.

Both of these ideas of unboundedness have had lasting influence beyond the fallout controversy. It is no surprise that Rachel Carson's environmentalist touchstone *Silent Spring* resonated so clearly with readers when it was published in 1962. After all, the opening pages of her book directly compared the dangers of DDT to strontium-90.[139] "Strontium 90," she wrote, "released through nuclear explosions into the air, comes to

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[138] LaMont Cole, "The Ecosphere," *Scientific American* 198 (1958); Francis C. Evans, "Ecosystem as the Basic Unit in Ecology," *Science* 123 (1956).
[139] Lutts, "Chemical Fallout: Rachel Carson's Silent Spring, Radioactive Fallout, and the Environmental Movement."

earth in rain or drifts down as fallout, lodges in the soil, enters into the grass or corn or wheat grown there, and in time takes its abode in the bones of a human being, there to remain until his death. Similarly, chemical sprayed on croplands or forests or gardens lie long in soil, entering into living organisms, passing from one another in a chain of poisoning and death"[140] Nor was it a coincidence that Barry Commoner should tell his readers in *The Closing Circle* that he "learned about the environment from the United States Atomic Energy Commission in 1953."[141] Two of those valuable lessons learned by Commoner centered on the idea of interconnectedness and found their way into the first two of his "laws of ecology": 1) Everything is connected to everything else; 2) Everything must go somewhere.[142]

Most professional ecologists like Odum and Donaldson, of course, hardly subscribed to this kind of popular ecology that was emerging in the early 1960s. Theirs was a technocratic brand of ecology, founded in "hard" scientific practices; radioactivity was a poison yes, but it was also a tool that enabled them to understand the environment and better manage and control the negative effects of progress.[143] Thus, for them, the holistic conceptualization of the environment did little to temper their particular brand of technocratic optimism. Yet for folks like Carson and Commoner, they adopted the mantle of the “environment” to express regret and deep pessimism about the bodily and environmental risks inherent in American technocracy.

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[140] Carson, *Silent Spring*, 6.
[141] Commoner, *The Closing Circle: Nature, Man, and Technology*, 45.
[142] Ibid.
[143] See, for example, Bocking, *Ecologists and Environmental Politics: A History of Contemporary Ecology*; Hagen, *An Entangled Bank: The Origins of Ecosystem Ecology*; Sharon E. Kingsland, *The Evolution of American Ecology, 1890-2000* (Baltimore: The Johns Hopkins University Press, 2008); Kwa, "Radiation Ecology, Systems Ecology and the Management of the Environment."; Worster, *Nature's Economy : The Roots of Ecology*.

For this reason, some environmental historians have played down the influence of ecosystems theory on environmentalist thought.[144] To be sure, Donaldson and Odum did not engage in any diatribes against modernity as a result of their work on fallout. But to miss the critical importance of their work in making body-environment-space connections legible is to miss the base of environmental knowledge that provided the scientific justification for the Limited Test Ban Treaty. In many ways the history of ecology during the fallout controversy is a story of progress—of the creation of new ecological knowledge that helped put a close to the first global environmental crisis.

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[144] For example, Nash, *Inescapable Ecologies: A History of Environment, Disease, and Knowledge*; Worster, *Nature's Economy : The Roots of Ecology*. Bocking notes that institutional circumstances played a critical role in how professional ecologists approached the politics of environmentalism. Bocking, *Ecologists and Environmental Politics: A History of Contemporary Ecology*.

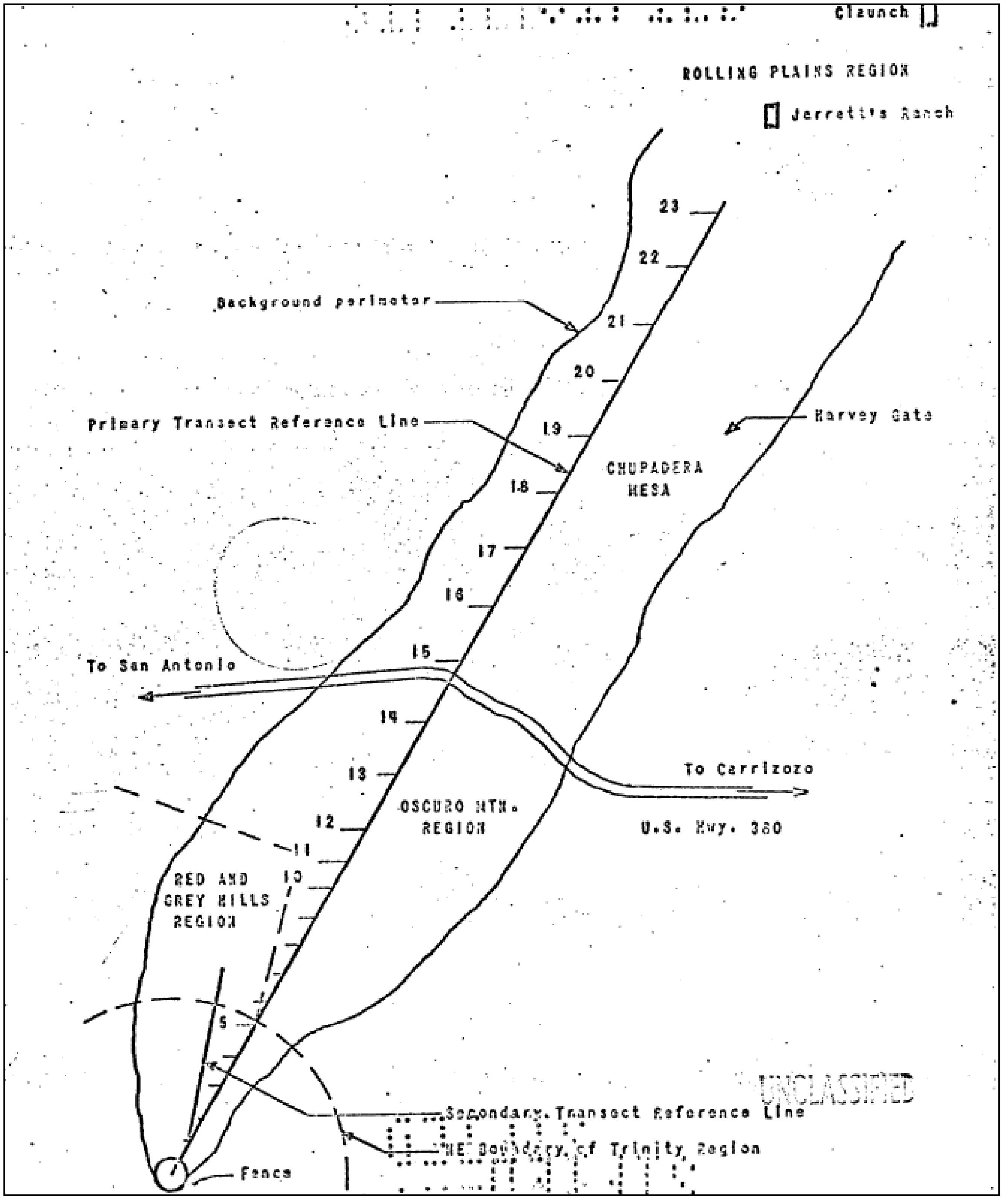

Figure 17 - AEP Transect. Source: Larson, Kermit et al., "The 1948 Radiological and Biological Survey of Areas in New Mexico Affected by the First Atomic Bomb Detonation," Atomic Energy Project Report UCLA 32. Public domain.

Figure 18 - Hanford Aquatic Laboratory, November 1951. Photo courtesy of the Hanford Declassified Document Retrieval System.

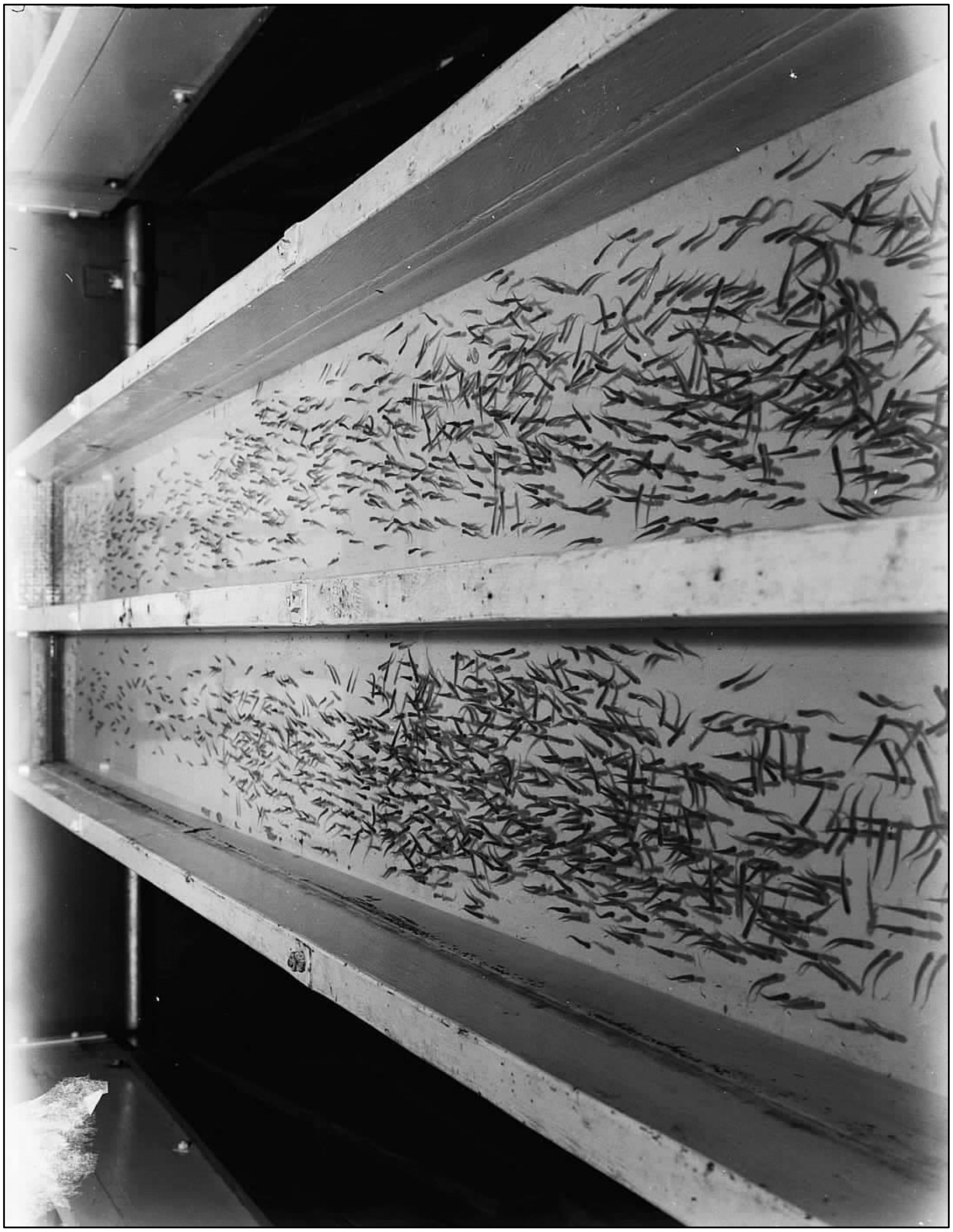

Figure 19 - Hanford Aquatic Laboratory fish troughs, August 1946. Photo courtesy of the Hanford Declassified Document Retrieval System.

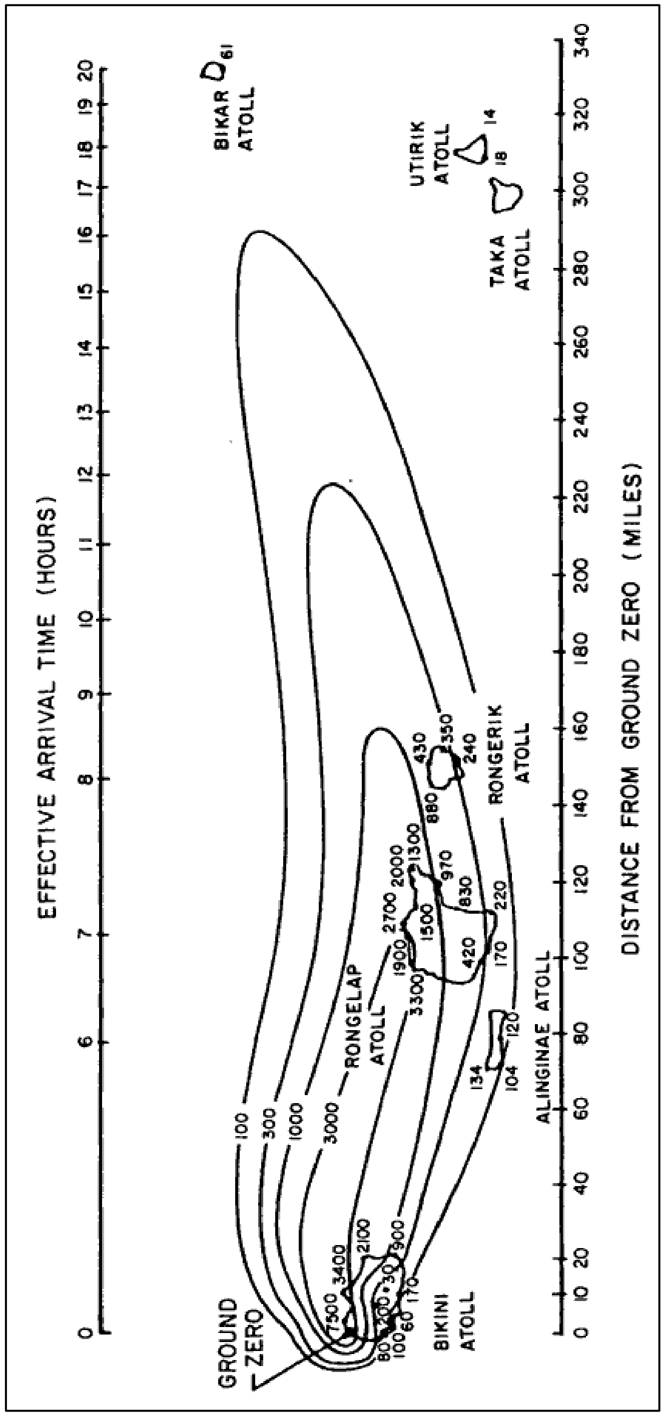

Figure 20 - Bravo fallout pattern. Source: Glasstone, Samuel, and Phillip J. Dolan. The Effects of Nuclear Weapons. 3rd ed. Washington D.C.: Department of Defense, 1977. Public domain.

IN SEARCH OF THE BRAVO FOOTPRINT:

FALLOUT, OCEANOGRAPHY, AND THE DYNAMIC OCEAN

In August of 1955, Roger Revelle, who left the Navy's Office of Naval Research in the late 1940s to head Scripps Institution of Oceanography, wrote a research proposal to the Atomic Energy Commission (AEC) requesting money for an intensive research program into the "use of nuclear tools in oceanographic research." The proposal, nearly identical to a paper that he and three of his Scripps colleagues were delivering at the first Atoms for Peace Conference in Geneva, Switzerland that same month, argued that radiation offered oceanographers a powerful tool to study the oceans:

> Many scientific problems of the ocean can be resolved into questions concerning the fluxes within its realms. Promising possibilities exist for the study of these fluxes by the tracer techniques of nuclear science, but to date the possibilities have only been timidly explored. A more vigorous and imaginative application of nuclear tools in the marine sciences would certainly result in important breakthroughs.[1]

Revelle had been a leading advocate for the increased use of radiotracer practices in oceanography since the Bikini Scientific Resurvey in 1947. Yet, in the intervening years between the Resurvey and the Geneva conference, his vision had not yet come to fruition. Although Scripps had conducted oceanographic work for the Navy as part of the nuclear tests out in the Pacific, their research was confined largely to blast wave and other oceanic phenomena of instrumental value to their patrons' interest in the conduct of undersea warfare. Similarly, the AEC in these years had little interest in expanding the

[1] Revelle, Roger, August 9, 1955, "Proposal for the Use of Nuclear Tools in Oceanographic Research," Scripps Institute of Oceanography Archives, La Jolla, CA (hereafter SIOA), John Dove Isaacs Papers, box 108, proposals 1955, 4. R. Revelle et al., "Nuclear Science and Oceanography," in *Proceedings of the International Conference on the Peaceful Uses of Atomic Energy, Volume 13 Legal, Administrative, Health and Safety Aspects of Large-Scale Use of Nuclear Energy* (New York: United Nations, 1956).

already severely limited marine radiological surveys to include pelagic processes that might distribute fallout radiation beyond atolls such as Bikini and Eniwetok.

Circumstances in 1955, however, were quite different. In March of the previous year, the Bravo incident touched off an international crisis when the thermonuclear test, which greatly exceeded its expected yield, irradiated downwind Marshall Islanders and a Japanese fishing vessel trolling in nearby waters. While much of the controversy centered on the direct human exposures (including the death of one of the Japanese crewmen), an equally contentious issue arose on the effect of the bomb on commercially important Pacific fisheries and their potential impact on human health. In the following months, the Japanese launched an oceanographic expedition aboard the *Shunkotsu Maru* in search of what Lauren Donaldson called the "Bravo footprint"—that is, the extent and amount of the bomb's radiation in the Pacific waters and organisms, especially critical food sources such as tuna.[2] It was the first such survey of its kind related to a nuclear weapons test and it demonstrated quite clearly that physical and biological oceanic processes were distributing significant amounts of Bravo's radiation far beyond the AEC's prescribed danger zone. In the wake of Bravo and the Japanese expedition, the AEC initiated similar oceanographic surveys with the help of the Health and Safety Laboratory, the Applied Fisheries Laboratory, and Scripps Institution of Oceanography. Thus when Revelle wrote his proposal, the political atmosphere was ripe for forming new lines of patronage for oceanography with the AEC. Yet, as Revelle's quote above suggests, he was not primarily interested in conducting applied research related simply to

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[2] Lauren R. Donaldson, Allyn H. Seymour, and Ahmad E. Nevissi, "University of Washington's Radioecological Studies in the Marshall Islands, 1946-1977," *Health Physics* 73, no. 1 (1997).

radiological effects. Rather, he hoped to capitalize on a political and environmental fact of life: if weapons tests were irradiating the oceans, why not take scientific advantage of it and use these tests as a way to understand radiological effects *and* simultaneously expand our knowledge of the oceans? What timidity he may have been expressed about the use of nuclear tools before Bravo soon gave way to a full-throated scientific thrust in its wake. As a result, by tracing fallout radiation throughout the oceans, oceanographers produced new knowledge about the effects of radiation of marine life beyond the confines of the testing sites and demonstrated that the ocean was a dynamically integrated space.

In this chapter, I build upon the arguments that I have made in previous chapters regarding the shift from health physics ideas of bodily and environmental boundedness to more ecological frames for thinking about the problems associated with and scientific opportunities enabled by environmental radiation. The case of oceanography and nuclear fallout, as we will see, was not dissimilar from developments in ecology. Both disciplines were associated with postwar nuclear weapons testing beginning with Operation Crossroads and likewise struggled to maintain authority in matters of environmental radiation until Bravo. Moreover, the fact that Lauren Donaldson and the Applied Fisheries Laboratory participated in the fisheries aspect of these oceanographic ventures demonstrates that in some ways the disciplinary differences between the fields (at least in the biological realm) were not as stark as one might think in marine science. Nonetheless, oceanography was a physical as much as a biological discipline. Radiotracers held opportunities for not only illuminating biological structures and functions in the ocean, but also physical mechanisms such as diffusion and wave mechanics. Ronald Doel has argued that by the early 1960s the distinctions between biological and physical environmental sciences had hardened, largely based on their sources of funding—the physical funded largely by the military and geared toward its operational needs, the biological by agencies concerned about human impacts on the environment.[3] In some ways, this split is apparent in oceanography during the fallout period. The Navy for example, was far more interested in the application of physical oceanography and the use radiation tools for making the oceans legible for the conduct of war, while the AEC was interested primarily in marine biological effects. Yet, the emerging realization that oceans were dynamic spaces that mediated the distribution and concentration of fallout well beyond the test sites was a function of both biological (marine food chain dynamics, for example) and physical (currents) mechanisms. In other words, the growth of hydrospheric understandings of the oceans was a product of the intimate linkage of biological and physical oceanography around the problem of fallout.

Unlike my other chapters on ecology and meteorology (the next chapter), the basic narrative outlines of the connections between fallout, radiotracers, and the growth of oceanography has been explored previously Jacob Hamblin and Ronald Rainger.[4] My approach, while in line with this research, is framed a bit differently. By focusing on the ways in which this oceanographic research conflicted with AEC's assumptions about the Pacific nuclear tests as bounded events, I provide needed context for understanding how

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[3] This is paraphrased from Doel, "Constituting the Postwar Earth Sciences: The Military's Influence on the Environmental Sciences in the USA after 1945," 653.

[4] Hamblin, *Poison in the Well: Radioactive Waste in the Oceans at the Dawn of the Nuclear Age*; Rainger, "'A Wonderful Oceanographic Tool': The Atomic Bomb, Radioactivity and the Development of American Oceanography."

new holistic ways of thinking about the relationship between bodies, the environment, and geographical space emerged and shaped the assessment of fallout risks and environmental hazards generally. Hamblin, for example, has argued that oceanographers were hardly the environmentalist type that popular representations in later years would make them out to be.[5] This is true; oceanographers were fierce proponents of nuclear testing for scientific purposes. Yet, by looking at the ways that oceanographic research contributed to the notions of the integrated biosphere, their work, despite their proclivities, nonetheless had profound impacts on the growth of environmentalist thought.

Oceanography and Nuclear Weapons Testing: From the Bikini Scientific Resurvey to Bravo

As I noted in chapter 3, Revelle had been particularly taken with the prospect of using bomb test radiation as a tool to trace out physical oceanographic processes during Operation Crossroads and the Bikini Scientific Survey. In 1949, Revelle, along with Walter Munk and Gifford Ewing, demonstrated their usefulness when they published the results of one of their diffusion experiments in Bikini Lagoon.[6] Nonetheless, in the late 1940s and early 1950s, such potential had not been capitalized on. In part, this can be explained by the fact that there were few opportunities for such work. In the years between Crossroads and the first thermonuclear test in 1952, only two testing series had been conducted in the Pacific (Sandstone, 1948 and Greenhouse, 1951). Even then, government and military interest in the kind of broad, large-scale oceanographic and

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[5] Hamblin, *Poison in the Well: Radioactive Waste in the Oceans at the Dawn of the Nuclear Age*.
[6] Munk, Ewing, and Revelle, "Diffusion in Bikini Lagoon."

biological surveys associated with nuclear tests that characterized Crossroads had waned. On the one hand, such expeditions were expensive. On the other, the Navy and the Atomic Energy Commission saw little need for such studies since the Resurvey had seemingly demonstrated that most radiological effects were ephemeral.

That did not mean that the Navy had lost interest in oceanography, however. In the years following Operation Crossroads, Revelle had been busy securing and strengthening naval patronage of oceanography. Shortly following Crossroads, he left the Navy's Bureau of Ships to head the Geophysics Branch of the newly-formed Office of Naval Research (ONR). It was at this moment that Revelle organized the Bikini Scientific Resurvey and began to forge lasting ties between oceanography and the Navy. In a memo to the Chief of Naval Research with a subject heading entitled "Oceanography in the Navy Post-war Program," Revelle outlined for the Navy a blueprint of his vision of an oceanography-naval alliance.[7] The plan, by and large, pointed to the immense usefulness of oceanography to naval operations and hence national security. But rather than simply couple their interests, Revelle, as Ronald Rainger has argued, carved out a space that enabled oceanographers to conduct basic science (with plenty of room for intellectual autonomy and opportunities to publish in the open literature) while simultaneously meeting the needs of the Navy.[8] Furthermore, according to Rainger, such a system did not privilege either the oceanographers or the Navy. Rather, "scientists knowingly and willingly did military work...they embedded or incorporated their interests

[7] Revelle to Chief of Naval Research, undated, "Oceanography in the Navy Post-war Program," SIO Office of the Director (Revelle), 1848-1964 Records (hereafter SIODR), SIOA, box 12, folder Operation Crossroads, 1946-47.
[8] Rainger, "Constructing a Landscape for Postwar Science: Roger Revelle, the Scripps Institution and the University of California, San Diego," 335-6.

within a military framework."[9] By the time Revelle left ONR to join Scripps Institute of Oceanography in 1948 his blueprint was firmly in place. Having set up the protocols and practices that established this understanding while at the Navy, he was now at the public institution that would receive the lion's share of this largesse.

A fine early example of the arrangement that Revelle worked out was the Scripps Mid-Pacific Expedition (MIDPAC) in 1950. A joint Scripps-Navy expedition, MIDPAC was the first of a string deep ocean surveys conducted by Scripps throughout the 1950s. The expedition has been noted for its important geophysical discoveries including a sea-floor mountain range and, through seismic refraction surveys, data helping to confirm the Dana-Darwin coral formation hypothesis.[10] Yet, this same data also improved the Navy's ability to wage undersea warfare.[11]

Scripps next major expedition, Capricorn, was closely allied with the first experimental thermonuclear test in 1952—Ivy-Mike. Revelle used the test as a launching point for the expedition, although no mention is made of this in the Capricorn reports.[12] In the weeks prior to the Mike shot, Scripps scientists set sail aboard the *Horizon* to begin setting up instruments for measuring and recording waves produced by the shot as well as seismic refraction surveys.[13] Only after the post-test measurements were completed did

[9] Rainger, "Science at the Crossroads: The Navy, Bikini Atoll, and American Oceanography in the 1940s," 369.
[10] Elizabeth Noble Shor, *Scripps Institution of Oceanography: Probing the Oceans, 1936-1976* (Tofua Press, 1978), 388.
[11] Rainger, "Constructing a Landscape for Postwar Science: Roger Revelle, the Scripps Institution and the University of California, San Diego," 339.
[12] Roger Randall Dougan Revelle, interview by Sarah L. Sharp, August 13 and 14, 1985, transcript, SIOA, 36.
[13] Willard Bascom, *The Crest of the Wave: Adventures in Oceanography* (New York: Harper and Row, 1988), 82-114.Bascom, 82-114. On the seismic refraction survey see Russell W. Raitt, "Report to the Scientific Director: Underwater Pressures of Seismic Waves," October 1953, NTA, NV051397.

the larger deep sea expedition commence with the arrival of the second Capricorn ship, the *Spencer F. Baird*. Capricorn, in both its guises, focused solely on physical oceanography. Indeed, the emphasis on the physical research was reflected in the pretest planning for potential nuclear accidents. When concerns were raised that the thermonuclear test might cause a submarine landslide at Bikini and produce a tsunami, the Navy provided Scripps with considerable funds to study the issue.[14] Such an event never occurred, but the fact that physical effects dominated much of the planning and preparation speaks to how little biological impacts concerned either the AEC or the Scripps scientists. In fact, the only biological work conducted during Ivy-Mike was a small-scale effort by the Applied Fisheries Laboratory to survey of Bikini and nearby atolls for radiation. The AFL surveys were not, however, strictly oceanographic and not associated in anyway with Capricorn. Biology played second fiddle to physical oceanography in the early postwar tests.

That would soon change with the next thermonuclear test. When the Castle Bravo device was detonated at Bikini on March 1, 1954, the unexpected high yield of the explosion took nearly everyone by surprise. At fifteen megatons of explosive energy, the shot exceeded its expected yield by roughly ten megatons. In the process, the shot rained down radioactive fallout on nearby Atolls. Rongelap was the hardest hit and its more than two hundred natives were evacuated, but not before most had been exposed to lethal levels of radioactivity. Moreover, a Japanese fishing vessel (the *Fukuryu Maru* or "Lucky Dragon") trolling outside the "danger zone" prescribed by the AEC was similarly exposed, unbeknownst to either the military or the AEC at the time.

[14] Walter Munk and Deborah Day, "Ivy-Mike," *Oceanography* 17, no. 2 (2004). See also ibid., 36-7.

The massive fallout reprioritized much of the scientific work in the weeks after Bravo. A number of Scripps scientists charged with measuring ocean phenomena resulting from the blast, as with Ivy-Mike, soon found themselves conducting radiological surveys of the surrounding water or doing biological work. Ted Folsom, for example, had to drop his physical work and begin assessing the extent and severity of the ocean contamination. As he later noted in his final report, the project was, by necessity, "hastily contrived."[15] Nonetheless, the evidence of widespread fallout contamination that he found, rushed or not, was shocking: "One conclusion evident from these contours [showing accumulated radiation exposures] is that total doses of 250 r[oentgens] or more could have been accumulated throughout an area of about 5,000 square miles."[16] The implications were deadly serious: any person within that area (which exceeded the danger zone) would have been exposed to nearly acute lethal levels of external gamma radiation. Folsom and his colleagues Martin Johnson and Feenan Jennings also collected some plankton for analysis. The collected samples, not surprisingly, displayed high concentrations of radioactivity. What the authors had not anticipated, however, was the role that such organisms would play in the distribution of the radioactivity in the ocean. "This paper reports the first direct in situ evidence of the profound influence of deep sea organisms on the partition of radioactive debris from atomic weapons, and directly demonstrates the inadequacy of a model that accommodates only the physical processes

[15] T.R. Folsom and L.B. Werner, "Operation Castle, Project 2,7: Distribution of Radioactive Fallout by Survey and Analyses of Sea Water," April 14, 1959, NTA, NV0015203, 7.
[16] Ibid, 77.

of mixing, advection, etc."[17] Because plankton concentrated such high levels of radiation, they, as much as the currents, contributed to the spread of radioactivity. Scripps oceanographers would heed that lesson in the future; following Bravo, ecological studies of metabolism and distribution of radiation would form a major part of their investigations associated with nuclear testing.

Bravo, The Tuna Scare, and the First Deep Sea Radiological Expeditions

In the meantime, a new controversy was erupting when the *Lucky Dragon* arrived at home port in mid-March. During the two week voyage home after being dusted with the Bravo fallout, crewmembers were beginning to show classic symptoms of radiation exposure, namely skin burns and nausea. By the time they reached port, it was apparent that all were suffering from acute radiation sickness. As Japanese newspaper reports of the crewmen reached across the Pacific, the AEC was faced with an escalating international crisis. On the 19th, after learning from some of its officials in Japan that the reports were true, the AEC quickly dispatched Health and Safety Laboratory director Merril Eisenbud to Japan to advise both the U.S. embassy and Japanese health officials on the "radiological aspects of the matter."[18] Although Eisenbud visited the crewmen in the hospital he could offer little help; months after the incident, one of the crewmembers would eventually succumb to his injuries.

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[17] T.R. Folsom, F.D. Jennings, M.W. Johnson, "Operation Castle—Project 2.7a, Report to the Scientific Director: Radioactivity of Open-Sea Plankton Samples," April 1958, NTA, NV018897, 6.
[18] Merril Eisenbud, *Environmental Odyssey: People, Pollution, and Politics in the Life of a Practical Scientist* (Seattle: University of Washington Press, 1990), 85.

Eisenbud also had to deal with another equally controversial finding in the wake of Bravo: the fish aboard the Lucky Dragon was seriously contaminated and some of it had already been shipped off to market. Once news of this reached the public, there immediately followed an industry crippling "tuna panic" that spread to not only suspected *Lucky Dragon* shipments, but tuna generally. And the concern was not confined merely to Japan. American canneries notified the Japanese that they would not accept any radioactive fish from Japanese unless they were certified as non-radioactive.[19] Soon enough, fresh tuna catches were displaying various levels of radioactivity. In the coming weeks, as a result, Eisenbud spent considerable time advising the embassy and the Japanese on methods for detecting radiation in tuna and dealing with questions about safe-levels of radiation consumption. The latter proved especially vexing as no standards were then available for determining what was a safe level of radiation in food fish. For this reason, as Eisenbud later recalled, he refused to recommend a standard without concrete data.[20] Nonetheless, an ad-hoc standard was eventually adopted that established a maximum permissible limit at 100 counts on a geiger-muller radiation detector per minute per gram of material.[21] Notably, this standard pertained only to external radiation and not specific radionuclides.

The Japanese, however, immediately began searching for data on which to devise a more reliable standard. Yoshio Hiyama, a fisheries scientist at the University of Tokyo, for example, wrote to Lauren Donaldson and Arthur Welander at the Applied Fisheries Laboratory to get their advice on the problem. Donaldson and Welander, accordingly,

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[19] Ibid., 95.
[20] Ibid., 96.
[21] Hines, *Proving Ground: An Account of the Radiobiological Studies in the Pacific, 1946-1961*, 175.

forwarded copies of various lab reports on their work at Bikini and Eniwetok, but demurred at suggesting a permissible radiation limit for tuna contamination.[22] Interestingly, as the correspondence between the three shows, Hiyama may have been less alarmed about the radiation than either of them. In letters to Welander, for example, he thought the 100 count standard too conservative by a factor of 10 and suspected that the panic was probably "caused by exaggerated news and some bad-minded people."[23]

Nonetheless, whatever one's views on the standard, the fact that tuna fish were absorbing significant amounts of fallout radiation was enough to cause considerable alarm because it directly called into question the AEC's contention that radiological effects to sea creatures was confined to the test site's danger zone [compare figures 20 and 21]. Despite the growing evidence to contrary, however, the Commission doubled down on this notion of spatial containment in a statement released by the U.S. Ambassador John Allison. In part, it read:

> [It is the] Opinion [of] Atomic Energy Commission scientific staff based on long-term studies of fish in presence of radioactivity is that there is negligible hazard, if any, in consumption of fish caught in the Pacific Ocean outside immediate test areas subsequent to tests...As to ocean currents, the warm currents which flow from the Marshall Islands area…move slowly (less than a mile an hour). Any radioactivity collected in test area would become harmless within a few miles…and completely undetectable within 500 miles or less.[24]

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[22] Lauren Donaldson to Yoshio Hiyama, April 16, 1954, Laboratory of Radiation Ecology Records (hereafter LRER), University of Washington Libraries Special Collections, Seattle, WA (hereafter UWSC), box 3, Radiation H.

[23] Yoshio Hiyama to Arthur Welander, April 5, 1954, LRER, UWSC, box 4, Radiation H; Yoshio Hiyama to Arthur Welander, April 12, 1954, LRER, UWSC, box 4, Radiation H. Quote is from the letter of April 12.

[24] Quoted in Hines, *Proving Ground: An Account of the Radiobiological Studies in the Pacific, 1946-1961*, 177-8.

This statement was not just public relations rhetoric, however. John Bugher, the AEC's director of the Division of Biology and Medicine concluded as much in an internal report:

> From consideration of the current, the depth of mixing of surface water, the rate of decay of fission products, I reached the conclusion that the probability of significant contamination of fish outside the immediate test area is inconsequential and that in all likelihood we will be unable to detect these waters more than a few hundred miles away from the Marshall Islands.[25]

This was a remarkable statement, coming as it did from a medical physician and not an oceanographer. It was also pure speculation. In none of the prior Pacific tests did the AEC conduct oceanographic surveys investigating the distribution of radiation in the deep sea. Instead, the few modest biological surveys that they did support through the Applied Fisheries Laboratory were localized affairs (see chapters 3 and 4). Moreover, as Bugher's statement suggests, AEC officials assumed that once in the open waters fallout would simply dissipate. The ocean, in this kind of thinking, was not a dynamic space requiring special consideration from scientists who knew it best (oceanographers). Rather, it was thought of in engineering terms—as a marine sink.

The persistent detection of radiation in tuna harvested after the *Lucky Dragon* incident, of course, suggested otherwise to many Japanese. In his correspondence with Donaldson and Welander in April, plans were being forged in Japan for an oceanographic survey to measure and map the spread of radioactivity in the ocean waters around Bikini. Hoping to have American participation in the expedition, Hiyama asked whether Donaldson or Welander would like to participate.[26] Donaldson wrote back agreeing that

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[25] John Bugher, March 31, 1954, "Effects of Pacific Tuna on Commercial Tuna Fishing," Nuclear Testing Archive, Las Vegas NV (hereafter NTA), accession # NV0408700.
[26] Yoshio Hiyama to Lauren Donaldson, April 20, 1954, LRER, UWSC, box 4, Rad iation H.

an American scientific representative would be a good idea and forwarded Hiyama's suggestion to the Division of Biology and Medicine for consideration.[27] (Donaldson, aware of the delicate international situation, in fact, had been copying Hiyama's correspondence to the DBM from the beginning).[28] Shortly later, the AEC, with the approval of the State Department, agreed to send Donaldson and DBM official Willis Boss to Japan in May to accompany the expedition.[29] When Donaldson and Boss arrived in Tokyo on May 24th, however, the expedition party aboard the ship the *Shunkotsu Maru* had already departed and been at sea for nine days. Although it is not clear exactly why the Japanese decided not to delay setting sail for Donaldson and Boss' arrival, it appears that the extreme sense of urgency to discover the extent of the radiation and its effect on tuna populations compelled them to leave earlier.[30] Donaldson and Boss, in the event, contented themselves by accompanying Hiyama (who did not participate in the expedition) on tours of various fisheries operations throughout Japan and dining, as openly as possible, on sushi in an effort to allay the tuna panic.[31]

Meanwhile, the *Shunkotsu Maru* expedition was making significant discoveries. Headed by Hiroshi Yabe, then chief of the Resources Department of Nankai District Fisheries Research Institute, the survey covered roughly 9,000 nautical miles from May

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[27] Lauren Donaldson to Yoshio Hiyama, April 30, 1954, LRER, UWSC, box 4, Radiation H.
[28] Lauren Donaldson to John Bugher, April 20, 1954, LRER, UWSC, box 1, folder 40.
[29] Hines, *Proving Ground: An Account of the Radiobiological Studies in the Pacific, 1946-1961*, 187.
[30] Leonard Graham, "The 1954 *Shunkotsu Maru* Expedition and American Atomic Secrecy," *International Public Policy Studies* 15, no. 2 (2011).
[31] Hines, *Proving Ground: An Account of the Radiobiological Studies in the Pacific, 1946-1961*, 188. See also, Stafford L. Warren, "An Exceptional Man for Exceptional Challenges," interviewed by Adelaide G. Tusler, 1966-1968, transcript, Oral History Program, University of California Los Angeles, Charles E. Young Research Library, Department of Special Collections, Los Angeles, CA, 543.

15 to July 4 and collected hundreds of water samples and marine organisms.[32] In all, twenty-two Japanese scientists, from meteorologists to ichthyologists, participated in the voyage.[33] The results of the expedition confirmed what many of them suspected: significant levels of radiation were detected in pelagic waters and organisms well-outside the "danger zone." In general, the radiation appeared to have been conducted furthest along the North Equatorial Current to the northwest, where activity of more than 100 counts/minute/liter of water with a geiger-counter was recorded as far as 2,000 kilometers from Bikini. To the south, the dispersion was not quite as pronounced owing to the presence of the Equatorial Counter Current. Still, significant activity was detected 1,000 km from Bikini in this direction.[34] Furthermore, the expedition demonstrated that the ecological dynamics involving the concentration of radiation by plankton, which Donaldson first discovered at Bikini Atoll during Operation Crossroads, was similarly operating in the open seas. According to Toshiharu Kawabata, who would later join the Applied Fisheries Laboratory as a visiting researcher, the expedition results suggested that "the contamination of large-sized fishes came chiefly from their baits, and the route of the contamination may be: sea water—plankton—small-sized fish—large-sized fish…Therefore, the difference in the extent of contamination of large-sized fishes may

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[32] Shinichi Watari, "Summary of Investigation by the Shunkotsu Maru into the Effects of Radioactivity in the Bikini Waters," in *Research in the Effects and Influences of the Nuclear Bomb Test Explosions*, ed. Committee for Compilation of Report on Research in the Effects of Radioactivity (Tokyo: Japan Society for the Promotion of Science, 1956), 941.
[33] Ibid., 939.
[34] Yasuo Miyake, "On the Distribution of Radioactivity in the North Pacific Ocean in 1954-1955," in *Proceedings of the International Conference on the Peaceful Uses of Atomic Energy, Volume 13 Legal, Administrative, Health and Safety Aspects of Large-Scale Use of Nuclear Energy* (New York: United Nations, 1956). This was published and made available to AEC scientists as Y. Miyake, Y. Suguira, and K. Kameda, "On the Distribution of Radioactivity in the Sea around Bikini Atoll in June, 1954," *Papers in Meteorology and Geophysics* 5, no. 3-4 (1955).

be related to their food habits, or food chain."[35] The concentration factor of plankton, they figured, was on the order of 1,000 to 10,000 times that of sea water.[36]

The *Shunkotsu Maru* expedition was the first oceanographic survey related to fallout of its kind. By demonstrating how physical and biological factors were contributing to the contamination of wide swaths of the ocean, the expedition helped usher in a new approach to the problem of environmental radiation. No longer confined to the test sites, fallout now held biological significance for the entire ocean. The Japanese, not surprisingly, were anxious to share this information with the Americans and a joint-conference was subsequently held over a week in mid-November in Japan. Although much of the conference focused on questions of radiological instrumentation and exposure standards, Merril Eisenbud, who attended the meeting, used the opportunity to gather as many of the results of the survey as possible to bring back home to AEC headquarters for analysis.

Accordingly, when Eisenbud got back to the States he called Allyn Vine of Wood Hole Oceanographic Institute to come to the AEC as a consultant and analyze the data.[37] Based on extrapolations from the *Sunkotsu Maru* data, Vine hypothesized that a significant mass of the Bravo fallout would soon make its way to the coasts of Japan and

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[35] Toshiharu Kawabata, "State of Radiological Contamination in Fishes and Its Possible Routes of the Basis of the Findings of 'Bikini Expedition'," in *Research in the Effects and Influences of the Nuclear Bomb Test Explosions*, ed. Committee for Compilation of Report on Research in the Effects of Radioactivity (Tokyo: Japan Society for the Promotion of Science, 1956).
[36] Watari, "Summary of Investigation by the Shunkotsu Maru into the Effects of Radioactivity in the Bikini Waters," 942.
[37] John H. Harley, April 1956, "Operation Troll: A Joint Preliminary Report," NTA, NV0407862 ; January 18, 1955, "Transcript of Biophysics Conference," National Archives and Records Administration, College Park, MD (hereafter NARACP), Records of the Atomic Energy Commission, Entry 73b, (hereafter RAEC), box 8, folder Biophysics Conference, 147.

East Asia by the middle of the next year.[38] A few days later, Eisenbud participated in a well-attended conference on fallout in Washington D.C. and told the attendees, which included Commissioner Willard Libby and DBM director John Bugher, that his and Vine's preliminary workup of the data was frankly "rather startling."[39] Indeed, as Bugher commented later in the conference, "As I see it, the main characteristic of this whole ocean system is the frequency which things which seem obvious turn out to be not true."[40] Bugher's remarks are telling for they illuminate how the material realities of the ocean were resisting the Commission's preconceptions about the spatial integrity of the borders they erected around the test sites (the "danger zone"). Furthermore, the evidence of widespread contamination threatened to undermine the Commission's long-held assumption that radiation in the oceans would be swiftly dispersed to insignificant levels. Still, Bugher held hope that a follow-up American expedition might help redefine the problem, telling the conference attendees that, "We would like to know what the boundaries of that problem are before the Japanese demonstrate them to us."[41] That fallout might be an environmental problem without boundaries had not yet occurred to him.

At the conclusion of the meeting in January, plans for an American follow-up to the *Shunkotsu Maru* survey were moving quickly. Two days after the Washington conference, Eisenbud and Bugher flew to Seattle to attend an Advisory Committee for Biology and Medicine meeting and proposed that the Commission "initiate immediately

[38] ibid.
[39] January 18, 1955, "Transcript of Biophysics Conference," NARACP, RAEC, box 8, folder Biophysics Conference, 146.
[40] Ibid., 151.
[41] Ibid., 154.

another oceanographic survey." Bugher also recommended that the expedition also include a Japanese contingent to "further diplomatic relations," but in the end did not come to pass.[42] Instead, organization and conduct of the expedition fell to John Harley of the AEC's Health and Safety Laboratory, Warren Wooster of Scripps, and Allyn Seymour of the AFL.

Dubbed "Operation Troll," the expedition was in many ways almost identical to the Japanese venture. Departing San Francisco in late February aboard Coast Guard cutter *Roger B. Taney*, the oceanographic party sailed for the Marshall Islands where they subsequently covered some seventeen and a half thousand miles traveling eastward from the proving ground and along the coasts of the Philippines and Japan.[43] Research activities included taking water and plankton samples roughly every one hundred and eighty miles at depths up to 600 meters and utilizing a newly developed scintillation probe to make continues profiles of radiation levels along their track. The latter proved of little use owing to technical difficulties and its incapability of detecting low-levels of radiation. All in all, the analysis of the samples confirmed what the Japanese had found. Although the radioactivity in the ocean a year after the Bravo detonation had decayed to biologically insignificant levels, enough was still present to affirm indeed that fallout material was traveling along major ocean currents rather than mixing and diffusing as previously thought. At one sampling station off the coast of Luzon in the Philippine archipelago, for example, one of the highest radiation levels was detected at about 190

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[42] "Minutes, Advisory Committee for Biology and Medicine, Atomic Energy Commission," January 1955, NTA, NV0708964.

[43] Results of expedition can be found in John H. Harley, "Operation Troll: A Joint Preliminary Report," April 1956, NTA, NV0407862

counts/minute/liter of water at 600 meters below the surface of the water.[44] Evidence of vertical mixing processes in the ocean remained rather obscure, however. While measurements of radioactivity from the ocean's surface to the level of the thermocline (the layer in the ocean in which temperature drops and density increases) demonstrated that mixing was quite evident, the behavior of radionuclides below this layer was basically unknown. What was clear, however, was that plankton played a critical role on the movement of radiation both horizontally and vertically. As plankton moved between various depths of the ocean in response to light, a concomitant increase or decrease in radioactivity levels in the ocean were starkly evident. Indeed, following Operation Troll, plankton would be regarded as a particularly useful indicator species and thus biological monitor for the overall radioactive burden of the ocean. As the final report put it, plankton afforded scientists "a sensitive indication of the activity in the ocean."[45]

Both the Japanese and American oceanographic expeditions marked an important turning point for redefining the spatial parameters of the fallout problem in the Pacific. Once thought of bounded problem whose effects were contained within the testing sites of Bikini and Eniwetok, fallout now held potentially deadly implications for wide swaths of the ocean. Operation Troll, however, was not the only oceanographic research related to nuclear weapons testing in the months following Bravo. On May 17, two weeks after the *Taney* sailed into port in San Francisco, the U.S. Navy set off a bomb underneath the surface of the ocean some 500 miles off the coast of San Diego, CA. A Navy rather than

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[44] Gordon M. Dunning, November 1956, "Review of Data, Radioactive Contamination of Pacific Areas from Nuclear Tests," NTA, NV0067884.
[45] John H. Harley, April 1956, "Operation Troll: A Joint Preliminary Report," NTA, NV0407862, 13.

and AEC project, Operation Wigwam similarly helped to redefine the problem of oceanic fallout.

A Bomb for Oceanography: Operation Wigwam

Navy interest in a deep water nuclear test dated back to Operation Crossroads. Crossroads, in fact, had been originally designed as a three shot series (air, shallow water, and deep sea). Yet, the radiological mess following Baker convinced the Navy that a deepwater test would not be prudent from a human health standpoint. Navy brass continued to press the issue, nonetheless, arguing that data on underwater blast effects was necessary for naval war preparations, in particular, submarine maneuvers and protection. Accordingly, the Navy in 1951 assembled what was later called the "Pelican Committee" to advise the Armed Forces Special Weapons Project (AFSWP) on the desirability and feasibility of conducting a deepwater test. Oceanographers were involved in the project from the very start. One of the sub-committees included in Pelican was a panel devoted exclusively to oceanography and included Allyn Vine of Woods Hole Oceanographic Institution and Arnold Arons and Gifford Ewing of Scripps. In spring of 1952, the Pelican Committee recommended going ahead with the project and AFSWP quickly formed an ad hoc committee to begin planning.[46]

One of the first priorities centered naturally on finding a suitable test site. Foremost in planners' minds was an area where a bomb of about 20 kilotons could be detonated approximately 2,000 meters below the surface of water in a particular deep part

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[46] Details of the organization of Operation Wigwam can be found in "Operation Wigwam, Scientific Director's Summary Report," May 1955, NTA, NV0009000; "Operation Wigwam, Report of Commander, Task Group 7.3," July 22, 1955, NTA, NV0008848.

of the ocean to "minimize effects from bottom reflection."[47] Radiological concerns also played an important role. To that end, planners wanted an area of the ocean with relatively predictable ocean currents and meteorological conditions.[48] Allyn Vine made a survey of potential sites in the Caribbean, but determined that the possibility of radioactivity washing up on foreign shores too great. Eventually, Scripps oceanographers found a site roughly 500 miles off the coast of California that fit the technical requirements and had the added benefit of being far removed from commercially important fishing grounds.

The AEC initially opposed the tests, fearing that the tuna scare in the wake of Bravo would lead to "adverse public reaction."[49] As a naval operation, there was little the AEC could do to stop the tests, however. Eventually, the Commission relented but only after they coordinated a public relations plan with the Navy and arranged to have the U.S. Food and Drug Administration monitor fish coming into market and canneries for processing.[50] The fact that the Applied Fisheries Laboratory did not participate in any aspects of Wigwam further cemented the Navy's control over the test. Nonetheless, concern over the radiological impacts on oceanic biota from Wigwam ensured that biological studies would accompany the physical data that the Navy sought.

From the beginning, the Scripps scientists considered the underwater test, which by 1954 was being referred to as Operation Wigwam, as less a nuclear test than a large-

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[47] "Operation Wigwam, Report of Commander, Task Group 7.3," July 22, 1955, NTA, NV0008848, 15.
[48] Ibid, 181.
[49] Quoted in Hacker, *Elements of Controversy: The Atomic Energy Commission and Radiation Safety in Nuclear Weapons Testing, 1947-1974*, 170.
[50] Correspondence regarding the AEC's concern and role in Wigwam see NARACP, RAEC, Entry 67B1, Office of the Secretariat, Subject Files, 1951-1958, box 49, Medicine, Health, and Safety— monitoring.

scale oceanographic experiment. Indeed, Wigwam constituted the kind of big radiotracer experiment that Revelle had been advocating for since Crossroads. As one oceanographer who participated in Wigwam put it, "oceanographers recognized in this operation a rare opportunity for study of some of the mechanisms controlling the movement of waters on the high seas."[51] In November of 1954, Roger Revelle wrote a proposal requesting increased funding from the Office of Naval Research to support the pre- and post-test oceanographic work on Wigwam.[52] Eventually, Wigwam included forty-one Scripps scientists devoted to studying a range of phenomena from radiation distribution to wave generation to ecological studies of uptake.[53]

Paul Horrer, who directed the physical studies, for example, deployed a series of unmanned drogues (free-floating vessels attached with parachutes below the water surface to draw current a various depths) to chart the current patterns in the region, which he later compared to the wave generation post-blast [Figure 22]. Horrer also launched drogues at roughly one mile intervals after the blast and tracked along side them with a radiation probe to map the distribution of radioactivity.[54] After seven days of survey, Horrer determined that the horizontal spread of the radioactivity in the water increased from about five and a half square miles to seventy-nine square miles. Vertical movement

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[51] Theodore R. Folsom, July 1956, "Report to the Scientific Director: Mechanism and Extent of the Dispersion of Fission Products by Oceanographic Processes and Locating and Measuring Surface and underwater Radioactive Contamination," NTA, NV0008800, 13. See also Theodore R. Folsom and John D. Isaacs, May 1955, "Report to the Scientific Director: Mechanism and Extent of the Early Dispersion of Radioactive Products in Water," NTA, NV0008835.
[52] Scripps Institution of Oceanography, November 10, 1954, "Proposal for Expansion of ONR Contract," SIOA, John Dove Isaacs Papers, box 151, Wigwam 1953-1959.
[53] The number of Scripps scientists who participated in Wigwam can be found in W. J. O. J. L. Sperling B. Collins C. W. Lowery and S. K. Obermiller S.E. Weary, "Operation Wigwam," (Washington, D.C.: Defense Nuclear Agency, 1981), 1-10. See also page 3-5 for description of projects.
[54] Paul L. Horrer, "Physical Oceanography of the Test Area," *Limnology and Oceanography* 7, no. supplement (1962). See also the classified report: Paul L. Horrer, August 1956, "Report to the Scientific Director: Physical Oceanography of the Test Area," NTA, NV0011582.

proved a bit more vexing; measurements with the radiation probe along the length of line connecting the parachute to the drogue demonstrated significant mixing to the thermocline, but gave little indication of what was happening below that level.[55]

The biological work conducted as part of Wigwam was a mixture of laboratory and field methods. In the months leading up to the test, Donald Lear and Carl Oppenheimer performed controlled experiments at one of the Scripps labs to analyze radiation uptake among marine plankton and bacteria. One experiment, for example, showed that one particular bacteria species proved able to concentrate radioactivity from 6,000 to 25,000 times the activity in the surrounding water.[56] Curious about the potential repercussions of this bacterial concentration for higher-order organisms, Lear and Oppenheimer then set up an experiment to determine whether this radioactivity could be transferred up the food chain to certain copepods (small crustaceans).[57] The results were uncertain, but a later experiment exposing radioactive water collected after Wigwam confirmed that plankton were concentrating large amounts of radiation. This was significant, they argued, because "activity could in turn be transferred to the next trophic level and might eventually be incorporated into fish or other marine resources important to man."[58] This was something of an understatement, however. Field studies on uptake of fission products following Wigwam suggested rather clearly that plankton were

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[55] Paul L. Horrer, August 1956, "Report to the Scientific Director: Physical Oceanography of the Test Area," NTA, NV0011582, 20.

[56] Donald W. Lear and Carl H. Oppenheimer, "Biological Removal of Radioisotopes Sr^{90} and Y^{90} from Sea Water by Marine Microorganisms," *Limnology and Oceanography* 7, no. supplement (1962).

[57] Donald W. Lear and Carl H. Oppenheimer, "Consumption of Microorganisms by the Copepod *Tigriopus Californicus*," *Limnology and Oceanography* 7, no. supplement (1962).

[58] William H. Thomas, Jr. Donalds W. Lear, and Francis T. Haxo, "Uptake by Marine Dinoflagellate, *Gonyaulax Polyedera*, of Radioactivity Formed During an Underwater Nuclear Test," *Limnology and Oceanography* 7, no. supplement (1962).

concentrating radioactivity and passing it along the food chain to higher animals. While a separate survey that collected fish around the test site over the course of a week after the shot indicated that larger marine mammals exhibited little radiation exposure, the authors of the uptake studies noted a sound of caution for the potential long-term consequences of trophic transfer and ecological metabolism for humans. Such implications, they argued, pointed to the need for continual study:

> For extending our understanding of biogeochemical processes, the permutations upon the the composition of sea water produced by the detonation of nuclear devices offer great promise. First the introduction of minute amounts of easily measurable radioactive substances provides ready access to the paths of such substances from the hydrosphere to the organisms and subsequently from organism to organism. Single elements, as well as suites of elements, can readily be studied by well-established radio-chemical techniques for their isolation and quantitative analysis. Second, time studies on the retention of assimilated substances can be undertaken. The biological half-lives of elements in marine organisms, in conjunction with the respective concentration factors, are of paramount importance in considerations upon the long-term effects of nuclear tests.[59]

Such sentiment—for both the value of rad tools and the need for study of long-term radiological effects—was, of course, not unique to oceanographers. Lauren Donaldson has been voicing these needs since Operation Crossroads. Still, the oceanographic studies associated with Wigwam, coupled with the post-Bravo expeditions, further drove home the need to move beyond the atolls to consider the larger oceanic repercussions for testing. Wigwam, in this sense, proved far more than a naval exercise in undersea nuclear warfare; it was an experimental bomb test of unique importance to oceanographers that opened up new possibilities for research that helped

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[59] Leo Berner et al., "Field Studies of Uptake of Fission Products by Marine Organisms," *Limnology and Oceanography* 7, no. supplement (1962). Results of the fishing survey can be found in Bell M. Shimada, "Results of Long-Line Fishing," *Limnology and Oceanography* 7, no. supplement (1962).

promote more ecological ways of thinking about the health risks associated with nuclear fallout. Indeed, in the months after Wigwam, oceanographers' participation in the Atoms for Peace Conference and the National Academy of Sciences Biological Effects of Atomic Radiation (BEAR) Committees afforded them unmatched opportunities to more clearly articulate the need for more radiotracer research and assert themselves as authorities in nuclear issues pertaining to the oceans.

Atoms for Peace and the BEAR Committee on Oceanography and Fisheries

If Operation Wigwam marked a turning point in oceanographers' use of bomb test radiation, they wasted little time in pleading their case for an expansion of radiotracer practices in environmental research when they attended the Atoms for Peace Conference in Geneva the following summer.[60] Conceived by the Eisenhower Administration as a means to promote the "peaceful uses of atomic energy" in the wake of growing concerns about fallout, the Geneva Conference instead became a forum for scientists to highlight the many uncertainties regarding the behavior and human health implications of radiation in the environment. By pointing to the unknowns surrounding fallout exposures and the promise of radiation as a tool to investigate the oceans, oceanographers were making a forceful case for greater AEC support for basic research in the discipline. By and large, the strategy worked.

[60] For example, Odum, "Consideration of the Total Environment in Power Reactor Waste Disposal."; H. Wexler et al., "Atomic Energy and Meteorology," in *Proceedings of the International Conference on the Peaceful Uses of Atomic Energy, Volume 13, Legal, Administrative, Health and Safety Aspects of Large-Scale Use of Nuclear Energy* (New York: United Nations, 1956).

While the majority of the papers on oceanography delivered at Geneva were reprints or reworks of some of the critical Japanese research on the distribution and biological concentration of fallout radiation after Bravo, the contribution by Revelle and his colleagues at Scripps made the biggest impact. In "Nuclear Science and Oceanography"—written by Revelle, Ted Folsom, Edward Goldberg, and John Isaacs—the oceanographers emphasized the already productive uses of radiation for studying important oceanographic topics such as current transport, diffusion mechanisms, biological uptake, and organic productivity. Yet, such radiotracer research, they argued, had only been "timidly explored": "A more vigorous and imaginative application application of nuclear tools in the marine sciences," they wrote, "would certainly result in important breakthroughs."[61] They were not alone. Eugene Odum and Harry Wexler made similar points in their papers on radiotracer research in ecology and meteorology, respectively.

The Geneva Conference, however, proved merely an opening salvo in oceanographers' quest to secure more lasting patronage ties with the AEC and, as Jacob Hamblin has argued, "assert a place at the nuclear table."[62] A more comprehensive attempt at patronage and fuller expression of the ideas Revelle outlined during the Geneva Conference proved to be the BEAR Oceanography and Fisheries Committee. Assembled in 1955 at the bequest of the National Academies of Science (with help from the AEC), the BEAR Oceanography and Fisheries Committee was charged with assessing fallout effects in the seas as well as the more recent concern over the ocean disposal of

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[61] Revelle et al., "Nuclear Science and Oceanography," 371.
[62] Hamblin, *Poison in the Well: Radioactive Waste in the Oceans at the Dawn of the Nuclear Age*, 6.

radioactive waste. Revelle, as chairman of the oceanography and fisheries committee, however, transformed the the committee from merely an evaluative board on the radiological effects on the ocean to a tool for furthering the interests of professional oceanographers. Indeed, as some historians of science have noted, the final report issued by the committee was less a treatise on ocean fallout than a "road map" for patronage.[63]

The task of producing the report, however, proved anything but easy. As the group met in Princeton New Jersey over three days in early March of 1956, Richard Fleming (head of the University of Washington oceanography department) and Revelle engaged in a rather heated debate about how to define the problem and the purposes of the committee.[64] Revelle, for one, saw the committee as an opportunity to assert themselves as experts in matters of setting standards for radiation levels in the ocean, much as health physicists were doing for fallout exposures generally. Fleming, however, completely rejected the health physics approach all together. The problem, Fleming argued, was that health physicists and the AEC considered fallout and radioactive waste as disposal problems. For him, though, the irradiation of the oceans was not disposal but introduction. To understand the effect of radiation in the ocean, one must first understand the environment in which the radiation is being introduced. Fleming, in effect, was inverting the health physics paradigm:

> If we consider the problem as one of the environment, then we are concerned with the natural background [of radiation] and it doesn't matter where this material comes from—whether from military action, bomb tests or reactor waste or factory smoke or anything else...I just think that this is

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[63] Ibid; Rainger, "'A Wonderful Oceanographic Tool': The Atomic Bomb, Radioactivity and the Development of American Oceanography."

[64] See especially, Hamblin, *Poison in the Well: Radioactive Waste in the Oceans at the Dawn of the Nuclear Age*, 86-93.

> a good point of view for us to take; that is, to start with the environment and approach the problem rather than take the problem and then go back to the environment.[65]

Revelle, who was not particularly keen on biological oceanography in the first place, was obviously perplexed. As Jacob Hamblin has argued, Revelle saw fallout and nuclear waste as political problems, not necessarily environmental ones.[66]

Yet, Fleming's argument about the need to understand the oceanic environment as a necessary precursor to standard setting was borne out as the committee discussed the current state of knowledge regarding ocean fallout coming out of the oceanic expeditions and Wigwam. One of the problems with Operation Troll, Allyn Vine and Warren Wooster explained to the group, was that the method of water analysis was crude, which prevented the expedition from producing contours maps showing the distribution of radiation by level of activity. Furthermore, while there appeared to be uniform mixing in the upper layers of the ocean, the data provided few clues as to the mechanism, although plankton was highly suspected.[67] This was answered in part, Revelle noted, by Wigwam. Wind sheer on the surface of the ocean, he said, produced a kind of sloshing effect, which drove much of the mixing. Since neither Troll nor Wigwam gave any indication of mixing at the bottom of the ocean, the introduction of radioactivity at lower levels was almost completely unknown.[68] That problem would preoccupy the committee as the question of ocean dumping or introduction hinged on knowing the behavior of the seas at

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[65] "Transcript of Proceedings of the Study Group on Oceanography and Fisheries," March 3, 1956, Princeton, NJ, National Academies Archives (hereafter, NAA), Committees on the Biological Effects of Atomic Radiation, Oceanography and Fisheries (hereafter CBEAR-OaF), 32-3.

[66] Hamblin, *Poison in the Well: Radioactive Waste in the Oceans at the Dawn of the Nuclear Age*, 90-1.

[67] "Transcript of Proceedings of the Study Group on Oceanography and Fisheries," March 5, 1956, Princeton, NJ, NAA, CBEAR-OaF, 341-2.

[68] Ibid, 351-364.

great depth. Setting standards for radiation levels in the ocean depended on thorough knowledge of the environment, not simply of radiological effects. One thing was clear, however. Radiation contamination from weapons tests was not merely a potential problem for marine life at the test sites. Rather these tests implicated all life forms throughout the oceans.

Still, even as some of the committee members were reluctant to establish a radiation standard in the face of inadequate ocean data, most felt that the committee should at least advance a recommendation for how to go about finding one. Not surprisingly, the committee unanimously advocated for increased use of radiotracer methodologies, including further bomb tests. During the final session of the last day of meetings, Scripps scientist Harmon Craig put forth a proposal suggesting that nuclear bomb tests for purely oceanographic purposes be conducted under the aegis of the National Academy of Sciences. The plan, of course, was far-fetched, but that such a scheme should be discussed points to not only the oceanographers' political non-opposition to testing, but also their enthusiasm for new radiotracer practices.[69] For Craig and many of the other oceanographers, promoting nuclear weapons tests as scientific endeavors did not implicate them in furthering the arms race; they saw the military and scientific application of nuclear testing as quite disconnected events. "They are experiments," Craig argued. "It is only incidentally that the atomic bomb should be considered as a way of learning to make better weapons. It should be considered

[69] Craig's proposal can be found on "Transcript of Proceedings of the Study Group on Oceanography and Fisheries," NAA, CBEAR-OaF, 452-454.

primarily as an experiment of the earth."[70] Indeed, the bomb offered opportunities to completely transform oceanographic practice. As Fleming hinted at in one of his comments on the expansion of weapons tests, the bomb offered powerful opportunities for "hardening" their discipline along physical lines. "What is interesting to me here, and this the first time it has become very obvious, although people have mentioned it to me, before, is that this allows us the exten[d] the experimental technique to the oceans. This is the important thing that we are talking about here. This applies to the biological as well as the physical side here. That is the terms I think we ought to discuss most of these things."[71] Bomb tests, in other words, meant better and more authentic knowledge of the oceans, which in turn could be applied to the problem of waste disposal and long-term irradiation from continued weapons tests.

Not surprisingly, the final oceanography and fisheries report issued by the committee in August made no mention of Craig's NAS bomb test proposal. It did, however, make a forceful plea along the lines suggested by Fleming for an expansion of radiotracer practices in order to learn more about the ocean environment.[72] Later in 1957, the committee issued a longer report that compiled research reports from various members of the committee ranging from radiological effects on fish to current and marine ecological studies. The report also contained an introductory chapter from Revelle and Milner Schaefer. In it, Revelle and Schaefer concluded that "Tests of atomic weapons can be carried out over or in the sea in selected localities without serious loss to fisheries if the planning and execution of the tests are based on adequate knowledge of the

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[70] Ibid., 460.
[71] Ibid., 479.
[72] "Oceanography, Fisheries, and Atomic Radiation," *Science* 124, no. 3210 (1956).

biological regime. The same thing is true of experimental introductions of fission products into the sea for scientific and engineering purposes."[73] This, no doubt, pleased the AEC.

Yet, the chapter also acknowledged that radiological problems could not be simply contained to nuclear weapons test or disposal sites. "The world-girdling oceans cannot be separated in isolated parts. What happened at one point in the sea ultimately affects the waters everywhere."[74] While the oceanographers perceived little short-term harm from weapons tests or dumping, their report nonetheless recognized that the ocean comprised one great open system. Oceanographers may have been vigorous advocates of nuclear testing for scientific purposes, but by demonstrating the integration of oceans (in large part by tracing the radiation) they subtly critiqued prior assumptions by the AEC that nuclear weapons tests were discrete and bounded events. They may not have suspected it at the time, but in those realizations laid the seeds for the Limited Test Ban Treaty. As the boundaries of the Pacific test sites broke down, so to did public confidence in the AEC's ability to control offsite exposures similarly erode.

Conclusion: Oceanography to the Limited Test Ban Treaty and Beyond

In the end, the oceanographers never got the experimental bomb that they wanted. In the remaining years leading up to the Limited Test Ban Treaty in 1963, however,

[73] Roger Revelle and Milner B. Schafer, "General Considerations Concerning the Ocean as a Receptacle for Artificially Radioactive Materials," in *The Effects of Atomic Radiation on Oceanography and Fisheries*, ed. Committee on Effects of Atomic Radiation on Oceanography and Fisheries (Washington D.C.: National Academy of Sciences, 1957), 23.
[74] Ibid., 24.

oceanographic research became an essential scientific and monitoring component of the weapons tests. During Operation Redwing in 1956, for example, the Applied Fisheries Laboratory conducted two relatively large-scale marine surveys to measure the distribution and uptake of fallout radiation among plankton. The first, aboard the *U.S.S. Walton*, traversed roughly 70,000 square miles of ocean encircling the Pacific test sites. This survey was followed a few weeks later by a deep sea expedition (the *U.S.S. Marsh*) that sought to track the ocean fallout from the tests across a thousand miles to Guam. Both endeavors further confirmed the original findings in the *Shunkotsu Maru* expedition. Through more sophisticated radiochemical analysis, they also demonstrated that rather than accumulate radionuclides such as strontium-90, which was prevalent in terrestrial animals, marine biota in the oceans showed a preference for radioactive zinc, cobalt, and iron.[75] Later with the Pacific arm of the Hardtack series in 1958, AFL scientist Frank Lowman was able to track a specialized tracer added to the bomb tests (tungsten-185) in the ecology of Eniwetok.[76] Scripps oceanographers too participated extensively in these tests. During Redwing, John Isaacs headed a Scripps team that conducted a number of studies to investigate the oceanographic mechanisms that contributed to the intensity and extent of radioactive contamination.[77]

Yet, by the early 1960s, oceanographers were no longer as infatuated with nuclear bomb tests as they once had been. This turn of events had less to do with any

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[75] Hines, *Proving Ground: An Account of the Radiobiological Studies in the Pacific, 1946-1961*, 221-33. The different uptake of radioactive materials between terrestrial and marine animals can be found on page 233.

[76] Frank G. Lowman et al., "The Biological and Geographical Distribution of W^{185} in the Vicinity of the Eniwetok Test Site, April-September, 1958," Report # UWFL-57, January 8, 1959, NTA, NV0050020.

[77] Feenan D. Jennings et al., "Operation Redwing-Preliminary Report: Fallout Studies by Oceanographic Methods," November 1956, SIOA, John Dove Isaacs Papers, box 176, pacific bomb tests. An extracted version of this report dated February 6, 1961 can be found in NTA, NV0011643.

dissatisfaction with the politics of testing or radiation as tool in oceanographic practice. To the contrary, in the years since the oceanography and fisheries report, they continued to press for increased use of radiotracer methodologies. The difference in focus proved to be the problem of ocean dumping. Unlike nuclear fallout, the disposal of radioactive waste in the oceans was a uniquely oceanographic problem. As such, oceanographers used the issue to assert their expertise in dumping policy. Until the late 1950s, the disposal of radioactive waste was largely seen as a health physics problem. Yet after Bravo, oceanographers were able to make a stronger case that evaluating the introduction of radiation in the ocean was better suited to those who knew that environment well. Special knowledge of ocean processes, they argued, proved the controlling factor as to whether or how much to dump, not merely direct radiological effects (the scientific purview of health physics). In the main, such arguments worked; by the mid-1960s, oceanographers had achieved a prominent position in policy matters related to ocean dumping.

One of the primary vehicles that enabled them to wrest away power from the health physicists was the creation of the National Academy of Sciences Committee of Oceanography (NASCO) in 1957. An outgrowth of the BEAR oceanography and fisheries committee, NASCO quickly became the preeminent advisory body for issues pertaining to the ocean. Throughout the next few years, NASCO would issue a number of reports on radioactive waste disposal, each time treading further into risk assessment and policy recommendation. That such advice was taken quite seriously speaks to the growing scientific and political authority that oceanographers gained in the early 1960s.

Yet, another equally powerful actor in oceanographers' quest to secure their authority was the radiation itself. As a field science, oceanography, like ecology, was increasingly judged by the epistemological standards of laboratory science. By using radiation as a tool to trace out oceanographic processes, oceanographers were able to bring laboratory-style practice to the field. As Richard Fleming's comments during the BEAR conference make clear, radiation enabled oceanographers to experiment in nature in ways previously foreclosed to them. In the process, oceanography gained considerable epistemological currency, which they argued put their discipline on par with (or perhaps even exceed) laboratory-based disciplines like physics or, indeed, health physics.

It is no small wonder, then, that the presence of radioactivity in the environment did not evoke the kind of dread that animated environmentalist critiques of weapons testing or nuclear power development. As John Isaacs argued in one of the radioactive waste disposal reports, the "benefits of the use of atomic tools are so great they cannot be compromised by unreasonable restrictions in disposal, yet there must be enduring safety in sea disposal. These facts demand that the recommendations ensue from a realistic conservative approach."[78] Still, one cannot simply dismiss these oceanographers as technocrats or deny their influence on environmentalist thought. Their contribution to the growth of knowledge about the material connections between human bodies and the environment, coupled with their research demonstrating the spatial integration of hydrosphere, formed a critical footing on which environmentalist critiques were based.

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[78] John D. Isaacs, "Foreword," in *Disposal of Low-Level Radioactive Waste into Pacific Coastal Waters*, ed. Committee on Oceanography (Washington D.C.: National Academy of Sciences, 1962), x.

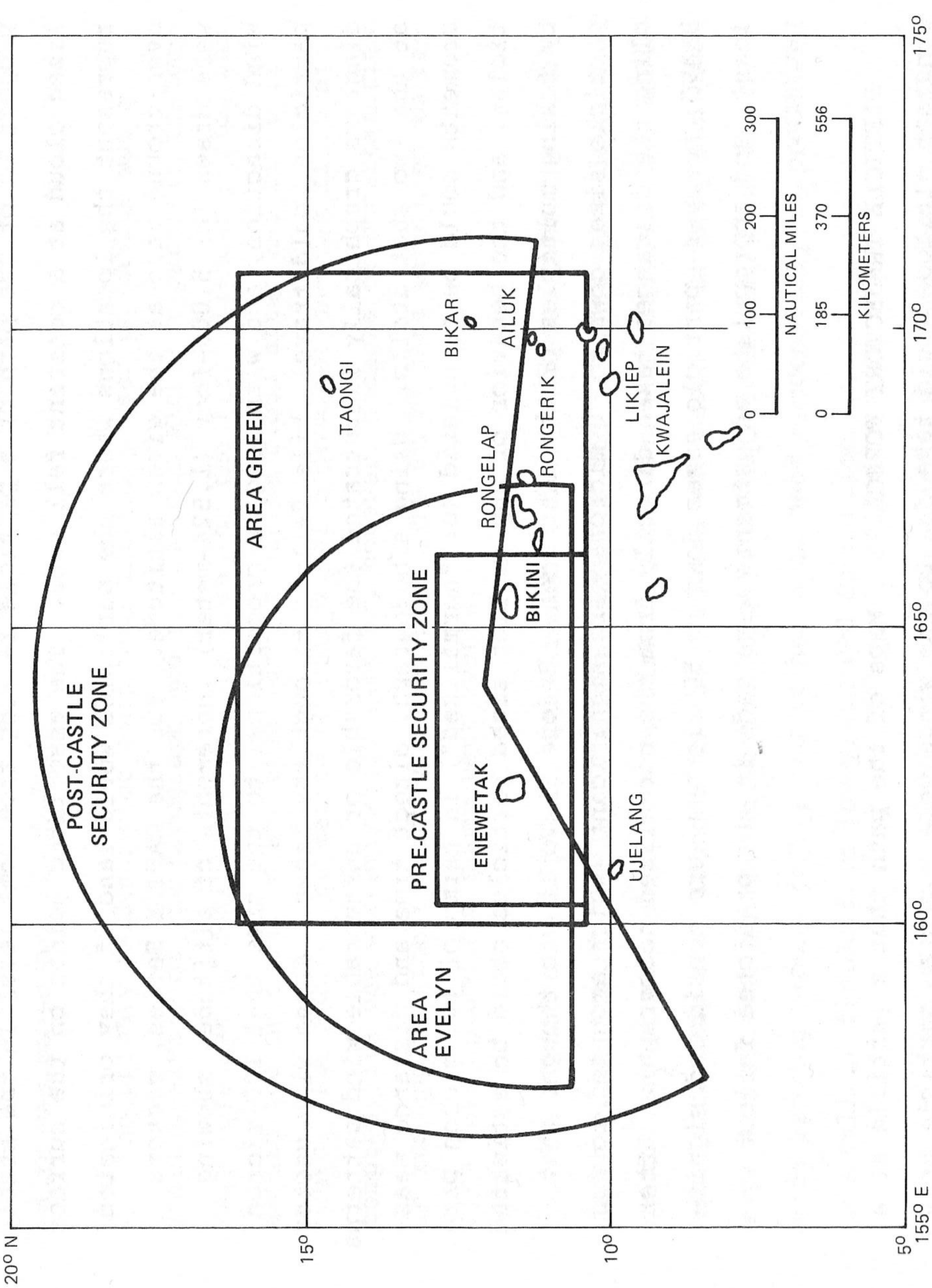

Figure 21 - Bravo "Danger Zone." Source: Edwin J. Martin and Richard H. Rowland, "Castle Series, 1954," April 1, 1982, Defense Nuclear Agency Report DNA6035F, 109. Public Domain.

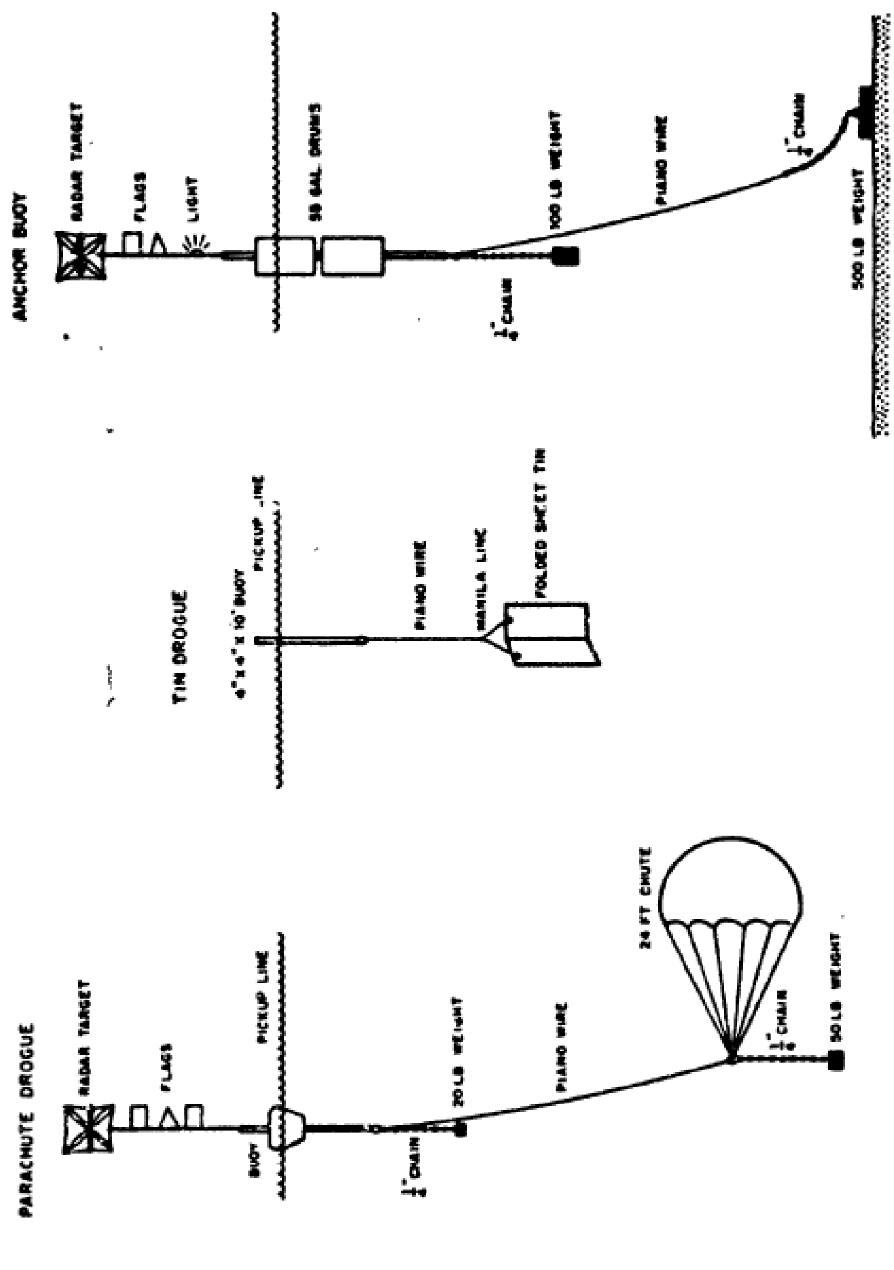

Figure 22 - Drogues. Source: Paul L. Horrer, August 1956, "Report to the Scientific Director: Physical Oceanography of the Test Area," NTA, NV0011582, 11.

BOUNDING THE ATMOSPHERE:

RESERVOIRS, HOT SPOTS, AND THE DYNAMIC ATMOSPHERE

On February 2, 1951, Merril Eisenbud, an industrial hygienist at the Atomic Energy Commission's (AEC) Health and Safety Laboratory (HASL) in New York State, received an urgent phone call from his colleague Henry Blair of the University of Rochester. The Eastman-Kodak Company in upstate New York, Blair informed Eisenbud, had just notified him that the company's film manufacturing plant had detected an abnormal rise in radiation in their air intake filters following a thunderstorm. The levels of radiation were not apparently elevated enough to warrant health concerns, but they did threaten the films being produced at the plant. Eisenbud immediately suspected the source of the radiation, even if the incident did catch him by surprise. Six days earlier, the AEC had inaugurated the recently established Nevada Test Site (NTS) with a series of nuclear weapons tests code-named Operation Ranger—the first on the North American continent since Trinity in July of 1945.

After getting off the phone with Blair, Eisenbud promptly called Thomas Shipman, the medical director charged with radiation safety at the NTS, to inform him of the incident. "You're crazy Merril," Eisenbud later recalled Shipman replying. "I was out to Ground Zero, and there's no radiation out there, and you're telling me it's up in Rochester."[1] When Eisenbud next contacted AEC headquarters in Washington, he

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[1] Eisenbud quoted in Stannard, *Radioactivity and Health: A History*, 963. The story is also recounted in Eisenbud, *Environmental Odyssey: People, Pollution, and Politics in the Life of a Practical Scientist*; Merril Eisenbud, "Monitoring Distant Fallout: The Role of the Atomic Energy Commission Health and Safety Laboratory During the Pacific Tests, with Special Attention to the Events Following Bravo," *Health Physics* 73, no. 1 (1997).

learned to his astonishment that the AEC had no program to monitor or track offsite fallout further than two-hundred miles from the NTS. Undeterred, over the next few days Eisenbud endeavored to uncover the extent of the contamination by employing HASL personnel and colleagues of his throughout the Midwest and East Coast to take local radiation measurements. By the end of February, Eisenbud had collected enough ground-level data to produce a map illustrating the general trajectory and pattern of the Ranger fallout as the radioactive clouds travelled eastward across the continent. The map, Eisenbud later wrote, "demonstrat[ed] that surprising amounts of fallout could occur over large areas thousands of miles from a relatively small air burst."[2]

The failure of the AEC to anticipate fallout beyond the boundaries of the NTS was a reflection and extension of technocratic assumptions about the nature of the atmosphere first evoked during safety planning for Trinity. As I argued in the second chapter, the MED's offsite safety criteria was based on the notion that if Trinity was detonated under ideal meteorological conditions (namely, in the absence of rain), the majority, if not all, of its radioactive debris would fall-out within the confines of the test site. Such was not the case. Serious concentrations of fallout, or "hot spots," were detected in inhabited areas thirty to fifty miles north of the Trinity site, prompting MED medical chief Stafford Warren to recommend to General Leslie Groves that any future tests be conducted at a site with "radius of at least 150 miles without population."[3] In other words, the notion that radioactive debris could be spatially contained was not found

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[2] Eisenbud, *Environmental Odyssey: People, Pollution, and Politics in the Life of a Practical Scientist*, 22.
[3] Quoted in Hacker, *The Dragon's Tail: Radiation Safety in the Manhattan Project, 1942-1946*, 108.

to be at fault. Rather, the MED had merely misjudged the scope of the testing area in need of control.

As it turned out, in the immediate postwar years the nuclear weapons testing program shifted to the Pacific, at sites so remote (once the native Marshall Islanders had been removed), that they more than fit Warren's prescription.[4] Yet, once the decision had been made to establish a continental proving ground in the face of growing tensions between the U.S. and U.S.S.R. in late 1950, the AEC was forced to confront public fears about radioactivity arising out of their own backyard. Not surprisingly, the AEC's assurances that the tests would not pose a harm to any persons downwind from the test site was predicated on the very kind of spatial reasoning that convinced them that fallout would be contained within the boundaries of the Nevada Test Site. As geographer Scott Kirsch has argued, containing public apprehension about fallout radioactivity was dependent upon the illusion of technical control over the spatial boundaries of the NTS.[5]

Such horizontal boundary making, however, relied equally on the insinuation of control on the vertical axis. If the bulk of the heavy particles in the atomic cloud were to fall-out within the NTS, it was assumed that the remaining radioactivity would be safely dispersed over long distances, suspended long enough to allow for radioactive decay, or simply diluted. Rendered passive and static, the atmosphere to the AEC functioned solely as an agent of diffusion, thereby limiting fallout hazards in the biosphere. This aspect of the AEC's safety discourse proved critical for allaying fears about fallout in the

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On the removal of the Marshall Islanders and the legacy of nuclear colonialism see Barker, *Bravo for the Marshallese: Regaining Control in a Post-Nuclear, Post-Colonial World*; Johnston and Barker, *Consequential Damages of Nuclear War: The Rongelap Report*; Weisgall, *Operation Crossroads: The Atomic Tests at Bikini Atoll*.

[5] Kirsch, *Proving Grounds: Project Plowshare and the Unrealized Dream of Nuclear Earthmoving*.

wake of thermonuclear testing in the mid-1950s. Unlike the "conventional" atomic bomb, thermonuclear tests produced exponentially higher explosive yields and were, therefore, exponentially "dirtier." Yet more importantly, because these tests injected radioactive debris high into the stratosphere, they revealed the possibility that fallout could circulate on a global scale and potentially impact the health of every human being on the planet. Nonetheless, despite the sinister implications of thermonuclear testing, the AEC maintained that the fallout dangers were minimal. The primary architect of this reassuring view was AEC Commissioner Willard Libby. According to Libby, the tropopause, the thin layer between the stratosphere and the troposphere, constituted a nearly impenetrable atmospheric boundary layer that reduced fallout risks by distributing radioactive particulate matter evenly throughout the stratosphere and preventing or, at least, moderating their downward transfer to the earth. The stratosphere, put another way, functioned as a natural atmospheric waste "reservoir," with the tropopause as its containment wall.

Libby was a hard-line conservative noted for his obdurate defense of the need for nuclear weapons development to counter the threat of Soviet Union. To be sure, his politics undoubtedly played an important role in his minimization of the radiation hazard, but he was also genuinely committed to studying the problem. His vertical "boundary work" is therefore best understood as a product of his adherence to what James Scott has described as the ideology of "high modernism." As a "muscle-bound... self confidence about scientific and technical progress," high modernism lends itself to a brand of technocratic thinking that tends to simplify complex social or environmental systems in order to establish control over problems arising from technological development.[6] Libby's bounded model of the upper atmosphere was thus a geographical expression of the high modernist imperative to make the unintended consequences of progress legible. It was, in essence, a simplified map (oriented vertically) that represented the atmosphere as a technologist would see it, not as the complex and interrelated whole that it is.

Libby would not go unchallenged. Beginning in the late-1950s, he faced opposition from meteorologists who charged that his model failed to conform to atmospheric realities. This movement, spearheaded by Weather Bureau scientist Lester Machta, centered their objections largely on newly-developed atmospheric circulation theories and firmly-grounded empirical data derived, ironically, from the use of radioactive fallout as a tracer to study stratospheric air motions. The model that Machta advanced to account for this new information demonstrated that stratospherically-injected radioactive debris was falling faster and more non-uniformly than Libby's model supposed. Although the controversy between Libby and Machta was initially played out behind closed doors, their testimony as part of Congressional hearings on the fallout hazard in the late 1950s pushed the stratospheric fallout question to the forefront of scientific and public debate about the risks of nuclear weapons testing. As Machta's model of vertical transboundary movement gained favor among AEC and independent scientists, it contributed significantly to the scientific rationale for ending above-ground testing in 1963.

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[6] James C. Scott, *Seeing Like a State: How Certain Schemes to Improve the Human Condition Have Failed* (New Haven: Yale University Press, 1998), 4.

This chapter connects the development of ideas about the ability of the atmosphere to dilute and diffuse radioactive debris and its effect on notions of spatial containment within the MED to safety practices in the postwar period by the AEC. I begin by exploring how horizontal spatial control of the boundaries of the NTS figured largely in the discourse and safety practices of AEC scientists in the early continental testing period. In particular, I show how the establishment of a national fallout monitoring network in the wake of the Ranger hot spot detected in upstate New York was motivated largely by concerns over litigation from the photographic film industry and military imperatives—not human health. I then move on the discuss how with the advent of thermonuclear testing, the AEC erected a new vertical boundary layer in the stratosphere to account for and assuage concerns about the massive amounts of radioactive being injected to the atmosphere by the H-bomb. I focus in particular on questions about the uniformity and storage time of stratospheric debris being played out in the debates between Libby, Machta, and others. In the final two sections, I examine how new practices and technologies for measuring the radioactive content and circulation patterns of stratospheric radioactive debris helped to close the controversy in favor of Machta and, returning to the question of hot spots at the NTS, show how the growing realization of the tranboundary movement of radiation (vertically and horizontally) helped contribute to the Limited Test Ban Treaty. In the conclusion, I discuss how new ideas about the atmosphere as a dynamic and spatially-connected realm, along with the development of new meteorological tools such as radiotracers, global data sets, and atmospheric circulation theories designed to settle questions of fallout contributed to the globalization of atmospheric science and profoundly shaped how scientists and lay persons have come to understand the global repercussions of the contamination of the atmosphere.

Bounding the Nevada Test Site

So far as the technical criteria went, the decision to site a continental proving ground in the southern Nevada desert was based largely on the region's relative sparseness of population and meteorological conditions.[7] In August, just months before President Truman gave the final order establishing the NTS, for example, meteorologists and nuclear experts including MED veterans Edward Teller and Enrico Fermi met at Los Alamos to evaluate the potential radiological hazardsto downwind offsite populations. The so-called "Reines Report" that emerged from these discussions confirmed the suitability of the site for weapons tests. A principal factor in this evaluation was aridity. Two meteorologists attending the meeting assured the group that if given the choice of a firing date, they could "predict within 99.9% accuracy that there would be no rainfall in the general vicinity of [ground] zero for a period of 10 hours following the shot." Moreover, they argued, the chance that a significant amount of fallout might reach as far as Salt Lake City was doubtful because "by the time the cloud reached that point it would have been so diffused by wind velocity and direction dispersion that the activity would be

[7] I do not mean to imply the decision to establish the proving ground in Nevada was based merely on technical criteria. Native Americans and the American West in general share a colonial history with the United States federal government. See, for example, Valerie Kuletz, *The Tainted Desert: Environmental and Social Ruin in the American West* (New York: Routledge, 1998). Southern Nevada was, of course, heavily populated—Las Vegas was roughly 70 miles to the southeast of the NTS. Yet again, meteorological factors seemed to exempt fallout on the city because the prevailing winds at the site blew from west to east.

negligible." The upshot of this analysis meant "the only places to worry about are those within a radius of 150 miles"—or, put another way, areas squarely within the boundaries of the test site [Figure 23]. Although some scientists suggested that nearby populations could conceivably exceed a safe dose of radiation, most judged that possibility "unduly pessimistic."[8] That populations eastward of Utah might be exposed to significant levels of fallout from Operation Ranger was not merely regarded as unlikely, it was not even considered. The AEC could not conceive of a situation where meteorological circumstances downwind from the test site might conspire to transport levels of fallout equal to or perhaps greater than those detected onsite.

No wonder, then, that NTS officials registered incredulity when Eisenbud relayed the news about the Rochester fallout. Incredulity, however, soon turned to action—but not for the apparent reasons that one might think. In the wake of the fallout incident, Eisenbud asked the AEC for to permission to begin initiating a continental fallout monitoring network beyond a two-hundred mile radius of the test site to which the Commission readily assented. The Commission's motive for the project, however, stemmed less from a concern over the potential health effects of fallout than from the prospect that contamination from further tests might prompt the photographic film industry to sue for damages. As Eisenbud later wrote, "the Eastman Kodak Company needed information to protect its processes and requested that the AEC provide them with advance information when a cloud from a weapons test was approaching one of their

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[8] Frederick Reines, September 1, 1950, "Discussion of Radiological Hazards Associated with A Continental Test Site for Atomic Bombs," Los Alamos report # LAMS-1173, Nuclear Testing Archive, Las Vegas, NV (hereafter NTA), accession # NV0030434. Fermi proved one of the more pessimistic scientists at the meeting and stressed the uncertainty of the fallout projections. He also refused to sign his name to the report.

manufacturing facilities."[9] Consequently, the AEC established the monitoring network "only because it was a scientific curiosity, not because fallout should be investigated for its public health implications."[10] For the Commission, then, the Rochester fallout represented merely an annoying legal blip in an otherwise carefully and spatially secured testing program.

The core of Eisenbud's monitoring program was the "gummed-film" network. Situated primarily at U.S. Weather Bureau sites across the nation, the gummed film stations included a three or four foot high stand attached with a square-foot adhesive film capable of fixing miniscule particles that happened to fallout upon it [Figures 24 and 25]. Weather Bureau employees collected these films monthly and shipped them to HASL for radiochemical analysis. The method, Eisenbud understood, had its drawbacks. For one, the films only collected radioactive beta particles, meaning that external gamma radiation levels had to be extrapolated from the beta counts.[11] For another, the collection efficiency of the papers were highly uncertain. Still, despite its weaknesses, the gummed-film network did enable HASL to produce radiation measurements that gave a reasonable, if largely qualitative, depiction of fallout deposition patterns and levels. Yet, the fact that the films only provided a sense of radiation levels more than a month after the initial fallout deposition reveals how little concern the AEC attached to offsite fallout, since effective countermeasures in the event of an episode of heavy fallout would have been useless at such a late date. After Ranger, HASL managed to set up forty-five

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[9] Merril Eisenbud, "The First Years of the Atomic Energy Commission New York Operations Office Health and Safety Laboratory," *Environment International* 20, no. 5 (1994): 567.

[10] Eisenbud, *Environmental Odyssey: People, Pollution, and Politics in the Life of a Practical Scientist*, 67.

[11] The absence of alpha particles can be explained by the fact that they were far too heavy to have been transported over long distances.

stations; by 1953, there were one-hundred and twenty stations, some of which were located outside the United States [Figure 25].[12]

In addition to the gummed-film network, the AEC contracted the U.S. Weather Bureau's Special Projects Section to begin a program to track and analyze the trajectory of the atomic clouds as they traversed the continent. The Weather Bureau had been involved with nuclear weapons development since the MED. Harry Wexler, who headed the WB's Scientific Services Division, had been commissioned by the MED to analyze pressure waves produced by Trinity.[13] The relationship between the Bureau and atomic weapons development intensified in the early postwar period as the military establishment, ever anxious about losing its atomic monopoly, sought meteorological assistance on the feasibility of detecting and tracking fission-produced radiation in hopes of spying in on nuclear developments in the Soviet Union. In 1948, Wexler established the Special Projects Section under the direction of recent Bureau addition Lester Machta specifically to aid the Air Force Office of Atomic Energy (AFOAT-1) in using meteorological data to track atomic clouds.[14] The AFOAT-1 project, known as Operation Fitzwilliam, began with the Sandstone tests at the Pacific test site in 1948.[15] During the testing series, the Bureau compared the nuclear cloud movements from air samples collected by unmanned drones with associated wind conditions to better ascertain the

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[12] Eisenbud, *Environmental Odyssey: People, Pollution, and Politics in the Life of a Practical Scientist*; Merril Eisenbud and John H. Harley, "Radioactive Dust from Nuclear Detonations," *Science* 117, no. 3033 (1953): 71.

[13] For more on Wexler, see Fleming, "Polar and Global Meteorology in the Life of Harry Wexler, 1933-62."

[14] For more on Machta's biography, see John Miller, Jim Angell, and Dian Seidel, "Lester Machta," *Physics Today* 55, no. 6 (2002).

[15] See Charles A. Ziegler and David Jacobson, *Spying without Spies: Origins of America's Secret Nuclear Surveillance System* (Westport, CT: Praeger Publishers, 1995).

mechanisms of the ended the American atomic monopoly, the news, if anything, doubled the military and AEC's resolve to establish a permanent detection system to monitor further atomic energy developments in the U.S.S.R. Thus, the cloud tracking system established during and after Operation Ranger did not arise out of any serious reservations regarding potential offsite exposures, but rather because the detonations provided an opportunity to further develop aerial detection technologies needed for a permanent Atomic Energy Detection System (AEDS).[16]

The cloud tracking system involved two basic phases. To begin with, the Air Force provided three manned WB-29 aircraft fitted with paper air filters to fly parallel to the clouds for a distance of up to 600 miles.[17] Once these flights determined the initial trajectory of the clouds, the Weather Bureau then set about mapping the general direction of cloud movement over the rest of the nation [Figure 26]. Their analysis was only partly determined by direct sampling. By and large, the trajectories were predictive as they were based upon prevailing meteorological forecasting practices such as observed wind data and standard six-hour constant-pressure charts.[18] For the eastern quarter of the nation, the Air Force also conducted flights along the 80th and 95th meridians to provide direct observation of radiation intensities and movements, but only on a relative basis.[19]

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[16] On the dual nature of the cloud sampling program see Taylor, "History of Air Force Atomic Cloud Sampling." On the development of the Atomic Energy Detection System see Ibid. Like the gummed-film stations, this system was also used to alert the photographic film industry of fallout levels that might harm their film production. See Atomic Energy Commission, Thirteenth Semiannual Report, January 1953, 100-12; n.a., "Chief Special Projects Section: Lester Machta," *Weather Bureau Topics* 17, no. 1 (1958).
[17] Taylor, "History of Air Force Atomic Cloud Sampling." The 600 mile radius figure comes comes from Atomic Energy Commission, Thirteenth Semiannual Report, January 1953, 104.
[18] L. Machta et al., "Airborne Measurements of Atomic Debris," *Journal of Meteorology* 14, no. 2 (1957): 165.
[19] Ibid.

Despite their origins in military imperatives, the cloud sampling and aerial trajectory analysis proved quite valuable for the fallout monitoring system and meteorological study in general. The cloud trajectories, as Lester Machta, explained in an article in the open literature of the period, held advantages over the gummed-film network because they "provide[d] a continuous profile over a long distance."[20] While HASL's gummed film network enabled the AEC to qualitatively determine the presence of fallout at the ground level, the maps produced by Machta and the Weather Bureau produced concrete pictures of the horizontal and vertical distribution of the radioactive cloud while in the atmosphere through time. Therein lay the interest that professional meteorologists like Machta had in the seemingly applied character of the tracking and monitoring programs. By sampling weapons testing radioactivity as it moved through the atmosphere and correlating cloud movement to weather phenomena, meteorologists were gaining a fundamental understanding of tropospheric-level atmospheric mechanisms. These early efforts at cloud tracking represented some of the first radioactive tracers experiments to study large-scale atmospheric phenomena.[21]

Fundamental meteorological studies of this sort would soon be in greater need as further incidents of hot spots were detected in the coming years. In 1953, for example, fallout from a NTS detonation rained once again on upstate New York. And similar to the Rochester incident, it was not the AEC that discovered the "exceptionally high"

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[20] Ibid.

[21] R.J. List, "On the Transport of Atomic Debris in the Atmosphere," *Bulletin of the American Meteorological Society* 35, no. 7 (1954); Machta et al., "Airborne Measurements of Atomic Debris."; Lester Machta, "Meteorological Benefits from Atmospheric Nuclear Tests," *Health Physics* 82, no. 5 (2002). Lester Machta, "The Use of Radioactive Tracers in Meteorology," *Annals of the International Geophysical Year* 5, no. part 5 (1958).

fallout, but an independent scientist at Rensselaer Polytechnic Institute in Troy.[22] Subsequent radiological surveys by the AEC and scientists at Rensselaer concluded that the radiation did not exceed permissible levels, but the incident exposed flaws in the Commission's continental monitoring system and the assumptions on which it was based. As Weather Bureau meteorologist Robert J. List noted in his analysis of the incident, "Although there are six monitoring stations within 150 miles of Albany, the fallout intensity would have underestimated by about three orders of magnitude in this area had there been no station at Albany."[23] But more importantly, he continued, "Because of the many unknowns involved, initial concentration, particle-sized distribution, terminal velocities, scavenging efficiency, eddy diffusion, wind shear, etc., it is impossible to make valid qualitative estimates of the maximum fallout or rainout likely to occur from continental tests."[24] The complex atmospheric mechanisms that contributed to these hot spots presented a direct challenge to the AEC's assumptions about the passivity and dilution capacity of the atmosphere and, by extension, their ability to control the boundaries of the NTS. Nevertheless, the Commission continued to maintain, publically at least, that the NTS detonations produced little, if any, offsite fallout. "High air bursts at the Nevada Test Site," one well-publicized AEC report assured Americans, "have produced no significant fallout; heavy fallout from near-surface explosions has extended only a few miles from the point of burst. *The hazard has been successfully confined to*

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[22] Herbert M. Clark, "The Occurrence of an Unusually High-Level Radioactive Rainout in the Area of Troy, N. Y," *Science* 119, no. 3097 (1954).
[23] Robert J. List, June 25, 1954, "The Transport of Atomic Debris from Operation Upshot-Knothole," Report NYOO-4602 (Deleted), NTA, NV0011554, 63.
[24] Ibid., 65.

the controlled area of the Test Site."[25] In spite of the insufficient geographical coverage of the offsite monitoring system and the meteorological complexities cataloged by List above, the Commission clung to its over-simplified spatialization of the NTS.

Tellingly, in the same report in which the Commission had doubled down on its rhetoric of spatial control at the NTS, they had also begun erecting a new boundary to account for a new fallout threat. In late 1952, the AEC ushered in the thermonuclear age when they detonated the world's first H-bomb at Eniwetok Atoll in the Pacific. And with the advent of thermonuclear testing came the specter of global fallout.

Bounding the Stratosphere

The introduction of thermonuclear testing in late fall of 1952 presented officials within the AEC with new problems for offsite radiation safety. As I argued in chapter 4, differences in the scale of the explosive yield of the H-bomb at the Pacific test sites compared to the more "conventional" atomic tests generated the most concern, initially at least; Ivy-Mike (the first H-bomb test) was approximately 500 times more powerful than Trinity. Aside from the problem of magnitude, however, thermonuclear testing also raised new questions about the nature of fallout risks. Prior AEC concern about fallout had been limited to reducing human exposure to short-lived gamma radiation, resulting in a dramatic underestimation of the potential health effects of longer-lived internal emitting radionuclides. The greater fallout produced by thermonuclear weapons tests, however, led AEC scientists to reconsider this initial risk calculus as they began to investigate the

[25] Atomic Energy Commission, February 1955, "Effects of High-Yield Nuclear Explosions," NTA, NV0049176, 7.

possible effects that the gradual build-up of radionuclides such as strontium-90 in the environment might have for human health.

Yet, the turn toward strontium-90 effects marked by the Rand Conference in 1953 centered not only on the biospheric aspects of the problem, but also on the geophysical mechanisms of environmental transport and diffusion of radioactive debris. To understand the biological impact of environmental radioactivity on human health the AEC first needed to understand where the fallout was going and how it got there. Again, the increased scale of the thermonuclear yields proved critical, not simply in terms of greater fallout production, but rather with regard to the vertical reach of the mushroom cloud; unlike fission tests at the NTS, which by and large remained within the troposphere, the caps of the H-bomb mushroom clouds penetrated high into the stratosphere. At the time, very little was known about the physical properties and circulation mechanisms of the stratosphere, largely because it was physically out of the reach of the science at the time. Most airplanes were incapable of penetrating the stratosphere in 1953 and balloon technologies had only recently improved to the point where they could feasibly carry the necessary scientific gear to such altitudes. Thus at the time of the Rand conference, not much was known about the stratosphere other than it appeared to be a relatively static atmospheric layer absent of the kinds of dynamics that produced the weather phenomena we experience in the troposphere.

The pressing need for stratospheric knowledge was compounded by the fact that, following the Mike test, the AEC had almost no knowledge of where its radioactive debris fell. Although HASL and the Weather Bureau installed gummed-film stations throughout the Pacific, the apparatuses detected only a fraction of the immediate fallout because sufficient coverage was hampered by the lack of suitable land-based sites in the vast Pacific waters.[26] Given the paucity of detected fallout, the Rand scientists speculated on two possibilities for Mike's radioactive debris: either the majority of it fell-out in the ocean in areas not covered by the gummed-film, or it remained suspended in the stratosphere yet to fall back to earth.[27]

The AEC and Willard Libby, in particular, pinned their hopes on the latter possibility. During the meeting, Libby introduced the idea that the stratosphere acted as a kind of natural vertical "reservoir" containing strontium-90 debris. This reservoir, Libby suggested, might potentially contain strontium-90 for upwards of a decade, which would allow for considerable radioactive decay to occur and thus "reduce greatly the probability that such materials become incorporated in the biochemistry of living organisms, including man."[28] There were other added advantages should this be the case, he noted. The longer hold-up time would likely allow the debris to become evenly mixed throughout the layer so as to "smooth out the distribution somewhat" thereby reducing the possibility of regional concentrations.[29] In effect, Libby was proposing that the tropopause (the layer between the stratosphere and the troposphere) functioned as a stratospheric containment vessel that ameliorated the potential effect that these massive tests could potentially wreak on the biosphere.

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[26] Eisenbud, *Environmental Odyssey: People, Pollution, and Politics in the Life of a Practical Scientist*, 79.
[27] The RAND Corporation, August 6, 1953, "Worldwide Effects of Atomic Weapons: Project Sunshine," 31.
[28] Ibid., 26.
[29] Ibid., 5.

As I noted above, however, there were profound uncertainties about the properties of the stratosphere. As the authors of the report summarizing the Rand conference acknowledged, even at the lower levels of the atmosphere there was "very little knowledge as to how the troposphere is cleaned of debris."[30] If little was known about the behavior of radioactive debris in the troposphere, there was almost no data that would suggest a stratospheric reservoir. The best evidence rested on observations of the Krakatoa volcanic eruption in 1883, where volcanic ash from the explosion produced spectacular sunsets throughout the world for years afterwards.[31] The Krakatoa example was, nonetheless, little better than anecdotal evidence. What was needed to better understand the character and magnitude of the strontium-90 hazard, the scientists at the Rand conference concluded, was a comprehensive sampling program of strontium-90 in the atmosphere and biosphere in order to "determine the mechanisms whereby undesirably high amounts of radioactive materials may be concentrated in man or products essential for his existence."[32] In that way, Libby hoped to create a worldwide project to "assay" the hazard and create an "equation that shows that everything checks."[33] Sunshine was thus a geophysical as much as a biological project. Yet, as Libby would learn over the next couple years, reducing the complexities of the global environment to a formula would prove difficult—complicated in no small way, as we will see, by the technical complexities involved in directly measuring the content and movement of radioactive materials in the stratosphere.

[30] Ibid., 49.
[31] Ibid., 33.
[32] Ibid., 47.
[33] See chapter 4.

Project Sunshine and Libby's theory of stratospheric hold-up remained a secret until 1956, even if the radiostrontium problem had not. As I have explained in previous chapters, questions concerning the effect of radiostrontium among the public were first raised following the AEC's release of the Bravo report in February of 1955, nearly a year after actual test. While the realization that fallout nuclides like strontium-90 were becoming incorporated into critical food supplies increasingly preoccupied Americans in the years to come, by far the most alarming response elicited by the publication of the Bravo report centered on the massive zones of lethal and nearly-lethal radioactive contamination that the explosion wrought throughout the Pacific. Roughly 7,000 square miles of land and ocean downwind from the test, the AEC admitted in the report, were so contaminated that anyone who stood in its path would have received a lethal dose of radiation if they had not immediately taken shelter. The sheer scale of this contamination was not lost on reporters who immediately began drawing spatial comparisons should a similar bomb be detonated over an American city. In its coverage of the report, the *New York Times*, for example, drew up a map demonstrating how a Bravo-type H-bomb dropped on Washington D.C. would likely result in a hundred-percent lethality rate from the Capital to Philadelphia.[34] It was a powerful message that immediately called into question the Commission's ability to control such an awesome device.

These comparisons, however, dealt solely with "local" fallout in the event of a nuclear attack on a city. Questions still remained, and would increase in intensity over the next few years, regarding the long-range effects that continued H-bomb testing would

[34] Hanson W. Baldwin, "H-Bomb Fall-out Poses New Defense Problems," *New York Times* February 20, 1955.

have on the strontium-90 problem. What was the fate and potential health effects of all the radioactive debris that did not fall-out locally? Such questions, in part, prompted Libby to reveal the existence of Project Sunshine in 1956, first in a speech to Northwestern University and later in the April 20th issue of *Science*.[35] Although both the speech and article were light on supporting data, Libby maintained that the Sunshine project demonstrated that fallout from thermonuclear weapons tests was essentially innocuous owing to the slow rate of stratospheric strontium-90 fallout—"corresponding to an average time in the stratosphere of about 10 years."[36]

Libby may have had other motivations for the timing of the releases. In late 1955, the National Academy of Sciences announced that the prestigious scientific organization would be forming a series of committees to independently investigate fallout effects on human health. Libby's speech and article, emerging just as the Committees were conducting their initial organizational meetings, appears to have been a calculated ploy to sway the tenor of their deliberations—specifically, the meteorological committee. Headed by Harry Wexler with Lester Machta as the rapporteur, the Meteorology Committee was charged with evaluating the possible effects of nuclear detonations on weather and providing an independent assessment of the meteorological aspects of nuclear fallout. From the beginning, however, the release of Libby's conclusions placed the Committee in an awkward position. Instead of culling together data and evaluating evidence on its own merit, the Committee found itself having to constantly contend with

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[35] For the transcript of Libby's speech, see Remarks Prepared by Dr. Willard, F. Libby, "The Radioactive Fallout and Radioactive Strontium, "January 19, 1956, NARACP, RAEC, Entry number 67B1, Box 26, folder Information and Publications: Speeches.
[36] W. F. Libby, "Radioactive Strontium Fallout," *Proceedings of the National Academy of Sciences of the United States of America* 42, no. 6 (1956): 658.

Libby's analysis, which was fast becoming the official AEC position on the question of stratospheric fallout.[37] Libby, a non-trained meteorologist, had shrewdly scooped the Meteorology Committee.

Libby was not finished. In April of 1956, as Machta was finishing the first draft of the Committee's report, Libby notified Harry Wexler that he wished to meet with the committee members during the upcoming American Meteorological Society meeting in May. The object of the meeting, Wexler informed the members, was to provide Libby with a forum to discuss his recent data concerning "the stratospheric storage of radioactive debris and its influence on the problem of radiostrontium fallout."[38] Libby, having evidently learned that the draft Machta was preparing took a rather unfavorable view of his stratospheric model, hoped to convince the group of the rightness of his model. No record or account of what Libby said at the meeting exists, but on May 2nd, in an another attempt to head-off the Committee, he submitted a paper detailing his model (this time with hard data) to the NAS's lead journal *Proceedings of the National Academy of Science*. In the paper, Libby based his estimates of the fallout rate and pattern of deposition on a comparison of the estimated stratospheric content of strontium-90 with the observed levels of deposition recorded by the gummed-film and other surface-based monitoring systems. (At the time, the Commission was still unable to conduct direct measurements of the stratosphere). From that data, Libby reasoned that the rate of fall

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[37] Libby forwarded a draft of his article to NAS President Detlev Bronk in hopes that it would be forwarded along to the Committees. See, Detlev W. Bronk to W.F. Libby, March 2, 1956, National Academies Archives (hereafter NAA), Committees on the Biological Effects of Atomic Radiation, Cooperation with Other Organizations, folder Atomic Energy Commission 1956.

[38] Harry Wexler, Chairman to Members of the National Academy of Science Study Group on Meteorological Aspects of the Effects of Atomic Radiation, April 19, 1956, NAA, Committees on Biological Effects of Atomic Radiation, Meteorological, (hereafter CBEAR-MET) in folder Drafts April 1956.

for stratospheric strontium-90 was roughly ten percent per year corresponding to a storage time of 10 +/- 5 years. Moreover, because of its long residence in the stratosphere, strontium-90 fallout patterns were "nearly uniform over the world" owing to latitudinal stratospheric mixing.[39] The effect of Libby's proposed model for the assessment of fallout risks was clear—the diffusion of strontium-90 throughout the stratosphere coupled with the limited vertical transportation of debris from the stratosphere to the troposphere reduced the amount of the radionuclide available for incorporation in human and non-human tissues.

On May 19th, despite the circulation of Libby's paper throughout the NAS, Machta forwarded another draft of the report to the Committee members which elaborated more fully on the weaknesses of Libby's model. In the main, Machta's criticism pivoted on two points. The first was that Libby's fallout rate relied too extensively on ground-level monitoring data. Recent data suggested that the low efficiency of gummed-filmed collection may have been underestimating the amount of fallout deposition by as much as seventy five percent.[40] Yet more importantly, Machta objected to Libby's failure to address the mechanisms of stratospheric removal. Although any consideration of stratospheric removal mechanisms were at the time speculation, Machta argued that these uncertainties were cause for a more conservative treatment of the storage time issue. With this in mind, Machta concluded in the May draft that the "most likely value for the mean storage time in the stratosphere is very roughly 5 years, with an upper bound of 10 years." In other words, in the absence of hard data confirming

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[39] Libby, "Radioactive Strontium Fallout." Quote on page 380.

[40] Lester Machta, May 28, 1956, "Preliminary Report of the Study Group on Meteorological Aspects of the Effects of Atomic Radiation," NAA, CBEAR-MET, folder Drafts May 1956, 24.

the relative boundedness of the stratosphere, the Meteorological Committee would be better served by exercising a precautionary approach to the crucial issue.

Although Libby and Machta engaged in a number of phone conversations and correspondence in an attempt to resolve the differences between the two reports, they failed to reach any consensus or compromise.[41] In the cover letter Machta enclosed with the May draft he sent to the Committee members, he noted that Libby "has taken exception to the reduction in the mean stratospheric storage time from 10 to 5 years, but we were unable during our discussions to argue the merits of this change."[42] While Libby and the Committee were at a deadlock, other events higher up in the echelon of the NAS administration intervened to settle the issue in favor of Libby. On May 21st, at a meeting including the President of the NAS, Detlev Bronk, and the chairman of the various BEAR committees, it was decided not to "go into great detail concerning the weapons effects of fallout."[43] As a result, in the final BEAR report made public on June 12, 1956, the Meteorological Committee pulled its criticism of Libby's stratospheric model, leaving his *Proceedings* paper as the definitive statement on strontium-90 stratospheric fallout. In its place, the Committee chose instead to highlight the uncertainties of estimating fallout rates and advocated for "a continuing program to investigate this phenomenon, including actual measurements of the radioactivity in the

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[41] Examples of this correspondence can be found in NARACP, RAEC, Entry # UD-UP 13, Office Files of Willard P. Libby, Box 1, folder Reading File, January-May 1956.
[42] Lester Machta, Rapporteur to Members of the National Academy of Science Study Group on Meteorological Aspects of the Effects of Atomic Radiation, May 28, 1956, NAA,CBEAR-MET, in folder Drafts May 1956.
[43] Ibid.

stratosphere."[44] Thus, despite the original intention of the committee to provide an independent assessment of stratospheric fallout, by failing to provide any concrete interpretations of the problem or present a countervailing analysis to Libby they merely served to strengthen his position as an authoritative voice on the matter.

Not to be outdone, a bristling Machta wrote Detlev Bronk a week after the publication of the report requesting that the NAS quickly follow-up with another cross-disciplinary study focusing exclusively on the strontium-90 problem. The urgent need for such a study, Machta wrote, stemmed from the Meteorology Committee's "serious reservations concerning the validity of certain of the measurements and interpretations which Dr. Libby has made." In particular, Machta cited Libby's residence time figure, which, even at the lower end, "may be too high."[45] Bronk agreed and later in the year established a meteorological working group to continue studying the fallout problem.[46]

In the meantime, Machta decided to go public with his criticism of Libby's model. In November, Machta delivered a paper to the Washington Academy of Sciences which in essence elaborated on the critique of Libby's stratospheric model that had been omitted in the Meteorological Committee's final report. There was one notable difference, however. Whereas the May draft had confined its criticism to Libby's estimation of stratospheric residence time, in this paper Machta raised serious doubt about the model's assertion of uniform global deposition. Fallout levels in New York City during 1955, he explained, were highest during a period of three or four months, rather than gradual as

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[44] National Academy of Sciences and National and National Research Council, "The Biological Effects of Atomic Radiation: Summary Reports," (Washington D.C.: National Academy of Sciences, 1956), 60.
[45] Lester Machta to Detlev W. Bronk, June 21, 1956, NTA NV0100790.
[46] On reformation of BEAR see Charles I. Campbell to Harry Wexler, January 31, 1957, NAA, CBEAR-MET, folder Meteorology General 1957.

assumed in Libby's model. While Machta acknowledged that "At the moment, there is no alternative to Libby's analysis," he suggested tactfully that the problems with the model outlined in the paper merited "changes in details."[47]

Privately, however, Machta was not merely trying to reconcile Libby's model to the available data, but was instead developing his own competing model to account for the irregularities in fallout deposition patterns detected in the gummed-film samples. Throughout the remainder of 1956 and into the following year, Machta and Libby kept in close written contact as each scientist forwarded new data and analysis of the stratospheric strontium problem. By spring, however, it was clear that the two were at a crossroads. In a letter Libby sent off to Machta in early April, Libby exasperatedly implored him to "give the model a good chance." In his own mind, Libby was doing the right thing in the absence of compelling data from the stratosphere itself. "Now what to do?" Libby pleaded, "Make the most of what data you have and fit together a working picture which tells the truth as best known on the whole, or must we say we know nothing about fallout. The latter most definitely is not correct, and the former may not be correct, but it certainly is preferable."[48] In the course of their correspondence, however, Machta never expressed or implied that they should say or do nothing to understand the problem. To the contrary, it is plain from Machta's letters that he felt Libby's model was so flawed that it demanded abandonment. When it became increasingly clear that Libby would not capitulate, Machta endeavored to bypass Libby completely and offer his own

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[47] Lester Machta, "Meteorological Factors Affecting Spread of Radioactivity from Nuclear Bombs," *Journal of the Washington Academy of Sciences* 47, no. 6 (1957): 177.
[48] Libby to Machta, April 8, 1957, NARACP, RAEC, Entry# UD-UP 13, Office Files of Willard P. Libby, Box 1, folder Reading File December 1956 to June 1957.

stratospheric model—one that would account for the complexities of the stratosphere, especially in regard to the notion of the tropopause as a nearly impenetrable global vertical boundary layer.

Machta's resolve to proceed with an alternative model seems to have been motivated by the recent decision of the Joint Committee on Atomic Energy (JCAE), the AEC's Congressional oversight committee, to hold a series of hearings on the fallout problem in late May and early June. There was sure to be much at stake in the hearings. With the growing distrust of the AEC and the failure of the BEAR committees to provide any firm conclusions as to the nature of the fallout hazard, the JCAE, as Robert Divine has written, was stepping in "to try to compel the scientists to come forth with a satisfactory explanation of the radiation problem."[49] Machta, in hopes of avoiding a situation similar to the BEAR Committee's failure to provide the public with an alternative to Libby's model, presented his own to the JCAE.

Machta's testimony began, innocuously enough, by describing tropospheric fallout patterns from tests at the NTS, of which he and Libby had disagreed little.[50] As Machta turned to his model describing the mechanisms of stratospheric fallout, their differences were apparent. Machta based his model, he told the Committee, on British meteorologists Alan Brewer and Gordon Dobson's stratospheric circulation theory. The Brewer-Dobson theory, which the two developed to account for the observed differences in ozone and water vapor distribution in the stratosphere, respectively, posited a slow

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[49] Divine, *Blowing on the Wind: The Nuclear Test Ban Debate, 1954-1960*, 129. For more on BEAR and the politics of fallout risk, see Toshihiro Higuchi, "Radioactive Fallout, the Politics of Risk, and the Making of a Global Environmental Crisis, 1954-1963" (PhD Dissertation, Georgetown University, 2011).
[50] U.S. Joint Congressional Committee on Atomic Energy, *The Nature of Radioactive Fallout and Its Effects on Man*, 85th Congress, 1st session, 1957, 141-61.

poleward drift of stratospheric air.[51] Thus rather than uniformly mix throughout the stratosphere, air masses tend to be directional and concentrate (depending on the season) in northern and southern temperate latitudes. What's more, he explained, there occur significant breaks in the tropopause in these same latitudes. Although the tropopause in equatorial regions is uniformly intense, at 30 and 60 degrees north and south latitude it is distinctly less impermeable as it encounters the jet stream. The poleward stratospheric drift of air, he concluded, results in greater atmospheric accumulation of air mass in temperate latitudes at points where a preferential exchange of air between the stratosphere and troposphere occurs, especially in the spring months. In other words, the Brewer-Dobson theory was based on a dynamic understanding of the atmosphere where uniformity or homogeneity of structure would have been the exception, not the norm [Figure 27].

That was theory, but how did it fare when compared to the empirical results gained from the fallout patterns detected in the ground-level monitoring network? In a word, perfectly. Samples provided by the gummed-film network and the recently established soil sampling and rain-water pot systems clearly demonstrated a strong preference for fallout deposit in the the temperate latitudes; so much so, in fact, that two-

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[51] A.W. Brewer, "Evidence for a World Circulation Provided by the Measurements of Helium and Water Vapour Distribution in the Stratosphere," *Quarterly Journal of the Royal Meteorological Society* 75, no. 326 (1949); G.M.B. Dobson, "Origin and Distribution of the Polyatomic Particles in the Atmosphere," *Proceedings of the Royal Society of London. Series A, Mathematical and Physical* 236, no. 1205 (1956). It is likely that Machta was alerted to the significance of the Brewer-Dobson theory after reading British reports on the detection of strontium-90 in England. Reprints of these reports can be found in "Environmental Contamination from Weapon Tests," (Health and Safety Laboratory, Atomic Energy Commission, 1958); *The Nature of Radioactive Fallout and Its Effects on Man*. Although I have not been able to find correspondence between Machta and either Brewer or Dobson, Wexler engaged in some correspondence with Brewer during and following the hearings on the applicability of his theory to fallout. See Harry Wexler Papers, Library of Congress Manuscript Division, Box 9.

to three-times more radiation was detected in the north as compared to areas near the equator.[52] To be sure, Machta acknowledged, the latitudinal spike could possibly be the result of tropspheric-level tests conducted at the NTS, which were known to deposit along the same lines of latitude in which they were detonated. Nonetheless, he reasoned, the spike could only be caused by fallout of stratospheric origin owing to estimates of the "age" of the debris and the high radioactivity levels detected.[53]

The impact of Machta's testimony on Libby's model and the assessment of risk was not lost on the JCAE members. When Libby went before the Congressmen, they pressed him to explain the obvious discrepancy between his and Machta's stratospheric models. As one might expect, Libby maintained that the differential deposition patterns were due to NTS fallout contributions, not thermonuclear.[54] Apart from that, Libby attempted to downplay his and Machta's disagreements. "This is a difference of point of view which is not serious," he explained disingenuously, "in the sense that further information will straighten it out."[55] New Mexico Senator Clinton Anderson, a fierce critic of the AEC's culture of secrecy, would have none of it. When Anderson failed to

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[52] *The Nature of Radioactive Fallout and Its Effects on Man,* 152 & 56. The evidence also showed that concentrations were higher in the northern hemisphere compared to the southern, although at the time Machta could not explain why this was so.

[53] Ibid., 153-5. For reasons not entirely clear, Machta did not discuss the stratospheric storage issue, which he had been on record as doubting the long residence time that Libby advocated. His omission may perhaps have been a result of his having felt that the uniformity and tropopause question was the larger issue. In a paper he presented at a special symposium on low-level radiation at the annual AAAS meeting a half-year later, he wrote, "Contrary to the views of some non-meteorologists [read: Libby], the prolonged suspension of contaminants in the stratosphere is due to the slowness of vertical mixing throughout the lower stratosphere and not because the tropopause is some kind of semi-impermeable barrier." Lester Machta, "Discussion of Meteorological Factors and Fallout Distribution " in *Low-Level Irradiation* (Washington D.C.: American Association for the Advancement of Science, 1959). Or, perhaps, he felt the point of difference between a residence time of 10 or 5 years mattered little when considering that the half-life of Sr-90 was 28 years, which he acknowledged in his original Washington Academies paper. Machta, "Meteorological Factors Affecting Spread of Radioactivity from Nuclear Bombs," 184.

[54] *The Nature of Radioactive Fallout and Its Effects on Man,* 1214.

[55] Ibid.

solicit a response from Libby on the significance of the dispute, he took the opportunity to drill Libby on the impact that the resolution of the matter would have for the assessment of fallout risks: "It affects tremendously," Anderson declared to Libby," the question of how much fallout is safe, how much testing is safe, because if you assume that the pattern is uniform around the world, when actually it is 2 times or 3 times heavier in a given place, then you have, by this assumption, lowered the possibility of damage from fallout." To which Libby finally conceded, "You are certainly right, Senator."[56]

During the hearings, both Libby and Machta acknowledged that their analysis of the pattern and rate of stratospheric fallout rested on sketchy data derived largely from the comparison of theoretical estimates of the amount of strontium-90 injected in the stratosphere and the levels of fallout detected in the ground-level monitoring systems. Both were problematic, not the least of which was the gummed-film network, which had come under scrutiny over its scant geographical coverage and poor collection efficiency. What was needed to settle the controversy, as the BEAR report stressed the previous year, were actual measurements of the total quantity and circulation of strontium-90 in the stratosphere. The AEC's failure to obtain stratospheric data was not from want of trying. At the time of the hearings, technical problems involving the collection of sufficient particulate matter in the upper atmosphere severely limited the ability of the AEC to make reliable measurements of radioactive debris in the stratosphere. Nonetheless, growing scrutiny of the AEC's fallout program from the public and the JCAE compelled the Commission to redouble its efforts.

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[56] Ibid., 1217.

Stratospheric Monitoring and Radiotracer Experiments

Balloon-borne observations of the upper atmosphere was not a new meteorological practice when the AEC began investigating the feasibility of collecting stratospheric strontium-90 debris. At least since the eighteenth century, scientists had utilized balloons to obtain measurements of air composition and other basic characteristics of the atmosphere.[57] Throughout the nineteenth and early twentieth century, meteorologists were relying ever more heavily on balloon technologies to provide synoptic measurements of the vertical and horizontal gradients of temperature, pressure, humidity, and wind speed to aid in weather forecasting and advance meteorological theory. By the mid-twentieth century, technical improvements enabling higher-altitude flights and better reliability had rendered balloons as the preeminent meteorological tool. The invention of radio-telemetry, for example, led to the invention of the radiosonde (1929), which could provide meteorologists with instantaneous transmission of atmospheric data. Another important technological advance was the polyethylene plastic balloon. Developed by American company General Mills in 1947 (its plastic food containers such as cereal bags provided the material basis for the balloon skins), the "sky hook" balloon reached upwards of 30 kilometers and was able to lift increasingly heavier scientific payloads. Thus, by the time the AEC set its sights on a program to measure strontium-90 in the stratosphere, the technology to reach such

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[57] The information for this paragraph is drawn from John L. DuBois, Robert P. Multhauf, and Charles A. Ziegler, *The Invention and Development of the Radiosonde, with a Catalog of Upper-Atmospheric Telemetering Probes in the National Museum of American History, Smithsonian Institution*, vol. 53, *Smithsonian Studies in History and Technology* (Washington D.C.: Smithsonian Institution Press, 2002); G. Pfotzer, "History of the Use of Balloons in Scientific Experiments," *Space Science Reviews* 13 (1972).

heights was readily available. Adequate sampling devices capable of collecting particulate matter, however, were not.

The AEC had been interested in utilizing skyhook balloons to sample radioactive debris in the stratosphere as early as 1953. Following the Rand conference, Merril Eisenbud developed a prototype of an electrostatic precipitator that he hoped would be capable of obtaining sufficient quantities of radioactive materials in the low pressure densities of the upper atmosphere.[58] When HASL and the Weather Bureau conducted trial flights at Holloman Air Force Base in New Mexico, the precipitator performed so poorly in the low-atmospheric pressure of the stratosphere that the resulting samples of radioactive particulates proved too meager for radiochemical analysis of strontium-90. Given the high cost of the program and the discouraging results, the AEC terminated the flights in July of 1954.[59]

Yet, by early 1956 as the dispute between Machta and Libby grew, the AEC was compelled to reinvestigate the feasibility of establishing a routine stratospheric balloon sampling program. In February, the AEC contracted General Mills to look into the feasibility of using the company's newly developed aerosol sampling apparatus, nicked-named "ashcan," to collect radioactive debris. In spring, the company conducted a series of test flights at its headquarters in Minneapolis and, with the cooperation of the Air

[58] Eisenbud, *Environmental Odyssey: People, Pollution, and Politics in the Life of a Practical Scientist*, 79. See also J.Z. Holland, "Stratospheric Radioactivity Data Obtained by Balloon Sampling, in *Fallout from Nuclear Weapons Tests*, 593.
[59] January 18, 1955, "Transcript of Biophysics Conference," NARACP, RAEC, E73b, box 8, folder Biophysics Conference, 137; J.Z. Holland, "Stratospheric Radioactivity Data Obtained by Balloon Sampling, in *Fallout from Nuclear Weapons Tests*, 593.

Force, at San Angelo Texas.[60] The test flights proved promising. Although there still remained some question about the efficiency of the collection device at high altitudes, the flights did amass sufficient particulate matter to enable HASL to perform radiochemical analyses for strontium-90 levels. With the successful flights, the AEC established "Project Ashcan" and expanded operations to the equatorial latitudes with regular flights at two new sites at the Panama Canal Zone and Sao Paul, Brazil.[61]

Despite the potential of the Ashcan flights, organizational problems within the bureaucracy of the AEC plagued the program. The difficulty stemmed from the fact that the Commission's Division of Biology and Medicine (DBM), which was given technical direction over the program, did not have the staff to coordinate the numerous contractors on the project much less analyze and interpret the results.[62] Eventually, HASL was given full authority to administer Ashcan in the early 1960s which served to stabilize the program. In the meantime, however, the Ashcan dilemma was acutely felt within the DMB because the Commission was planning a series of special radiotracer experiments during the spring and summer of 1958 right before a temporary moratorium on testing that the nuclear powers had negotiated was to take effect.

The tracer studies consisted of the addition of two unique radioactive tags—tungsten-185 and rhodium-102—that were added to detonations the Pacific Hardtack series. With a half-life of seventy-four days, the tungsten tracer was injected into the

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[60] Ibid.; see also S.C. Stern, "The Collection Efficiency of I.P.C. Filter Mats for Radioactive Particulates in the Stratosphere," in same volume.

[61] Ibid. See also J.Z. Holland, "AEC Atmospheric Radioactivity Studies," NTA, NV0137293, 8.

[62] J.Z. Holland to C.L. Dunham, March 12, 1959, "Monitoring Projects, Especially Ashcan," RAEC, E73b, box 3, folder Stratospheric monitoring 1958. Libby was considering dropping the program altogether. See J.Z. Holland to C.L. Dunham, July 15, 1958, "Meeting with Dr. Libby," NARACP, RAEC, E73b, box 3, folder Stratospheric monitoring 1958.

atmosphere with shots conducted between April and July. The rhodium tracer was associated with a single high altitude test, Orange, that was detonated 141,000 feet in the atmosphere. The tracers were added to simplify analysis of the movement of radioactive debris in the stratosphere. One of the major complexities in using strontium-90 as a tracer pivoted upon the fact the stratospheric burden of the element was the product of several injections at various times, places, and altitudes. The tungsten and rhodium tracers were designed, then, to more precisely identify the mechanisms of stratospheric circulation in order to settle residence time and uniformity questions.[63] Without a fully functioning Ashcan program, these studies were put at some jeopardy.[64]

Unbeknownst to the AEC, however, the DoD had instituted their own stratospheric sampling project around the same time the Commission was developing Ashcan. In 1955, the DoD ordered the Armed Forces Special Weapons Project (AFSWP) to initiate a study into the possibility of incorporating high efficiency paper filters on its top-secret U-2 spy planes. The purpose of the study was to investigate the feasibility of incorporating highly efficient paper filters on the planes in order to "define and delineate

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[63] J.Z. Holland, "AEC Atmospheric Radioactivity Studies," NTA, NV0137293. For more on results of the tungsten and rhodium tracers see Herbert W. Feely, "Strontium-90 Content of the Stratosphere," *Science* 131, no. 3401 (1960); Herbert W. Feely and Jerome Spar, "Tungsten-185 from Nuclear Bomb Tests as a Tracer for Stratospheric Meteorology," *Nature* 188 (1960); Herman Hoerlin, "United States High-Altitude Test Experiences, Report La-6405," (Los Alamos: Los Alamos Scientific Laboratory, 1976); M. I. Kalkstein, "Rhodium-102 High-Altitude Tracer Experiment," *Science* 137, no. 3531 (1962); Robert J. List, Leonard P. Salter, and Kosta Telegadas, "Radioactive Debris as a Tracer for Investigating Stratospheric Motions," *Tellus* 28, no. 2 (1966); Robert J. List and Kosta Telegadas, "Using Radioactive Tracers to Develop a Model of Circulation of Stratosphere," *Journal of the Atmospheric Sciences* 26 (1969); Lester Machta, "Transport in the Stratosphere and through the Tropopause," *Advances in Geophysics* 6 (1959); E.A. Martell, "Tungsten Radioisotope Distribution and Stratospheric Transport Processes," *Journal of the Atmospheric Sciences* 25 (1968); K. Telegadas and R. J. List, "Global History of 1958 Nuclear Debris and Its Meteorological Implications," *Journal of Geophysical Research* 69, no. 22 (1964).

[64] The AEC could, of course, rely on the gummed-film network to measure the tungsten and rhodium fallout, but as with strontium the key lay in direct measurement of the stratosphere.

the stratospheric reservoir of radioactive debris."[65] The next year, AFSWP reported that such filters were indeed achievable and began further developing the system. By 1957, with the sampler complete, AFSWP commenced regular north-south stratospheric flights at its bases in New York State and Puerto Rico.[66] So began the High Altitude Sampling Project, or HASP.

Despite the obvious importance that the HASP project held for the AEC, the DoD kept the project a secret throughout 1957 and well into the next year, ostensibly because they would "have no security control in the AEC."[67] Eventually, AFSWP informed the AEC of the HASP flights sometime in mid-1958 but withheld the data.[68] On December 11, however, AFSWP forwarded the Joint Chiefs of Staff a report detailing the preliminary HASP analysis. "Recent indications," the report said, "are that the radioactivity in the stratosphere has a residence half-life of about 2 years (in contrast to the previously assumed value of about seven years)" and the "concentration of the Sr90 on the surface of the earth is greater in the United States than in any other area of the world."[69] On Christmas Eve, AFSWP officials met with AEC Commissioners and DBM scientists to reveal the HASP findings.[70] Although Libby, not surprisingly, dismissed the

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[65] *Fallout from Nuclear Weapons Tests*, 772.
[66] Ibid., 774; Frank H. Shelton, *Reflections of a Nuclear Weaponeer* (Colorado Springs: Shelton Enterprise, Inc, 1988), chapter 7, 50-55. Early reports on the HASP data can be found in Albert K. Stebbins, "Second Special Report on the High Altitude Sampling Program (HASP): Technical Analysis Report," (Washington D.C.: Defense Atomic Support Agency, 1961); Albert K. III Stebbins, Ed., "A Special Report on the High Altitude Sampling Program," (Washington D.C.: Defense Atomic Support Agency, 1960).
[67] Shelton, *Reflections of a Nuclear Weaponeer: Operation Crossroads 1946*, chapter 8-5.
[68] J.Z. Holland to C.L. Dunham, July 16, 1958, "Meetings with AFSWP and AFOAT-1 on Stratospheric Sampling," NARACP, RAEC, E73b, box 2, folder Fallout air upper.
[69] Memorandum for: The Chairmen, Joint Chiefs of Staff, December 11, 1958, "Status Report on Fallout, NTA, NV0757969.
[70] J.Z. Holland to Files, December 24, 1958, "AFSWP Briefings on HASP," NTA, NV0025839.

import of the HASP data, he nevertheless notified the Joint Committee on Atomic Energy of the new stratospheric results soon after.[71]

Once Libby informed the JCAE of the HASP results, a controversy erupted between the Committee, AEC, and DoD. On February 19th, General Herbert Loper, assistant to the Secretary of Defense for Atomic Energy, summarized the conclusions of HASP for the JCAE on a confidential basis. In the letter, Loper informed the committee that "Tentative conclusions to date indicate that three-tenths of the quantity of radioactive debris leaves the stratosphere each year, that the north-south diffusion of radioactive particles in the stratosphere does exist, and that in both hemispheres, there is a latitude band of maximum drip out which us from 35 to 50 north or south."[72] The following week, Libby wrote a rebuttal letter to Loper, which he copied to the JCAE. Based on his analysis of the HASP data provided to him by AFSWP back in December, Libby revised his residence time figure to four years, but insisted to Loper that it was "difficult to push it down to the 2 years you give."[73] Moreover, he argued, the "old" non-uniformity argument "still is not quite settled" and blamed local fallout from tests at the NTS for the discrepancies in hemispheric fallout.[74] While this debate was being conducted behind closed doors, Libby continued to cling to his simplified model in his public addresses. On March 13, he delivered a speech in Seattle that recapitulated the conclusions drawn in his original model. Instead of acknowledging his reinterpretation of the residence time in

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[71] J.Z. Holland to Files, March 27, 1959, "Review of Recent Estimates of Stratospheric Inventory and Residence Time," NARACP, RAEC, E73b, box 2 folder Fallout General. See also, "Brief Chronology of Action..." in *Fallout from Nuclear Weapons Tests*, 2540. DBM scientist Hal Hollister notified DBM chief Charles Dunham of the HASP results early the next month. See Hal Hollister to C.L. Dunham, January 7, 1959, "1959 Soil Sampling," NARACP, RAEC, E73b, box 2 folder Fallout collection analysis.
[72] *Fallout from Nuclear Weapons Tests*, 2538.
[73] Ibid.
[74] Letter can be found in *Fallout from Nuclear Weapons Tests*, 2539.

light of new data, Libby maintained that the "rate of decent... is so small that something like 5 to 10 years appears to be the average time they spend before descending to the ground."[75] Upon hearing of Libby's Seattle speech, Senator Clinton Anderson, the chair of the JCAE's subcommittee on radiation, was infuriated. A few days later, Anderson attempted to have the content of Loper's letter declassified for speech he was preparing to refute Libby. The DoD demurred. Although they were apparently willing to declassify the letter, on the advice of Libby, they refused, citing the preliminary nature of the data. Further enraged, Anderson subsequently accused the DoD of "gagging" the JCAE when it had pertinent data to share with the American public.[76] Anderson's ploy worked; the DoD declassified the letter and on March 22, Anderson made the correspondence between the JCAE, Loper, and Libby available to the public. In Anderson's press release, he wrote, "it looks like strontium 90 isn't staying up in there as long as the AEC told us it would, and the fallout is greatest on the United States." Furthermore, Anderson announced that the JCAE would be holding hearings in May to get to the bottom of the matter.

Conducted over four days in early May, the hearings were nothing short of a disaster for Libby and his stratospheric model. The death knell proved to be AFSWP technical director Frank Shelton testimony regarding the HASP data. Shelton reported that HASP measurements of strontium-90 since 1956, in conjunction with the detection of tungsten-185 during the Hardtack series, demonstrated conclusively a preference for

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[75] Ibid., 2227.

[76] Clinton P. Anderson and James T. Ramey, "Congress and Research: Experience in Atomic Research and Development," *Annals of the American Academy of Political and Social Science* 327 (1960): 87; Edward Gamarekian, "Defense and AEC Clash on Fallout Rate," *The Washington Post and Times Herald*, March 22 1959.

northern hemispheric spreading of radioactive debris. With regard to the residence time question, Shelton confirmed the estimates contained in Herbert Loper's letter to the JCAE: for shots detonated by the U.S. and U.K. in equatorial regions, the average hold-up of radioactive debris was on the order of two years. Even more alarming, he also informed the Committee that Soviet thermonuclear tests conducted in the northern latitudes had a residence time of merely one year. Given these estimates, Shelton reasoned that if there was no further testing, the amount of strontium-90 fallout on the earth's surface would more than double by 1960.[77]

Following Shelton's testimony, the Committee next turned to Machta to explain the atmospheric mechanisms behind the rapid rate and non-uniform pattern of stratospheric fallout. Machta reasoned, as he had in 1957, that the observed differences in fallout deposition could be explained by breaks in the tropopause in temperate latitudes as predicted by the Brewer-Dobson stratospheric circulation model. When asked what he thought of Shelton's testimony about HASP, Machta responded that "I have complete confidence in it." Moreover, he elaborated, "it is my view, in light of Dr. Shelton's comments about the uneven distribution of tungsten 185, plus the uneven distribution at ground level air concentration, plus the uneven distribution of fallout...represents a fairly convincing picture."[78]

The Committee would hear further testimony from Air Force Cambridge Research Center scientist E.A. Martell on another possible model of stratospheric circulation different from Machta that could account for the atmospheric and ground-

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[77] *Fallout from Nuclear Weapons Tests*, 763-78.
[78] Ibid., 784-85.

level data collected by programs such as HASP. Regardless, the differences between Martell and Machta's models were largely academic—their discrepancies held significance for fundamental understandings of the nature of the stratosphere. Both agreed that Libby's model seriously underestimated the extent and pattern of strontium-90 fallout by assuming the tropopause as uniformly impermeable atmospheric boundary layer.

The emerging consensus of opinion on the deposition patterns of stratospheric fallout was reflected in the publication of the second round of BEAR reports, a year after the hearings. Indeed, the first conclusion drawn in the report signaled closure of the controversy. Based upon the stratospheric measurements conducted by HASP and Ashcan, in addition to the radiotracer experiments in 1958, the Committee concluded that "The non-uniform distribution of Sr90 fallout...has been confirmed." Further, with regard to the residence time question, the Committee put to rest the notion that a single figure could be ascribed to the rate of stratospheric removal: "It is now generally recognized that the concept of a fixed fractional removal rate from the stratosphere is untenable and that the removal rates depend on the latitude and altitude of injection of the debris, the season, and upon stratospheric circulations, which have a spatial and temporal variability. As a first approximation, the idea of a variable mean storage time may be used. For equatorial injections the values range from about one to five years, the shorter times applying to lower stratospheric injections; for temperate and polar latitude injections [i.e. the Soviet tests at Semipalatinsk and Novaya Zemlya, respectively], the time is under one year.[79] In other words, the tropopause could not be relied upon as an effective barrier preventing or reducing the amount of strontium 90 fallout. If anything, atmospheric mechanisms tended to make matters worse by concentrating fallout in the northern hemisphere where the majority of the Earth's population dwelled.

While the publication of the BEAR report served to effectively close the Libby-Machta controversy, it also gestured toward new uses of radioactive debris and the atmospheric monitoring programs as tools for better understanding atmospheric circulation. As the final sentence in the report makes clear, further study of fallout as a tracer would help clarify some minor points of uncertainty in the fallout rate and pattern, but its greatest benefit perhaps laid in the creation of a "new fund of knowledge to our understanding of atmospheric circulation."[80] The technologies and concepts that arose out of the military-industrial complex to account for the poisoning of the atmosphere were to soon be employed toward more fundamental and academic ends. It is not without some irony, then, that new techniques of knowing the environment should have their origins in the very thing that could guarantee its destruction.

The emerging consensus about the non-uniform pattern and swift stratospheric fallout rate did not, however, signal the end of atmospheric testing. Perhaps because the hearings and the publication of the BEAR report occurred during the testing moratorium, press reporting of the hearings lagged behind their predecessor. Nonetheless, growing public awareness of these issues would be put in stark focus for many Americans as reports of heavy fallout made their way onto the front page of newspapers following the

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[79] "The Biological Effects of Atomic Radiation: Summary Reports," (Washington D.C.: National Academy of Sciences--National Research Council, 1960).
[80] Ibid., 46.

resumption of testing in the summer of 1961. In the meantime, however, the detection of further episodes of hot spot contamination in the upper Midwest from America's own backyard testing site began to generate as much scrutiny as stratospheric fallout. Boundaries were beginning to collapse everywhere around the AEC.

The Nevada Test Site Revisited: Hot Spots

The AEC, as I explained earlier, had been aware of the presence of hot spots in certain regions of the U.S. (namely, upstate New York) since the first NTS tests back in 1951. Nonetheless, throughout the middle years of the 1950s the AEC continued to maintain that the potential health effects from such episodes of intense and localized fallout activity were exceedingly remote. When the AEC commissioned a restudy of the need and feasibility of testing the NTS in 1953, for example, the effect of hot spots was considered, but only insofar as the phenomena pertained to nearby communities in Nevada and southern Utah.[81] Toward that end, the Commission contracted Lester Machta to initiate a broad study of the regional meteorological conditions of the NTS in hopes of developing a fallout prediction model that might enable safety planners and monitors to anticipate potentially affected areas within roughly a 300 miles radius of the test site.[82] The prospect that NTS detonations might results in hot spots farther afield was not discussed as they were not apparently germane to safety considerations at the NTS.

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[81] "Report of Committee on Operational Future of Nevada Proving Grounds," May 11, 1953, NTA, NV0720368; "Report of Committee to Study the Nevada Proving Grounds: Attachments", February 1, 1954, NTA, NV0061647.

[82] Ibid.; William R. Kennedy Jr., "Fallout Forecasting--1945 through 1962, Report No. La-10605-Ms," (Los Alamos: Los Alamos National Laboratory, March 1985), 15. See also the correspondence between Weather Bureau and AEC officials in NTA, NV0062753.

In spite of the growing evidence to the contrary, the Commission still deemed the atmosphere as a reliable fallout sink.

During the summer of 1957, however, events following the Plumbbob tests at the NTS posed a fierce challenged those assumptions when heavy fallout was detected in the upper-Midwestern states. Follow-up measurements conducted by Sunshine teams demonstrated a remarkable rise of strontium-90 levels in soil, water, and milk supplies. Despite the high levels (some milk samples contained nearly a quarter of the maximum permissible burden of strontium-90), no precautionary measures were taken and the citizens of those states were not notified of the fallout.[83] Later in the spring of the following year, the *Grand Forks Herald* and the *Minneapolis Tribune* ran stories on the fallout of the previous summer. The reporters learned of the fallout by coincidence when J. Laurence Kulp, a sunshine researcher at Columbia University's Lamont Geological Laboratory, let it slip that he was in the region to collect bones samples for strontium-90 content analysis in the wake of the fallout.[84] News of the fallout and the bone collection spurred a series of investigations by independent scientists into the severity of the exposures, which subsequently confirmed the rise of radiation levels in food supplies associated with the fallout.[85] In Minnesota, one such study on radiation levels detected in

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[83] The MPC for strontium-90 levels was 100 strontium units. Milk in Mandan North Dakota registered 23 strontium units, or nearly one quarter of the MPC. *Fallout from Nuclear Weapons Tests*, 2115. Some precautionary measures were taken in Belle Fourche which included washing the streets. See Patrick Springer, "Forgotten Fallout: What Is the Legacy of the Radioactive Rains?," *Fargo Forum*, May 1 1988.

[84] E.W. Pfeiffer, "Some Aspects of Fallout in North Dakota," *North Dakota Quarterly* 24, no. 4 (1958).

[85] See volume 3 of *Fallout from Nuclear Weapons Tests*.

local wheat crops so concerned the governor that he appointed a special committee to study the problem.[86]

Despite the detection of these Midwestern hot spots, the problem received little attention during the 1959 hearings—nearly two years after the incidents occurred. The omission was not from want of knowing on the part of the JCAE. Prior to the hearings, the JCAE, which had received various reports of the Midwestern hot spots from the independent investigations, requested that the AEC give the problem "comprehensive treatment" and included the topic in the Committee's outline for discussion.[87] When the hot spot issue was not covered during the hearings, Senator Clinton Anderson (who as we've seen was an outspoken critic of the Commission) wrote the AEC requesting that the Commission generate a report on the hazards associated with hot spots and the effect that short-term fallout radionuclides might have on human health in these localized concentrations.[88] In the meantime, Anderson made sure that the materials submitted by the independent scientists with accompanying AEC responses were included in a special appendix to the published record of the hearings.[89]

In the meantime, the lack of testimony about hot spots during the hearings was not lost on at least one perceptive journalist. On June 7, *Washington Post and Times Herald* reporter Edward Gamarekian wrote a lengthy piece on the Midwestern hot spots in which he chastised the JCAE Congressmen for not inquiring "on what was happening

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[86] The report of this committee and other correspondence between it and the AEC can be found in NARACP, RAEC, E73b, box 35, Folder Fallout Minnesota.
[87] *Fallout from Nuclear Weapons Tests*, 2114.
[88] For a timeline of events regarding the AEC and Anderson's request, see, "GAC Fallout Question, no date, RAEC, E73b, box 11, folder GAC fallout questions, 1959-1960.
[89] See volume 3, appendix B: "Hot spot" problem and strontium-90 in foods," in *Fallout from Nuclear Weapons Tests*.

in the Nation's 'hot spots' despite the ominous note in reports to them from scientists in several Midwestern universities."[90] The presence of hot spots was critical for fallout risk assessment, as Gamarkenian made clear in another article, because when the AEC issued public announcements regarding fallout hazards it presented Americans with averaged figures for the entire country or, in some cases, the entire globe.[91] This had the effect of minimizing the potential effect that localized concentrations of fallout could have on peoples residing in particularly affected regions. The AEC's General Advisory Committee (GAC) at the start of the 1959 hearings, for example, released a statement which reflected this kind of statistical trickery.[92] External fallout levels when put in "proper perspective," the GAC statement noted, were currently "less than 5 per cent as much as the average exposure to cosmic rays... [and] Less than 5 per cent of the estimated average radiation exposure of…X-rays for medical purposes." For internal emitters such as strontium-90, the GAC used similar statistical logic: "the amount of strontium 90 which has been found in food and water is less of a hazard than the amount of radium normally present in public drinking water."[93] True enough, perhaps, but by failing to account for the higher-than-average fallout levels in certain areas of the United States, the report seriously downplayed the radioactive burdens that people in those affected regions were being asked to carry in the name of national security. As E.W.

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[90] Edward Gamarekian, "Serious Fallout Cases Uncovered in Middle West," *The Washington Post and Times Herald*, June 7 1959.

[91] See Edward Gamarekian, "Report Attacked: Fallout Hearing Data Challenged," *Washington Post and Times Herald*, May 16 1959.

[92] It is worth noting that the GAC, made up almost entirely of physicists, issued this report, not the Division of Biology and Medicine, whose expertise was obviously better suited for analysis of biological effects. It is not clear why the GAC was chosen for this assignment, but the fact that the AEC charged them with this assignment suggests the AEC Commissioners were up to some chicanery.

[93] The GAC report was reprinted in full in "Fall-out Statement by A.E.C. Advisers," *New York Times*, May 8 1959.

"Bert" Pfeiffer wrote in his article that detailed the high levels of fallout radioactivity detected in the Dakotas during the 1957 tests, "it is difficult to understand how the people of North Dakota can intelligently express their opinions on the vital question of continuation of nuclear weapons tests…if they do not have information concerning the results of these tests."[94] National averages, put another way, only served to conceal the greater risks being borne by North Dakotans.

It took the AEC a year to compile and analyze the data for the hot spot report that Anderson requested at the close of the 1959 hearings. Written by Fallout Studies Branch analyst Harold Knapp, the report marked a significant turn within the AEC toward the investigation of the effect of short-lived isotopes such as radioiodine on human health (see chapter 4). In part, the new focus on radioiodine was an outgrowth of the closure of the residence time question: faster fallout rates coupled with the hot spot problem stressed the growing need to understand the environmental behavior shorter-lived radionuclides. The report proved that the growing concern was justified. In his analysis of four hot spot events (Jefferson City, MO, North Dakota, Troy, and Salt Lake City), Knapp pointed to a fundamental flaw in the AEC's monitoring efforts. Whereas evaluation of fallout levels were typically reported in terms of external gamma exposure, he showed that concentration of radionuclides within human bodies results in doses to specific organs that can be five- to-ten times higher than the external whole body dose. At Jefferson City roughly 1,500 miles from the NTS, for example, he estimated that children drinking a liter of milk per day for a two year period between the springs of 1957 and 1959 were given a dose of 2.5 rads of radiation to their thyroid, despite external

[94] Pfeiffer, "Some Aspects of Fallout in North Dakota," 93.

measurements for the same period that amounted to a few hundredths of a rad.[95] (At the time, a 3 rad dose in a year for the general population would have exceeded the maximum permissible limit. Later the next year, however, the standards would be revised to 1.5 for non-exposed populations and 0.5 for previously exposed groups.)[96] Although the report drew no conclusions as to level of added risk to offsite populations from these hot spot episodes, in the cover letter sent to Clinton Anderson with the report, AEC Chairman John McCone sounded a note of conservative optimism: "The average doses are...well below the level where biological damage has been observed in humans." McCone did acknowledge, however, that this assessment did not imply that should testing resume that observable effects could occur and pledged that the Commission would step up its research in this field and institute a more comprehensive plan for monitoring radioiodine levels in milk.[97]

Testing did resume a little over year later. In August of 1961, the Soviets broke the moratorium prompting the Americans to follow suit a month later, resulting in an unprecedented spate of weapons tests: between 1961 and 1963 the tests conducted by the U.S. and U.S.S.R. more than doubled the amount of radioactive debris in the atmosphere than had been amassed in all the years prior to the moratorium in 1958.[98] And now, both the AEC and general public armed with new knowledge and and new methods of

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[95] Harold A. Knapp, "The Contribution of Short Lived Isotopes and Hot Spots to Radiation Exposure in the United States from Nuclear Test Fallout, June 6, 1960, NTA, NV00019168, 38. The comparison of the thyroid dose to external dose comes from Joint Committee on Atomic Energy, *Fallout, Radiation Standards, and Countermeasures*, 88th Congress, 1st sess., 1963, 1079.
[96] Federal Radiation Council to The President, May 11, 1960, "Radiation Protection Guidelines for Federal Agencies, NTA, NV0407484.
[97] John A. McCone to Senator Anderson, June 15, 1960, NARACP, RAEC, E73b, Box 11, Folder GAC Fallout Question 1959-1960. The Public Health Service would be charged with milk monitoring.
[98] Congress, *Fallout, Radiation Standards, and Countermeasures*, 10.

sampling and monitoring stratospheric and hot spot fallout, the chickens were coming home to roost.

In September, the *New York Times* ran an article describing the elevated fallout levels in North America from the series of Soviet tests at its northern testing site. Machta was featured heavily in the article, explaining how the rapid decent of the radioactive debris was due to the conduct of the tests in northerly latitudes.[99] The next month, as more fallout rained down in the northern hemisphere, the *Times* ran on consecutive days a primer of sorts on fallout describing the deposition patterns of stratospheric and tropospheric fallout. Published just prior to the end of the month, the writer opened the article with the ominous note that "In this Halloween season, a new man-made specter— fallout from atomic tests— is beginning to haunt the world."[100] And haunt the world it did. Throughout the next two years before the signing of the Limited Test Ban Treaty, papers across the nation reported on the dramatic increase of radiation levels in milk and other food supplies. It wasn't just the megaton weapons being fired that worried people either. Hot spots of radioiodine levels in milk were detected throughout the Midwestern states and mountain-West whose origin could only have been from the Nevada Test Site. In one well-publicized incident, a hot spot in the Salt Lake City area prompted the State's public health officials to order dairy farmers to switch their milk cows to uncontaminated dry feed because iodine-131 levels exceeded the maximum permissible limit.[101]

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[99] Marjorie Hunter, "12 States Record Rise in Fallout," *New York Times*, September 19 1961.
[100] John W. Finney, "Fall-Out: What It Is and the Threat It Poses," *New York Times*, October 29 1961. See also Robert K. Plumb, "Hazard from Radioactive Fall-Out: What Causes It, How It Affects Human Beings," *New York Times*, October 30 1961.
[101] Hacker, *Elements of Controversy*, 221; "Radioactive Content of Milk Found Sharply Higher in Utah.""

It was not only the increased scale of testing that alarmed the American (and global) public. By 1963, Americans knew substantially more about the environmental behavior of nuclear testing fallout than they did prior to the moratorium. They knew, for example, that radioactive particles produced by nuclear explosions often thousands of miles away from their homes circulated throughout the globe, rained on agricultural fields, sometimes within days, became incorporated into living things which they consumed, and remained in their bodies until their death. And critically, they understood that they did not have a choice in the matter. The contamination of strontium-90 and iodine-131 in the food supply was simply a Cold War matter of fact; whatever one's view on nuclear testing, you could not escape fallout. It was an unsettling moment and one that held great portent for how people would increasingly come to view the contamination of the environment: no matter where one lived or how far one was from a source of pollution, the vagaries of nature held open the possibility that one's body could be harmed by something that happened on the other side of the globe.

Conclusion

Following the LTBT, meteorologists would continue to study the circulation of strontium-90 as well as the tungsten and rhodium tracer experiments in subsequent iterations of HASP and Ashcan.[102] These studies, however, were geared toward a more fundamental scientific goal. Fallout radiation, despite its role as a global poison, offered meteorologists an unmatched tool for studying the stratosphere, which prior to the advent of nuclear weapons testing had been largely foreclosed to scientific scrutiny. Debates

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[102] See note 48.

about the Brewer-Dobson theory and other models of stratospheric circulation would continue to swirl as data from these projects came in, but one thing remained certain: the Earth's atmosphere was a complex and dynamically integrated space that resisted human efforts to simplify and control it.

In an oral history conducted in the 1990s, Lester Machta was asked about his role in atmospheric nuclear weapons testing. "I think I was misled...," he commented, "in underestimating what potential damage might actually have occurred from the fallout from U.S. tests. Although by publicizing the fallout, as I did, I think the world got quite an abhorrence to nuclear testing and contributed, in my opinion, significantly to the nuclear test ban."[103] Machta understated his role. His work did not only publicized an alternate view to Libby, but perhaps more importantly, signaled a profound shift in how scientists considered the ways in which the environment mediated human exposure to fallout radiation.

At the advent of nuclear weapons testing in 1945, protecting humans from the hazards of fallout was predicated, in part, by ensuring that the bombs were detonated under favorable local weather conditions. The undergirding assumption within this framework held that local weather forecasting was the key to preventing fallout outside the boundaries of the NTS. With the coming of thermonuclear testing, Willard Libby assumed, in a similar manner, that fallout could be bounded within the atmosphere, albeit on a much higher vertical plane. Yet, as scientists like Machta began focusing their attention toward the environment as the critical factor in assessing the risks of fallout,

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[103] Lester Machta, interviewed by Julius London, October 31, 1993, transcript, American Meteorological Society Tape Recorded Interview Project, The National Center for Atmospheric Research Archives, Boulder, CO, 7.

meteorologists turned from weather forecasting toward the fundamental study of large-scale atmospheric phenomena and mechanisms. Aided by new tools such as radiotracers, balloons, and air- and ground-level sampling systems, the notion of containment gave way to new understandings of the atmosphere as spatially integrated and dynamic. This new way of seeing the atmosphere highlighted the idea that seemingly distant and isolated events held global significance. In this way, Machta's work with radiotracers and global data collection was part of a larger technological revolution in meteorology (e.g. numerical weather forecasting, satellite development, the creation of World Meteorological Organization's World Weather Watch) that was rendering the globe into a knowable object. It is no surprise, then, that the modern study of global warming should follow closely on the heels of atmospheric nuclear weapons testing and these other trends.[104] Put simply, the atmosphere in the early 1960s was a profoundly different space than it was in 1945—and as a consequence, so too were our ideas about the hazards of the modern world.

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[104] See, for example, Edwards, *A Vast Machine: Computer Models, Climate Data, and the Politics of Global Warming*, 207-15.

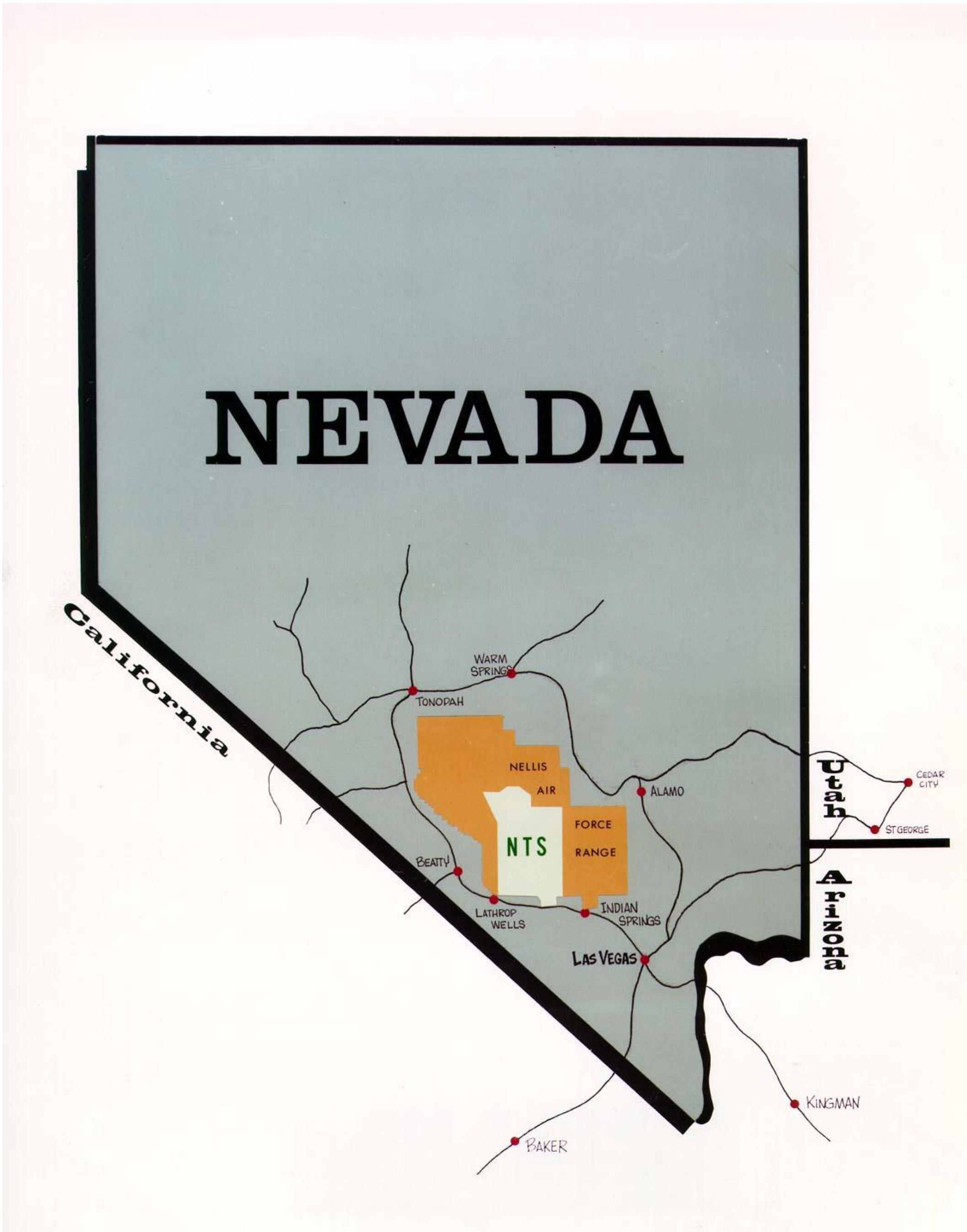


Figure 23 - Map of the Nevada Test Site. Source: Dept. of Energy. Public Domain.

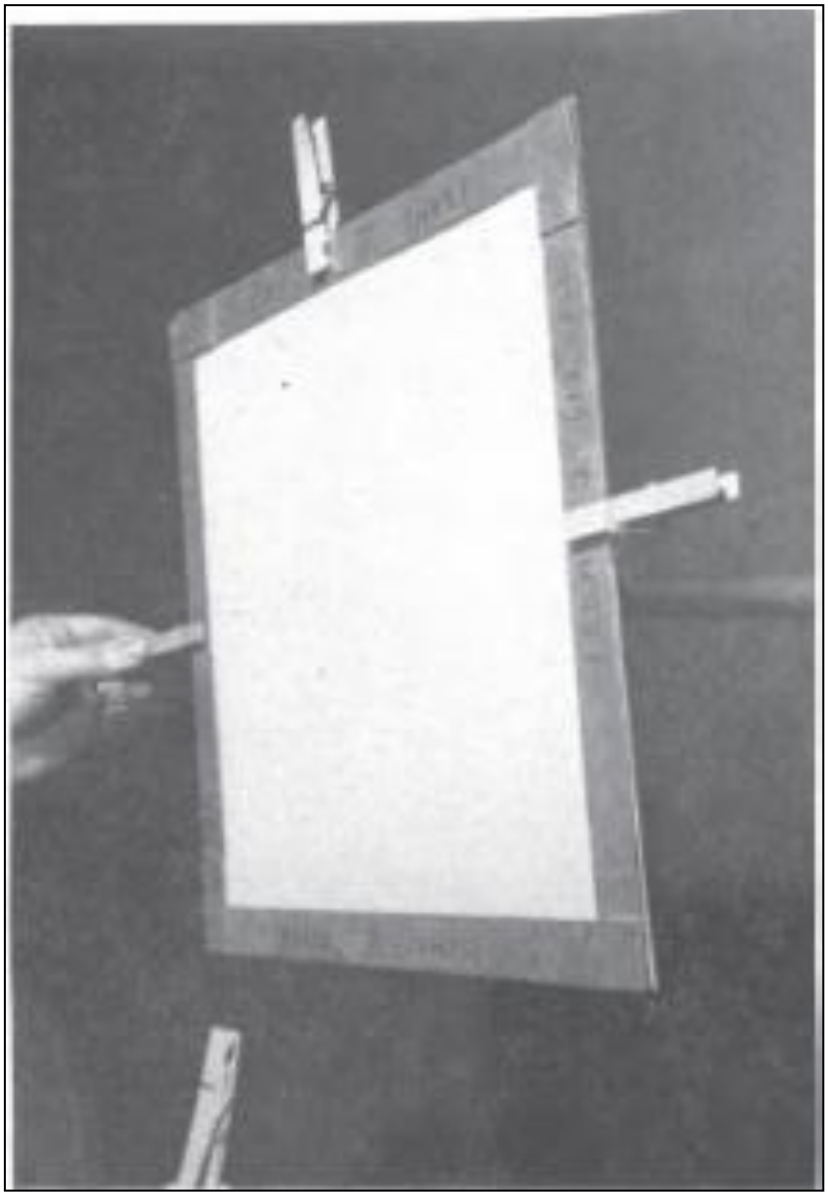
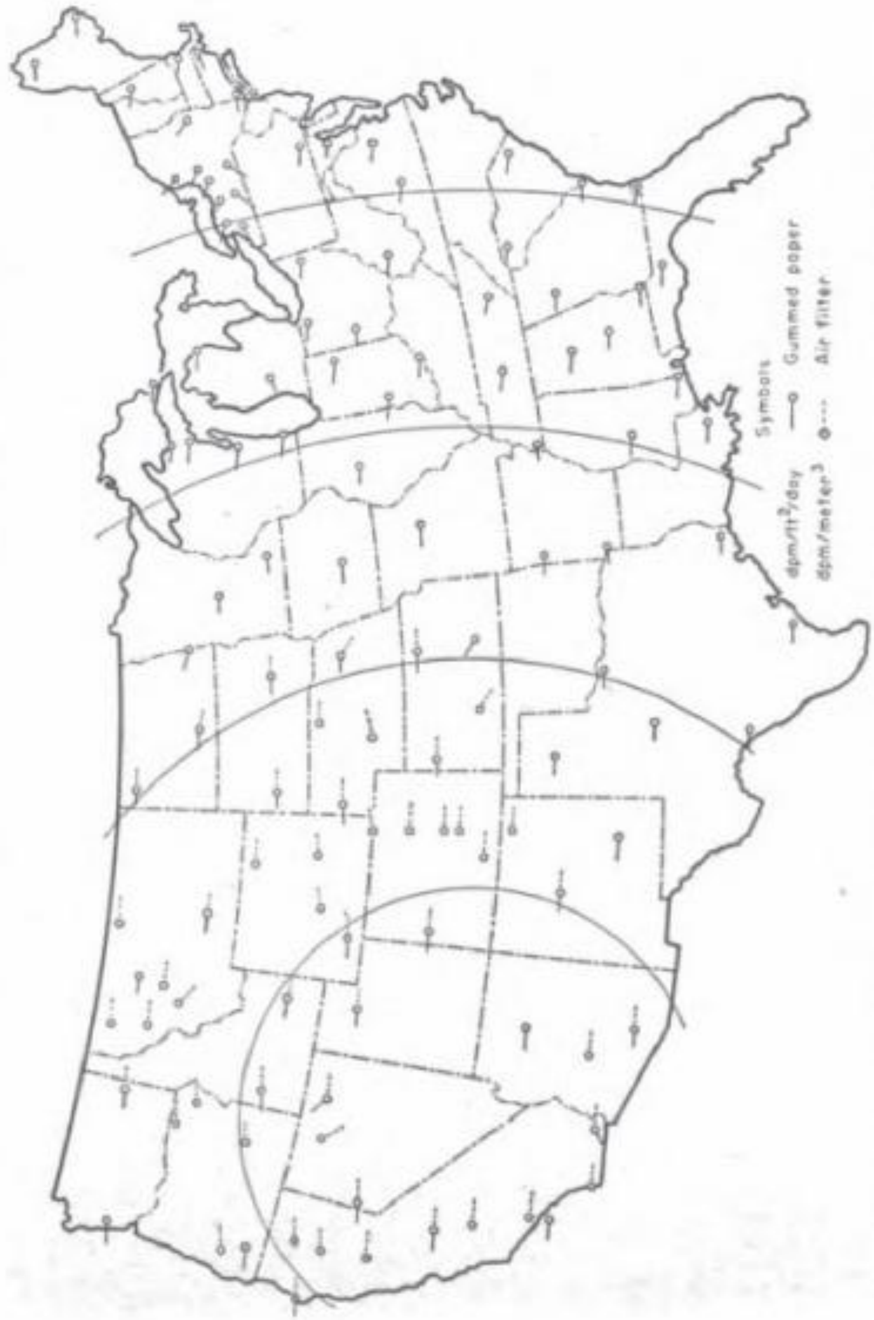


Figure 24 - Gummed film stand. Source: "Thirtheenth Semiannual Report of the Atomic Energy Commission." Washington D.C.: Atomic Energy Commission, 1953. Public domain.



Locations of the 121 fixed monitoring stations are shown on the above map. The symbols indicate whether the stations use air filters, gummed paper or both to collect fall-out samples. The arcs are 500 miles apart.

Figure 25 - Location of gummed film and air monitoring stations. Source: "Thirteenth Semiannual Report of the Atomic Energy Commission." Washington D.C.: Atomic Energy Commission, 1953.

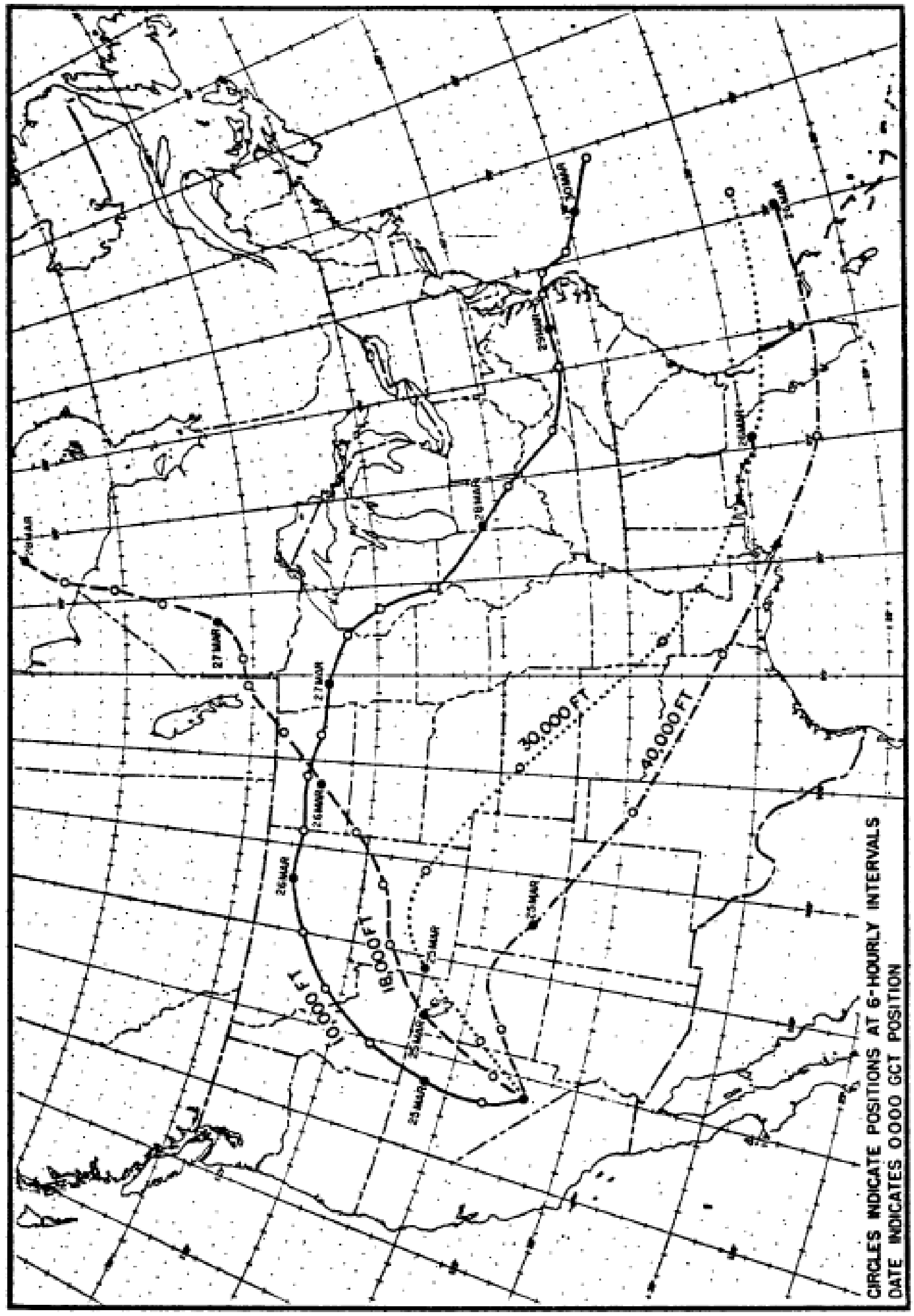


Figure 26 - Aerial trajectory map for a shot during the Upshot-Knothole series, NTS, 1953. Source: Robert J. List, June 25, 1954, "The Transport of Atomic Debris from Operation Upshot-Knothole," Report NYOO-4602 (Deleted), NTA, NV0011554. Public domai

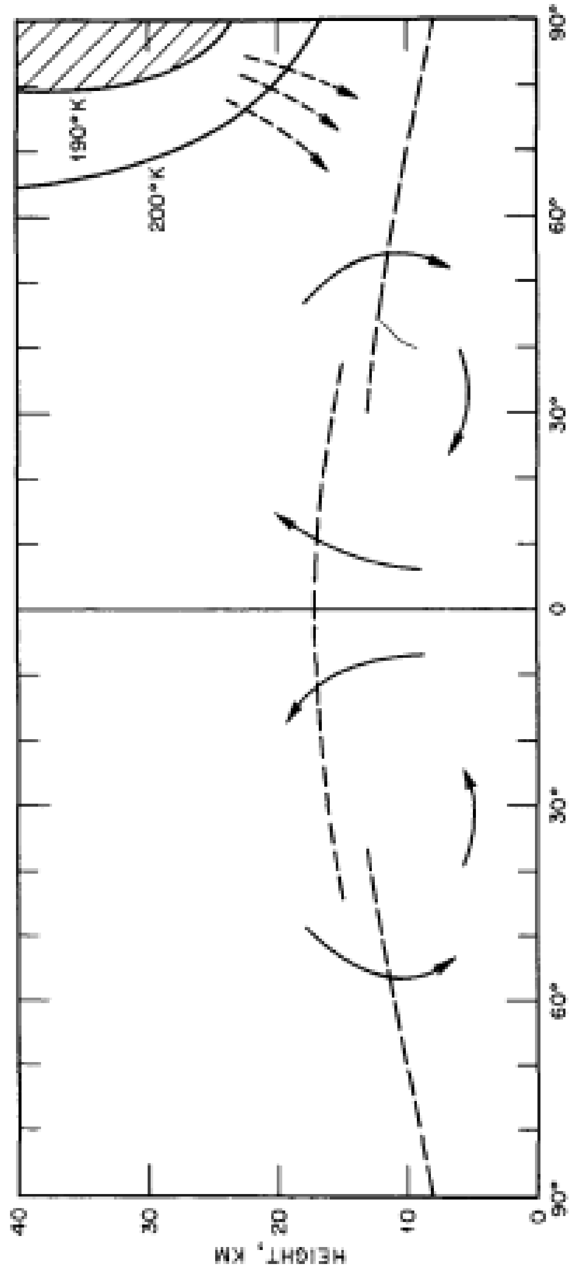


Figure 27 - Bewer-Dobson stratospheric circulation theory. Source: Machta, Lester. "Discussion of Meteorological Factors and Fallout Distribution." In *Environmental Contamination from Nuclear Weapons Tests*, Report no. HASL-42. Washington D.C.: Atomic Energy Commission, 1957. Public Domain.

A RADIOACTIVE DEMOCRACY:
SCIENCE, ENVIRONMENT, AND THE WESTERN MONTANA SCIENTISTS' COMMITTEE FOR RADIATION INFORMATION

On October 20, 1960 a group of University of Montana scientists led by botanist Meyer "Mike" Chessin and zoologist E.W. "Bert" Pfeiffer gathered to form a local scientific information group committed to the dissemination of radiation and fallout information to the citizens of Montana. Science information groups sprung up in cities across the country in the late 1950s and early 1960s largely in response to growing concerns about the possible health effects posed by radioactive fallout from the U.S. Atomic Energy Commission's (AEC) nuclear detonations at the Pacific and Nevada test sites. Like the more national fallout information groups such as the Committee for Nuclear Information (CNI) founded in part by Barry Commoner, the Western Montana Scientists' Committee for Radiation Information (WMSCRI) as the University of Montana group named themselves, abhorred what they felt was the AEC's cult of expertise. The AEC, whose legislative mandate charged the agency to both promote and regulate atomic energy, assured Americans throughout the decade of the 1950s that fallout was for the most part harmless, couching the possible radiation exposures attending the tests as not dissimilar to what an American might receive from a chest x-ray. In the absence of any other regulatory agency to suggest otherwise, the public by and large had to take them at their word.

Nevertheless, by the late 1950s, the hazards associated with fallout no longer seemed so familiar, or comforting. The most alarming hazard, it soon became apparent, derived not from external exposures from gamma ray radiation, but from specific radionuclides like strontium^{90} and iodine^{131} which, once in the environment, assimilated into food chains posing an internal hazard to the human body. Faced with the specter of a contaminated food supply, the WMSCRI endeavored to enlighten Montanans of the hazard, believing that "it is the special responsibility of scientists to provide the community with pertinent, objective information, which is the necessary basis for a sound public policy."[1] Benign as such a democratic vision might be, the WMSCRI faced considerable challenges from an AEC resentful of having its authority questioned and from a politically charged cold war atmosphere where the line between activism and objectivity could not be so starkly drawn. WMSCRI, nonetheless endured, helping to win the Limited Test Ban Treaty in 1963 and, furthering the scope of their original mission, participated in the launching of the Scientists Institute for Public Information (SIPI), a group devoted to distributing scientific information regarding other problems associated with material progress.

Michael Egan has written recently of Barry Commoner and his role in the birth of the science information movement during the fallout controversy. According to Egan, the science information movement constituted a "novel apparatus" that restructured environmental activism in the late 1950s and 1960s. "That apparatus," he writes, "consisted of the importance of dissent; the dissemination of accessible technical information; and the need for more public discussion of environmental risk. The

[1] "Minutes of meeting of Western Montana Scientists' Committee for Radiation Information," October 20, 1960, Meyer Chessin Papers (hereafter Chessin Papers), Archives and Special Collections, Maureen and Mike Mansfield Library, The University of Montana, Missoula (hereafter UMSC), box 22, folder Western Montana Scientists Committee for Public Information.

adoption of this apparatus and its effective use were the mechanisms of Commoner's science of survival—method and practice—and constituted the remaking of American environmentalism."[2] I agree. But rather than recapitulate the history and insights that Egan offers in his telling of the origins of the science information movement during the fallout period, this chapter treats Pfeiffer and WMSCRI as case studies to explore the ways that biological or ecological thinking about fallout hazards were communicated to the public at the local level. As such, this chapter provides a useful reminder that the growing culture of scientific dissent (largely biological) against industrial production and the culture of technocracy and secrecy pervading the U.S. government and military was not only expressed in the metropolitan core at hallowed institutions like the AAAS or the National Academies of Sciences. Rather, significant and transformational scientific resistance was also apparent in the periphery, where many of the consequences of nuclear security were most keenly felt. Indeed, places like Montana in the intermountain West were positioned squarely on the front lines of Cold War defense; these were places that disproportionally housed the facilities where nuclear weapons were made, readied for deployment, and tested. During the late 1950s, of course, many people saw these militarized landscapes as unique social and economic opportunities. Yet, it was precisely through the efforts of groups like WMSCRI to illuminate the local health effects of nuclear fallout that the costs of the nuclear arms race in the West would first be audited. By providing a more ecological view of fallout, WMSCRI offered Montanans an alternative to the deeply modernist focus of the AEC on external hazards. As a result, the benefits of nuclear testing to national security no longer seemed so unalloyed.

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[2] Egan, *Barry Commoner and the Science of Survival: The Remaking of American Environmentalism*, 5.

This chapter begins by exploring how Bert Pfeiffer first got his start as a voice in the science information movement when he began investigating a series of fallout episodes in North Dakota where he was a faculty member at the University. Although the public knew little of this fallout, by gathering data and reports generated by the AEC, he demonstrated significant levels of strontium-90 were accumulating in local milksheds. In the next section, I explore how Pfeiffer's work formed a critical first salvo in the science information movement by pointing to the critical need for publically and readily available fallout data and for more-biologically-centered approaches to their interpretation. I also show how Pfeiffer was part of a growing and coordinated force of scientists (including Commoner) and journalists devoted to combating the official AEC estimates of the fallout hazard. Finally, I discuss Pfeiffer's move to the University of Montana and the creation of WMSCRI as a local branch of the science information movement and the importance of their work in informing Montanans of the hazards of ingested radiation.

Offsite Fallout: Bert Pfeiffer and the "Hot" Upper Midwest

In the summer of 1957, portions of the upper Midwest were blanketed with fallout radiation. The main culprit appeared to be a shot (Diablo) during the Operation Plumbob series on July 15th. As the Diablo cloud travelled east over the Black Hills of South Dakota the following day, it intersected with a rain storm near Belle Fourche just north of the mountain range. John Willard, a chemistry professor at the South Dakota School of Mines in Rapid City and AEC consultant, was in Belle Fourche taking radiation readings while tracking the cloud as the rain began. The radiation levels he recorded shocked him. Radiation levels he measured outside his car as he drove around the area showed levels as high as 2,500 times normal background. In one spot, he figured that the total radiation exposure to be roughly 1 roentgen per 72 hours, a figure that in three to four weeks would constitute a permissible dose for a total year. "It was just hotter than hell," he later recalled.[3] It stayed hot too. Gamma radiation levels had hardly abated when fallout from another shot (Stokes) was detonated on August 7.[4] As alarming as the external readings that Willard recorded were, he took special note of the potential that contaminated food supplies might have for internal exposures. In addition to "washing every goddamn gutter in town," Willard advised ranchers to avoid feeding locally-grown hay to dairy cattle, ordered them to dump their milk, and arranged for an alternative supply from Denver and Sioux Falls to be shipped in.[5]

On August 9, Willard, who prior to the fallout incident happened to have scheduled a training meeting at the NTS, flew to Nevada and there met with a special AEC fallout team from the University of California Los Angeles Atomic Energy Project (AEP) to discuss the fallout readings. Willard relayed his readings to the team, recalling later that they accused him of being overly alarmist. Nevertheless, the following week the AEP group set out for the Dakotas to take a look for themselves. Based primarily on an aerial survey of the region, they concluded that radiation levels were roughly one to

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[3] Springer, "Forgotten Fallout: What Is the Legacy of the Radioactive Rains?," 2.
[4] Ibid. See also Kermit H. Larson et al., August 27, 1957, "Preliminary Report on 'Rain Out' from Shots Diablo and Stokes in the Belle Forche-Rapid City, South Dakota Area," NTA, NV0724181. This document is also in The Stafford Warren Leak Administrative Files, University Archives, Charles E. Young Research Library, University of California Los Angeles, Los Angeles, CA, box 36, folder AEP August-December 1957.
[5] Ibid.

one and a half times background. Three team members performed a ground survey along some of the local highways and their readings seemed to show low-level activity as well.[6] Subsequently, the Commission disregarded Willard's findings as "meaningless" and pressured him to withhold further radiation readings to the press, lest the public become alarmed. He agreed and kept the fact that he ordered milk to be dumped secret for over thirty years.[7]

Despite AEC suppression of Willard's findings and precautionary actions, the seriousness of the fallout episode would reach the public when in May of 1958 (nearly a year after the Diablo fallout) the *Grand Forks Herald* revealed that the AEC had recently sent Lamont Geological Laboratory scientist J. Laurence Kulp to the upper Midwest to collect bone samples for strontium-90 analysis. The primary impetus for the sample collection, according to the story, originated in the alarmingly high amounts of strontium-90 the AEC had detected in its milk sampling network in Mandan and Fargo North Dakota. In the interview for the story, Kulp reported (apparently reluctantly) that strontium-90 levels in North Dakota milk were four times the global average.[8] When asked why that was so, Kulp explained that low calcium in the regional soils (thereby making strontium-90 more bioavailable) or recent Russian tests were the likely causes.[9] In a later article, the director of the North Dakota Health Department Willis van Heuvelen, attempted to reassure North Dakotans that, despite Kulp's figures, the hazard

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[6] Ibid.
[7] Ibid.
[8] A subsequent story from the *Minneapolis Tribune* later in the month, told its readers that "the study was to be kept off the record." Quoted in E.W. Pfeiffer, "Letter to the Editor: North Dakota and the Non-Informative AEC," *Nation* 1959.
[9] Declassified memos show that the AEC thought that the NTS fallout was at least a partial cause. Charles Dunham to Distribution List, May 14, 1958, NTA, NV0710364; Charles Dunham to Members of Advisory Committee for Biology and Medicine, May 28, 1958, NTA, NV0711604.

was small. Adopting rhetoric characteristic of the AEC, van Heuvelen emphasized the fact that the strontium concentrations were lower than the permissible limits established by the Commission. “There was no reason to shun milk,” he told the reporter, “since the concentrations of strontium-90 found by the AEC were not in the dangerous range.”[10] Neither Kulp nor van Heuvelen broached what was perhaps a more plausible explanation for the high radiation levels—nuclear fallout from NTS tests was disproportionately settling in the upper Midwestern states. One who did immediately suspect the NTS was E.W. "Bert" Pfeiffer, a newly-hired professor at the University of North Dakota Medical School.

Bert Pfeiffer had spent the previous two years as a professor at Utah State College working with chemist Norman Bauer on, among other things, fallout.[11] A political firebrand with a natural distrust of authority, Pfeiffer was a firm believer in the democratic value of open and free science, ever ready to take on any person or organization which in his mind sought to denigrate science for untoward social effect. While at Utah State, for example, Pfeiffer wrote a seething piece on the biological warfare program at nearby Dugway Proving Grounds in the western magazine *Frontiers: The New Voice of the West*, which was edited by famed leftist journalist Carey McWilliams. Decrying the “monstrous distortion of the educational process to encourage students to work in the fog of secrecy and nihilism that surrounds such antisocial and amoral activities as BW [biological warfare] work,” Pfeiffer’s article won him considerable notoriety among the military brass at Dugway. After reading the article,

[10] Quoted in Gamarekian, "Serious Fallout Cases Uncovered in Middle West."
[11] Utah State was known as Utah Agricultural College at the time.

military officials there forwarded information they dug up on Pfeiffer's leftist politics to J. Edgar Hoover in hopes the FBI director might open an investigation.[12]

Fallout, more than biological warfare, dominated Pfeiffer's attention at the time, however.

After learning from Utah Senator Arthur Watkins that he could request data from the Salt Lake City branch of the Public Health Service (PHS), he wrote the agency on October 17' 1957 requesting any available information on their method of data collection, the total amount of exposure to Utah residents during the testing series, and the radiation levels in milk and vegetables throughout the state.[13] Pfeiffer received a response in January of the next year from Oliver Placak the officer in charge of the PHS's offsite monitoring group at the NTS. Apologizing for the two and a half month delay in writing back, Placak responded with the data requested for southern Utah and noted that his team only conducted monitoring and surveys of fallout within a 300 mile radius of the NTS (thereby excluding Salt Lake City). In the event that Pfeiffer should want further information on the Plumbbob operation, Placak forwarded a copy of the testing series operations plan and ended his letter writing, "If we can furnish other information or clarify this in any way, please call on us."[14]

When Pfeiffer did call on Placak again two weeks later he hit a bit of a roadblock. After reading the Plumbbob operations plan, Pfeiffer took note that the plan specified that PHS medical and veterinary officers were "on duty to investigate reports of incidents

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[12] Minor K. Wilson to Director, Federal Bureau of Investigation, October 11, 1957, "Magazine Article Discredits Army Research Activities," E.G. "Bert" Pfeiffer Papers, Accession no. 2009-006, UMSC, box 2. This collection has not been fully inventoried as of 2012.
[13]E.W. Pfeiffer to U.S. Public Health Service, October 18, 1957, NTA, NV0151779.
[14] Oliver R. Placak to E.W. Pfeiffer, January 3, 1958, NTA, NV0151782, 5.

attributed to radiation injury."[15] Pfeiffer wrote to Placak on January 14 requesting information on these officers and a copy of their reports which "would indicate if any incidents occurred."[16] Placak responded two weeks later, informing him regretfully that he could not transmit copies of their reports and appealed to Pfeiffer's sense of medical ethics by way of explanation. "As a doctor," Placak reasoned, "you will appreciate that even under these circumstances there is a patient-doctor relationship in that these reports contain personal clinical histories of these people." Placak, apparently, was unaware of redaction. In any event, Placak hoping to assuage Pfeiffer, wrote "there was no authenticated case of radiation injury to a human and only one known case of a potential injury to an animal."[17] Placak's reticence to provide the reports to Pfeiffer, however, proved less about medical ethics than institutional furtiveness as the last two lines of his letter revealed. "We have no record in our correspondence for your reasons for desiring this information or the uses you have in mind. It would be appreciated if you would enlighten us about these matters."[18] No enlightenment would be forthcoming; no documentation exists suggesting Pfeiffer ever wrote back.

Ironically, Pfeiffer's correspondence with Placak on Utah fallout levels transpired during his first year at the University of North Dakota, a move which was precipitated, in part, by concern that his family was being irradiated by their westernmost neighbor.[19] One can imagine his shock when realized he had stepped into a radiological hornets nest

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[15] E.W. Pfeiffer to Oliver R. Placak, January 14, 1958, NTA, NV0151783.
[16] Ibid.
[17] Oliver R. Placak to E.W. Pfeiffer, January 29, 1958, NTA, NV0151778.
[18] Ibid.
[19] Mea Andrews, "'Swan Song': Professor Finally Vindicated after Years of Nuke Warnings," *Missoulian*, January 29, 1995.

when the AEC bone collecting story broke. Accordingly, his efforts to obtain fallout data soon turned toward his newly adopted state.

Pfeiffer's motivation for investigating the Dakotas fallout, however, was driven as much by his politics as his concern for his family's wellbeing. As a scientific socialist, he was well attuned to the social impacts of scientific and technological progress, especially with regard to the military applications of science and technology. Consequently, Pfeiffer felt he had a special duty as a scientist to warn his fellow citizens about the risks they were being asked to assume in the name of the Cold War. As he wrote in 1958, "it is difficult to understand how the people of North Dakota can intelligently express their opinions on the vital question of the continuation of nuclear weapons tests…if they do not have information concerning the results of these tests."[20] Indeed, for Pfeiffer, the question of fallout was ultimately about democratic participation in national security policy. And public participation in the literally "vital" question of testing hinged on the dissemination of honest and readily available knowledge of fallout levels and their potential health costs to Americans.

Pfeiffer first attempts to obtain that information for North Dakota began with contacting the main players in the newspapers articles, especially van Huevelen the state health director. Van Huevelen eventually directed Pfeiffer to contacts in the AEC, Weather Bureau, and the Public Health Service.[21] By August, Pfeiffer had contacted a number of individuals in the Commission, including the AEC's Health and Safety Laboratory (HASL) which was responsible for collecting and interpreting fallout data

[20] E.W. Pfeiffer, "Some Aspects of Radioactive Fallout in North Dakota " *North Dakota Quarterly* 24, no. 4 (1958): 93.
[21] Pfeiffer, "Letter to the Editor: North Dakota and the Non-Informative AEC."

across the nation. Although he received a few reports and data for fallout levels in North Dakota from HASL, Pfeiffer grew increasingly frustrated at the slowness of the analysis and reporting. In one letter to the AEC's Public Information Officer on September 26 he wrote, "I would greatly appreciate receiving the raw data as soon as possible for the North Dakota stations as I hope to make use of these data in a grant application. I am surprised that it takes so long to publish these data. According to an AEC release dated May 13, 1957 monitoring of different areas of the US, including North Dakota, was carried on 'to keep the public informed on levels of radioactivity.' This implies informing the public at least before the end of the year, I should think. It is now over a year since the end of operation PLUMBBOB." Sarcasm was only one of Pfeiffer's tactics to goad the Commission into releasing data. In the same letter, Pfeiffer concluded with a thinly veiled threat: "Do you think that it would speed up the release of this information if I informed Senator Langer [of North Dakota] of the situation? He has been most helpful in the past."[22]

Pfeiffer's letter writing and information gathering won him no friends, particularly within the North Dakota public health establishment. He later accused van Huevelen, for instance, of scolding him and threatening to have him fired from the university as he was untenured at the time. Van Heuevelen later denied any such actions but did acknowledge that many resented Pfeiffer as an agitator and alarmist, especially given that he was an outsider without training in radiological effects.[23] No doubt Pfeiffer's biting sarcasm and aggressive pursuit of data, which may have been viewed by some as paranoid, played a

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[22] E.W. Pfeiffer to Grace M. Urrows, September 26, 1958, NTA, NV0132987.
[23] Springer, "Forgotten Fallout: What Is the Legacy of the Radioactive Rains?."

part in this reaction to him. Nevertheless, he had good reason to be suspicious that data collected by the AEC was being withheld. In November, Pfeiffer caused a bit of a stir within the DBM after he requested that they send him the preliminary report that the UCLA AEP group had written shortly after Willard's discovery of fallout in the Belle Fourche region. When asked what to do with the request, Gordon Dunning, a high ranking official within the DBM recommended that the "any reply be coordinated with Dr. Dunham [the DBM chief] to avoid possible embarrassment." The source of the embarrassment stemmed from the fact that the AEC had not forwarded the report along to South Dakota Senator Francis Case when he had requested information on the Plumbbob fallout a year and half previously. If they granted Pfeiffer's request, they would have been in a position of forwarding him a report that Case should have received months prior. This predicament left Dunning with three options: either send the report to both of them now, refuse to send the document and risk Pfeiffer's ire since the document was not classified as either confidential or "for official use," or inform them that a "final" report would be soon forthcoming. Dunning advocated for the third option reasoning that the preliminary report was over a year old.[24] Sending a final report as opposed to the preliminary one written by the AEP had other unstated benefits, however. If they sent the preliminary report, both Case and Pfeiffer would have learned about the extent of the fallout levels detected initially by John Willard that the AEC had later deemed "meaningless." Waiting to send a final report would have enabled the AEC to "sanitize" Willard's findings. After all, even though the preliminary report was over a year old the

[24] Gordon M. Dunning to R.L. Corsbie, November 18, 1958, "Request for Report on Radiation incident Belle-Fourche-Rapid City, South Dakota," NTA, NV0070871.

basic data collected was not going to change. This is all speculation, of course, but the incident nonetheless reveals the extreme slowness and reticence of the AEC to release data on fallout, whether motivated by their desire to cover-up the severity of the hazard or from genuine dismissal of Willard's data. In any event, no report appears to have been sent to either Case or Pfeiffer. A statement on the Dakotas hot spot would eventually be forthcoming, but not until the JCAE requested one in 1959 when Pfeiffer published a paper on the subject.

Throughout all his correspondence with the PHS and the AEC, Pfeiffer kept his cards close to his chest. Although he told the AEC that he wanted the fallout data for a grant proposal, in truth he was gathering materials to for a special report he was writing on the nature and severity of the Plumbbob fallout for North Dakotans. In fact, by the time he was writing to the AEC to obtain the preliminary Belle Fourche report, he had already accumulated enough data on radiation levels in milk, water, and soil to to write his article. (He was, however, still waiting for the bone data that Kulp had collected in May). That fall, he published "Some Aspects of Radioactive Fallout in North Dakota" in the *North Dakota Quarterly*.

Pfeiffer opened his article with an inscription from a recent resolution of the American Advancement of Science (AAAS) published in the journal *Science*. It read, "It is our further task to help in the transmission and translation of this knowledge [of the effects of radiation] to the public, for the final and effective decisions on nuclear control must be made not by scientists alone, nor by the military, but by all citizens." Pfeiffer meant the inscription as a shot across the bow of the AEC. In order to make informed decisions about the value and risk of nuclear weapons testing, the public had to have facts. The paper, he argued, therefore "presents 'for the record' some of the results which have been conducted on radioactivity from fallout in North Dakota."[25] One graph that Pfeiffer compiled from data he received from his correspondence tells the story. Within the Dakotas, the AEC ran four milk monitoring stations at Vermillion, SD; Williston, ND; Fargo, ND; and Mandan, ND. From June to August as the AEC conducted the Plumbbob shots, strontium-90 in milk at those stations rose precipitously. More alarming, however, was the great degree of variation in the rise of the levels between the stations. While Vermillion registered 5 strontium units (S.U.) in July, Mandan milk counts were upwards of 35 S.U.! Should anyone doubt that the shots from the NTS were to blame, Pfeiffer included a map obtained from the Weather Bureau depicting the radioactive cloud from shot Diablo traveling over the Dakotas on July 17.

Having concluded that sections of the Dakotas were hot spots of fallout activity, what was the biological significance of these results? The AEC's maximum permissible concentration standard for strontium-90 in foods was 100 S.U. Therefore, the "average level of Sr-90 in the milk processed at Mandan has been approximately 20 Strontium units for the year…This is one-fifth the present maximum permissible concentration."[26] The figures for "very young children in certain farms of the Upper Mid-west who drink only local milk," he warned however, "may now have Sr-90 bone burdens which are higher than those indicated by average milk levels because these averages are derived

[25] Pfeiffer, "Some Aspects of Radioactive Fallout in North Dakota ": 93.
[26] Ibid.: 98.

from pooled milk samples."[27] In light of these uncertainties, data from Kulp's bone collecting trip in May "will be of the utmost significance" in determining what the strontium-90 body burdens are for North Dakotans drinking contaminated milk.[28]

In this single article, Pfeiffer accomplished what the AEC had failed to do—provide North Dakotans with the general levels of strontium-90 that they were being exposed to and their potential risks. The only question that remained was the human bone analysis. If readers of Pfeiffer's paper hoped Kulp's data would be shortly forthcoming, they would be disappointed. By spring of 1959, nearly a full year had passed since Kulp's visit and no word on the strontium analysis had been published or disseminated to the public. Pfeiffer, ever wary of the AEC's penchant to dissemble, wrote to the Chairman of the JCAE Clinton P. Anderson on March 22 to find out what the hold-up was. Earlier in the year, Pfeiffer told Anderson, he had written Kulp's Lamont Lab inquiring when the analysis might be completed. When told that the analysis would be ready in a couple months, Pfeiffer anxiously awaited for correspondence from the lab delivering the results. None came, so two days before he wrote to Anderson, he contacted the lab again and learned that the analysis was delayed due to "contamination" at a lab where Lamont had forwarded the samples to. "It is evident," Pfeiffer told Anderson, "that something is peculiar about this investigation." Hoping that Anderson would apply some pressure on the AEC and Lamont to speed up the analysis, Pfeiffer ended his letter recommending "that your committee on atomic energy investigate the situation at Lamont Geological Laboratories with respect to the

[27] Ibid.
[28] Ibid.

North Dakota bone analysis."[29] On April 7, Anderson had JCAE executive director James Ramey write a reply informing Pfeiffer that they were looking into the delay and notified him that a JCAE hearing on fallout was already scheduled for May. Upon hearing this news, Pfeiffer apparently contacted North Dakota Senator Langer of his interest in attending the hearings. Langer agreed and wrote to Anderson to express his desire to have Pfeiffer appear before the JCAE.[30]

Pfeiffer never did testify. Nor was the hot spot problem discussed at any length at the hearings. Nevertheless, shortly following the hearings the chairman of the JCAE's Special Subcommittee on Radiation Chet Holifield wrote the AEC asking them to prepare a statement addressing the hot spot and radionuclide problem, which, he felt, were "not sufficiently covered during the course of the hearings."[31] Nearly three weeks later, Pfeiffer received a letter from James Ramey notifying him that his *North Dakota Quarterly* paper would be included in fifteen-hundred page appendices to the hearings along with AEC commentary on the paper and discussion of the Lamont delay and results.[32] Appendix B, "'Hot Spot' Problem and Strontium 90 in Foods," marked the first instance in the public record where the AEC divulged a significant portion of its data and interpretations of the vexing issue. In various documents and tables included in the appendices, the AEC acknowledged the high fallout levels in the upper Midwest, but argued that despite such alarming high figures, it was "virtually certain that the chances for bone cancer or leukemia induction …would be extremely low even for higher body

[29] E.W. Pfeiffer to Senator Clinton Anderson, March 22, 1959, NTA, NV0100290.
[30] Clinton P. Anderson to Senator William Langer, April 20, 1959, NTA, NV0100293.
[31] *Fallout from Nuclear Weapons Tests*, 2114.
[32] James T. Ramey to E.W. Pfeiffer, June 5, 1959, NTA, NV0100295.

burden individuals."[33] They also prepared a response to Pfeiffer's paper. They agreed that the data he presented "are as factual as available data permit." They could not deny that much, after all, since the data was collected by the Commission. Nonetheless, they quibbled with Pfeiffer's interpretation of the potential health risks, arguing that it "seems unlikely that concentrations of strontium 90 in the bones of children will be as high as in the milk they drink."[34]

By mid-May, the AEC had the results of four of the twelve samples collected in North Dakota. In a letter included in the appendices dated May 13, Charles Dunham, the director of the AEC's Division of Biology and Medicine, apologized for the delay in analysis from Lamont which, he added, were out of their control. After noting that the four samples "represent too small a sample to provide a reliable indication of human uptake in the area," Dunham took special note of a three-year old child whose bone sample showed a concentration of 2.62 strontium units.[35] The figure, Dunham commented rather awkwardly, was "about 30 percent higher than the average of values observed in young children from other areas, [but] is *smaller than might be expected in this area*."[36] After the hearings and the subsequent analyses, the AEC never initiated a follow-up study investigating the relationship between strontium body-burdens and contaminated milk in North Dakota despite the overwhelming evidence presented after the hearing suggesting the critical need for one.[37]

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[33] *Fallout from Nuclear Weapons Tests*, 2116.
[34] Ibid., 2162-3.
[35] The maximum permissible concentration for strontium-90 at the time was 100 units of strontium per 1 gram of calcium.
[36] *Fallout from Nuclear Weapons Tests*, 2162. Emphasis added.
[37] According to Stephen McDonough, Pfeiffer conducted an independent analysis of an infant vertebrae which showed a strontium level of 4.7 S.U. Stephen L. McDonough, *Downwind in North Dakota: An*

In the meantime, news reports of the occurrence of these hot spots in the upper Midwest were making front page news. While the AEC was compiling the data requested by Holifield (and before the hearings and the appendices were published), Edward Gamarekian of the *Washington Post and Times Herald* wrote a piece critical of the JCAE's neglect of the hot spot problem in the June 7 edition of the paper. Gamarekian, who interviewed Pfeiffer for the article, decried the JCAE's unwillingness to tackle the problem of hot spots "despite the ominous note in reports to them from scientists in several Midwestern universities."[38] Throughout the lengthy article, Gamarekian skillfully relayed the events of the Midwest fallout, highlighting the AEC's troubling failure to report the incidents. In the final line in the piece, he wrote that "None of these cases were originally reported by either the Atomic Energy Commission or the U.S. Public Health Service, the two federal agencies with the greatest knowledge of the situation."[39] Pfeiffer too went public, hammering the AEC's lack of candor regarding fallout. In an editorial on May 23 in the *Nation*, Pfeiffer shared his own efforts to uncover the severity of the post-Plumbbob fallout. "Had it not been for the local papers," he concluded, "we never would have learned the state of affairs in North Dakota, despite the fact that it is one of the hottest areas in the country."[40] Gamarekian and Pfeiffer's scathing critiques marked merely an opening salvo of a much larger campaign attacking the credibility of the AEC.

Uncertain Legacy: North Dakota State Department of Health Abd Consolidated Laboratories' Study of Nuclear Fallout in North Dakota and Possible Health Effects (Bismarck: North Dakota State Department of Health abd Consolidated Laboratories, 1994), 74.

[38] Gamarekian, "Serious Fallout Cases Uncovered in Middle West." Pfeiffer appears to have been Gamarekian's primary source of information.

[39]Ibid.

[40] Pfeiffer, "Letter to the Editor: North Dakota and the Non-Informative AEC."

In fact, both the Gamarekian and Pfeiffer's articles might be characterized as a coordinated attack. Prior to and following the May congressional hearings, Gamarekian was in correspondence with Washington University botanist Barry Commoner regarding the controversy. A week after the hearings on May 17th, Gamarekian wrote Commoner belittling the hearings and advocated for a letter writing campaign to local and national newspapers and Congress members which, he hoped, "would at least make the public aware that the fallout hearings were a fraud."[41]

Commoner was an appropriate choice to coordinate the campaign. A year earlier, Commoner had helped found the Greater St. Louis Citizens' Committee for Nuclear Information (CNI) to act as a clearinghouse for data on fallout measurements and effects for dissemination to the public. CNI would become the most prominent group in the science information movement that sprung up in the late 1950s in response to the AEC's glaring inadequacy in providing timely and accessible data on fallout to the public. Eschewing activism in favor of scientific objectivity, CNI understood that the decision as to whether or not to test nuclear weapons was at its core a value judgment. But how one weighed the value of testing against the health effects hinged on what they knew about fallout. While CNI acted largely at the national level, numerous smaller groups affiliated with CNI emerged in cities across the country to provide information at the local level. One such group materialized in 1960 at Missoula Montana where Bert Pfeiffer had just relocated.

[41] Edward Gamarekian to Barry Commoner, May 17, 1959, Barry Commoner Papers, Library of Congress, Washington D.C. (hereafter LOC), box 326, folder Gamarekian.

The Science Information Movement and the Western Montana Scientists' Committee for Radiation Information

The origins of the science information movement can be found within the ranks of the American Association for the Advancement of Science (AAAS). As the premier scientific organization in the U.S., the AAAS was the principal organization through which scientists could voice concerns about the state of their profession. During the postwar years, there seemed much to be concerned about. Wartime scientific and technological successes like radar, the proximity fuse, and, of course, the atomic bomb highlighted the social and political utility of technoscientific knowledge. As a result of these successes, federal support for physical sciences boomed in the 1950s. Growing federal patronage and "military work," however, raised vexing questions: If technoscientific knowledge proved instrumentally useful to American policy and military might, what role did scientists have in confronting the potential social and political ramifications of their work? Were they ethically or morally culpable for how the knowledge and objects that they produced were deployed or applied? What was their responsibility for informing Americans of the costs of technoscientific progress? In 1955, the AAAS attempted to confront these questions when it formed the Interim Committee on the Social Aspects of Science to study the "present state of science in the United States and its relation to social forces and issues."[42]

After deliberating for nearly two years, the committee published their findings in the AAAS's flagship journal *Science* in 1957. "There is an impending crisis in the

[42] Barry Commoner, "Social Aspects of Science," *Science* 125 (1957): 143.

relationship between science and American Society," the committee concluded. "This crisis is being generated by a basic disparity. At a time when decisive economic, political, and social processes have become profoundly dependent upon science, the discipline has *failed to attain its proper place in the management of public affairs*."[43] By way of example, the committee cited a number of social problems arising out of technological progress including air pollution, natural resource depletion, and fallout. Despite recognizing their responsibility in these social and public health effects, the statement did little to suggest how scientists might rectify these issues or how to practically engage them. As the authors wrote in the last line of the article, "what is needed now is a way to meet it."[44]

The way to meet that challenge, many of the committee members agreed, was to confront a specific issue in practice. In 1957, no better or more difficult issue was to be had than the controversy over fallout hazards. The committee did just that at the urging of committee members Chauncey Leake and Barry Commoner. Over the next few months Commoner drafted a fallout statement to be presented to the AAAS board for formal adoption. Hoping to use the statement as an effective device to educate the public about the dangers of fallout, the statement Commoner drafted was never adopted. The problem lay in the uncertain nature of fallout hazards and the role that scientists should play in the debate over testing. The AAAS board wanted no part in furthering the controversy by making what they deemed as value-laden statements. Eventually,

[43] Ibid. Emphasis added.
[44] Ibid.: 147.

Commoner published his draft under his own name as "The Fallout Problem" in the May 1958 issue of *Science*.

Michael Egan has referred to Commoner's article as the "seminal document in the creation of the science information movement."[45] Certainly no other published document better encapsulated the philosophical underpinnings of the movement. Opening his article by recapitulating the AAAS interim committee's conclusion the year before, Commoner cited the fallout as perhaps the most troubling example of how the relationship between science and society was "far from harmonious." At the outset, Commoner notified the reader explicitly that the he would avoid discussing the question of whether or not there was a need to test. Those questions, he wrote, were "a fact of political life." Instead, Commoner focused on the hazards of fallout. Using the JCAE congressional hearings as his primary source of information, Commoner paid special attention to the uncertainty of the biological hazards and the wide range of interpretations about effects as a result. The fact that scientists disagree over the precise effects of fallout on human health, he told the readers, was not the source of public confusion over fallout hazards. Instead, public confusion arose because "scientists have been marshaled on both sides of the debate… violat[ing] science's traditional devotion to objectively ascertainable truth." Even then, the division was only partly a result of the uncertainties, Commoner continued. More importantly, it resulted from the dual role that scientists have played in the debate. Scientists interpret nature and communicate what he or she knows. But scientists were also informed citizens free to express themselves on that level, he explained. There was the rub. Scientists could estimate probable damages from

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[45] Egan, *Barry Commoner and the Science of Survival: The Remaking of American Environmentalism,* 58.

nuclear fallout, but in matters of policy they had no right to expertise beyond that of the informed citizen. "There is" Commoner wrote, "…no scientific way to balance the possibility that a thousand people will die from leukemia against the political advantages of developing more efficient retaliatory weapons. This requires a moral judgment in which the scientist cannot claim a special competence which exceeds that of any other informed citizen." For Commoner, the key word was "informed" and that was precisely what was lacking in the fallout debate. The AEC's monopoly on fallout data left the American public without the requisite information needed to make informed decisions. Moreover, as Pfeiffer had complained in his editorial to *Nation*, the unnecessary "lag in information" to the American public "has given the scientists their apparent monopoly on these opinions." Without direct public input, the alternative was an illegitimate plutocratic technocracy, which Commoner implied characterized the current situation with the AEC: "Unless the public has sufficient information to provide a reasonable basis for independent judgment, the moral burden for future effects of nuclear testing will rest on some smaller group. And no such group alone has the wisdom to make the correct choice or the strength to sustain it."[46] Commoner never took a political position in the article, although his opposition to testing had been made unambiguously clear in other venues.[47] Instead, as Egan has written, Commoner in the article "challenged the public to 'do the right thing.'"[48]

From the vantage point of today, Commoner's article hardly seems controversial. Yet given the politically charged atmosphere surrounding testing, it's no wonder the

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[46] Barry Commoner, "The Fallout Problem," *Science* 127, no. 3305 (1958).
[47] Commoner, for example, signed the Linus Pauling petition calling for a nuclear test ban in 1957.
[48] Egan, *Barry Commoner and the Science of Survival: The Remaking of American Environmentalism*, 59.

AAAS found it a bit radical to their liking. Commoner would continue to advocate for greater social awareness within the AAAS, but the board's rejection of his fallout statement convinced him that the AAAS was ill-suited to carry out the ideas he set forth in "The Fallout Problem."[49] For one, the board's unwillingness to ruffle feathers signaled the rather conservative nature of the organization. For another, given the populist sentiment permeating the article, Commoner needed to find a more grassroots venue to put his ideas into action. He began that endeavor in his own home of St. Louis.

Just as his paper was going to press, Commoner cofounded the Greater St. Louis Citizens' Committee for Nuclear Information (later reduced to the Committee for Nuclear Information or CNI, for short). Made up of both scientists from St. Louis area universities and citizens, CNI was the "The Fallout Problem" put into practice. Although there was some debate as to whether the group would take a particular political position on the fallout debate, the committee was and remained steadfastly neutral and non-partisan, just as Commoner had wished.[50] CNI's mission as stated in its monthly bulletin, *Nuclear Information*, was devoted to the "gathering and public distribution of facts about the effects of nuclear testing and the military and peaceful uses of nuclear energy." CNI disseminated much of its information through its publications and speaking engagements. Although many of these efforts remained local and distribution modest, CNI soon

[49] Commoner remained quite active in the AAAS, however.

[50] Egan, *Barry Commoner and the Science of Survival: The Remaking of American Environmentalism*, 60-1; Jr. William Cuyler Sullivan, *Nuclear Democracy: A History of the Greater St. Louis Citizens' Committee for Nuclear Information, 1957-1967, College Occasional Papers No. 1* (St. Louis: Washington University, 1982), 20-3.

garnered widespread readership when national publications began referring to their material as a way to sort through messy and complex nuclear issues.[51]

CNI also initiated an independent and quite innovative monitoring project to determine the amount of strontium-90 uptake in children. In 1958, the AEC—in response to pressure applied by the JCAE—agreed to publish the fallout levels detected in its monitoring system in quarterly installments. Although the reports detailed strontium-90 levels in water, soils, and milk at various stations across the nation, comprehensive data on accumulation in human bodies was still wanting, as the Kulp bone collecting trip demonstrates. Bones were quite simply hard to come by. They were not, however, the only calcium dependent human organ. In August of 1958, Herman Kalckar of the U.S. National Institutes of Health wrote an article in *Nature* proposing that an international assay of strontium-90 build-up in children's teeth be instituted as a way to bypass the problems associated with collecting adult bones.[52] CNI was immediately intrigued. Collecting baby teeth had obvious benefits over other sources like adult bones. First, assaying baby teeth would provide information of strontium-90 accumulation in populations uniquely susceptible to strontium-90 uptake owing to children's greater need for calcium and thus higher milk consumption compared to adults. Second, teeth were in abundant supply and could provide not only more quantity of materials for strontium-90 assay, but also more comprehensive geographical coverage. Finally, a baby tooth census appealed to CNI's politics. Collection of the teeth would not be responsible by some technocratic agency like the AEC, but rather depended upon the public actively engaging

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[51] Egan, *Barry Commoner and the Science of Survival: The Remaking of American Environmentalism*, 64.
[52] Herman M. Kalckar, "An International Milk Teeth Census," *Nature* 182, no. 4631 (1958).

in the census since it would be parents who donated their children's teeth "to science" [see figure].

CNI initiated the Baby Tooth Survey in December of 1958. CNI member and St. Louis physician Louise Reiss directed the effort. By 1961, CNI had collected 61,000 teeth, mostly from the St. Louis area. When Reiss published the preliminary findings in *Science* in 1961, the results confirmed CNI members' suspicions that strontium-90 levels in children's teeth was increasing.[53] None of the levels of strontium-90 detected were high enough to surpass the recommended permissible limit, however. For CNI, that fact was largely beside the point. What truly mattered was that the survey proved that the hazard did exist and that American were now aware of it.

While CNI's information dissemination campaigns and programs like the Baby Tooth Survey operated at the national level, a good measure of work as part of the science information movement was conducted at the local level. In addition to CNI, roughly twenty other groups emerged in cities across the country hoping to build upon the efforts of the St. Louis group within their own locales. None of these groups were formally aligned or associated with CNI. Nonetheless, a sort of informal liaison existed between them as each group would willingly share information or tactics with other groups in the object of getting as much information to the public as possible.

The Western Montana Scientists' Committee for Radiation Information (WMSCRI) was one such local group. Bert Pfeiffer, who in the summer of 1960 had arrived with his family to Missoula to accept a position in the zoology department at the

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[53] Louise Zibold Reiss, "Strontium-90 Absorption by Deciduous Teeth," *Science* 134, no. 3491 (1961).

university as an assistant professor, was the ringleader and public face of the group.[54] Ever the instigator, Pfeiffer had within a few short months of arriving succeeded in gathering a small coterie of like-minded colleagues at the university with the intent of forming a group akin to CNI in Missoula. In addition to Pfeiffer, attending the foundational meeting in late October of 1960 were fellow faculty members Meyer Chessin, Harry Fritz, Clyde Sanger, R.S. Hoffman, and Otto Stein, and Braun. The resolution adopted by WMSCRI at this founding meeting was drawn explicitly from a similar New York group and read like a précis of Commoner's "The Fallout Problem." "Recognizing the urgent problem which arise from military, industrial, and medical uses of nuclear energy…," the resolution began, "and recognizing that the development of public policy on these matters is the responsibility of scientists to provide the community with pertinent, objective information which is the necessary basis for development of a sound public policy, we urge scientists of all communities to develop such public information programs." In accordance with this mission, WMSCRI pledged that information to be disseminated must conform to the rules of "traditional scientific objectivity," that the group "maintain an independent status without abridging the right of individual members to express their own opinion," and finally that the information gathered "be made publically available to all." Besides offering periodic speaking engagements on relevant radiation issues, the Committee formed a subcommittee made up of three members to act as an "editorial board" with the purpose of writing news

[54] Meyer Chessin, interviewed by E. Jerry Jessee, April 20, 2011, recording in author's possession.

releases.[55] As the resolution suggests, WMSCRI was in effect, a smaller, more localized version of CNI.

Pfeiffer in many ways was the Barry Commoner of the group, albeit with a good deal more panache tinted by his uncompromising, perhaps bordering on overbearing, commitment to the committee's mission. This aspect of Pfeiffer's temperament would at times put him squarely within the crosshairs of some staunch anticommunists looking for any sense of subversive action within the ranks of any group appearing to dissent from U.S. policy. Nonetheless, he was an adept communicator and effective at mobilizing his colleagues to commit themselves to applying their knowledge beyond the confines of academe toward more socially productive ends. Meyer Chessin, for example, remembered Pfeiffer imploring him to "get active" when he first approached him about starting the radiation group.[56] Whatever reluctance Chessin may have felt about starting the group did not stem from any personal or political difference between the two. Both grew up in New York, albeit from starkly different social classes, and both held similar leftist politics.[57] Speaking out simply came naturally to Pfeiffer.

The two major orders of business in the first year or so of WMSCRI's existence centered on public speaking engagements by members of the committee and news reports generated for publication in local newspapers. In an example of the latter, Senger, Hoffman, and Pfeiffer provided staff reporter Lou Linley with source material for an

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[55] Minutes of Meeting of Western Montana Scientists' Committee for Radiation Information, October 20, 1960, Chessin Papers, UMSC, box 22, folder Western Montana Scientists Committee for Public Information.

[56] Meyer Chessin, interviewed by E. Jerry Jessee, April 20, 2011, recording in author's possession.

[57] Chessin and Pfeiffer grew up within five miles of each other. Chessin, the son of Jewish Russian immigrants, lived in an impoverished neighborhood in the Bronx. Pfeiffer, however, was the son of a wealthy corporate lawyer and lived in the stately suburb of Riverside. Ibid.

article on fallout in the local Missoula newspaper in December. Entitled “The Atom and Its Living Shade,” the article began by outlining a history of the development of nuclear weapons, including the hydrogen bomb. The bulk of the article, however, explored the nature of radioactive fallout and its effects on human health, paying special attention to the grass-cow-milk environmental pathway of strontium-90. One image in the article cartoonishly depicted the problem by showing a cow with electrons orbiting its body. The caption proved more illuminating. Titled, “Not So Benign as She Looks,” it read “After feeding on grass which has concentrated a quantity of strontium 90 fallout, ‘Bossie’ innocently chews her radioactive cud. Because of an affinity for calcium, the radioactive particles of Sr 90 will find their way into her milk, eventually into the grocery store and finally into the bones of an unsuspecting human.”[58] Despite this known pathway of radiation exposure, the article noted, Montana had only recently initiated a monthly milk testing program (the results were not in yet) and neither the state nor the AEC had yet conduct soil or other food monitoring or analysis.[59] That fact was rather alarming, the article implied, because of the tendency of plants to concentrate radioactivity, which Pfeiffer had documented in the state’s "hot" neighbor, North Dakota. It was an effective article. In addition to informing Missoulians of the strontium-90 hazard, it also contained a thinly veiled criticism of the AEC's monitoring program. Montana, despite its relative close proximity to the NTS, appeared to be a relative back hole in the AEC's monitoring system.

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[58] Lou Linley, "The Atom and Its Living Shade," *The Daily Missoulian*, December 4, 1960.
[59] See, for example, "Health Board Sampling for Radioactivity," *The People's Voice*, November 25, 1960.

Most of WMSCRI's public addresses were centered largely at local community groups like the PTA, Kiwanis, and churches. Usually Chessin and Pfeiffer paired up for these meetings, although Senger has been documented as a speaker in at least one meeting. As Meyer Chessin recalled in an interview, he normally began the talks with some basic facts about radiation and Pfeiffer followed with a discussion on health effects. Pfeiffer, according to Chessin, took up the "human angle" of the story because his facility at public speaking which was buoyed by his passion for the subject. Milk, naturally, was a frequent subject of the talks. When the issue was broached, WMSCRI members advised the audience not to drink it, or at the very least suggested using canned or dried sources—something both Chessin and Pfeiffer had been doing for their families for some time.[60]

Most of the community discussions that Chessin recalled were quite cordial and agreeable. In one instance following a talk at the First Christian Church in Missoula by Chessin, Pfeiffer and Senger, the pastor of the church felt compelled to write the president of the university to commend the three men for their talk, noting that "our community and state are enriched by such participation by our scholars and scientists in matters related to public information."[61] Not all were so pleased, however. Shortly after forming, two WMSCRI members who also happened to be professional radiologists left the organization. Chessin later chalked up their leaving to their discomfort with "any searching critique of the government."[62]

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[60] Meyer Chessin interview. Pfeiffer also toured eastern Montana discussing Sr-90 and wheat to farmers.
[61] J.E. Roberts to President Newburn, November 27, 1961, Chessin Papers, UMSC, box 20, folder Radiation Biology Correspondence.
[62] Meyer Chessin interview.

The radiologists' trepidation with the political implications of WMSCRI cut to a core dilemma of the science information movement. By their very existence, groups like WMSCRI constituted an inherent critique of the AEC; it could not be. By legislative mandate, the AEC not only developed nuclear energy, but was also charged with regulating its uses. The need for public availability of fallout information, and with groups like WMSCRI filling that role, put the science information movement in the position of regulator of the regulator. This was surely an odd place to be in and one that, as the radiologists resignation suggests, could make it difficult to straddle the line between objectivity and activism because, by its very nature, WMSCRI was suggesting that the government was not doing its job properly. The trick to gaining and maintaining public confidence lay in their efforts to remain as objectively neutral as possible. Groups like WMSCRI needed to simply provide the "facts" and allow the public to, hopefully, make the "right" choice.

But such a delicate balancing act could and did prove difficult. Pfeiffer, never one willing to mince words, often found himself the subject of attack from folks on the right for what they perceived as his activism, agitation, or worse, agitprop. To be sure, Pfeiffer was provocative. His politics was filled with righteousness and when confronted by people whom he perceived as the enemy of science or democracy, he could be quite caustic in response. Chessin, by far the cooler head and perhaps Pfeiffer's closest colleague at the university, often felt his role at speaking engagements was to keep

Pfeiffer from losing *his* head.[63] Still, Pfeiffer was deeply committed to the science information movement and the objectivity on which it was based.

One of the first attacks on WMSCRI came, somewhat ironically, from Lou Linley, the author of the fallout article that he had written with the assistance of members of the committee earlier in the year. In the sixth article of a series entitled "Campus Agitators Play Into Soviet Strategy," Linley branded WMSCRI as an organization which "carries certain of the earmarks of an agitating force." Although Linley acknowledged that WMSCRI's "avowed purpose…to present information on radioactivity to the public…is of vital importance," he argued that the committee's various public appearances at PTA's and the like seemed to suggest that they had "assumed the character of a political action group." Specifically, Linley cited WMSCRI's opposition to civil defense and fallout shelters. On this point, Pfeiffer was his main, although unacknowledged, target. As evidence of WMSCRI's supposed political agitation in opposition to fallout shelters, Linley pointed out that a WMSCRI officer had recently written a series of articles on civil defense and fallout in the *People's Voice*, a left-wing weekly newspaper published in Helena. The author of the articles was Pfeiffer and in them he criticized any notion of surviving a nuclear attack, shelter or no. From Pfeiffer's articles, Linley fallaciously concluded that "The Western Montana Scientists' Committee for Radiation Information apparently would have us throw down our arms and defenses and be totally at the mercy of our enemies."[64] As this last line suggests, Linley painted WMSCRI as both an agitating force and as an inept group of egg heads unwittingly

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[63] Meyer Chessin interview.
[64] Lou Linley, "Has Reason Gone Underground? Campus Agitators Play into Soviet Strategy," *The Daily Missoulian*, May 14, 1961.

playing into Soviet hands. That those two characterizations might constitute a contradiction was apparently lost on Linley.

In the articles for the *People's Voice* that Linley cited, however, Pfeiffer was quite careful to disassociate himself from WMSCRI so that he might be free to espouse his own political positions. "These articles," Pfeiffer declared, "are based upon technical data made available by the WMSCRI which offers all its information to all persons interested in the hope that it will contribute to a better informed public. The opinions on matters of governmental policy and on other controversial points are entirely those of the author and not necessarily those of the committee." For Pfeiffer and the other members of WMSCRI, this was precisely what the function of the science information should be; as scientists they collected and reported data and as citizens outside of that endeavor, they were free to make of it as they saw fit. All was right so long as members of WMSCRI did not conflate those two roles. Still, for casual readers of the *People's Voice*, the subtle differences that Pfeiffer was balancing might not be so apparent. For proponents of fallout shelters and nuclear weapons testing, Pfeiffer's balancing act was fodder for criticism.

Such was the case with Pfeiffer's second article in *People's Voice* in April of 1961. In the article, Pfeiffer described a potential nuclear war as the "ultimate catastrophe." Nevertheless, he argued, "there are uninformed or misinformed groups who are trying to convince the American people that it, as a nation, could survive a nuclear war." Pfeiffer specifically referred to Rand scientist Herman Kahn's widely read and influential (in policy circles at least) book *On Thermonuclear War,* but he was also no doubt implicating local civil defense planners too. Beyond the immediate devastation of a nuclear exchange, Pfeiffer argued a basic understanding of biology and ecology revealed that, even supposing one survived the initial attack, the lingering fire and radioactivity in the environment would render the landscape nearly completely unfit for human habitation, food production, or foraging. Despite this logic, Pfeiffer concluded, "there are those who are deluding people into thinking that they can survive a nuclear war through civil defense."[65] Could one survive the initial attack? Perhaps, Pfeiffer admitted. But what kind of environment would they face when they left the shelter? Only when one considered the conflagrated and irradiated world outside the shelter did the prospects of "civil defense" appear futile and absurd. His was an ecological view of a situation. You could not engineer survival in the face of nuclear annihilation.

Pfeiffer made similar points at some of the WMSCRI speaking engagements throughout the state and it was only a matter of time before he drew the ire of the Montana director of civil defense, Robert A. Keyes. In November 1961, Keyes happened to be in the audience at one of the WMSCRI meetings at the First Methodist Church in Great Falls. Furious at Pfeiffer's insistence that fallout shelters and civil defense in general were useless in the face of such destructive weapons, Keyes went public to refute Pfeiffer and the WMSCRI in an article in the *Great Falls Tribune*. The writer of the story quoted Keyes as remarking that WMSCRI were "hysterical and misinformed individuals who aren't doing Montana or the nation any good." Keyes was apparently particularly upset with Pfeiffer's claim that anybody (shelter or no) within a twenty mile

[65] E.W. Pfeiffer, "Radioactive Fallout, the Effects of Nuclear War, and Civil Defense," *The People's Voice*, April 28, 1961.

radius of ground zero would certainly die. This, said Keyes, was "misinformed" and referred to recent military maneuvers at the NTS where soldiers were placed within five miles of a weapons test and survived as evidence to Pfieffer's faulty statements. Keyes also claimed that ninety-seven percent of people in shelters in downwind areas would survive—a figure that given the evidence Pfeiffer provided in his *People's Voice* article regarding the long-term ecological effects of a nuclear war can only be regarded as specious at best. Keyes' statements were for the most part absurd, but his attack highlighted the precarious position that members of the science information movement placed themselves in when discussing matters of national policy such as civil defense.

Even Dorothy M. Johnson, a Missoula author best known for penning the story that was the basis for John Ford's *The Man Who Shot Liberty Valance*, weighed in on the Pfeiffer-Keyes flap. The disagreement between Keyes and Pfeiffer, Johnson wrote in an editorial in the paper, was a classic case of the old "damned if you do, damned if you don't" adage. "I am going to be one of those who do," she decided. "The people who say don't," she reasoned, "have nothing to offer but despair. With enough of that around, Russia won't need to use any bombs at all."[66]

Keyes' criticism of Pfeiffer and the WMSCRI, however, did not go unchallenged. A little over a week after the story ran in the *Great Falls Tribune*, the editor of *People's Voice* slammed Keyes and another WMSCRI detractor, Norman "Jeff" Holter, for their "smear technique and insinuations" in which they suggested that "any person who questions the infallibility of fall-out shelters is 'suspect' and, by some twist of reasoning, aiding Khrushchev." But perhaps the editor's most trenchant criticism centered on the

[66] Dorothy M. Johnson, "Fallout on Fallout," *Missoulian*, January 3, 1962.

proper role that civil defense planners like Keyes should take in matters of public policy. “By what fiat do they set themselves up as arbiters of the attitude their fellow citizens should take on this matter?” Civil defense policy was a public issue, and thus they had no special right or expertise beyond that. The same was true of course with WMSCRI. In WMSCRI’s case, however, their main goal was information, information necessary for Americans to judge for themselves whether fallout shelters were worth the cost. That was the main point of a letter that WMSCRI member Clyde Senger wrote to Keyes after reading the *Great Falls Tribune* article. “My own attitude has been and will be that if our local civil defense people are going to give this area obviously incorrect and misleading information, I was going to try to present scientifically objective answers when asked to talk on the subject. I suspect,” Senger continued, “that the civil defense program here in the state as well as the national program will continue to be attacked by some of our leading scientists as long as individuals in charge of the program continue to disregard the advice of these scientists.”[67] Senger was clearly referring to Pfeiffer here, but he was also likely alluding to recent statements made by AEC environmental branch chief John Wolfe.

In September, Senger, Chessin and Pfeiffer attended the keynote address delivered by Wolfe at a radioecology symposium sponsored by the AEC in Fort Collins, Colorado. Wolfe began his talk by discussing the ecological relationship that linked humankind to their environment. In classic ecological fashion, Wolfe elaborated the ways that humans have altered their environment in order to prosper and thrive.

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[67] Clyde M. Senger to Robert Keyes, November 29, 1961, Chessin Papers, UMSC, box 20, folder Letters about Radiation Biology.

Sometimes those changes resulted not in progress, but instead wrought severe environmental degradation forcing the societies embedded in that environment to either adapt or perish. That part of his talk seemed straightforward. The advent of the atomic age and its attendant weapons of mass destruction, he continued, had presented humankind with the possibility of so devastating the environment that adaption might prove impossible. "The effects of nuclear war on man and his environment are awesome to contemplate," he told the audience. "Thermal and blast effects, and concomitant radiation, would create vast areas that would be useless to the survival of man." With this long-term environmental view point in mind, Wolfe then pointed to the folly of civil defense. "Fallout shelters in many areas seem only a means of delaying death and represent only a part of a survival plan. With an environment so completely modified, the questions is where does man go after his sojourn in shelters? What does he do upon emergence?"[68] A post nuclear environment offered no hope of restoring ecological balance, merely death.

The essence of Wolfe's keynote address spread quickly following the symposium. Colorado State University, who hosted the symposium, issued a press release detailing Wolfe's message the day after his address. Pfeiffer was so thrilled with the talk that the week following the conference he wrote to CNI member Dr. Florence Moog encouraging her to take up the subject in one of CNI's publications.[69] Wolfe's talk, Pfeiffer wrote, "put fallout shelters in their true light" and Montanans needed to hear that truth because

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[68] Quoted in Marquis W. Childs, "Question on Fallout Shelters: What Happens on Emergence?," *St. Louis Post-Dispatch*, October 5, 1961.
[69] E.W. Pfeiffer to Dr. Moog, September 17, 1961, Barry Commoner Papers, LOC, box 431, folder Committee for Nuclear Information Correspondence 1961.

the state was "being subjected to massive amounts of completely misleading information." Pfeiffer didn't wait for CNI to get the message to the citizens of Montana. The same day he wrote to Moog, he, Chessin, and Senger published an article detailing Wolfe's major points in the *Missoulian* on September 17.[70]

For the members of WMSCRI, Wolfe's talk was important not simply for the unabashed ecological message it contained. They had been hammering that point home in their articles and talks for the last year. Instead, Wolfe's talk was significant because it carried the authoritative weight of a high-ranking official within the AEC. Since the late 1950s, the American public had been told on numerous occasions from civil defense officials and even from scientists allied with the nuclear weapons program such as Edward Teller (the "father" of the H-bomb) that a nuclear war was not only survivable, but winnable. Biologists familiar with nuclear radiation like those in the WMSCRI understood those proclamations to be quite preposterous. Following Wolfe's keynote, however, they now had the words of a government official to bolster their own claims. And use them they did. Wolfe's talk became the cornerstone of WMSCRI's public addresses on civil defense, including the one Keyes criticized in the *Great Falls Tribune*. Their attribution of Wolfe put Keyes in a tough spot. For Keyes, combating a local scientific group on the issue of fallout shelters was one thing. Dealing with an apparent contradiction that pit civil defense officials against a high ranking AEC administrator was another. The best rebuttal to Wolfe that Keyes could muster in the article, however, was to isolate him as an outlier within the Commission: "Pfeiffer has quoted from Dr. John N.

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[70] "A Boon for Construction Industry, But...Fallout Shelters Offer Only a Temporary Reprieve from Death," *The People's Voice*, September 29, 1961.

Wolfe, head of the Atomic Energy Commission's Environmental Science Branch, but I say this is not the official opinion of the Atomic Energy Commission, and as far as I can determine he is the only gentleman on the commission to express such views." Keyes wisely avoided directly responding to Wolfe's assertions, opting instead to reassure Montanans that "I have a shelter in my home and I have *faith* in it."[71]

Faith, of course, was the problem that WMSCRI had with the civil defense program. Faith lured American citizens into the false hope that they could survive a nuclear attack, that once they emerged from their shelters, all they would have to do is rebuild. But biology taught otherwise. That too was part of the allure of Wolfe for WMSCRI. As Pfeiffer mentioned in his letter to Moog, atomic physicists like Teller who assured Americans that nuclear war was survivable, are "not a biologist[s]." Wolfe as an ecologist, however, "is in the best position of any government official to assess the effects of nuclear war."[72] Chessin and fellow WMSCRI member Lynn Graves, Jr, similarly argued that biology was the key to informing the public about nuclear war and fallout. In a letter to the local Missoula paper, Chessin and Graves rebutted a recent editorial in the paper which suggested that most academics supported the civil defense program. Quoted extensively from a talk delivered by renowned geneticist Bentley Glass at a recent AAAS meeting, they provided evidence to the contrary. "Biologists," they quoted Glass, "more than atomic physicists, can tell what the perils are that we face. All wild and domesticated animals will be killed…seed plants will be killed or made

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[71] "State Director of Civil Defense Disputes Professors on Shelters," *Great Falls Tribune*, November 22, 1961. Emphasis mine.
[72] E.W. Pfeiffer to Dr. Moog, September 17, 1961, Barry Commoner Papers, LOC, box 431, folder Committee for Nuclear Information Correspondence 1961.

sterile…Floods and erosion will be largely unchecked because of the loss of the forests." For Chessin and Graves, such projections did not constitute hysteria, as someone like Keyes charged, but rather reflected the sober predictions of biologists who knew better than any the ecological effects of radiation and fire. True to their mission, Chessin and Graves signed off their editorial beseeching the paper to "publish this letter in the interest of an informed public."[73] Biologists, not physicists, they maintained, held the key to understanding fallout and nuclear war, not physicists. And in that knowledge laid the information necessary for Americans to evaluate whether continued weapons tests and a fallout shelter program outweighed the costs and risks.

Events in late 1961 put the fallout shelter issue on the back burner for WMSCRI. With the resumption of nuclear testing in August of 1961, the group had more immediate problems to attend to as record levels of fallout began to rain down throughout the globe. They also had a new fallout hazard to worry about—iodine-131.

As I discussed in previous chapters, radioiodine emerged as a distinct threat to human health only in the late 1950s. Like strontium-90, radioiodine reaches humans via the grass-cow-milk environmental pathway. The radionuclides, however, bypasses root uptake and instead is consumed by cows directly from the surface of grass leaves resulting in far quicker assimilation into their bodies. Once in the body it accumulates in the thyroid and can deliver massive amounts of radiation to the organ despite its short half-life of about eight days.

[73] A copy of the editorial can be found in the Meyer Chessin Papers, UMSC, box 22, folder Western Montana Scientists' Committee for Public Information.

The U.S. Public Health Service began to monitor radioiodine levels in milk in 1958, but the project only began in earnest following the resumption of Soviet testing in 1961. Almost immediately, the PHS picked up significantly elevated radioiodine levels in milksheds across the country. Detection of such high levels raised the question of what to do should levels exceed permissible limits. It also highlighted the question of who would be responsible for taking countermeasure actions. That was not an easy question to answer since the AEC had recently lost some of its regulatory powers when Eisenhower, in response to criticism of the Commission's dual and contradictory role as a promoter and regulator of atomic energy, created a presidentially-appointed Federal Radiation Council (FRC) to create, oversee, and enforce radiation standards. The FRC's first standards were made public in a report in September of 1961, almost concurrently with the resumption of testing.[74]

To simplify exposure standards for the public, the FRC created the Radiation Protection Guidelines [RPG] system, which divided exposures into three ranges. Exposures within ranges I and II were deemed safe and required merely continued surveillance. Ranges above range III, defined as 100 to 1,000 micromicrocuries of radioiodine exposure *per day*, required some sort of control action to reduce exposure levels down to range II. Because the Public Health Service was conducting the milk monitoring, any necessary countermeasures required to reduce iodine-131 levels would ostensibly fall to them.

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[74] "Report No. 2: Background Material for the Development of Radiation Protection Standards," (Washington D.C.: Federal Radiation Council, 1961).

The system, however, had numerous problems. In July of 1962, for example, CNI wrote to PHS Surgeon General Luther Terry alerting him to the inadequacy of the daily standard. As CNI demonstrated convincingly, ensuring that daily allowances of iodine-131 exposure were kept below the range III threshold did not necessarily mean that total exposures to populations for the year were below the annual standard of 0.5 rems (roentgen equivalent man).[75] By way of example, CNI showed how iodine-131 doses in Des Moines and Minneapolis had already reached two-thirds of the total RPG for the year, but nothing had been done to limit exposure for remainder of the year because the quantities found did not exceed the daily RPG. In other words, the FRC's focus on "transitory" doses failed to provide a reasonable standard for instituting countermeasures when the cumulative dose exceeded the yearly RPG. Moreover, the focus on the daily dose actually limited the PHS's ability to control exposures. "It should be equally clear," CNI argued, "that, when as a result of the cumulative dosage from a series of transient I-131 peaks, it is discovered that the RPG has been exceeded it will be too late for any remedial action—for this can only be taken during the time when I-131 is present in the environment."[76]

CNI made the letter available as a press release and reports of it were published in a number of news outlets including the *People's Voice*. Terry replied two weeks later with a carefully worded response that was short on specifics and long on trite reassurances. "The Public Health Service," Terry wrote in hopes of appeasing the group,

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[75] The REM is basically equivalent to the roentgen, the difference being that it is a measure of absorbed biological dose rather than ambient dose.

[76] CNI to Luther L. Terry, June 7, 1962, Barry Commoner Papers, LOC, box 431, folder CNI Correspondence 1962.

"is very mindful of the recommendations of the Federal Radiation Council…As in all phases of public health, we are deeply concerned about protecting the American people from health hazards."[77]

Further problems with the system were revealed by WMSCRI. A couple weeks after Terry's reply to CNI, the PHS did take remedial action in Salt Lake City when radioiodine levels in milk exceeded range III following an underground test that vented radiation at the NTS. PHS officers subsequently ordered milk producers to divert their fresh milk supplies to making cheese or powdered milk in order to allow for sufficient decay of the radioisotope.[78] Spokane Washington too experienced similar elevated levels of radiation during this incident, but nothing about iodine-131 levels in Montana had been reported. Having no data on iodine levels in milk for Montana, WMSCRI hoped to obtain the data on the Spokane levels in order to extrapolate what the dose to persons living in western Montana was.

Accordingly, on August 24, Meyer Chessin wrote to the director of the Spokane Health Department inquiring about the Spokane situation, which according to a PHS release showed the city with a radioiodine level above the FRC's Range III. After requesting data on the daily levels of radioiodine in fresh milk for the area, Chessin ended his letter by also asking whether "the public was advised by the appropriate authorities about this contamination during June, and whether there are plans to take preventative

[77] Luther L. Terry to Eric Reiss, June 21, 1962, Barry Commoner Papers, LOC, box 431, folder CNI Correspondence 1962.

[78] Hacker, *Elements of Controversy: The Atomic Energy Commission and Radiation Safety in Nuclear Weapons Testing, 1947-1974,* 221.

action in the event of a further increase in I-131."[79] Chessin received a reply the following week from Hampton Trayner, the city's Health Officer. Trayner responded that he was in a similar position to Chessin in that he was privy only to the "official information" provided by the PHS. With regard to any countermeasures, he wrote that he did not receive any notification from higher authorities to "apply any restrictions or make any announcements with respect to an unusually high Iodine-131 content." All he had received was a "routine mimeographed weekly report." That fact apparently miffed Trayner. He hoped that such important information or directives for countermeasures measures would be provided promptly. "This immediate type of reporting," he told Chessin, "with a definite plan of emergency operation is what we consider the public is entitled to receive if the program is to afford any protection worthy of the name."[80] After Chessin shared this revealing news with the other members of WMSCRI, Pfeiffer wrote to CNI informing them of the letter, adding that after a phone call with Trayner it became apparent that none of the proper authorizes in Washington State had any plan for instituting protective procedures in the event of high fallout readings in milk.[81]

The same was true in Montana. The PHS published monthly figures for all the stations in its milk monitoring network, including the one at Helena. Unfortunately for WMSCRI's purposes, this data was too late in coming as the data was usually published a month or so after the fact rendering the data useless for warning local citizens of the

[79] Meyer Chessin to Director of Spokane City Health Department, August 24, 1962, Chessin Papers, UMSC, box 1. folder Correspondence 1962.
[80] Hampton H. Trayner to Meyer Chessin, August 30, 1962, box 20 folder Radiation Correspondence, 1962-1972. This document can also be found in Committee for Environmental Information Records, 1956-1977 (hereafter CEIR), Western Historical Manuscript Collection, University of Missouri St. Louis, MO (hereafter WHMC), box 26, folder 389.
[81] E.W. Pfeiffer to Virginia Brodine, September 6, 1962, CEIR, WHMC, box 26, folder 389.

potential need for countermeasures. Throughout the fall of 1962, WMSCRI wrote to both the state health agency in charge of the milk station and the PHS in hopes of obtaining daily milk readings to report to the public. They got the run-around which aggravated Pfeiffer. In December he wrote Virginia Brodine of CNI complaining that WMSCRI had "long tried to get prompt reports on I-131 in milk, but when we try the state people they refer us to the USPHS who then refer us to the state again." Pfeiffer did eventually receive word from a PHS official that the agency was apparently not permitted to directly transmit laboratory data. WMSCRI would have to wait for the monthly publications.[82]

In fact, the Montana State Board of Health was as confused about the the situation as WMSCI was. The Board of Health helped institute a milk monitoring station in 1960, but the method of assaying the milk for radioiodine was plagued from the beginning. The problem stemmed from the fact that milk samples collected at three Helena dairies for sampling had to be sent to a PHS lab in Las Vegas for analysis. Because radioiodine was a short-lived radionuclide, such a lengthy process for analysis rendered the results nearly meaningless. The State Board was well aware of the problem, noting in one meeting that "There is also a time lag on collection of samples and securing results for testing. By the time the results are received the milk may no longer be troublesome." Moreover, the Board also mentioned the lack of clear lines of responsibility with regard to countermeasures. "Apparently no one wants to take responsibility as to when radiation level reaches the dangerous concentration for people." Nonetheless, the board did direct

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[82] E.W. Pfeiffer to Virginia Brodine, December 7, 1962, CEIR, WHMC, box 26, folder 389. .

one of their members to write dairy producers informing them of the possibility that some "individuals might have to go on powdered milk."[83] WMSCRI did eventually learn of these plans when a customer of a local dairy, who had cancelled his order of fresh milk, received a report from the dairy farmer outlining the state's protocol for fallout protection. The plan sounded reassuring, WMSCRI members notes, "except for the question of what levels of I 131 [sic] are considered high enough to prompt these preventative measures, and the fact that daily reports have not been made available to the public."[84]

Indeed, this reference to the lack of standards was a reflection of the ongoing struggle between CNI and the FRC about the problems with the RPG. By late 1962, the issue now also involved the Joint Committee on Atomic Energy, whose members were as concerned as CNI over the apparent laxity in fallout standards. In August, the head of the FRC Anthony Celbrezze drafted a letter to the Chairmen of the JCAE in response to the growing apprehension and confusion over the issue. In the letter, which was made public, Celebrezze acknowledged that the RPGs were not in strict sense standards since they were not the "sole criteria in evaluating the significance of fallout." Nor were they "intended to set a line at which protective action should be taken or to indicate what kind of action should be taken." Measures taken to prevent over exposure to radioactive fallout required "careful consideration of local conditions." While the FRC guidelines "have some relevance…they were never intended to provide the sole basis for deciding

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[83] Minutes of Meeting of State Board of Health held November 3, 1962, Helena Montana, Montana State Board of Health Records, Record Collection 238, Montana Historical Society, Helena, Montana, box 2.
[84] Minutes of Western Montana Scientists Committee on Radiation Information, November 7, 1962, Chessin Papers, UMSC, box 22, folder Western Montana Scientists Committee for Public Information.

how and when to act." But who had the authority to act? Celebrezze vaguely replied that the Food, Drug, and Cosmetic Act allowed the federal government to control the interstate shipment of adulterated food, and by "definition, food stuffs containing excessive radioactivity would be adulterated." Intrastate contamination of food supplies were a state problem subject to local laws.[85]

Not surprisingly, Celebrezze's letter did little to clarify the problem for either the members of CNI or WMSCRI. In its vagueness, however, one thing was clear: there were no standards, nor were there clear lines of authority for whom might have the power to act. Celebrezze's reference to the Food, Drug, and Cosmetic Act was little help as WMSCRI learned for the milk situations in Montana and Spokane. In Montana, the countermeasure program was ad hoc at best. In Spokane, as the letter from Trayner made clear, officials were waiting guidance from federal officials within the PHS before instituting preventative measures. Given the global nature of fallout, rules governing inter- and intra-state commerce of food were of little help. What was needed was a firm set of standards governing the institution of countermeasures preventing overexposure to iodine-131 at the federal level.

CNI argued as much in a special issue devoted to the "Iodine Story" in *Nuclear Information* in September 1962. In addition to reprinting their letter to Surgeon General Terry, CNI also included a letter in response to Celebrezze. Celebrezze's letter, they argued, made it "evident that *no radiation standards exist at the present time with respect to fallout.*" Given the lack of concrete standards, the letter continued, the purpose behind

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[85] A copy of this letter dated August 17, 1962 can be found as an attachment to E.W. Pfeiffer to Virginia Brodine, September 19, 1962, CEIR, WHMC, box 26, folder 389.

establishing the FRC Radiation Protection Guides lost “any meaning whatever.” That fact coupled with questions concerning the legal responsibility and authority for countermeasures made the problem acute.[86]

WMSCRI also weighed in on the Celebrezze letter. In an article published in the *People’s Voice*, WMSCRI provided data for iodine-131 in milk monitoring stations throughout the West which they had finally been able to gather from reports submitted by the PHS. In Montana, radioiodine levels in milk were elevated for a week long period in October and had those levels continued would have called for countermeasures under the FRC guidelines. The problem according to WMSCRI, however, was that Celebrezze’s letter effectively rendered the guidelines impotent. “Without guides,” the article ended, “it would seem impossible to assess whether any given level of iodine in milk is a hazard.”[87]

Things were not any better in early 1963. Despite growing public unease about the radioiodine threat, the FRC continued to assure Americans that the hazard was negligible. In its May 1963 report, the FRC concluded that the “health risks from radioactivity in foods, now and over the next several years, are too small to justify countermeasures to limit intake of radionuclides by diet modifications or altering the normal distribution and use of food, particularly milk and dairy products.”[88] For Chessin and Pfeiffer, this was too much. Celebrezze’s letter the year before implied a lack of standards but for WMSCRI the hope still remained that standards might be development

[86] Robert H. Wurtz, "The Iodine Story," *Nuclear Information* 4, no. 9 (1962).
[87] "Lack of Radiation Guidelines Deplored by Scientific Group," *The People's Voice*, November 30, 1962.
[88] "Report No. 4: Estimates and Evaluation of Fallout in the United States from Nuclear Weapons Testing Conducted through 1962," (Washington D.C.: Federal Radiation Council, 1963), 2.

and implemented in the near future. Now, apparently, the FRC was eschewing even the need for standards or countermeasures. In late July, Chessin and Pfeiffer wrote to Celelbrezze bemoaning the FRC report. "We are confused by this statement," they wrote understatedly. "Are we to assume that the public and the scientific community were misled previously in believing that there were radioactivity levels above which counter measures, such as those taken by State Departments of Health in Utah and Minnesota last summer, were to be taken?" Recent scientific studies linking fallout radionuclides with childhood incidences of cancer, they argued, seemed to support the glaring need for standards. Given that data, "at what point does the increase from weapons testing become a cause for concern?" At the very least, the FRC was obligated to inform the public of the evidence on which this new approach to the radioiodine problem was based.[89] Chessin and Pfeiffer cc'd copies of the letter to newspapers across Montana and to CNI which published the letter in *Nuclear Information* later in the year. They never received a reply from Celebrezze.[90]

Nevertheless, radioiodine would not simply go away, however much federal officials like Celebrezze might have wished. A month after Chessin and Pfeiffer mailed their letter to Celebrezze, the JCAE opened hearings on the radioiodine problem. The crucial issue of the hearings centered on radioiodine exposure from NTS fallout. Although the AEC had lost some of its regulatory duties to the FRC, safety to downwind populations around the NTS remained their special area of responsibility. During the

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[89] E.W. Pfeiffer and Meyer Chessin to Anthony Celebrezze, July 24, 1963, Chessin Papers, UMSC, box 20, folder Radiation Correspondence, 1962-1972. Copies of the letter can also be found in the Barry Commoner Papers, LOC, box 432, folder CNI Correspondence 1963; *Nuclear Information*, 5 (9), 13.
[90] E.W. Pfeiffer to Virginia Brodine, October 3, 1963, CEIR, box 26, folder 389.

hearings, the AEC downplayed the radioiodine threat arguing essentially that in no cases was the exposure standard of 3.9 roentgens ever exceeded for any offsite populations. When CNI was invited to provide testimony on the AEC safety program, they agreed that the Commission's position was valid, but "*only if the fallout which gives rise to this radioactivity does not enter into the food chain.*" For CNI and the other science information groups, that was the critical issue. The AEC's environmental monitoring program, CNI argued, based its hazard risk estimates on the faulty assumption that external exposures from radiation outside the body was the controlling factor in delivering radiation to the human body. But such assumptions missed the critical role that the food chain played in linking human bodies to their environment. Their explanation for the critical differences between the two approaches is worth quoting at length:

> Thus if a gamma radiation monitor in a pasture outside the Nevada Test Site shows a reading of .087 r/hr at 12 hours following the time of a nuclear test, measured at three feet above ground level, this indicates the apparently maximum safe level, since the permissible standard for continuous exposure from a source external to the body is 3.9 r/year (effective biological dose), which is equivalent to a dose rate of .087 r/hr at 12 hours following a test. However, if a milk cow feeds on this pasture and its milk is freshly consumed by a small child, this conclusion becomes invalid. Under these circumstances it can be shown that this same gamma reading (i.e., .087 r/hr) probably reflects a concentration of iodine 131 in the grass, which after passing into the cow's milk and being consumed by the child may deliver to the child's thyroid gland a radiation dose of 175 to 1200 rads. This dosage exceeds even the safety standards for radiation workers (30 rads to the thyroid per year) by a factor of 5 to 40 and is so high as to represent a serious potential cause of thyroid cancer.[91]

[91] *Fallout, Radiation Standards, and Countermeasures*, 601. A copy of the testimony is included in Committee for Nuclear Information Technical Division, "Local Fallout: Hazard from Nevada Tests," *Nuclear Information* 5, no. 9 (1963).

Given this discrepancy in values between external and internal sources of radiation exposure, the CNI testimony called into question the entire AEC monitoring program. Internal emitting radiation, not external gamma rays, proved to be the greater hazard. And even when the federal government instituted monitoring of radioiodine in milk, it was ineffective owing to delays in analysis and reporting, lack of clear guidelines governing protective action, and inadequacy of the geographical coverage of the stations. All of these issues pointed to an even more alarming realization, however: If radioiodine now seemed to be the critical factor, what were the levels of the radionuclide like in the previous decade when it was not being monitored at all?

For the American and global public, questions such as these were indicative not only of the U.S. government's failure to adequately address the problem of fallout radionuclides in the environment, but also cast doubt on the Atomic Energy Commission's forthrightness about the risks associated with the weapons testing program. The fact that groups like CNI and WMSCRI had become the public's major source of information on fallout effects reflected the growing lack of trust with the AEC. As the science information movement expanded in response the increasing need for information on new or previously ignored radiological threats, the chorus of anti-testing similarly swelled.

By 1963, new knowledge about the hazards of fallout coupled with the growing mistrust of the agencies responsible for the regulation of atomic energy convinced the American public and many of its representatives that the risks associated with atmospheric testing were not worth the national security benefits. In October, the United

States, Soviet Union, and Great Britain signed the Limited Test Ban Treaty prohibiting atmospheric, underwater, and outer space testing. The treaty, which limited each country to underground testing, was a landmark piece of disarmament diplomacy, helping to ease cold war tensions that threatened to destroy the planet during the Cuban Missile Crisis. Nevertheless, the core issue underlying the treaty was health. As President Johnson remarked to the world on the test ban anniversary, the treaty "has halted the steady, menacing increase of radioactive fallout. The deadly products of atomic explosions were poisoning our soil and our food and the milk our children drank and the air we all breathe."[92] In the ensuing years of the cold war, the arms race would continue, but in large measure the public would be spared the radioactive consequences, thanks in part to the efforts of scientists within the science information movement.

Conclusion

A couple years after the signing of the Limited Test Ban Treaty, Mike Chessin and Bert Pfeiffer were sipping beer in a bar in Washington D.C. where they were attending an annual meeting of the AAAS. While working on their beers, and no doubt discussing the session they just attended on the impacts of science on society, a man at the bar noticed from their conference badges that they were from Montana. "What do you guys do in Montana?" Chessin later recalled the man asking. The three subsequently began a discussion of Chessin and Pfeiffer's work on fallout and civil defense as part of the science information movement. "You know," the man commented, "you guys were

[92] Quoted in Egan, *Barry Commoner and the Science of Survival: The Remaking of American Environmentalism*, 75.

responsible for creating the climate that made it possible for the Senate to ratify the Limited Test Ban Treaty." The man was a high-ranking federal health official in the capital.[93]

One cannot underestimate the critical role that the science information movement played in alerting the American public to the hazards of radioactive fallout from atmospheric nuclear weapons testing. Through the public lectures, publications, research, and testimony before Congress, groups like CNI and WMSCRI reconfigured the ways Americans understood the cold war calculus that pit fallout concerns against national security. By emphasizing the role of the food chain in mediating exposure to radioactive fallout, the science information movement successfully recast the debate by linking the health of the human body to the health of the environment. Environmental radiation was not something external to humans, but was, through food, materially part of us. Once that link was made, above-ground testing no longer seemed worth the risk.

This ecological vision forged during the fallout controversy of the 1950s and early 1960s would have a lasting impact on environmental politics within the U.S. The popular ecology behind the movement, coupled with the scientific politics of information, would form two great cornerstones of the environmental movement as it was constituted in the early 1970s.[94] CNI's Barry Commoner would lead and become the public face of that movement following the success of his writings on the subject such as *The Closing Circle*.

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[93] Meyer Chessin interview. See also Betsy Cohen, "Retired UM Professor Bert Pfeiffer Dies," *Missoulian*, April 28, 2004.
[94] Egan discusses Barry Commoner and his role in creating the Science Information Movement in Egan, *Barry Commoner and the Science of Survival: The Remaking of American Environmentalism*.

Chessin and Pfeiffer too would go on to both local and national notoriety for their work on environmental issues. In 1963, just as the Limited Test Ban Treaty was being debated in the Senate, Chessin and Pfeiffer, along with Commoner, Margaret Mead and the other scientists involved in the science information movement, created the Scientists Institute for Public Information—a national group comprised of some twenty public information groups dedicated to providing scientific information on a host of environmental issues. Following this development, WMSCRI would become the Western Montana Scientists' Committee for Public Information, reflecting the broadening scope of their mission. Through WMSCPI, Chessin would later work on issues of air pollution, the installation of ICBM missiles in Montana, and underground venting of radiation from tests at the NTS. Throughout the 1960s and later, Pfeiffer spearheaded the scientific movement to end the U.S. military's herbicide program in Vietnam.[95] Both remained on the faculty of the University of Montana until their respective retirements.

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[95] David Zierler, *The Invention of Ecocide: Agent Orange, Vietnam, and the Scientists Who Changed the Way We Think About the Environment* (Athens: University of Georgia Press, 2011).

CONCLUSION

"I belong to a Clan of One-Breasted Women." - Terry Tempest Williams.[1]

One of the first things I do in the morning before I begin the daily process of grinding out this dissertation is to surf through Facebook to check up on friends and get the latest news and stories from all my page "likes." Today, National Public Radio posted a link to one of its stories on my wall about an "atomic veteran" who participated in military operations during the Operation Plumbbob tests at the Nevada Test Site in 1957. As a seventeen year old Army private, Joel Healy remembered being dug-in in a trench near ground zero and seeing the bones in his hand as the bomb was detonated. He was told that neither he nor his fellow soldiers were in harm's way. Now, he believes, that was not the case and the federal government knew it all along.

> They had a motto then, 'Atoms for Peace.' And, you know, I'm 17 years old and I buy into it...because I'm thinking, they spent a lot of money training me to be a soldier. They wouldn't intentionally put me in harm's way. And this is 1957. We dropped those bombs on Japan in 1945, so they've known for 12 years. Troops going into battle know that there is a very inherent risk that they may not be coming out, unless it's in a black bag. In this instance, they never said a word. And they knew it. It's just a disgrace…A lot of good men died.[2]

Healy is not alone in expressing such sentiments. In her book *Refuge: An Unnatural History of Family and Place,* Terry Tempest Williams has written eloquently of her deep connection to the Great Salt Lake and the sanctuary that natural places provided her while her mother battled cancer. Yet as the subtitle to her book suggests,

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[1] Terry Tempest Williams, *Refuge: An Unnatural History of Family and Place* (New York: Vintage Books, 1991).
[2] Kelli Healy Salazar, "Veteran: Risks in the 1950s Bomb Test 'A Disgrace'," (National Public Radio, October 12, 2012).

the landscape held a double-edged sword for her and her family. For Williams, the Great Basin is a land of fallout as much as a landscape of beauty. In the former, she argues, can be found the cause of her mother's cancer.

Fallout, as these examples suggest, has continued to be a controversial topic, even after the atomic bombs went underground nearly fifty years ago. Did nuclear fallout cause an increase in cancer and other forms of disease to heavily exposed populations such as military personnel or downwind communities? Modern science will tell you that we don't know. While numerous dose reconstruction and epidemiological studies have been conducted, none have been able to determine firm links between fallout exposure and cancer. Nonetheless, in 1990 the U.S. Congress passed the Radiation Exposure Compensation Act providing monetary compensation to atomic veterans and downwinders based largely on the theoretical possibility that their exposure levels almost surely would have induced some cancers.

Did the AEC and its scientists knowingly expose people to harmful amounts of radiation? Some scholars have contended, as Healy suggests above, that the radiological effects were well known and that the AEC put workers and bystanders at undue risk in the name of national security. Still others argue that the effects were not well known and that given the knowledge of the time AEC scientists did their best to ensure safety. In this dissertation, I have argued that such quests to indentify the whats and whens of knowledge about radiological effects fails to account for the complex ways that scientific knowledge is produced and translated into risk and regulatory action. By focusing on how ideas about the separation of humans from non-human nature have shaped the production of knowledge and risk, I show knowledge rarely (or perhaps never) enters into public policy in a straightforward manner.

The behavior of radioiodine in human bodies and the environment, for example, was known as far back as the late 1940s. Yet, radioiodine emerged as a fallout hazard only by about 1960. Why? When nuclear testing began with Trinity in 1945, health physicists based their concepts and practices after the discipline of toxicology. As a laboratory-based discipline, toxicology was founded in practices that isolated certain chemicals from their larger environments in order ascertain single causal links between exposure and effect. Once clear causal lines between a chemical and an effect had been established, they then moved to engineer factory floors to better manage worker exposures. Yet, the process of controlling the environment imbued them and health physicists with a deep hubris that place mattered little when it came to protecting humans. Thus, when health physicists began to consider the problem of fallout when radiation moved beyond the bomb-building factories, they did not see a complex and dynamic environment, they saw a factory. As such, they perceived that the greatest hazard to humans was from external gamma radiation. A far distant concern was the potential for internal exposure since in order for a person to ingest enough radioactive material to cause harm they would have to consume massive quantities of radiation directly off the ground. In effect, health physicists operated under the assumption that human bodies and the environment were fundamentally disconnected. Health physicists understood that once in the body radioiodine concentrated in the thyroid and could cause cancer. What they could not see was how significant amounts of radioiodine could actually make their way to the body from the environment. This idea of environmental passivity was similarly expressed in the idea that, like a factory, radiological effects would be contained within the boundaries of the test site.

These ideas of bodily and spatial boundedness, however, gradually gave way to more ecological ways of seeing fallout only as environmental scientists in ecology, oceanography and meteorology began to assert their expertise in matters of fallout. By tracing radiation as it moved through the atmosphere, the oceans, and ecological food chains, they demonstrated that human bodies, the environment, and geographical space were linked. Radioiodine, therefore, emerged as a fallout hazard when environmental scientists showed that significant levels of the radionuclides were transported offsite and became incorporated into ecological food chains. When the environment became an actor in mediating human exposure to fallout radiation, health physicists began to look for and monitor radioiodine.

In pointing to the ways that these basic kinds of assumptions about bodies, the environment, and space shaped the production of knowledge and thus risk is not to excuse instances where AEC officials covered-up or mislead the public about certain fallout episodes (it happened). Nor am I suggesting that AEC should be given a "free pass" for exposing millions of people to radioiodine in the early 1950s because they didn't yet have the proper knowledge. Rather, my point is two fold. First, I am arguing that even if the AEC had been more rigorous and forthcoming about what they knew about fallout effects, their primary scientific source for radiation knowledge, health physicists, were on a fundamental level incapable of seeing the environmental aspects of the problem. Second, in maintaining that certain scientific models shaped the perception of risk I am in agreement with Nancy Langston whose recent work on the history of the endocrine disruptors has demonstrated how the regulatory standard to prove harm rather than safety has failed to protect human health.[3] In this current era where news headlines on environmental issues like carbon dioxide or mercury emissions are dominated by questions about the certainty (or uncertainty) of scientific knowledge, I argue that the history of nuclear testing fallout teaches us that policymakers might be better served not merely by access to "better" scientific information, but also by critical reflection on the nature of scientific knowledge and its relationship to governance. In this way, perhaps regulatory bodies will operate with precaution rather than reaction.

Moreover, as I have argued throughout this dissertation, the history of nuclear weapons testing fallout provides further opportunities for rethinking the relationship between science, technology, and the material environment and consequently how we narrate environmental change. By centering my analysis on radiation, I contend that the walls that we put up between these categories are becoming more and more difficult to maintain. Fallout radiation, I show, proved much more than simply a frightening contaminant. It was also an extremely valuable scientific tool that mediated scientists' interaction with and comprehension of the material environment in ways previously foreclosed to them. Its agency, therefore, went beyond the unintended health consequences of nuclear testing; it was both a cause of changes in the landscape and a means of animating it. In telling the story of radiation as an impure or hybrid object that

[3] Nancy Langston, *Toxic Bodies: Hormone Disruptors and the Legacy of DES* (New Haven Yale University Press, 2010).

limned the natural and technological, this dissertation has avoided overly simplistic declension narratives common to environmental history. Radiation undoubtedly caused harm. But it also also played a critical role in the production of knowledge that helped close the first modern environmental crisis.

The impact of radiation was felt beyond the Limited Test Ban Treaty, however. While environmental historians have paid scant attention to the fallout controversy, I argue that the period marked an important moment when ecological views of the relationship between bodies, the environment, and geographical space that so animated environmentalist thought in the late 1960s were first beginning to be widely articulated. In particular, I contend that notion of the Earth as a unified biosphere emerged in part as a result of the realization that fallout was a global phenomenon. This idea, I show, was rooted both in the popular imagination and in scientific practice. On one level, it expressed the growing anxiety about planetary threats among concerned citizens. On another, the biosphere was empirically instantiated by radiotracer practices in meteorology, oceanography, and ecology, which showed that fallout was travelling throughout the globe and assimilating into ecological food chains. It was during the fallout controversy too that the science information movement first got its start. As I argue in chapter 7, this movement operated on both national and local scales and incorporated experts and the laity to great effect.

Environmental science was impacted as much as the environmental movement by radiation. The fallout controversy, I argue, offers a unique lens to explore the ways in which the authority of environmental field sciences were enhanced by the development of radioactive tools. Indeed, as the shift from a health physics to a more ecological approach to the fallout problem suggests, the growth of the environmental sciences challenged the epistemological dominance of the lab in the culture of the AEC and the West more broadly. Rad tools also helped enable the environmental sciences to scale up and, at least in the case of meteorology, become global. Moreover, by helping to develop biospheric concepts, the environmental sciences also contributed to the growing sense that modern progress was threatening the entire planet. It is not surprising, for example, that concerns about global climate change should emerge shortly after the fallout controversy. It was during this period that new practices were developed (including radiation, computer, and satellite technologies) that turned the globe into a new epistemological frame capable of scientific scrutiny.

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