AN ABSTRACT OF THE THESIS OF

D. Erik Ellis for the degree of Master of Science in History of Science, presented on December 17, 2002.
Title: The Hanford Laboratories and the Growth of Environmental Research in the Pacific Northwest, 1943 to 1965.

Redacted for privacy

Abstract approved: ______________________

William G. Robbins

The scientific endeavors that took place at Hanford Engineer Works, beginning in World War II and continuing thereafter, are often overlooked in the literature on the Manhattan Project, the Atomic Energy Commission, and in regional histories. To historians of science, Hanford is described as an industrial facility that illustrates the perceived differences between academic scientists on the one hand and industrial scientists and engineers on the other. To historians of the West such as Gerald Nash, Richard White, and Patricia Limerick, Hanford has functioned as an example of the West's transformation during in World War II, the role of science in this transformation, and the recurring impacts of industrialization on the western landscape. This thesis describes the establishment and gradual expansion of a multi-disciplinary research program at Hanford whose purpose was to assess and manage the biological and environmental effects of plutonium production. By drawing attention to biological research, an area in which Hanford scientists gained distinction by the mid 1950s, this study explains the relative obscurity of Hanford's scientific research in relation to the prominent, physics-dominated national laboratories of the Atomic Energy Commission. By the mid 1960s, with growing public concern over radiation exposure and changes in the government's funding patterns for science, Hanford's ecologically relevant research provided a recognizable and valuable identity for the newly independent, regionally-based research laboratory. With funding shifts favoring the biological and environmental sciences in the latter half of the twentieth-century, Hanford scientists were well prepared to take advantage of expanding opportunities to carve out a permanent niche on the border of American science.

The Hanford Laboratories and the Growth of Environmental Research in the Pacific Northwest, 1943 to 1965

by
D. Erik Ellis

A THESIS

submitted to

Oregon State University

in partial fulfillment of
the requirements for the
degree of

Master of Science

Presented December 17 , 2002
Commencement June 2003

Master of Science thesis of D. Erik Ellis presented on December 17, 2002.

APPROVED:

Redacted for privacy

Major Professor, representing History of Science

Redacted for privacy

Chair of Department of History

Redacted for privacy

Dean of the Graduate School

I understand that my thesis will become part of the permanent collection of Oregon State University libraries. My signature below authorizes release of my thesis to any reader upon request.

Redacted for privacy

D. Erik Ellis, Author

ACKNOWLEDGEMENTS

So many individuals have generously given of their time, ideas, and encouragement throughout the course of this project. I want to especially thank Bill Robbins and Ron Doel, two scholars and friends who have made me think critically about the West, the Cold War, science, and the environment. Mary Jo Nye and Paul Farber, as well as other members of the History department, provided a stimulating and supportive environment in which to think about the importance and practice of history. Thanks to all of these, the named and the unnamed, who have helped me complete this project.

My sincere thanks are extended to numerous individuals at archives, libraries, and government agencies, who guided me through a sea of documents. The following individuals deserve special recognition: Marjorie Ciarlante at the National Archives in College Park, Maryland; Rob Cox at the American Philosophical Society; Gary Lundell in the Manuscript and Archive center, University of Washington Library; and Janice Goldblum provided kind service at the archives of the National Academy of Science. Francis 'Skip' Gosling offered me a fellowship at the Department of Energy. Working with him and Terence Fehner helped me better understand the complexity of the Atomic Energy Commission. I thank them for their important contributions.

Finally, my fellow graduate students at Oregon State University have offered intellectual stimulation and personal friendship. At numerous points during the previous three years, Chris Jolly, Brian Schefke, Kris Harper, Carmel Finley, Melinda Gormly, and now Nathan Boltseridge and Leslie McCabe have been supportive friends and supplied needed distractions! Kevin Stoller and Katie Hubler became special friends during the past three years – I gained much from time spent with them. Finally, Kristin Johnson has shown me true love and happiness. Thank you for these inestimable gifts, not to mention the careful reading, encouragement, and constructive criticism of my work.

TABLE OF CONTENTS

TABLE OF CONTENTS (CONTINUED)

The Hanford Laboratories and the Growth of Environmental Research in the Pacific Northwest, 1943-1965

Chapter 1

Introduction

Harry Kornberg, a biochemist and director of the Hanford Laboratories biology program summarized a curious, but in his words "most noteworthy" accomplishment for the research team in 1959:

> In recognition of the nonclassified nature of our work and of the desirability of making the laboratories more available to visiting scientists, the Atomic Energy Commission's Division of Biology and Medicine recommended relocation of the "exclusion area" fence, which once enclosed a neighboring reactor and associated facilities along with the Biology laboratories. This ... resulted in an increased number of visiting scientists, both from the United States and from foreign countries."[1]

The earliest roots of radio-biological research at Hanford could be traced back to 1943 with the establishment of Hanford as the industrial plutonium production site for the Manhattan Project. Thus, by 1960 as Kornberg reflected on the successes of the past year, the program could boast well over a decade of research on biological and environmental effects of radiation. Mounting concern to the potential dangers of radioactive fallout from nuclear weapons testing made research on the various pathways and effects of radioactive isotopes ever more valuable to the nation. It is odd, then, that Kornberg chose to highlight this decidedly non-scientific accomplishment – the relocation of a fence.

---

[1] Harry Kornberg, "Introduction," *Hanford Biology Research Annual Report, 1959* (General Electric, Hanford Atomic Products Operation, 1960). Department of Energy Reading Room, Richland WA (hereafter cited as R-DOE): Document # HW-65500).

Kornberg's statement suggests a number of conclusions, and poses as many questions, about Hanford research specifically and the changing state of American science during the Cold War. Certainly, Hanford biologists were gaining new levels of power and influence. In turn, greater prominence seemed to attract colleagues from outside the small world of Atomic Energy Commission (AEC) biologists. But if AEC and Hanford managers kept biological research shrouded for over a decade, why increase access to the laboratory now? Since the Hanford site existed mainly as an industrial facility, Kornberg's statement suggests a lessening relevance of biological research to daily plutonium production concerns. If so, why did biologists remain at an isolated facility operated by an industrial giant (General Electric) with little interest in the biological sciences? And since biologists continued to pursue research at Hanford, how did they make their research relevant to a broad spectrum of practical minded patrons while simultaneously continuing to build interest among fellow biologists?

This study of a geographically isolated research laboratory, pursuing various kinds of biological research from the mid 1940s through the mid 1960s, describes the practice and the justification of science in what Robert Kohler calls a "scientific borderland."[2] Hanford scientists struggled to build a strong research program. Thus, in many ways this is a study of a scientific institution that was undistinguished yet somehow found ways to expand and take advantage of opportunities resulting from American society's faith in the progressive qualities of science and technology. This is

---

[2] Robert Kohler's most recent work, *Landscapes and Labscapes: Exploring the Lab-Field Border in Biology* (University of Chicago Press, 2002), uses the term "borderland" to denote a set of practices that fall somewhere outside the traditional (and privileged) laboratory setting of science. Much of Hanford's research fits this definition, but here I am also using the term borderland to suggest that Hanford existed towards the geographical and institutional periphery of power, which added to the relatively slow evolution of Hanford's research program.

not a study of theory and ideas in radiological and biological sciences. Rather, as more historians of science have been doing, I aim to describe the social, political, and cultural geography of modern science through the history of a common, inconspicuous research laboratory evolving under the particular forces of Cold War America.[3]

Indeed, the crucial turning point in the evolution of Hanford's early research laboratory occurred in the years surrounding the fence relocation. Kornberg had good reason to tout this triumph, though it appears almost comical to the outside observer unfamiliar with the particular context within which Hanford research evolved during the early Cold War period. The months and years immediately following the Soviet Union's successful launch of Sputnik in 1957 witnessed a great sense of urgency to improve America's education, research, and industrial systems. Charles Kidd, a science policy analyst, stated in 1959 that "after the initial shock [of Sputnik] ... the problem of strengthening our scientific capacity over the long run emerged as a matter of vital concern to the nation."[4] In the Pacific Northwest, planning for the 1962 World's Fair quickly reoriented itself from a "Festival of the West" to "America's Space Age" when

---

[3] Two recent editions of *Osiris* provide useful studies from which my research on the culture, sociology, and politics of American science draws inspiration. Arnold Thackray explicitly states the historiographical current driving my work: "Great thoughts of great men have been replaced by such issues as funding, politics, pay, environmentalism...." Because this is a relatively new turn in the history of science, Thackray continues, "we know surprisingly little about how and why science has changed in the time since World War II began." See *Osiris, Science After '40*, 2nd Series, 1992, 7:vii-viii. In a different volume, John Servos suggests the fruitfulness of studying research groups in order to get at the broader social context of science. Focusing on the "research group" requires a historian to struggle with "demarcation problems, since, as Geison pointed out a decade ago, the study of the schools cannot entirely be severed from investigations of the individuals that compose them and the larger networks through which they interact. On the one side their study merges with biography; on the other with the histories of disciplines, universities, traditions of thought, and even national styles of science. Far from being a liability, however, this situation is advantageous, since it gives the historian license to move freely from consideration of the largely private realm of creative effort to the more public arena of justification and persuasion." See John Servos, "Research Schools and Their Histories," *Osiris*, 2nd Series, 1993, 8:13.

[4] Charles V. Kidd, *American Universities and Federal Research* (The Belknap Press, 1959), v.

Sputnik made its successful orbits in 1957.[5] Behind these kinds of public displays promoting the value of American science, a serious reassessment of research orientation was taking place.

Edward Teller, the prominent physicist known for his role in developing the hydrogen bomb, as well as for his criticism of more liberal colleagues, succinctly stated the underlying fear. Looking back to the successes of the Manhattan Project of World War II and forward to a predictably second-rate future, Teller remarked, "ten years ago there was no question where the best scientists in the world could be found - here in the U.S.... Ten years from now the best scientists in the world will be found in Russia."[6] Teller's remark was not simply aimed at predicting the nation's future, rather he meant to rouse American society to invest more in science and technology.

The sense of alarm produced by Sputnik moved Congress to question its current patterns of science education and research. The Atomic Energy Commission, the successor of the Manhattan Project and the agency in charge of the numerous research laboratories spawned by that project, at once undertook an assessment of its laboratory

---

[5] For Seattle World's Fair promotion of American science as a response to Sputnik see John M. Findlay, *Magic Lands: Western Cityscapes and American Culture after 1940* (University of California Press, 1992), 228-239.

[6] Daniel J. Kevles, *The Physicists: The History of a Scientific Community in Modern America* (2nd edition, Harvard University Press, 1995), 384. For the remarkable reassessments and transformations in American physics and the federal funding apparatus following Sputnik see pages 384-392. Numerous historians have explored the expansion of the scientific enterprise and the redefining of disciplinary boundaries through the mid twentieth-century, in part as a consequence of Sputnik. Of particular importance to this essay is scholarship on biology and the "earth sciences." See Tobey Appel, *Shaping Biology: The National Science Foundation and American Biological Research, 1945-1975* (Johns Hopkins University Press, 2000), 68, 78-79, & passim; and Ronald Doel, *Solar System Astronomy in America: Communities, Patronage, and Interdisciplinary Science, 1920-1960* (Cambridge University Press, 1996), 2, 188-190, 224-225, & passim. For an overall history of Sputnik's effect on American education and research see Barbara B. Clowse, *Brainpower for the Cold War: The Sputnik Crisis and National Defense Education Act of 1958* (Greenwood Press, 1981). For

system to ensure maximum productivity, accountability, and relevance in its research programs.[7] In the eastern half of Washington State, scientists at the AEC's Hanford Engineer Works positioned themselves to take advantage of new and expanding opportunities for research in the biological sciences as the Cold War intensified. The establishment of research at Hanford traces back to the Manhattan Project, but it is in the late 1950s as concern with American science reshuffled scientific priorities, that a cohesive and independent research program began to emerge.

At about this time, important research into the biological and ecological effects of atmospheric nuclear testing gained prominence within the AEC as weapons testing escalated around the world and as arguments over the dangers of fallout entered public discourse.[8] Together these events created ample justification for increasing research. As the AEC developed its research laboratory report, the various laboratories used this as an opportunity to promote the relevance and importance of their particular programs. Hanford's major research arm, referred to as the "Hanford Laboratories" in the 1950s, was engaged in broadly radio-ecological and radio-biological research as one of its primary functions. Hanford's research program was, however, largely unrecognized by top AEC administrators who concentrated on promoting research at the National

---

Sputnik's effects on Eisenhower policies, see Robert A. Divine, *The Sputnik Challenge* (Oxford University Press, 1993).

[7] The product of this laboratory system analysis was a 1960 Joint Committee on Atomic Energy (JCAE) report, "The Future Role of the Atomic Energy Commission Laboratories: A report to the Joint Committee on Atomic Energy by the United States Atomic Energy Commission, Washington, D.C., January 1960" (United States Government Printing Office, 1960).

[8] J. Christopher Jolly focuses on the unsettled scientific debates and public perceptions concerning fallout in the United States, *Necessary Uncertainty: The Scientific Debate Over Fallout, 1954-1963* (Oregon State University, PhD, in progress). Barton Hacker presents a detailed history of the AEC's weapons testing program and the efforts taken to mitigate dangers, *Elements of Controversy: the Atomic Energy Commission and Radiation Safety in Nuclear Weapons Testing, 1947-1974* (Berkeley, CA: University of California Press, 1994).

Laboratories and in the physical sciences which were seen as the basis for advancing nuclear weaponry and nuclear power. Biological and especially ecological research was more often viewed as an interesting but non-essential endeavor in the AEC's research programs.[9]

The AEC's analysis of its research programs in preparation for the report to Congress offered Hanford scientists an opportunity to promote its broadly ecological research program. In a 1959 set of memos scientists from the newly formed Environmental Sciences Branch of the AEC's Division of Biology & Medicine argued that ongoing research at "production facilities [involving] contamination of the biological environment" should remain a part of those laboratories' missions.[10] At the time, "production facilities" referred directly to the plutonium production facilities at Hanford and Savannah River, and to a lesser extent the uranium enriching facility at Oak Ridge. Another Environmental Sciences Branch ecologist complained that the AEC's text describing current "problems in 'biophysics'...and the biological course of radioactive materials released into the environment" was overly "brief and inadequate."[11] In an effort to defend why similar research should be done at Oak Ridge and Hanford, this ecologist argued that "the environments are distinctly different, one being the deciduous forest and the other desert. The contaminated White Oak Lake bed at Oak

---

[9] F Ward Whicker & Vincent Schultz. *Radioecology: Nuclear Energy and the Environment, vol.1* (CRC Press, 1982). See page 5 for the authors' recollection of working as an ecologist at the AEC in the late 1950s. For the AEC's focus on its National Laboratories for basic research see Peter Westwick, *The National Laboratory System in the U.S., 1947-1962* (University of California at Berkeley, PhD, 1999), 1-32.

[10] Memo from I.E. Wallen to C.W. Shilling, Aug. 18, 1959. National Archives & Records Administration (College Park), Records Group 326, Division of Biological & Environmental Research, Central Subject File (hereafter cited as: DBER), Box 61, Folder 16.

[11] Memo from V. Schultz to H.A. Stanwood, Nov. 6, 1959. DBER, Box 61, Folder 16.

Ridge is unique and the two river systems [Columbia & Clinch rivers] draining the onsite areas are not comparable. The vegetation and animal populations are also in great contrast."[12]

Because of the unprecedented quantities of radioactive waste being produced and stored, Hanford possessed undeniable justification for its onsite biological and ecological research. Hanford scientists could defend the existence of programmatic and applied ecological research programs and, with the growing popularity of ecology, could argue for increasing basic research due to the "uniqueness" of the environment. By the time Hanford's research program gained independent status in 1965 (still funded by AEC but no longer directly tied to support of plutonium production programs), the chairman of the AEC, Glenn Seaborg, made use of this argument when congratulating the Division of Biology & Medicine on the new status of the renamed Pacific Northwest Laboratory. Seaborg wrote, "I am pleased that you feel that the Laboratory has great potential both because of the existing excellent research staff and because of the rather unique geographic surroundings, making possible certain ecological studies which could be of fundamental importance."[13]

The focus of this thesis is the establishment of biological and environmental research at Hanford in the late 1940s and the evolution of a collection of temporary research programs into a permanent, multidisciplinary regional laboratory by the mid 1960s.[14] A primary motivation for this topic is a simple question - why do we find a

---

[12] Memo from V. Schultz to H.A. Stanwood, Nov. 30, 1959. DBER, Box 61, Folder 16.

[13] Letter from G. Seaborg to F. Hodges (chair of ACBM), July 1, 1965. DBER, Box 106, Folder 5.

[14] This study follows the history of the "Biology Program" within the Hanford's changing research departments. Within the "Biology Program" there were biologists, biochemists, physicists,

major, multi-million dollar research facility situated in the desert (and relative isolation) of eastern Washington and how did it come to the particular set of research programs that it did? Two answers are suggested in the paragraphs above, the first having to do with the massive infusion of support for 'science' as a consequence of World War II and the Cold War. A second important factor has to do with specific local conditions. Tons of exotic and lethal radiological waste were annually produced as a byproduct of the plutonium manufacture. Understanding the behavior and effect of these chemicals on the local biological and ecological systems, it was believed, could best be done at Hanford.[15]

On the local level, immediate and practical concerns having to do with safe handling of Hanford's products combined with a Richland resident's desire to ensure long-term viability of the regional economy. Finding a set of ongoing scientific research agendas was seen as vitally important in guaranteeing a significant and continued federal presence as the demand for nuclear weapons waxed and waned.[16] The biological

---

meteorologists, radio-ecologists, veterinarians, health physicists, and others. With full knowledge of the disciplinary diversity within Hanford's research programs, I nonetheless will often use the term "biologists" to refer to this multidisciplinary group, and "radio-biology" to refer to their common research focus.

[15] Leslie Groves, *Now It Can Be Told: The Story of the Manhattan Project* (Harper & Row, 1962), 421, 422; Neal Hines, *Proving Ground: An Account of the Radiobiological Studies in the Pacific, 1946-1961* (University of Washington Press, 1962), 3-18; Newell J. Stannard, *Radioactivity & Health: A History* (Battelle, Pacific Northwest Laboratory, 1988), 754-756; C.D. Becker, *Aquatic Bioenvironmental Studies: The Hanford Experience, 1944-1984* (Elsevier, 1990), 61-64, 81-85; Michele Stenehjem Gerber, *On the Home Front: The Cold War Legacy of the Hanford Nuclear Site* (University of Nebraska Press, 1992, 2nd edition), 1-2, 28, 186-190.

[16] Links between science/technology, the federal government, and arguments for regional economic prosperity have been nicely described by historians Robert Kargon, Stuart Leslie, and Erica Schoenberger in "Far Beyond Big Science: Science Regions and the Organization of Research and Development," *Big Science: The Growth of Large-Scale Research*, edited by Peter Galison & Bruce Hevly (Stanford University Press, 1992), 334-354; Stuart Leslie and Robert Kargon in "Selling Silicon Valley: Frederick Terman's Model for Regional Advantage," *Business History Review*, 1996, 70(4):435-472; and Stuart Leslie and Robert Kargon, "Imagined Geographies: Princeton, Stanford and the Boundaries of Useful Knowledge in Postwar America," *Minerva*, 1994, 32:121-143. John Findlay

sciences in general, and broadly ecological research in particular, became strong programs around which the Hanford Laboratories could justify escalating investment on the part of the AEC. Not only could these programs help to serve the needs of the growing waste-storage problem but they were increasingly justifiable in their own right as the 'environmental sciences' became an accepted, and later fashionable, collection of multi-disciplinary research pursuits.[17]

Possibly the least recognized factor contributing to the establishment of the Biology Program as a major arm of the Hanford Laboratories has to do with ecologist's rhetoric emphasizing the distinctive natural features of a local environment. AEC ecologists looking to justify continued support from sometimes skeptical administrators, emphasized the unique qualities of a given region. In a way that recalls earlier traditions in natural history, Hanford ecologists could argue that studies of the Columbia Basin ecosystem must be supported because they could not be duplicated at another, more developed research center. Peter Bowler has made a similar connection:

> "At one level, ecology was a reaction against the old natural history, a determined effort to put field studies on a new, more scientific footing. Various groups exploited the rhetoric of modernization in their efforts

---

has also discussed aspects of regional development and science in *Magic Lands*, 117-159 and his article "Atomic Frontier Days: Richland, Washington, and the Modern American West," *Journal of the West*, 1995, 34(3):35-36, where he describes local attempts to create a stable community.

[17] Peter Bowler, *The Norton History of the Environmental Sciences* (W.W. Norton & Co., 1992), 383-84. Bowler argues that the "environmental sciences" have been an ever-changing set of disciplines with varying cohesiveness. Nonetheless, these groupings "have been brought together because all are seen to have a bearing on problems arising from human activity." Literature is scarce on the "environmental sciences;" for a start see *Science And Nature: Essays in the History of the Environmental Sciences*, edited by Michael Shortland (British Society for the History of Science, Alden Press, 1993). In this volume Peter Bowler's essay "Science and the Environment: New agendas for the History of Science?" (1-21) is particularly useful. He attempts to place the "environmental sciences" in an interdisciplinary context, part "earth science" and part "life science." The term "environmental sciences" was not used to describe any of the research programs at Hanford until 1963. However, the AEC created an "environmental science" program in 1956 which provided much of the funding for Hanford biological research (discussed in chapters 2 and 3).

> to create new research programmes that could exploit the opportunities now being offered by the flood of government and private money available for scientific research."[18]

Though research at Hanford encompassed other disciplines, primarily nuclear physics (for reactor technology) and chemistry (in support of the plutonium separation process), this study will focus on the ways in which research in the biological sciences helped to establish the laboratory's independence from strictly AEC-prescribed "project development" research.[19]

The scope of this study, a history of biology research at Hanford, requires clarification since scientists from many disciplines worked at Hanford, and "biology" cannot be considered the only subject of research. The "Hanford Laboratories" came into existence over a period of years and drew together various research groups operating at Hanford since, in some cases, 1944. By the mid 1950s three disciplinary divisions existed in the Hanford Laboratories: Chemistry, Physics & Instrumentation, and the Biology Program. While not denying the importance of the physical sciences to Hanford's research efforts, I focus attention on the biology division and its precursors. A number of factors justify this attention: the first research program associated with Hanford addressed the effects of radiation on fish; in contrast to physicists and chemists,

---

[18] Bowler, *History of the Environmental Sciences*, 384. Other scholars have expanded on the compatibilities between ecology, local politics, and social ideals. See Daniel W. Schneider "Local Knowledge, Environmental Politics, and the Founding of Ecology in the United States: Stephen Forbes and 'The Lake as a Microcosm,'" *Isis*, 2000, 91(4):681-705; Gregg Mitman, *The State of Nature: Ecology, Community, and American Social Thought, 1900-1950* (University of Chicago Press, 1992). Keith Benson, "The Emergence of Ecology from Natural History," *Endeavour*, 2000, 24(2):59-62

[19] "Project Development" was a term used to describe research that was directly tied to improving production processes. The AEC also used common definitions of "basic" and "applied" research to describe the various kinds of research performed at its laboratories (see JCAE, "Future Role…," 7). An important issue in the late 1950s and into the early 1960s was to what extent the AEC should allow Hanford's Biology Division to pursue "more basic approaches to the problems being studied." (JCAE, "Future Roles…." 88).

biologists created an active research program; and within the Hanford Laboratories proper, a majority of the basic research took place within the Biology Program under the guidance of the AEC's Division of Biology & Medicine.

Beyond documenting the formative early history of an environmental sciences laboratory, this thesis tells a story of "scientific commoners," in order to both critique science and explore its variety.[20] Hanford scientists are well-suited to this effort because of their "ambiguous social role," to use Leo Marx words. They are at once implicated in massive environmental degradation and esteemed "as society's most knowledgeable, reliable, and effective protectors of the environment."[21] These qualities evident in the history of Hanford's scientists - social complexity and relative anonymity - provide an opportunity to explore the ordinary life of science rather than the elite, those inhabiting the fringes instead of the scientific winners. As the rhetorical and institutional power of science has expanded and the ranks of scientists have swelled in the twentieth-century, it becomes increasingly important to study the full spectrum of scientific practice – the varieties of culture and the common accomplishments of scientific communities. In the end Hanford offers a story of occasional accomplishments, secrecy, the success and failure of scientific management of nature, and above all, opportunism. It is a story about very human scientists doing their best to build careers and manage the

---

[20] Daniel Kevles used the term "scientific commoner," as I do here, to call for increased attention to everyday scientists, those largely unstudied masses who fill laboratories and universities, defining less the intellectual life of science but certainly embodying science's cultural traditions. Daniel Kevles, "Genetics in the United States and Great Britain 1890-1930: A Review with Speculations," *Biology, Medicine and Society 1840-1940*, edited by Charles Webster (Cambridge University Press, 1981), 193.

[21] Leo Marx, "Environmental Degradation and the Ambiguous Social Role of Science and Technology," *Journal of the History of Biology*, 1992, 25(3):449.

environment while (with timely irony) creating the defining, deadly element of the Cold War – plutonium.

Chapter 2

Initiating a Research Program: The Environment, Health, and Radiation at Hanford, 1943 to 1951

According to early twentieth-century boosters in Spokane and Wenatchee, the dry hills and vast alluvial flats of the Columbia River Basin had the potential to bloom and bring wealth to an "inland Empire" to rival that of Puget Sound and the Willamette Valley. To a nation full of technocratic optimism, it seemed the height of wastefulness not to harness the Columbia River for irrigation and power as it cut its S-shaped course through eastern Washington. As an expanding population in the Columbia Basin brought increasing political pressure to improve management of the river, the Army Corps of Engineers, following Franklin D. Roosevelt's approval, drew up a plan to dam the Columbia for electricity, irrigation, navigation, and flood control. Construction began on the Bonneville dam in 1933 and the linchpin of the system, Grand Coulee dam, in 1934. With the prospect of abundant irrigation water and electricity, farmers and land speculators throughout the Columbia Basin could now purchase property with confidence that the dry hills could become productive.[1]

For centuries the Columbia River provided a natural focal point to the region. As Richard White has argued, the Columbia provided energy to do work and sustain life for the land's inhabitants, from the earliest Native Americans to the later European explorers and settlers. As missionaries and settlers moved into the region, they brought

---

[1] For a detailed history of the Grand Coulee dam and its various promoters see Paul Pitzer, *Grand Coulee: Harnessing a Dream* (Washington State University Press, 1994). For a biography of probably the most influential booster see Robert Ficken, *Rufus Woods, The Columbia, and the Building of Modern Washington* (Washington State University Press, 1995). For two recent studies of the Columbia River see: Richard White, *The Organic Machine: The Remaking of the Columbia River* (Hill & Wang, 1995); and William Dietrich, *Northwest Passage: The Great Columbia River* (Simon & Schuster, 1995).

with them their own visions of how best to utilize Columbia Basin rivers in ways shaped by their own mechanized conceptions of nature. Samuel Parker, a missionary who began traversing the Columbia Basin in the 1840s, remarked in his diary that Celilo Falls offered "a situation for water power equal to any in any part of the world."[2] William Robbins has described the 1830s as "the great divide" – a time in which the region underwent dramatic demographic, cultural, and ecological transformations as euro-americans placed their mark on the landscape. Over the next century the Columbia River system and the surrounding country continued to be transformed as successive generations increasingly engineered the natural environment for agriculture, transportation, and industry. Harnessing the abundance of nature and taming its wild moments of excess was, in fact, an imperative of expansion-minded, progressive American thought. The massive river engineering projects of the 1930s were but a continuation and expansion upon a century's worth of work. Yet, visions spawned by Grand Coulee and Bonneville could not accurately predict all the changes in store for the region.[3]

---

[2] White, *Organic Machine*, 28.

[3] For the idea of a "great divide" see William Robbins, *Landscapes of Promise: The Oregon Story, 1800-1940* (University of Washington Press, 1997), 61 & passim. For various efforts to manage and transform Northwest waterways see Robbins, *Landscapes*, 238-266. For a history of the Columbia River, focusing mainly on the twentieth-century, see Dietrich's *Northwest Passage.* Donald Worster has written a comprehensive history of the major western rivers, contextualizing their use and development within the expansion of the American state: *Rivers of Empire: Water, Aridity, and the Growth of the American West* (Pantheon Books, 1985); discussion of the Columbia River can be found in chapter six. On how American thought encouraged manipulation of the river, see White, *Organic Machine*, 34, 35, 55, passim; and William Robbins, "Narrative Form and Great River Myths: The Power of Columbia River Stories," *Environmental History Review*, 17(2):1-22. Pre-World War II examples of how river 'development' promoted American ideals can be found in Lewis Mumford's "Regional Planning in the Pacific Northwest: A Memorandum by Lewis Mumford" (Northwest Regional Council, 1939); and the chapters titled "The Biggest Thing On Earth" and "Hydroelectric" in Richard Neuberger's *Our Promised Land* (University of Idaho Press, 1938).

Hanford and the Manhattan Project

Grand Coulee dam and its companions throughout the Columbia drainage certainly had the potential to improve the region's agricultural productivity. But throughout the 1940s large-scale irrigation was more hope than reality. The first canal of the Columbia Basin Reclamation Project did not begin delivering water until 1948. Electricity proved to be the more important commodity as economic and military demands of World War II began to transform the Pacific Northwest. As predicted, industry boomed along the Columbia. As a result of the war-time demand for aluminum, numerous production plants sprang up in the region, consuming 60 percent of Bonneville Power Administration mega-watt hours in 1943.[4] Less known at the time, but equally important for the war-time effort, was the military's quest for an even more exotic material – plutonium. The region's abundant hydropower as well as the relatively sparse population of the Columbia Basin attracted Manhattan Project managers, as they searched for a site to build a full-scale and unproven plutonium production facility. Plutonium producing nuclear reactors and chemical separation plants required massive amounts of electricity which Grand Coulee and Bonneville could supply without restricting residential or industrial use.

General Leslie Groves, the Manhattan Project's petulant but proficient military director, selected a site centered on the small town of Hanford, along the banks of the Columbia River. The residents of the town and the few farms dotting the surrounding 500,000 acres were ordered to leave in February of 1943. In all some 2000 landowners eventually received $5 million as the Manhattan Project bought 600 square miles of land

[4] White, *Organic Machine*, 73.

and began construction of the reactors, separation plants, and supporting structures for plutonium production. By the middle of 1944 more than 42,000 workers were transforming this barren farmland into a facility whose full purpose was known to only a handful of individuals. Full-scale plutonium production started in late December of 1944, less than two years after Du Pont and the Army Corps of Engineers began construction. Although the Columbia Basin had been on the verge of a monumental transformation with the ambitious plans for reengineering the regions' rivers, World War II redirected those changes in important ways, none more so than the decision to build the Hanford Engineer Works in the arid desert of eastern Washington.[5]

To manage Hanford, General Groves set his sights on the Du Pont Corporation for the industrial-scale production of plutonium. At the University of Chicago, Nobel Prize winning physicist Arthur Compton, leader of the scientific effort to produce plutonium and demonstrate its effectiveness as fissile material, had also recognized the need for a competent industrial partner. After initial reservations due to the unproven and hurried nature of the proposed mission, the Du Pont chemical engineering company agreed in late 1942 to assume full responsibility for the engineering, construction, and operation of the nuclear reactors and plutonium separation plants. Up to that point plutonium had only been isolated in micro-chemical quantities while Los Alamos bomb designers required kilograms of the element. Whereas normal industrial production

[5] Robert Ficken, "Grand Coulee and Hanford: The Atomic Bomb and the Development of the Columbia River," *The Atomic West*, edited by John Findlay & Bruce Hevly (University of Washington Press, 1998), 22 & passim. Richard White succinctly covers the industrial transformation of the Columbia River during the Great Depression and WWII, *Organic Machine*, 70-73, 81-88. See also Richard Hewlett & Oscar Anderson, *The New World: Vol. I, 1939-1946, A History of the United States Atomic Energy Commission* (U. S. Atomic Energy Commission, 1962), 188-190; Richard Rhodes, *The Making of the Atomic Bomb* (Simon & Schuster, 1986), 498-500, 557-60; Leslie Groves, *Now It Can Be Told*, 68-93.

proceeded from initial design through multiple, intermediate mockups, this project required Du Pont engineers to start designing full-scale nuclear reactors at a time when only one other reactor had ever been built. Possibly the most unattractive feature of this project was the expected use of the product itself – for a weapon of nearly unimaginable power. Du Pont's selection was due in large part to the company's long experience and excellent reputation for producing military explosives. Yet Du Pont had become keenly aware of negative publicity from its military business; following World War I critics had labeled the company "merchants of death" for its role in developing and supplying munitions. Only with deep reservations and careful consideration of the company's reputation did president Walter S. Carpenter accept Groves and Compton's contract. In an attempt to minimize future public relations repercussions Carpenter inserted two stipulations: Du Pont would only receive $1 of profit, and after the war they must be allowed to opt out of the atomic bomb business. Immediately following the war these provisions proved important to a sense of insecurity felt by those scientists who had made long-term commitments to Hanford.[6]

Selection of Du Pont to oversee Hanford's operation contributed to the perceived division between the scientists and engineers within the Manhattan Project. As the Manhattan Project evolved, bringing together ever larger numbers of academic scientists with industrial scientists and engineers, it became clear that considerable professional rivalries existed. Academic scientists would do research, while industrial scientists and engineers carried out production. As an industrial chemical engineering firm, scientific research was not part of Du Pont's responsibility. In fact, a contentious relationship

[6] Hewlett & Anderson, *The New World*, 29, 105-108, 187.

developed between Du Pont and some of the scientists in Compton's "Metallurgical Laboratory" (Met Lab), a result of the perceived differences in 'science' and 'engineering'. Met Lab scientists designed a graphite reactor core with 1500 channels bored into it, with just the right configuration and exactly enough space to sustain a chain-reaction. Du Pont engineers found that they could add about 500 more channels to the graphite core, providing room for more uranium fuel. The extra time and expense required to add the extra capacity drew strong criticism from Met Lab scientists who were confident in their calculations and believed the reactors would work as designed. Upon initial start-up, with the reactor filled to the scientist's specifications, the operators quickly discovered that a chain-reaction could not be sustained. An unanticipated build-up of xenon in the reactor core absorbed neutrons, gradually snuffing out the chain-reaction. However, by filling the reactor to the capacity insisted upon by Du Pont engineers, xenon-poisoning could be overcome and the reactors quickly and easily restarted at full power. In many ways this episode (and the emphasis this event has received from historians) reinforced the perceived disjunction of science and research versus engineering and production, placing Hanford squarely in the latter category.[7] For the few scientists who would attempt to establish a research program at Hanford, these perceptions presented significant obstacles for establishing their scientific respectability.

---

[7] Rhodes, *Making of the Atomic Bomb*, 557-560. Ferenc M. Szasz, "Introduction" in *Working on the Bomb: An Oral History of WWII Hanford*, edited by S. L. Sanger (Portland State University, Continuing Education Press, 1995), 5. For the perceived differences between academic scientists and industrial scientists/engineers see Peter Galison's "Three Laboratories," *Social Research*, 1997, 64(3):1127-1155. He specifically addresses the difficulties encountered between Du Pont employees and Met Lab personnel, regarding Hanford, on pages 1136-1140. David Hounshell goes into greater detail regarding these events in his essay "Du Pont and the Management of Large-Scale Research and Development," *Big Science*, 245-254.

Although Hanford's reactors nearly failed to produce plutonium, Eugene Wigner, one of the many émigré physicists from Europe who contributed to the atomic bomb program, designed simple, reliable, and effective reactors for the job at hand.[8] The design's simplicity and effectiveness for producing plutonium, however, also released unprecedented amounts of radiation into the environment. For physicists designing the reactors, the environment was of secondary consideration. Wigner's team saw the ample, clean, and cold water of the Columbia River as a ready and convenient source for reactor coolant. River water, after being treated to improve purity, neutralize acidity, and reduce its corrosiveness as it traveled through Hanford pipes, would flow directly through the reactor block, in the channels with the uranium slugs, and then be discharged into cooling ponds. After six to eight hours – time for much of the radioactivity and heat to dissipate - the water was returned to the river. Yet Manhattan Project scientists knew that considerable amounts of both radioactivity and heat would remain; the water would continue to be "hot, mildly radioactive, and contain certain toxic chemicals," all posing a "potential hazard to aquatic life."[9] Although scientists speculated that full-strength effluent would be dangerous, the dilution capacity of the Columbia was relied on to render it acceptably safe. Under normal conditions some 75,000 cubic feet of river

---

[8] Eugene Wigner took up leadership of the Manhattan Project's reactor design team in 1942, at the University of Chicago, following Enrico Fermi's proof that a chain reaction could be successfully controlled. Abraham Pais, in a short biographical sketch, writes that Wigner's team of "theoretical physicists…had to do a great number of engineering calculations." Du Pont is portrayed simply as the general contractor for reactor construction and, Pais writes, "Eugene's contacts with that corporation caused him many unhappy moments." See Abraham Pais, *The Genius of Science: A Portrait Gallery* (Oxford University Press, 2000), 341-342. See also Eugene Wigner oral history in S. L. Sanger, "Working on the Bomb: An Oral History of WWII Hanford" (Portland State University, 1995), 32-35.

[9] R. F. Foster, "Biological problems associated with the discharge of pile effluent into the Columbia River" *Biology Research Annual Report – 1951* (General Electric, Richland Washington, 1952), 11. See also the 1946 report, "Some effects of pile area effluent water on young Chinook salmon and steelhead trout" in which Foster mentions the prior knowledge of dangers, which initiated on-site studies (R-DOE, HW-7-4759), 2-3.

water flowed past the Hanford reactors per second (ranging as low as 30,000 cf/s in late summer and upwards of 400,000 cf/s in spring). With three reactors being built, the design team calculated that 1 percent or less of the river volume might be used as coolant.[10]

The whole purpose of Hanford's reactors was to convert uranium into plutonium.[11] Yet plutonium comprised but one of many products of the fission process and had to be separated from the larger quantities of other elements. Chemical separation made up the second half of the plutonium production cycle and contributed its own kind of radioactive and toxic wastes. Glenn Seaborg, co-discoverer of plutonium, led a team of chemists who devised a method of chemical separation and purification that would work at industrial scale. The key ingredient in this complex soup of chemicals turned out to be a bismuth-phosphate carrier which would ferry the plutonium through a series of reduction-oxidation reactions, slowly separating the scarce plutonium from the other, unwanted products. All these reactions produced a slew of radioactive wastes: gas, liquid, and solid.[12] Initially gaseous wastes from the separation processes

---

[10] Becker, *Aquatic Bioenvironmental Studies*, 41-45. Gerber, *On The Home Front* (Gerber claims effluent was held in retention ponds for as little as 30 minutes), 115. For dilution and river volume figures see R. F. Foster, "Some effects of pile area effluent ..." 2-3.

[11] The Manhattan Project pursued two kinds of fission bombs: one using uranium, the other plutonium. Uranium has two significant isotopes, only one of which can be used for a bomb (uranium-235). The fissionable isotope makes up less than 1 percent of natural uranium. Creating "enriched uranium" by separating the isotopes proved to be extremely difficult. Plutonium, on the other hand, could be isolated using common chemical techniques. The fact that plutonium had excellent fissionable characteristics and that it could be more easily isolated than uranium convinced Manhattan Project scientists to attempt industrial-scale production.

[12] Gaseous wastes included radioactive isotopes of iodine (produced in the greatest quantity and of the most concern to health), xenon, and ruthenium. Solid radioactive wastes included strontium, cesium, cerium, iodine, yttrium, and plutonium. Chemical wastes included bismuth phosphate, nitric acid, tributyl phosphate, hexone, and many others. Mixture of these radioactive and chemical wastes produced new, often unanticipated substances when dumped into the soil or stored in tanks. The quantities of waste are staggering: between 1943 and 1964, 149 waste storage tanks were built with a

(and the reactors, to a lesser extent) went into the atmosphere via the stacks. Crude filters limited the amount and variety of radioactive elements escaping the stacks. Longer cooling time prior to separation proved a more effective strategy for reducing radioactive gas emission, but was less desirable under General Groves' hurried war-time production schedule. For early production runs, cooling time lasted about 35 days. Liquid and solid wastes were stored in underground tanks (for the most toxic wastes) or simply dumped into the ground where, earth scientists believed, ion-exchange mechanisms would trap chemicals and prevent migration toward underground reservoirs and the Columbia River. Both the reactors and the chemical separations, therefore, introduced known but largely unstudied sources of radiological and chemical dangers into the environment.

Managing the Environment – The Birth of a Research Program

The decision to build the Hanford Engineer Works in the Columbia Basin rested in part on the notion that the sparsely populated West provided ample water, soil, and air in which to dump the inevitable waste that would be produced. The environment, in essence, could absorb whatever Manhattan Project scientists might subject it to.[13] There

---

total capacity of some 94 million gallons. Additional tanks were required by the mid 1960's. Low-level waste (defined as radioactive substances, when diluted with water, producing radioactivity of less than $5x10^{-5}$ μCi/ml) from just one of the separation plants was discharged to the ground at a rate of well over 6 million gallons per day by the mid 1950's. Gerber, *Home Front*, 35, 77, 144, 154. For discussion of Seaborg's development of a plutonium separation process see Hewlett & Anderson, *New World*, 89, 90, 182-85.

[13] The compatibility between atomic energy and the notion of an 'empty' Western environment is explored by Hevly & Findlay "The Atomic West: Region and Nation, 1942-1992," *The Atomic West*, 3-18. Stanley Goldberg has described General Groves' decision to site Hanford in the Northwest: "General Groves and the Atomic West: The Making and the Meaning of Hanford," *The Atomic West*, 39-89. Patricia Nelson Limerick has argued that dominant, but inaccurate, ideas about the West played a large role in Hanford's history: "The Significance of Hanford in American History," *Washington Comes of Age: The State in the National Experience*, edited by David H. Stratton (Washington State University Press, 1992), 153-171.

were, however, efforts to understand and minimize environmental dangers. Even though Hanford was not to be a research center in the mold of other Manhattan Project sites that studied various aspects of nuclear physics, a handful of scientists were recruited at the earliest stages to investigate local problems resulting from human and environmental exposure to radiation. The need for applied research into the health and environmental effects of radiation established the origin of Hanford's post-war radio-biological research program.

The first part of this research program came together soon after Wigner decided to use the Columbia River as the reactor coolant in early 1943. The leader of Du Pont's Hanford operation, Crawford Greenwalt (who would become president of Du Pont in 1948) met with General Groves, Arthur Compton, the noted radio-biologist Stafford Warren, and Dr. Robert S. Stone, director of the newly established Health Division of the Met Lab, to discuss the outlines of a radiation biology research program tailored to the unique problems posed by Hanford's reactors. According to the recollections of Herbert Parker, a radiation physicist in Stone's Health Division and by 1944 head of Hanford's Health Instruments division, concern for aquatic life was "due to Greenwalt's efforts to insist that we do not open this place until we were sure of possible radiation effects."[14] However, Du Pont's desire for a rudimentary radio-biological research program was also supported within the Manhattan Project. Stafford Warren, chief of the Manhattan Project's Medical Section, was keenly interested in the biological effects of

[14] Hines, *Proving Ground,* 7-10. For quotation see J. N. Stannard, "Oral History Interview: Herbert Parker" (R-DOE, Doc. # PNL-9915, date: 6/04/1979), 7.

radiation and had published important research in radiation biology during the 1930s.[15] The health and safety challenges of the Manhattan Project represented a new opportunity for radiation biologists to assert the value of their research.

The decision to initiate a small aquatic biology research program did not result in the setting up of a laboratory at Hanford because little could be done amidst the frenzied construction taking place in 1943. As a result, Greenwalt, Groves, Warren, and Stone approached a young fisheries biologist at the University of Washington with a vague research proposal. Lauren Donaldson accepted a contract, beginning in August 1943, with the Office of Scientific Research and Development to begin studies of mortality rates and developmental effects of X-rays on salmon and trout. Donaldson was told only that this research was vital to national defense. Donaldson and the university set up what they called the Applied Fisheries Laboratory and hired an assistant, Richard Foster, a recent School of Fisheries graduate. The twenty-six year old fisheries biologist had found work with the Washington State Pollution Commission but eagerly returned to the university to pursue work on an advanced degree.[16]

Donaldson and Foster's study exposed salmon and trout at various stages in their life-cycle to doses ranging up to 10,000 roentgens of x-radiation. Only a very small amount of research had been done on the effects of radiation on aquatic organisms, and coupled with the war-time urgency of the program, Donaldson's group sought only the most general limits of radiation damage. By the end of 1944 the research team, now

---

[15] Stafford Warren, "The Physiological Effects of Radiation upon Organ and Body Systems," *Biological Effects of Radiation*, edited by B.M. Duggar (McGraw-Hill, 1936), 473-539. See Hines, *Proving Ground*, 7-9, for Warren's involvement in setting up UW program.

[16] Hines, *Proving Ground*, 10. See also Richard Foster, "Some Effects on Embryo and Young Rainbow Trout (*Salmo Gairdnerii* Richardson) from Exposing the Parents to X-rays" (University of Washington, PhD, 1948), 108-109.

expanded by two new recruits, established that significant developmental problems arose with exposure levels of either larvae or fingerling salmon to 250 roentgens (R) or more.[17] Below this radiation level, Donaldson's team could not confidently identify developmental disorders arising from radiation exposure.[18]

Donaldson and his team were not told of the direct tie between their research and the secret operation going on at Hanford until the middle of 1944. With this knowledge scientists estimated that each reactor would produce an exposure level in fish equal to about 5 millirep, which in turn suggested the need for more finely tuned studies.[19] Greater detail of the motivations for starting the Applied Fisheries Laboratory in Seattle, convinced Donaldson in early 1945 to make the case for onsite studies using the actual reactor effluent in a controlled set of experiments. Greenwalt readily agreed and ordered construction of an aquatic biology laboratory. Located near the most downstream of the three reactors (F reactor, using Hanford's designation), the laboratory consisted of unused military quonset huts and twenty rearing ponds supplied with reactor effluent and

[17] First public account of this research appeared in Arthur D. Welander's "Studies of the Effects of Roentgen Rays on the Growth and Development of the Embryos and Larvae of the Chinook Salmon (*Oncorhynchus tschawytscha*)" (PhD thesis, University of Washington, 1945). Journal publications began to appear in 1948: A.D Welander, L.R. Donaldson, R.F. Foster, K. Bonham, and A.H. Seymour, "The Effects of Roentgen Rays on the Embryos and Larvae of the Chinook Salmon," *Growth*, 1948, 12:203-242; K. Bonham, L.R. Donaldson, R.F. Foster, A.D. Welander, and A.H. Seymour, "The Effect of X-ray on Mortality, Weight, Length, and Counts of Erythrocytes and Hematopoitic Cells in Fingerling Chinook Salmon, *Oncorhyncus tschawytscha* Walbaum," *Growth*, 1948, 12:107-121. For reference to previous radio-biological research, see Hines, *Proving Ground*, 12; and A.D. Welander, et al, "The effects of roentgen rays…" 241-42. Publication of AFL research in *Growth*, a new journal dedicated not to a discipline but to "a fundamental property of nature regardless of the discipline," suggests both the novel nature of Donaldson's research and that the research was not easily incorporated into existing disciplinary domains.

[18] Gerber, *Home Front*, 118.

[19] Radiation measures can be confusing and terms have changed considerably since the 1940's. The roentgen (R) is a measure of x-ray dose no longer in common use. The roentgen, however, is basically equal to the rep (roentgen equivalent physical). Therefore 250 R is considerably larger than 5 millirep, by orders of magnitude.

river water. In July of 1945 Richard Foster moved to Hanford and began rearing thousands of fish in various mixtures of effluent and river water.[20]

Research on the economically important fish runs of the Columbia River was important, but fish would not be the only creatures exposed to radiation. Once the reactors began operating, thousands of Hanford workers handled radioactive material on a daily basis. Du Pont created the "Health Instruments" division to monitor radiation doses, assess and implement exposure standards, and operate a massive detection program. As at all other Manhattan Project sites, Hanford management knew that radiation would be a working hazard for most Hanford employees. Prior to Hanford's start-up, the Met Lab at Chicago began a radiation monitoring program under the direction of Robert Stone in July of 1942. With radiation protection experience gained from years working at Ernest Lawrence's Radiation Laboratory, Stone instituted safeguards for the scientists working on the various aspects of uranium enrichment, 'pile' operation, and plutonium isolation. Needing to recruit experts on radiation safety, Stone found two in Seattle - Herbert Parker, a physicist in his mid thirties and a world expert on therapeutic radium dosimetry, and Simeon Cantril, an established radiologist and authority on the treatment of cancer with radiation.

Parker earned a Master of Science in Physics from the University of Manchester in 1931, acquiring several distinguished honors along the way. Unable to secure financial support to continue his education, Parker found a research position at the Holt Radium Institute where he made important contributions to the development of standardized medical radium dosimetry. After receiving almost immediate recognition

[20] Hines, *Proving Ground*, 11-13; Becker, *Bioenvironmental Studies*, 82-85. Foster, "Some effects of Pile area effluent..." (op. cit. 9), 1-6.

for his work, Parker accepted an offer to move to the United States. At the age of twenty-eight, he began collaboration with the radiologists Simeon Cantril and Franz Buschke at Swedish Hospital's Tumor Clinic in Seattle.

World War II offered new opportunities to Parker. With the start of the Manhattan Project in 1942, both Parker and Cantril were asked to lend their expertise to develop safety standards for the thousands of workers who might be exposed to radioactive substances. After short stints at Chicago and Oak Ridge (the site of the uranium enrichment project and where the first plutonium producing reactor was built), Parker returned to Washington State in the summer of 1944 to oversee radiation protection at Hanford as director of the Health Instruments division.[21] The scope of radiological monitoring Parker directed at Hanford was truly unparalleled. All workers who might possibly be exposed to radiation wore some form of radiation monitoring device. Through the first eight months of 1945 and with production at full throttle for the first time, Parker's Health Instruments division processed over 1.5 million measurements.[22]

With an extensive monitoring program requiring Parker's primary attention, research could not be a top priority. Parker has recalled that Du Pont's contract "did not include any research" yet "there were many scientists here available to improve the operational program." Because of the lack of a research environment most scientists, especially ambitious individuals working in traditional disciplines, remained at Hanford

---

[21] Gerber, *Home Front*, 48-51. For start of the Met Labs "Health Division" see Hewlett & Anderson, *New World*, 206-207; also Barton Hacker, *The Dragon's Tail: Radiation Safety in the Manhattan Project, 1942-1946* (University of California Press, 1987), 31. For biographical information on Parker, see Ronald Kathren, "A Brief Scientific Biography," *Herbert M. Parker: Publications and Other Contributions to Radiological and Health Physics* (Battelle Press, 1986), xiii-xxiv.

[22] Gerber, *Home Front*, 51.

only for the duration of the war.[23] Jane and David Hall, two young physicists just out of the University of Chicago typify this generalization. Both came to Hanford to assist in the design and operation of the reactors. However, since Du Pont prohibited spouses from working on the same project, Herbert Parker convinced Jane Hall to manage an ad hoc "special studies" project designing radiation protection and measurement devices. In Jane Hall's short stay at Hanford she became one of the few women to have a leadership role as a manager and researcher.[24] With the end of World War II the Halls left Hanford for the more stimulating research environment (for physicists) of the Los Alamos scientific laboratory. Yet of the small group of scientists associated with Hanford, not all could (or wanted) to leave. For scientists interested in the health and environmental effects of radiation, Hanford offered opportunities for research that few places could match. Parker and Foster remained at Hanford, hoping to continue their research and possibly establish a permanent radio-biological program.

Post-war Reorganization - Applied Radio-biology in the Columbia Basin

Hope for a research program at Hanford, however, had to overcome major obstacles. In March of 1946 Du Pont announced to General Groves that the government would need to find a new manager for Hanford or shut the facility down. Though all

---

[23] S. L. Sanger interviewed eleven physicists and six chemists involved in Hanford's early history. Of these only one physicist and two chemists remained after 1945. Normal Hilberry explained the fundamental reason most physicists and chemists chose not to remain at Hanford after the reactors and separation plants had been successfully tested: "Du Pont took absolutely no responsibility for the basic science." Sanger, *Working on the Bomb*, 43.

[24] For Parker's descriptions of research during the war see J. N. Stannard's oral history interview (R-DOE, Doc # PNL-9915), 4-5, 10-11. Jane Hall's influential role in the upper levels of Hanford operations reminds us that, especially at Hanford, this was a very male dominated environment. No women played significant roles in the establishment of Hanford's radio-biology research laboratory, but for an overview of women's contributions at Hanford, including Hall and others, see Ruth H. Howes & Caroline L. Herzenberg, *Their Day in the Sun: Women of the Manhattan Project* (Temple University Press, 1999), 140-144 & passim.

Manhattan Project contracts had been written to expire six months after hostilities ended, General Groves approved extensions while the Truman administration and Congress wrangled over legislation creating an atomic energy agency. Du Pont's withdrawal from Hanford and the short-term contracts being offered by Groves created a feeling among local observers that Hanford might be closed down in the near future.[25]

In fact Northwesterners' first in-depth public description of Hanford suggested a bleak future. Writing for the *Oregonian* in February of 1946, historian Earl Pomeroy painted a vivid picture of Hanford, depicting it as a typical Western boom town about to bust:

> "They are tearing down the badlands Bagdad-on-the-Columbia, latter-day city of the 700 Nights which, though not Arabian, certainly were arabesque.
> They are pulling asunder what was America's largest civilian construction camp.
> What now is the greatest of the nation's ghost towns?
> The community which profoundly may have altered the course of the world's destiny.
> That would be Hanford, on the great bend of the river in south-central Washington.
> It was the city which provided sweat and sinew for the creation of a miracle in the sand and sage – the Hanford Engineer Works, which has wrought something not existing in nature, plutonium, the terrible element of the atomic bomb.
> It was the city in which lived, for varying periods, an aggregate of 137,000 persons who built they knew not what, but who did it marvelously well.
> It was a community which, while aborning, knew that its life span was to be most short and, in the interests of the United States, the shorter the better.[26]

To those actually working at Hanford it was clear that some level of plutonium production would continue for the immediate future. Yet unsettled issues remained:

---

[25] Hewlett & Anderson, *New World*, 629.

[26] Earl Pomeroy, "Back to Ghosts and Goats," *Oregonian*, Feb 24, 1946, Sunday Magazine, page 1.

would there be international control of atomic energy? If so, what would that mean for domestic plutonium production? Would federal legislation create a civilian or military agency? How much plutonium would be needed in the coming years? And for Hanford scientists, would Du Pont's replacement discourage, tolerate, or encourage biological research at a production plant?[27]

As insecurity rippled through scientists in the Manhattan Project complex, (especially at Oak Ridge, Los Alamos, and Hanford), there were also signs that research would form an integral part of postwar atomic policy.[28] Prominent scientists such as Robert Oppenheimer and Vannevar Bush were intimately involved in shaping legislation as various proposals made their way through congress. Bush, architect and director of the Office of Scientific Research and Development during the war was now a seasoned political insider and keenly interested in creating a post-war environment more conducive for research in federal agencies. In reference to an atomic energy agency, Bush's landmark report *Science – The Endless Frontier* appeared in July of 1945 and clearly argued in favor of a "civilian-controlled organization with close ties to the Army

---

[27] International control of atomic energy was a distinct possibility in early 1946. A number of scientists in the Manhattan Project (specifically the Committee on Political and Social Problems) argued for international control in a written report, June of 1945. For those in favor of it, international control would prevent nations from engaging in an arms race. After the war and through the summer of 1946 numerous proposals suggested means of securing international control; all would have limited the production of fissionable material. An International Atomic Energy Agency did not come into being until 1957, much too late to prevent an arms race. For international control strategies through 1946 see Hewlett & Anderson, *New World*, 531-619; for a more recent treatment see Allan M. Winkler, *Life Under a Cloud: American Anxiety About the Atom* (Oxford University Press, 1993) 34-83. For uncertainty over the future of Hanford and other Manhattan Project sites see: Gerber, *Home Front*, 61-62; Hewlett & Anderson, *New World*, 625-628, 633-637.

[28] The tenuous existence of Oak Ridge and Los Alamos have been well documented. Hewlett & Anderson cover both sites, *New World*, 624-637. For Oak Ridge and Los Alamos respectively see Leland Johnson and Daniel Schaffer, *Oak Ridge National Laboratory: The First Fifty Years* (University of Tennessee Press, 1994), 26, 49-52; Edith C. Truslow & Ralph Carlisle Smith, *Project Y: The Los Alamos Story, Volume II of The History of Modern Physics, 1800-1950* (Tomash Publishers, 1983), 265-270.

and Navy, but with funds direct from Congress, and the clear power to initiate military research." More broadly, Bush articulated distinctions between "basic" versus "applied" research. "Basic" science, he argued, must be supported in the nation's universities. On the other hand applied research must be vigorously pursued by "government and industry" in consideration of "national welfare."[29] Bush's blueprint for post-war science was highly influential. For Herbert Parker, an opportunity to promote his own applied research program at Hanford found ample justification within this new post-war mandate for science.[30]

By mid-summer 1946, those within Hanford found out General Electric had agreed to take over Hanford management from Du Pont. Reinforcing the perception that Hanford lacked a research environment, terms of the agreement stated that GE could develop a government funded fundamental research center on property in upstate New York. The Knolls Atomic Power Laboratory, focusing on atomic reactor development, would be GE's center for research.[31] While GE's lack of interest in a Hanford centered

---

[29] Vannevar Bush, "Science – The Endless Frontier: a report to the President" (United States Government Printing Office, July 1945), 1-3. See also the text of Bush's speech before a Senate Committee on atomic energy, 3 Dec., 1945: "Control of Atomic Energy" (printed in Vannevar Bush, *Endless Horizons*, Public Affairs Press, 1946, pages 101-106). Bush specifically noted that "the manufacture of fissionable materials…is accompanied by the production of radioactive by-products as poisonous as the basic material itself…. Improper or incautious manipulating of substantial amounts…by inadequately trained or irresponsible investigators is a danger to the public safety which government must avert." In conclusion he stated that any legislation "must insure to the American people their control of plant and process, must safeguard knowledge of the military applications of atomic energy, must properly guard the physical well-being of the people against the many hazards to life and health which the investigation and production of atomic energy involve, must provide free and full research and interchange of knowledge in this new and promising field…." Parker may not have had direct knowledge of Bush's statements, yet the broad outlines of the debate over government supported research, and what kinds of research (basic or applied), would certainly have been familiar to him.

[30] For an analysis of Bush's report, and an exploration of its impact see Daniel Kevles' essay "Principles and Politics in Federal R&D Policy, 1945-1990: An Appreciation of the Bush Report" in a reprint of "Science – the endless frontier" (National Science Foundation, 1990), i-xxxi.

[31] Hewlett & Anderson, *New World*, 629.

research program did not preclude ongoing health and environmental research, little enthusiasm existed for expanding the small research program in eastern Washington. Accordingly, Parker composed a memo outlining his goals for a minor "radio-biological research and development" at Hanford in late July of 1946. Reflecting the uncertainty over Hanford's future and GE's lack of clear support, Parker struck a hesitant, tentative tone. In making the case for a research program in the biological sciences, Parker emphasized its limited and applied nature. "The following brief account" he began, "covers the general proposals for possible...biological studies and may form the basis for a discussion of the merits of a small radio-biological program here." He then stressed how "the whole program is keyed to the practical development side, with no fundamental research included." In accord with similar efforts at Argonne and Brookhaven to include regional universities in the development and operation of research, Parker noted that "stress is placed here on the merits of close cooperation with existing biological research units, with State Colleges for agricultural contacts." To reemphasize the modest goals of radio-biological research at Hanford, Parker concluded by stating, "the proposed program should be minor in comparison with established research groups [referring to research programs at the University of Chicago and the University of California, Berkeley]. In particular, the establishment of a regular laboratory animal farm at Hanford would be singularly inappropriate."[32]

---

[32] H. M. Parker to W. E. Milton (GE), July 26, 1946, "Preliminary Suggestions for Radiobiological Research and Development at Hanford Engineer Works or in Association with other units." Department of Energy, Coordination & Information Center, Nevada. (Hereafter cited as CIC) Document # HW-8527. (The 1946 memo is included as an attachment to a 1948 report identified as HW-8527).

Though the emphasis on regionally important and applied research was appropriate for the time, Parker's cautious proposal is striking. His colleagues at other Manhattan Project sites were building research programs of their own, but in a more aggressive manner. The other major research centers were: Argonne; Oak Ridge (then known as the Clinton Labs); Los Alamos; Ames (Iowa State College); and the new entrant representing prestigious Northeast colleges vigorously arguing for their own, regional nuclear-sciences research center at Brookhaven, Long Island.[33]

Oak Ridge, like Hanford a production site with an undefined future following the war, provides a particularly interesting comparison with Parker's research program. Eugene Wigner assumed leadership of the research division in early 1946 and proceeded to build ambitious programs not just in the physical sciences, but in the biological sciences as well. Wigner brought in the energetic and articulate young physicist Alvin Weinberg as an assistant director and attracted prominent program leaders such as Alexander Hollaender. Those bold moves ensured that the incoming commissioners would have to cut a well-established program, an unlikely course of action. Moreover,

---

[33] The early development of research programs at Argonne, Oak Ridge, Los Alamos, and Brookhaven have been well documented. For an overview see Westwick, *The National Laboratory System*, 1-30. Argonne's development into a 'national laboratory' can be found in Jack M. Holl, *Argonne National Laboratory, 1946-96* (University of Illinois Press, 1997), 47-80; and Leonard Greenbaum, *A Special Interest: The Atomic Energy Commission, Argonne National Laboratory, and the Midwestern Universities* (University of Michigan Press, 1971), 1-29. For Oak Ridge, see Johnson & Schaffer, *Oak Ridge*, 26-52. For Brookhaven, see Robert Crease, *Making Physics: A Biography of Brookhaven National Laboratory, 1946-1972* (University of Chicago Press, 1999), 7-46. Very little has been written about the history of the Ames research program which remained small and highly focused on uranium metallurgy. However, some information on how it fit within the other AEC laboratories can be found in Westwick, *The National Laboratory System*, 15-20. E. O. Lawrence's Radiation Laboratory had a unique relationship with the AEC; in the early post-war years it was neither wholly government owned nor completely unattached from AEC programs, thus it presents a very different case. For its history see John L. Heilbron and Robert W. Seidel, *Lawrence and His Laboratory: A History of the Lawrence Berkeley Laboratory*, (UC Berkeley Press, 1989); and Robert W. Seidel, "Accelerating Science: The Postwar Transformation of the Lawrence Radiation Laboratory," *Historical Studies in the Physical Sciences (HSPS)*, 1983, 13(2):375-400.

unlike Parker's emphasis on limited, applied research, Oak Ridge biologists took a different approach. Alexander Hollaender, recently arrived from directing a radio-biology laboratory at the National Institutes of Health, made a proposal in which the "basic aspects of the effects of radiation on living cells" would form the foundation of their research.[34] Part of this research effort soon came to include William and Liane Russell's massive mouse genetics study, a program that received the endorsement of eminent geneticists Sewell Wright and Herman Muller. In one year the Oak Ridge biology program would include more than seventy scientists. As Brookhaven national laboratory came into being, it made an even more aggressive push to focus on basic research. With powerful members on its board of directors, including Isidor I. Rabi, Lee DuBridge, and Henry Smyth, Brookhaven successfully sold itself as the nation's laboratory for basic nuclear science research.[35]

In contrast to Hanford, then, Oak Ridge rapidly established a vigorous and respected research program, formally declared a "national laboratory" in 1948. Hanford took nearly a decade to gain recognition as a research center with independence from the overall mission of the site. Much of this disparity may be attributable to the differences in prestige and leadership qualities of the laboratory directors, Parker and Alvin Weinberg (Wigner only managed the Oak Ridge laboratory for one year before returning to Princeton). Whereas Weinberg had a PhD in physics from the University of Chicago, Parker had only received an MSc from the University of Manchester. Both were physicists: Parker became known as one of the early founders of a relatively obscure

---

[34] Richard Hewlett & Francis Duncan, *Atomic Shield: A History of the United States Atomic Energy Commission, 1947-1952*, (U.S. Atomic Energy Commission, 1972), 505-506. Hewlett and Duncan quote from Hollaender's "Outline of Research of Biology Division," Dec. 1, 1946.

[35] Hewlett & Duncan, *Atomic Shield*, 505-506 & 224-226. Johnson & Schaffer, *Oak Ridge*, 37-39.

discipline, "health physics," whereas Weinberg was an accomplished nuclear physicist. Most importantly, Weinberg was, by all accounts, an articulate, and supportive manager who fostered a creative, progressive environment for his scientists while at the same time developing a high profile in government circles and among the general public through service on the Presidential Science Advisory Committee and in numerous, widely distributed publications. Parker has been described as shy, uncompromising of fools, and quick to criticize. He published a handful of highly technical contributions to health physics and radiation protection. His most important committee assignment came as chairman of numerous subcommittees of the National Committees for Radiation Protection. In short, qualities of leadership such as charisma, research reputation, leadership style, and institutional power, sociological attributes explored in Gerald Geison's studies of research schools, can help to make sense of the very different trajectories of the early Oak Ridge research program versus Hanford's. The absence of a "charismatic leader" at Hanford helps to explain the research program's early struggles as much as the presence of Weinberg helps explain Oak Ridge's sudden success. (Similarly, the establishment and immediate prominence of the Livermore laboratory may be partly understood through the well-documented "charisma" and "institutional power" of E. O. Lawrence and Edward Teller.) Hanford, on the other hand, developed slowly and in relative obscurity. Parker's leadership qualities, his lack of prestigious academic credentials and a personality that combined shyness and combativeness, certainly would affect the reputation of 'his' laboratory.

This kind of sociological analysis may be useful in understanding the evolution of the Hanford Laboratories in relation to other, similar institutions.[36]

Parker's comparatively less successful institution building aside, his modest effort to initiate a biological research program had both a local base of support and a short history of success. Topping the list of accomplishments was the ongoing research with aquatic life which was of "economic interest to the surrounding communities." Parker argued that these studies should be continued in order to identify the most harmful constituents of reactor effluent and insisted that a broadened examination of the river system, including the "feeding of aquatic life on" radioactive foods was "particularly needed." Recognizing that considerable radioactive waste gases were being released, Parker laid out a plan to study the absorption of radioactive elements "on representative plant life of the Pacific Northwest." Finally, the most significant new proposal involved establishing a program to study the "exposure of important domesticated animals and fowls to activities [radiation] acquired from contaminated food." Though this would be an expensive study, Parker argued, "the potential findings...are of critical value in the agricultural States in the Northwest, and justify the

---

[36] Qualities of leadership in 'research schools' have been explored by Gerald Geison, "Scientific Change, Emerging Specialties, and Research Schools," *History of Science*, 1981, 19(1):20-40. Many of the thorny but intriguing questions that emerge in studies of 'research schools' have been more recently addressed in a volume of *Osiris* edited by Gerald Geison and Frederic L. Holmes, *Research Schools, Historical Appraisals* (2nd series, 1993, Vol. 8). John Servos' essay ("Research Schools and their Histories," pp.3-15) suggests that while the term 'research school' may be defined in many ways, one of the main values in using this conceptual framework is in allowing the historian to "move freely from consideration of the largely private realm of creative effort to the more public arena of justification and persuasion" in the study of groups of scientists tied to a particular time and place. On the nebulous, notion of charisma, originally used by Max Weber as an antithetical leadership quality of the modern age, has been studied as it pertained to an important Manhattan Project scientist (and the institution he is credited with founding). See Charles Thorpe & Steven Shapin, "Who Was J. Robert Oppenheimer? Charisma and Complex Organization," *Social Studies of Science*, 2000, 30(4):545-590. To get a sense of Parker's personality, see the oral history with J. N. Stannard (op. cit. 14); and Kathren, "A Brief Scientific Biography," *Herbert M. Parker*, xiii-xxiv.

proposed investigations." To allay potential fears of unauthorized empire-building, Parker explained that he only needed a handful of new scientists, including a biochemist and a "plant biologist."[37]

The incoming GE managers and Atomic Energy Commissioners found no reason to scuttle this small proposal. GE's primary research interests lay in nuclear reactor technology, and envisioned great economic rewards from developing research expertise in nuclear power technology. However, with the AEC funding radio-biological research, GE had no objection to Parker's small program. AEC administrators, on the other hand, apparently had little time to monitor Hanford projects as long as it plutonium production continued. Beginning November 12, 1946 the newly appointed AEC commissioners toured Oak Ridge, Los Alamos, the Radiation Laboratory at Berkeley, and Argonne. Although Hanford had been on the schedule, more pressing business convinced Commission members to bypass the Columbia Basin altogether.[38] The AEC's General Advisory Committee (GAC), composed primarily of physicists, also had larger issues to tackle. Led by J. Robert Oppenheimer, the GAC spent its first meetings deliberating over the management, mission, and location of the Commission's major laboratories – Argonne, Oak Ridge, and Los Alamos. Oppenheimer's committee argued over whether the Commission should combine nuclear physics research at one central laboratory (a proposition the GAC soon dismissed) or support the scattered laboratories as they had

[37] Parker memo, July 26, 1946. HW-8527. (op. cit. 32)

[38] For GE's Schenectady research center, see Hewlett & Duncan, *Atomic Shield*, 62-63. Background for pre-WWII research at GE can be found in George Wise, *Willis R. Whitney, General Electric, and the Origins of U.S. Industrial Research* (Columbia University Press, 1985); also see Leonard S. Reich, *The Making of American Industrial Research: Science and Business at GE and Bell, 1876-1926* (Cambridge University Press, 1985). For lack of attention to Hanford see Hewlett & Anderson, *New World*, 642.

evolved during the Manhattan Project. While the GAC's decision to allow multiple laboratories to compete with each other (a prized American method of stimulating creativity) had nothing to do with Hanford, the long-term implications of this policy opened the way for all Manhattan Project sites to develop into 'national laboratories' over time. However in early 1947 the primary research problem for the AEC inevitably involved reactor physics, not radio-biology. In short, a small radio-biology program at Hanford received almost no scrutiny from early AEC policy makers despite efforts to streamline research programs and curtail unnecessary expansion.[39]

The low profile of Parker's proposed research program may have spared it criticism from the top levels of the AEC, but in order to ensure long-term stability he needed to cultivate support within the agency. As a member of Stafford Warren's "Medical Board of Review" in early 1947, Parker helped formulate an AEC research program that would encompass the distinct interests of biologists and medical researchers. Warren's report urged the AEC to become more actively involved in support of radio-biological studies which they described as a vital field of research as the nation moved into the atomic age. In an endorsement of "regional laboratories," the Board's report called for the AEC to make "its unmatched equipment and unique conditions for observation" available to the nation's biological scientists.[40] Based on

[39] For discussion of production issues and regional or centralized laboratory see GAC meeting minutes #1-4, Germantown DOE Archives, GAC Minutes file, Folder #1. Early GAC meetings dealt primarily with production issues and with developing a comprehensive research policy. The committee expressed concern that some labs were expanding outside the immediate scope of AEC interests. This was particularly true at Oak Ridge about which the GAC cautioned: "expansion cannot be contemplated in view of the present extended commitments of the Committee in support of research and the need for urgent programmatic research and development." (GAC meeting #3, March 28 1947, p.16.)

[40] "Report of the Medical Board of Review," 4. DBER, Box 103, Folder 2.

this report, the AEC created a "Division of Biology and Medicine" in 1947 (with its own scientific advisory committee[41]) under the direction of Shields Warren. Discussion and approval of Parker's radio-biology program proposal finally took place at the second meeting of the new Division's advisory committee now that interested colleagues held positions within the AEC.[42] Support from the AEC allowed Parker to proceed with the modest expansion of his program.

Harry Kornberg, a thirty-three year old biochemist became the first recruit. Following military service Kornberg landed a teaching job in Oregon State College's chemistry department, where the department chair described him as "a man of considerable promise." But after one year in Corvallis, Kornberg jumped at an offer in 1947 to direct Hanford's newly established radio-biology program. There is no evidence that Kornberg worked with radiation in his academic research which illustrates what one historian of industrial science has noted: transferring from an academic to an industrial research environment often entails the adoption of entirely new techniques and research problems.[43] Not only did transfer to Hanford provide Kornberg with a whole new set of

---

[41] Both Richard Hewlett and Richard T. Sylves have argued that the AEC's advisory committees, the GAC and the newly formed Advisory Committee for Biology and Medicine, were the most influential policy making bodies in the early years of the agency. See Hewlett & Duncan, *Atomic Shield*, 16 (See also Alice Buck's "Institutional Origins of the U.S. Department of Energy" [http://www.dpi.anl.gov/dpi2/instorig/instorig1.htm] footnote 3); Richard T. Sylves, *Nuclear Oracles: A Political History of the General Advisory Committee of the Atomic Energy Commission, 1947-1977*, (Iowa State University Press, 1987), 18.

[42] Minutes of Advisory Committee for Biology & Medicine, meeting #2, Oct. 11, 1947 (DBER, Box 103, Folder 4).

[43] George Wise, "Ionists in Industry: Physical Chemistry at General Electric, 1900-1915," *Isis*, 1983, 74:7-21. Wise also mentions another disjunction between academic and industrial science pertinent to Hanford's early research history: "Academic scientists found it impossible to carry over unscathed into industry ... their publication practices" (p.9). Kornberg's early research focused on the biochemistry of a common bacterium associated with fermentation – *Leuconostoc mesenteroides*. See Kornberg, Langdon, Cheldelin, "Microbiological Assay for Riboflavin," *Analytical Chemistry,* 1948,

research problems, but accepting the offer to work for GE included a handsome pay increase. At Oregon State College Kornberg earned $4,200, a sum described as significantly higher than similar aged colleagues in the chemistry department. At GE his salary started at $6,500.[44] The research environment at Hanford may have paled in comparison with other AEC laboratories such as Brookhaven, Argonne, or Oak Ridge where the kind of "programmatic"[45] research characteristic of Hanford played a less prominent role. Nevertheless, the opportunity to work at Hanford had benefits in the novel kinds of research projects that could be pursued and the generous salary. GE's industrial pay-scale would prove to be an important issue a decade later when the radio-biology program considered changing to an academic manager.

Kornberg's arrival stimulated an effort to broaden the research program in order to make it more attractive to academic scientists. Evidence of this can be seen in Parker's updated radio-biology research proposal sent off in early 1948 to the new director of AEC biological research, Dr. Shields Warren. Using the report written for the Medical Board of Review, Parker laid out a revised plan that would more "closely parallel the freedom of scientists in academic circles."

> "One of the major satisfactions of an investigator in a university or an enlightened research institute is his freedom to choose his research problems. The makers of Atomic Energy Commission policy will realize that the motives of war-time service must now undergo a change because

---

20(1):81-83; Cheldelin, Nygaard, Kornberg, Williams, "Vitamin B_6 Phosphates, Growth Factors For *Leuconostoc mesenteroides*," *Journal of Bacteriology*, 1951, 62(1):134-135.

[44] Harry Kornberg, "Personal File," Oregon State University Archives. For quotes see letter from F.A. Gilfillan (dean, school of science) to A.L. Strand (President), May 17, 1947; letter from H.A. Kornberg to F.A. Gilfillan, Aug. 18, 1947.

[45] "Programmatic" as used by the AEC meant research assigned by agency managers that directly addressed the practical concerns of weapons production and atomic energy. It could be both "basic" and "applied," but the term invariably indicated a lack of autonomy on the part of the individual scientist to define his or her own research problem.

> scientific workers will find programmatic research or imposed assignments unsatisfying unless sweetened with freedom to follow their own choice of problem for part of their working time..."[46]

Since the "work planned is entirely programmatic," he argued, some leeway must be allowed because "it has been found extremely difficult to attract...men to an organization exclusively concerned with programmatic research."[47] With a few years experience behind him, Parker was beginning to acquire the skills necessary to successfully manage a research program – namely, the talent to balance the patron's goals (programmatic research) with the desires of his scientific staff to pursue some independent research.

Although Parker argued for a more ambitious program, in fact the broad outlines of the 1948 research proposal remained largely unchanged from the 1946 proposal. The Aquatic Life section continued to be the most prominent feature of the program, expanding studies of contamination of Columbia River biota as the Commission pushed ahead with reactor construction. Research into the effect of Hanford emissions on terrestrial plant life was progressing "poorly" due to lack of support from the AEC. Parker reasoned that a more vigorous program was needed in order to better understand how radio-elements, specifically iodine-131, traveled through the food chain. Thyroid sampling studies were showing "extensive contamination" in wild ducks and rabbits. Parker's proposal concluded by noting that approval of a domestic animal farm had allowed Hanford scientists to set up a regionally important study of long-term effects on sheep of ingested radio-iodine. As long as funding remained in the AEC budget, Parker

[46] Herbert Parker, "Present Status of the Hanford Works Biological Program, and its Proposed Future Development" (CIC, HW-8527), 1.

[47] Ibid, 1.

believed the full study could begin by early 1949 along with smaller studies using cattle, pigs, horses and poultry.[48]

When Parker's report made it to the Division of Biology & Medicine's advisory committee in October of 1948, both positive reaction and a touch of reservation greeted the proposal. The aquatic biology program and large animal farm, deemed "highly desirable…because of problems of specific concern to operations at Hanford," drew enthusiastic support and were awarded a construction budget of $800,000. But Parker's effort to convince AEC administrators to create a formal "national laboratory" out of Hanford's research program met with a cool response. The advisory committee felt it would be "unwise to set up an additional national laboratory at Hanford as it was primarily a production center and research in biology and medicine there would be directly applicable to local problems." In the AEC's view Hanford's radio-biology research program should remain narrow in scope and focus almost exclusively on applied science of local interest. Federal managers sought to curtail any ideas of a rapid expansion of Hanford's overall research program, thereby making it unsuitable for "national laboratory" status.[49]

Fred Schlemmer, the AEC's Hanford Operations manager, explained the Commission's position regarding research expansion at Hanford in a cautionary letter to the GE manager and to Shields Warren. While supportive of a limited research function at Hanford, Schlemmer's 1948 letter emphasized that "fundamental" research, as well as "any portion of the programmatic research which is suitable," must be pursued "at the

---

[48] Ibid, 1-3.

[49] Minutes of Advisory Committee for Biology & Medicine meeting #12, Oct. 8,9, 1948 (DBER, Box 103, Folder 5). For budget approvals, also see Minutes of meeting #3, Nov. 7, 1947 (Box 103, Folder 4).

national laboratories, Knolls, or one of the university laboratories under contract to the Commission." Yet, he would not shut the door entirely on basic research at Hanford. Recognizing the need to keep Hanford scientists content, he concluded "there may be other infrequent and isolated cases where it would be desirable to conduct fundamental work at Hanford."[50]The Atomic Energy Commission was attempting to walk a fine line – there was great pressure to promote radiological sciences at its own facilities, yet the federal government's funding of science remained a relatively new and controversial practice. In what ways and to what extent the federal government should be involved in science provoked impassioned debate as the nation first demobilized from World War II and then remobilized against the perceived threats of the Cold War. Henry Smyth, a Princeton physicist and the second scientist appointed to the five member Commission,[51] defended the AEC research programs but simultaneously tried to convince AEC scientists that there were important different differences between academic and government laboratories. In a letter to the head of the Joint Commission on Atomic Energy, Senator Brien McMahon, Smyth made a case for "the importance of research"

---

[50] Letter from Fred Schlemmer to GE (R.C. Muir) and AEC/DBM (Shields Warren), Sept. 17, 1948. "Subject: Proposed Technical Center." (DBER, Box 103, Folder 5). Schlemmer's letter illustrates an obstacle Hanford scientists had to deal with as, in later years, critics questioned the 'objectivity' of their research: "Certain basic policy considerations will limit the extent of any program and facilities approved for Hanford, and should be used as a guide in preparing the overall program.... [these include] **D.** Studies necessary to insure against hazardous contamination of plant, animal and marine life indigenous to the area affected by Hanford operations. **E.** *Studies necessary to establish the legal position of Hanford Works in the event of legal action based on alleged damage caused by hazards originating from the operation of the Hanford Works.*" (Italics added for emphasis) A prime example of AEC sponsored science (and scientists) being viewed as inevitably biased in support of AEC projects is Project Chariot, the AEC's effort to build a build a marine harbor using nuclear explosives. Hanford radio-ecologists were responsible for the terrestrial ecology survey associated with Project Chariot. See Dan O'Neill, *The Firecracker Boys*, (St. Martins Press, 1994). While O'Neill takes the position that AEC research was compromised, J. Samuel Walker has argued that AEC science was generally of good quality but suffered from public perceptions of bias. See Walker's article, "The Atomic Energy Commission and the Politics of Radiation Protection," *Isis*, 1994, 85:57-78.

[51] The five member commission appointed to five year terms included, by tradition, one scientist who often became the AEC's spokesman on scientific matters.

within a national-defense agency. Although one may think it unrelated to a strong defense, Smyth explained that fundamental biological research was critical for understanding the effects of radioactive substances and therefore vitally important for the protection of the American people. Furthermore, the AEC must provide consistent support to researchers, because good science could not be "turned on and off."[52]

In a speech at Oak Ridge a few months later, Smyth stressed the need for combining basic and applied, theoretical and practical, kinds of research at AEC laboratories.

> "Few of our great laboratories, and I hope none of those supported by the AEC, are so narrowly dedicated to the immediate, practical results as to ignore completely basic scientific research. By the same token I hope none of the AEC laboratory groups will ever look down on applied work. With this kind of intellectual snobbery I have no sympathy. There is nothing inherently evil or degrading about being useful."[53]

Whereas universities were seen as the proper home for basic research, and industry for applied, the AEC consciously tried to bridge this gap. Too much applied research might deprive industry of valuable, patentable technologies while difficulty in explaining the utility of 'fundamental' research provided a ready target for taxpayer anger.

Plutonium Production and the Justification of Research

The small amount of research taking place at Hanford in the late 1940s certainly could be described as applied and programmatic. Manhattan Project scientists, and later AEC managers, supported aquatic biology research, ostensibly, to ensure safe operation

---

[52] Letter from Smyth to McMahon, July 28, 1949. Henry M. Jackson papers, University of Washington Archives, Box 59, Folder 1. This collection hereafter cited as HMJ.

[53] Press release of Smyth speech, "General Information Meeting Banquet, Oak Ridge, Oct. 25, 1949" (DBER, Box 34, Folder 22).

of Hanford's reactors. Sampling of terrestrial and aquatic biota for absorption and accumulation of radio-elements, such as iodine-131 and phosphorus-32, showed where much of the contamination came to rest and how it moved through food chains. This knowledge could be used to limit Hanford production cycles when dangerous levels of contamination were observed. By 1950 controlled experiments with domestic animals were just beginning, designed to help determine tolerance levels of iodine exposure through food. At times, though rarely, knowledge gained through Hanford's radio-biology research program did directly result in changes in plant operation. As Parker's environmental monitoring team discovered that atmospheric iodine often settled on nearby agricultural land in much higher concentrations than predicted, and that iodine readily passed from plants, through the food chain, to humans, Parker insisted that cooling times for the irradiated fuel be lengthened. Cooling time for the irradiated 'slugs' started at 35 days but with Parker's advice the cooling period increased to more than 50 days (and often up to 125 days) in order for more of the short-lived radio-nuclides to decay. Over time Parker also successfully lobbied for improved filters that could more effectively remove radio-elements from the stacks.[54]

The more common situation at Hanford, especially during the early expansion of production, can be seen in the minimal effect Richard Foster's aquatic biology program had in modifying Hanford practices. Foster's ongoing research at Hanford, in general, supported the earlier research findings of the Donaldson's Applied Fisheries Laboratory: radiation levels produced by reactor operation were far too low to produce noticeable

[54] Gerber, *Home Front*, 83-90. C.C. Gamertsfelder, a physicist in the Health Instruments division, gives Parker the credit for increasing cooling time. See oral history interview at http://tis.eh.doe.gov/ohre/roadmap/histories/0467/0467toc.html viewed 1 August 2002.

health and developmental problems in river organisms. Chemicals added to the river water were probably more dangerous, but again posed little threat at the low concentrations released to the river. And even considering the remarkable ability of organisms to concentrate radio-isotopes, there appeared to be no danger either to the organisms or the humans who might consume them. Rather than modifying Hanford operations by limiting production in any way, the findings of the aquatic biologists opened the door to vastly increased use of the river as political decisions ramped up weapons production.[55]

Another example of the non-applicable nature of Hanford's biological research appears to suggest that scientists were ignored if they believed the AEC to be pursuing biologically unsound policies. At a National Academy of Sciences meeting Richard Foster was asked what the most favorable and unfavorable aquatic environment would be for the use of single pass reactors. He replied, "I would say that probably the most undesirable from a biological point of view is a river like the Columbia, which is highly productive. You have a good deal of important aquatic life, and an abundance of life form. Probably the most desirable in terms of avoiding biological accumulations is a river which is already highly polluted, to the extent that you don't have any aquatic items to worry about."[56] In short, the extent to which Hanford research was literally

---

[55] Explicit use of Hanford's aquatic biology research to justify increased plutonium production can be found in an internal AEC document: "Atomic Energy Commission, Columbia River contamination, Note by the sect., Nov. 22, 1955" (DBER, Box 40, Folder 12). Parker's summary of aquatic studies points out areas of concern, but concludes that future expansion of Hanford's reactors will only raise contamination to "20% of the effective permissible limit" (see p.5).

[56] See "Meeting Transcript, 3 March, 1956" p.81. Washington DC, NAS/NRC archive, Committees on the Biological Effects of Atomic Radiation, Oceanography & Fisheries (hereafter cited as NAS/BEAR), folder title: "ORG : NAS Coms on BEAR: Oceanography & Fisheries: Meetings Transcript, 3 Mar 1956".

applied to local operations is difficult to ascertain. When explicitly called upon to assess the impact of increased production, biological research at Hanford tended to support and justify political demands.

Clearly then, while biological and health research at Hanford made contributions to the ways in which the site operated over the long term, national and international events had greater immediate effect. The construction and operation of reactors and separation facilities waxed and waned according to the needs of political and military policy makers. As perceptions of a communist threat gripped America in the late 1940s and into the early 1950s, three additional reactors were being constructed at Hanford to increase the output of plutonium. By 1950 Hanford reactors produced a quarter ton of plutonium (this would increase to two tons annually, by 1956).[57] Maintaining nuclear superiority quickly became the established policy of U.S. administrations in an intensifying Cold War. Two months before the first Soviet atomic test - with the U.S. government still believing that the USSR would not be capable of nuclear weapons before 1951 - President Truman stated, "since we can't obtain international control we must be strongest in atomic weapons."[58]

This approach to national defense meshed perfectly with the politics of a young but prominent member of Washington State's congressional delegation, Henry "Scoop" Jackson.[59] Elected in 1941to the House of Representatives at the age of twenty-eight as

---

[57] David Albright, et al. *World Inventory of Plutonium and Highly Enriched Uranium, 1992* (Oxford University Press, 1993), 31-33.

[58] Robert G. Kaufman, *Henry M. Jackson: A Life in Politics* (University of Washington Press, 2000), 55.

[59] Kaufman, *Jackson*, 13. The nickname "Scoop" came from a popular comic strip character who had a knack for recruiting others to do his chores, a characteristic noticed by his older sister Gertrude when he was a young boy.

a Roosevelt Democrat, he gave consistent attention to local issues and enjoyed widespread support in his mostly rural district north of Seattle. For the next ten years Jackson easily won re-election to the House, gradually gaining a reputation for bringing home major military contracts to local business. Joining Warren Magnuson in the Senate in 1952, the two became known as the "gold dust twins" for their ability to sprinkle federal money on Boeing, the Bremerton Shipyard, and Hanford.[60] Yet, underpinning Jackson's politically expedient support of the regional economy lay a hawkish anticommunist philosophy justifying his support of the state's military interests within a larger political ideology. The threat of totalitarianism constituted a core element of Jackson's international policy which, he believed, must be countered with a strong military. He was not alone either among politicians or scientists. Jackson's appointment in 1949 to a subcommittee assessing the feasibility of building a hydrogen bomb helped forge an important relationship with a close ideological ally – Edward Teller. The bond between Jackson and Teller revolved around their shared beliefs that the only way to counter the expansion of Soviet communism lay in superior military capability.[61]

---

[60] Ibid, 112. Jackson would also become known as the "man from Boeing" for, in Kaufman's words, "the perception among those critical of Jackson's Cold War views that he was…in thrall to the so-called military-industrial complex." See page 145.

[61] Ibid, 54. There is consistent correspondence, both on policy and personal issues, between Jackson and Teller, going back to the early 1950s, in Jackson's papers. An August 8, 1960 letter from Teller to Jackson expresses their shared pessimistic views of the Cold War and Teller's approval of having a political ally as chairman of the Democratic National Committee: "I know that you are one of the very few people who see clearly the great and imminent dangers of the Cold War which we are losing so rapidly." HMJ papers, Box 6, Folder 2. Jackson exemplifies the kind of dominant Cold War liberalism Jessica Wang describes in *American Science in an Age of Anxiety: Scientists, Anticommunists, and the Cold War* (University of North Carolina Press, 1999). Wang posits that a strong anticommunist agenda infused liberal politics starting in the late 1940's transforming it from an idealistic, progressive activism in the early decades of the twentieth-century into a pragmatic politics lacking social vision during the Cold War. See, for example, p.289.

Jackson's appointment to the Joint Committee for Atomic Energy (JCAE) in 1949 enhanced his ability to represent Hanford as a central part of a vigorous AEC mission. But for Jackson, echoing Smyth's argument for including research in the AEC's mission, weapons production should not be the agency's sole objective. Rather, Jackson believed, the AEC must pursue a national defense strategy that could out-think what he perceived as the more powerful Soviet military. In one congressional speech Jackson argued that, "In raw quantitative power – power measured by the yardstick of foot soldiers and ordinary weapons – the Soviets have an actual and potential advantage. But in qualitative military power – in the power of laboratories, scientific skills, and specialized brains – the advantage is overwhelmingly on our side."[62] General acceptance of this argument by the Truman and Eisenhower administrations ensured that the United States would pour money into its nuclear weapons programs, including associated laboratories and scientists. Jackson asserted that within Congress the members of the JCAE were the primary advocates for bolstering the AEC's role in national defense. Rather than reigning in an overly zealous agency, Jackson described the JCAE as encouraging the Commission to take on larger and more ambitious programs. In 1952 he wrote, "the JCAE most often finds itself saying 'Do more; do it more boldly.' And for every time we ask, 'Isn't this program too ambitious?' we find ourselves asking a dozen times: 'Isn't this program too cautious?'"[63] In the context of an escalating Cold War, then, the fate of modest, limited research programs tied to national defense found ample justification to expand, if not for scientific reasons than on political grounds.

---

[62] Kaufman, *Jackson*, 61.

[63] Henry Jackson speech to University of Michigan Law School, 28 June, 1952 (HMJ papers, Box 67, Folder 1).

Herbert Parker had successfully pulled together the nucleus of a small research program at Hanford by 1950. Parker, Foster, and Kornberg – the senior scientists in charge of the Health Instruments, Aquatic Biology, and Biochemistry units respectively, had been joined by two others. Roy Thompson, a young biochemist recently hired from the University of Texas (and who had done post-doctoral research at Berkeley's Radiation Laboratory), was just beginning studies on the effects of internally deposited plutonium. Completing the small group of lead scientists was a veterinarian from Washington State College, Leo Bustad, directing the iodine ingestion study at the experimental large animal farm. A staff of more than sixty junior scientists and technicians assisted these senior scientists. Most of the growth and important organizational changes would take place in the coming decade.

Herbert Parker successfully established a small research program by not substantially hindering plutonium production at Hanford and tapping into a national desire for the expansion of science. He took advantage of concerns for regional health and safety and thereby constructed a small research program on the margins of traditional disciplinary, institutional, and geographic boundaries of science. How and to what extent Parker's research program would endure was yet to be determined as the contexts of science and politics changed through the 1950s. The emergence of "an ideology of anticommunism" that would "dominate American politics" for the next several decades served not only to restrict political sentiment and scientific debate, as Jessica Wang has emphasized in her study of Cold War American science, but it also provided new research opportunities and a conducive environment for pursuing practical research in support of national defense. Some AEC laboratories thrived immediately under these demands while others, such as Hanford's, were slower to capitalize on new opportunities. As scientists in Hanford's radio-biology program entered the 1950s, they were just learning how to construct research programs consonant with national political sentiment and Commission interests.[64]

[64] Jessica Wang, *American Science*, 2; see also pages 253, 254 where Wang summarizes her argument that the early Cold War was particularly harsh for scientific and intellectual freedoms. Wang's study highlights an antagonistic relationship between politics and science; the history of Hanford's radio-biology research program emphasizes the ways in which scientists worked within, and profited from, the dominant political culture. Hugh Gusterson, a cultural anthropologist, has suggested how scientists reconciled potential conflicts between science and politics. Gusterson studied the Lawrence Livermore National Laboratory in the late 1980's and described what he called the "central axiom" subscribed to by most workers at that weapons laboratory: workers supported the purpose of the lab because they felt nuclear weapons were a great deterrent to war. If one country were to gain a superiority, it must be the United States. These sentiments spanned the political spectrum, from conservative to liberal. Finally, beyond ideological commitments encouraging scientists to work at a weapon's laboratory, the opportunity to pursue "cutting edge" science was cited as a major factor. No similar cultural study of an early AEC laboratory exists, but Gusterson's research suggests a number of interesting conclusions: for scientists who took jobs at weapons laboratories, moral and ethical obstacles were few and far between; and not surprisingly, a well-funded laboratory, working on novel problems, was attractive to a large percentage of scientists, cutting across political orientation. See Hugh Gusterson, *Nuclear Rites: A Weapons Laboratory at the End of the Cold War* (University of California Press, 1996), 38-59.

Chapter 3

Expanding Research at Hanford: From Obscurity to Controversy, 1951 to 1958

Herbert Parker's accomplishments in establishing radio-biological research at Hanford through 1950 were but a start. Building up a vigorous research program that would attract new scientists, gain recognition from colleagues, and carve out a stable institutional relationship required substantial changes in the way Hanford scientists interacted with colleagues outside of their laboratory. First and foremost, scientists and administrators had to reconstruct and rearticulate the terms of secrecy within which national defense research took place. Issues of free and open exchange, perceived as fundamental characteristics of healthy scientific practice, were severely restricted during World War II as scientists placed national security above professional traditions of open communication. Individuals such as Vannevar Bush believed that scientists could accommodate the unique demands of war-time secrecy, but following the war science should return to principles of openness and political disinterest.[1] Growing ideological tensions with the Soviet Union ensured, however, that considerable parts of the war-time economy would persist as the United States committed itself to retaining nuclear superiority. Following Japan's surrender, a complete dismantling of the war-time research structure, including the secret and compartmentalized environment in which

---

[1] Literature on the origins of the NSF provides insight into the goals of scientists and politicians for the post-war conduct and funding of science. See, for example, Robert Franklin Maddox, "The Politics of World War II Science: Senator Harley M. Kilgore and the Legislative Origins of the National Science Foundation," *West Virginia History*, 1979, 41:20-39; Nathan Reingold, "Choosing the Future: The U. S. Research Community, 1944-1946," *HSPS*, 1995, 25(2):301-328; and Jessica Wang, "Liberals, the Progressive Left, and the Political Economy of Postwar American Science: The National Science Foundation Debate Revisited," *HSPS*, 1995, 26(1):139-166. This and other scholarship points out that the debates did not seriously consider returning to pre-war levels of government funding of research. Rather, debates centered on the extent to which scientists would be allowed to self-govern versus the level of politician's oversight into policies and funding patterns.

scientists operated, did not take place. Many laboratories continued to function after the war, emerging by the early 1950s as new and permanent entities on the landscape of American research. Michael Dennis, who has studied the institutional basis of Cold War science, describes national defense oriented laboratories as "neither purely military nor purely civilian institutions, but fascinating hybrids blending the two spheres together."[2] Atomic Energy Commission (AEC) laboratories, and particularly Hanford, struggled to find their place in this new and ambiguous region of science. The central problem in this chapter is to understand how Hanford biologists made the transition from the highly classified and shrouded research environment of the early 1950s to a more traditional scientific culture that emphasized open dialogue. Through these changes the primary issues of 'national defense' ultimately shaped the way scientists pursued research at weapons-related facilities and how science was presented to the public. Practical considerations of national defense ensured that these new laboratories would help to define new interests and practices in science, neither wholly academic nor industrial.[3]

Although it was a civilian agency, the AEC worked in lock-step with various branches of the military in pursuit of national defense. As a consequence, issues of security received primary attention. Beyond the obvious precautions having to do with handling of nuclear materials, security within the AEC involved restrictions on the transmission of information. Security clearances for AEC personnel limited access to sensitive information and provided mechanisms for screening individuals with

---

[2] Michael A. Dennis, "'Our First Line of Defense:' Two University Laboratories in the Postwar American State," *Isis*, 1994, 85:427-455. Quote found on page 430.

[3] Paul Forman "Behind Quantum Electrodynamics: National Security as Basis for Physical Research in the United States, 1940-1960," *HSPS*, 1987, 18:149-229; Stuart W. Leslie, *The Cold War and American Science: The Military-Industrial-Academic Complex at MIT and Stanford* (Columbia University Press, 1993).

questionable affiliations. Documents created within this system received classifications such as "restricted," "secret," or "confidential" if the information was deemed to be of importance to the national security. In practice the system classified the vast majority of documents, thereby placing them off-limits to a large percentage of the scientific community. For example, this practice restricted 81 percent of AEC laboratory reports from public distribution in 1948.[4] As a historian of the AEC has documented, a relaxation of classification policies in 1948 declassified large areas of research that did not investigate the fission process, weapons, or the characteristics of elements above thorium, including uranium and plutonium. However, since Hanford research centered on the biological effects of fission products, and since the facility was an integral part of the weapons program, all aspects of Parker's research program remained classified.[5] At many of the AEC laboratories the practice of classification came under intense criticism because it contradicted a commonly held ideal of scientific practice – open, communal sharing of knowledge. Yet scores of scientists, such as those at Hanford, quickly learned to live with classification as a condition of working in a well-funded AEC laboratory. Although unconstrained communication remained a fundamental ideal within the scientific community in the early post-war years, Cold War

[4] Robert W. Seidel, "The National Laboratories of the Atomic Energy Commission in the Early Cold War," *HSPS*, 2002, 32(1):145-162. Classification figure found on page 153.

[5] Richard Hewlett, "'Born Classified' in the AEC: A Historian's View," *Bulletin of the Atomic Scientists*, 1981, 37(10):20-27. Michelle Gerber describes the highly secretive culture at Hanford, and argues that this played an important role in limiting the effectiveness of Parker's research program. See *On the Home Front*, 45-48 & 215-217.

realities ensured that many scientists worked with a set of scientific colleagues determined by political and governmental policies.[6]

Biological research at Hanford prior to 1951 remained relatively obscure, shut off from much of the scientific community as a result of the classified nature of its operations. AEC scientists - managers of the Division of Biology and Medicine and biologists at laboratories such as Oak Ridge, Brookhaven, or Argonne - certainly knew about their colleagues at Hanford, but wider recognition among American biologists was lacking due in part to classification restrictions. Beyond security issues, Hanford scientists remained relatively unknown because of the highly programmatic nature of their research programs. Research applied to the local conditions of plutonium production did not demand widespread consultation with outside scientists. Yet Herbert Parker's ability to establish a more active and respected research program in the 1950s developed hand-in-hand with Hanford biologists' increasing participation in traditional disciplinary activities. The major transformation of Hanford's biological research program in this decade involved its greater interest in communicating with a wide array of colleagues, establishing ties with main-stream academic researchers, and thereby gaining a reputation as a capable and vigorous biological research laboratory. These transformations were made possible by gradual loosening of restrictions on classified

[6] Whereas secrecy and science are often represented as contradictory, Michael Dennis has argued that in practice scientists profited under the terms of 'classified' national defense research. In fact, not only did scientists benefit in this environment but science often thrives on the social power of restricted knowledge. Michael Dennis "Secrecy and Science Revisited: From Politics to Historical Practice and Back," *Secrecy and Knowledge Production*, edited by Judith Reppy, 1-17. For the classic exposition of the sociopolitical terms in which science excels, see Robert K. Merton, "The Normative Structure of Science," reprinted in *The Sociology of Science: theoretical and empirical investigations*, edited by Norman W. Storer (University of Chicago Press, 1973), 267-278. A 'Mertonian' ideal of communal knowledge can be seen to under gird Robert Crease's interpretation of Brookhaven's early struggles with the AEC's restrictive classification requirements. See Crease, *Making Physics*, 54-55, where he states that many of the AEC labs "were severely hampered" by classification.

research accompanied by a growing community of scientists engaged in classified research, changes in the management and autonomy of Hanford's research program, and an increasing interest in biological and ecological research on the part of the AEC.

Extending Radiation Studies in the Columbia Basin

Just as Richard Foster's aquatic biology studies introduced biological research at Hanford during the war, his program was the first to gain significant attention from outside observers. Plutonium production introduced a wide array of radio-isotopes into the environment, which in turn presented scientists with a new set of practical problems and research tools to study biological and ecological effects of radiation. Earlier investigations into the biological effects of radiation had flourished prior to World War II, most notably in the independent work of Herman Muller and Nikolai Timofeeff-Ressovsky using radiation to induce mutation, research which produced both theoretical and practical results for genetics, evolutionary theory, and radiation safety. As a form of industrial contamination, radium began to be studied intensively throughout the 1920s and 1930s when commercial and scientific use of the element proliferated. The high mortality and gruesome illnesses suffered by watch-dial painters provided graphic illustration of the health dangers associated with chronic radiation exposure. Yet prior to the discovery of fission and its implementation in the production of weapons, radiation was studied mainly from naturally occurring elements in laboratory settings. Scientists at Hanford, and slightly later at Oak Ridge, were the first to study human produced radio-elements out of the laboratory and through various biological pathways.[7]

[7] For the scientific interest in radiation, see Lawrence Badash, "Radium, Radioactivity, and the Popularity of Scientific Discovery," *Proceedings of the American Philosophical Society*, 1978, 122(3) 145-154. For the growth in awareness of radioactivity's long-term health effects, see Claudia Clark, *Radium Girls: women and industrial health reform, 1910-1935* (University of North Carolina Press,

Hanford's biological research was, naturally, of interest to the AEC. Richard Foster was able to describe Hanford's aquatic biology program at a meeting of AEC scientists at Oak Ridge National Laboratory in 1949. Those dealing with radiation from nuclear reactors, Foster argued, would face a completely new set of health problems as well as new opportunities for research. Foster's presentation focused on the fact that neutron activation produced a wide array of radioactive elements from the naturally existing components of river water. Long-lived radio-elements metabolized by organisms living in the river posed the major threat. These elements included iron, sulphur, calcium, and phosphorus, with isotope half-lives ranging from 14 to 180 days. In response, Foster's aquatic biology program had been organized to answer two fundamental questions: what was the effect of biologically active radiation on organisms, and how did these particular radio-elements move through the food chain? Foster's most significant finding showed that plants and animals concentrated biologically important radio-isotopes at levels significantly higher than in the river water. The activity found in plankton near the reactors was some 4000 times that of the water; algae about 1000 times, and fish accumulated radioactivity about 200 times greater than radiation levels in the water. Of all the long-lived radio-isotopes Foster calculated that phosphorus would contribute about 90 percent of an organism's total dose. As for the effects of radio-isotope concentration on the survival of these organisms, Foster pointed out that they were within acceptable radiation exposure limits. However, since these findings were unexpected, Foster explained that Hanford biologists were initiating wider ecological studies of the aquatic environment coupled with more

---

1997). For a sense of the ways in which radiation was both studied and used in the biological sciences prior to World War II, see B. M. Duggar, "Preface," *Biological Effects of Radiation,* v-vi.

extensive laboratory research to determine the limits of radio-isotope concentration under controlled conditions.[8]

The "radiobiological-ecological"[9] survey being proposed by Foster not only seemed to be the most appropriate expansion of his research program, but studies into "ecosystem" dynamics were becoming popular among ecologically oriented biologists. The developing concept of ecosystems, which viewed communities of organisms and their nonliving environment as one system through which elements and nutrients continuously cycled, was establishing what seemed to be a more rigorously scientific basis for ecology. Raymond Lindeman's highly mathematical 1942 paper "The Trophic-Dynamic Aspect of Ecology," became a model upon which later biologists explored the ecosystem model for ecological investigations. Ecosystem ecology, with its focus on quantifiable concepts of energy transfer and cybernetic stability, "caught the imagination of a generation of post-World War II ecologists," as the discipline repositioned itself as more than simply a descriptive field science.[10] Foster's research program began to

---

[8] For Richard Foster's presentation of Hanford's aquatic biology research see, "Radiobiological Problems in the Columbia River," April 1949 (R-DOE, HW-23793).

[9] Richard Foster, "Proposed Radiobiological-Ecological Survey of the Columbia River" February 9, 1949 (R-DOE, HW-12574).

[10] For early history of ecosystem concept, see Joel Hagan, *An Entangled Bank: The Origins of Ecosystem Ecology* (Rutgers University Press, 1992), 78-79. Pages 108-120 sketch out some of the institutional connections between ecosystem research and AEC programs, especially as the use of radio-tracers became a common tool for ecological research following the war. Keith Benson has described how ecology distanced itself from 'natural history' in "The Emergence of Ecology…," 59, 60. Robert Kohler argues that much of the success of ecology in the latter part of the 20th century can be ascribed to its adoption of universalized, laboratory-influenced methods which allowed ecology to be perceived as more than a "field" science. Robert Kohler, "Place and Practice in Field Biology," *History of Science*, 2002, 40(2):189-210. Sharon Kingsland has traced a strong mathematical basis in ecology back to the 1920s, in the work of population ecologists. The effort to create mathematical models, which Kingsland describes as an attempt to make ecology "ahistorical," was the central tool in modernizing and molding "ecology in the image of the physical sciences." *Modeling Nature: Episodes in the History of Population Ecology* (The University of Chicago Press, 2nd Edition, 1995), 217-218.

reflect the influence of ecosystem rhetoric by 1949.[11] Whereas earlier research focused first and foremost on salmon and trout in the immediate vicinity of Hanford reactors, then expanded to include "bottom-living organisms" in 1947, Foster now argued for a much more comprehensive study based on the notion that the Columbia River was a complex energy transfer system. The fate of radioactive elements would be traced through interrelated organisms, from algae and associated microorganisms, aquatic insects and invertebrates, plankton, finally to fish and vascular aquatic plants. Once again the primary justification for this proposal was its direct applicability "for long range hazard control." Yet the language of "ecological balance" and the argument that there existed a delicate interdependence of all forms of life established this program squarely in terms familiar to a growing and influential group of ecologists.[12] Foster's expansion of research on the Columbia coincided with the creation of a regional advisory group composed of Oregon and Washington health officials, United States

[11] In fact, the appearance of an 'ecological' perspective in Hanford reports came primarily from J. J. Davis, a young assistant of Foster's. Davis received his MS in entomology from Washington State College in 1948, the year he arrived at Hanford. By the mid 1950s he became the manager of Hanford's radio-ecology unit. For early examples of ecologically oriented research see R. W. Coopey, J. J. Davis, R. F. Foster, "Cursory Survey of the Radioactivity in Biological Materials of the Lower Columbia River," February 23, 1949 (R-DOE, HW-12573); C. L. Cooper and J. J. Davis, "Effect of Hanford Pile Effluent upon Aquatic Invertebrates in the Columbia River," January 19, 1951 (R-DOE, HW-20055). Eugene Odum, an ardent proponent of the ecosystem foundation for ecology, consulted with Hanford biologists in 1954 as he prepared a chapter on radiation ecology for the second edition of his ecology textbook. Odum's time spent at Hanford encouraged the establishment of Davis's radio-ecology unit and encouraged the overall program to present their ecological research using ecosystem rhetoric. This became evident at the 1956 Atoms for Peace conference when Foster and Davis suggested that reactor-site ecological surveys could do more than simply survey and monitor radio-isotopes. Biologists at places such as Hanford could, they argued, collect valuable information on "nutrient cycles, metabolic rates, and ecological relationships" by using the radio-isotopes as tracers to illuminate the structure of the local ecosystem. R. F. Foster and J. J. Davis, "The Accumulation of Radioactive Substances in Aquatic forms," *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* (United Nations, 1956), 364. For Odum's interpretation of Hanford research in terms of the ecosystem concept, see *Fundamentals of Ecology* (W. B. Saunders Company, 2nd edition, 1959), 452 & 467-470.

[12] Richard Foster, "Proposed Radiobiological-Ecological Survey …."

Public Health personnel, AEC representatives, and General Electric (GE) managers. The Columbia River Advisory Group focused on pollution issues related to Hanford as well as other industrial and agricultural users of the river. The advisory group established an over-lapping study of Columbia River biology by 1951 that worked in cooperation with Foster's aquatic biology group.[13]

Promoting Research – The Radiological Sciences Department

Foster's aquatic biology field studies and the complementary laboratory radio-biological research represented one half of a larger research effort. Herbert Parker recognized the advantages of bringing together all Hanford research into one unit. A united research division would present a more coherent, effective, and impressive program to GE, the AEC, and to colleagues in academia. Following agency approval at a 1951 meeting of the advisory committee for Biology and Medicine, Parker created the Radiological Sciences Department with two research sections: "Biology" and "Biophysics." The Biology section included six research units with a staff of seventy-seven, while Biophysics was divided into eight units and employed forty-eight. Although this reorganization created little change in the research being pursued, it allowed Parker to convincingly present his program to managers and colleagues as a

[13] A series of Hanford reports document the establishment of the Columbia River Advisory Group and Public Health Service's interest in starting their own river studies program. See meeting transcripts from November 1949 to October 1951 (R-DOE: HW-15861, HW-17595, HW-17732, HO-1, HW-22181, GEH-19226). A summary of the ecological survey through 1951 (and noting collaboration with the Public Health Service study) can be found in J.J. Davis, et al "The Radioactivity and Ecology of Aquatic Organisms of the Columbia River," *Hanford Biology Annual Report - 1951* (R-DOE, Hanford HW-25021) 19-29.

unified and efficient attack on Hanford's diverse biological problems resulting from plutonium production.[14]

Creation of the Radiological Sciences Department was not only beneficial for Parker's prestige as laboratory director; but also provided an opportunity to publish the Department's research accomplishments in reports distributed throughout AEC affiliated laboratories. Beginning in 1951 Harry Kornberg began publishing an annual review of research, distributing it to a small but growing community of colleagues with interests (and security clearances) in AEC sponsored radio-biology. Initial distribution of this review went to only thirty-four different institutions including the AEC national labs, select army and navy laboratories, industrial laboratories operating nuclear facilities, and a handful of academic researchers at such places as Columbia, the University of Washington, UCLA, and the Western Reserve University. Two years later the list of recipients nearly doubled to sixty different institutions, including six international recipients. By the mid 1950s more than 300 copies of Hanford's biology review were being distributed to over 100 institutions. The rapidly expanding circle of recipients reflected not only the growing importance of the AEC in American science, but more importantly the annual review provided an outlet for Hanford scientists to communicate their research at a time when they published little in the open literature.[15]

---

[14] For staff numbers see "Radiological Sciences Quarterly Progress Report for Research and Development, July 1951" (R-DOE, HW-22576). For the organizational structure and presentation of laboratory as a coherent program, see "Summary of Research and Development Functions and Activities, 1951-1954," Prepared for meeting of AEC Advisory Committee on Biology and Medicine January 14 and 15, 1955 (DBER, Box 104, Folder 9).

[15] The *Hanford Biology Annual Report* began in 1951 and ran through 14 editions until the laboratory was taken over by Battelle in 1964. For information on early distribution see annual reports for 1951, 1952, and 1953 (R-DOE, HW-25021, HW-28636, and HW-30437)

Beyond allowing Hanford's biologists a convenient means of communicating their research to a wider audience, the annual reviews provided information on the extent of Hanford scientists' participation in traditional scientific activities: publication of journal articles and attendance at scientific meetings. Publications from the six research units amounted to only eleven articles in 1951 - ten published internally and available through AEC information bureaus, and only one article in a refereed journal. The following year Hanford biologists published only six articles, but three of these were in traditional scientific journals. Although journal publications by Hanford biologists were exceedingly scarce in the early 1950s, increasing participation in scientific conferences also helped to establish a reputation among colleagues. During the year in which they published six articles, Hanford biologists presented papers at twelve meetings such as the American Society of Limnology and Oceanography, the American Chemical Society, the AAAS, and a veterinarian's conference. Hanford's origins as an industrial production plant working under intense security measures during the war left a lingering culture which discouraged open communication. Other AEC laboratories, such as Oak Ridge, Argonne, and even Los Alamos, more aggressively, quickly, and successfully learned to operate within the restrictive environment of the post-war nuclear sciences. By the mid 1950s Hanford's biologists also began to understand the necessity of participation in traditional scientific activities for the success of their research program.[16]

A study of the biologist's Annual Report clearly demonstrates that the early efforts to publish and participate in the wider scientific community continued and accelerated. Biologists published eighteen journal articles and made sixteen

[16] *Hanford Biology Annual Report* – 1951, 211; *Hanford Biology Annual Report* - 1952, 187-189; *Hanford Biology Annual Report* - 1953, 160-163.

presentations at scientific meetings in 1957. Six years later these figures rose to 58 and 38 respectively.[17] Creation of the Radiological Sciences Department, a semi-autonomous research unit within GE's Hanford organization, demanded a higher level of professional, scientific conduct. Increasing participation in traditional professional activities illustrates the internal desire on the part of Hanford biologists to build a respected scientific reputation. On the other hand, the growing level of disciplinary participation among Hanford biologists also demonstrates the proliferating opportunities available to scientists. As Hanford biologists became more involved in the larger community of scientists, they were encouraged to broaden their research horizons as increasing support for biology spread through numerous federal agencies. The creation of the National Science Foundation in 1950 to support basic science did not result in the AEC abandoning any of its research programs. Rather, AEC program managers and scientists found ample justification for preserving and expanding applied, programmatic research of the kind pursued at Hanford. [18]

In what Toby Appel has described as the "abundant funding system of the 1950s," the AEC budgeted well over $170 million for radio-biology and genetics research through 1955, primarily at its own laboratories. The annual budget for the Division of Biology and Medicine rose from $25 million in 1952 to more than $36 million by the close of the decade. Although small in comparison to the AEC's funding of physics, during the 1950s the AEC was second only to the National Institutes of Health in its support for biology. Not surprisingly, Hanford scientists eagerly broadened

---

[17] *Hanford Biology Annual Report* – 1957, 158-160. *Hanford Biology Annual Report* - 1963, 230-241.

[18] Appel, *Shaping Biology*, 101. For the AEC's response to the creation of the NSF see Westwick, *National Laboratory System*, 74-79.

their research agenda to tap into this ready source of funding. The radio-elements of interest - iodine, plutonium, tritium, strontium, cesium, and yttrium – all produced in relative abundance by Hanford's reactors, were targets of a small number of research programs around the country. Hanford biologists believed expansion of their environmental and health oriented program could profitably contribute to these important areas of research. Whereas Harry Kornberg stated in 1951 that his program's goal was "to perform such research in biology that the practice of radiation hazard control may be constantly improved," by 1955 he had widened its horizons to include "research activities which may be extended to national problems or to problems of fundamental biological importance" as a "small" part of the overall program.[19]

Atoms for Peace

The expansion of biological research in the 1950s had many causes, and within the AEC the importance of presidential policy decisions cannot be overlooked. The election of Dwight D. Eisenhower in 1952 brought important changes for the portrayal, use, and support of atomic energy. Growing alarm over the Cold War build-up of nuclear weapons convinced Eisenhower that public attempts must be made to change the perception of atomic energy. The president initially supported *Project Candor*, a disarmament initiative drawn up under the leadership of Robert Oppenheimer and Vannevar Bush to broaden public knowledge about nuclear weapons. Much of the program centered on making people aware of the dangers of human annihilation posed by the growing stockpiles of nuclear weapons. With a July 1953 truce in the Korean

[19] Appel, *Shaping Biology*, 130-131 & 135-138 (quote page 131). For AEC budget, see Hewlett and Duncan, *Atomic Shield*, appendix 7; and Richard G. Hewlett and Jack M. Holl, *Atoms for Peace and War, 1953-1961: Eisenhower and the Atomic Energy Commission* (University of California Press, 1989), appendix 2. For Kornberg quotes see *Hanford Biology Annual Report* - 1951, 9; and *Hanford Biology Annual Report* - 1955, 5.

conflict seeming to lessen international tensions, *Candor* appeared a viable option for pursuing Eisenhower's peace initiatives. By late September however, evidence that the Soviet Union possessed a hydrogen bomb caused the administration to reconsider the *Candor* program in light of the intensifying international nuclear threat.[20]

Atoms for Peace, Eisenhower's proposed alternative to *Candor*, differed in its emphasis on the positive aspects of atomic energy, requiring the United States and the Soviet Union to provide a substantial amount of their fissionable material to the United Nations for "peaceful uses." Eisenhower laid out his *Atoms for Peace* initiative at a speech to the United Nations in December, 1953. Drawing attention to the future benefits of atomic energy, Eisenhower predicted that "if the entire body of the world's scientists and engineers had adequate amounts of fissionable material … this capacity would rapidly be transformed into universal, efficient, and economic advantage."[21] While the primary thrust of the plan aimed to harness atomic energy for civilian power, the symbolic redirection of research from military to civilian purposes held immense benefit for the life sciences. As one AEC historian stated, "no Commission activity held greater promise for the peaceful uses of nuclear energy than did research in biology and medicine."[22]

---

[20] Robert Divine, *Blowing on the Wind: The Nuclear Test Ban Debate, 1954-1960* (Oxford University Press, 1978); Robert Divine, *Eisenhower and the Cold War* (Oxford University Press, 1981), 110-123. Hewlett & Holl, *Atoms for Peace*, 55-62.

[21] Hewlett & Holl, *Atoms for Peace*, 72.

[22] Ibid, 262. Disagreements among historians over how to interpret the intentions of the Eisenhower administration are well illustrated in the apparent incongruity between the 'Atoms for Peace' arms control initiative and the administration's 'massive retaliation' rhetoric in foreign policy. Contradictions in Eisenhower's nuclear weapons policies are explored in Robert A. Strong, "Eisenhower and Arms Control," *Reevaluating Eisenhower: American Foreign Policy in the 1950s*, edited by Richard A. Melanson and David Mayers (University of Illinois Press, 1987), 241-266.

One immediate result of Eisenhower's Atoms for Peace initiative was an international conference for the exchange of scientific findings and developments in atomic energy. The first International Conference on the Peaceful Uses of Atomic Energy, held in Geneva, Switzerland during the summer of 1955 highlighted national research programs in civilian reactor technology. For American biologists, recent declassification of most AEC biological research provided an important opportunity to communicate with national and international colleagues. With this partial declassification, the Geneva conference became especially significant for Hanford biologists since it offered the first occasion to present their research before an audience of non-AEC colleagues. Jack Healy, head of the environmental radiation monitoring program, joined Herbert Parker in delivering a paper that attempted to estimate the environmental consequences of a major power reactor accident. Drawing on what they felt was Hanford's impressive record of environmental management, Parker and Healy claimed that environmental damage could be limited to acceptable levels with implementation of "high standards of engineering and operation." In another paper, Parker summarized the environmental hazards associated with Hanford's water-cooled reactors. Suggesting how scientists could contribute to this growing industry, Parker noted that an "extensive radio-ecological program is necessary to validate effluent release practices."[23]

Three other presentations at the Geneva gathering described the surprising radio-element accumulation findings of Hanford biologists. Both aquatic and terrestrial

[23] H. M. Parker & J. W. Healy, "Environmental Effects of a Major Reactor Disaster" 106-109; and H. M. Parker, "Radiation Exposure from Environmental Hazards," *Proceedings of the International Conference on the Peaceful Uses of Atomic Energy, volume 13*, 305-310.

studies had shown that plants constituted the primary gateway for fission products entering the food chain. Fred Hungate, head of Hanford's plant nutrition and microbiology unit, presented evidence that iodine-131 and strontium-90 were the most important radio-isotopes concentrated in common agricultural crops because of their affinity for particular organs or tissues and their persistence in the environment. Richard Foster, Hanford's senior aquatic biologist, described research on the accumulation of radio-isotopes in aquatic organisms, but noted that this particular contamination could also be used to enhance fundamental ecological knowledge. Radioactivity in target organisms could "serve as tracers," he argued, providing "information on nutrient cycles, metabolic rates, and ecological relationships." Rounding out the presentation of Hanford's research, Harry Kornberg summarized the findings of radioactivity in terrestrial animals, particularly radio-iodine accumulation in the thyroid.[24] Evidence of the accumulation of certain radio-isotopes in plants and organisms could present a health danger, the Hanford biologists argued, but it also offered continuing opportunities for biological research as nations aggressively pursued development of civilian and military atomic energy.

Ecology and the Origins of Environmental Science in the AEC

The year 1955 was a crucial turning point in Hanford's development of an explicitly "ecological" framework for its research. At Oak Ridge, Savannah River, the Eniwetok testing site, and at a handful of universities, the AEC had begun supporting ecological studies similar to those pursued at Hanford. Most notably Eugene Odum, a

[24] J. H. Rediske & F. P. Hungate, "The Absorption of Fission Products by Plants" 354-356; R. F. Foster & J. J. Davis, "The Accumulation of Radioactive Substances in Aquatic Forms" 364-367; W. C. Hanson & H. A. Kornberg, "Radioactivity in Terrestrial Animals Near an Atomic Energy Site" 385-388. All found in, *Proceedings of the International Conference on the Peaceful Uses of Atomic Energy, volume 13.*

young and enthusiastic ecologist at the University of Georgia, successfully solicited grants from the AEC beginning in 1951 for support of an ambitious ecology program at Savannah River. Although AEC support for Odum's research started off at a modest $10,000 per year, he became a successful and tireless advocate for increasing ecological research as an essential component of the "nuclear age."[25]

Largely due to Odum's continual insistence that the AEC support explicitly ecological research, the Division of Biology and Medicine in 1955 took a formal survey of its ongoing support in these fields with the aim of developing formal funding mechanisms for the ecological sciences. Increasing atmospheric testing of nuclear weapons and the stepped-up efforts to develop commercial power reactors provided the AEC with ample reason to begin more explicit support of ecology.[26] In order to accomplish this goal, the AEC began to develop an ecologically oriented program within the Division of Biology and Medicine. Managers from the AEC's Division of Biology and Medicine joined ten scientists from the Ecological Society of America to evaluate

---

[25] Hagan, *Entangled Bank*, 107-110. Also see Eugene Odum, "Ecological Aspects of Waste Disposal," Atomic Energy Commission Report, Technical Information Division, Report #7512, 95-103; and "Ecology and the Atomic Age," *Associated Southeastern Biologists Bulletin*, 1957, 4:27-29.

[26] Although Hanford's ecological research did not draw direct motivation from nuclear testing, the AEC's interest in ecology must be understood in light of the nation's rapidly expanding testing program. The first hydrogen bomb, detonated in late 1952, placed radioactive material into the stratosphere for the first time. This drastically altered and expanded fallout patterns. In 1953 a secret AEC program – Project Sunshine – began intensively studying the environmental behavior of strontium-90, the fallout element of most concern due to its long half-life and similarity to calcium. With the *Castle* series of tests in late spring of 1954, high levels of radiation endangered the lives of hundreds of people when fallout from the Bravo shot spread much further than estimates predicted, as well as in unexpected directions. The most severely harmed were a group of 23 Japanese fishermen, trolling off the island of Rongelap, over 100 miles from the Bikini island test site. One of the fishermen died six months later. For the AEC, these kinds of events provided motivation for greater ecological knowledge, if only to add scientific justification for their weapons production and testing programs. Nuclear weapons testing by the AEC was directly responsible for the radio-biological/radio-ecological research program at the University of Washington's Applied Fisheries Laboratory: see Hines, *Proving Ground.* Pages 150 to 195 document the labs involvement in the *Castle* series of nuclear tests.

presentations from ten scientists engaged in radiation related ecology. The presenters included Hanford scientists Richard Foster, Fred Hungate, and D.W. Pearce, manager of the Biophysics Section. Other scientists, including Odum, presented research underway at the Pacific Proving Ground, Oak Ridge, and Savannah River. Similar methods, goals, and results in this AEC sponsored research could give the impression that much unnecessary duplication existed. All the research attempted to trace radiation through ecosystems and describe its effects on populations of organisms. Recognizing that administration budget analysts would seize on any duplication of research as wasteful, conference participants carefully stressed the necessity of similar research at multiple sites by drawing attention to the incommensurable qualities of "different ecological situations."[27]

Based on the practical ecological (including geophysical and oceanographic) research already established at a number of AEC sites, the Division of Biology and Medicine created an "Environmental Science program" to encourage the kind of broad, applied, inter-disciplinary research pioneered by Hanford scientists.[28] AEC managers

---

[27] United States Atomic Energy Commission, "Transcript of Proceedings: Ecology Conference" May 20, 21, 1955. (CIC, Accession #NV0040441), 234. The Environmental Science program gained permanence and stature within the AEC administrative structure in 1958 when it was given the new designation as the Environmental Science Branch within the Division of Biology and Medicine. From 1956 to 1958 John Wolfe employed Allyn Seymour, a young fisheries biologist from the Applied Fisheries Laboratory, as one of two program analysts. Seymour was, of course, a student of Lauren Donaldson, closely following Richard Foster through Donaldson's program. Program analysts wielded great power in approving or denying proposals, as Toby Appel has shown in her study of the NSF. Having Seymour in this key position was important for the success of both the Applied Fisheries Laboratory (which became the Laboratory of Radiation Biology in 1958) and Hanford's aquatic biology and radio-ecology programs. For role of science program analysts in post war federal agencies see Appel, *Shaping Biology*, 30.

[28] The origins of the term "environmental science" are unclear. Presumably AEC managers selected a descriptive, rather than disciplinary, term for the program because it was intended to foster multi-disciplinary research efforts on broadly defined problems of radiation in the environment. The AEC's close relationship with branches of the military suggests that the term "environmental science" was borrowed from the military institutional context in which it referred to multidisciplinary research

announced at the ecologists' conference that Dr. John Wolfe, a respected biologist from the University of Ohio, would be joining the staff with orders to organize and promote the ecological research programs of the AEC. Members of the Ecological Society concluded the conference by laying out general guidelines around which the AEC could structure its program. They called for "much more intensive training of radiation ecologists" and echoed earlier calls for diverse locations of research that could not only support the nation's growing weapon's production program but also make comparative studies of different ecosystems. Thus by the end of 1955 Hanford biologists were calling considerable attention to their research program and began to develop, along with a handful of colleagues, a new and receptive institutional patron within the AEC that would promote Hanford's novel form of environmentally oriented science.[29]

These two major events of 1955 - the Atoms for Peace conference and the AEC's formal initiation of an environmental science program – highlight two very different ways that Hanford research began to attain greater visibility and acceptance. Hugh Gusterson, an anthropologist who has studied the Lawrence Livermore weapons' laboratory, argues that "the workings of military secrecy and 'big science'" have collaborated to radically de-emphasize the impact of individual or small groups of scientists. Individual anonymity, it seems, did become an increasingly common attribute of many Cold War national defense laboratories. Yet the history of Hanford's biological

---

including the earth, biological, and physical sciences. Ronald Doel has explored the origins of the term "environmental science," as well as the disciplinary make-up of the field, "Constituting the Postwar Earth Sciences: The Military's Influence on the Environmental Sciences in America after 1945." Typescript.

[29] United States Atomic Energy Commission, "Transcript of Proceedings: Ecology Conference" May 20 & 21, 1955 (CIC, Accession #NV0040441), 245.

research program illustrates the vital importance of peer recognition in establishing and expanding a laboratory. Just as Edward Teller and Ernest Lawrence, two well-connected and respected scientists, were critical to establishing the Livermore laboratory, so too the growing reputations of Herbert Parker, Harry Kornberg, Richard Foster, and others convinced colleagues, GE officials, and AEC managers that important research was taking place at Hanford.[30]

Another measure of Hanford's growing research expertise is Richard Foster's and Harry Kornberg's selection in 1955 to serve on prestigious national committees evaluating radiation dangers.[31] The intensifying nuclear arms race of the early 1950s and President Eisenhower's goal of harnessing atomic energy for public power led to the creation of National Academy of Science (NAS) committees to formulate up-to-date statements on the known biological effects of radiation. The six committees of the "Biological Effects of Atomic Radiation" (commonly referred to as the BEAR committees) recruited many of the nation's experts in the fields of pathology, oceanography and fisheries, meteorology, agriculture and food supplies, and disposal of radioactive wastes. According to Roger Revelle, director of the Scripps Institution of Oceanography and chair of the Oceanography and Fisheries committee, his eighteen-

---

[30] Hugh Gusterson, "Secrecy, Authorship, and Nuclear Weapons Scientists," *Secrecy and Knowledge Production*, 58.

[31] Foster and Kornberg were not the only members on the NAS BEAR committees with connections to Hanford, but they had the most influential appointments. R. W. Wager, head of a pharmacology and experimental therapeutics unit at Hanford, served on the Pathology Committee until his death in 1956. Kornberg recruited three scientists with long Hanford connections to serve on his subcommittee ("Effects of inhaled radioactive particles"): W. J. Bair, a young radio-biologist heading Hanford's canine radio-isotope inhalation study; C. C. Gamertsfelder, a physicist who had come to Hanford in 1943 to work with Parker on radiation exposure control; J. W. Healy a long-time member of the environmental radiation monitoring program. Along with Foster, Lauren Donaldson served on the Oceanography and Fisheries committee. As director of the Applied Fisheries Laboratory in Seattle, Donaldson had continued to act as a regular consultant for Hanford's aquatic biology program.

member committee represented about 75 percent of the nation's experts in that field.[32] The large number of AEC scientists serving on the committees and the AEC's role as a major patron of radiation research created a sense that the BEAR committees were less than objective. Never-the-less, selection of Hanford scientists to these committees served to elevate the prestige of the Radiological Sciences department.

For Hanford biologists, selection to a National Academy of Sciences committee came as affirmation that their research programs were making important discoveries and gaining respect from colleagues. Service on these NAS committees also provided a conspicuous platform from which to argue for increased support for different fields of research. No one did this more effectively than Revelle's Committee on Oceanography and Fisheries.[33] Foster co-authored two chapters for the committee's first publication, and clearly followed Revelle's example in arguing for greater support. Foster's article on accumulation of radio-isotopes in aquatic organisms stressed that "relatively little is known about the mechanisms of uptake, concentration, retention, and excretion of fission products" and that much research remained to be done. Addressing the known effects of radiation on aquatic organisms, Foster and Lauren Donaldson reiterated that much more knowledge was needed in order to make accurate judgments concerning low-level radiation exposure.[34] In his summary Revelle made full use of the opportunity to

---

[32] Oceanography and Fisheries meeting transcript, 3rd March, 1956, page 4. NAS/BEAR, Folder: "ORG : NAS Coms on BEAR: Oceanography & Fisheries: Meetings Transcript, 3 Mar 1956"

[33] Revelle's facility for building a research program based on government and military patronage has been explored in a recent article by Ronald Rainger, "Patronage and Science: Roger Revelle, the U.S. Navy, and Oceanography at the Scripps Institution," *Earth Sciences History*, 2000, 19(1):58-89.

[34] Richard Foster and Louis A. Krumholz, "Accumulation and Retention of Radioactivity from Fission Products and other Radiomaterials by Fresh-Water Organisms;" and Lauren Donaldson and Richard Foster, "Effects of Radiation on Aquatic Organisms," *The Effects of Atomic Radiation on Oceanography and Fisheries* (National Academy of Sciences - National Research Council, 1957), 88-95 & 96-102.

point out deficiencies in inter-disciplinary knowledge of fresh-water and marine radiation biology and the need for increased research. Current understandings of "physical, chemical, and biological processes" were "woefully incomplete," Revelle argued. Answering questions about human-induced radiation hazards to aquatic environments would require a "long-range program of research on the physics, chemistry, and geology of the sea, and on the biology and ecology of its contained organisms." As fall-out and radioactive waste management became important topics of social, political, and technical concern in the mid 1950s, such reports could be used not only to define what was known, but also to lobby for an increased portion of research dollars.[35]

Radiation, Public Criticism, and the Management of Scientific Information

The growing visibility of Hanford's radio-biology research program was not only noticed by other biologists. Interested citizens, if they persevered, could gain considerable information in the early 1950s about Hanford operations and the possible health effects resulting from plutonium production. Senator Henry Jackson began receiving letters from constituents as early as 1954, with one writer suggesting that it would be "wise to stop this wide spread practice [of weapons production and testing] where-by the Pacific may become so contaminated that no time will the people be safe from it." The letter went on to implicate both atmospheric testing of nuclear weapons at

---

[35] Roger Revelle and Milner B. Schaefer, "General Considerations Concerning the Ocean as a Receptacle for Artificially Radioactive Materials," *The Effects of Atomic Radiation on Oceanography and Fisheries*, 1-25 (quotes from pp.7 & 21). Harry Kornberg chaired the subcommittee on the effects of inhaled radioactive particles and published the findings of his committee in 1961. Though nowhere near as explicit in its demand for increased research, Kornberg's report did conclude by describing eleven areas in which more research needed to be undertaken. See "Effects of Inhaled Radioactive Particles," *Report of the Subcommittee on Inhalation Hazards, Committee on Pathologic Effects of Atomic Radiation* (National Academy of Sciences / National Research Council, 1961).

the Pacific Proving Ground and the Hanford production facility for spreading dangerous, mutagenic materials through the environment. Detailed reference was made to the use of the Columbia River to cool the reactors and flush out radioactive effluent. The writer, citing research done at the Applied Fisheries Laboratory and by Hanford's aquatic biologists, called Jackson's attention to scientific studies connecting radiation exposure with increased developmental disorders.[36]

As the concerns of this uncommonly vigilant citizen suggest, the growing awareness of research in radiation biology did not necessarily reflect well on the state of radiation protection. The mid 1950s marked the beginning of a passionate debate over the dangers of nuclear weapons, especially atmospheric testing which produced worldwide radioactive fallout. Significant anti-nuclear sentiment had formed among a few Manhattan Project scientists, a movement that gained limited but short-lived public support in the immediate aftermath of the bombing of Hiroshima and Nagasaki. The development of hydrogen 'super' bombs beginning in 1952 rejuvenated the anti-nuclear movement among scientists and intellectuals and resulted in public statements calling for cessation of nuclear weapons development and testing. Albert Einstein, Bertrand Russell, Max Born, Herman Muller, Frḗderic Joliot-Curie, and Linus Pauling signed a broadly pacifist and anti-nuclear statement in 1955, a move that complemented a similar

[36] Letter from Herbert Nelson to Henry M. Jackson, May 28, 1954. HMJ Papers, Box 15, Folder 5. Before roughly 1956, constituent's concerns (as expressed to Jackson) focus on compensation issues having to do with the taking of Hanford area property during the war. Other than the Nelson letter, health and environmental dangers posed by Hanford radiation are seldom found. As greater public concerns developed over nuclear testing and fallout in the latter half of the 1950s, an increasing number of letters to Jackson display anxiety over Hanford's role in the public radiation safety debates. Yet even by the early 1960s there are relatively few such letters in Jackson's correspondence, suggesting both a lack of information and little concern on the part of average constituents.

proclamation signed by fifty-two Nobel Laureates.[37] Over the next two years anti-nuclear activists sharpened their criticism, yet the public appeals contained little specific information about the dangers of non-lethal radiation exposure.

Dr. Albert Schweitzer, recipient of the 1952 Nobel Peace Prize, produced one of the first detailed and widely distributed criticisms of nuclear fallout in a worldwide broadcast on April 24, 1957. The speech, under the auspices of the Nobel Prize Committee, began with a general description of radiation, its varied sources and forms, and the ways in which select radioactive isotopes accumulate in organisms. Schweitzer then called attention to the environmental radiation findings of Hanford's biologists:

> "The radioactive elements in grass, when eaten by animals whose meat is used for food, will be absorbed and stored in our bodies.
>
> In the case of cows grazing on contaminated soil, the absorption is effected when we drink milk. In that way small children run an especially dangerous risk of absorbing radioactive elements. ...
>
> What this storing of radioactive material implies is clearly demonstrated by the observations made when, on one occasion, the radioactivity of the Columbia River in North America was analyzed. The radioactivity was caused by the atomic plants at Hanford, which produce plutonium for atomic bombs and which empty their waste water into the river. The radioactivity of the river water was insignificant. But the radioactivity of the river plankton was 2,000 times higher, that of the ducks eating plankton 40,000 times higher, that of the fish 15,000 times higher. In young swallows fed on insects caught by their parents in the river the radioactivity was 50,000 times higher, and in the egg yolks of water birds more than 1,000,000 times higher.
>
> From official and unofficial sources we have been assured, time and time again, that the increase in radioactivity of the air does not exceed the amount which the human body can tolerate without harmful effects. This is just evading the issue. ...we are indirectly affected through that which has fallen down, is falling down, and will fall down."[38]

---

[37] Linus Pauling, *No More War!* (Dodd, Mead & Co., 1958), 158.

[38] Albert Schweitzer, "A Declaration of Conscience," *The Saturday Review*, May 18, 1957, 17-20. The editors' introduction to Schweitzer's statement notes that "the United States, Soviet Union, and Communist China were the only major nations which did not broadcast the full text." Following Schweitzer's Declaration, the *Saturday Review* published a response from Willard Libby, member of the Atomic Energy Commission; a counter-response from Harrison Brown, geochemist from the California Institute of Technology; and an analysis by *Saturday Review* science editor John Lear.

Although most media outlets in the United States, including all the Northwest newspapers, barely mentioned Schweitzer's *Declaration of Conscience*, those intimately involved in AEC matters took notice.[39] Simeon Cantril, Seattle radiologist, chairman of the AEC's Advisory Committee for Biology and Medicine, and consultant to the Hanford Radiological Sciences Department, included in a review of the Applied Fisheries Laboratory that Dr. Schweitzer's understanding of the Columbia River aquatic studies "was unfortunately misinformed."[40] Stafford Warren, the radio-biologist who played a central role in setting up Hanford's aquatic biology program, provided a more general criticism of Schweitzer's statement. "I'm convinced that what's been done so far is well within safe limits," Warren remarked. But not only were atomic tests

---

Predictably, Libby wrote in response to Schweitzer that he (Libby) "did not know what data you have utilized in studying this question, but I seriously doubt, from the evidence of your statement, that you have had access to the most recent information." Libby asserted that up-to-date information on radiation effects indicated that increased levels of radiation posed virtually no risk. Those risks resulting from nuclear testing must be accepted, he concluded, "as payment for our pleasures, our comforts, and our material progress. Here the choice seems much clearer – the terrible risk of abandoning the defense effort which is so essential under present conditions to the survival of the free world against the small controlled risk from weapons testing." For the scientific debate over fallout hazards from 1954 to 1963 see J. Christopher Jolly's, *Necessary Uncertainty.*

[39] The *New York Times* ran a front-page headline announcing Schweitzer's statement and included a full transcript the following day. In the Northwest, however, the *Oregonian*, the *Seattle Times* and *Post-Intelligencer*, the *Spokesman Review*, and the *Tri-City Herald* only gave the story minimal coverage. None of the regional papers made reference to Schweitzer's account of Columbia River studies. A story in the May 14, 1957 *Tri-City Herald*, however, provides a reminder of the extent of regional ignorance concerning Hanford and the AEC's increasing need to display a positive public image. For unknown reasons the AEC decided to open the Hanford plant to journalists for the first time since 1945. After the brief guided tour which showed off the scientific research programs, one of the journalists speculated as to the timing of the AEC's invitation to see Hanford and suggested that it had "something to do with AEC's defensive precautions it is taking to avoid radioactive fallout, currently a politically hot subject." See "Why was Hanford Opened to Newsmen – at this time?" *Tri-City Herald*, May 14, 1957 (Don Pugnetti).

[40] Simeon T. Cantril, "Evaluation off Atomic Energy Contract with the Applied Fisheries Laboratory, University of Washington, Seattle," July 1957. University of Washington Archives, Collection: W. U. Radiation Biology Laboratory, Accession #00-065, Box 1, Folder 10.

important for national security, he continued optimistically, they were important as "giant tracer experiments from the medical and biological standpoint."[41]

Schweitzer's reference to Hanford research quickly caught the attention of Northwest politicians. Oregon Senator Richard Neuberger and Washington Senator Henry Jackson dashed off letters to the AEC asking for the agency's reaction to Schweitzer's statements about the Columbia River. Jackson received a prompt reply from the chairman of the AEC, Lewis Strauss, informing him that Schweitzer had probably received his information from the presentations made by Foster and Kornberg at the Atoms for Peace Conference. To assure Jackson that the implication of Hanford's radio-isotope accumulation studies was not as bad as Schweitzer claimed, Strauss wrote that the data presented in the *Declaration of Conscience* was "not meaningful from the standpoint of health unless the concentrations are converted into amounts of radioactivity and a comparison is made with permissible concentrations of radioactivity in food consumed by humans." Jackson was told that radio-isotope concentrations remained well below permissible levels.[42]

After discrediting Schweitzer's use of Columbia River data, Strauss reminded Jackson of the AEC's substantial research investment to ensure safe operations at Hanford. "As you know," he wrote,

---

[41] Stafford Warren, "Money Woes? UCLA Medical School Dean Has $3,000,000 Worth Yearly" *Los Angeles Times,* September 22, 1957.

[42] Letter from Jackson to Strauss, May 21, 1957. Letter from Strauss to Jackson, May 24, 1957 (HMJ papers, Box 29, Folder 5). Copy of a letter from Charles Dunham (director of the AEC's Division of Biology and Medicine) to Neuberger indicates that the Oregon senator solicited similar information from the AEC. Wayne Morse, Oregon's other senator, also showed interest in Schweitzer's "Declaration" when he unsuccessfully attempted to convince major U.S. radio broadcasters to air the Schweitzer speech. See *Oregonian,* April 28, 1957 p.1.

> "the AEC has a substantial health research program at the Hanford site and at all other major AEC industrial sites, and at the Nevada and Pacific test areas. These programs include not only precise monitoring of radioactivity in air, water, and soil, but follow radioactive products as they pass along the plant-animal food chain. Special emphasis is given to consideration of the levels of radioactivity required to produce detectable injury in individuals and in population groups. A conservative estimate of the Commission's expenditures in environmental health research at AEC sites and at universities throughout the nation, is about $1.8 million a year. This estimate does not include a substantial amount of laboratory support at AEC sites, nor does it include a vast amount of related research, such as, for example, the metabolism of fission products and instrumentation."

Finally, Strauss offered to have Allyn Seymour, a program managaer for the AEC's new environmental sciences branch and a biologist at the Applied Fisheries Laboratory, detail the AEC's investments at Hanford and at the University of Washington.[43] Although Schweitzer's use of Hanford research aroused alarm, it also provided an opportunity to justify and promote radio-biological research in general and Hanford's growing research program in particular.

Hanford scientists were keenly aware of potential "misuse" of their research, suspicious that the public might misunderstand the technical basis of the science.[44] The reluctance to publicize their findings, however, was gradually overcome through the 1950's by the rewards that came with greater recognition. As a result, Hanford scientists were forced to gain public relations skills as their program evolved from a closed, military research program into a quasi-open laboratory. As director of the laboratory,

[43] Letter from Strauss to Jackson, May 24, 1957 (HMJ papers, Box 29, Folder 5).

[44] An example of this can be found in Foster's work on the BEAR committee, where he expressed the need for the committee to "exercise the utmost caution" in how they presented their findings in order to "allay the fears of those persons who arbitrarily, perhaps through lack of knowledge or understanding of the technical factors involved, voice strong objection to the introduction of any radioactive materials into the sea." See letter from Foster to Carritt, January 27, 1960, (NAS/BEAR, Folder: "Summary Reports, Drafts, 1960").

Herbert Parker drew primary responsibility for ensuring that Hanford could turn publicity into an asset. A report, written by Parker in 1954, illustrates the growing awareness that consideration of public opinion must factor into any discussion of Hanford's research and policies. As the AEC considered the effects of increased production on the Columbia River, Parker stated that "no detrimental changes in the general ecology of the river [would be] anticipated." However, with greater effluent discharge creating higher concentrations of radiation in fish, limitations on sport-fishing would have to be enforced. The prospect of restricting fishing, an important commercial and sporting activity on the Columbia, because of Hanford operations, raised the "always potentially dangerous" issue of public relations. Parker wrote that in the past, "well-chosen press releases" by the AEC and GE had "maintained a favorable situation at all times." Yet, information in the public domain presented a threat: "adverse interpretations can be given by distinguished technical individuals…whose appreciation of the radiological hazards is perhaps limited to rather recent exposure to these complex problems." Parker lamented that there appeared "to be no ready defense against" the unregulated environment of public debate. Yet unfavorable attention might have a silver lining. "In the long term," he concluded, "such criticisms may have a net positive value to the local program. Palpably erroneous impressions can be corrected by the weight of authoritative technical data, and other criticisms may serve to stimulate timely strengthening of the research and control programs."[45]

However, as Hanford biologists expanded and "strengthened" their research program, gaining wider exposure within the scientific community and to a limited extent

[45] Internal AEC document, "Atomic Energy Commission, Columbia River Contamination, Note by the Sect. Nov. 22, 1955" (DBER, Box 40, Folder 12). For quotes see pages 6, 7, 13.

with the general public, GE managers found it necessary to reevaluate whether biology research should remain part of its operation. To what extent did the increasingly independent radio-biology program fit within GE's vision for research at the Hanford site? Could GE continue to provide a stimulating environment for biological scientists? With radio-biological research central to the politically sensitive debate over weapons testing, how would Hanford's biological research reflect on GE? Could GE provide a supportive environment for biologists to continue the expansion of the past years? Or, as radio-biology developed into an established academic field, would Hanford's program benefit from the less programmatic environment that would result from University management? With Herbert Parker's research program becoming increasingly independent from GE's production assignment, questions over the future management of Hanford research began to occupy scientists, managers, politicians, and local residents concerned for the long-term viability of the community.

Chapter 4

From Hanford Laboratories to Pacific Northwest Laboratory: Biology for the Northwest, 1956 to 1964

From 1951 to 1956 Hanford's Radiological Sciences Department combined two divisions, biology and biophysics, in an increasingly prominent research program focused on the environmental and health effects of radiation. The Atomic Energy Commission (AEC) defined Herbert Parker's research program as a "single program," "project development" research effort in support of Hanford's sole mission - plutonium production. In order to continue the expansion of the past decade and to become more like the AEC's prestigious national laboratories, Parker lobbied to combine Hanford's biological and physical research programs in one distinct division – the Hanford Laboratories Operation. The Hanford Laboratories consolidated scientists formerly working within the nuclear reactor, plutonium separation, and waste management divisions with scientists from the Radiological Sciences Department. Parker, now director of a substantially larger, multi-disciplinary research division, oversaw not only in biological research but also physics and chemistry aimed at improving reactor operation and plutonium separation processes. The consolidation of Hanford's research programs created a laboratory with more than 1000 personnel, nearly 500 classified as "scientific or technical."[1]

Thus, by the end of 1956 Parker directed a significantly expanded research program, still focused primarily on "project development" but now making a bid to be

---

[1] For size and distribution of Radiological Sciences and Hanford Laboratories staff respectively, see: H. M. Parker, "Radiological Sciences Department, Quarterly Progress Report, Research and Development Activities, January to March, 1956;" and "Hanford Laboratories Operation, Monthly Activities Report, November 1956" (R-DOE, HW-42403 and HW-47291)

classified as a "multi-program" laboratory, much like Oak Ridge, Argonne, and the other major AEC laboratories. However the laboratories' reorganization did not create a more unified, efficient, and coherent overall research program as Parker had hoped. Biologists, the most successful and prominent group within the laboratory, had little incentive to continue working in an industrial laboratory with a preponderance of chemists, physicists, and engineers. With a growing reputation among colleagues, Hanford's biologists were inclined to pursue opportunities within the more traditional academic world. Yet if they were to do so, it would likely mean abandoning any hope of creating a prestigious "National Laboratory" at Hanford. This chapter explores difficulties encountered by Hanford scientists and managers as they attempted to transform the relatively small and coherent radiological research program into a large, multi-disciplinary laboratory.[2]

The institutional evolution and biological research focus of prominent Hanford scientists presents a different picture of AEC and Cold War laboratories. Unlike other AEC laboratories in the 1950s and 1960s, Hanford gained distinction primarily for its biological and environmental research while the agency channeled its major research efforts, at least financially, to the physical sciences. The building of high energy particle physics research laboratories at Brookhaven, Berkeley, Stanford, and the Fermi Laboratory in Illinois were but the most public faces of the agency's generous support of the physical sciences, and thereby contributing to these institution's prominence. In

---

[2] Peter Westwick provides an overview of how the AEC classified its various laboratories. He stresses that there was never a single policy regarding the classification of laboratories. Rather, tradition held that the "national laboratories" were those that engaged in multiple lines of research, both basic and applied. All other AEC funded laboratories were considered "single program" labs such as Hanford Laboratories, Bettis Laboratory, Cambridge Electron Accelerator, Sandia Laboratory, and the Aircraft Nuclear Propulsion Facility. See Westwick, *The National Laboratory System*, 4-25.

turn, histories of the National Laboratories have reflected the primacy of physics: consider Robert Crease's history of Brookhaven National Laboratory entitled, "Making Physics," or Jack Holl's description of Argonne National Laboratory which emphasizes the labs importance to nuclear reactor and particle accelerator research. Similarly, Robert Seidel's numerous studies of the Lawrence Radiation Laboratory and the AEC's national laboratory system have called attention to the role these institutions played in developing high-energy physics.[3]

Studies of Cold War science and its institutional setting have focused on the large number of "gadget-development" laboratories of the era, requiring the collaboration of physical scientists and engineers. Michael Dennis has described the difficulties encountered in establishing military-related post-war research and development laboratories at MIT and Johns Hopkins. Stuart Leslie has argued that the success of military-funded research laboratories at Stanford and MIT fundamentally altered the conduct of academic science. A "national defense laboratory," to be located at Princeton and proposed in the mid 1950s by a group of prominent physicists and politicians, ultimately failed to secure adequate institutional and financial support.

---

[3] Crease, *Making Physics*; Holl, *Argonne National Laboratory*; Seidel, "Accelerating Science:" Robert Seidel, "A Home for Big Science: The Atomic Energy Commission's Laboratory System," *HSPS*, 1986, 16(1):135-175; Robert Seidel, "Accelerators and National Security: The Evolution of Science Policy for High-Energy Physics, 1947-1967," *History and Technology: An International Journal*, 1994, 11:361-391; and Seidel, "The National Laboratories of the Atomic Energy Commission." Other important histories of AEC laboratories have also emphasized physics: Catherine Westfall and Lillian Hoddeson, "Thinking Small in Big Science: The Founding of Fermilab, 1960-1972," *Technology & Culture*, 1996, 37:457-492; Catherine Westfall, *The First 'Truly National Laboratory': The Birth of Fermilab* (PhD, Michigan State University, 1988); Allan Needell, "Nuclear Reactors and the Founding of Brookhaven National Laboratory," *HSPS*, 1983, 14(1):93-122; W. K. H. Panofsky, "SLAC and Big Science: Stanford University," *Big Science*, 129-146; Lillian Hoddeson, "Mission Change in the Large Laboratory: The Los Alamos Implosion Program, 1943-1945," *Big Science*, 265-289.

However, Finn Aaserud has used this event to illustrate the levels to which the physical sciences became an integral part of the Cold War state.[4]

Even though the AEC (and other Cold War laboratories) have received greater attention for their support of physics, the Commission's patronage of science was not limited to the physical sciences as demonstrated in the history of Hanford and certain other laboratories.[5] All of the National Laboratories developed significant, if not prominent, research programs in the biological sciences. Historians have looked most closely at Oak Ridge National Laboratory's development of an environmental focus in its research program. Stephen Bocking has argued that the multi-disciplinary atmosphere at Oak Ridge, coupled with the political sensitivity of a 'National Laboratory,' contributed to the closely related development of "ecology" and "environmental science." To a large extent, Bocking asserts, a tendency to conflate the environmental sciences with "environmental" political and social movements can be traced to the particular institutional basis within which these terms gained currency in the latter half of the twentieth-century.

---

[4] Dennis, "'Our First Line of Defense'..."; Leslie, *The Cold War and American Science*; Finn Aaserud, "Sputnik and the 'Princeton Three:' The National Security Laboratory that was not to be," *HSPS,* 1995, 25(2):185-239. Much of the recent historical analysis of Cold War science stems from Forman's influential essay, "Behind Quantum Electrodynamics."

[5] The enormous sums of money required to build the particle accelerators attracted the attention of scientists, politicians, and journalists. In 1959 Senator Jackson argued before the JCAE that Washington State already possessed the kind of underground tunnel needed for the $100 million linear accelerator that physicists at Stanford were trying to build with AEC funds. See Daniel Greenberg, *The Politics of Pure Science* (The University of Chicago Press, 1967, 1999), 235-239. A few years later, as the AEC conducted a search for a site to build a $250 million accelerator, journalists in Seattle wrote that "The most important single struggle over this state's economic future is being waged, quietly but bitterly, in a scattering of congressional committees." See "Northwest Economy, Scientists vs. Politicians, Bitter Battle for Machine Which Will Study Energy 1000 Times Greater Than Hydrogen – Hanford May Win: Two Senators Work for $300 million facility Which Would Boost Economy," *Argus*, May 7, 1965. In all parts of the nation, politicians' and journalists' interest in science most often centered on high-priced and exotic endeavors, such as these AEC projects, and far less often on the relatively mundane pursuits of biologists.

In another study of Oak Ridge, Albert Teich and Henry Lambright have described how a large, multi-disciplinary laboratory dealt with a changing research agenda. Oak Ridge's survival, they concluded, was due to the laboratory's ability to recast itself in terms acceptable to the changing social and "environmental" demands of the 1960s and 1970s. As environmental issues gained importance, the laboratory gave greater emphasis to its ecological and environmental science programs.[6] The emergence of the Hanford Laboratories and its eventual transformation into the Pacific Northwest Laboratory adds new perspective to our understanding of the institutional basis of the biological sciences during the Cold War.[7] At Hanford, debates over the virtues of "basic," "applied," and "programmatic" science help to explain why and how various fields of research developed under the distinct patronage concerns of the AEC and the federal government, as well as the disciplinary goals of scientists.

For the AEC (and GE), Hanford's radio-biology program through the mid 1950s remained too small and peripheral to warrant considerable analysis. Unlike the National Laboratories, the AEC gave Hanford research programs only cursory attention as long as plutonium production continued at an acceptable pace. However, with the creation of the Hanford Laboratories in 1956, GE, the AEC, regional university administrators, and scientific colleagues began to inquire into Parker's research objectives. The resulting debate produces intriguing insights into how AEC laboratory managers formulated and

---

[6] Stephen Bocking, "Ecosystems, Ecologists, and the Atom: Environmental Research at Oak Ridge National Laboratory," *Journal of the History of Biology*, 1995, 28:1-47. Albert Teich and Henry Lambright, "The Redirection of a Large National Laboratory," *Minerva*, 1977, 14:447-474.

[7] Sharon Kingsland has called for greater research into this period in the history of the biological sciences, particularly for ecology. See *Modeling Nature*, 216. Here she argues that "Ecology in the United States can be considered one of the more surprising intellectual offspring of the Cold War. The effect of the Cold War climate on ecology could use more analysis, especially for the period between World War II and the rise of the popular 'ecology movement' of the 1960s."

directed research programs and how the biological sciences figured in a multi-disciplinary, industrial laboratory. Further, as political interest in promoting regionally based research programs grew in the 1960s, the debate over Hanford's biology program illustrates the particular suitability of the biological sciences in supporting non-elitist science funding strategies. Previously marginal universities, government laboratories, and private research institutions profitably positioned themselves to take advantage of new opportunities to build research programs under changing governmental patronage guidelines.

There is also a broader social and political background within which the Hanford Laboratories evolved. Scientific and public concerns over fallout took center stage at congressional hearings held in the summer of 1957, the result of a growing public protest over the health dangers posed by low-level radiation. In the "age of the atom," issues related to radiation danger, a central area of research for the Hanford Laboratories, increased the urgency of debate over how best to stimulate the Biology Program. However, wider concern for the condition of American science following the October launch of Sputnik quickly eclipsed interest in the particular biomedical effects of environmental radiation. Although discussion over the future of Hanford's research laboratory and its radio-biology program did not hinge directly on fallout or an all-consuming space race, these events provided a stimulus for debates over the future needs of American science. Sputnik ushered in a new era in American science policy that de-emphasized the elitist policies of a previous generation and delivered increased funding for practical science, while growing concern over the health effects of radiation provided one justification for the increasing prominence of the biological sciences.[8]

Biology's Future at Hanford – Industrial or Academic?

The important national debates over appropriate funding of science and dangers of fallout provided context as plans were proposed for Hanford's future research and management. GE's conclusion that its corporate goals should not include education and biological research sparked a reassessment of Hanford's research program. Immediately on the heels of Parker's creation of the Hanford Laboratories in 1956, GE began considering changes in the management of research at Hanford. Since the late 1940s and with support from regional academic institutions, GE operated a School of Nuclear Engineering for the continuing education of Hanford employees. With prompting from the AEC, GE recognized that local, graduate level education for some of the thousands of technicians working at Hanford would help to make operation and management less dependent on a continual infusion of outside expertise. Yet formal education in nuclear science and engineering, however valuable for Hanford's operation, did not fit well into GE's corporate activities. With the creation of the Hanford Labs, GE managers realized that one or more of the regional universities might take a leading role in Hanford's education and research activities. The University of Washington (UW), the largest

---

[8] For connections between fallout and increasing relevance of biological sciences see Hacker, *Elements of Controversy*, 159-210. John Beatty has outlined both intellectual and institutional factors in the growth of the biological sciences in the postwar years, "Ecology and Evolutionary Biology in the War and Postwar Years: Questions and Comments," *Journal of the History of Biology*, 1988, 21(2):245-263. Daniel Kevles' social history of physics provides the best overview of the dramatic changes in science funding and policy following Sputnik: *The Physicists*, 384-410. Robert Divine has argued that the United States was never 'behind' the Soviets in science and technology but that Sputnik caused Eisenhower to grudgingly increase US funding of science in order to appease politicians and the public clamoring for evidence of a vigorous response to the Soviet challenge. *The Sputnik Challenge*, 200-205.

research university in the region, appeared to be the logical choice to assume operation of a nuclear engineering school at Richland. Taking action on this idea, GE crafted a proposal in the spring of 1957 outlining a strategy for the university's greater involvement in Hanford's educational and research activities.

UW administrators received GE's proposal with considerable excitement. Henry Burd, dean of the graduate school and chair of a committee evaluating GE's proposal, urged UW's president to act quickly to ensure that a rival institution not get a chance to seize GE's offer. This "golden opportunity," he argued, presented the UW with access to a $35 million dollar research facility which included specialized equipment far beyond what could be acquired with the university's financial resources. With other universities aggressively pursuing costly research in the nuclear sciences, Burd argued that the University of Washington should pursue closer ties with the Hanford Laboratories and thereby keep pace with competing institutions "at trifling cost."[9]

The first part of GE's proposal, operation of the nuclear engineering education program, presented no obstacles. Within a year, having considered ways to involve all the regional universities in a management consortium (à la the University of Chicago's leadership of Midwest research universities' management of Argonne), the UW alone signed a contract with the AEC to operate a "Center for Graduate Study." To the disappointment of AEC managers who favored institutional cooperation at its regional research facilities, no formal association of Northwest universities was created. However, students from Washington State College, Oregon State College, and the

[9] Report of "Committee to Evaluate Hanford Graduate Center Proposal" to UW President Henry Schmitz, May 1957. University of Washington Archives, Collection: W. U. Presidents, Accession #71-34 (hereafter cited as, WU Pres), Box 56, Folder: "C&S: Graduate School – Center for Graduate Study at Hanford – General, 1957-1958."

University of Idaho were guaranteed access to courses offered at the Center.[10] With no opposition on the part of AEC administrators or Hanford employees, transfer of GE's graduate education responsibilities proceeded quickly. On the other hand, a far more contentious aspect of GE's proposal - transferring the biology program of the Hanford Laboratories to the University of Washington – proved to be a much thornier issue.

The Biology Operation's relatively greater emphasis on non-Hanford related research distinguished it from the physical and chemical programs in the newly consolidated Hanford Laboratory. The Biology Operation's prominence stemmed from its increasing engagement in nationally important research "concerned with radio-biological problems derived from the general needs of radiation hazard control."[11] Yet, for GE, it was becoming less clear how the biologist's research contributed to the immediate interest in plutonium production. The growing stature of the Laboratories' radio-biology program and its decreasing relevance to Hanford's day-to-day operation, convinced GE management that the biology program might also interest UW scientists and administrators.

GE's Hanford manager, W. E. Johnson, indicated that operation of the Center for Graduate Study and transfer of the biology program constituted two related parts of an effort to streamline the company's plutonium production responsibilities. GE's interest in divesting itself of the biology program was, however, not simply due to a lack of relevance. Growing distrust in the region over GE's ability to act as its own "authority

---

[10] A public announcement of the creation of UW's Graduate Center can be found in the *Columbia Basin News*, "The City That Shook the World: Steady Change Typified Plant 20 Year Growth," April 23, 1958. For UW's interest in the University of Chicago's management of Argonne, see UW Archives, Collection: "W. U. Graduate Study Joint Center (Richland)," Accession # 72-4, Box 1, Folder "CS: Graduate School: Special: Hanford – Argonne National Lab."

[11] Harry Kornberg, "Introduction," *Hanford Biology Annual Report – 1956*, 8.

with respect to the amount of radiation to be discharged to the environs," Johnson asserted, made it wise to establish a friendly but "independent authority on this subject, whose voice would be accepted by the public and by the press." Johnson added that Senator Jackson, Washington's governor Albert Rossellini, and UW administrators had all been receptive to the idea of bringing in a local academic research partner. Yet transferring the Biology Operation would be "most sensitive," he concluded. Herbert Parker's reaction (and that of his colleagues in the AEC's Division of Biology and Medicine) could certainly turn out to be the major obstacle.[12]

The University of Washington responded to the second part of GE's proposal with even greater enthusiasm than it had for the nuclear engineering education program. UW president Henry Schmitz portrayed the acquisition of the Biology Operation as critical to the long-term success of the university. In a memo to the Board of Regents, Schmitz stressed that the radio-biology laboratory proposal was separate from the offer to operate the graduate education center. Although the nuclear engineering program provided access to important resources for the University, Schmitz believed that the biology program

> "may be far more important to the University ... both in the near term and in its long-term implications. The Laboratory, in short, must be viewed not merely as a remote facility to be acquired but also as a part of a larger and very necessary effort by the University to attain a new position of leadership in one of the most critical research areas of the nuclear age."[13]

---

[12] Letter from W. E. Johnson to F. K. McCune, Vice President, GE Atomic Products Division, July 17, 1957. WU Pres., Box 57, Folder "C&S: Graduate School – Hanford Biology Laboratory, 1957-1958."

[13] Memo from Schmitz to Board of Regents, "Subject: Hanford Biology Laboratory" November 1, 1957. WU Pres., Box 57, Folder "C&S: Graduate School – Hanford Biology Laboratory, 1957-1958."

GE's interest in creating a 'neutral' radiation safety advocate and the UW's eagerness to acquire a respected radio-biology program met headlong with Herbert Parker's objections. In the late summer of 1957 he fired off an impassioned letter to W. E. Johnson, refuting the company's need to cultivate independent radiation safety expertise. In the event that public agencies or political activists took legal action over past radiation exposure practices, Parker claimed that "every member of the scientific force would testify" to the soundness of Hanford's radiation protection standards. Believing firmly in the authority of objective scientific knowledge, Parker confidently asserted that "no one can seriously make stick the accusation that technical results are distorted to suit business ends." GE, not surprisingly, recognized that perceptions of bias, even if largely unfounded, could have a powerful influence on corporate success.[14]

Parker's more convincing arguments against separation of the Biology Operation from the Hanford Laboratory centered on the positive contributions biology could make within an integrated radiological sciences laboratory. Countering the University of Washington's desire to incorporate Hanford's radio-biology research into an interdisciplinary program of radiation studies, Parker claimed that the newly created Hanford Laboratories' non-academic environment could more effectively coordinate the biological, physical, and chemical research programs around unifying problems emerging from nuclear energy development. In recounting the historical basis of Hanford's biological research, Parker declared that the recently broadening investigations could not "stand alone" but only made sense "through integration with all

---

[14] Herbert Parker to W. E. Johnson, August 9, 1957 "Biology Program," 4. WU Pres., Box 57, Folder "C&S: Graduate School – Hanford Biology Laboratory, 1957-1958."

other radiological sciences." Of most importance, Parker concluded, the best strategy for gaining 'national laboratory' status required that biology - "the best and best publicly known research" – be presented as the primary constituent of a multi-program research effort.[15] In short, academic institution's tendency to segregate research disciplines worked against the multi-disciplinary needs of the radiological sciences as well as the ultimate goal of creating a 'national laboratory.' The Hanford Laboratories, Parker believed, could provide an integrating atmosphere in which biological research would effectively contribute to Hanford's national defense mission in close cooperation with physicists, chemists, engineers, and others studying the various facets of radiation.

Because of Parker's power as overall manager of the Laboratory, and because many of his Manhattan Project colleagues now held positions of power within the AEC, Parker's opposition to the transfer of the radio-biology program threw up a significant roadblock in what would have been a rather simple transaction. Because radio-biology was not directly applicable to weapons production and was largely unclassified, the AEC had long supported the creation of academic biomedical programs such as those at the University of Rochester and the University of California at Los Angeles. With considerable advice from Harry Kornberg, Oak Ridge National Laboratory and the University of Tennessee had just completed negotiations to develop a close cooperative relationship for biomedical research.[16] Therefore, recent trends in the division of

---

[15] Ibid, 7.

[16] See minutes to the 61st meeting of the Advisory Committee for Biology & Medicine, March 15, 16, 1957. DBER, Box 105, Folder 11. Harry Kornberg was one of three scientists advising Oak Ridge management on how to cultivate relations with nearby universities. Oak Ridge, like Hanford, was managed by an industrial firm and hoped to develop ties to academic researchers in order to overcome the perceived "industrial laboratory" stigma.

biology and medicine's policy seemed to encouraged GE's plan for academic management of Hanford's biology program.

A January 1958 meeting of the AEC's advisory committee for biology and medicine provided an opportunity for GE, the UW, and Hanford scientists to present their opinions. Citing a report prepared by an independent management consultant, GE argued that biologists had little opportunity for advancement in the corporation and moreover, in recent years the biology program had contributed little to the plutonium production operation. The report's findings, coming from non-scientists, had little impact on the advisory committee. Parker, expressing his objections to the plan, drew attention to the fact that GE's report had come from non-scientists. Biology, he asserted, contributed vitally to the Hanford Laboratories' integrated research efforts to understand, manage, and optimize Hanford operations. In this regard biologists constituted just one discipline that might be considered "unemployable by General Electric." As manager of a multi-disciplinary laboratory, Parker believed he could not give biologists special consideration without damaging the overall morale. Finally, Parker believed that if the UW were to assume management of the biology program, its vigor would inevitably suffer due to the isolating tendencies of academic disciplines. Hanford's applied radiation biology program would be ill-suited to an academic setting, but conversely, Parker portrayed it as an ideal constituent of a government laboratory responsive to industrial concerns.[17]

---

[17] A copy of the independent review by the consulting firm of Booz, Allen and Hamilton can be found in UW Archives, Collection: "W. U. Graduate Study Joint Center (Richland)," Accession # 72-4, Box 1, Folder "CS: Graduate School: Special: Hanford – Biology Operation Booz-Allen-Hamilton Report, 1957." For Parker's presentation see "Biology in the Hanford Laboratories," WU Pres, Box 57, Folder "C&S: Graduate School – Hanford Biology Laboratory, 1957-1958;" and minutes from the 66th meeting of the Advisory Committee for Biology and Medicine (DBER, Box 105, Folder 1).

Following Parker, Harry Kornberg presented his own view supported by most, but not all, biologists involved in the proposed transfer. Kornberg emphasized the advantages of academic management for the AEC, GE, and the Biology Operation, including lower costs, increased creativity, more effective training of students, and a better prospect for long-term stability. Some drawbacks existed, he conceded, notably the difficulty to be expected in transferring high-paid industrial employees to lower-wage academic positions. In reference to the central issue of accountability, Kornberg denied the assertion that "academic freedom" might result in a lack of focus and productivity for the research program. He assured the AEC that the majority of research conducted by Hanford biologists would "continue to be programmed, planned, and appraised" in accordance with Commission priorities. Kornberg's appeal ended by suggesting that a transfer of the radio-biology program would be the best way to strengthen "the biological sciences in the Northwest" and thereby help to meet important "national objectives" for improved scientific training. Looking to the future, Kornberg could see that the current "applied" radio-biology program at Hanford could diversify and expand into vital areas of "fundamental biology" thereby helping to "make the University one of the Nation's outstanding centers for biological sciences."[18]

The clash of opinions suggested a great degree of "internecine warfare in the GE organization." Harry Kornberg's presentation, openly disagreeing with his own supervisor, only added to the sense of disarray.[19] As a result of the mixed signals being

[18] Harry Kornberg, "Hanford Biology" January 1958. (Quotes from page 27, 29.) American Philosophical Society archives, Bentley Glass Papers, Ms Coll 105, Series II, Professional, Atomic Energy Commission (hereafter, B. Glass Papers), Box 14.1.

[19] Quotation comes from Henry Burd, UW Graduate School Dean, present at the Advisory Committee meeting. Memo from Burd to Schmitz, "Meeting of the Advisory Committee on Biology and

sent out by GE management and top scientists, UW administrators offered muted interest in Hanford's biology program. Schmitz' emissary, Henry Burd, explained that the UW had been "marking time" while GE prepared a clear statement of its intentions. The university was in the process of strengthening and reorganizing its various radiation research programs and Hanford's radio-biology program would, Burd indicated, complement the university's ongoing efforts in this field.[20] The AEC advisory committee, however, could see little preparation on the part of the university for integrating Hanford's radio-biology operation into its own research plans.

Recognizing that a tentative decision needed to be made quickly, the advisory committee sided with Herbert Parker. The University of Washington did not appear prepared to make full and efficient use of Hanford's biologists. Consequently, the Biology Operation would remain an important research unit of the Hanford Laboratories for the time being. The committee firmly supported Parker's conception of biology at Hanford: "the present biology program at Hanford is important for the Hanford operation in particular and for the practice of applied radio-biology in general. It would be extremely inefficient to interrupt it now, or to separate the applied from the basic parts of the program, since both are closely interwoven and since the basic program helps to support the applied program."[21]

Medicine of the Atomic Energy Commission." WU Pres, Box 57, Folder "C&S: Graduate School – Hanford Biology Laboratory, 1957-1958.

[20] Memo from Burd to Schmitz, "Meeting of the Advisory Committee on Biology and Medicine of the Atomic Energy Commission." WU Pres, Box 57, Folder "C&S: Graduate School – Hanford Biology Laboratory, 1957-1958.

[21] 66th Advisory Committee for Biology and Medicine meeting minutes. DBER, Box 105, Folder 1.

In a concession to those arguing for academic management, the director of the AEC's division of Biology and Medicine ordered an immediate separation of Hanford's biology laboratory from restricted parts of the reserve. The movement of Hanford's security fence, what Kornberg claimed as the year's "most noteworthy" accomplishment, therefore represented both loss and gain in the Biology Program's effort to gain increased independence.[22] Furthermore, wider professional interaction needed to be encouraged through increased attendance at conferences and by building "closer liaisons" with both the UW and Washington State College. Yet even with these modifications, the AEC recognized that the question of biology at the Hanford Laboratories remained unsettled. The AEC appointed a three-member panel of scientists to review the fundamental question brought about by GE's proposal: how best to "further research and training in radio-biology in the Northwest."[23] The AEC's decision to consider the long-term vitality of Northwest science came at a time of rising concern over the safety of radiation and as politicians devised new strategies for improving the status of American science in the wake of Sputnik.

For residents of the Tri-Cities, whether or not radio-biologists worked under GE or University of Washington management made very little difference. On the other hand, as employment decreased, residents showed keen interest in the prospect of a "national laboratory" in Richland. Overall employment at Hanford peaked at more than

---

[22] Kornberg, "Introduction," *Hanford Biology Research Annual Report - 1959* (R-DOE, HW-65500).

[23] 66th Advisory Committee for Biology and Medicine meeting minutes. DBER, Box 105, Folder 1. For appointment of a scientific inquiry committee, see Memo from Burd to Schmitz, "Meeting of the Advisory Committee on Biology and Medicine of the Atomic Energy Commission." WU Pres, Box 57, Folder "C&S: Graduate School – Hanford Biology Laboratory, 1957-1958.

8,500 workers in 1956, but had been in steady decline thereafter.[24] Residents eagerly looked for any sign of AEC expansion in the region, especially in areas not directly tied to fluctuating plutonium production cycles. A front-page story in the *Columbia Basin News* in February 1958 proclaimed that the University of Washington was getting ready to assume responsibility for all research operations at Hanford with the goal "that the area could become a national laboratory." With information supplied from the university's director of public relations, the story claimed that although scientists supported the idea, the outcome would depend on Senator Jackson who had "repeatedly called for establishment of a national laboratory at Hanford."[25] Although Jackson had expressed interested in expanding the scope of Hanford's research laboratory, the decision over UW involvement at Hanford rested solely with AEC administrators.[26] The local journalist targeted his story to a population continually looking for signs of economic diversification. For AEC biologists back in Washington DC, economic stability in the Columbia Basin presented less of a concern than the growing criticism that AEC research programs were losing their national relevance.

---

[24] For an overview of Hanford employment and major trends in operation intensity through the 1950s, see *Tri-City Herald*, "Phase Out At GE Will Mark 2nd Big Change At Hanford," January 21, 1964.

[25] Patrick J. Owens, "'U' Operation of 300 Area Suggested," *Columbia Basin News*, February 19, 1958. See letters between Owens and Neal Hines, Director of University Relations, UW Pres., Box 56, Folder "C&S: Graduate School – Center for Graduate Study at Hanford – General, 1957-1958."

[26] Jackson's role in the transfer debate is difficult to determine. The GE manager portrayed Jackson as being in favor of the transfer in 1957, but Jackson's papers suggest that he made no strong commitment either way. Jackson's responses to Ken Englund, an AEC manager at Hanford who favored UW management and repeatedly lobbied the senator for support, were noticeably ambiguous. By 1960 Englund pointedly remarked, "Actually, Scoop, I've never been really sure of where you stand on this whole biology program at Hanford." Englund to Jackson, January 13, 1960. HMJ papers, Box 52, Folder 8.

AEC Laboratories and the Redirection of American Science

Nearly two years would pass between the advisory committee's provisional decision and release of the review panel's recommendation on how best to manage Hanford's biology program. During this time the AEC began to suffer from accusations that its research programs were unresponsive to national needs. Clinton Anderson, chairman of the Joint Committee on Atomic Energy, wrote to the AEC in early 1959 expressing congressional unease over whether the government's "increasingly large financial outlays" were being efficiently utilized for the "pursuit of broad objectives in the field of science and technology."[27] Anderson's letter mobilized an agency-wide re-assessment of Commission laboratories and their research objectives. Debate among AEC scientists and administrators mirrored many of the issues that confronted Hanford Laboratory scientists and administrators. At issue was the optimum organization of science: could the nation gain the most benefit from increasing support for academic science and decreasing the role of federal (in this case AEC) laboratories? Or could government research centers do a better job of producing significant and socially useful knowledge? For AEC laboratories the matter boiled down to how much unfettered basic research the agency would allow as opposed to a more strict and politically defensible policy of organized, programmatic research. With the success of Sputnik displaying Soviet competency in science and technology, the United States began to demand increased evidence of a return on its post-war investments in science. President Eisenhower's science advisor stated bluntly that the nation risked being "doomed to unnecessary weakness and backwardness in a world where other nations are not so

[27] Letter from Anderson to John McCone, chairman of the AEC, February 3, 1959. DBER, Box 61, Folder 16.

foolish" if it did not commit to a massive "national effort ... to strengthen our scientific knowledge and technological efforts in all fields, aimed at the advance of knowledge and the enhancement of the general welfare."[28]

With this general concern for the productivity of American science as a backdrop, the AEC began to assess how the allocation of its research dollars – to scientists in academia versus scientists at its own laboratories – might be improved. Max Zelle, a geneticist and director of the AEC's Biology Branch, summarized his department's view by unequivocally stating that no "clear distinction [existed] between basic and applied research." As such, national laboratories could not be expected to "devote their energies only to applied research." Because of an inability to clearly distinguish between basic and applied science, Zelle continued, it made no sense to try and whittle away basic programs at AEC laboratories. Furthermore, Commission laboratories occupied a novel place in the nation's research community, because they brought together "competent people in the many diverse fields ... required for a rational attack on many problems." The interdisciplinary strengths of the AEC's huge laboratories, Zelle concluded, should not be considered a drain on scientific talent but rather as a resource to strengthen

> "the somewhat lower level of scientific sophistication in the regional universities. To emasculate the national laboratories of their competent staffs, which would be the inevitable result of too strict a policy of programmatic applied research for the laboratories, would make them

---

[28] President's Science Advisory Committee, "Education for the Age of Science" (U.S. Government Printing Office, 1959), 4-5. Not only did politicians demand greater accountability from federal science programs, but journalists also took a more critical attitude towards the established funding mechanisms. For example, Daniel Greenberg, a writer at *Science* in 1960, began to scrutinize the special (often lax) treatment scientists received in Washington as they requested ever more federal money for projects with questionable social relevance, such as Stanford's $100 million linear accelerator. For a short description of Greenberg's early career, see John Maddox, "Foreword to the 1999 Edition," in Greenberg's, *The Politics of Pure Science*, ix-xiv.

totally unable to meet their responsibility of stimulating and encouraging the research development of the universities."[29]

John Bugher, advisory committee member (and former director) of the AEC's Division of Biology and Medicine, took another tack on the problematic distinction between basic and applied research. If Congress were looking for ways to promote the efficiency of the nation's scientific endeavors by redirecting basic programs to universities, it would be making a major mistake. Bugher argued that "almost never does a line of scientific research get transferred from a governmental agency to a university" without major changes in focus and application. Under these conditions it did not matter whether a program could be defined as basic or applied; if it was important to the AEC then it should be retained within an agency laboratory.[30]

The Biology and Medicine advisory committee continued its analysis by addressing the problems and opportunities for the AEC's individual laboratories. The advisory committee warned that AEC laboratories were becoming less interested in pursuing research applicable to Commission needs. For the Hanford Laboratories, the advisors believed that all biological investigations must be oriented to the applied problems unique to Hanford's particular situation: "the isolated and difficult Hanford location is not appropriate for a continuing major biological research program which does not depend upon the operational problems of the Hanford Works for its logic." In summary, the committee noted, with a hint of exasperation, "institutions of this kind seem never to be closed save by war or depression," and therefore the AEC must find

[29] Memo from Zelle to C. W. Schilling, Deputy Director AEC Division of Biology and Medicine, August 20, 1959. DBER, Box 61, Folder 16.

[30] Letter from Bugher to C. L. Dunham, director of the Division of Biology & Medicine, August 27, 1959. DBER, Box 61, Folder 16.

ways to build a vigorous program despite the inherent disadvantages of a geographically isolated, programmatic research center. With this in mind the advisory committee urged more vigilant supervision of the research programs in its laboratories to ensure that they addressed appropriate basic and applied programmatic needs. At the same time, "steps should be taken to promote interest in these programs…within the scientific world at large" so that first-rate scientists would be attracted to the laboratory.[31]

As the laboratory review progressed, a consensus began to form within the AEC's Division of Biology and Medicine and its advisory committee: instead of spinning off marginally productive research programs, the AEC needed to reinforce the programmatic mission of its laboratories. While practical, results-oriented science would undoubtedly receive support in Congress, considerable effort would need to be made to convince scientists that the programmatic research undertaken by the AEC was in fact good science. But, as John Bugher hinted, generous project-specific "AEC budgeting" would be the best way to ensure that scientists consider the interests of the Commission. Those scientists accepting AEC research funds but not interested in its programs should be, he argued, encouraged to "seek employment elsewhere."[32]

The ability of AEC patronage to produce compelling research programs in the life sciences was most notable in "radio-ecology" - a small sub-field of ecology that gained wide recognition in the early 1960s.[33] John Wolfe and Allyn Seymour, Division

---

[31] Minutes, 75th meeting of Advisory Committee for Biology and Medicine, June 12, 13, 1959. DBER, Box 105, Folder 1.

[32] Bugher to Dunham, Aug. 27, 1959. DBER, Box 61, Folder 16.

[33] Toby Appel claims the "AEC practically created the field of radiation ecology," *Shaping Biology*, 137. Other historians echo this idea. See Hagan, *Entangled Bank*, 115-118; Frank Golley, *A History*

of Biology and Medicine coordinators of environmental research cultivated considerable AEC support for ecology. Due to Wolfe and Seymour's success, the AEC approved an "Environmental Sciences Branch" within the Division of Biology and Medicine in 1958, an administrative move that provided consistent funds to develop ecology programs of interest to the AEC. The kinds of descriptive field surveys of radio-isotope accumulation pioneered by Hanford biologists constituted a major portion of research in the Environmental Sciences Branch.[34]

The appointment of J. J. Davis to direct the Hanford Laboratory's "Radio-ecology Unit" in 1956 illustrates radio-ecology's direct linkage with Wolfe's "environmental science" program. Beyond the local aquatic and terrestrial radio-ecology studies ongoing at Hanford, Davis's unit became heavily involved in ecological surveys of an Alaskan coastal region just above the Arctic Circle, starting in 1958.[35] Hanford biologist's involvement in research outside the Columbia Basin illustrates not only the growing prominence of ecology and the AEC's ability to build up an 'environmental science' program, but also the tightly intertwined nature of political

---

of the Ecosystem Concept in Ecology: More Than the Sum of the Parts (Yale University Press, 1993), 71-73; Chunglin Kwa, "Radiation Ecology, Systems Ecology and the Management of the Environment," *Science and Nature: Essays in the History of the Environmental Sciences,* 218.

[34] For an overview of what constituted radio-ecology (and the pioneering role of Columbia River radio-isotope accumulation studies) see Odum, *Fundamentals of Ecology*, (2nd edition, 1959), chapter 14. For a later description of radio-ecology's origins and institutional basis, see Whicker and Schultz, *Radioecology: Nuclear Energy and the Environment,* 1-7; and Stannard, *Radioactivity and Health,* 750-782.

[35] For Davis' appointment to head a radio-ecology unit see *Hanford Biology Annual Report – 1957.* Hanford scientists' involvement in Project Chariot is documented in J. J. Davis, "Project Chariot Environmental Program, Phase II Interim Final Report," May 31, 1960 (CIC, Accession #NV0173280). Hanford scientists were involved in three phases of the overall study: "1) limnology of streams and inland ponds, 2) ecology of terrestrial invertebrates, and 3) radio-ecology of the terrestrial and freshwater communities. The objectives of the limnological and terrestrial invertebrate studies are to determine the structure, dynamics and interrelationships of communities representative of the region. The radio-ecology studies are designed to investigate the accumulation and transfer of radio-elements in the natural communities" (pp., 2-3).

events with the development of new kinds of research. Two years before Davis took his radio-ecology group to Alaska, Lawrence Livermore scientists had begun championing the use of nuclear explosions for civil engineering. The AEC quickly endorsed "Project Plowshare" as a way to demonstrate the peaceful applications of nuclear power. Plowshare scientists, led by Edward Teller, drafted an audacious plan to carve out a harbor on the far northwest coast of Alaska using a series of nuclear explosions. As atmospheric testing of nuclear weapons came under intense public criticism and increased the possibility of a moratorium, Plowshare was seen by some weapons scientists as a way to continue working on (and testing) nuclear explosives.[36]

For biologists in the newly created Environmental Sciences Branch, the Alaska harbor construction experiment (code named Project Chariot) provided an opportunity to undertake an equally ambitious ecological survey of a relatively unknown ecosystem to determine the environmental effects of multiple underground (and underwater) nuclear explosions. More than seventy scientists from academic and government laboratories attempted to measure the "ecological baseline" of an arctic ecosystem prior to nuclear detonations that would affect the "total biota and physical environment." Project Chariot provided just the kind of AEC funded biological research that John Bugher called for. It directly addressed the needs of the AEC and simultaneously attracted attention of a

---

[36] For the history of Project Chariot see O'Neill, *Firecracker Boys*. In a shorter essay, "H-Bombs and Eskimos: The Story of Project Chariot," *Pacific Northwest Quarterly*, 1994, 85(1):25-34, O'Neill argues that the possibility of a testing moratorium spurred weapon's scientists to begin thinking of ways to use nuclear bombs for civil engineering purposes. Hugh Gusterson notes that Edward Teller and E. O Lawrence vigorously lobbied President Eisenhower to resist a weapons testing moratorium based on their view that it would be a "crime against humanity" to delay developing "clean bombs" for civil engineering purposes, *Nuclear Rites*, 140-141.

wider community of scientists.[37] The creation of the Environmental Sciences Branch and its ability to redirect the agency's science programs is but one instance of the ways in which the AEC, in the late 1950s and into the 1960s, attempted to redefine its mission and its place in the scientific community to include broadly applied biological sciences.[38] For Hanford, the AEC's increasingly 'environmental' focus became particularly important as the Hanford Laboratory was slowly finding its research niche within the AEC's laboratory system.

Discord in Unity – The Preservation of Biology in the Hanford Laboratories

By the end of 1959, GE was still seeking to rid itself of the biological research unit and University of Washington administrators continued to plan for a radiological studies program that would include Hanford's radio-biologists. For their part, Hanford's radio-biologists were more eager than ever to work for the UW. In apparent disregard of the AEC's directive not to turn established programs over to universities, the advisory committee's scientific review team (made up of three AEC-affiliated scientists) recommended in favor of the Biology Operation's speedy transfer to university administration. The review team believed that under university management Hanford's program would be "strengthened scientifically so as to assume a proper position of

[37] The ecological findings from Project Chariot were published as, *Environment of the Cape Thompson Region, Alaska*, edited by Norman Wilimovsky & John Wolfe (AEC Division of Technical Information, 1966), v-viii. The largest single contingent of scientists (seventeen) came from Hanford's radioecology unit and from the University of Washington's Laboratory of Radiation Biology (formerly the Applied Fisheries Laboratory).

[38] Teich and Lambright's essay, "Redirection of a Large National Laboratory," argues that Oak Ridge's relative pre-eminence in radiation biology (broadly defined) resulted in that laboratory's attempt, beginning in the mid 1960s, to redefine itself as a life sciences (rather than the physical sciences) laboratory. Teich and Lambright's argument situates Oak Ridge's reorientation within the growing "environmental" movement of the post-Carson years whereas I am emphasizing that Hanford's interest in the ecological sciences initially springs from 1) local environmental problems beginning in the 1940s and 2) the AEC's explicit and ample environmental science patronage starting in the mid to late 1950s.

leadership in the development of biological and especially radio-biological research in the Northwest." In reviewing Hanford's biology program the scientists found little to praise and suggested the program would be at best a "pedestrian operation" if left in its present state.[39]

In private communication to the advisory committee the review team evaluated each research program, noting that only the radio-ecology program was "active and promising" while research in aquatic biology and plant microbiology was "uninspired" and "disappointing." The review team suggested that the solution to these problems would be found in greater communication and interaction with academic colleagues. The review team dismissed Parker's argument that biological research was integrated throughout the Hanford Laboratories operation as "not within the province of this Committee's responsibilities." In conclusion the review team pointed out what it saw as the fundamental problem with the quasi-industrial research environment at Hanford:

> "it is highly programmatic to the AEC. Perhaps it illustrates the danger of too highly programmatic orientation which is reflected in possibly lower general staff competence and in a lack of stimulatory interest from the outside scientific community at large. To this extent, the more basic and broader orientation which would likely result from a University affiliate may be the most important reason for a transfer."[40]

Even as top level AEC administrators tried to strengthen the programmatic aspects of Commission research, scientists argued that "basic" research must form the foundation of quality, creative science. However, lofty rhetoric about basic science may have been

---

[39] "Report by the Hanford Biology Operation Review Team to Dr. Charles L. Dunham," November 2, 1959. B. Glass papers, Box 15.2. The review team consisted of Alexander Hollaender, director of the Oak Ridge biology program, and two members of the Division of Biology and Medicine (DBM): Max Zelle and Herbert Stanwood.

[40] "Hanford Biology Program Transfer Proposal, Confidential Report of the Review Team Committee to the Director, Division of Biology and Medicine" (November 2, 1959). B. Glass papers, Box 15.2.

concealing simple opportunism. Max Zelle, review team member, director of the AEC's Biology Branch, and long-time consultant to both the UW Radiation Biology Laboratory (formerly the Applied Fisheries Laboratory) and Hanford's Biology Program, accepted a position at the University of Washington to "coordinate radio-biological research" less than a year after the review team's vigorous support of the Biology Program's transfer. The AEC's advisory committee, in noting Zelle's move wrote, "it is hoped Dr. Zelle can develop closer relations between the University and the Hanford group."[41]

Lurking behind the question of how to stimulate scientific creativity lay a far more mundane problem. Herbert Parker, director of the whole research program, was becoming an increasingly difficult person to work with. Part of his plan to integrate the Hanford Laboratories' various research efforts involved relocating the Biology Operation closer to the rest of the Laboratory. When the Hanford Laboratories acquired research buildings on the outskirts of Richland in 1957, the biologists decided to remain at their original location nearly thirty miles inside the Hanford reservation. Kornberg, unwilling to allow closer cooperation under the domineering Parker, viewed the attempt to relocate the biologists as part of Parker's misguided effort to control all research. A private letter from Kornberg to Hanford's GE manager, W. E. Johnson, complained of Parker's unreasonably meddlesome control over everyday research activity and his complete unwillingness to accept advice once he had made a decision. This situation,

[41] In addition to simple opportunism that may have played a part in Zelle's moves, this episode illustrates how institutions and federal agencies often used a 'revolving door' between funding agencies and researchers in order to create well-funded research programs. Recall also how Allyn Seymour, from the Applied Fisheries Laboratory, went to the AEC to help establish the Environmental Science program. See Advisory Committee meeting minutes, #77 and #88, DBER, Box 105, Folder 2 and Box 106, Folder 3.

Kornberg wrote, made Parker's desire for greater disciplinary integration untenable among the biology staff. Kornberg believed that "integration" would not result in more innovative biology, but rather was only a self-serving ploy on Parker's part "to show that biological research is a vital part" of Hanford's overall operation. Combining Hanford's diverse research programs, according to Kornberg, would dilute them all and inevitably result in "a rather indefinable mixture." Beyond dulling the biology program, Kornberg believed that a multi-disciplinary attack on Hanford's problems would end up costing GE, and taxpayers, money that would be better spent "developing atomic energy as a normal industry."[42] For Kornberg, health and safety problems directly applicable to plutonium production seemed to provide little more for biologists to study.

With the review team's report delivered to the Division of Biology and Medicine, a decision was imminent. Although the majority of Hanford scientists, GE and UW officials, favored the transfer, the all-important advisory committee had given indications that it would support Parker. Ken Englund, a staunch supporter of the transfer and director of the AEC's Richland office of radiation science, noted in a letter to Senator Jackson that much of the laboratory staff would resign if the AEC accepted Parker's "viciously recommended" plan.[43] As he had done previously, Jackson remained interested but refused to take sides. Without Jackson's intervention it came as no surprise when the AEC found in favor of the laboratory director, Herbert Parker. The

---

[42] Letter from Kornberg to W. E. Johnson, "Strictly Private," January 20, 1960. W.U. Pres., Box 57, Folder "C&S: Graduate School – Hanford Biology Laboratory, 1957-1958." The review team recognized the incompatibility between Parker and much of the biology staff and cited this as a major reason to endorse management change. See "Hanford Biology Program Transfer Proposal, Confidential Report of the Review Team Committee to the Director, Division of biology and Medicine" (November 2, 1959). B. Glass papers, Box 15.2.

[43] Letter from Englund to Jackson, January 13, 1960. HMJ papers, Box 52, Folder 8.

Biology Operation would remain part of the Hanford Laboratories. Although the decision satisfied Parker, the working conditions would make productive and distinguished research difficult to achieve. Charles Dunham, the director of the Division of Biology and Medicine placed blame for the lackluster biological research program on "the geographical and scientific isolation of the Hanford biology program from the scientific community at large, and more particularly from the universities of the northwest." Somehow, Dunham concluded, a way must be found to "greatly strengthen basic biological and radio-biological research in the northwest."[44] This was, of course, a politic statement all could agree on.

Ken Englund's prediction that the biological staff would resign if forced to work under Herbert Parker never materialized. In an effort to mend the open rift between Parker and Kornberg, GE management drafted a separate "charter" for the biology program to define its relationship within the Hanford Laboratories. Reflecting Kornberg's criticism that Parker exercised too much oversight, the charter directed that the biologists were free to pursue research in "any field of the biological sciences of interest" to the AEC, and it encouraged informal communication between Hanford biologists and colleagues in the Division of Biology and Medicine. In deference to Parker's concern for an integrated research program, the charter stipulated that the biology program "will be expected to recognize, initiate and foster in their components inter-group cooperation in all radiological sciences programs."[45]

[44] Letter from C. L. Dunham to UW President Odegaard and WSU President French, June 22, 1960. W. U. Pres., Box 57, Folder "C&S: Graduate School – Hanford Biology Laboratory, 1957-1958."

[45] "Charter of Biology Laboratory of Hanford Laboratories Operation." (undated) W.U. Pres., Box 57, Folder "C&S: Graduate School – Hanford Biology Laboratory, 1957-1958."

Rather than fracturing Hanford research programs, the AEC sought to preserve an integrated laboratory. This decision pleased Herbert Parker and at least some Richland community members looking for signs of increased AEC investment in the region. To people in Richland the development of a national laboratory (or its equivalent) had long been the ultimate objective. An expanded research program would satisfy the demands of scientists for a more vigorous and creative research environment and it would appease community leaders seeking to diversify and stabilize the regional economy.

In an effort to communicate local sentiment to the AEC, a task force of Hanford Laboratory scientists developed detailed alternatives for future growth of the Hanford Laboratories. A key component of any expansion, they argued, would be based on the Hanford Laboratories implementation of "closer cooperative research studies involving biology, physics, and chemistry." Therefore, the task force viewed the Hanford Laboratories' retention of the biology program as the most important step in becoming a multi-disciplinary "National Laboratory." With the Biology Operation leading the way, the Hanford Laboratories would be able to gradually "remove the 'provincialism' remaining from the days when [research] was strictly production oriented."[46]

Community leaders echoed Hanford scientists' interest in creating a National Laboratory. Richland's total dependence on AEC employment meant that the region's economy fluctuated directly with the nation's demand for plutonium. Prospects for economic growth appeared slim in the late 1950s as construction of the seventh and

[46] Report of the Hanford Laboratories Task Force to W. E. Johnson and H. M. Parker, "Rough Draft," March 21, 1960. W.U. Pres., Box 57, Folder "C&S: Graduate School – Hanford Biology Laboratory, 1957-1958."

eighth reactors at Hanford had been completed in 1956. Reduced demand for plutonium as a result of the nuclear test moratorium, signed in 1958, only added to Richland's sense of economic gloom. In Richland, as in other federal communities founded during the Manhattan project, the AEC made efforts to ameliorate the periodic economic downturns. Richland community leaders repeatedly informed AEC representatives of the deep interest that existed for building upon Hanford's scientific and technical "assets." If the AEC loosened its constrictive grip on research activities, residents believed Hanford scientists' ideas and skills could be put to economic use "creating and sustaining an environment … attractive for the broadest possible spectrum of activities." A key component of this vision lay in expanding Hanford's research activities, an idea broadly expressed by area residents in an AEC report calling for "continued expansion of non-defense oriented R&D at Hanford labs;" "conversion of the Hanford Laboratories into a National Laboratory not limited to nucleonics;" "expansion of the Hanford Laboratories by every possible means;" or creation of a "Northwest Research Institute."[47] At the time the success of regional models of technological and economic cooperation, such as the Stanford Industrial Park and Route 128 near Boston, suggested to community planners that scientific research provided one of the surest foundations for robust economic growth.[48] Richland residents believed it would be a waste not to capitalize on the knowledge and skills already existing in the Tri-Cities.

---

[47] "Report on AEC Cooperation in Industrial Development Efforts of Communities Such as Richland, Washington, and Oak Ridge, Tennessee." (undated). NARA, RG 326, "Records relating to fallout monitoring studies," "Project Sunshine," Entry 73. Box 25, Folder 2.

[48] For the economic diversification of Richland, and the AEC's role in it, see Findlay, "Atomic Frontier Days," 35-37. Although no explicit connections were made between the economic development of the Tri-Cities and the establishment of technology based industrial parks in other regions of the country, studies of such places as Silicon Valley have suggested that by the early 1960s there existed a wide-spread belief that science and technology provided the building-blocks for

The connections between national nuclear weapons' policy and local economic conditions in Richland were direct and immediate. Through 1958 the United States demanded a consistent flow of plutonium for its expanding weapons stockpile. Although the government's construction and operation projects at Hanford created a boom and bust cycle for local business, through the Eisenhower years Hanford employment gradually increased. By the late 1950s a more effective anti-nuclear movement, with its intensifying criticism of atmospheric testing, helped crystallize an agreement on a nuclear testing moratorium. Ironically, this positive international development ushered in a prolonged period of uncertainty over Hanford's future. In the face of a production slow-down, Senators Jackson and Magnuson, long-time advocates of public power, convinced congress to authorize construction of a dual-purpose reactor and thereby helped ease local anxiety. Hanford's ninth (and final) reactor was the first to produce electricity as well as plutonium and represented the first major economic diversification achievement. Much as the Columbia River appeared as an untapped energy source to an earlier generation, Hanford promoters hoped to transform military reactors into cheap energy generators for the benefit of the region's civilians.[49]

The end of the testing moratorium in the summer of 1961 presaged a return to full production at Hanford. However, the very real fears of nuclear war sparked by the Cuban Missile Crisis in the fall of 1962 helped bring about international agreement on a

---

sustained economic growth. Kargon, Leslie, and Schoenberger have described the important role of federally funded "big science" in the creation of "science regions." See "Far Beyond Big Science," 334-336. See also Leslie and Kargon, "Selling Silicon Valley," 435-472; and Findlay, *Magic Lands*, 119-142.

[49] Bruce Hevly, "Hanford's Postwar Voices," *Working on the Bomb*, 231-236. Hanford's dual-purpose reactor was not only unique because it produced electricity; it was the first and only Hanford reactor not to run Columbia River water directly through the reactor.

Limited Test Ban Treaty the following year. As a result of the treaty, President Lyndon Johnson announced a shut-down of three plutonium reactors at Hanford. A front-page story in the *Oregonian* spelled out the impending effect on the Tri-Cities: more than 2,000 jobs would be lost, representing a quarter of GE's workforce.[50] Just as the nuclear build-up of the 1950s caused Hanford to expand, attempts to limit nuclear weapons in the 1960s resulted in dramatic cutbacks in plutonium production and a negative impact on the economic fortunes of the Tri-Cities.

Neither Academic nor Industrial – The Birth of a Regional Laboratory

The fluctuating demand for plutonium, along with the AEC's interest in helping to diversify Richland's economy, convinced GE to reconsider its varied commitments at Hanford. Six months after the AEC made its final decision regarding the biology operation, W. E. Johnson once again approached the University of Washington with a suggestion to transfer GE's research responsibilities. This time Johnson proposed that the UW should organize an association of Northwest universities to petition the AEC for management of the Hanford Laboratories. In Johnson's mind GE's future at Hanford was limited, and therefore Northwest universities needed to make one final pitch for Hanford's research program before it was "sectioned off to other AEC labs." Whatever Johnson's motives were for trying to divest GE of the Hanford Labs, his suggestion for creating an association of regional colleges was in accord with current AEC national laboratory management practices, and with specific recent recommendations for strengthening Hanford's research programs. Brookhaven and Argonne laboratories had for over a decade worked in formal association with regional research institutions. Los

[50] A. Robert Smith, "'65 Slash to affect 2,000 Jobs: Arms surplus prompts move by government" *The Oregonian*, January 9, 1964.

Alamos, the Lawrence Radiation Laboratory, and the Livermore laboratory were all successfully managed by the University of California system in what I. I. Rabi enviously called the "University of California atomic trust."[51] Oak Ridge, although managed by an industrial firm, had developed research agreements with the University of Tennessee, as well as other nearby universities. If Hanford were to become a "national laboratory," Johnson believed that a consortium of regional universities must lead the way.[52] However, cooperation among often competitive regional universities was seldom easy.[53]

AEC laboratories presented academic managers with a quandary. On the one hand the labs could provide access to cutting-edge technology in a quasi-academic atmosphere, epitomized by Brookhaven National Laboratory. On the other hand, AEC laboratories were often places of mundane, programmatic, and often classified research. AEC laboratories inhabited an institutional space somewhere between their industrial and academic counterparts. While being neither academic nor industrial could be viewed as an asset, more often laboratory managers felt a need to become more 'academic' in order to satisfy scientists. Government analysts pushed for increasingly

---

[51] Gregg Herken, "The University of California, the Federal Weapons Labs, and the Founding of the Atomic West," *The Atomic West*, 123.

[52] Johnson's proposal was transmitted, informally, in a memo from the manager of the UW's Richland Graduate Center (Bengtson) to the dean of the Graduate School (McCarthy): Bengtson to McCarthy, Feb 16, 1961. WU Pres., Box 57, Folder "Graduate School – Hanford Biology Laboratory, 1961-1962." For AEC recommendations that Hanford work in closer cooperation with all Northwest universities see "Report on AEC Cooperation in Industrial Development ..." (op cit. 44) and letter from C. L. Dunham to UW president Odegaard and WSU president French (op cit. 41). For an overview of AEC's research management practices see Harold Orlans, *Contracting for Atoms: A Study of Public Policy Issues Posed by the Atomic Energy Commission's Contracting for Research, Development, and Managerial Services* (The Brookings Institution, 1967), 41-100.

[53] Leonard Greenbaum has described the difficulties encountered as Midwestern universities tried to cooperatively manage Argonne National Laboratory. Between 1946 and 1965 four different consortia were organized in an effort to effectively incorporate the competing interests of universities to devise a coherent and feasible research program for the laboratory. *A Special Interest*, 30-37, 72-77, 187.

results oriented research.[54] Although the Hanford Laboratories offered the UW (and other regional universities) access to unique facilities and a cadre of well-qualified scientists, the overall program was highly programmatic and only marginally distinguished. In short, acquiring the Hanford Laboratories presented regional university administrators with a unique opportunity. However, the challenge of incorporating Hanford's applied research programs into an academic educational mission caused AEC and university officials to question the compatibility of the two institutions. In the absence of a zealous university suitor for the Hanford Laboratories, an industrially oriented contract research organization entered the scene. Unlike academic researchers, this new entrant arrived in the Northwest with the expressed aim of targeting its research expertise at industry, using science to create economic wealth.

The non-profit Battelle Memorial Institute, based in Ohio, was in many ways ideally suited to manage the particular demands of Hanford's research programs. Neither academic nor industrial, the Institute came into existence in the 1920s as one of the first research laboratories designed to exploit the expanding technical needs of industry and government.[55] During World War II Battelle entered into government-

[54] For a discussion of AEC laboratories' industrial versus academic goals see notes from a laboratory managers meeting: H. A. Stanwood to C. L. Dunham, "Commission Meeting with Laboratory Directors - Role of the National Laboratories," November 30, 1959. DBER, Box 61, Folder 16. Roger Geiger has outlined the difficulties and opportunities that emerged during the Cold War as universities became increasingly involved in government sponsored research; "Science, Universities, and National Defense, 1945-1970," *Osiris*, 1992, 7:26-46. See pages 27-31 for issues peculiar to AEC laboratories.

[55] Historians have become increasingly interested in the institutional context of science and the ways in which prominent philanthropies have influenced the direction of various disciplines. For the role of industrial research laboratories see: Leonard S. Reich, *The Making of American Industrial Research: Science and Business at GE and Bell, 1876-1926* (Cambridge University Press, 1985); Wise, *Willis R. Whitney, General Electric*; Paul Israel, *From Machine Shop to Industrial Laboratory: Telegraphy and the Changing Context of American Invention, 1830-1920* (Johns Hopkins University Press, 1992); David Hounshell and John K. Smith, Jr., *Science and Corporate Strategy: Du Pont R&D, 1902-1980* (Cambridge University Press, 1988). The literature on the role and influence of philanthropic

sponsored science by performing metallurgical research in the Manhattan Project. Following the war the AEC's reliance on contract research allowed Battelle to supplement its traditional industrial contracts with consistent federal research. However, Battelle's primary interest (and the source of its growing wealth), involved turning ideas into marketable products for industry. In the late 1930s Battelle worked with inventor Chester Carlson and the Haloid Company on "electrophotography" – a process of paper document duplication. By the late 1950s the Haloid Company, soon to be renamed Xerox, had a wildly popular copier on the market. In post-war America, Battelle was able to capitalize on a proven reputation for helping corporations bridge the research and marketing gap.[56]

Battelle's increasing wealth in the late 1950s, mainly from Xerox patents, allowed it to expand into new areas of research as well as new regions of the world. Soon after opening laboratories in Frankfurt and Geneva, Battelle began surveying the western United States for a potential West Coast laboratory. As early as 1962 Battelle's president, a graduate of the UW, began making contacts in the Seattle business, political,

---

institutions is extensive; for an introduction see: John Servos, "Changing Partners: the Mellon Institute, Private Industry, and the Federal Patron," *Technology and Culture*, 1994, 35:221-257; Robert Kohler, *Partners in Science*; Lily E. Kay, *The Molecular Vision of Life: Caltech, the Rockefeller Foundation, and the Rise of the New Biology* (Oxford University Press, 1993); Sharon Kingsland, "An Elusive Science: Ecological Enterprise in the Southwestern United States," *Science and Nature: Essays in the History of the Environmental Sciences*, edited by Michael Shortland, 151-179; and Allan Needell, "The Carnegie Institution of Washington and Radio Astronomy: Prelude to an American National Observatory," *Journal for the History of Astronomy*, 1991, 22:55-67.

[56] George Boehm and Alex Groner, *Science in the Service of Mankind* (Battelle Press, 1986), 25-48. John Servos' study of the decline of the Mellon Institute suggests that varying post-war success for the few independent research institutions in the country (Battelle, Mellon, the Stanford Research Institute, and the Midwest Research Institute) lay in their willingness to undertake mundane research projects that would yield quick results, and in willingness to pursue government work. Battelle profited not just from the major successes of its industrially sponsored research (principally the Xerox patents) but also from its interest in pursuing less glamorous federal government research and management assignments. John Servos, "Changing Partners," 254-255.

and academic community as the company looked to establish an "industrial research institute" in the Northwest.[57] Battelle's interest in a Northwest research center coincided with a broadening of the Institute's research agenda. Battelle's scientific expertise through the 1950s remained squarely in areas of material science and engineering. The growing influence of the social and biological sciences in the 1960s led Battelle to develop research specialties in such areas as regional planning, education, health care, ecology, alternative energy development, pollution control, and oceanography.[58]

At the time Battelle management began investigating the possibility of a Northwest research center, there was little reason to believe the Institute would settle on the Hanford Laboratories. GE had yet to publicly announce any intention of withdrawing from management of the Hanford Laboratories. A potentially more worrisome problem lay in a growing perception that Parker's reorganizations were ruining the research environment at Hanford. Both within and without the Hanford Laboratories, people questioned the direction and quality of its research program. A reorganization following the AEC's decision to preserve Parker's version of the Hanford Laboratories resulted in the creation of a new "Environmental Sciences" section to go along with the "Biology" program.[59] Much as Kornberg had feared, this new research focus was aimed directly at documenting and managing the environmental consequences

[57] See correspondence between Battelle president B. D. Thomas and UW president Odegaard, 1962 onward: Box 3, Folder "CS: Graduate School: Special: Hanford – Current Negotiations Battelle (1964)."

[58] Boehm and Groner, *Science in the Service of Mankind*, 73-75.

[59] The AEC analyst describing Parker's reorganization stated that the proliferation of research projects at Hanford, which was in part responsible for the creation of the "environmental sciences" unit, was "leading to [a] complex and unmanageable sort of organization." For this analyst, a simpler organizational plan needed to be implemented along the lines of Brookhaven, which had just two life science programs: Biology and Medicine. James Liverman, director, Biology Branch, DBM, "Notes – ACBM Meeting, January 11-12, 1963." DBER, Box 106, Folder 4.

of Hanford operations. Being a virtually unrecognized disciplinary specialty, Kornberg saw this new "environmental" focus as a waste of valuable research dollars. A substantial environmental science program would, Kornberg believed, "turn the inconsequential into the hazardous."[60] Kornberg's pointed skepticism of the Laboratories' direction was echoed by an AEC analyst who wrote at the time of reorganization, "Looking at the present organization anyone would see it would be improved with organization."[61] In the early 1960s Parker's attempts to create an inter-disciplinary research program simply appeared to many scientists as a waste of money and a befuddling of clearly demarcated scientific disciplines. Battelle, interested less in the disciplinary coherence of the Hanford Laboratories than in the possibility of applying research for economic gain, took a more optimistic view.

GE's continued difficulty in managing the Hanford Laboratories, coupled with the Johnson administration's decision to shutdown three reactors, convinced the corporation to pull out of its Hanford contract. A press release on January 21, 1964 stated that GE would gradually withdraw from day-to-day responsibility for Hanford operations and that multiple contractors would be hired in the ongoing effort to "stimulate commercial diversification of industry in the Hanford area." The AEC stated that GE's withdrawal was accepted primarily for the opportunity to bring in a non-profit research organization to manage the Hanford Laboratories.[62] Just four months later

---

[60] Kornberg to W. E. Johnson, "Strictly Private" January 20, 1960. W.U. Pres., Box 57, Folder "C&S: Graduate School – Hanford Biology Laboratory, 1957-1958."

[61] John Totter, "Notes – ACBM Meeting, January 11-12, 1963." DBER, Box 106, Folder 4.

[62] AEC press release, "AEC and General Electric Company agree to Bring new contractors into management of Hanford facilities," January 21, 1964. DOE archives, AEC press releases – 1964, vol. 1. See also Senator Jackson's statement, "Diversification Aided," *Tri-City Herald*, January 21, 1964.

Richland residents learned that the Battelle Memorial Institute had won the contract for the Hanford Laboratories. As contract details were finalized over the summer, the AEC authorized Battelle to rename the laboratory as the Pacific Northwest Laboratory.

Battelle's arrival at Hanford signaled a new beginning and greater independence for the research program. Herbert Parker's vision of a unified, multi-program laboratory was preserved even while he stepped down as manager of the laboratory. No longer under Parker's management, Harry Kornberg showed greater "willingness to take on programmatic tasks" and accepted that the laboratory's biology program would necessarily have a distinctly applied and multi-disciplinary focus.[63] Where the Hanford Laboratories had been portrayed as geographically and scientifically isolated, it was now suggested that the Pacific Northwest Laboratory was ideally situated to stimulate biological research in the region. AEC administrators wrote in glowing terms that the Laboratory could build on "the great potential of an excellent research staff already established in unique geographical surroundings." In particular, "access to the Columbia River and the unusual ecological opportunities to be found in an arid region now being transformed by irrigation promises great opportunity for exciting biological investigations."[64] While little had truly changed in the day-to-day research at Hanford, by the mid 1960s the acceptance of and opportunities for applied biological research were being rapidly transformed.

---

[63] Letter from C. L. Dunham to H. A. Kornberg, April 2, 1965. DBER, Box 106, Folder 5.

[64] Letter from ACBM (Fred Hodges, chair) to G. Seaborg, AEC chair, June 4, 1965, based on the advisory committee's first visit to the newly christened Pacific Northwest Laboratory. DBER, Box 106, Folder 5.

The Politics of Practical Science and a Place for Environmental Research

The AEC decision to reinvent the Hanford Laboratories as a regional research center under Battelle management illustrates a broad trend within American science during the 1960s to promote socially useful science in neglected geographic regions. Taking its most explicit form during Lyndon Johnson's administration, federal patronage of science became tied to an egalitarian and utilitarian ethic to a far greater degree than during the previous two decades. Johnson, famous for asking his budget analysts to continually consider "what science can do for grandma," placed strong emphasis on practical, useful science. Under the influence of Johnson's anti-elitist rhetoric, funds for science were increasingly allocated in a geographically sensitive manner rather than based on the current prestige of a given program.[65] Johnson's over-arching "Great Society" policy objectives of the 1960s encouraged all federal agencies to consider how their funding patterns would contribute to the material well-being of every American. In this respect the modest, applied research programs originated by Herbert Parker in the late 1940s finally came of age in an era that prized practical research contributing to the scientific and technical competence of a "provincial" region.[66]

---

[65] A 1965 policy speech clearly stated Johnson's goals: "Our policies and attitudes in regard to science cannot satisfactorily be related solely to achievement of goals and ends we set for our research.... We must, I believe, devote ourselves purposefully to developing and diffusing – throughout the nation – a strong and solid scientific capability.... At present, one-half of the Federal expenditures for research go to 20 major institutions, most of which were strong before the advent of Federal research funds. During the period of increasing Federal support since World War II, the number of institutions carrying out research and providing advanced education has grown impressively. Strong centers have developed in areas which were previously not well served. It is a particular purpose of this policy to accelerate this beneficial trend since the funds are still concentrated in too few institutions in too few areas of the nation." Quotation and original citation can be found in Greenberg, *The Politics of Pure Science*, 285-286.

[66] For an overview of how the Johnson administration changed science funding policy debates in the 1960s see Kevles, *The Physicists*, 410-415. For an example of the arguments in favor of geographical distribution of funds, see Paul Gross, "R & D, and the Relations of Science and Government: A

Finally, the long drawn-out establishment of the Pacific Northwest Laboratory illustrates a different side to the traditional story of Cold War, AEC laboratories. Rather than being based on the experimental use of exotic equipment of unprecedented expense, or on scientists' ability to construct clever gadgets for the nation's military, the establishment and evolution of Hanford's laboratory revolved primarily around concerns for managing the environment. In the immediate post-war context, applied environmental and biomedical research found relatively less support within the scientific community and within the institutional structure of the AEC. Two decades later the dramatic expansion of the scientific enterprise as well as a reorientation of scientific, technological, and social goals began to create a more hospitable space for the biological sciences. Mounting alarm over the fate of the environment, eloquently conveyed to a broad audience by Rachel Carson, called attention to both the positive and negative role science played in managing an industrialized world. Increasingly, ecology and the 'environmental' sciences – an interdisciplinary mixture of sciences oriented at managing environmental problems – came to be seen as acceptable scientific specialties as society placed greater emphasis on practical, socially relevant science. The development of Hanford's research interests within a particular regional setting, from institutional patronage motivated by Cold War concerns, and encouraged by a changing political landscape, illustrate how certain areas of the life sciences emerged as influential forces within American science in the latter half of the twentieth-century. Hanford, situated along the Columbia River, drew its original research agenda from the natural setting and justified continued research on biological and economic grounds. As the

---

Statement Before a Congressional Subcommittee by the Retiring President of the AAAS," *Science*, 1963, 142:645-650.

Pacific Northwest Laboratory took up the research programs of the Hanford Laboratories, the natural setting and economic conditions continued to provide the rationale for research. But now, less marginalized by secrecy, institutional oversight, and scientific disdain, Hanford's practical research was expected to contribute to an improving science, and society, in the Northwest.

Chapter 5

Hanford and Science in Context

Because they were operating a particularly dangerous kind of industrial plant, Hanford scientists developed research programs that studied radiation levels, first in fish and later in numerous forms of plant and animal life important to the regional economy (and in the case of salmon, to regional culture). The various research projects focused on individual organisms and gradually branched out into studies of communities and ecosystems. Practical considerations of radio-isotope transfer and accumulation and the increasing fruitfulness and popularity of "ecological" research spurred these programs to expand. What began primarily as field studies – monitoring radiation levels in fish, plants, and air – gradually evolved into a combination of field monitoring and laboratory studies designed to provide practical feedback on Hanford operation as well as the health effects of industrial pollution.

Radio-isotope inhalation studies with dogs, pigs, and other organisms gave scientists "controlled" data from a more respectably "scientific" environment and enabled them to interpret, evaluate, and validate the mass of field data. Hanford attracted a wide spectrum of disciplinary specialists resulting from its origin in the Manhattan Project (seen as a physics-based, but highly inter-disciplinary project) and the practical emphasis of its industrial context. The diffuse practical problems encountered in radiation protection required specialists in biochemistry, botany, radio-biology, entomology, physics, chemistry, meteorology, and other disciplines. Facets of Hanford's research program - its multi-disciplinary nature, its direct relevance to industry and industrial pollution, and scientists' success in establishing reasonable freedom and autonomy within national defense related laboratories – mirrored certain aspects of research at other Atomic Energy Commission (AEC) labs. The particular regional importance of the research program and its intellectual roots in the biological and ecological sciences set Hanford apart from AEC laboratories that came to prominence immediately following World War II to foster "national" research in the physical sciences.

The most recent study of U. S. National Laboratories has called attention to the interconnected and competitive nature of these institutions as they strove to establish unique identities within the AEC. "Systemicity" as Peter Westwick puts it, "fostered specialization and diversification of lab programs and ensured the responsiveness of the labs to national priorities." Moreover, as a major agency within the nation's national defense effort, the substantial resources available to the AEC ensured that Commission priorities received attention from scientists. From the agencies' inception in 1947, Westwick argues, AEC laboratories had the "power to shape the terrain of science, to build up certain disciplines and neglect others." Yet even with ample funds to influence scientific trends, the success of commission laboratories hinged on their ability to attract and retain quality scientists.[1] Westwick's study of the National Laboratories sheds light on how AEC research centers operated. However his analysis excludes important developments that took place at the lesser laboratories, such as Hanford. The forces of specialization Westwick uses to explain the diversity of research programs at the National Laboratories apply equally to Hanford. The unique nature of Hanford's biological and environmental research focus encouraged its preservation within an

[1] Westwick, *The National Laboratory System*, 1, 17.

agency that, on the surface, prized physics. As the status of the practical (and in comparison to physics, relatively economical) biological sciences grew in the 1960s, Hanford's research program began to emerge from obscurity. Its regional context, its aim to manage industrial waste, and its potential for practical economic application became, in fact, the features by which Hanford's research could be promoted.

Expansion of Biology

Hanford's biological research programs, unique within the AEC's research community, evolved within broader contexts that also encouraged their development. For the establishment of Hanford's research program, the most important trend within twentieth-century American science has been the increasing emphasis placed on the biological sciences. In his recent examination of the biological sciences and their social setting in America, Philip Pauly writes that the rising status of scientists brought about by World War II primarily profited physical scientists and only "gradually incorporated biologists." "In the immediate postwar setting," he continues, biologists "struggled for an identity independent of medical scientists and for a recognized place at the federal table. Gradually, however, they became visible and successful." By 1960, he concludes, "biologists were full participants in the increasingly systematized American system of scientific research."[2]

This generality held true within the AEC. Physicists and chemists dominated the agency's single research division at its start in 1947. Almost as an afterthought, biologists convinced the Commission to create the Division of Biology & Medicine in

[2] Philip Pauly, *Biologists and the Promise of American Life: From Meriwether Lewis to Alfred Kinsey* (Princeton University Press, 2000), 239. For a wider perspective on twentieth-century changes in biology see *The Expansion of American Biology*, edited by Keith Benson, Jane Maienschein, and Ronald Rainger (Rutgers University Press, 1991).

1948 to foster research in such areas as genetics and radiation biology.[3] Even though the AEC continued to be most closely associated with the physical sciences throughout its existence, genetics, ecology, and radio-biology became disciplines in which the AEC provided major support. In ecology, a discipline central to Hanford's research effort, Chunglin Kwa has described the AEC as the "most important patron" through the 1960s.[4] Hanford scientist's interest in radio-biology and ecology reflected the particular demands of plutonium production in the Columbia Basin and the larger expansion of opportunities in the biological sciences and the AEC's growing participation in these disciplines.

One explanation for the growth of the biological sciences has been the deliberate crossing of disciplinary boundaries, a practice that occurred most explicitly in molecular biology. Historians and scientists have suggested that biology expanded with the introduction of new tools and methods from physics and chemistry. Pnina Abir-Am, Lily Kay, and Robert Kohler have productively explored this development in their studies of molecular biology, research institutes, and philanthropic foundations in the early decades of the twentieth-century. Abir-Am has emphasized collaboration between consciously interdisciplinary scientists in forging a new discipline: molecular biology. Focusing on institutional factors, Lily Kay has argued that molecular biology flowered at the California Institute of Technology because no "competing disciplinary traditions" existed and the institutional culture "championed new fields grounded in interdisciplinary cooperation." Robert Kohler's study highlights the importance of

---

[3] For organization of the AEC's Biology & Medicine division, see Hewlett and Duncan, *Atomic Shield*, 112-14 & 251-52.

[4] Kwa, "Radiation ecology, systems ecology," *Science and Nature: Essays in the History of the Environmental Sciences*, 215.

patronage in encouraging new forms of scientific inquiry. With varying success, Warren Weaver at the Rockefeller Foundation promoted a vision of science focused on "vital processes" in which "chemists, physicists, and mathematicians, as well as biologists" pooled their skills, methods, and instruments.[5] As this thesis shows, an interdisciplinary culture, the lack of entrenched institutional traditions, and the targeted support of a sponsoring agency made fundamental contributions to the development of Hanford's Biology Program.

In the early post-war period, important social and political factors also played a role in encouraging the development of practical and constructive biological sciences. Nicolas Rasmussen has argued that another interdisciplinary field, biophysics, developed because a heightened sense of social responsibility, following the atrocities of World War II, fostered interest in problems that could directly improve life, rather than destroy it.[6] Ecology experienced marked success in the twentieth-century due in large part to the belief that the natural world could be efficiently managed in the face of industrial and population pressures. In his overview of the environmental sciences, Peter Bowler writes that ecology emerged "as a means of helping the human race to manage its interference" of the natural environment "more effectively."[7] In general, the greater

---

[5] Pnina Abir-Am, "The Discourse of Physical Power and Biological Knowledge in the 1930s: A Reappraisal of the Rockefeller Foundation's 'Policy' in Molecular Biology," *Social Studies of Science*, 1982, 12:341-382; and "The Biotheoretical Gathering, Transdisciplinary Authority and the Incipient Legitimation of Molecular Biology in the 1930s: New Perspective on the Historical Sociology of Science," *History of Science*, 1987, 25:1-70. Kay, *The Molecular Vision of Life*, 15. Robert E. Kohler, *Partners in Science: Foundations and Natural Scientists, 1900-1945* (The University of Chicago Press, 1991), 391.

[6] Nicolas Rasmussen, "The Mid-century Biophysics Bubble: Hiroshima and the Biological Revolution in America, Revisited," *History of Science*, 1997, 35(3):245-293.

[7] Bowler, *The Norton History of the Environmental Sciences*, 377-78. For more on the social utility of the biological sciences see: Ronald Rainger, "Introduction," in *The Expansion of American Biology*, 7; and Mitman, *State of Nature*, 108 & passim.

sense of positive social utility in the biological sciences has been a powerful factor propelling them into prominence as science became a larger component of modern American society.

John Beatty suggests that biology appeared to hold solutions to social problems and that, especially in the AEC, biological research often conformed to political considerations. In a case study of the AEC-sponsored Atomic Bomb Casualty Commission, Beatty argues that publicized scientific results, though contested within the scientific community, supplied important justification for continuation of U.S. nuclear weapons policies.[8] Hanford's biological and ecological studies supplied similarly positive justification for continued and increased plutonium production, thereby illustrating the mutually beneficial relationship that could exist between scientific research and political objectives. In sum, a confluence of factors propelled the biological sciences into a "vast, well-endowed enterprise of considerable scientific as well as social and political significance."[9] It is within this context of expanding biological research opportunities that an understanding of the Hanford Laboratory's particular concerns begins to make sense.

Science, Politics, & National Defense

As a new institution at the cross-roads of science, the military, and industry, Cold War concerns deeply influenced the evolution of Hanford's research programs. President Eisenhower's valedictory speech to the nation in January of 1961 gave widespread attention to the rapidly changing relationship between the military, science,

---

[8] John Beatty, "Genetics in the Atomic Age: The Atomic Bomb Casualty Commission, 1947-1956," *The Expansion of American Biology*, 284-324.

[9] Rainger, "Introduction," *Expansion of American Biology*, 15.

and society. "In the councils of governments," Eisenhower stated, "we must guard against the acquisition of unwarranted influence…by the military-industrial complex." The United States must be wary of the "prospect of domination of the nation's scholars" from unprecedented levels of federal patronage, and "we must also be alert to the equal and opposite danger that public policy could itself become the captive of a scientific-technological elite."[10]

Charles Kidd, a prominent science policy analyst during the Cold War, provided one of the earliest scholarly warnings that "large-scale federal financing of research has set in motion irreversible forces that are affecting the nature of universities … establishing new political relations, and changing the way research itself is organized."[11] Historians of science have begun to analyze the fundamental changes that took place in this time period. A major issue, both in the early Cold War and reiterated more recently by some historians, involves the distortion of science's perceived "disinterested pursuit of truth" by new power relations.[12] Paul Forman, a prominent exponent of the distortionist thesis, argues that the physical sciences in America "underwent a qualitative change in…purposes and character" in the early postwar period. Scientists in this period, he concludes, "lost control of their discipline. They were now far more used by

---

[10] *Public Papers of the Presidents of the United States: Dwight D. Eisenhower*, vol. 8, 1960-1961 (U.S. Government Printing Office), 1038-39.

[11] Kidd, *American Universities*, v.

[12] One of the many Cold War expressions of this sentiment can be found in Barry Commoner's *Science and Survival* (The Viking Press, 1963). Commoner begins his commentary by claiming that science has strayed from its foundational ethos. "In the last few decades serious weaknesses in this system of principles have begun to appear. Secrecy has hampered free discourse. Major scientific enterprises have been governed by narrow national aims. In some cases, especially in the exploration of space, scientists have become so closely tied to basically political aims as to relinquish their traditional devotion to open discussion of conflicting views on what are often doubtful scientific conclusions…. No new principles are needed; instead, scientists need to find new ways to protect science from the encroachment of political pressures." (p. 128)

than using American society, far more exploited by than exploiting the new forms and terms of their social integration."[13] Stuart Leslie makes a similar argument in his study of World War II and postwar American research universities. The transformation of science in this period, he claims, must be understood in "terms of our scientific community's diminished capacity to comprehend and to manipulate the world for other than military ends."[14]

Daniel Kevles' influential study of the American physics community suggests that scientists have always been looking for consistent patronage and that the Cold War shift to military support and national defense priorities is consistent with the embedded nature of science and politics. Summing up twentieth-century associations, Kevles states that physics "found its most effective claims to patronage in economic, then in defense arguments; its most faithful allies, in the sometimes popularly suspect institutions of big business and the military."[15] Thus, while there is consensus over the existence of fundamental changes to the relationship between science and government, divergent explanations can be found as to what these transformations have meant for the scientific enterprise and for the society in which science exists.

How do the issues of scientists' altered and contested commitments inform a history of research at Hanford? Certainly Hanford's origins as part of the military efforts of World War II and the ensuing national defense issues of the Cold War

[13] Forman, "Behind Quantum Electronics …," *HSPS*, 1987, 18:150-229.

[14] Stuart Leslie, "Science and Politics in Cold War America," in *The Politics of Western Science, 1640-1990*, edited by Margaret C. Jacob (Humanities Press, 1992), 231. For a more complete defense of the distortionist thesis see Leslie's *The Cold War and American Science.*

[15] Kevles, *The Physicist*, 425. See also his more recent essay, "Cold War and Hot Physics: Reflections on Science, Security and the American State," *Proceedings of the International Conference on the Restructuring of Physical Sciences in Europe and the United States, 1945-1960* (World Scientific, 1989) 1-30.

complicate any understanding of the scientific accomplishments of Herbert Parker's research program. The distortionist approach, or the more benign interpretation using a concept of dynamic patronage networks, must inform the national-defense context and meaning of Hanford research. In fact Hanford (and the AEC) have often been portrayed as promoting questionable scientific research in order to justify certain political agendas; other writers defend the institutions and scientists as acting prudently within the context of the Cold War.[16] Thus, important historiographic questions of Cold War science revolve around notions of 'good' and 'bad' science. The effect of particular funding arrangements and the commitments engendered by these relationships are crucial to understanding the establishment of a formal research program at Hanford.

Another development in twentieth-century science - "big science" - also helps make sense of the Hanford Laboratories. Derek de Solla Price's influential book, *Little Science, Big Science*, turned "the tools of science on science itself."[17] Price graphically demonstrated the massive growth that was taking place in science, especially in the rapidly rising number of scientists and scientific publications. The unprecedented utilization of scientific talent in World War II, exemplified by the Manhattan Project, convinced both the government and scientists of the value of their collaboration. This

[16] Michael d'Antonio, *Atomic Harvest: Hanford and the Lethal Toll of America's Nuclear Arsenal* (Crown Publishing, 1993) and Russell J. Dalton, et al, *Critical Masses: Citizens, Nuclear Weapons Production, and Environmental Destruction in the United States and Russia* (MIT Press, 1999) portray Hanford/AEC as either consciously deceitful or at best blindly confident. Barton Hacker, in *Elements of Controversy*, defends the position that the AEC was using acceptable caution (and competent science) to back up its safety measures. Michelle Stenehjem Gerber, *On the Home Front*, switches between indictment and defense of Hanford, while C.D. Becker's *Aquatic Bioenvironmental Studies*, portrays Hanford science as of good quality and unaffected by institutional or political forces.

[17] Derek de Solla Price, *Little Science, Big Science ... and Beyond* (Columbia University Press, 1986), xv. Price, however, discounted the set of events surrounding WWII as little more than a temporary upsurge in science's long-term "straight road of exponential growth" going back to the seventeenth-century (p.15).

fruitful alliance, cemented during World War II, fed the technocratic, progressive tendencies that had been building in American society since the early Progressive era. As Price and others interested in the social composition of science were discovering, increasing involvement in science by the federal government fed the growing production of and demand for scientists.

Through influential advisory bodies such as the General Advisory Committee (GAC) of the AEC and the Presidential Science Advisory Committee (PSAC), the nation's elite scientists provided technical knowledge deemed necessary for problems at hand.[18] The attempted solutions found expression in places like Hanford where ordinary scientists delivered the sort of rational management and "technical fix" society expected.[19] In short, as government (and society) demanded more scientists, members of the profession (from all levels) eagerly assumed powerful leadership positions and worked to expand the scientific enterprise.

---

[18] Sylves, in *The Nuclear Oracles*, argues that especially in the early years of the AEC the GAC (made up of nine prominent scientists) exerted tremendous influence over policies (p.17 & passim). For the formation of the PSAC, its influence (and elitism) see Kevles, *The Physicists*, 385-90 & 394. See also Zuoyue Wang's "The Politics of Big Science: PSAC and the Funding of SLAC," *HSPS*, 1995, 25(2):324-356 in which he describes PSAC's crucial role in influencing Eisenhower and Kennedy administration's science funding policies. The National Science Foundation created a different kind of advisory system, but one that nonetheless gave scientists authority to control what, where, and whom to fund. See J. Merton England's *A Patron for Pure Science: The National Science Foundation's Formative Years, 1945-1957* (National Science Foundation, 1982), for an institutional history which capably describes the agency's structure and early programs. Toby Appel's *Shaping Biology* provides a more contextualized analysis of the NSF. Appel shows how the NSF adopted the Office of Naval Research's funding model, using program directors who relied extensively on the advice of prominent scientists (pp.1, 24-28, passim).

[19] The prominent role some expected scientists to play in American society during the 1950s and 1960s is enthusiastically argued by Alvin Weinberg, long-time director of the Oak Ridge National Laboratory (and PSAC member). In his classic article, "Can Technology Replace Social Engineering?" Weinberg argues that the technical expertise deployed during WWII should be used to help solve social problems (*University of Chicago Magazine*, Oct. 1966; more commonly available in Albert Teich's *Technology and the Future*, St. Martins Press, 1997, 7th edition, 55-64).

Bruce Hevly has argued that while the prominent AEC labs could attract the most prestigious scientists (often physicists), Hanford provided working opportunities for and required the services of "chemical engineers, mechanical engineers, and electrical engineers."[20] This thesis shows that these technical experts found employment at Hanford as well as (in smaller numbers) environmental scientists in the form of fisheries biologists, health physicists, radio-biologists, biochemists, geologists, meteorologists, and others. The departure of promising physicists at the end of the war created an opportunity for Herbert Parker, Harry Kornberg, Richard Foster and others, to steer the research program into less established fields of radiation biology and applied ecology. In the end, Hanford's relatively low status among research institutions encouraged the development of novel research programs in an effort to utilize the technocratic expertise deemed necessary for well-managed plutonium production.

Besides the overall growth of science in the twentieth-century, "big science" is associated with a particular model of social organization: large teams of researchers developing and using massive, complex instruments. Particle accelerators, most often funded by the AEC, provide the most striking example of big science.[21] Although nuclear and particle physics epitomized big science in the AEC, William and Liane Russell's mouse genetics programs at Oak Ridge, studying colonies of up to 100,000 mice, showed that the biological sciences could profitably employ large-scales experiments. With the success of this and other biology programs at Oak Ridge by the mid 1960s, Alvin Weinberg began to emphasize the significant contributions that big

[20] Hevly, "Hanford's Postwar Voices," *Working On The Bomb*, 230.

[21] See for example, Part 1 "The Big Physics of Small Particles" of Peter Galison and Bruce Hevly's edited volume *Big Science: The Growth of Large-Scale Research* (Stanford University Press, 1992).

science could make in the biological sciences.[22] Hanford's research program – not itself associated with big science - suggests that the big science common in AEC system created institutional space (and cover from criticism) for smaller projects. My study of Hanford looks at the margins of big science to explore, in Peter Galison's words, "the many kinds of activities that are subsumed under the term 'Big Science.'"[23]

Finally, issues having to do with the social and spatial organization of science - often referred to as "center and periphery" - are important to the evolution of Hanford's research program. In a study of the French university system, Mary Jo Nye has argued that "influence networks…and research trends" did not invariably originate in the center of power (Paris) and then spread to the outlying provincial schools as generally assumed; rather, innovative research schools were often established and remained outside of Paris.[24] In a study of Swedish science, Svante Lindqvist notes that "the position of Sweden on the northern periphery of Europe has … influenced the choice of subject and research traditions" with the effect being most pronounced in the earth and biological sciences such as "geography, geology, botany, and biology."[25] It is likely that Hanford's

---

[22] For Alvin Weinberg's emphasis on the biological sciences' potential, see *Reflections on Big Science*, (MIT Press, 1967), 104-108. The recent literature on big science is substantial. One must start with Galison & Hevly's *Big Science: The Growth of Large-Scale Research*; see also the useful overview essay by James H. Capshew & Karen A. Rader, "Big Science: Price to the Present," in *Osiris*, *Science After '40*, 2nd Series, 7:3-25. Robert Seidel has argued that AEC labs were particularly amenable to a "big science" style of research. See, "A Home for Big Science." Issues having to do with management and direction of a big science laboratory are nicely explored in Teich and Lambright's, "The Redirection of a Large National Laboratory."

[23] Peter Galison, "The Many Faces of Big Science," *Big Science*, 2

[24] Mary Jo Nye, *Science In The Provinces: Scientific Communities and Provincial Leadership in France, 1860-1930* (University of California Press, 1986), 239.

[25] Svante Lindqvist, "Introductory Essay: Harry Martinson and the Periphery of the Atom," *Center on the Periphery: Historical Aspects of 20th-Century Swedish Physics*, edited by Svante Lindqvist (Science History Publications, 1993), xi-lv. Quotation is found on p.xxiii.

particular location – on the margins of traditional scientific power networks – played a significant role in the kinds of research programs it developed. The eventual establishment of particular kinds of research programs at Hanford tests our understanding of the ways in which scientific trends and practices travel through social networks.[26]

Science in the Western United States

Finally, this study of Hanford's research program both supports and complicates a major theme in the historiography of the American West. As in the history of science, World War II is often interpreted as a major twentieth-century watershed event for the western United States. For historian Gerald Nash, the war years mark an economic and cultural transformation in which a region that had "emphasized colonialism" now possessed "self-sufficiency and innovation."[27] In his interpretation, science played an important role; certain regions in the West could boast of significant change, most notably the urban centers in California and the Southwest.

---

[26] In *The Molecular Vision of Life* (ch. 2, pp.55-76, "Technological Frontier: Southern California and the Emergence of Life Science at Caltech") Lily Kay approaches the development of CIT in something of a 'center/periphery' manner. She does not, however, explicitly develop these ideas. George E. Webb similarly recounts a regional history of science without explicit recourse to center and periphery arguments, "Scientists in the American Southwest: The Birth of a Community, 1906-1938," *Historian*, 1988 50(2):173-195.

[27] Gerald Nash, *The American West Transformed: The Impact of the Second World War* (Indiana University Press, 1985), vii. William Robbins has argued that a more comprehensive interpretation of the West should emphasize the powerful "Weltanschauung" of capitalism, integrating pre and post World War II transformations. See his overview of Western 'transformation' themes in *Colony And Empire: The Capitalist Transformation of the American West* (University Press of Kansas, 1994), 3-21. Roger W. Lotchin has utilized and critiqued the transformative role of World War II in regards to western urban studies. See "World War II and Urban California: City Planning and The Transformation Hypothesis," *Pacific Historical Review*, 1993, 62:143-171; and his broader study of the region which stresses the role of federal military investment, *Fortress California 1910-1961: From Warfare to Welfare* (Oxford University Press, 1992).

However, Nash's work suffers from a rather superficial treatment of science's role in twentieth-century transformations of the American West resulting, mainly, from a lack of pertinent literature.[28] Nash makes the broad claim that the American West contained a "range of important scientific research activities" by the end of World War II that in quality "presented a startling contrast to the prewar years."[29] This claim is supported through a retelling of events at the Manhattan Project's Los Alamos and Radiation Laboratory sites – a history that highlights the Southwest. For the Pacific Northwest, Nash points to the arrival of a prominent psychoanalyst in Seattle and the creation of a "science city" at Hanford to bolster his argument.[30] How Hanford constitutes a "science city" is left to the readers' imagination.

Carlos Schwantes has pointed out that the West and the Pacific Northwest were undeniably transformed by World War II, yet Nash's thesis is a better fit in the Southwest. The Northwest economy, Schwantes claims, remained "closely tied to the economic fate of its forest, mining, fishing, and agricultural industries" well into the post-war period.[31] Elsewhere Schwantes has argued that in the Northwest one can see

[28] This is especially pronounced in the Northwest, a point brought out in Keith Benson's recent essay, "The Maturation of Science in the Pacific Northwest: From Nature Studies to Big Science," *The Great Northwest: The Search for Regional Identity*, edited by William Robbins (Oregon State University, 2001), 94-106.

[29] Gerald Nash, *The American West Transformed*, 177. Nash has updated his arguments, yet science in the Northwest continues to be glossed over. See *World War II and the West: Reshaping the Economy* (University of Nebraska Press, 1990), 1, 7; and *The Federal Landscape: An Economic History of the Twentieth-Century West* (The University of Arizona Press, 1999), 51, 155.

[30] Ibid., p.172 & 177. Richard White's important history of the West, *It's Your Misfortune and None of My Own: A New History of the American West* (University of Oklahoma Press, 1991,) presents a similarly vague account of "science" in the Northwest. Scientists of the Manhattan Project, he writes, "transformed an earlier, largely parochial western science." Not without good reason (lack of documentation), Hanford is presented as an important center of science but only in reference to other, more well-known sites of Manhattan Project science (p.502-03).

[31] Carlos Schwantes, "The Pacific Northwest in World War II," *The Pacific Northwest in World War II*, edited by Carlos Schwantes (Sunflower University Press, 1986. Originally published in *Journal of*

greater continuity (rather than dramatic transformation) within the New Deal, World War II, and Cold War periods.[32] While World War II brought a handful of researchers to Hanford, it is in the Cold War period that the Hanford Laboratories gain independence and recognition. In regard to Schwantes' claim that the Northwest economy has retained close ties to natural resources, it may be that Hanford's development of environmentally oriented research can be attributed to the greater utility of the biological sciences for resource management than the AEC's primary scientific concerns – nuclear and particle physics.

Hanford's emphasis on environmental research highlights an irony that journalists and historians have exposed since the 1980s about long-standing practices of what we now see as reckless environmental contamination. Historian Patricia Limerick argues that Hanford's existence, practices, and particular setting are potent symbols of twentieth-century Western history. Our "failure to reckon with nuclear waste is a national shortcoming, even an international one," she argues, and should invite historians to point out the particular disjunctions between Western myth and Western reality.[33] But as an important symbol of the West, Hanford should not be seen imply as an environmental disaster. Hanford scientists reflected particular and historically situated ways of thinking about and managing the environment. And Hanford's history

---

the West, 1986), 17,18. Schwantes readdresses this theme in *The Pacific Northwest: An Interpretive History* (University of Nebraska Press, 1996. revised edition), 420-428.

[32] Carlos Schwantes, "Wage Earners and Wealth Makers," *The Oxford History of the American West*, edited by Clyde A. Milner II, Carol A. O'Connor, & Martha A. Sandweiss (Oxford University Press, 1994), 455. "In a real sense," Schwantes writes, "World War II never ended. The infusion of federal dollars into the western economy, which had become so noticeable during the New Deal and World War II years, continued with the cold war."

[33] Limerick, "The Significance of Hanford in American History," 168.

also illustrates interesting developments that arose out of struggles, however flawed, to deal with unique technical and social problems.[34]

In the end Hanford's expanding research program, based on ideals of the efficacy of scientific management, took advantage of rapidly multiplying opportunities in order to establish a permanent, multi-disciplinary research center. Monitoring radiation in communities of organisms, determining dangerous levels of radiation, and managing industrial environmental contamination lacked scientific identity and respectability at the beginning of Hanford's existence. The creation of a new field of research – environmental science – helped bring coherence to a congeries of seemingly heterogeneous studies. The environmental science program in the AEC lent institutional support to scientists engaged in broadly ecological applied science, beginning in the mid 1950s. No longer were Hanford scientists simply a multi-disciplinary group of physicists, biochemists, radio-biologists, veterinarians, botanists, geologists, and meteorologists; they were now working on common problems grouped under the term environmental science.[35]

---

[34] For the ways in which the West has been most closely associated with the "atom" see the essays in Bruce Hevly and John M. Findlay's *The Atomic West*. Joshua Silverman has written that 'environmental safety' practices developed at nuclear production sites arose in a complex milieu of competing technical experts. These experts assumed, without the participation of affected populations, that their practices posed no immediate danger. Only with increased public participation in nuclear debates, largely in the 1970s and 1980s, did environmental safety practices include wider concerns of public safety. Michael Joshua Silverman, *No Immediate Risk: Environmental Safety in Nuclear Weapons Production, 1942-1985* (Carnegie-Mellon University, PhD thesis, 2000).

[35] The two scientists most in favor of preserving the biology program within the Hanford Labs – Herbert Parker and Richard Foster – appear to be the first to adopt an 'environmental science' identity. In *American Men and Women of Science*, Parker identifies himself as a "physicist" through the 1965 edition but by 1971 cites his research expertise as "environmental science." Foster identifies himself as a "radio-biologist" in 1960, as a "radio-biologist" in charge of "environmental studies" in 1965, and simply as an "environmental scientist" in 1971. This transition in professional identification took place, explicitly, in the late 1960s and into the 1970s, but the methodological roots

Robert Kohler's recent monograph on field sciences argues that, in order to gain respectability, field scientists needed to adopt practices of laboratory science – something ecosystem (and radiation) ecologists did to great effect in quantifying chaotic natural systems. The ability of biologists to bridge the field-lab divide, Kohler asserts, led to the establishment of vigorous disciplines on the "borders" of traditional science.[36] Hanford scientists development of particular styles of research and the gradual elevation of the laboratory's reputation suggests one way in which the borderlands of science developed and gained prestige. For the environmental sciences this complex process found practical institutional justification in an unlikely source - the AEC of the 1950s – and then built on the popular 'environmental' movement of the 1960s and 1970s.[37]

The expansion of Hanford's research program can be understood in terms of 'disciplinary' formation (the growing relevance and popularity of a certain set of research goals, practices, traditions, objectives), and equally in the social and political uses of science itself. Vannevar Bush, arguably the most influential architect of post-war science, imagined a future in which American society would invest in basic,

---

and institutional support for this style of science, I claim, reaches back to the 1940s and 1950s respectively.

[36] Kohler, *Landscapes and Labscapes*. See especially Kohler's discussion of the development of ecosystem ecology, pp., 270-280?

[37] In reference to disciplinary construction Bernadette Bensaude-Vincent has suggested a useful way to think about recent "disciplinary" inventions such as "environmental science." The "material sciences," she argues, did "not result from a process of diversification within a field that keeps cohesion, but from the clustering of specialists from various disciplines and the amalgamation of science and technology." "The Construction of a Discipline: Materials Science in the United States," *HSPS*, 2001, 31(2):223-248. Ronald Doel has addressed disciplinary formation for the earth sciences in "The Earth Sciences and Geophysics," *Science in the Twentieth Century*, edited by John Krige & Dominique Pestré (Harwood Academic Publishers, 1997), 391-416.

unfettered science, the "endless frontier,"[38] in order to preserve democratic vigor in the face of totalitarian opposition. Along with most scientists at the time, Bush prized above all else scientific pursuit unalloyed by political influence. Yet, his experience and pragmatism pushed him to believe that scientists must contribute materially to national defense. In *Modern Arms and Free Men: A Discussion of the Role of Science in Preserving Democracy*, Bush articulated his belief that only with "planning" and through scientists' "professional partnership" with national defense interests could the nation fight off post-war threats posed by the Soviet Union and its communist allies.[39]

Bush's ideas on "planning" the scientific enterprise drew vigorous response from other scientists equally disturbed by perceived threats to scientific and political freedom.[40] Yet, as scientists and the public struggled to understand their responsibility

[38] Bush's use of Frederick Jackson Turner was not unwarranted. In later developments of his thesis, Turner argued that Americans could find a new, invigorating frontier in science. At a speech to University of Washington graduates in 1914, Turner claimed, "In place of old frontiers of wilderness, there are new frontiers of unwon fields of science. ... The test tube and the microscope are needed rather than ax and rifle." See *Rereading Frederick Jackson Turner: 'The Significance of the Frontier in American History' and Other Essays*, with commentary by John Mack Faragher (Yale University Press, 1994), 8.

[39] Vannevar Bush, *Modern Arms and Free Men: A Discussion of the Role of Science in Preserving Democracy* (Simon and Schuster, 1949), 253. Bush used 'planning' to mean "bringing the light of reason to bear on the future as a basis for logical action." Though Bush worked closely with the Roosevelt and Truman administrations, ideologically he allied with Hoover-style Republicans. Thus he explained, "We want our planning done by sound men in whom we have confidence, and not by faddist advocates of a rigidly socialized state, or advocates of any ism. ... we want the kind of planning that will release the energies of our people so that the free-enterprise system can work still better." See page 249. For a biography of Bush, including his political leaning, see G. Pascal Zachary, *Endless Frontier: Vannevar Bush, Engineer of the American Century* (The Free Press, 1997), 65, 249-61, 348-54, and passim.

[40] For example, Michael Polanyi argued that free, democratic societies were clearly in danger of forsaking "knowledge for its own sake, regardless of any advantage to the welfare of society." For Polanyi, "pure" science constituted the greatest defense of democracy: "the pursuit of science can be organized ... in no other manner than by granting complete independence to all mature scientists." What may appear as wasteful chaos to the non-scientist, Polanyi argued, will actually result in more efficient coverage of "the whole field of possible discoveries." Freedom, not planning and external organization, provided the stimulus "on which the progress of science truly depends." See Michael Polanyi, *The Logic of Liberty: Reflections and Rejoinders* (The University of Chicago Press, 1951), 3, 89, and passim. The point here is that, especially following WWII, most prominent scientists

for World War II, agreement among scientists over the inherent value of basic science overshadowed slight variations in emphasis. Basic science, in this view, must be society's primary emphasis. As a result, places like Hanford which epitomized practical, planned, applied science were seen as a necessity, but certainly not a site for major scientific investment. Herbert Parker's conservative early research proposals reflected scientist's general reservations about applied, programmatic research.

By the latter half of the 1950s one can detect a softening of the line between basic and applied science, between the superiority of free research and the necessity (but inferiority) of directed science. This coincided with a slackening of explicit anti-communist rhetoric in American politics, but more concretely with the dramatic successes of Soviet science and technology. Within two weeks of Sputnik's launch in the fall of 1957, President Dwight Eisenhower appointed a full-time science advisor and elevated an advisory committee to the White House. The President's Science Advisory Committee stressed the need for a massive increase in the output of scientists in order for the nation to successfully compete with the Soviet Union.[41] Two years after Sputnik, the committee issued a forceful statement on the nation's need for science. Now, with the United States losing ground to a more organized foe, the nation's foremost scientists conceded that "enhancement of the general welfare" held equal status with the "advance of knowledge." After Sputnik, selling science to the American people required that it be done "on the most practical grounds."[42] Even though post-war federal commitments to

---

espoused the complete freedom of science as necessary for the health of science as well as for the health of liberal democracy.

[41] Daniel Kevles, *The Physicists*, 385.

[42] Presidential Science Advisory Committee, "Education for the Age of Science," 4-5. The value of certain kinds of information, such as that from oceanography, proved that "basic" science could be

science were not threatened, elite scientists began to accept that the federal government demanded more immediate return on its massive investment. Taking place at a time of growing criticism of elite scientists and institutions, this change helped to redirect attention to scientists and institutions working on practical science in previously neglected regions of the country.

Finally, as the Hanford Laboratories gave way to the Pacific Northwest Laboratory in 1964, the political and social context of science had altered in important ways. In the early 1950s faith in American science allowed Washington State Senator Henry M. Jackson to proclaim that in "laboratories, scientific skills, and specialized brains – the advantage is overwhelmingly on our side." America's clear superiority in science allowed 'basic' science to gradually filter down to practical application.[43] By the mid 1960s, however, a crisis of confidence pushed Jackson to state that, "we have lost scientific and engineering races we wanted to win. In one critical scientific project after another our problem has been not how to stay ahead, but how to catch up." As Jackson pointed out, losing this race in science held significance for the very foundation of American democracy: Soviet dominance in science threatened to prove "that the Soviet system is superior to ours in every way." For the ultimate Cold Warrior, a politician steeped in the battle of ideologies, evidence of Soviet ascendance in science and technology required a reappraisal of American aims and strategies. For science the

---

useful to the military while simultaneously fundamental to oceanographers. The differing motivations for pursuing "basic" science has been explored by Joseph Darwin Hamblin, "The Navy's 'Sophisticated' Pursuit of Science: Undersea Warfare, the Limits of Internationalism, and the Utility of Basic Research, 1945-1956," *Isis*, 2002, 93:1-27. Similarly, Ronald Rainger has argued that the growth of post-war oceanography can be explained, in part, by the coincident goals of scientists and the military. "Science at the Crossroads: The Navy, Bikini Atoll, and American Oceanography in the 1940s," *HSPS*, 2000, 30:349-371.

[43] Kaufman, *Jackson*, 61.

challenge now lay, Jackson believed, in producing socially useful results: "all our cyclotrons, all our atomic reactors, all our wondrous space missiles, all our Nobel prize winners in science will avail us naught" without equal progress in understanding how best to use this knowledge.[44] The kinds of environmental science pursued at Hanford, science that aimed to maintain economically viable resources and minimize unnecessary environmental damage from industry, could supply the corrective Jackson called for.

The evolution and growth of scientific research at Hanford hinged on the development of particular kinds of research that gradually gained acceptance as the expectations for science changed during the Cold War. As America's commitment to practical science strengthened through the 1950s and into the 1960s, Hanford biologists took advantage of the perceived usefulness of environmental research and, with the establishment of a truly regional laboratory, ground their identity in an increasingly fashionable, relevant, and inclusive environmental science.

---

[44] Henry M. Jackson, *Fact, Fiction and National Security: A Key Senator Looks at the Cold War and Discusses how the Stable, Sensible Majority of Americans can make Freedom Prevail* (Macfadden-Bartell Capitol Hill Book, 1964), 34-36.

Bibliography

Aaserud, Finn. "Sputnik and the 'Princeton Three:' The National Security Laboratory that was not to be," *Historical Studies in the Physical and Biological Sciences,* 1995, 25(2):185-239.

Abir-Am, Pnina. "The Discourse of Physical Power and Biological Knowledge in the 1930s: A Reappraisal of the Rockefeller Foundation's 'Policy' in Molecular Biology," *Social Studies of Science*, 1982, 12:341-382.

-------. "The Biotheoretical Gathering, Transdisciplinary Authority and the Incipient Legitimation of Molecular Biology in the 1930s: New Perspective on the Historical Sociology of Science," *History of Science*, 1987, 25:1-70.

Albright, David, Frans Berkhout, and William Walker. *World Inventory of Plutonium and Highly Enriched Uranium, 1992.* Oxford, England: Oxford University Press, 1993.

Appel, Toby A. *Shaping Biology: The National Science Foundation and American Biological Research, 1945-1975.* Baltimore, MD: The Johns Hopkins University Press, 2000.

Badash, Lawrence. "Radium, Radioactivity, and the Popularity of Scientific Discovery," *Proceedings of the American Philosophical Society*, 1978, 122(3):145-154.

Battelle Memorial Institute, Pacific Northwest Laboratory. "Hanford Biology Annual Report, 1964," Document # BNWL-122. Richland, WA: Pacific Northwest Laboratory, 1965.

Beatty, John. "Genetics in the Atomic Age: The Atomic Bomb Casualty Commission, 1947-1956," *The Expansion of American Biology*, edited by Keith R. Benson, Jane Maienschein, & Ronald Rainger, 284-324. New Brunswick, NJ: Rutgers University Press, 1991.

-------. "Ecology and Evolutionary Biology in the War and Postwar Years: Questions and Comments," *Journal of the History of Biology*, 1988, 21(2):245-263.

Becker, C. Dale. *Aquatic Bioenvironmental Studies: The Hanford Experience, 1944-1984.* New York, NY: Elsevier Science Publications Company, 1990.

Bensaude-Vincent, Bernadette. "The Construction of a Discipline: Materials Science in the United States," *Historical Studies in the Physical & Biological Sciences*, 2001, 31(2):223-248.

Benson, Keith R. "The Maturation of Science in the Northwest: From Nature Studies to Big Science," edited by William G. Robbins. Corvallis, OR: Oregon State University Press, 2001.

-------. "The Emergence of Ecology from Natural History," *Endeavour*, 2000, 24(2):59-62.

Benson, Keith, Jane Maienschein, and Ronald Rainger. *The Expansion of American Biology*. New Brunswick, NJ: Rutgers University Press, 1991.

Boehm, George A. W. and Alex Groner. *Science in the Service of Mankind.* Columbus, OH: Battelle Press, 1986.

Bocking, Stephen. "Ecosystems, Ecologists, and the Atom: Environmental Research at Oak Ridge National Laboratory," *Journal of the History of Biology,* 1995, 28:1-47.

Bonham, K., L.R. Donaldson, R.F. Foster, A.D. Welander, and A.H. Seymour. "The Effect of X-ray on Mortality, Weight, Length, and Counts of Erythrocytes and Hematopoitic Cells in Fingerling Chinook Salmon, *Oncorhyncus tschawytscha* Walbaum," *Growth*, 1948, 12:107-121.

Bowler, Peter J. *The Norton History of the Environmental Sciences.* New York, NY: W. W. Norton & Company, 1993.

-------. "Science and the Environment: New Agendas for the History of Science?" *Science And Nature: Essays in the History of the Environmental Sciences*, edited by Michael Shortland, 1-21. Oxford, England: British Society for the History of Science, Alden Press, 1993.

Bush, Vannevar. *Science – The Endless Frontier: A Report to the President on a Program for Postwar Scientific Research, July 1945.* Washington, DC: National Science Foundation, 1990.

-------. *Endless Horizons.* Washington, DC: Public Affairs Press, 1946.

-------. *Modern Arms and Free Men: A Discussion of the Role of Science in Preserving Democracy.* New York, NY: Simon and Schuster, 1949.

Capshew, James, H. and Karen A. Rader. "Big Science: Price to the Present," *Science After '40, Osiris*, 2nd Series, 1992, 7:3-25.

Cheldelin, Vernon and Agnar Nygaard, Harry Kornberg, Roger Williams. "Vitamin B_6 Phosphates, Growth Factors for Leuconostoc Mesenteroides," *Journal of Bacteriology*, 1951, 62(1):134-135.

Clark, Claudia. *Radium Girls: Women and Industrial Health Reform, 1910-1935.* Chapel Hill, NC: University of North Carolina Press, 1997.

Clowse, Barbara B. *Brainpower for the Cold War: The Sputnik Crisis and National Defense Education Act of 1958.* Westport, CT: Greenwood Press, 1981.

Commoner, Barry. *Science and Survival.* New York, NY: The Viking Press, 1963.

Crease, Robert. *Making Physics: A Biography of Brookhaven National Laboratory, 1946-1972.* Chicago, IL: University of Chicago Press, 1999.

Dalton, Russell J., et al. *Critical Masses: Citizens, Nuclear Weapons Production, and Environmental Destruction in the United States and Russia.* Cambridge, MA: MIT Press, 1999.

D'Antonio, Michael. *Atomic Harvest: Hanford and the Lethal Toll of America's Nuclear Arsenal.* New York, NY: Crown Publishers, 1993

Dennis, Michael Aaron. "'Our First Line of Defense': Two University Laboratories in the Postwar American State," *Isis*, 1994, 85:427-455.

-------. "Secrecy and Science Revisited: from Politics to Historical Practice and Back," *Secrecy and Knowledge Production*, edited by Judith Reppy, 1-17. Cornell University, Peace Studies Program, Occasional Paper #23, 1999. <http://www.einaudi.cornell.edu/PeaceProgram/publications/occasional_papers/occasional-paper23.pdf>

Dietrich, William. *Northwest Passage: The Great Columbia River.* New York, NY: Simon & Schuster, 1995.

Divine, Robert A. *Blowing on the Wind: The Nuclear Test Debate, 1954-1960.* New York, NY: Oxford University Press, 1978

-------. *Eisenhower and the Cold War.* Oxford, England: Oxford University Press, 1981.

-------. *The Sputnik Challenge.* Oxford, England: Oxford University Press, 1993.

Doel, Ronald E. *Solar System Astronomy in America: Communities, Patronage, and Interdisciplinary Science, 1920-1960.* Cambridge, England: Cambridge University Press, 1996.

-------. "The Earth Sciences and Geophysics," *Science in the Twentieth Century*, edited by John Krige & Dominique Pestre, 391-416. Amsterdam: Harwood Academic Publishers, 1997.

Donaldson, Lauren and Richard Foster. "Effects of Radiation on Aquatic Organisms," *The Effects of Atomic Radiation on Oceanography and Fisheries*, 96-102. Washington, DC: National Academy of Sciences - National Research Council, 1957.

Duggar, B. M. "Preface," *Biological Effects of Radiation*, edited by B. M. Duggar. New York, NY: McGraw-Hill, 1936.

England, J. Merton. *A Patron for Pure Science: The National Science Foundation's formative years, 1945-1957.* Washington, DC: National Science Foundation, 1982.

Ficken, Robert. *Rufus Woods: The Columbia, and the Building of Modern Washington.* Pullman, WA: Washington State University Press, 1995.

-------. “Grand Coulee and Hanford: The Atomic Bomb and the Development of the Columbia River,” *The Atomic West*, edited by Bruce Hevly and John Findlay. Seattle, WA: University of Washington Press, 1998.

Findlay, John M. *Magic Lands: Western Cityscapes and American Culture after 1940.* Berkeley, CA: University of California Press, 1992.

-------. "Atomic Frontier Days: Richland, Washington, and the Modern American West," *Journal of the West*, 1995, 34(3):32-41.

Forman, Paul. “Behind Quantum Electrodynamics: National Security as Basis for Physical Research in the United States, 1940-1960,” *Historical Studies in the Physical and Biological Sciences*, 1987, 18:149-229.

Foster, Richard F. *Some Effects on Embryo and Young Rainbow Trout (Salmo Gairdnerii Richardson) from Exposing the Parents to X-rays.* Seattle, WA: University of Washington, PhD, 1948.

Foster, R.F. & J. J. Davis. “The Accumulation of Radioactive Substances in Aquatic Forms,” *Proceedings of the International Conference on the Peaceful Uses of Atomic Energy, vol.13*, 364-367. New York, NY: United National Publications, 1956.

Foster, Richard F. and Louis A. Krumholz. “Accumulation and Retention of Radioactivity from Fission Products and other Radiomaterials by Fresh-water Organisms,” *The Effects of Atomic Radiation on Oceanography and Fisheries*, 88-95. National Academy of Sciences - National Research Council, 1957.

Galison, Peter. “Three Laboratories,” *Social Research*, 1997, 64(3):1127-1155.

-------. “The Many Faces of Big Science,” *Big Science: The Growth of Large-Scale Research*, 1-8. Stanford, CA: Stanford University Press, 1992.

Geiger, Roger L. “Science, Universities, and National Defense, 1945-1970,” *Science After '40, Osiris*, 2nd Series, 1992, 7:26-48.

Geison, Gerald L. “Scientific Change, Emerging Specialties, and Research Schools,” *History of Science*, 1981, 19(1):20-40.

Geison, Gerald L. and Frederic L. Holmes, eds. *Research Schools, Historical Appraisals, Osiris*, 2nd Series, 1993, volume 8.

General Electric, Radiological Sciences Department. “Biology Research Annual Report, 1951,” Document # HW-25021. Richland, WA: General Electric, Nucleonics Division, 1952.

-------. “Biology Research Annual Report, 1952,” Document # HW-28636. Richland, WA: General Electric, Hanford Atomic Products Operation, 1953.

-------. "Biology Research Annual Report, 1953," Document # HW-30437. Richland, WA: General Electric, Hanford Atomic Products Operation, 1954.

-------. "Biology Research Annual Report, 1954," Document # HW-35917. Richland, WA: General Electric, Hanford Atomic Products Operation, 1955.

-------. "Biology Research Annual Report, 1955," Document # HW-41500. Richland, WA: General Electric, Hanford Atomic Products Operation, 1956.

General Electric, Hanford Laboratories. "Biology Research Annual Report, 1956," Document # HW-47500. Richland, WA: General Electric, Hanford Atomic Products Operation, 1957.

-------. "Hanford Biology Research Annual Report, 1957," Document # HW-53500. Richland, WA: General Electric, Hanford Atomic Products Operation, 1958.

-------. "Hanford Biology Research Annual Report, 1958," Document # HW-59500. Richland, WA: General Electric, Hanford Atomic Products Operation, 1959.

-------. "Hanford Biology Research Annual Report, 1959," Document # HW-65500. Richland, WA: General Electric, Hanford Atomic Products Operation, 1960.

-------. "Hanford Biology Research Annual Report, 1960," Document # HW-69500. Richland, WA: General Electric, Hanford Atomic Products Operation, 1961.

-------. "Hanford Biology Research Annual Report, 1961," Document # HW-72500. Richland, WA: General Electric, Hanford Atomic Products Operation, 1962.

-------. "Hanford Biology Research Annual Report, 1962," Document # HW-76000. Richland, WA: General Electric, Hanford Atomic Products Operation, 1963.

-------. "Hanford Biology Research Annual Report, 1963," Document # HW-80500. Richland, WA: General Electric, Hanford Atomic Products Operation, 1964.

(For 1964 report see citation under Battelle Memorial Institute)

Gerber, Michele Stenehjem. *On the Home Front: the Cold War legacy of the Hanford Nuclear Site*. Lincoln, NE: University of Nebraska Press, 1992, 2nd Edition.

Goldberg, Stanley. "General Groves and the Atomic West: The Making and the Meaning of Hanford," *The Atomic West*, edited by Bruce Hevly and John Findlay, 39-89. Seattle, WA: University of Washington Press, 1998.

Golley, Frank. *A History of the Ecosystem Concept in Ecology: More Than the Sum of the Parts*. New Haven, CT: Yale University Press, 1993.

Greenbaum, Leonard. *A Special Interest: The Atomic Energy Commission, Argonne National Laboratory, and the Midwestern Universities*. Ann Arbor, MI: University of Michigan Press, 1971.

Greenberg, Daniel S. *The Politics of Pure Science.* (1999 reprint includes introductory essays by John Maddox and Steven Shapin.) Chicago, IL: University of Chicago Press, 1999 (1st published in 1967).

Groves, Leslie. *Now It Can Be Told: The Story of the Manhattan Project.* New York, NY: Harper & Row, 1962.

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

-------. "Secrecy, Authorship, and Nuclear Weapons Scientists," *Secrecy and Knowledge Production*, edited by Judith Reppy, 57-75. Cornell University, Peace Studies Program, Occasional Paper #23, 1999.

Hacker, Barton C. *The Dragon's Tail: Radiation Safety in the Manhattan Project, 1942-1946.* Berkeley, CA: University of California Press, 1987.

-------. *Elements of Controversy: The Atomic Energy Commission and Radiation Safety in Nuclear Weapons Testing, 1947-1974.* Berkeley, CA: University of California Press, 1994.

Hagan, Joel B. *An Entangled Bank: The Origins of Ecosystem Ecology.* New Brunswick, NJ: Rutgers University Press, 1992.

Hamblin, Joseph Darwin. "The Navy's 'Sophisticated' Pursuit of Science: Undersea Warfare, the Limits of Internationalism, and the Utility of Basic Research, 1945-1956," *Isis*, 2002, 93(1):1-27.

Hanson, W. C. & H. A. Kornberg. "Radioactivity in Terrestrial Animals Near an Atomic Energy Site," *Proceedings of the International Conference on the Peaceful Uses of Atomic Energy, vol.13*, 385-388. New York, NY: United National Publications, 1956.

Heilbron, John L. and Robert W. Seidel. *Lawrence and his Laboratory: A History of the Lawrence Berkeley Laboratory.* Berkeley, CA: University of California Press, 1989.

Herken, Gregg. "The University of California, the Federal Weapons Labs, and the Founding of the Atomic West," *The Atomic West*, edited by Bruce Hevly and John Findlay. Seattle, WA: University of Washington Press, 1998.

Hevly, Bruce. "Introduction to the Nuclear Northwest," *Pacific Northwest Quarterly*, 1994, vol. 85(1):4-5.

-------. "Hanford's Postwar Voices," *Working on the Bomb: An Oral History of WWII Hanford*, edited by S. L. Sanger, 231-236. Portland, OR: Portland State University, Continuing Education Press, 1995.

Hevly, Bruce and John M. Findlay. "The Atomic West: Region and Nation, 1942-1992," *The Atomic West*, edited by Bruce Hevly and John M. Findlay, 3-18. Seattle, WA: University of Washington Press, 1998.

Hewlett, Richard G. "'Born Classified' in the AEC: A Historian's View," *Bulletin of the Atomic Scientists*, 1981, 37(10):20-27.

Hewlett, Richard G. and Oscar Anderson. *The New World: Vol. I, 1939-1946, A History of the United States Atomic Energy Commission.* Washington, DC: U.S. Atomic Energy Commission, 1962.

Hewlett, Richard G. and Francis Duncan. *Atomic Shield: Vol. II, 1947-1952, A History of the United States Atomic Energy Commission.* Washington, DC: U.S. Atomic Energy Commission, 1969.

Hewlett, Richard G. and Jack M. Holl. *Atoms for Peace and War, 1953-1961: Eisenhower and the Atomic Energy Commission.* Berkeley, CA: University of California Press, 1989.

Hines, Neal. *Proving Ground: An Account of the Radiobiological Studies in the Pacific, 1946-1961.* Seattle, WA: University of Washington Press, 1962.

Hoddeson, Lillian. "Mission Change in the Large Laboratory: The Los Alamos Implosion Program, 1943-1945," *Big Science: The Growth of Large-Scale Research*, edited by Peter Galison & Bruce Hevly, 265-289. Stanford, CA: Stanford University Press, 1992.

Holl, Jack M. *Argonne National Laboratory, 1946-96.* Urbana, IL: University of Illinois Press, 1997.

-------. "The National Reactor Testing Station: the Atomic Energy Commission in Idaho, 1949-1962," *Pacific Northwest Quarterly*, 1994, 85(1):15-24.

Hounshell, David. "Du Pont and the Management of Large-Scale Research and Development," *Big Science: The Growth of Large-Scale Research*, edited by Peter Galison & Bruce Hevly, 236-261. Stanford, CA: Stanford University Press, 1992.

Hounshell, David and John K. Smith. *Science and Corporate Strategy: Du Pont R&D, 1902-1980.* Cambridge, England: Cambridge University Press, 1988.

Howes, Ruth H. & Caroline L. Herzenberg. *Their Day in the Sun: Women of the Manhattan Project.* Philadelphia, PA: Temple University Press, 1999.

Israel, Paul. *From Machine Shop to Industrial Laboratory: Telegraphy and the Changing Context of American Invention, 1830-1920.* Baltimore, MD: Johns Hopkins University Press, 1992.

Jackson, Henry M. *Fact, Fiction and National Security: A Key Senator Looks at the Cold War and Discusses How the Stable, Sensible Majority of Americans Can Make Freedom Prevail.* Washington, DC: Macfadden-Bartell Capitol Hill Book, 1964.

Johnson, Leland and Daniel Schaffer. *Oak Ridge National Laboratory: The First Fifty Years.* Knoxville, TN: University of Tennessee Press, 1994.

Joint Committee on Atomic Energy. "The Future Role of the Atomic Energy Commission Laboratories: A report to the Joint Committee on Atomic Energy by the United States Atomic Energy Commission, Washington, D.C., January 1960." Washington, DC: Government Printing Office, 1960.

Jolly, J. Christopher. *Necessary Uncertainty: The Scientific Debate Over Fallout, 1954-1963.* Corvallis, OR: Oregon State University, PhD in progress.

Kargon, Robert, Stuart W. Leslie, and Erica Schoenberger. "Far Beyond Big Science: Science Regions and the Organization of Research and Development," *Big Science: The Growth of Large-Scale Research*, edited by Peter Galison & Bruce Hevly, 334-354. Stanford, CA: Stanford University Press, 1992.

Kathren, Ronald. "A Brief Scientific Biography," *Herbert M. Parker: Publications and Other Contributions to Radiological and Health Physics*, edited and annotated by Ronald L. Kathren, Raymond W. Baalman, & William J. Bair, xiii-xxiv. Richland, WA: Battelle Press, 1986.

Kaufman, Robert G. *Henry M. Jackson: A Life in Politics.* Seattle, WA: University of Washington Press, 2000.

Kay, Lily E. *The Molecular Vision of Life: Caltech, the Rockefeller Foundation, and the Rise of the New Biology.* Oxford, England: Oxford University Press, 1993.

Kevles, Daniel J. *The Physicists: The History of a Scientific Community in Modern America.* Cambridge, MA: Harvard University Press, 1995 (originally published in 1971).

-------. "Principles and Politics in Federal R&D Policy, 1945-1990," *Science – The Endless Frontier*, i-xxxi. Washington, DC: National Science Foundation, 1990.

-------. "Cold War and Hot Physics: Reflections on Science, Security and the American State," *Proceedings of the International Conference on the Restructuring of Physical Sciences in Europe and the United States, 1945-1960*, edited by Michelangelo De Maria, Mario Grilli, and Fabio Sebastiani, 1-30. Teaneck, NJ: World Scientific, 1989.

-------. "Genetics in the United States and Great Britain 1890-1930: A Review with Speculations," *Biology, Medicine and Society 1840-1940*, edited by Charles Webster, 192-208. Cambridge, England: Cambridge University Press, 1981.

Kidd, Charles V. *American Universities and Federal Research.* Cambridge, MA: The Belknap Press of Harvard University Press, 1959.

Kingsland, Sharon. "An Elusive Science: Ecological Enterprise in the Southwestern United States," *Science and Nature: Essays in the History of the Environmental Sciences*, edited by Michael Shortland, 151-179. Oxford, England: British Society for the History of Science, Alden Press, 1993.

-------. *Modeling Nature: Episodes in the History of Population Ecology.* University of Chicago Press, 1985.

Kornberg, H. A. "Effects of Inhaled Radioactive Particles," *Report of the subcommittee on inhalation hazards, Committee on Pathologic Effects.* Washington, DC: National Academy of Sciences, 1961.

Kornberg, H. A. and J. J. Davis. "Food Chains in Fresh Water," *Radioactivity and Human Diet*, edited by R. Scott Russell, 385-418. London, England: Pergamon Press, 1966.

Kornberg, Harry and Ruth Langdon, Vernon Cheldelin. "Microbiological Assay for Riboflavin," *Analytical Chemistry*, 1948, 20(1):81-83.

Kohler, Robert E. *Landscapes and Labscapes: Exploring the Lab-Field Border in Biology.* Chicago, IL: University of Chicago Press, 2002.

-------. *Partners in Science: Foundations and Natural Scientists, 1900-1945.* Chicago, IL: University of Chicago Press, 1991.

-------. "Place and Practice in Field Biology," *History of Science*, 2002, 40(2):189-210.

Kwa, Chunglin. "Radiation Ecology, Systems Ecology and the Management of the Environment," *Science and Nature: Essays in the History of the Environmental Sciences*, edited by Michael Shortland, 213-250. Oxford, England: The Alden Press, 1993.

Leslie, Stuart W. *The Cold War and American Science: The Military-Industrial-Academic Complex at MIT and Stanford.* New York, NY: Columbia University Press, 1993.

-------. "Science and Politics in Cold War America," *The Politics of Western Science, 1640 – 1990*, edited by Margaret C. Jacob, 199-233. Atlantic Highlands, NJ: Humanities Press, 1994.

Leslie, Stuart W. and Robert Kargon. "Selling Silicon Valley: Frederick Terman's Model for Regional Advantage," *Business History Review*, 1996, 70(4):435-472.

-------. "Imagined Geographies: Princeton, Stanford and the Boundaries of Useful Knowledge in Postwar America," *Minerva*, 1994, 32:121-143.

Limerick, Patricia Nelson. "The Significance of Hanford in American History," *Washington Comes of Age: The State in the National Experience*, edited by David H. Stratton, 153-171. Pullman, WA: Washington State University Press, 1992.

Lindqvist, Svante. "Introductory Essay: Harry Martinson and the Periphery of the Atom," *Center on the Periphery: Historical Aspects of 20th-Century Swedish Physics*, edited by Svante Lindqvist, xi-lv. Canton, MA: Science History Publications, 1993.

Lotchin, Roger W. *Fortress California 1910-1961: From Warfare to Welfare.* Oxford, England: Oxford University Press, 1992.

-------. "World War II and Urban California: City Planning and the Transformation Hypothesis," *Pacific Historical Review*, 1993, 62:143-171.

Maddox, Robert Franklin. "The Politics of World War II Science: Senator Harley M. Kilgore and the Legislative Origins of the National Science Foundation," *West Virginia History*, 1979, 41:20-39.

Marx, Leo. "Environmental Degradation and the Ambiguous Social Role of Science and Technology," *Journal of the History of Biology*, 1992, 25(3):449-468.

Merton, Robert K. "The Normative Structure of Science," (reprint) *The Sociology of Science: Theoretical and Empirical Investigations*, edited by Norman W. Storer, 267-278. Chicago, IL: University of Chicago Press, 1973.

Mitman, Gregg. *The State of Nature: Ecology, Community, and American Social Thought, 1900-1950.* Chicago, IL: University of Chicago Press, 1992.

Mumford, Lewis. *Regional Planning in the Pacific Northwest: A Memorandum.* Portland, OR: Northwest Regional Council, 1939.

Nash, Gerald D. *The American West Transformed: The Impact of the Second World War.* Bloomington, IN: Indiana University Press, 1985.

-------. *World War II and the West: Reshaping the Economy.* Lincoln, NE: University of Nebraska Press, 1990.

-------. The Federal Landscape: An Economic History of the Twentieth-Century West. Tucson, AZ: The University of Arizona Press, 1999.

Needell, Allan A. "Nuclear Reactors and the Founding of Brookhaven National Laboratory" *Historical Studies in the Physical Sciences,* 1983, 14(1):93-122.

-------. "The Carnegie Institution of Washington and Radio Astronomy: Prelude to an American National Observatory," *Journal for the History of Astronomy*, 1991, 22:55-67

Neuberger, Richard. *Our Promised Land.* Moscow, ID: University of Idaho Press, 1989 (originally published by The Macmillan Company, 1938).

Nye, Mary Jo. *Science in the Provinces: Scientific Communities and Provincial Leadership in France, 1860-1930.* Berkeley, CA: University of California Press, 1986.

O'Neill, Dan. *The Firecracker Boys.* New York, NY: St. Martins Press, 1994.

-------. "H-Bombs and Eskimos: The Story of Project Chariot," *Pacific Northwest Quarterly,* 1994, 85(1):25-34.

Odum, Eugene. *Fundamentals of Ecology.* Philadelphia, PA: W. B. Saunders & Company, 1959, 2nd edition.

-------. "Ecology and the Atomic Age," *Associated Southeastern Biologists Bulletin,* 1957, 4:27-29.

Orlans, Harold. *Contracting for Atoms: A Study of Public Policy Issues Posed by the Atomic Energy Commission's Contracting for Research, Development, and Managerial Services.* Washington, DC: The Brookings Institution, 1967.

Pais, Abraham. *The Genius of Science: A Portrait Gallery.* Oxford, England: Oxford University Press, 2000.

Palladino, Paolo. "Defining Ecology: Ecological Theories, Mathematical Models, and Applied Biology in the 1960s and 1970s," *Journal of the History of Biology,* 1991, 24:223-243.

Panofsky, W. K. H. "SLAC and Big Science: Stanford University," *Big Science: The Growth of Large-Scale Research,* edited by Peter Galison & Bruce Hevly, 129-146. Stanford, CA: Stanford University Press, 1992.

Parker, Herbert M. *Herbert M. Parker: Publications and Other Contributions to Radiological and Health Physics,* edited and annotated by Ronald L. Kathren, Raymond W. Baalman, & William J. Bair. Richland, WA: Battelle Press, 1986.

Parker, Herbert M. "Radiation Exposure from Environmental Hazards," *Proceedings of the International Conference on the Peaceful Uses of Atomic Energy, vol.13,* 305-310. New York, NY: United National Publications, 1956.

Parker, Herbert M. and J. W. Healy. "Environmental Effects of a Major Reactor Disaster," *Proceedings of the International Conference on the Peaceful Uses of Atomic Energy, vol.13,* 106-109. New York, NY: United National Publications, 1956.

Parker, Herbert M. and J. N. Stannard. "Oral History Interview: Herbert Parker." DOE Reading Room, Richland, WA, Document #PNL-9915. June 4, 1979.

Pauling, Linus. *No More War!.* New York, NY: Dodd, Mead & Co., 1958.

Pauly, Philip J. *Biologists and the Promise of American Life: From Meriwether Lewis to Alfred Kinsey.* Princeton, NJ: Princeton University Press, 2000.

Pitzer, Paul. *Grand Coulee: Harnessing a Dream.* Pullman, WA: Washington State University Press, 1994.

Polanyi, Michael. *The Logic of Liberty: Reflections and Rejoinders.* The University of Chicago Press, 1951.

President's Science Advisory Committee, United States. "Education for the Age of Science." Washington DC: U.S. Government Printing Office, 1959.

Price, Derek J. de Solla. *Little Science, Big Science...and Beyond.* New York, NY: Columbia University Press, 1986.

Rainger, Ronald. "Introduction," *The Expansion of American Biology*, edited by Keith R. Benson, Jane Maienshein, & Ronald Rainger, 1-22. New Brunswick, NJ: Rutgers University Press, 1991.

-------. "Patronage and Science: Roger Revelle, the U. S. Navy, and Oceanography at the Scripps Institution," *Earth Sciences History*, 2000, 19(1):58-89.

-------. "Science at the Crossroads: The Navy, Bikini Atoll, and American Oceanography in the 1940s," *Historical Studies in the Physical and Biological Sciences,* 2000, 30:349-371.

Rasmussen, Nicolas. "The Mid-century Biophysics Bubble: Hiroshima and the Biological Revolution in America, Revisited," *History of Science*, 1997, 35(3):245-293.

Rediske, J. H. & F. P. Hungate. "The Absorption of Fission Products by Plants," *Proceedings of the International Conference on the Peaceful Uses of Atomic Energy, vol.13*, 354-356. New York, NY: United Nations Publications, 1956.

Reich, Leonard S. *The Making of American Industrial Research: Science and Business at GE and Bell, 1876-1926.* New York, NY: Cambridge University Press, 1985.

Reingold, Nathan. "Choosing the Future: The U. S. Research Community, 1944-1946," *Historical Studies in the Physical and Biological Sciences*, 1995, 25(2):301-328.

Revelle, Roger and Milner B. Schaefer. "General Considerations Concerning the Ocean as a Receptacle for Artificially Radioactive Materials," *The Effects of Atomic Radiation on Oceanography and Fisheries*, 1-25. Washington, DC: National Academy of Sciences, 1957.

Rhodes, Richard. *The Making of the Atomic Bomb.* New York, NY: Simon & Schuster, 1986.

Robbins, William G. *Landscapes of Promise: The Oregon Story, 1800-1940.* Seattle, WA: University of Washington Press, 1997.

-------. "Narrative Form and Great River Myths: The Power of Columbia River Stories," *Environmental History Review*, 17(2):1-22.

-------. *Colony & Empire: The Capitalist Transformation of the American West.* Lawrence KS: University of Kansas Press, 1994.

Sanger, S. L. *Working on the Bomb: An Oral History of WWII Hanford.* Portland, OR: Portland State University, Continuing Education Press, 1995.

Schneider, Daniel W. "Local Knowledge, Environmental Politics, and the Founding of Ecology in the United States: Stephen Forbes and 'The Lake as a Microcosm'," *Isis*, 2000, 91(4):681-705.

Schwantes, Carlos A., ed. *The Pacific Northwest in World War II.* Manhattan, KS: Sunflower University Press, 1986.

-------. "Wage Earners and Wealth Makers," *The Oxford History of the American West*, edited by Clyde A. Milner II, Carol A. O'Connor, & Martha A. Sandweiss, 431-467. New York, NY: Oxford University Press, 1994.

------. *The Pacific Northwest: An Interpretive History.* Lincoln, NE: University of Nebraska Press, 1996, revised edition.

Secretary of State's Committee on Atomic Energy. "A Report on the International Control of Atomic Energy" (prepared by: Chester Barnard, J. R. Oppenheimer, Charles Thomas, Harry Winne, and David Lilienthal, chairman). Washington, DC: U. S. Government Printing Office, March 1946.

Seidel, Robert W. "A Home for Big Science: The Atomic Energy Commission's Laboratory System," *Historical Studies in the Physical & Biological Sciences*, 1986, 16(1):135-175.

-------. "The National Laboratories of the Atomic Energy Commission in the Cold War," *Historical Studies in the Physical & Biological Sciences*, 2002, 32(1):145-162.

-------. "Accelerating Science: The Postwar Transformation of the Lawrence Radiation Laboratory," *Historical Studies in the Physical Sciences,* 1983, 13(2):375-400.

-------. "Accelerators and National Security: The Evolution of Science Policy for High-Energy Physics, 1947-1967," *History and Technology: An International Journal*, 1994, 11:361-391.

Servos, John W. "Changing Partners: The Mellon Institute, Private Industry, and the Federal Patron," *Technology and Culture*, 1994, 35(2):221-257.

-------. "Research Schools and Their Histories," *Research Schools, Historical Appraisals*, edited by Gerald Geison & Frederic L. Holms, *Osiris*, 2nd Series, 1993, 8:3-15.

Shortland, Michael, ed. *Science And Nature: Essays in the History of the Environmental Sciences.* Oxford, England: British Society for the History of Science, Alden Press, 1993.

Silverman, Michael Joshua. *No Immediate Risk: Environmental Safety in Nuclear Weapons Production.* Carnegie-Mellon University, PhD, 2000.

Stannard, J. Newell. *Radioactivity and Health: A History.* Edited by Raymond W. Baalman. Richland, WA: Battelle, Pacific Northwest Laboratory, 1988.

Strong, Robert A. "Eisenhower and Arms Control," *Reevaluating Eisenhower: American Foreign Policy in the 1950s*, edited by Richard A. Melanson & David Mayers, 241-266. University of Illinois Press, 1987.

Sylves, Richard T. *The Nuclear Oracles: A Political History of the General Advisory Committee of the Atomic Energy Commission, 1947-1977.* Ames, IA: Iowa State University Press, 1987.

Szasz, Ferenc M. "Introduction," *Working on the Bomb: An Oral History of WWII Hanford,* edited by S. L. Sanger, 1-8. Portland, OR: Portland State University, Continuing Education Press, 1995

Teich, Albert & W. Henry Lambright. "The Redirection of a Large National Laboratory," *Minerva,* (Winter 1976-1977), 14:447-474.

Thackray, Arnold. "Preface," *Science After '40, Osiris*, 2nd Series, 1992, 7:vii-ix.

Thorpe, Charles & Steven Shapin. "Who Was J. Robert Oppenheimer? Charisma and Complex Organization," *Social Studies of Science*, 2000, 30(4):545-590.

Truslow, Edith C. and Ralph Carlisle Smith. *Project Y: The Los Alamos Story (Volume II of The History of Modern Physics, 1800-1950).* Los Angeles, CA: Tomash Publishers, 1983.

Turner, Frederick Jackson (with commentary by John Mack Faragher). *Rereading Frederick Jackson Turner: 'The Significance of the Frontier in American History' and Other Essays.* New Haven, CT: Yale University Press, 1994.

Walker, J. Samuel. "The Atomic Energy Commission and the Politics of Radiation Protection, 1967-1971," *Isis*, 1994, 85:57-78.

Wang, Jessica. *American Science in an Age of Anxiety: Scientists, Anticommunism, and the Cold War.* Chapel Hill, NC: University of North Carolina Press, 1999.

-------. "Liberals, the Progressive Left, and the Political Economy in Postwar American Science: The National Science Foundation Debate Revisited," *Historical Studies in the Physical & Biological Sciences*, 1995, 26(1):139-166.

Wang, Zuoyue. "The Politics of Big Science: PSAC and the Funding of SLAC," *Historical Studies in the Physical & Biological Sciences*, 1995, 25(2):329-356.

Warren, Stafford. "The Physiological Effects of Radiation Upon Organ and Body Systems," *Biological Effects of Radiation*, edited by B. M. Duggar, 473-539. New York, NY: McGraw Hill, 1936.

Webb, George E. "Scientists in the American Southwest: The Birth of a Community, 1906-1938," *Historian*, 1988, 50(2):173-195.

Weinberg, Alvin M. *Reflections on Big Science*. Cambridge, MA: The M.I.T. Press, 1967.

-------. "Can Technology Replace Social Engineering?" *University of Chicago Magazine*, October, 1966.

Welander, Arthur D. *Studies of the Effects of Roentgen Rays on the Growth and Development of the Embryos and Larvae of the Chinook Salmon (Oncorhynchus tschawytscha)*. Seattle, WA: University of Washington, PhD, 1945.

Welander, A. D., L.R. Donaldson, R.F. Foster, K. Bonham, and A.H. Seymour. "The Effects of Roentgen Rays on the Embryos and Larvae of the Chinook Salmon," *Growth*, 1948, 12:203-242.

Westfall, Catherine. *The First 'Truly National Laboratory': The Birth of Fermilab*. Michigan State University, PhD, 1988.

Westfall, Catherine and Lillian Hoddeson. "Thinking Small in Big Science: The founding of Fermilab, 1960-1972," *Technology & Culture*, 1996, 37:457-492.

Westwick, Peter J. *The National Laboratory System in the U.S., 1947-1962*. Berkeley, CA: University of California at Berkeley, PhD, 1999.

Whicker, F. Ward and Vincent Schultz. *Radioecology: Nuclear Energy and the Environment, vol.1*. Boca Raton, FL: CRC Press, 1982.

White, Richard. *The Organic Machine*. New York, NY: Hill and Wang, 1995.

-------. *It's Your Misfortune and None of My Own: A New History of the American West*. Norman, OK: University of Oklahoma Press, 1991.

Wilimovsky, Norman and John Wolfe, eds. *Environment of the Cape Thompson Region, Alaska*. Washington, DC: AEC Division of Technical Information, 1966.

Winkler, Allan M. *Life Under a Cloud: American Anxiety About the Atom*. New York, NY: Oxford University Press, 1993.

Wise, George. *Willis R. Whitney, General Electric, and the Origins of U.S. Industrial Research*. New York, NY: Columbia University Press, 1985.

-------. "Ionists in Industry: Physical Chemistry at General Electric, 1900-1915," *Isis*, 1983, 74:7-21.

Worster, Donald. *Rivers of Empire: Water, Aridity, and the Growth of the American West.* New York, NY: Pantheon Books, 1985.

Zacharay, G. Pascal. *Endless Frontier: Vannevar Bush, Engineer of the American Century.* The Free Press, 1997.